Earth Dams, Slopes and Open Excavations

Barrages en Terre, Talus et Tranchées Ouvertes

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F. C. Walker
General Reporter, Division 6 / Rapporteur Général, Division 6

The Chairman

I welcome delegates to the ninth session, the last of our conference. This morning we are to discuss earth dams, slopes and open excavations, and I call on the General Reporter.

General Reporter

Previous Reporters have proposed various methods for subdividing soil engineering. I like a three-stage division that more or less corresponds to the background of the various portions of our membership. Those whose contact with soil has largely been in our halls of learning will of necessity favour a theoretical approach, piling theory upon theory in an attempt to explain observed phenomena.

As we proceed into the more practical world of actual construction we see so many violations of the basic assumptions of theoretical approaches and situations which theoretical knowledge does not explain that we develop a set of conservative practices, more or less simple procedures which we hope will prove to be adequate—that is, empirical methods. With still more experience, as we move into the class of senior consultants, we lose faith even in the empirical methods and fall back on our own experiences as a basis for all decisions—that is to say, on case histories.

I have tried to propose a question for discussion in each of these fields of knowledge to give an opportunity for all our membership to comment if they desire. However, if anyone has had an interesting problem to discuss, I have encouraged him to present it whether or not it was within the scope of my proposals. I hope that this meets with your approval.

In the written report I have tried to summarize our present situation with regard to the status of our knowledge on the subject of earth dams, slopes and open excavations. Although it did not appear that there had been any spectacular progress in this field since the third conference in Zurich, there were many implications that new developments which would have a marked effect on our future operations were in process of development. I have proposed for general discussion questions which I hope will hasten this development. Judging from the discussions we have already had, these developments are coming faster than anticipated and have to some extent already been covered in other sessions.

Although the subject of this session consists of three parts, in past practice these have been treated by the same procedure, and experience covering one of these parts has been assumed to be applicable to the others. However, an examination of these three parts will show that stresses in the soils are increased as a dam is built, they are relieved as an open excavation is made, and they are apparently constant in the condition of natural slopes. Up to now our efforts seem to have been directed primarily to the condition of increasing stresses and our soil testing procedures have been developed primarily for this condition.

There are several factors in our common experience that indicate that the soil strength parameters will be different for the conditions where stress relief is involved, that is to say, the condition of over-consolidation. At the same time there are more and more situations developing in engineering practice where past practices of accepting a calculated risk and repairing the failures are not economically satisfactory. It seems, therefore, very desirable that we give some attention to the further development of methods for more effectively evaluating the conditions of stability in excavated slopes.

In our consideration of failures in natural slopes, particularly those situations where the physical dimensions remain unchanged, it does not seem proper to attribute failure to a change in stress. For those situations we can only conclude that changes in the strength of the soil suddenly occur due to some change in the immediate environment. Before we can hope to be able to predict potential instability for natural slopes we must know what the potential ranges in change of strength are and what the conditions are that cause them.

The present state of our knowledge is that changes in moisture content, in particular changes in ground water level and consequently increases in pore pressure, are primarily responsible for many slope failures, and that changes in strength of 30 to 50 per cent may be expected to occur. However, the number of case histories where comparisons have been made between the apparent strength of the soil on the basis of tests prior to failure and the apparent strength at the time of failure are far too few to warrant definite conclusions at this time.

There are indications from many quarters that a considerable part of the soil strength that we attribute to cohesion is actually the result of capillary forces in the soil pores. If we can develop methods for measuring these forces and devise a theory for predicting their magnitude, a suitable correction can be applied to the shearing strength. It is possible that this correction will be of sufficient magnitude that shearing strength can be reduced to a simple factor dependent only on stress. If this can be accomplished it should provide a new impetus to the development of mathematical analyses for structural adequacy that heretofore have been too complicated for any practical use even after many simplifying assumptions were made.

I have thought enough of this idea to encourage its investigation by the laboratory staff of the Bureau of Reclamation as rapidly as possible. I had hoped to be able to contribute some positive results at this meeting, but pressure of other work prevented completion of even a progress report in time. However, the indication from the work so far done is positive that at least for some soil states the indicated cohesion can be largely attributed to capillary stresses. I suspect that this condition will account for the high indicated strengths in some unconfined compression tests which have led to a false indication of security.

I anticipate, therefore, that our present concepts of shearing strength as far as cohesion is concerned will go through much the same process as did our ideas of internal friction when the pore pressure concept was introduced.

We have already had some comments to the effect that cohesion should be eliminated from consideration, at least for some conditions of stability analysis. This is not a new idea to me: I have heard it proposed many times. I have not felt that cohesion should be omitted entirely, but I have been concerned that the indicated values as determined by present test procedures are not entirely dependable and therefore, until quite recently, I have followed a procedure of asking that in a stability analysis a safety factor of at least 1 be obtained without benefit of cohesion.

When the idea that cohesion could to a large extent be explained on the basis of capillary action was demonstrated to me by J. W. Hilf, one of my associates in the Bureau of Reclamation, it appeared to offer an explanation for many situations that have been puzzling us. We have set up a programme of experiments to measure the capillary stresses in soil, and apparatus and methods are being developed that should accomplish this.

When a large number of tests are made on a foundation or embankment material there will be considerable variation in the soil properties. Further, under conditions of construction and operation there will be variations in these soil properties from those determined in the laboratory under ideal test conditions. The question then arises as to the proper choice of values to use in analysing the performances of the structure.

Until recently there has been no mention of the actual practices used in this regard. I myself have used a variety of procedures including such requirements as: that with minimum strengths a safety factor should be at least 1, that safety should be maintained either with the worst materials under optimum conditions or the average material under the worst condition, and that a safety factor of 1·5 should be maintained with a soil whose properties are an average between the worst and the apparent average.

It is our practice at the Bureau of Reclamation to make only a few strength and consolidation tests, but to make as thorough a test as possible. Average and worst conditions are determined by visual examination and with the use of Atterberg and other index tests. The result is that although the individual tests are expensive, the cost of a site investigation is relatively cheap. Although the volume of test information is relatively small, we do believe that it is sufficiently representative and that it is reliable. In addition, we use graphical methods for our analyses and can in consequence use actual plotted curves in the determination of pore pressures and consolidation and thereby avoid the difficulties that sometimes occur when coefficients are first determined and then used in computations.

In spite of the numerous test procedures that we have available, much of our soil engineering is being done and will continue to be done on the basis of visual examinations. To aid in this practice we have developed procedures for the identification of soils as materials and have to some extent correlated soil properties with material types. However, most of us are aware that the performance of a soil mass is greatly dependent upon the soil structure, but we do not yet have a similar system by which soil structure can be defined. This is certainly a major handicap in our being able to exchange information on our experiences.

Within the Bureau of Reclamation I have encouraged the securing of very detailed geological descriptions of sites, particularly as to the conditions of the soils in place, and tried to discourage the use of those terms which apply to a soil structure when describing soil as a material. That is, the term 'loess' should not be used in place of 'silt'. Some of our geologists have become quite good at differentiating between conditions where there is a significant difference in engineering properties of the soil structure and in conveying this information; but geological classification has not been developed to discriminate between soil structure with differing engineering properties except along very broad lines. We need something equivalent to the soil classification system initiated by A. Casagrande which will describe soil structure.

Our present ideas for such a system involve an initial grouping according to origin of the soil structure; that is: residual, alluvial, glacial and aeolian, with subdivisions according to geological practices. Beyond this point it becomes a matter of determining how far we can go with visual discrimination and still maintain definition along lines comparable to variations in engineering properties. Perhaps if we can get some of your ideas we may be able to lay the ground work for more efficient communication of ideas at future conferences.

T. K. Huizinga (Netherlands)

In Vol. V of the Proceedings of the Second Conference A. W. Koppejan, B. M. van Wamelen and L. J. H. Weinberg gave a contribution on the coastal flow slides in the Dutch province of Zeeland. After a description of the phenomena, the soil characteristics and the investigations executed, an explanation of the relatively slow rate of the flow slides was given.

The slides chiefly occur in the older Holocene sand layers. These sands are fine and uniform of size, their density on the whole being below the critical density. The primary cause of the slides is the effect of tidal streams on the shore causing a steeper slope than the original one and seepage pressures during falling water. A local loss of equilibrium may lead to an even further spreading flow slide.

Fig. 1

A flow slide is a gradual process where at intervals of a few minutes soil masses slide downward and flow out. Observation is only possible above the water line, where a steep slope is formed. Then at a place one or more metres landwards cracks appear after which the soil mass in front starts to slide. In this way the slides go on progressing about 50 m per hour. The duration of the complete process varies from a few hours to a day.

May I now draw your attention to two articles on slides where these phenomena have taken place for the greatest part in dry sand-layers above the ground water line, although the primary cause of the slides must have been a loss of equilibrium below the water level. Here also the sands were fine, uniform in size and with a low density.

O. Linke describes the small ridges of sand-dunes and valleys on the isle of Norderney (Germany). The valleys, 20 to 60 m wide and some hundreds of m long, are flat, and capillary saturated with water. The dunes up to 4 m high were only humid. The valleys were rather hard but by the author alternately bringing his weight from one foot to the other gradually the surface of the valley came into an undulating situation and water appeared at the surface. The bearing capacity diminished so that the author could no longer stand on the surface, which settled over the whole area so that a pond arose. After a while slides in the nearest dunes appeared (Fig. 1).

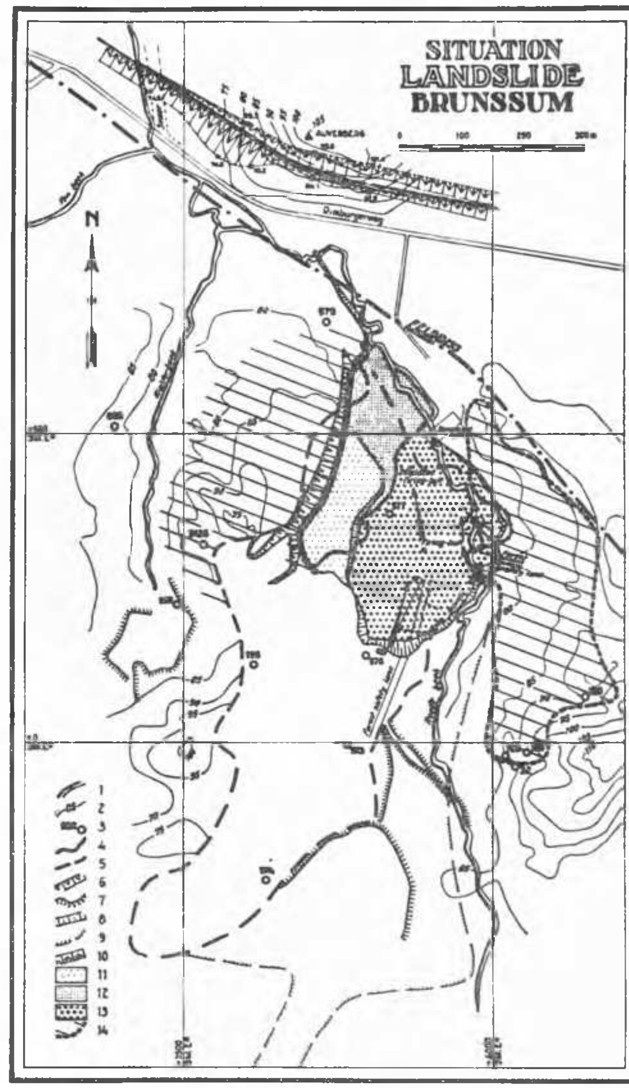


Fig. 2 Situation of landslide, Brunssum, Holland
Glissement de terrain à Brunssum, Pays Bas

A. A. Thiadens describes a flow slide in an old open brown coal pit in Brunssum, Holland, on 10 June 1955. This is shown in Figs. 2 and 3. Brown coal was excavated here in an open pit from 1917 to 1924: the sandy overburden was deposited in the emptied pit. In 1924 the workings were shut down and the ground water rose and formed a pond. The old pit was filled on the bottom with spilled coal, above it a layer of about 5 m of loose tipped sand under the ground water level and above that a body of dry loose sand partly wooded. In May and

June 1955 but chiefly in the period 7 to 10 June there was a heavy rainfall, there being 75 mm in the last three days. The

Fig. 3 Displaced fire lane with tilted steps
Déplacement d'un passage débrisé montrant un effet de gradins en oblique

ultimate cause of this flow slide most probably was this exceptional rainfall.

References

LINKE, O (1955). Ein bemerkensweiter Grundbuch, *Natur u. Volk*, **85**, Heft 6, 165

THIADENS, A. A. (1956). De grondverschuiving in Brunssum. *Tijdschr. ned. aardrijksk. Genoot.*, **73**, No. 1

R. V. WHITMAN (U.S.A.)

I should like to make a few remarks regarding the first question proposed by the General Reporter, that is: the implications of eliminating the term called 'cohesion' from stability computations. I appreciated the General Reporter's analogy between the situation at the present time regarding cohesion and that a number of years ago regarding the effect of pore pressures, and I believe, even more so after attending this conference and talking with many people here, we are on the threshold of understanding much more clearly the nature and importance of this cohesion. I would urge that all soil engineers bear with this question for a few more years, and I think that we may find, in this situation as in the case of the dilemma regarding pore pressures, that we will arrive at a position where we can understand and use this component of shear strength.

K. Terzaghi, in his opening remarks to the conference, emphasized that the solution to an immediate practical problem, although it may not be based upon calculations or detailed consideration of all factors, is ultimately based upon the individual's fundamental understanding of the types of processes involved. Along this line I would prefer, for the moment, not to use the word 'cohesion' but to use the term 'colloidal phenomena,' because I believe that in an understanding of colloidal phenomena lies the answer to the question.

The colloidal contribution to shear strength is the result of a very complex and complicated process. Others have spoken on this matter already. I suggest, with the risk of oversimplification, that the extent to which there exists a colloidal contribution to shear strength is determined by three factors: (1) the magnitude of the intergranular stress; (2) time, since the water layer which is adsorbed on clay particles cannot be depressed or expanded quickly; and (3) the presence or absence of large net attractive forces between particles.

Now let us look at the General Reporter's question and see what, at the present time, we may state as a partial answer to that question. We would say that in many soils the strength contribution which we call cohesion is in some manner dependent upon the intergranular stress, and hence there is some basis for lumping together all contributions to shear resistance and saying that the shear resistance is some function of the intergranular stress at a point. Certainly this is an approximation, but it may be sound for some soils, such as preconsolidated soils which are given a great length of time to come into water content equilibrium. For soils where the time factor is short we must still retain in our expression for shear strength some term which is not dependent upon the intergranular stress at that time; and in soils where strong attractive forces exist between particles, such as in recent marine clays, there will exist a shear resistance which will not change with time and which is not dependent upon the intergranular stress. The absence or presence of such 'cohesion' is perhaps associated with whether or not a clay will slake when immersed in water.

As a final conclusion, let me suggest once again that during the next few years we should consider this question, not in terms of the statement 'is the cohesion zero?', but in terms of understanding the nature of this colloidal resistance and how the conditions which exist at a given time may affect the degree that this resistance will be present.

A. J. DA COSTA NUNES (Brazil)

Monsieur le président, messieurs, je voudrais discuter trois points du rapport général du P. C. Rutledge.

Je crains que la conclusion présentée aujourd'hui par le rapporteur général sur la limitation de l'utilisation des pieux aux terrains de base pulvérulents est peut-être trop générale. Je voudrais à ce propos mentionner ce qu'a dit A. Cummings, ancien président du comité américain de mécanique des sols à la Purdue Conference:

(Pile Foundations—Proceedings of the Purdue Conference on Soil Mechanics and its Applications, 1940.)

In connection with this remoulding theory, it is well to remember that over the world there are thousands of structures which are being supported satisfactorily by piles driven into deep beds of saturated clay. How these structures would have behaved if no piles had been used is largely a matter of conjecture.

Dans la discussion de l'article de N. B. HOBBS (3b/8) on lit dans le rapport que 'the remedy is, of course, to cast the piles in shells of sufficient strength to maintain the original open hole and to support fully the concrete of the pile'.

Je pense que l'effet est provoqué par la force ascensionnelle de la vibration employée pour l'exécution du pieu et qu'on pourrait aussi éviter l'accident en damant le béton au mouton ou bien en le comprimant à l'air comprimé, ou encore, en faisant un trou au préalable dans le terrain.

Finalement, dans son appréciation de l'article (3b/5) P. C. Rutledge dit qu'il n'apparaît pas clairement en quoi le flambage est un problème.

Nous voudrions éclaircir le fait que, dans ce cas particulier, comme il est indiqué dans le titre même de l'article, il s'agit de pieux avec grande hauteur libre au-dessus du sol.

L'élancement du rapport entre la longueur libre et le rayon de giration atteint, ainsi qu'on peut le déduire du l'article, environ 130 pour un pieu de 50 cm de diamètre. Si on tient compte de la longueur dans le sol jusqu'au point de rotation on aura encore des rapports plus grands, donc une réduction radicale de la charge admissible.

Au-dessus de la cote d'érosion, le sol est très résistant. On a du sable assez compact, du sable argileux aussi assez compact et de l'argile sablonneuse dure. Le nombre de coups du carottier normalisé est d'environ 20 à 30, en moyenne et la résistance à la pénétration du cône hollandais est d'environ 80 à 150 kg/cm^2.

B. Fellenius (Sweden)

In south-west Sweden the Göta river flows from the large lake Vänern to the North Sea. On the banks of this river great landslides have occurred lately, namely at Surte and Göta.

The slide at Surte is described in *Proceedings* No. 5 from the Royal Swedish Geotechnical Institute published in 1952, and in *Proceedings Ser Ca* No. 27 from the Geological Survey of Sweden published in 1956. The slide occurred on the morning of 29 September 1950. The *Proceedings* of the Royal Swedish Geotechnical Institute was published before all investigations were completed, but that of the Geological Survey is more complete and the conclusions are based on all the results of geological and geotechnical investigations.

The slide at Göta occurred on 7 June 1957 at 11.25 a.m., and no details have yet been published. It began in the southern part of the slide area. Cracks were observed several hours previously; these grew wider and wider and the slide started, which caused a flood wave. The first small slide plugged the river in the south, and the flood wave 3 km downstream was 1·5 m high. The water which was raised by the following slides could not flow downstream, so there was a very big flood wave going upstream. Opposite the slide the rise of the water was 10 to 20 m and the flood wave in the upstream direction was 6 m high, 1 km from the slide. By the water-power station, Lilla Edet, 2 km upstream from the slide, there were two waves each 3 to 3·5 m over the ordinary water level.

It is possible that the start of the slide was caused by erosion of the river bank. The extent of the slide in the direction of the river can be indicated by the trough that passes after the high flood wave, but people who saw the slide did not observe a trough.

A. Kézdi (Hungary)

I should like to make some remarks on the factor of safety of slopes. In the case of a given slope we determine the related values of Φ and c, belonging to $\nu=1$, and construct the curve

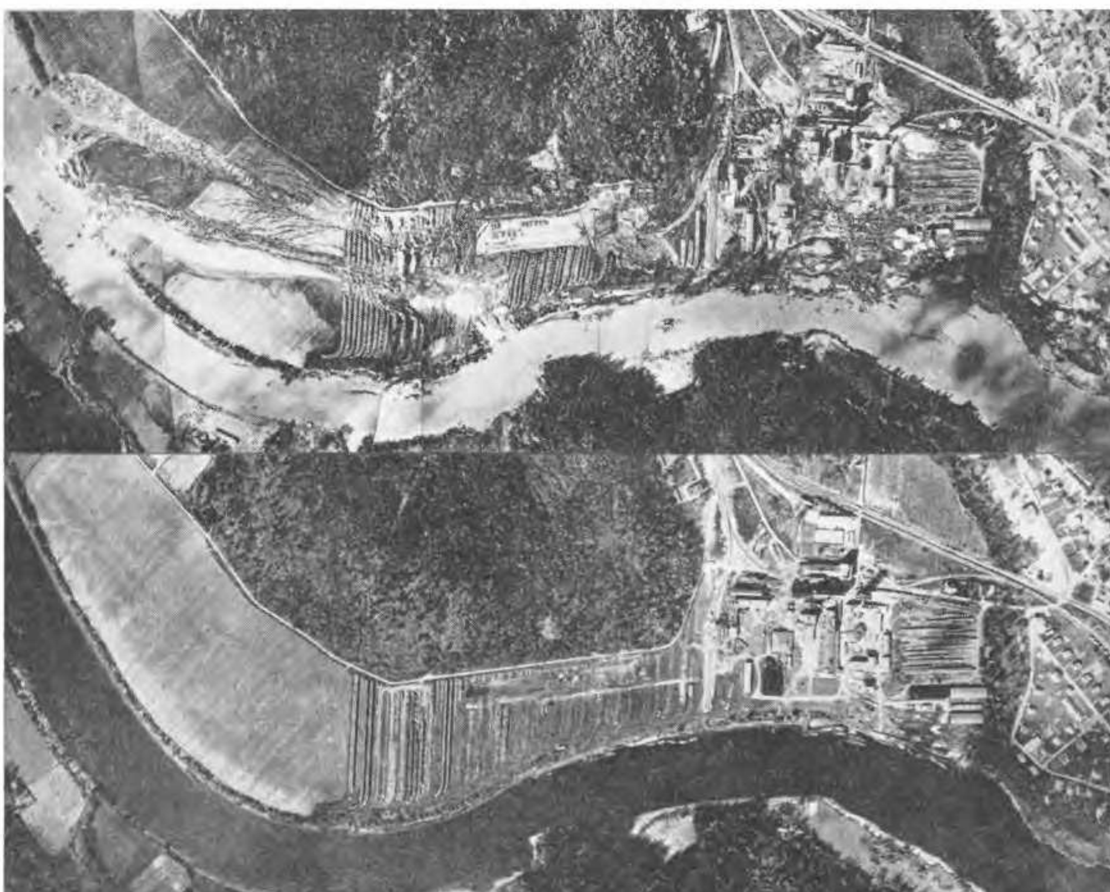


Fig. 4 The slide at Göta 7, 6, 1957. The upper picture, photographed 11, 6, 1957, showing the slide some days after it happened: the lower, photographed 1, 6, 1955, showing the same place before sliding. The stream flows from left to right; and the southern part of the slide area is to the right

Glissement de terrain à Göta, le 7, 6, 1957. La photo supérieure prise le 11, 6, 1957, montre l'état du terrain quelques jours après le glissement: la photo inférieure, prise le 1, 6, 1955, montre l'état du terrain avant le glissement. La rivière coule de gauche à droite et la partie sud du glissement est située vers la droite

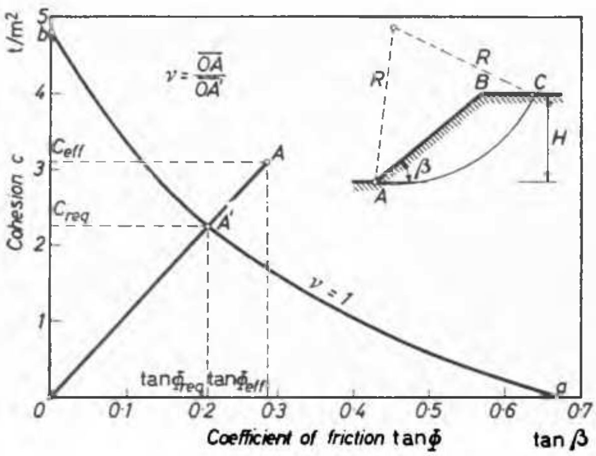


Fig. 5 Curve giving related values of c and $\tan \Phi$, belonging to the limiting state of equilibrium

Courbe montrant le rapport entre les valeurs c et $\tan \Phi$ pour la limite d'équilibre

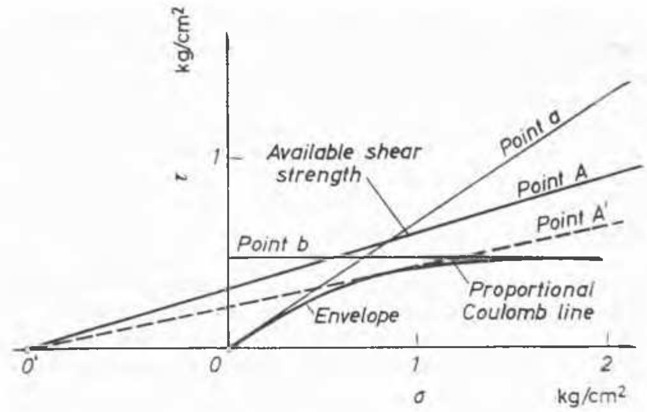


Fig. 6 Coulomb lines corresponding to the points of curve on Fig. 5

Lignes de Coulomb correspondant aux points de la courbe dans la Fig. 5

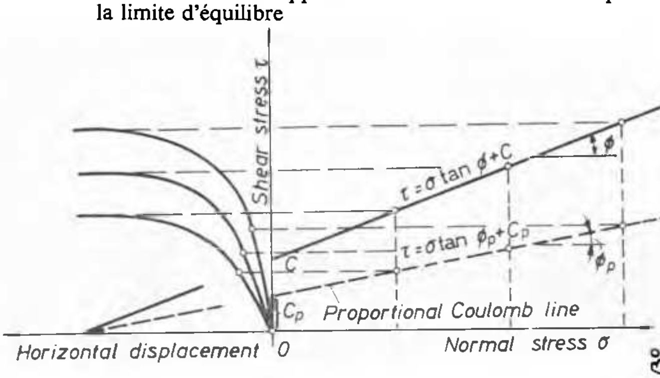


Fig. 7 Determination of the proportional Coulomb line

Détermination de la ligne de Coulomb proportionelle

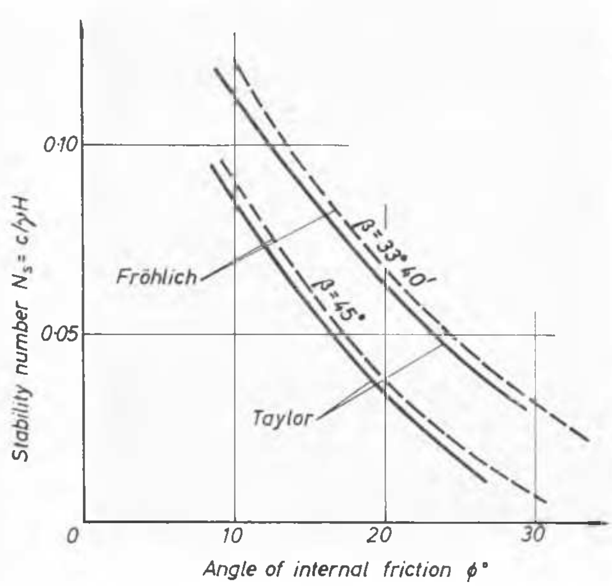


Fig. 8 Comparison of stability factors as determined by Fröhlich and Taylor

Comparaison des facteurs de stabilité déterminés par Fröhlich et Taylor

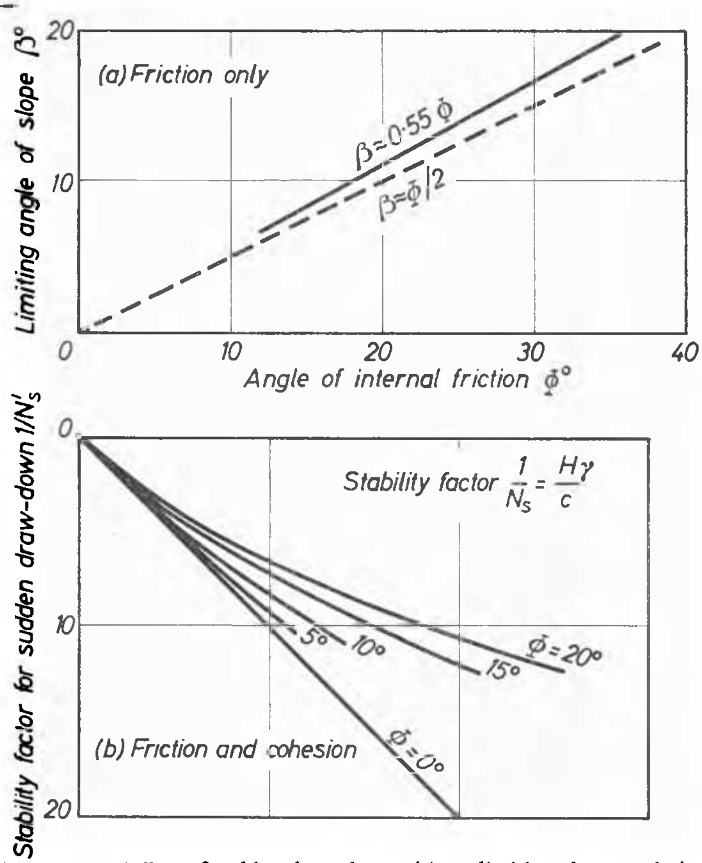


Fig. 9 Effect of sudden draw-down: (a) on limiting slope angle in the case of cohesionless soils; (b) on stability factor in the case of cohesive soils

Effet d'une chute de niveau soudaine: (a) sur l'angle d'inclinaison limite dans le cas de sols incohérents; (b) sur le facteur de stabilité dans le cas de sols cohérents shown in Fig. 5. The factor of safety can be determined from the line connecting the point O and point A belonging to the real values of cohesion and friction; this intersects the $\nu=1$ curve at point A'; the ratio of the two distances OA and OA' furnishes the factor of safety. This known idea can be expanded. With the aid of the curve $\nu=1$ all corresponding Coulomb lines can be traced in the system σ, τ and also the envelope of them (Fig. 6). The previously mentioned line OA gives in the system σ, τ a system of radiating lines with point O' as centre; the line corresponding to point A' is tangential to the envelope. The more distant the effective Coulomb line lies from the envelope the greater the safety. The minimum value of the ratio of corresponding shearing stresses furnishes the factor of safety. It is advisable that the proportional Coulomb line, determined according to Fig. 7, be tangential to the envelope; then, an elastic state of stress develops in the slope.

Fig. 8 compares the stability factor determined according to Taylor with that according to Fröhlich, as a function of the angle of internal friction, for two different slope angles. The differences are at a maximum, 8 per cent.

Fig. 9 serves to determine quickly the effect of sudden draw-down. Making use of Terzaghi's well-known method, the stability factor has been determined. With friction only, the determination of the limiting slope angle at sudden draw-down is very simple:

$$\beta \cong 0{\cdot}55\Phi$$

When there is cohesion, too, there are different curves for different Φ values. The greater the Φ, the greater the influence of sudden draw-down.

D. P. Krynine and R. J. Woodward (Presented by R. J. Woodward) (U.S.A.)

A proper way of writing the generalized Coulomb equation, stating the equality of the driving and resisting forces, depends on an adequate understanding of the factors controlling soil strength. For example, in contrast to the $\phi=0$ method, which omits friction as a component of soil strength, the General Reporter proposes as an item of discussion the essential elimination of cohesion as a factor in stability analysis. The General Reporter of Division 5 asks the conference to discuss if the $\phi=0$ method should give place to the C', ϕ', u method, and for long-term stability computations, even to the $C'=0$ method.

These suggestions of the two General Reporters refer, however, to homogeneous earth masses, through which both soil material and water are distributed uniformly. In such cases the shearing (or failure) surface is curved, as both observations and computations (e.g. by Frontard) show. Krynine and Woodward have observed many slides in natural slopes in California, however, in which sliding occurs on a practically plane shearing surface, which is roughly parallel to the slope of the ground surface; and have arrived at the conclusion that such shear failures are due primarily to geotechnical non-uniformity of the mass in which the failure has occurred. Such is the case when a weathered mantle covering a rock or stiff clay massif slides down. In many of these slides water from heavy or persistent rains percolates down until a completely saturated layer is formed at the interface of the weathered and sound materials. The ground water then starts to flow downhill with the formation of a temporary aquifer. The transformation from static to kinetic energy results in a build-up of body forces caused by the water flow, which adds to the gravity action.

Paper 6/27 by M. Vargas and E. Pichler depicts in part a situation very similar to that described above and observed by Krynine and Woodward in California. In the slide of Paper 6/27, which occurred in Brazil, there can be no question as to why the sliding surface is roughly plane rather than curved. It is more difficult to explain why plain shearing surfaces are found in more geologically homogeneous slopes, such as described in Paper 6/24 by A. W. Skempton and E. A. de Lory. It seems probable that during periods of drying out of a clay mass the upper layer near the surface, which may be called layer A, loses water and acquires strength because of desiccation. This effect gradually decreases with depth; under the desiccated material there is a fully saturated layer, which we may call layer B, and which has considerable strength, partly because of negative pore pressure (tension) near its upper boundary. Rain water may infiltrate through the fissures of the upper dry layer without seriously damaging it. The underlying saturated layer, however, at least near its upper boundary, may be seriously weakened, thus the upper layer A loses the support of layer B and becomes unstable.

We wish to call the attention of the conference to the general inadequacy of the term 'cohesion', which in some cases is misleading: this is especially true when a large soil layer tends to move with respect to the underlying mass, without resulting in a short time failure, as happens in the case of creep. Our mechanics is still a mechanics of points (remember the Mohr circle) whereas when dealing with a soil mass we need a mechanics of 'fields'. The term 'bond' may often depict the situation in these circumstances better than cohesion and friction. We use the term in our practice and were quite satisfied in finding it in Papers 6/11 by M. Goldstein and G. Ter-Stepanian and 1a/31 by Ú. Nascimento. It should also be added that in many cases the decrease of 'cohesion' after a period of time is simply the result of fatigue of the earth material under the action of reversible stresses which have gone through many cycles of reversal.

A. W. Skempton (U.K.)

I should like to comment on a topic which seems to have given rise to some confusion in this conference, namely, the question of whether the cohesion in clays is zero. The natural slopes in London clay, described in Paper 6/24 by E. A. DeLory and myself and Paper 6/12 by D. J. Henkel, show that in cuttings and slopes the cohesion intercept tends to zero with increasing time after the clay is exposed. This cohesion intercept is simply one of the terms in the Coulomb equation for shear strength. It is not the true cohesion, and it is due to over-consolidation. Normally consolidated clays have no cohesion intercept.

What we have shown, in effect, is that the effects of over-consolidation, as expressed by the cohesion intercept, are 'washed out' of the clay in the course of time: that does not mean that the true physical cohesion of the clay is destroyed. As defined by Hvorslev, for example, true cohesion is present in a normally consolidated clay, just as in an over-consolidated clay. What we are suggesting is that when an over-consolidated clay is exposed in a cutting or a slope, weathering and internal softening along fissures will ultimately reduce the clay, in effect, to a normally consolidated clay. That is, the cohesion intercept tends to zero with time in these cases.

I wish to emphasize, therefore, that in discussing the cohesion intercept we are not saying anything about true cohesion.

A. C. Meigh (U.K.)

I wish to present some field data concerning the short-term stability of steep excavation slopes in London clay, and discuss briefly the problem of estimation of the stability of such slopes. The points I shall make are relevant to Topic for Discussion No. 2, and also to the General Reporter's comments concerning the use of effective-stress shear-strength parameters in

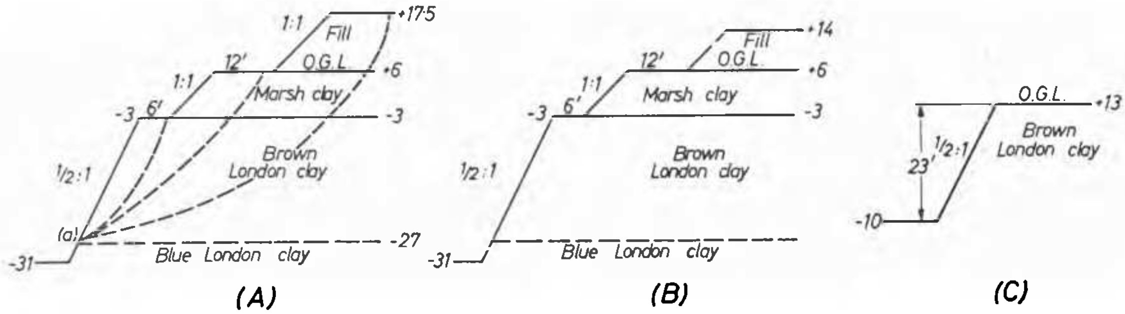


Fig. 10 London clay at a site in Essex. (*A*) slip after 1 day; (*B*) slip after 19 days; (*C*) still stable after 4 months
Argile, London clay, dans le comté d'Essex. (*A*) glissement après 1 jour; (*B*) glissement après 19 jours; (*C*) toujours stable après 4 mois

stability analyses in preference to the use of total stress parameters.

It is well known that the use of the undrained strength of an over-consolidated clay such as the London clay in the $\phi=0$ analysis will lead to calculated factors of safety which are greatly in excess of the true long-term factor of safety. It is also known, through the work of A. W. Skempton and D. J. Henkel, that long-term stability can be estimated on the basis of an effective-stress analysis, taking into account a progressive decrease in c, the effective cohesion intercept. However, as a preliminary to the establishment of the long-term condition there must be a re-distribution of the excess pore pressures, principally negative pore pressures, set up by the stress changes. For a rigorous estimate of short-term stability we would therefore require to know the transient conditions of pore pressure, and the problem is further complicated by the effects of opening of the fissures. The problem of short-term stability is in fact so complex that it is doubtful whether a satisfactory solution can be found on these lines. In cases where excavations are only required to be open for a few weeks, it is uneconomical to design for the equilibrium pore pressure conditions, using an effective-stress analysis, even if the full value of c' is used; we are forced to rely on stability analyses using the $\phi=0$ method and based on an estimate of the reduced shear strength.

Fig. 10 shows sections through three slopes of deep excavations in the London clay at a site in Essex. Slope *A*, which has a total height of 48·5 ft., failed within 1 day of completion of excavation. The first sign of failure was a bulging at the junction of the brown clay and the underlying blue clay, followed by a slip into the lower berm; 4 hours later the slip extended back into the marsh clay, and some 5 hours later, back into the fill. A stability analysis showed that the strength of the brown London clay necessary for equilibrium was 970 lb./sq. ft. compared with an average measured strength of about 1,700 lb./sq. ft.

The second slope (*B*), which has a total height of 45 ft., also failed, but after a period of 19 days. In this case the strength required for stability is 890 lb./sq. ft.

The third slope (*C*), which has a total height of only 23 ft., is still stable after some 4 months; the strength required for equilibrium is only 650 lb./sq. ft.

These results show that the reduction in shear strength with time, following excavation in the London clay, progresses at an unexpectedly fast rate in the early stages.

D. J. Henkel (U.K.)

I wish to give some details of the types of failure that are occurring on London clay slopes on the North Kent Coast where the toes of the slopes are being eroded by the sea.

In the large-scale circular arc type of failure, followed by the development of mud flows in hollows left by movement of a large mass of material, the shoulders left between adjacent slides are very steep but even in wet weather they remain very dry and stable. Water drains out of these shoulders into the hollows and the pore pressure in the shoulders is consequently very low. Nearly all the material removed from the slopes is carried away in the mud flows in the hollows. The average slope from the beach to the top of the slopes in the area where the mud flows are occurring is 1 in 3 and the overall height of the slopes about 200 ft.

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

One of the promising branches of soil mechanics is the investigation of deformation properties of soils during shear. The results of such investigations must serve as a basis for the computation of earth slope movements in the initial phase of a landslide process, or what we have called the phase of depth creep of slopes. The investigation of the depth creep of slopes can render an invaluable service in predicting the catastrophic phase of earth slope movement and in design of remedial works in the stage when the landslide correction is most effective.

In such cases a knowledge of the mechanism of a landslide based on the relationship between the intensity of the landslide producing factors and the rate of displacement can help in estimating the proper time and amount of correction work necessary. These relationships are very interesting from both the theoretical and practical points of view, showing the localization of the seat of the landslide, and its development and migration as well. Such data can serve as an important appendix to the results of field exploration, the significance of which must not be under-estimated.

During the last three decades many observations of landslide movements were made in different parts of the Soviet Union, and valuable data were collected. One of the results of this has been the development of a method of field observation. It is well known that the reference point method in many cases does not give satisfactory precision, and that the triangulation method consumes much time when efforts are made to increase the precision of the results.

We have developed the graphical differential method which permits many simultaneous readings by means of optical theodolites to be made from fixed stations on the same moving point on the landslide. Using this method on medium-sized landslides, approximately 300 m in width, we have obtained an accuracy of measurement ranging from 2 to 4 mm.

Observations made on a large landslide in oligocene clays in Transcaucasia permitted the maintenance of safe railway traffic for several years. In another case the catastrophic phase of the landslide in lower cretaceous clays on the Volga river was predicted; the slide actually happened several months afterwards.

We believe that landslide movement observations of this sort will be of the same value for the investigation of the landslide mechanism as settlement observations have been necessary for the understanding of the science of foundations.

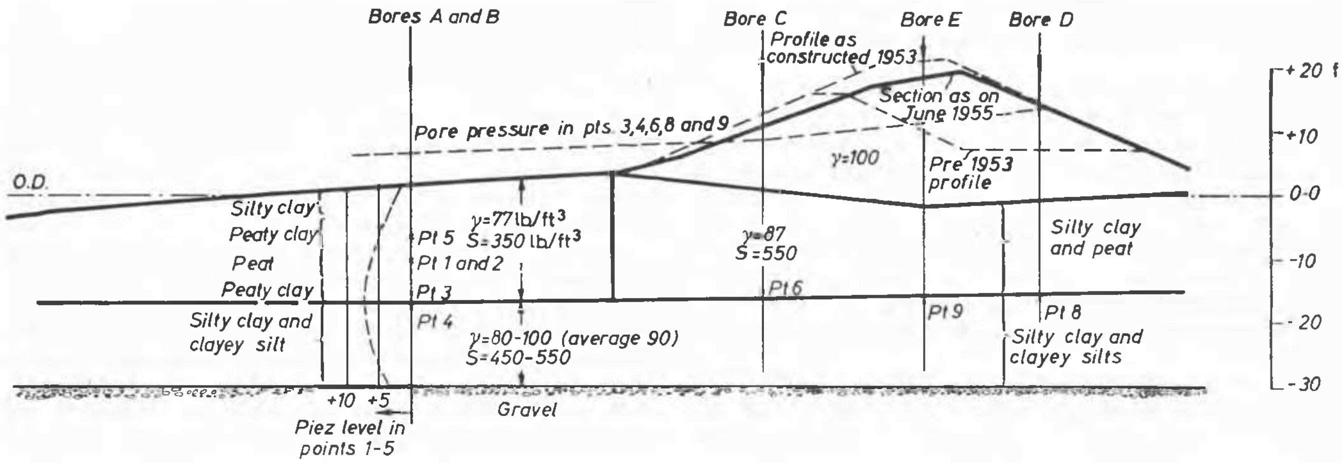


Fig. 11 Cross-section through the bank at a slip site
Schéma de la digue a l'endroit du glissement

A. Marsland (U.K.)

I should like to make a few comments in relation to Paper 6/19 by R. Peterson, N. L. Iverson and P. J. Rivard.

Since the catastrophic floods early in 1953 I have been studying the various aspects of flood bank failures. One of the problems is the stability of banks built on soft alluvial deposits; these consist of very soft organic silty clays interspersed with peat having a total thickness of 30 to 40 ft. and resting in turn on gravel or firm London clay. The LL of the clays is in the range of 90 to 150, the PL 30 to 70, and with moisture contents approaching the LL. Undisturbed unconfined shear strengths are in the range 350 to 550 lb./sq. ft. with fully remoulded strengths about one-quarter of these values.

Many slips occurred during the emergency reconstruction works due to the increased height of the rebuilt banks. Estimates of the factor of safety using unconfined compression tests and $\phi=0$ analyses were in the range of 0·9 to 1·1 for these constructional failures, which took the form of landward slips extending to the borrow pit. These failures were easily remedied by making a new borrow pit at a greater distance from the bank.

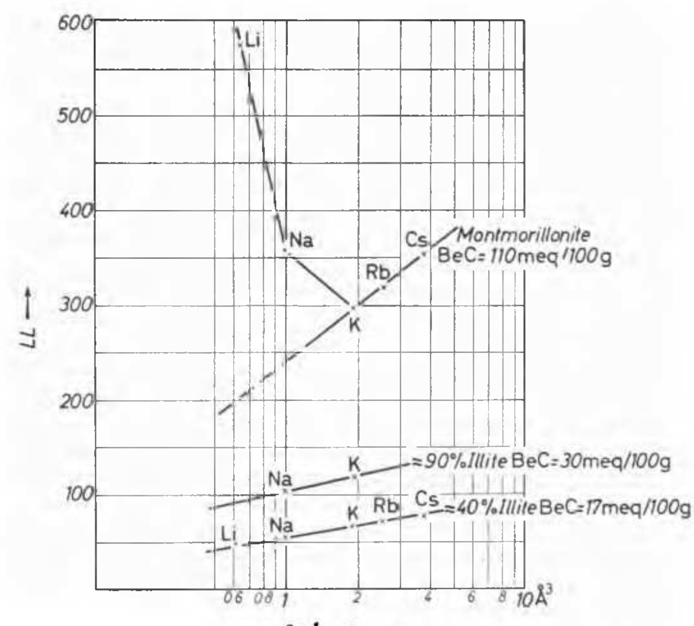
A much more serious problem was the occurrence of delayed slips on the river side of some of the banks along the River Thames. A cross-section through the bank at such a slip site is shown in Fig. 11, which also shows the positions of pore pressure points installed as the failure developed. In this case small tension cracks did not appear until 18 months after reconstruction was completed. From this stage level and distance observations were made on steel pegs spaced approximately 10 ft. apart over the cross-section. During the next 3 months the bank slipped at almost a steady rate, the crest dropping by more than 2 ft. and the 'toe' lifting about 7 in. There was a slight tendency for the rate of movement to increase during periods of higher tides. The factors of safety calculated using $\phi=0$ and effective stress analyses using the measured pore water pressures were of the order of 1·4 and 1·2 respectively. This difference between the $\phi=0$ and the effective stress analysis could possibly be explained by the re-distribution of pore pressure.

A significant factor in this type of failure is, however, in my opinion, the lack of a definite rupture at the toe. In the less delayed failures the heave at the toe was an appreciable proportion of the downward movement at the crest, and a definite rupture occurred, indicating a condition approximating to that used in circular slip analysis. In this case, however, the maximum upward movement at the toe was only about one-quarter of the total downward movement at the crest and the upheaval zone was virtually smooth. Is it not possible, therefore, to explain both the slow development of this failure and the excess of the calculated factors of safety by plastic deformations under partially mobilized shear forces particularly in the passive part of the slip?

These aspects only cover part of this interesting study, but I feel that the rate and extent of the strains developed are an important aspect in the problem of embankments on soft foundations. In consequence, I feel that the paper by Peterson *et al.* would be much more valuable if detailed notes were made of the time of the failure after construction and the rate of slipping, together with the factors of safety for each case.

I. Th. Rosenquist (Norway)

The question of the shear strength of soils in natural slopes was mentioned by the General Reporter, who referred to confusion in the discussion of the concept of cohesion. R. V.



$\alpha = \frac{\varepsilon - 1}{\varepsilon + 2} R^3 \longrightarrow$

Fig. 12

Whitman in this connection referred to the colloidal properties. S. Matsuo has presented Paper 6/15 on the effect of cation exchange on the stability of slopes, and from this it may be inferred that the influence of the cation exchange is an important one.

For about 15 years we have worked on the concept of cation exchange and the cations absorbed on clay minerals, and I should like to show some results as to the LL of two clays depending on their cations. From these results, it is shown in Fig. 12 that base exchange in soils has an influence upon the LL, but this depends also upon the nature of the mineral, and as long as nothing is stated as to the mineralogical conditions in Matsuo's paper there is difficulty in finding out what the changes mentioned by him mean. Matsuo has however certainly shown that base exchange has an influence upon the shear strength.

All the results lead us to assume that the LL of a soil depends solely upon the following three factors: (1) the amount and nature of the minerals; (2) the degree of electrochemical saturation; and (3) polarizability of the adsorbed ions. The shear strength of the soil depends upon factors 1, 2, 3—the same factors as are influencing the LL—but also upon the stress condition and stress history, the water content, and the diagenetic cementation between the minerals.

A. L. LITTLE (U.K.)

I want to speak on Paper 6/3 by M. BAR-SHANY, G. KORLATH and J. G. ZEITLEN which describes the use of fat clay in a dam construction in Israel. My firm is confronted with the possibility that it may have to use a fat clay to build a dam about 90 ft. high in England. This material has a LL of 65 per cent approximately, and that compares reasonably well with the clay described. I was very encouraged to find that in Israel they had been bold enough to use clays with LL from 71 to 95 per cent, and, moreover, they had built a dam 24 m high, that is, about 75 ft. high, and they had even contemplated raising it eventually to about 100 ft. I am therefore interested in comparing the behaviour of the dams which have been built in Israel with fat clay with two very small dams which my firm has completed in this country.

The two dams are about the same height as the one at Beit Netufa, that is, about 11 m high. For both dams we used clay with LL ranging from 65 to 88 per cent, but there was incorporated in the downstream slope some material with a LL as low as 43 per cent. Although the dams were at different sites, with clays of quite different geological ages, the two clays were both stiff, fissured clays, heavily over-consolidated, resembling one another in their general properties.

We measured the pore water pressure in the dams during construction and afterwards with the B.R.S. adaption of the United States Bureau of Reclamation type piezometer tips and we made observations that agree quite well with those made on the Israel dams. We also observed negative pore water pressure ranging down to about -4 lb./sq. in., although some of the pore water pressures were a little higher than that and some of them even came into the positive range, although they were still very low. There does seem to be, over a period of about 18 months, some tendency for the pressures to increase slightly, but they remain very low indeed.

I presume that the negative pore water pressures are associated with the swelling of the clay, as the authors of the paper say. Obviously, if such swelling is taking place, it must be accompanied by a serious drop in strength, and it is necessary to take this drop in strength into account in the design. For the stability analysis we worked on the $\phi = 0$ basis and ensured that the dam would remain stable even though the cohesion should drop to as low as 400 lb./sq. ft. It is not necessary to consider a drop below this value because with the very long period required for the cohesion to drop to zero, the useful life of the dam will be terminated from other considerations at a much earlier date.

I was also interested in the authors' remarks on drainage. They point out that rock toes or drainage ditches are not really very satisfactory for dams of this type. On the other hand, in the two dams which I am describing, fairly extensive blanket underdrains were placed beneath the downstream slopes and they appear to be quite satisfactory. There is usually a small but measurable discharge from the drains.

In the case of compaction, our experience favours rubber-tyred rollers. On both dams we had rubber-tyred equipment—in one case the construction equipment, and in the other case a special compacting roller; it was always possible to obtain 95 per cent of the Proctor maximum dry density without undue effort. A cut had to be made through one of these banks in connection with the installation of the pore water pressure apparatus, and this revealed a dense homogeneous mass, and there seemed to be no visual evidence of stratification. From this and from density tests we concluded that the method of compaction was quite satisfactory.

We found that a sheepsfoot roller tended to get balled up, and on one site the contractor removed the feet from the sheepsfoot roller in order to make it into a smooth roller.

The moisture content in the borrow pit was about right for placing but during transporting and placing there was a tendency for the fill to dry out, which reduced the moisture content by about 1 per cent. In hot dry weather it was necessary to do some watering. It was found, in fact, that there was a slight surface drying out if the bank was left for a long time, and it was then necessary to scrape off perhaps 2 or 3 in. before the next layer could be placed.

O. DE SCHNAKENBOURG (France)

Monsieur le président, mesdames, messieurs, me référant à l'avis du Rapporteur Général concernant le manque de publication touchant les matériaux graveleux, je souhaite vous communiquer quelques résultats acquis lors de l'étude et pendant la première année de construction du barrage de Serre-Ponçon sur la Durance.

Il s'agit d'un noyau central assis sur une coupure étanche sous-alluviale et enserré entre deux recharges (shells) à peu près symétriques. Les matériaux du noyau proviennent de sources différentes, mais sont tous constitués de marnes décomposées contenant de 10 à 60 pour cent de squelettes pierreux de 5–150 mm de diamètre, d'ailleurs plus ou moins friables, les fines du mortier inférieures à 5 mm sont faiblement plastiques, avec un index de plasticité se situant entre 8 et 16 pour cent.

Les matériaux des recharges sont des alluvions cristallo-calcaires modernes, plus ou moins graduées, possédant de 60 à 80 pour cent de squelette arrondi supérieurs à 5 mm et atteignant 2 et 300 mm de diamètre. Le sable inférieur à 5 mm est soit cru (moins de 5 à 6 ù de fines limono-argileuses) soit moyennement riche de ces fines (25 à 30 pour cent) dont l'IP varie de 4 à 12 pour cent.

Il y a deux façon d'étudier de tels matériaux: ou bien ne soumettre aux essais que la fraction fine sable ou mortier, disons inférieurs à 5 mm de façon à utiliser le matériel courant de laboratoire et à attribuer ensuite aux matériaux complets les résultats acquis, ou bien considérer le matériaux graveleux lui-même, auquel cas il est indispensable de posséder des appareils d'essai de grandes dimensions, appropriésà une dimension raisonnable de gravier le plus gros.

Si l'on n'étudie que le mortier sur le sable, on peut se demander si les éprouvettes doivent être moulées en fonction de leur état dans les vides laissés par le squelette ou bien en fonction d'une courbe de compactage donnée par une énergie appropriée au matériau complet et résultant soit d'une digue d'essai, soit d'essais de compactage des pots de grandes dimensions.

Avec les mortiers des matériaux très graveleux, il est matériellement impossible de mouler les échantillons représentant leur état d'interstice. Les densités sont si faibles et les teneurs en eau si élevées que les éprouvettes n'ont aucune tenue et ne représentent en soi, aucune réalité et pourtant ces masses détrempées composent, avec le gravier des textures qui ont de hautes caractéristiques mécaniques.

Il faudrait donc utiliser des sable et des mortiers compactés de telle façon que les échantillons aient quelque chance de représenter le matériau complet. Or cela est aussi impossible car la densité sèche varie avec le diamètre du gravier et la teneur en squelette pierreux et on ne peut jamais obtenir ce que l'on désirerait pour représenter le matériau complet.

On a avancé qu'avec moins de 30 pour cent de squelette, le matériau graveleux se comporte comme son mortier ou son sable et qu'au dessus de 70 pour cent de squelette c'est celui-ci qui confère ses propres caractéristiques au matériau complet, mais on ne sait pratiquement pas: ni ce qui se passe entre les teneurs en squelette limite, ni même au delà et en deça de ces limites, qu'elle est l'influence réelle du squelette sur la compactabilité, la perméabilité, le tassement et la résistance au ciseillement d'un matériau graveleux.

Après quelques tâtonnements, l'Electricité de France a préféré exécuter tous les essais dans des appareils de 400 m de diamètre, afin d'y utiliser, sans effet de paroi, du gravier de 63 mm. Nous avons donc conçu et réalisé divers appareils dont: 6 pots de consolidation d'une hauteur de 400 mm, capables d'une pression maximum de 32 kg/cm^2 et un appareil triaxial pour échantillons de 1 m de hauteur dans lequel on peut appliquer les pressions maxima suivantes: $\sigma_3 = 12$ kg/cm^2 et $\sigma 1 = 80$ kg/cm^2.

Lors de l'exécution, en 1953, d'une digue d'essai nous avions couvert toutes les dispersions d'état des divers matériaux dans les ballantières à l'aide d'échantillons préfabriqués arrêtés aux grains de 5–63–100 et 200 mm. Ces échantillons, ainsi que les échantillons tout venants, nous ont permis de tracer les courbes pratiques: pour les alluvions $D_s = f$ (pour cent en sable et pour cent en fines limono argileuses) pour les matériaux cohésifs: $D_s = f$ (pour cent en eau et pour cent en squelette).

Les échantillons de sable et de mortier de 5 mm nous ont permis de reproduire au laboratoire l'énergie pratique; les échantillons à grains de 63 mm nous ont donné les valeurs initiales des envois dans les gros appareils; enfin, ceux de 100 et 200 mm ont servi à déterminer les bases de contrôle des compactages.

On s'est alors rendu compte que l'énergie de compactage dite de laboratoire variait, en fait, avec la dimension du grain graveleux, que, quelque soit le soin qu'on ait pris pour reproduire au laboratoire les courbes $d_s = f$ (pour cent en eau) sur les sables et les mortiers obtenus sur la digue d'essai, l'allure des courbes état différente et qu'on pouvait choisir une énergie entre 2 ou 3 valeurs toutes aussi valables les unes que les autres. Quoiqu'il en soit, les essais au laboratoire ont été exécutés sur des matériaux de 63 mm et le sable ou le mortier d'interstice nous intérassait bien moins.

Depuis 1953 nous avons pu vérifier sur d'autres types de matériaux plus plastiques même, deux autres faits très intéressants: (1) sur les alluvions perméables ou semi perméables, les valeurs les plus fortes sont fournies par un matériau complet dont le sable contient environ 15 pour cent de fines limono-argileuses qui ont un index de plasticité de 8 à 10 pour cent. De telles alluvions sont plus résistantes et se tassent légérement moins que les alluvions à sable franchement cru. (2) Sur les matériaux cohésifs réservés au noyau, on a constaté que le mortier d'interstice est nettement mieux compacté quand il y a moins de 30 pour cent de squelette dans le matériau complet que quand il est seul compacté. Le rapport

$$\frac{d_s}{d_s} = \frac{\text{densité du mortier d'interstice}}{\text{densité du mortier compacté seul}} \text{ est égal à}$$

104 pour cent pour 10 pour cent de squelette
102 pour cent pour 20 pour cent de squelette
100 pour cent pour 30 pour cent de squelette

ensuite, c'est la dégringolade et on a 92 pour cent pour 50 pour cent de squelette.

Simultanément on note pourtant une augmentation de la résistance au cisaillement et une diminution des tassements sous charge; les plus mauvais résultats sont fournis par les matériaux à densité d'_s, base normale d'évaluation de la compactabilité des matériaux graveleux quand on ne travail au laboratoire que sur le mortier.

Il est dangereux par conséquent d'imaginer que le contrôle des compactages de sols graveleux puisse être fait sur le mortier compacté avec l'énergie standard ou avec celle résultant d'une digue d'essai.

Il n'y a aucune difficulté avec les alluvions crues; le tassement est classique, immédiat et peu important.

Avec les alluvions plus riches en limon-argile, et, encore plus avec les graviers à mortier cohésif, on note des effondrements continuels qui donnent des courbes $\Delta h = f$ (pressions) à 2 ou 3 points anguleux.

Ces courbes montrent, par exemple, que la consolidation procède par paliers, dont certains commencent plusieurs dizaines d'heures après le chargement. Nous avons pu, notamment noter un certain week-end compliqué d'une fête et d'un pont, qu'au bout de 96 heures un matériau, placé, il est vrai, à 1–2 pour cent du coté humide, qui avait 50 pour cent de squelette pierreux, s'était tassé pour la troisième fois et que la valeur de Δh était plus forte que lors du deuxième tassement qui s'était produit, jecrois, 28 heures avant, sous une charge de 16 kg/cm^2. Cela est certainement dû à l'effondrement des voûtes et à l'accommodement du mortier.

Pour finir, je mentionnerai que nous aussi, nous enregistrons des faibles déformations (3 à 6 pour cent) lors du cisaillement d'alluvions; nous notons des cohésions d'imbriquage des grains de l'ordre de 1 à 1·5 kg/cm^2, même dans les alluvions crues. De plus, je tiens à préciser que la dimension des éprouvettes permet de mieux se rapprocher de la réalité dans les essais de cisaillement triaxial et probablement de mieux répartir les contraintes. Je pense que c'est la raison pour laquelle nous obtenons sur les sables, toutes choses égales par ailleurs des valeurs ϕ toujours supérieures (de 1 à 3 pour cent) à celles que nous trouvons dans un triaxial de diametre 70 mm. Le squelette lui même ajoute, par ailleurs, 1 à 3 pour cent. Merci.

A. Casagrande (U.S.A.)

Evidence is increasing that under certain conditions the impervious cores of dams, even plastic clay cores, can develop transverse cracks, and that the usual thin filters offer insufficient protection when the supporting sections consist of highly pervious materials, so that cracks may lead to serious damage to the core and in extreme cases to piping failure. A number of observations on the development of cracking and on cases of piping that almost led to failure have not been published but are receiving increasing attention by the designers of earth dams. There is a growing awareness of the seriousness of this problem and a realization that the design procedures as developed by soil mechanics offer under certain conditions entirely insufficient protection against cracks, and that more effective design against cracks is needed.

Probably the most frequent cause of transverse cracks is the combination of tension and shear forces resulting from differential settlements. The most dangerous combination seems to be a high dam with steep abutments and a thin core. However, dangerous transverse cracks have developed also in relatively low dams as well as in essentially homogeneous dams. Among other factors that have contributed to such difficulties are: (1) the juncture of concrete and earth sections; (2) steep transverse slopes for closure sections; (3) irregular foundation compressibility; and (4) variations in the stress–strain properties of successive layers in the core in which case the cracks appear only in the stiffer layers.

The application of the water load may widen such cracks and the water pressure inside the cracks may assist in keeping them open.

It is important to recognize that thin filters do not assure sufficient protection against cracks: in fact, cracks through the core may be propagated through such filters. Only a substantial thickness of well-graded material, not a filter but a wide transition section between the core and a highly pervious downstream section, will assure positive protection against piping failure through the dam.

A typical zoned dam, such as is the favoured design by the Bureau of Reclamation, does provide such protection. However, in the U.S.A. the dam sites which assure a supply of all the materials needed for such zoned dams are getting rare, and we are now frequently faced with the problem of building dams at sites where we may have excellent highly pervious materials and good core materials but no suitable transition materials. Whereas the presently accepted design methods will permit us to design with such materials dams that are theoretically safe, they may nevertheless be potential candidates for piping through the structure.

In my considered opinion it is not realistic for an engineer to assume that his design assures that no transverse cracks will develop in the core. He would be much wiser to assume the following viewpoint: 'I believe that I have designed a dam which will not develop any cracks and I am proud of my design: but now I am willing to swallow my pride, and I will assume that the core will crack due to unknown causes, let us say sabotage by nature, e.g. an earthquake, and I ask myself: Is my dam so designed and constructed that, beyond any human doubt, the cracks cannot result in piping failure? If the answer to this question is No, then I must provide a second line of defence.' This defence will consist primarily of a wide transition section of well-graded materials. Other changes that will be helpful are widening of a thin core, use of compacted rock fill in lieu of dumped rock fill, and arching the dam axis upstream. Arching will assure that the water load will introduce compressive forces which will counteract the tension forces, and thus tend to close open cracks. In the case of essentially homogeneous dams it will be necessary to provide complete internal drainage by means of a vertical or inclined drainage layer ('chimney drain') which discharges into a horizontal drainage blanket or a drainage gallery.

F. A. Sharman (U.K.)

Some observations on a trial clay embankment, built to guide the design and specification of the impermeable layer of a large rock-fill dam, have a bearing on the questions discussed in Papers 6/8 by A. W. Bishop and 6/18 by E. Nonveiller.

The bank was 23 ft. high and the clay was sandwiched between two layers of filter sand, sloped at 1 to 1, and kept drained for 18 months. One set of piezometer points was put in 5 ft. from the bottom, and Fig. 13 shows the pressures recorded at 6 such points in the drained, post-construction period. As can be seen, there was a significant rise 2 to 3 months after completion, in spite of the fact that no load was added and no water accumulated in the filters. Weekly rainfall totals are shown in block form, but the correlation with pressure changes is not clear.

The second set of points was at a level 10 ft. above the first set, i.e. 8 ft. below the top. They behaved as shown in Fig. 14. Even larger pressure increases, with no corresponding load change, were recorded with some of these; but it was also observed that pressure differences up to 6 ft. of water, between points only 3 ft. 6 in. apart, were sustained for months on end.

These observations seem to suggest that for a drained clay placed and remaining within a few per cent of optimum moisture content, with no connection to a defined water table, and with boundaries wetting and drying in a tropical climate, pore water does not form a continuous body throughout the mass but may join up in discrete continuous clouds which give appreciable positive pressures at some points. Outside the clouds the

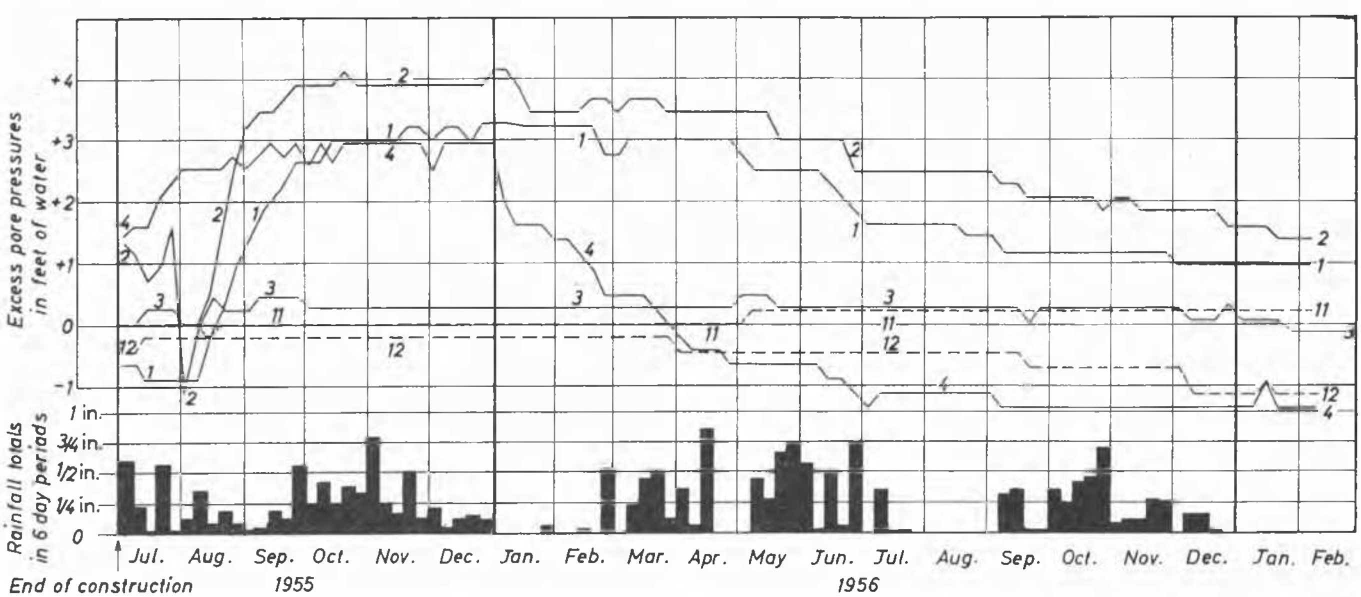


Fig. 13 Pore pressure variation at 6 points (Nos. 1 to 4, 11 and 12) 18 ft. below the top of a drained sloping clay layer 12 ft. thick
Variations des pressions interstitielles à 6 points (Nos. 1 à 4, 11 et 12) à une distance de 5·5 m en dessous de la surface d'une couche d'argile inclinée et drainée, d'un épaisseur de 3·7 m

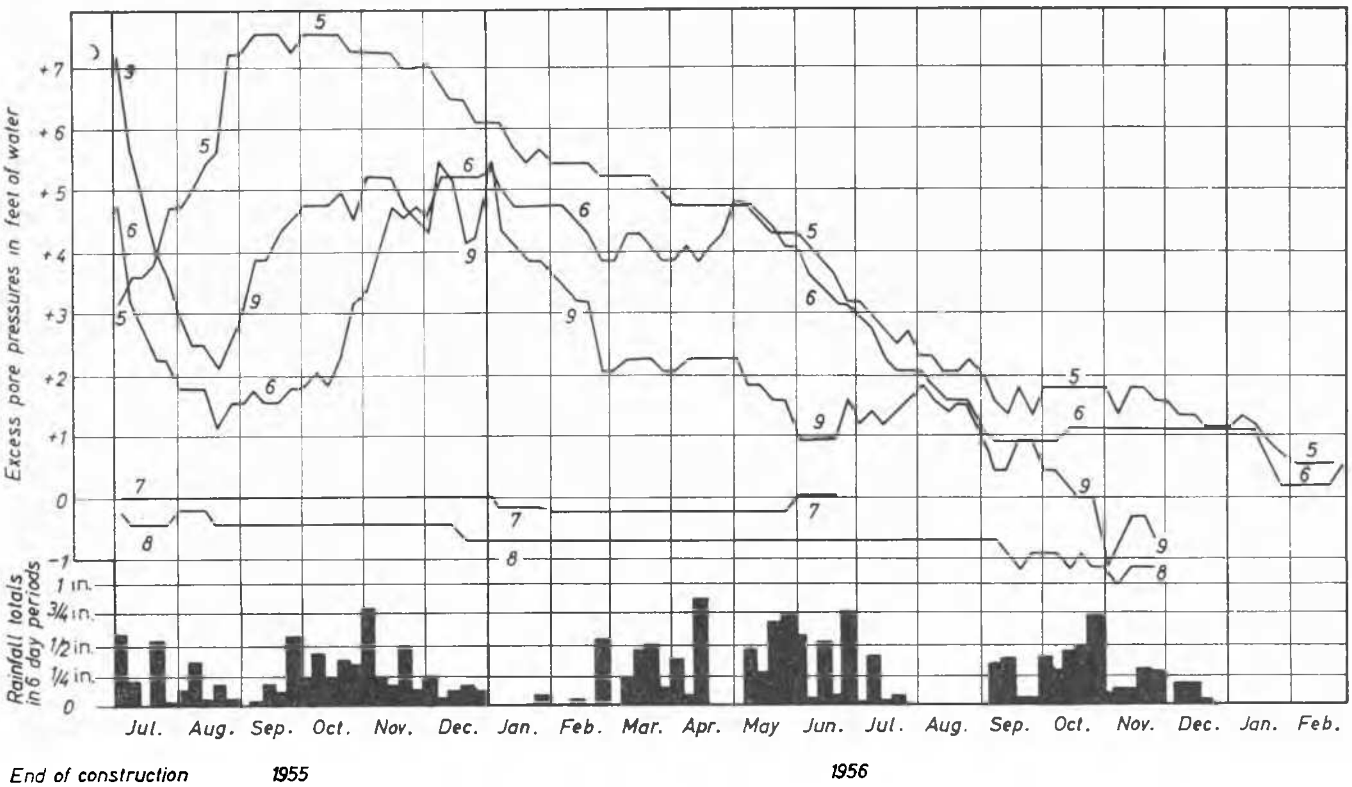


Fig. 14 Pore pressures at 5 points (Nos. 5 to 9) 8 ft. below the top of a drained sloping clay layer 12 ft. thick
Variation des pressions interstitielles à 5 points (Nos. 5 à 9) à une distance de 2·4 m en dessous de la surface d'une couch d'argile inclinée et drainée, d'une épaisseur de 3·7 m

drier parts of the mass may show zero or negative pore pressure.

More important excess pore pressures affecting stability may of course develop as a result of extra load and water level changes. For the clay concerned in the trial embankment it did not prove possible to establish consistent values of any of the Bishop parameters A B or $\bar{B}$ by laboratory tests. This may have been due to faulty technique or variable material, but it is relevant that the problem did not submit to the application of normal facilities and considerable effort. The pore water predictions from theoretical flow nets at Lokvarka were therefore read with much interest. The values of $\bar{B}$ deduced are rather surprising at first sight, but this seems to be due to the author's method of calculation rather than to anything extraordinary in the observations themselves.

Figs. 6 and 7 of Paper 6/18 show how necessary it is, in considering a slip surface passing through a clay with a complex pore pressure pattern, to assess the intergranular, neutral and excess pressures separately for each slice. Where large intergranular pressure changes have occurred, however, it would not seem possible to draw the patterns without some assessment of parameters depending on the compressibility of the material.

The General Reporter expresses confidence in our ability to design safe embankments in any kind of soil. No doubt this conclusion is correct, but the uncertainties about the shear strength of clay, which he summarizes so well, leave room for considerable advance in, for instance, the economical design of a rock-fill dam with a clay blanket.

A. Casagrande's remarks on the dangers of cracking of thin clay cores provoke the reflection that as far as rock-fill dams are concerned, the designer is obliged to make a judgment of the impermeable layer distortions which may be caused by rock-fill settlement. I understand that rock-fill properties are deemed to be outside the scope of this conference, but I think it is worth remarking that although dumped and sluiced rock-fill will certainly go on settling for long periods by amounts sufficient to cause considerable stresses in layers resting on it, it has proved possible to produce a rolled rock-fill which did not settle measurably during or after the placing of the upstream impermeable facing.

B. Löfquist (Sweden)

My contribution is related to the same subject as A. Casagrande has already spoken about, i.e. cracking in earth dams.

In the first instance I refer to Paper 6/25, by D. H. Trollope, dealing with an arching theory for a triangular fill. The theory is applied to stability but is of interest also in other respects.

The author describes the stress distribution in the base of a triangular fill of granular, cohesionless material. By settlements in the foundation, due to the fill, the vertical pressure decreases in the central part of the fill and increases in the outer parts. The case where the vertical pressure in the middle portion is zero is called the *full arching case*.

There is much evidence from the field which proves that such arching effects often occur. In his Fig. 6 the author shows some examples of pressure measurements in dams. In one of them—lower right—the vertical pressure in the core is only about 30 per cent of the weight of the overburden. However, the author assumes that the *full arching case* cannot be fully attained in practice. This may be true for granular, non-cohesive material but, in my opinion, not true as far as soils in general are concerned. In my own experience there is one earth dam at which it was quite evident from settlement observations that we had the *full arching case* in the core. I have also been informed of observations on other dams which indicate full arching condition.

Arching may take place in the longitudinal direction of the dam as well as in the transverse direction. I think many of you will remember a photograph in a report to the Zurich Conference: it was from a dam failure in Canada—a complete tunnel was formed through the dam from the upstream side to the downstream side. This may obviously be an example of a full arching case.

The full arching case, even when it occurs only for a short time, represents of course a serious condition for a dam as it may give rise to horizontal cracks or loose zones. However, the case where the vertical pressure decreases to a value lower than the water pressure at the same level may also be considered as detrimental to the water tightness and performance of an earth core.

In this connection I will also refer to Paper 6/2 by G. Baroncini and A. Croce, on the performance of the Arvo dam in Italy. This dam has been in use for 25 years and a new boring has shown that the core material in the lower part of the core has still a higher water content and does not yet seem to be consolidated to any measurable degree. The authors explain the condition by the hypothesis that the slow consolidation is due to a very low permeability, but no value of the permeability is shown. I think the behaviour of the Arvo dam can also be explained by considering the arching effect as outlined by D. H. Trollope.

In a report to the conference on Large Dams in New Delhi, 1951, I have shown, as a result of pressure measurements in dams, that the earth pressure in thin cores due to arching effects can be computed in the same way as pressure in silos.

The author of the paper on the Arvo dam also reports that the settlement of the crest in various points along the dam is independent of the height of the dam, and that the core material placed in the structure below a plane 13 m under the crest has not contributed appreciably to the total settlement. This is an extremely interesting observation.

Before I close I would like to point out that arching and cracking phenomena in general are the most important remaining problems in earthwork design. We have in principal solved important questions on stability, filter requirements and so on. Much work has also been carried out in measuring and predicting the amount of settlement under various conditions but we do not know much about which settlement or differential settlement can be safe and which can be unsafe.

A. W. Bishop (U.K.)

I wish to refer to two points relating to the measurement of pore pressure.

The General Reporter mentions an interesting doctorate thesis by J. W. Hilf. Unfortunately this is not available to most members of the conference so I will not refer to it in detail, but I will draw attention to one important conclusion which occurs in it. J. W. Hilf shows that the conventional laboratory apparatus for measuring pore pressure may fail to give realistic results in the case of compacted clayey soils, particularly if placed on the dry side of the optimum water content. This is most marked if the clay fraction is high. It is due to the inability of the porous element of the piezometer to differentiate between the pore pressure in the water phase and the pore pressure in the gaseous phase present in partly saturated soils.

As a result of the more accurate measurements which J. W. Hilf has carried out it is apparent that in soils of this type a considerable error in pore pressure measurement can occur, up to one atmosphere or more in magnitude. The use of the correct values will displace the strength envelope sideways on the Mohr diagram, and will in general lead to a reduction in the value of the intercept denoting effective cohesion.

J. W. Hilf did not carry his work far enough to determine the actual effect of this error on strength measurements made in the triaxial apparatus, but he inferred that it might account for a considerable part of the cohesion intercept conventionally measured in apparatus of this type. He went a little further, in fact, and suggested that true cohesion is negligible in unsaturated soils. The General Reporter has perhaps been less cautious than J. W. Hilf in his remarks about cohesion; for, if we look into the other work to which he refers, particularly that of M. J. Hvorslev and of later workers who have examined the differentiation between friction and cohesion, we find results difficult to reconcile with the view that cohesion in fine-grained soils can be explained away in terms of surface tension.

However, it is quite definite that capillary tension is responsible for a considerable component of the conventionally measured cohesion intercept in clayey soils compacted on the dry side of the optimum water content. This means that we must be more careful in choosing the type of piezometer element when dealing with materials placed under these conditions. As you have probably gathered from the rainfall during this conference, it is of less practical importance to us in this country than possibly to the U.S. Bureau of Reclamation, but as an academic problem it is extremely important to have the position clarified.

J. W. Hilf's conclusions point, of course, to a serious limitation of much of the field piezometer equipment now in use. The grain size of the material forming the porous elements in this equipment is such that it can only withstand a difference between the water and air pressure in the soil of a few lb./sq. in. I would like to ask the General Reporter whether the Bureau of Reclamation is undertaking any reassessment of the value of using conventional piezometer tips in clay type soils placed under rather dry compaction conditions.

There is a further point to which I wish to refer in relation to the cohesion intercept measured in standard tests. A large intercept is often measured at moisture contents below the optimum, but even if this were representative of the character of the soil as placed, the most critical condition in many cases is long-term stability or rapid draw-down when saturation, either partial or complete, has been able to occur. We find under these conditions, if they are reproduced in the laboratory, that the cohesion intercept drops very rapidly with a rise in water content even though the volume may remain sensibly constant. In the case of soil compacted on the dry side of the optimum water content the value of the cohesion intercept may drop to one-half or one-third of its initial value, and it is this value, rather than that obtained in the conventional undrained test, which should then be used in design.

In a series of tests recently performed at Imperial College the saturation was carried out at an average pore pressure of about 90 lb./sq. in., a value likely to be encountered under field seepage conditions (in contrast to the more common technique of saturation at atmospheric pressure). For a soil of relatively low clay fraction compacted at a water content 2 per cent below the optimum the values of c' and ϕ' (the apparent cohesion and angle of shearing resistance in terms of effective stress) were 9 lb./sq. in. and $33\frac{1}{2}$ degrees for the conventional undrained test, and 0 and $32\frac{1}{2}$ degrees for the saturated sample respectively. This drop in the cohesion intercept is an important fact which must be taken into account in dealing with stability problems in partly saturated soils.

The last point which I should like to raise briefly is that of pore pressure predictions. One or two pessimistic statements have been made about this point already. I think that the difficulty arises not so much in soils in which the water content is above the optimum and the pore pressure is high, but in soils placed at or below the optimum water content, in which the pore pressure may initially be low, or even negative under low stresses. A previous speaker has given an example of this second category.

It would be interesting to know whether the Bureau of Reclamation, on the basis of the work it has carried out, is adopting this rather pessimistic attitude and is basing its designs on intuition rather than on quantitative measurement, invoking observed pore pressures if difficulties arise during construction rather than using them as a basis for design in the preliminary stages.

A. Mayer (France)

Le très intéressant rapport (6/16) du G. Meardi sur la consolidation d'argiles molles dans la fondation d'une digue au moyen de drains en sable m'a donné l'idée de dire ici quelques mots d'une étude sur modèle réduit qui a été faite sur ma demande par P. Habib au Laboratoire du C.E.B.T.P. pour étudier un problème analogue. Il s'agissait de construire en bordure de mer, sur un terrain recouvert chaque jour par une marée de 4 m une digue de 8 m de haut destinée à protéger les terrains riverains et ultérieurement à permettre leur utilisation.

Le sol, à cet endroit, est constitué jusqu'à plus de 20 m de profondeur par une vase molle, à frottement pratiquement nul en l'état naturel, à cohésion par endroits de 50 g/cm^2. C'est-à-dire qu'un homme ne peut se tenir debout sur ce terrain sans enfonçer. L'exécution d'une digue de 8 m sur une pareille fondation, avec des marées de 4 m, paraissait impossible. L'objet de la recherche a été d'essayer de déterminer si on pourrait, en consolidant le terrain au moyens de drains en sable, arriver à construire un cavalier en enrochements de 4 m de haut, à l'abri duquel on pourrait déposer des matériaux dragués sans risque de les voir emportés par la marée.

Nous avons cherché à étudier le problème au moyen d'un modèle réduit. Dans l'impossibilité pratique où nous étions de nous procurer une quantité importante de vase intacte, il nous a paru que le terrain naturel pourrait être représenté par une graisse consistante, relativement molle, à frottement nul et cohésion de l'ordre de 20 g. Un grand récipient a été rempli de cette graisse; des drains en sable y ont été implantés, en enfonçant dans la graisse des tubes que l'on remplissait de sable et que l'on retirait par la suite. Des essais de chargement superficiel ont été effectués d'abord sur la graisse seule, ensuite dans la zone où avaient été implantés les drains. Il est bien évident que les pieux ainsi implantés n'avaient aucun effet drainant. L'essai devait donc donner une force portante très inférieure à celle susceptible d'être obtenue en place du fait de la consolidation du terrain.

Il a montré que:

(1) Les drains devaient avoir une dimension suffisante, tant en diamètre qu'en longueur pour mettre en jeu la totalité de leur capacité de résistance. Sinon la rupture avait lieu à l'intérieur même du sable du drain. La tête du drain s'écrasait sans permettre d'obtenir la résistance maxima possible. Un calcul simple permet d'ailleurs de donner cette limite.

(2) Une fois ceci réalité, on a constaté que la drain ajoutait bien à la résistance du sol en place une résistance égale au produit de la surface latérale par la cohésion. C'était ce que le calcul indiquait; l'expérience l'a confirmé de façon très satisfaisante.

Il y a lieu de rappeler que cette valeur est un minimum puisque la cohésion de la graisse reste invariable, tandis que le sol subirait une certaine consolidation, difficile à évaluer d'ailleurs en raison de l'action des marées.

On a ainsi trouvé que pour une longueur de 10 m chaque drain augmentait de 8 fois environ la résistance du sol à l'endroit où il était implanté.

On a ainsi pu calculer le nombre de drains nécessaires pour tenir une levée en enrochements derrière laquelle on viendrait déverser des matériaux dragués. Bien entendu il n'est possible de donner ici qu'une idée générale du projet. Un système de clapets au bas de la digue en enrochements doit permettre d'évacuer l'eau apportée par les produits de dragage, tandis que le choix des matériaux doit permettre d'avoir une étanchéité suffisante.

Il nous a paru que quelques mots sur cette étude faite dans un cas particulier très voisin de celui exposé par le G. Meardi complèteraient utilement son rapport.

The Chairman

I now have pleasure in calling on the last speaker at this session, and I think it is unique in the history of our science that the speaker on whom I now call is a lady—Mrs. Troitskaya, of the U.S.S.R.

Mrs. M. Troitskaya (U.S.S.R.)

In a number of papers presented at this conference mention has been made of the changes in the mechanical properties of soils with depth. As a rule it has been assumed that with the increase in depth both the density and the strength increase. Although such conditions are to be found in many cases, very often the opposite can be observed when the density and strength of the soil do not increase under the self-weight of the soil.

This is mainly the case where there is a deep strata of clay which can lose its water only to a limited extent owing to the fact that the pressure gradient produced by its own weight is small. Samples of the same soil when tested in the laboratory, however, show large gradients due to the small size of samples, and hence the compressibility may be found to be quite high.

It should be borne in mind that the compression of soils in natural as well as laboratory conditions is always limited, and the compression curve, which is usually assumed to be logarithmic, has in fact a horizontal and a vertical asymptote corresponding to the limit of compaction. This limit in its turn determines the presence of the limit of strength, and in particular resistance to shear, which it is necessary to consider in all calculations of stability of slopes, and especially of natural slopes where the loads can be very great.

General Reporter

I will say only a few words in summing up the session. I am very sorry that so many people who wanted to make contributions to this session were unable to do so, since we had to eliminate some names; I feel sure that their contributions would have been just as interesting as those that we have heard, but to have heard all of them would have kept us here another hour and a half at least.

I have only one other comment to make. A. W. Bishop asked a question as to what procedures my organization proposed to follow. We have no set procedures in the Bureau of Reclamation. As our knowledge of soil mechanics changes, we constantly change our practices. For some time we followed the practice of dry embankment construction but I am at present engaged in directing work on two dams only about 50 miles apart, one will be built with a very wet embankment and the other a moderately dry embankment because the local conditions dictate that that is the only proper way of handling it. I have been in the situation time and time again where, having told somebody that this was the way we did something, on the next job I did it in exactly the opposite way.

We have still a few minutes left this morning and I will leave them to K. Terzaghi.

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

Our General Reporter has provided us with a clear picture of the present trend of thought in two important fields of soil mechanics: the design of earth dams and the evaluation of the degree of stability of slopes on natural ground.

During the past few decades methods have been worked out for the design of earth dams on a rational basis. As a result of these developments earth dam failures became very infrequent in spite of the fact that the height of the dams increased. As late as 1901 the Board of Consultants for the New York Water Supply considered it unsafe to build earth dams with a height of more than 70 ft. Yet during the last decade earth dams have been successfully built with a height up to almost 400 ft. However, in order to apply rational methods to the design of dams, the subsoil conditions must be reliably known. Dam sites with dubious foundation conditions were avoided.

At the present time most of the desirable dam sites have already been utilized and more and more often we are compelled to construct earth dams at sites which were considered hopeless 20 or 30 years ago. Many of these sites have the feature in common that it is impracticable to determine the essential characteristics of the subsoil in advance of construction. In this connection a brief account of some of my experiences with dams at such sites may be of interest.

One of the dams with which I am at present connected is located in the high Sierra Nevada. The site of the dam was invaded by the Pleistocene ice sheet at least twice. The bedrock is located at depths up to 250 ft. below the valley floor. The sediments covering the bedrock consist of two layers of stony, locally rather pervious till, separated from each other by glaciofluvial sediments containing lenses of silt. The upper till sheet supports a set of crescent-shaped terminal moraines separated from each other by troughs lined with slope wash. The dam site intersects three of the moraines, and one abutment rests on a very pervious kame deposit.

The dam site was explored by means of 14 borings up to a depth of 250 ft. and two test shafts with a depth of 30 ft. A great number of soil tests were performed and, in addition, a competent geologist spent two working seasons at the site, engaged in an attempt to unravel the pattern of stratification of the subsoil. Yet, at the time when construction started, it was still impossible to make a reliable estimate of the quantity of water which would escape from the reservoir through the subsoil of the dam. The forecast of a loss of 15 cu. ft./sec was no more than a wild guess. The location of the lines along which water may escape out of the reservoir was also unknown.

During construction sketches were made of all the geological details which could be seen on the slopes of fresh cuts. Yet even these left a wide margin for interpretation. Therefore conclusive information concerning the loss of water and the hydrostatic pressure conditions in the subsoil could be obtained only by direct observation during the first filling of the reservoir. In order to get that information, 70 observation wells were installed, with their lower ends located at different elevations. One group of wells was drilled within the area occupied by the dam, a second one along the downstream toe and a third one on the terrain downstream from the dam. As the pattern of drainage emerged from the observational data, supplementary wells were drilled at strategic points. On the basis of the results of the piezometric readings, the stability computations were revised. The quantity of water which entered the toe drain was measured at two points by means of automatic gauges. However, while the water level in the reservoir was rising three springs came out of the slopes downstream from the dam. The sites of the springs were covered with inverted filters and gauging devices were installed to keep a continuous record of the discharge of the springs. At full reservoir the total loss of water due to seepage amounted to about 6 cu. ft./sec or less than one-half of the estimated amount. About 4 cu. ft./sec came out of the toe drain and the balance out of the slopes downstream from the dam. Owing to the observations which were made during construction, the completed dam is as safe and satisfactory as if it had been built on a foundation the properties of which were fully known in advance of construction.

Entirely different were the uncertainties and difficulties encountered at a dam site in central British Columbia. The site is located in the proximity of the upper end of a narrow canyon which was carved by the river out of granitic rocks. About 100 years ago a rockslide of vast magnitude descended into the valley from an elevation of about 2,000 ft. above the valley floor. The slide buried the forest which covered the valley floor over a length of about five miles beneath a layer of slide material with a thickness up to 100 ft. The slide material consisted of a random mixture of silt, sand, gravel and well-rounded basalt and andesite boulders with diameters up to several feet. Judging from the texture of the slide deposit, the slide material had the consistency of liquid concrete during its downward movement and the deposition was associated with local segregation according to particle size.

The deposit of slide material is separated from the original ground by a continuous layer consisting of a mixture of sand, gravel, shrubs and broken trees with a diameter up to several feet. Upstream from the area covered by the slide the river forms a shallow lake, the surface of which is located about 30 ft. below the lowest part of the original surface of the deposit of slide material. The project involved the construction of a dam with a maximum height of about 100 ft., for the purpose of raising the water level in the lake and diverting the water through a large diameter rock tunnel six miles long to a power station located on the floor of an adjacent valley.

Preliminary subsoil exploration involving the excavation of several test shafts showed that the slide deposit contained pockets and veins of rather pervious material, probably formed by segregation during deposition, and that the permeability of the contact layer containing the remnants of the buried forest was locally very high. No construction materials other than the slide material and talus rock were available for building the dam; the average permeability of the slide material being that of a silty sand. However, since the owners had no objections to severe losses of water due to seepage out of the reservoir, I declared the project to be feasible, provided the danger of piping through the layer containing the buried forest was eliminated by adequate provisions. However it was impracticable to locate the danger spots in advance of construction. Subsequent borrow pit surveys showed that the slide material exposed in the bulldozer cuts was so wet that the bulldozers bogged down in some of the cuts, and the local climatic conditions eliminated the possibility of reducing the water content by exposure and evaporation.

In order to take full advantage of the available construction materials I provided the dam with a rock toe with a height somewhat smaller than that of the dam. The rock toe serves as a lateral support for an upstream portion made out of slide material, to be placed at natural water content. A stability computation for the upstream slope could not be made because the fill material had to be placed at the water content at which it was encountered. Therefore I assigned to the upstream surface of the dam the conservative slope of 4:1, with the intention of flattening the slope during construction if it started to bulge. In order to avoid excessive loss of water through the contact layer between river sediments and slide material I sealed the outcrops of this layer to a distance of several hundred feet upstream from the upstream toe.

At the present time the dam is practically completed. The upstream slope remained stable and the leakage through the porous contact layer is smaller than anticipated. The water which comes out of the slide material within the area covered by the rock-fill passes through an inverted filter and enters a gauging box at the downstream toe of the dam, equipped with an automatic gauging device. The springs coming out of the ground downstream from the dam are also under control. Therefore the completed structure is satisfactory in every respect although the basis for the original design was rather shaky, to say the least.

Another topic of this session was the stability of slopes on natural ground. Those of you who have had little or no personal experience with landslides may be optimistic enough to believe that the evaluation of the factor of safety of such slopes is essentially a matter of sampling, testing and computing. However, such optimism is not justified. In 1950 I made an attempt to define and to classify the factors responsible for slope failures on natural ground. The findings were published in the Engineering Geology (Berkey) Volume of the Geological Society of America. If you read that paper you will realize that in many instances it is impracticable to determine the factor of safety of a slope with respect to sliding in advance of the occurrence of a slope failure. The best we can hope for under such conditions is to find out whether the slope is safe or of doubtful stability. The transformation of a slope with doubtful stability into a safe one can be a very expensive operation and we cannot know in advance whether or not the investment is justified: therefore, in many cases of this kind, sound engineering involves taking a chance. If such a decision is reached, sound engineering also requires the installation of all those means of observation which are necessary to recognize the imminence of a slope failure and to isolate the area before the failure occurs. After the failure we have to re-establish stable conditions at minimum expense. When facing such a situation we cannot help realizing that every bit of the time we spent on the study of soil mechanics was well invested in spite of the fact that we were unable to compute the factor of safety of the slope with respect to sliding in advance of the occurrence of failure.

B. Aisenstein (Israel)

Je m'excuse auprès du Rapporteur Général de revenir à l'article 6/1 auquel j'ai participé, mais j'ai pensé intéressant de complèter cet article en résumant quelques uns des essais qui se font en Israël, et qui consistent à recouvrir une roche calcaire perméable avec de l'argile alluviale grasse, à montmorillonite, afin de déterminer les conditions les plus favourables, tant techniques qu'économiques de l'étanchement par recouvrement d'un réservoir à fond rocheux perméable.

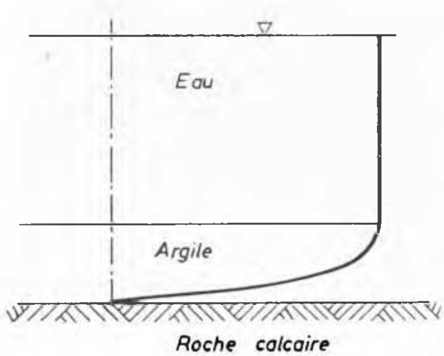


Fig. 15 Diagramme des charges
Load diagram

Les essais se font sur une surface en pente douce, de 2000 m^2 que l'on a entourée d'une digue en terre de hauteur maxima de 5 m constituant ainsi un réservoir que l'on a rempli d'eau. Après que cette surface ait été recouverte de 50 cm d'argile compactée, les pertes par infiltrations, de 2 m par jour avant le traîtement, tombèrent à 5 mm par jour. Ce chiffre, quoique relativement bas, était encore élevé et s'accrut subitement après quelques mois jusqu'à 50 mm par jour. Le réservoir vidé, on aperçut alors, à l'endroit le plus profond, un trou cylindrique vertical de 15–20 cm diamètre qui traversait la couverture. On avait là un cas typique d'érosion interne ou 'piping' dans l'argile. En examinant de plus près le phénomène, qui se répéta d'ailleurs, on remarque que, non seulement à la base de l'argile on avait laissé quelques cailloux, mais que la perte de charge, mesurée par des cellules piézométriques laissées dans la couverture était — selon le diagramme Fig. 15 — faible dans la partie supérieure se la couverture et grande dans la partie inférieure. En même temps on savait que le gonflement de l'argile, mesuré à diverses profondeurs, avait été très rapide et, naturellement, plus grand en haut qu'en bas. Le phénomène d'érosion interne s'explique donc simplement par la présence d'un matériau grossier au dessous le d'argile et par la création d'un gradient hydraulique très élevé dans la partie inférieure de la couverture.

Dans un deuxième essai on a nettoyé complètement la roche, colmaté les fissures et mis une couverture d'un mètre d'épaisseur, se reservant la possibilité de diminuer l'épaisseur plus tard. On n'a pas mis de filtre, ce qui aurait été trop cher. Dans cet essai, qui se poursuit déjà près d'un an le phénomène d'érosion n'a pas réapparu et les pertes par infiltration sont d'environ 1–2 mm par jour. Quoiqu'il en soit, il semble bien du point de vue pratique qu'une couverture d'argile grasse, pour être efficace, doit être posée sur un terrain convenablement préparé, et doit être elle-même recouverte d'une surcharge qui sert aussi de protection.

Remarquons que des phénomènes d'érosion semblables ont été observés dans des réservoirs plus on moins grands au travers du silt apporté par les torrents.

Z. Bažant (Czechoslovakia)

Vibration of soil is to be recognized as the cause of failure in many cases. Unfortunately soil which is affected by vibration makes problems very difficult, and so the solutions of vibration are scarce.

Even more complicated is the combined effect of vibration and seepage in cohesionless soils. Collapse of stability at combined vibration and seepage is sometimes desirable, as at vibroflotation (S. Steuerman and G. J. Murphy, Paper 3a/39), sheet-pile driving in loose saturated sands (D. D. Barkan, Paper 3b/1) or vibration of open caissons, mentioned by T. E. Mao in discussion of Division 3b. Sometimes the collapse of stability is undesirable, as in the following cases: (1) piping beneath overflow weirs, (2) collapse of breakwaters founded on sand, (3) collapse of sandy beaches due to repeated shocks produced by waves, (4) the sudden liquefaction of saturated sandy embankments caused by the vibration of rolling trains, equipment machinery or blasting, (5) instantaneous loss of bearing capacity of saturated sand during an earthquake, (6) sinking of sheet-piles used as supports of pumping stations in cofferdams, and so on.

In Paper 6/4 I have tried to establish the necessary model laws and to find the influential factors by means of dimensional analysis for the case of piping, which is very important due to the large sums involved in the construction of weirs. However, the solution obtained needs the use of big models, the greatest scale of reduction being considered to be 1:10. Due to the large size of weirs it will be necessary to find some analytical procedure.

In the evaluation of my paper the Reporter mentioned he was not able to follow the approximate analytical solution which I proposed in the second half of my article. This was due to the fact that I have cited a paper which was not accessible to him, but an abstract, Paper 3a/22, by N. N. Maslov, may be of help to the readers.

A. A. BELES (Rumania)

Le traitement thermique du sol en vue d'en obtenir une consolidation, a été connu depuis longtemps, mais il n'a été que rarement utilisé, et les méthodes employées se sont relevées difficiles et d'un prix élevé. Récemment on a mis au point en Roumanie, une méthode remarquablement simple pour assécher et stabiliser les terrains d'argile et enrayer les glissements,

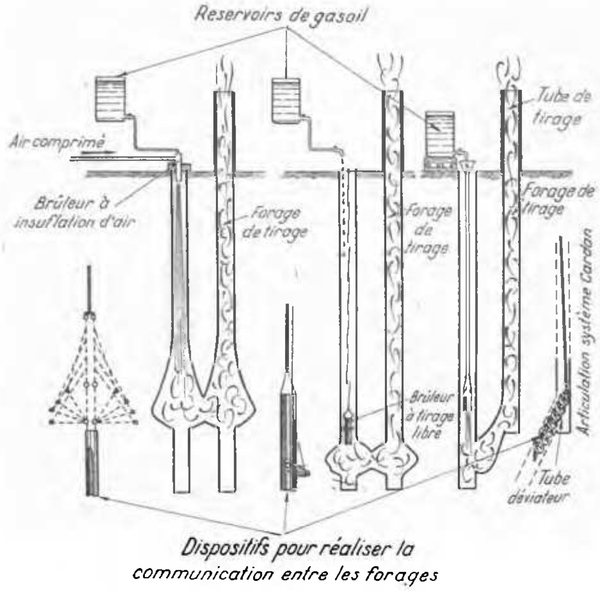


Fig. 16

méthode dont les résultats sont encourageants. J'exposerai la méthode, telle qu'elle a été mise au point par l'ingenieur Ion Stănculesco.

En principe, elle consiste à créer des piliers de terre brûlée, ayant une grande résistance au cisaillement. Pour obtenir un pilier, on exécute deux forages de 20 à 40 cm de diamètre, espacés de 80 à 120 cm, qui pénètrent sous la zone des glisse-

Fig. 17

ments. Les deux forages sont mis en communication à leur partie inférieure, au moyen de l'une des méthodes indiquées sur la Fig. 16.

On applique ensuite, à la surface du terrain, sur un des deux forages, un tube métallique du même diamètre et de 150 à 200 cm de hauteur. A l'aide d'un feu, ou avec une lampe à benzine on échauffe le tube métallique pour provoquer par tirage, un courant d'air qui entraine l'air du forage voisin.

Dès que le circuit d'air est bien établi, on introduit dans le second forage un dispositif formé par un tube métallique de 8 cm de diamètre et 80 cm de longueur, ayant soudé à environ 10 cm de l'extrémité supérieure un disque d'un diamètre égal à celui du forage (Fig. 17). Audessus du tube est placé un capuchon en tôle, de forme conique, fixé avec des barres permettant le passage de l'air. Le tout est suspendu à l'aide d'un

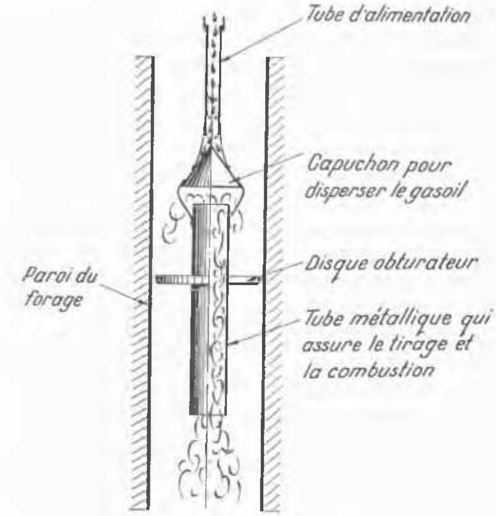


Fig. 18

tube qui sert pour l'alimentation de la combustion avec du gasoil (Fig. 18).

On commence part chauffer le capuchon à l'aide de cotton imbibé de gasoil que l'on place sur le disque. Une fois bien chauffé, on introduit le dispositif au fond du forage sans tube de tirage et on laisse tomber le gasoil, goutte à goutte, sur le capuchon chauffé.

Le gasoil prend feu et par suite du courant d'air crée par le

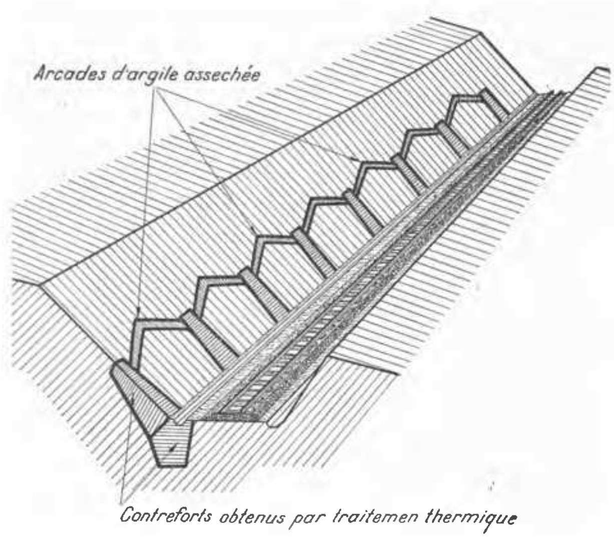


Fig. 19

tirage, la flamme est attirée dans le forage voisin et les gazes à hautes températures résultant de la combustion échauffent dans leur passage la paroi du forage de tirage et entrainent la vapeur dégagée du terrain. Par le déplacement du dispositif porteur de la flamme, on peut brûler aussi une partie de la paroi du forage qui n'a pas de tube de tirage.

Une fois la combustion du gasoil amorcée, elle se poursuit d'elle-même, pourvu que le gasoil arrive en quantité suffisante sur le capuchon en tôle métallique.

Pour obtenir de bons résultats, la température des gazes de combustion ne doit pas dépasser 800° C, ce que l'on obtient par le réglage de la quantité d'air admise entre le tube et le capuchon. La température est verifiée de temps en temps à l'aide d'un pyromètre. Le diamètre des piliers ainsi réalisés, peut ateindre environ 1·50 m.

Afin d'accélérer l'asséchement du terrain on exécute autour du groupe de forages pour le traitement thérmique, six à douze forages de 5–8 cm de diamètre espacés de 25–50 cm.

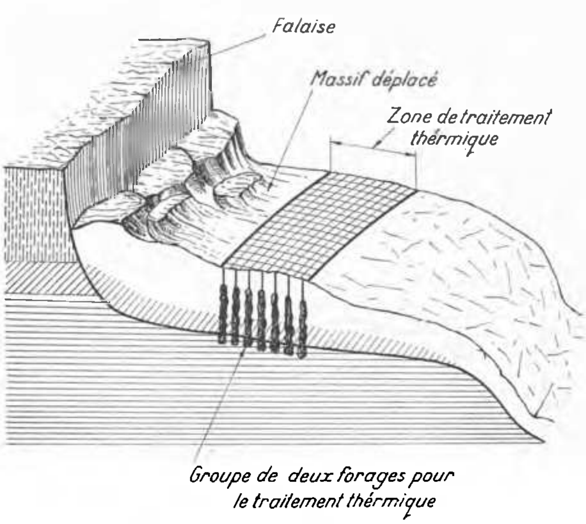


Fig. 20

Pour réaliser un groupe de forages de 8 m de profondeur, une équipe composée de 6 hommes avec une foreuse à main, a besoin d'une journée et demi de travail. La durée de la combustion doit être de 150 à 180 heures et la consommation de gasoil ateint 800 à 1,000 litres par groupe.

Le traitement thérmique, d'après la méthode exposée, peut être utilisé pour exécuter des contreforts reliés par des arcades sur le talus des déblais, afin de les stabiliser (Fig. 19). Etant donnée la fissuration des massifs d'argile due = à l'asséchement, on peut injecter du cut-back, et même un coulis de ciment pour imperméabiliser et consolider le terrain.

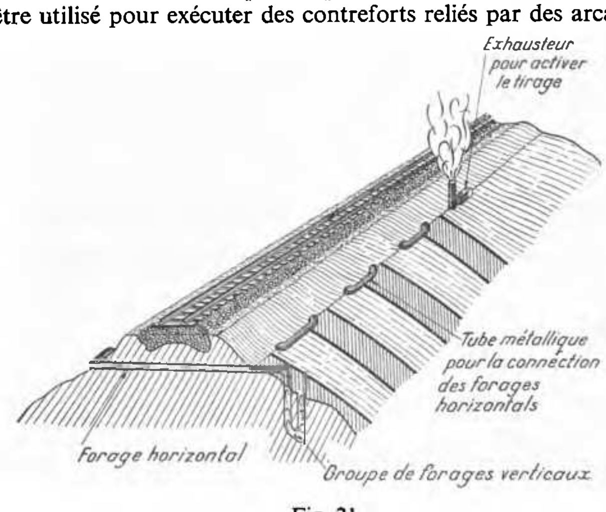


Fig. 21

A Constanţa, sur le litoral, on a réussi à stabiliser des glissements de la falaise. La disposition adoptée pour les forages est indiquée sur la Fig. 20.

Des remblais en argile peuvent être sechés et stabilisés en exécutant des forages verticaux et horizontaux connectés (Fig. 21).

Enfin, l'ingenieur Ion Stanculesco a réalisé le redressement d'un château d'eau fondé sur du loess et qui s'était incliné par suite de la rupture d'une conduite d'eau qui avait mouillé le loess. En appliquant le traitement thérmique à la zone imbibée, on a obtenu sa consolidation et, par l'humidification dirigée du loess du côté opposé, on a obtenu le redressement du château d'eau.

Par la simplicité et la rapidité d'éxécution, par l'outillage facile à réaliser, par son prix relativement reduit en comparaison aux autres méthodes et par les résultats favorables obtenus, le procedé mérite d'être signalé au congrès.

D. Krsmanović and R. Jovanović (Yugoslavia)

En raison des données intéressantes obtenues par les recherches sur le terrain et au laboratoire, nous désirons présenter en discussion quelques détails sur un glissement qui s'est formé dans une série alternante de grès, marne et schiste, de trias bas, très profondement remaniée et plissée par l'effet des forces tectoniques et affaiblie par l'action des eaux souterraines.

Par dessus le glissement, sur une longueur de 90 m, une ligne de chemin de fer est construite, qui traverse le glissement en partie en terre découverte, en partie par un tunnel de 30 m de long (voir situation et profil géologique, Figs. 22 et 23). Quelques mètres au dessous de la ligne de chemin de fer se trouve le bassin d'accumulation qui oscille entre 37·50 m de hauteur pendant l'activité de la centrale hydraulique et 47·00 m quand le bassin est entièrement vide.

Ayant constaté que le mouvement du terrain était parfois nul et que d'autres fois il durait assez longtemps, il a été possible d'établir, par des observations détaillées sur le terrain, que les mouvements étaient causés par deux effets différents, et que le tunnel se déplaçait soit par l'action du niveau exhaussé des eaux souterraines de la pente au dessus du lac (durant les précipitations atmosphériques ou les fontes de neiges), soit par l'effet de l'abaissement du niveau des eaux du lac.

Les observations du mouvement du tunnel pendant les deux dernières années et demie sont représentées par la Fig. 24. D'autre part les observations de l'affaissement du terrain sur le profil de surface transversal sont montrées à la Fig. 25, et à la Fig. 26 le trajet des differents points du profil sur la surface du terrain.

Grâce à ces observations et aux travaux de recherches, la profondeur de la surface de glissement a été trouvée sur plusieurs points du profil transversal et on a constaté qu'elle atteignait une profondeur d'environ 30 m (voir Fig. 27). A cause du très fort et épais plissement des couches et de l'affaiblissement de la série sédimentaire, on a pu par l'examen de la stabilité adopter une surface de glissement circulaire, quoique dans la nature il faut s'attendre à être écarté d'une telle surface idéalisée par suite de l'hétérogénéité du terrain (voir Fig. 25).

Les recherches géotechniques ont été effectuées dans une large mesure à cause de l'hétérogénéité des matériaux. Les détails au sujet du nombre des échantillons examinés sont montrès à la Fig. 28. Il est nécessaire de souligner que les recherches sur la résistance au cisaillement ont été faites dans les boites avec cisaillement direct, la consolidation ayant été effectuée préalablement. Cette manière de rechercher a démontré que la cohésion chez tous les matériaux etait égale à zéro et que l'angle de frottement interne avait fortement varié.

Les observations effectuées sur le terrain et les examens de stabilité de la pente pour différents cas (différents niveaux du bassin, pente saturée ou non saturée) — exécutés d'après la méthode élargie de Felenius-Taylor — ont donnée en rapport avec les actions des eaux de la pente (souterraines) les résultats suivants (voir Fig. 29):

(a) Si l'on adopte comme compétente la valeur moyenne de résistance au cisaillement avec angle de frottement interne

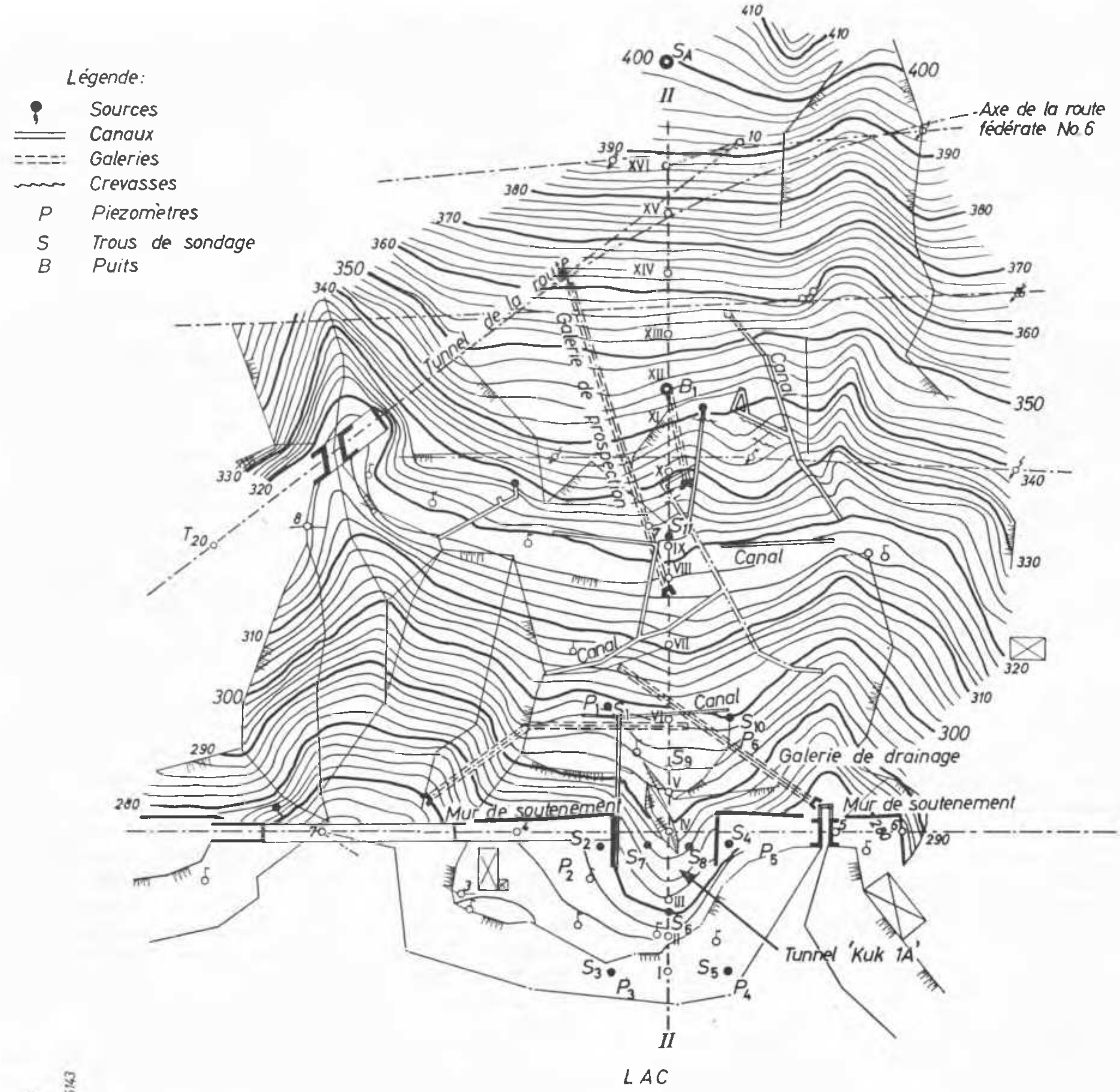


Fig. 22 Situation du tunnel 'Kuk 1A' de la voie ferrée Konjic–Jablanica
Location of the 'Kuk 1A' tunnel on the Konjic–Jablanica railway line

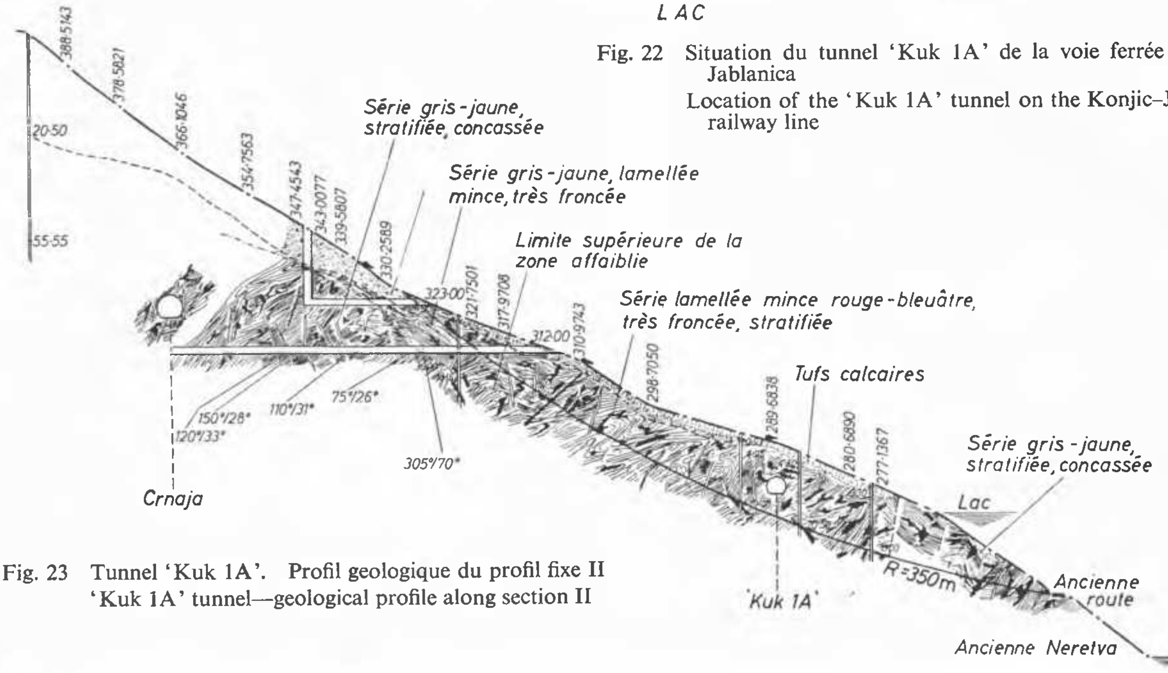


Fig. 23 Tunnel 'Kuk 1A'. Profil geologique du profil fixe II
'Kuk 1A' tunnel—geological profile along section II

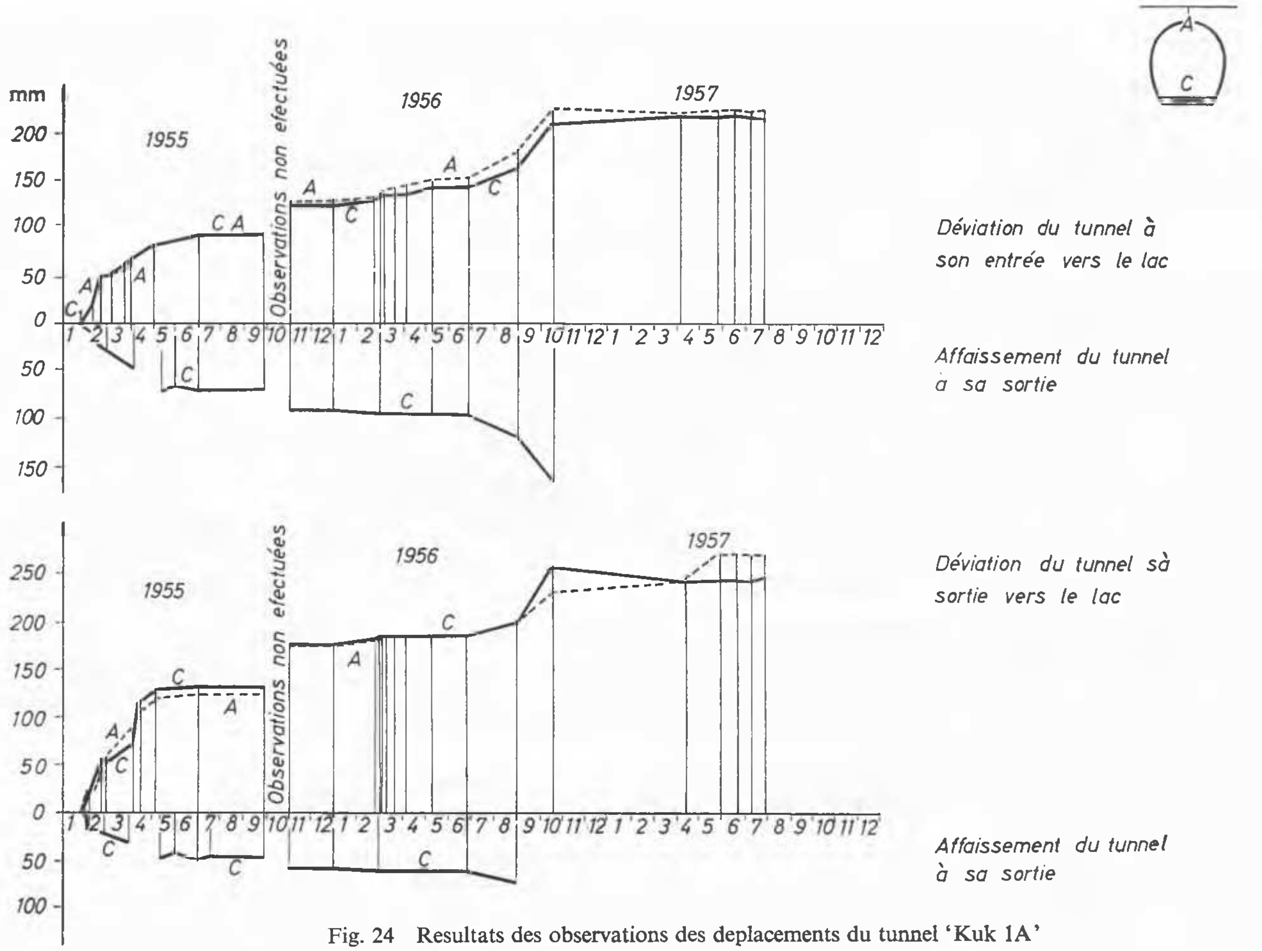


Fig. 24 Resultats des observations des deplacements du tunnel 'Kuk 1A'
Results of displacement measurements on the 'Kuk 1A' tunnel

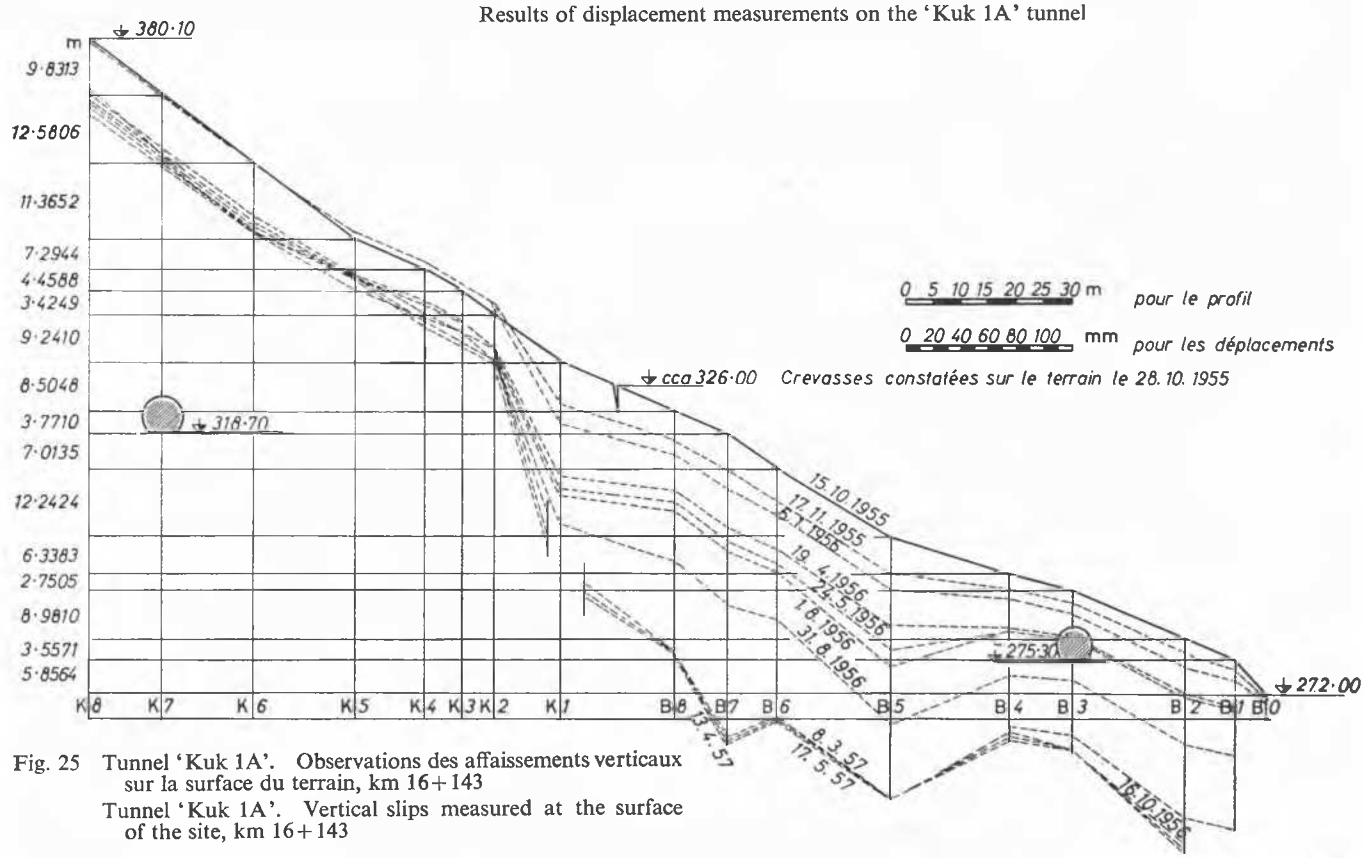


Fig. 25 Tunnel 'Kuk 1A'. Observations des affaissements verticaux sur la surface du terrain, km 16+143
Tunnel 'Kuk 1A'. Vertical slips measured at the surface of the site, km 16+143

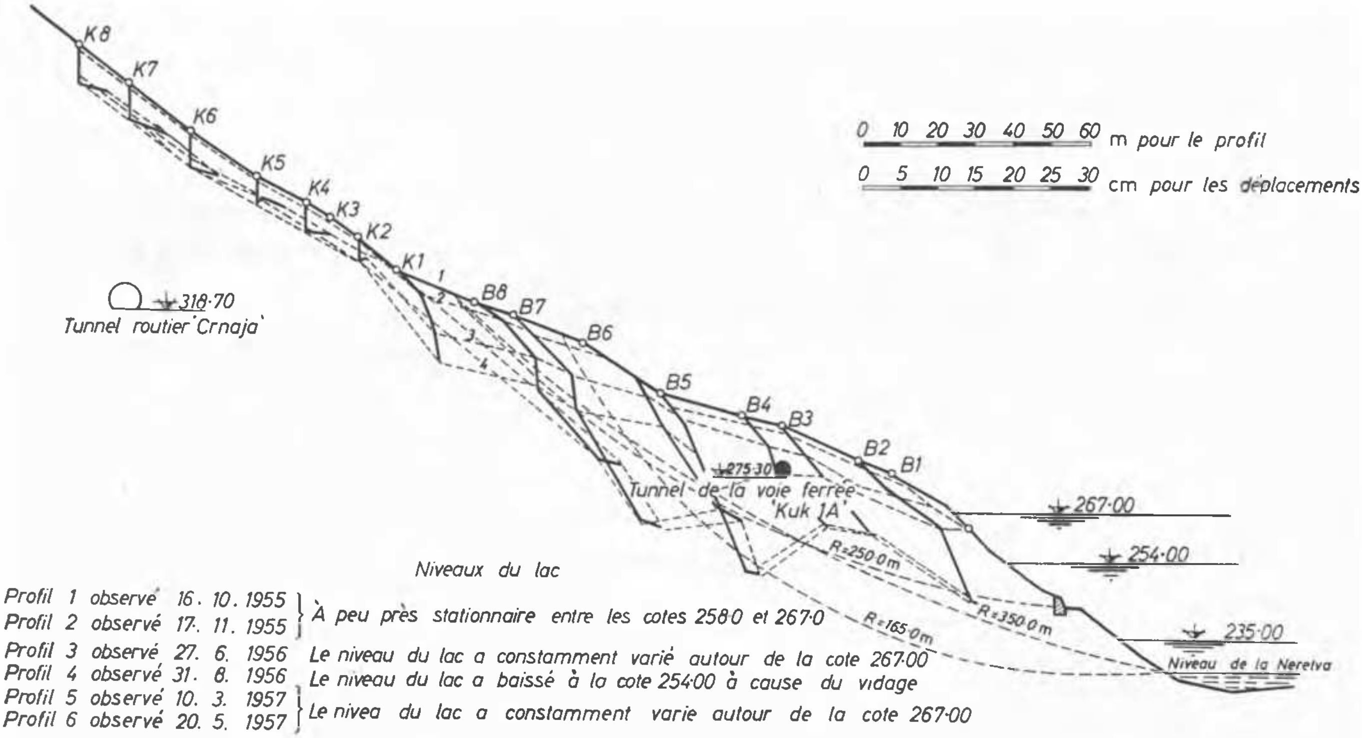


Fig. 26 Resultats des déplacement sur le profil de surface, km 16+143
Changes in the surface profile as a result of movements, km 16+143

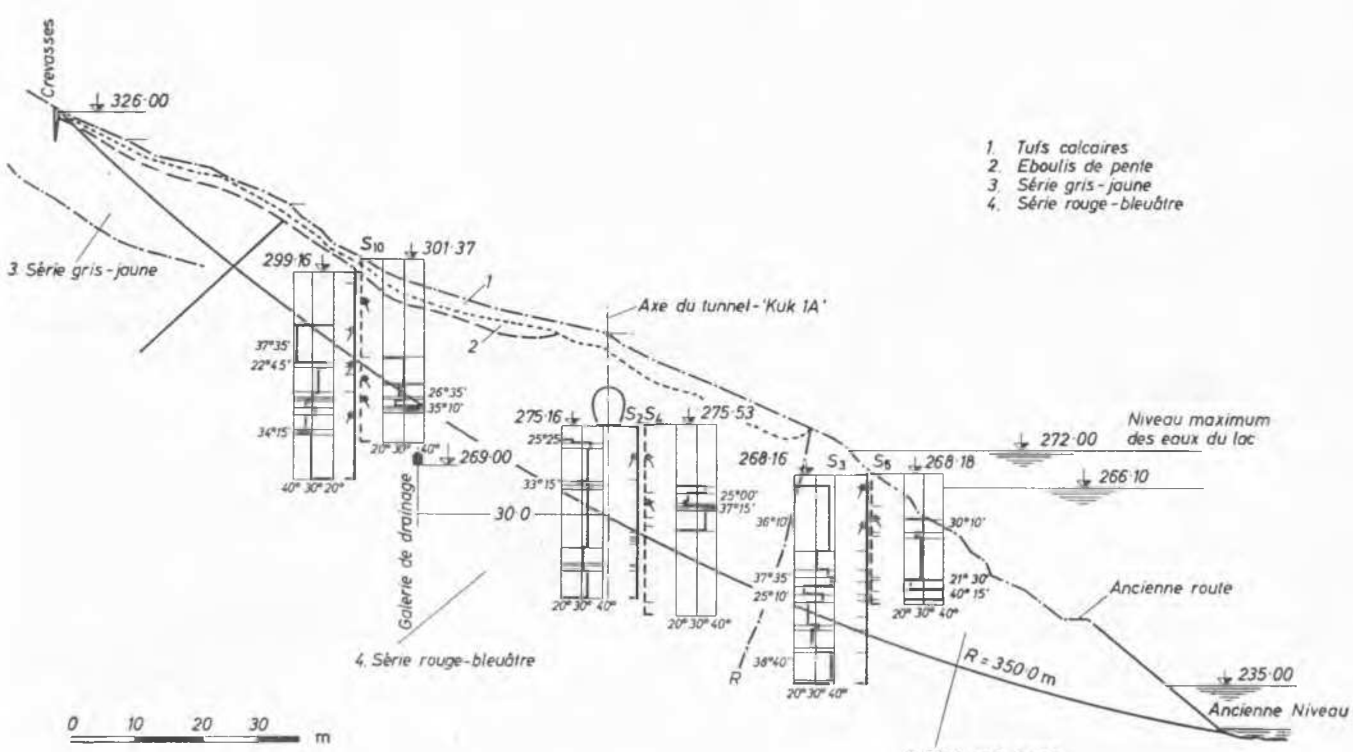


Fig. 27 Profil geologique transversal avec les surfaces des glissements suposées, km 16+143
Cross-sectional geological profile with assumed slip planes, km 16+143

30° 50′ obtenue au laboratoire, la pente naturelle et non saturée était stable avant l'existence du bassin, avec le coefficient de sécurité 1·21. Ce coefficient baisse avec l'exhaussement du niveau du bassin et descend à 1·09 quad le bassin est rempli.

(b) Si l'on suppose que la pente au dessus du lac est saturée,

Table des valeurs moyennes

No.	*Sortes d'essais*	*Nombr. des echan. exam.*	*Valeurs limites*	*Valeurs moyennes*
1	Classification AC	54	CL–SF	
2	Porosité	29	22·70–34·90%	29·70%
3	Poids volumétriques	29	195–235 t/m³	211 t/m³
4	Limite de liquidité	54	17·74–30·70%	26·20%
5	Limite de plasticité	54	12·06–21·30%	17·55%
6	Indice de plasticité	54	—	8·65
7	Résistance au cisaillement	52	21° 30 –40° 15	30° 50′
8	Pérméabilité	non examinée		
9	D'aprés M.I.T. argile sableuse á limoneuse			

Fig. 28

la résistance au cisaillement exigée est si grande qu'une telle situation peut être considérée comme impossible. De ce fait, dans la nature, la saturation de la pente peut se présenter seulement dans une certaine mesure jusqu'à une hauteur qui n'est pas beaucoup plus grande que le niveau du bassin.

(c) Le degré de saturation de la pente joue un rôle décisif sur la stabilité de ce glissement. On a constaté plusieurs fois des mouvements après les grandes pluies alors que le niveau du bassin était à peu près le même.

(d) Même avant l'apparition des fissures sur le terrain, il était possible de constater sur le profil de surface approximativement la limite supérieure du glissement (voir Fig. 25).

Les observations effectuées sur le terrain et l'examen de la stabilité de la pente quant aux baisses rapides du niveau du lac, depuis son plus haut niveau jusqu'à differents points plus bas, ont donné les résultats représentés par la Fig. 30, et il en a été constaté ce qui suit:

(a) Quand le niveau des eaux du bassin baisse rapidement, d'environ 5 m ou plus de la plus haute cote, la pente devient instable. Un abaissement rapide encore plus grand augmente cette instabilité. Ces recherches correspondent aux observations faites, car il a été constaté à plusieures reprises que quand le niveau du bassin baisse rapidement pour plus de 5 m, le tunnel commence à se mouvoir.

(b) En prenant pour base les observations du trajet des différents points du profil de surface, on a constaté que les directions de mouvement des divers points étaient différents selon que les déplacements étaient causés par l'abaissement du niveau du bassin ou par l'action des eaux de la pente (voir Fig. 26).

(c) La série des sédiments du trias bas, qui est généralement considérée imperméable est devenue perméable par suite de l'existence de fissures.

En se basant sur les observations sur le terrain, sur les essais au laboratoire et sur les analyses de stabilité, on peut supposer que la valeur moyenne de la resistance au cisaillement obtenue et effectuée de la manière décrite, répond à peu près à la résistance au cisaillement réelle, vu le fait, qu'avec la vitesse du cisaillement des ces matériaux, leurs résistances changent très peu. Que de plus grands mouvements de la pente, il faut chercher la cause dans l'inégalité de la surface de glissement et dans le fait que les recherches ont été exécutées sur le profil le plus dangereux.

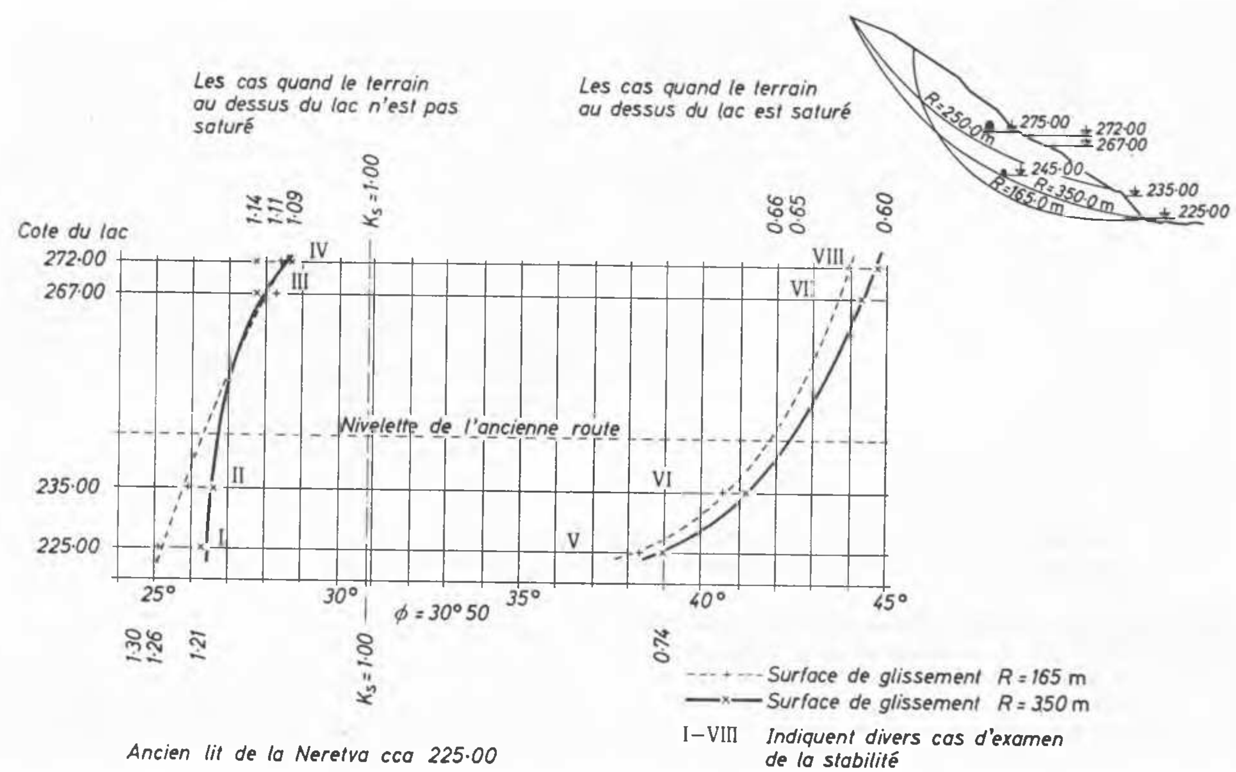


Fig. 29 Examen de la stabilité de la pente du tunnel 'Kuk 1 A'
Investigation of the stability of the slope of the 'Kuk 1A' tunnel

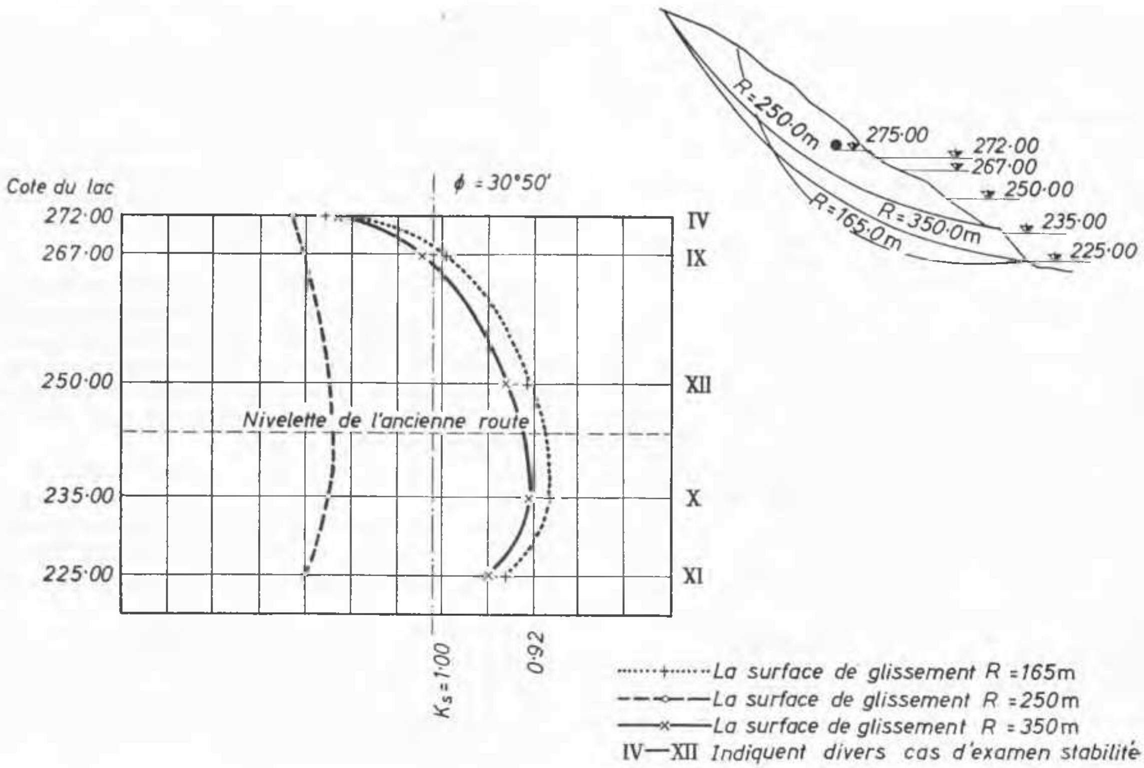


Fig. 30 Examen de la stabilité de la pente du tunnel 'Kuk 1A' lors d'abaissement rapide du niveau du lac de la cote 272·00 aux cotes 267·00, 250·00, 235·00 et 225·00
Investigation of the stability of the slope of the 'Kuk 1A' tunnel in case of rapid draw down of the lake from a level 272·00 to a level of 267·00, 250·00, 235·00 and 225·00

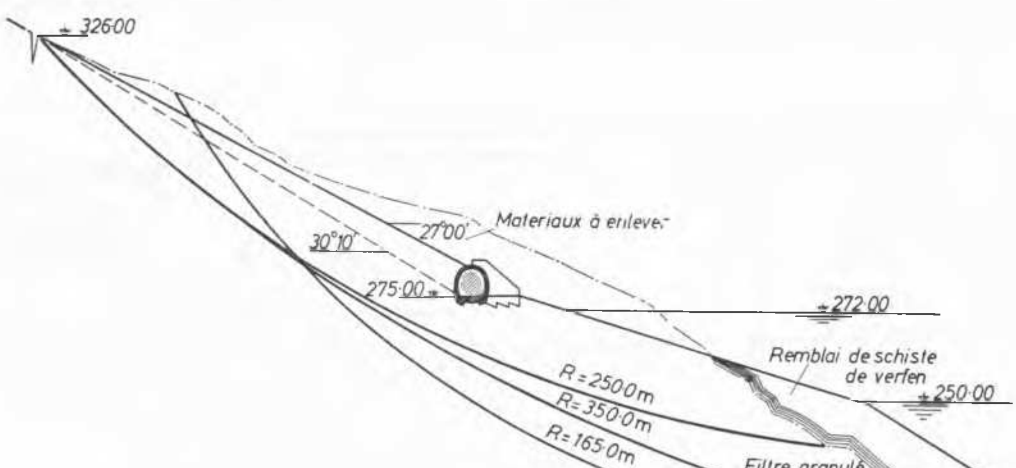


Fig. 31 Assainissement de la partie upérieure par creusement de la pente, de la partie inférieure par remblayage dans le lac
Improvement of the top part by flattening out the slope and of the bottom part by placing fill into the lake

Après examen des différentes propositions d'assainissement, il a été constaté qu'on peut le faire avec succès uniquement en transportant les masses supérieures dans le lac, au pied de la pente. Le déblaiement des masses de terre est prévu jusqu'à environ la calotte du tunnel pour aussurer la circulation ferroviaire, ainsi que pour la raison qu'un déblaiement plus profond mettrait en danger la stabilité de la pente supérieure (voir Fig. 31).

B. Ladanyi (Yugoslavia)

In the very interesting Paper 6/28, by J. Zeller and R. Wullimann, a review of the results of 146 triaxial shear tests on non-cohesive gravelly sand and boulder materials for the Göschenenalp dam is presented.

I should like to make some remarks concerning the cohesion of interlocking of grains. As stated by the authors and represented in Fig. 6a and 7 of their paper, by plotting the results of triaxial shear tests in a failure stress diagram of Mohr and designing the failure envelopes corresponding to constant porosities at 0·9 kg/cm² hydrostatic pressure, the envelopes thus obtained passed above the origin of co-ordinates, showing an apparent cohesion of the material. I agree with the statement of the authors that this cohesion has most probably to be considered as an actual property of the material, and that it is due to the interlocking of grains. However, I could not quite agree with the diagram represented in Fig. 6a, where the cohesion is assumed to be constant and equal to its average value for all tested porosities.

In connection with that I should like to mention that this phenomenon can be observed not only in coarse-grained granular materials, but also in fine sands, provided that shear tests are carried out with a careful control of porosity. During my recent stay in the State Geotechnical Institute in Ghent, Belgium, E. De Beer, director of the Institute, proposed that I should make a series of tests with the purpose of investigating this phenomenon. In this programme I carried out about 50 tests in a ring shear apparatus on a dry uniform sand with particle size between 0·15 and 0·30 mm. Initial porosities were measured very carefully at a constant normal load, and particular care was taken by performing tests at very small normal loads, so as to obtain points near to the origin. For the sand tested it was found that within the range of porosities between $n=40$ and $n=44$ per cent $\tan \phi$ increased from 0·57 to 0·82, and cohesion of interlocking from 0·015 to 0·040 kg/cm². No curving of the envelopes could be observed near the origin of co-ordinates, so they were assumed to be straight lines throughout the tested range.

It may be concluded from the results of the tests mentioned and published on this problem that the cohesion of interlocking of grains is an actual property of granular materials, the magnitude of which is dependent on the size, shape and roughness of grains for different materials, and increases with the initial density before shear for a certain material.

Because of its magnitude relative to the part of shear strength caused by friction, the cohesion of interlocking will not have a significant influence in cases where large normal loads are applied, but it must be taken into account in practical cases of small normal loads, and especially for the purpose of evaluating the results of model loading tests in dry sand performed on a laboratory scale.

E. Nonveiller (Yugoslavia)

The interesting theoretical considerations on the stability of slopes given in Paper 6/25, by D. H. Trollope, show that the usual concept of circular sliding surfaces can lead to serious over-estimation of the stability of dam slopes. It was shown by Samsioe in 1954 as well as by Paper 6/25 that the form of sliding surfaces in triangular fills according to theory can be convex, just the opposite of the concave shape on which our analyses generally are based. This theoretical result was proved on model tests of embankments loaded to failure, which

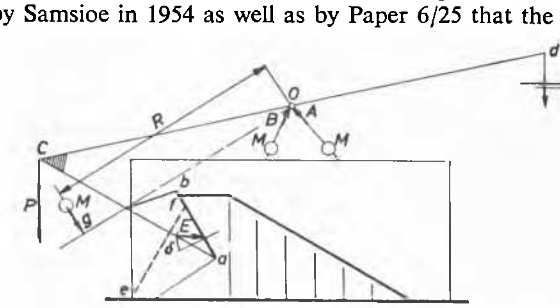


Fig. 32 Device for testing models of dams
Appareil pour essais sur maquettés de barrages

a–b rigid support fixed on lever a–c–d
O hinge
A, B reaction forces on hinge O, measured by proving rings M
P' balancing weight
P load forcing support a–b against dam slope

Fig. 33 Model of fine sand after sliding
Modèle de sable fin après glissement

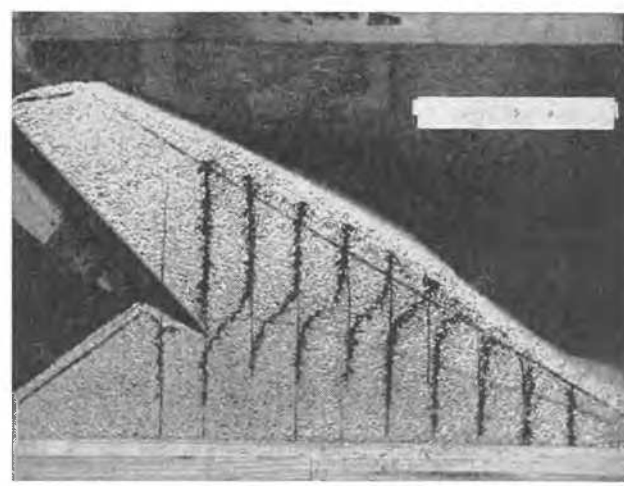


Fig. 34 Model of coarse sand after sliding
Modèl de sable gros après glissement

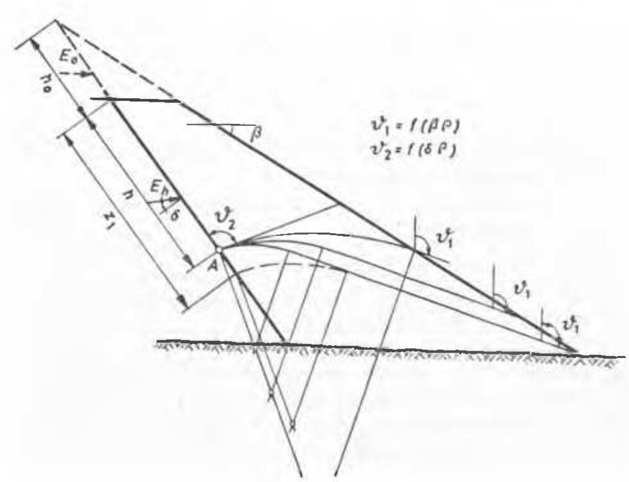


Fig. 35 Various sliding surfaces starting at point A
Surfaces de glissement à partir du point A

I carried out so as to study the behaviour of slopes of rockfill dams with clay core, in a device shown in Fig. 32. The slope portion of the model dam was loaded by forcing the rigid support a–b, fixed on a lever which rotates about the hinge O, against the slope portion by incrementing loads P in c.

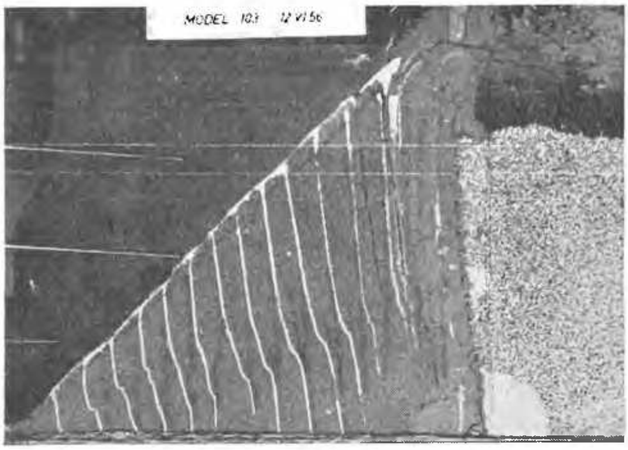


Fig. 36 Model dam loaded by water load
Maquette de barrage chargée hydrauliquement

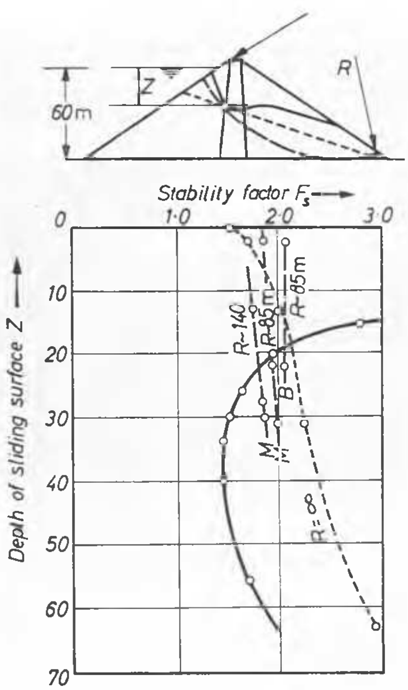


Fig. 37 Comparison of stability factors obtained by different methods of calculation

Comparaison des facteurs de stabilité obtenus par différentes méthodes de calcul

The intensity, the point of application and the obliquity of the resistance E of the slope were determined, and the resulting sliding surface at failure of the slope found out. In Figs. 33 and 34 the sliding surfaces of two of these model tests are shown. In all the tests convex sliding surfaces developed, which are straight at the slope side and curved at the core side.

It was found that the angles δ—Fig. 35—at which the sliding surface in the model dams meet the slope and the core correspond to the equilibrium condition of stresses at those points. The resistance E acted near the lower-third of the length involved at an obliquity equal to the angle of friction of the sand on the support. Fig. 36 shows one of the models of a dam with a thin vertical clay core. It was loaded by impounding water on the upstream side of the core. The slope collapsed after large deformations developed. The sliding surface was again convex.

The results of these tests give experimental confirmation of the theoretical results of Samsioe and Trollope. Assuming convex surfaces in the stability analyses, lower stability factors are obtained than the concave circular surfaces. For the 60 m high Preuča dam the stability was calculated with plain, concave and convex sliding surfaces. The results plotted in Fig. 37 show clearly that the lowest stability factor is obtained for convex surfaces which cut deep into the slope.

R. Peterson (Canada)

A. Marsland suggested in his discussion that Paper 6/19 (R. Peterson, N. L. Iverson and P. J. Rivard) would be more valuable if data were included with regard to the timing of the failures. This aspect has consequently been re-examined and there does not appear to be any significant correlation between the elapsed time to failure, following construction, and the

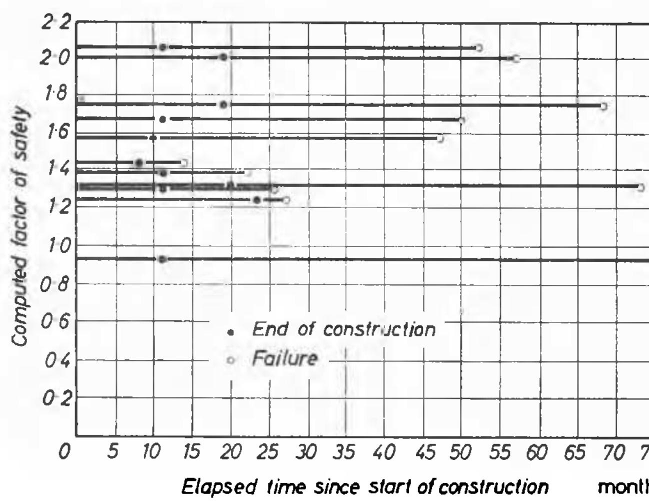


Fig. 38 Relationship between computed safety factor and elapsed time to failure
Rapport entre la facteur de sécurité calculé et le temps écoulé jusqu'à la rupture

computed safety factor using the total stress method. The information for each of the areas for which a computed factor of safety is available is shown in Fig. 38.

Since Paper 6/19 was prepared the items listed therein for further investigation have been studied and no important new facts have been brought to light. However, in spite of the fact that laboratory compression tests indicate very similar stress–strain curves for both foundation and fill material, it would appear that in some cases at least a definite rupture plane has developed only through the dyke with a plastic readjustment in the foundation after the failure of the dyke. Fig. 39 shows an area where the differential crest movement was about 16 in. and no failure plane or heave was evident at the toe. This movement started in November and apparently was pretty well stabilized by the following May when the photograph was taken. In this case it is likely that an actual rupture plane occurred only in the dyke as a result of plastic yield of the foundation. The differential movement in the average case however was more than this, ranging up to a maximum of about 5 ft.

Fig. 39 Dyke showing crest settlement
Digue avec tassement de la crête

Unfortunately I only heard a portion of A. Marsland's discussion, but it would appear that there are similarities between the case which he described and those covered in Paper 6/19.

Both A. Casagrande and B. Löfquist have discussed the possibility of cracks forming through homogeneous embankments as a result of differential settlements or movements. Fig. 40 shows a photograph of a crack on the crest of a 33 ft. high homogeneous embankment. The crack is 2 to 3 in. wide and apparently was formed as a result of a settlement of about 1 ft. (due to saturation of the embankment) at the upstream edge of the crest and 30 ft. to the left of the photograph. This case clearly shows the tendency for cracks to develop as a result of differential movement.

Fig. 40 Embankment crest showing cracks
Crête de remblai fissurée

B. RAJČEVIĆ (Yugoslavia)

The intention of this communication is to explain the methods we use in studying earth dam designs and in settling their dimensions, in connection with Paper 6/17 by A. MYSLIVEC.

At the Fourth Congress on Large Dams held in New Delhi in 1951 W. P. Creager, General Reporter, mentioned Paper No. 16 which dealt with the problem. In this paper it was indicated that each small volume in an earth dam must be compacted to a density corresponding to that which the superimposed load will consolidate it to.

Further: Daehn and Hilf (Paper 39) show that excessive pore pressures are created in the impervious zone of earth dams due to the pressures of superimposed earth loads within the mass with insufficient time for complete drainage. These pore pressures may create the most critical condition of the design.

It is stated that these excessive pressures can be eliminated by using a compaction moisture content 1 to 3 per cent less than laboratory optimum.

It is indicated that the amount of such pore pressures can be estimated by triaxial tests or by calculations based on the results of the less expensive drained compression tests.

Triaxial tests of this type by Lee (Paper No. 100), on samples at 2 per cent above the optimum water content developed about 6·3 and 11·2 kg/cm^2 pore pressures at 7·0 and 14·0 kg/cm^2 applied total stresses, respectively; while samples at 2 per cent below optimum developed only about 0·56 and 1·4 kg/cm^2 at 7·0 and 15·0 kg/cm^2 pressures.

During the 3rd International Conference on Soil Mechanics in Zurich, L. Bjerrum introduced a discussion between those who are for the wetter side and those for the drier side, inviting the eminent representatives (F. Walker, today our General Reporter, and the late T. Middlebrooks) of these opinions to express their views.

Since 1950 many earth dams have been constructed following the principle that the compaction of each zone must result in hydrostatic equilibrium on application of the superimposed load; at the Fifth Congress on Large Dams I presented first results on the behaviour of these dams (Paper No. 91, question No. 18).

In spite of this method being in the stage of development we have decided to use it, due to the lack of better solutions, with the same explanation given by our President, Professor Terzaghi, at this conference.

The method as applied to the Vlasina dam, 37·5 m height (over 100 ft.) is explained in Figs. 41 and 42.

We have constructed also the following dams, applying the same method:

Arandjelovac dam	26 m
Ovčar Banja dam	27·5 m
Vrla II dam	24·0 m
Mavrovo dam	60·0 m

Appropriate fill pressure measurement cells are installed in these dams and recordings are being made for a few years.

A paper presenting the comparison between estimated and measured values of stresses, beside the general behaviour of Mavrovo dam, is now prepared for the next Congress on Large Dams in 1958.

According to our observations we are content with the behaviour of these dams, being also satisfied with the construction conditions which have been shown to be practical and simple when applying the technique mentioned above. That is why, from my point of view, A. Myslivec's paper is of great

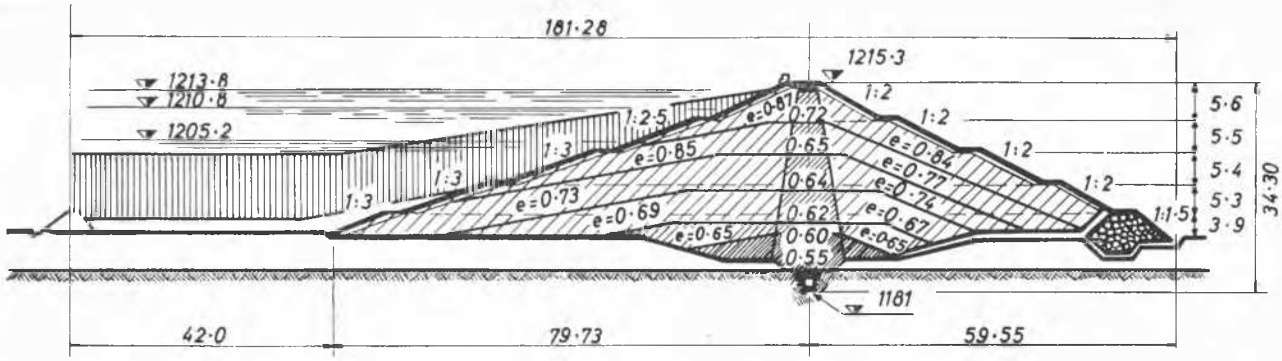


Fig. 41 Zones of different void ratios
Zones avec indices de vides différents

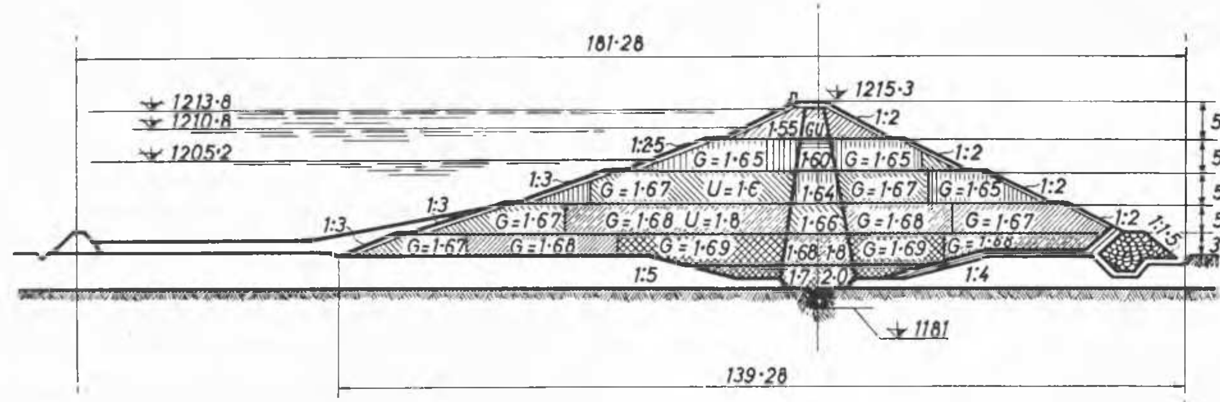


Fig. 42 Zones of different densities
Zones de densités differentes

interest, showing that the laboratory compaction tests should not be simply applied to earth dams. I would like to add that from the theoretical point of view the method is justified by the fact that it is possible to estimate shear resistance along the potential sliding surface and that we will not have settlement due to consolidation or heave action caused by over-compaction. In other words, there will be no changes in the condition of a dam with time, but we will have to calculate the stability of a dam with greater security, since we can determine factors with more accuracy according to appropriate tests.

In general, and with respect to settlements and pore water pressures, it may be considered convenient to have the lower part of a high dam compacted to a higher density than that of the upper part.

Apart from the necessity for further research which should bring further proof and theoretical confirmation, I believe that I have shown to you the advantages of this technique and I hope you will believe me when I say, according to my experiences on different works, that this technique is easily applicable and does not introduce any difficulty at all.

K. L. Rao (India)

In India a large number of large-sized earth dams are under construction. With the development of field laboratories for locating correct borrow pits, placement, compaction and control procedures, it is possible to ensure that the embankments are quite impermeable and stable. There have been a few failures of earth dams in recent months but none of these was due to either overtopping or faulty construction of the embankment itself. Foundations of earth dams have become nearly as important as the foundations of masonry dams.

In view of the large size of the Indian rivers, even foundation investigations assume special importance. It has become almost a necessity to employ geophysical investigations of both the electrical resistivity and sensitive refraction types at practically every important site to reduce the cost and period of investigations by drilling alone. What is actually done, is that a few bore holes are put in and the data supplied to the geophysicist, who can then adjust his constants to suit the site conditions so that his resulting survey may prove more accurate. A few bore holes are put in, after the survey is finished, to check the accuracy of the survey. Geophysical investigations are employed even in the Gangetic plain, where the alluvium is several thousand feet deep, to know if any relatively impervious or denser layers exist.

Fig. 43 is the Shervathi dam site: with the completion of this project it will be possible to generate ¾ million kW of power; the full length of the dam is not shown in the illustration. In accordance with the original designs, a masonry dam is to be built in the bed of the river where gneissic rock is exposed, with earth dams on either flank. The rock profile was determined by geophysical methods aided by a few bore holes. Investigations have revealed that beneath the top hard crust of 3 to 8 ft. of clayey loam, deep layers of residual soils, pinkish or yellowish in colour with white patches, exist. These have resulted from the decomposition of the rock *in situ* due probably to heavy rainfall in the locality, which varies from 200 to 250 in. per year. Due to lateritic weathering, the rock minerals have been decomposed and kaolin or lithomarge has been formed, and silica and bases have been leached away. The original structure of the rock has been maintained to some extent so that the soil has a low density and is porous. The natural moisture content of the soil varies from 24 to 54 per cent; while

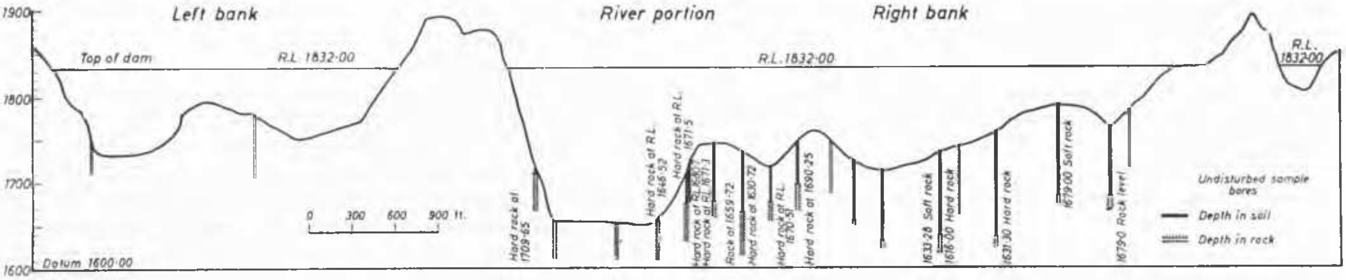
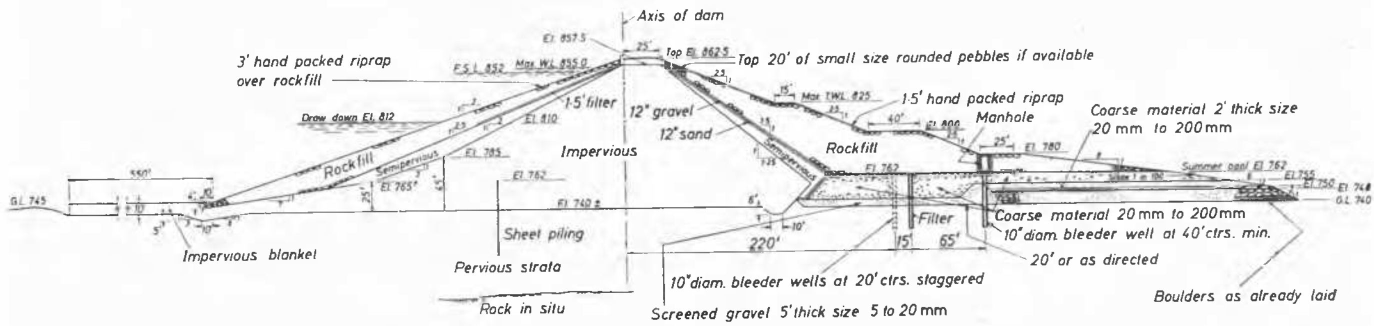


Fig. 43



Kotah Dam maximum section

Fig. 44

other properties are: LL = 50, PI = 12, dry density 70 to 94 lb./cu. ft., and saturation 85 to 90 per cent. With such substrata it is felt that it is not safe to found the earth dam on the existing ground and that the entire earth to rock level has to be removed: if this were done, a masonry dam would be cheaper than an earth dam. If it were possible to avoid a masonry dam it would cost £3 million less. The failures reported in Paper 6/19 (R. Peterson, N. L. Iverson and P. J. Rivard) of Northridge dam 70 ft. high in Canada, where the dam was founded on highly plastic clay, points to the necessity of a cautious practice in building dams on light soils with high moisture content.

Fig. 44 is the site of Kotah dam, where again the subsoil presents difficulties. The project is intended to provide irrigation for a million acres. To achieve economies, an earth dam has been designed across the river channel portion, with the spillway located on the right side. It was realized from the beginning that the river channel was filled with boulders at the lower levels. The design consists of a heavy clay blanket in front, a generous rock toe with filter and relief wells in the rear, and a sheet pile cut-off in the middle: the sheet piles could not be driven to penetrate more than half of the alluvium. The subsoil is highly pervious, coefficient of permeability being of the order of $1{\cdot}8 \times 10^{-1}$ cm/sec. Discussions and consultations are still being held as to whether it is necessary to provide additional safety measures such as a clay grout curtain below the sheet piles to the rock. Some engineers in India hold that clay injections are no good, but all of us are aware of the utility and reliability of clay injections to provide a positive cut-off under suitable conditions. A statement from this conference recognizing this, and indicating that it is a great aid to the soils engineer to be utilized where it proves economical, will be helpful. At the Fourth International Conference on Large Dams, it was decided that the short masonry cut-off wall that used to be built at the bottom of the trench under an earthen dam may be omitted. This has resulted in considerable saving, as the engineers who were accustomed to such practices do not adopt them any longer.

The difference in treatment of foundations is shown in Papers 6/2 (G. Baroncini and A. Croce) and 6/10 (D. Finzi and C. Niccolai). Thus, the two Italian dams, Arvo and San Valentino, though of equal height (i.e. 35 m) show settlements of 14 cm and 40 cm respectively. The seepage was 2 litres per sec in the first and 25 litres per sec in the second: both are founded on permeable strata. San Valentino dam lies on a thick alluvial deposit of medium permeability while in the Arvo dam the core has been taken to rock level. At the Arvo dam during idle periods in its construction water was standing on the dam partially constructed, and when work was resumed the wet soils were not removed but were covered with drier soils before rolling. During subsequent sampling operations of the dam some patches were found which were so soft that it was not possible to obtain intact samples. This corresponds to our experience at Hirakud dam in India.

While the foundation problems under buildings receive great attention in our conferences the same cannot be said of the foundation problems under earth dams. As the failures of earth dams can in the main, in future, be only due to faulty foundation treatment it is necessary to concentrate on this aspect of the problem.

P. Śliwa (Poland)

Investigations have been made on the propensity of slides to form in certain tertiary clays. Seven sliding terrains appearing in clay formations of the continental Miocene, marine Miocene and in Eocene clay (shale detritus) have been carefully investigated.

A classification has been made of soils investigated, of their propensity for sliding based on field measurements, and observations regarding the inclination of slope surface and the height of the slope, as well as the duration of stability of the slope under a given inclination. The stability of slopes has been considered as a function of time.

The laboratory investigations of the clays concerned related to their mineralogical character, grain-size characteristics, water content, unit weight, porosity, Attenberg limits, rapidity of slaking in water and shearing resistance (angle of internal friction, cohesion).

Table 1 gives a summary of the results of field and laboratory investigations. The results of about 700 laboratory designations have been compared with the classification of stability based on field observations and measurements.

Table 1

No.	*Location*	*Geological formation*	*Stability in field observations*	*Mineralogy*	*Clay fraction* <0·002 mm %	*Plasticity index* %	*Slaking time* min.	*Shear strength* kg/cm²	*Stability coefficient*	*Skempton's activity*
1	Turoszów	Miocene continental	Great	Quartz Kaolinite	50	25	43	0·82	2,150	0·50
2	Jaroszów				32	29	32	1·00	864	0·91
3	Benczyn	Eocene (Karpaty)	Middle	Quartz, Illite, Montmorillonite	28	33	22	0·72	616	1·18
4	Sucha				11	34	15	0·45	165	3·10
5	Biaty Kościót	Miocene marine	Little		15	42	11	0·67	165	2·80
6	Lagiewniki				10	32	12	0·45	120	3·20
7	Sadowie				8	41	8	0·43	64	5·12

The value of individual laboratory characteristics of clay for the determination of the stability of slopes (factor of safety) has been analysed, and the conclusion has been reached that the propensity to form slides may be best characterized by grain size characteristics and the time of slaking of weather-dried specimens of clay in water. It is proposed to apply the product of the percentage of the clay fraction and the time of slaking per minute—called 'coefficient of stability'—to the quantitative evaluation of the stability of slopes in clay soils.

T-K. TAN (China)

With reference to the factors causing failures in natural slopes after an elapse of considerable time, mentioned in the excellent analysis of the General Reporter, I wish to make the following remarks.

Due to the complicated properties of clays it is not very easy to determine the failure strength. It is a well-known fact that the value of the ultimate strength of clays is dependent on the manner in which the sample is brought into the state of failure. For instance, the shear strength of fat clays is strongly dependent on the rate of deformation, therefore the common routine testing procedure may be insufficient to get any idea of the long-term stability of slopes. Moreover the stress distribution in the samples in some current testing apparatus is so complicated that nobody knows what exactly has been measured. These shortcomings of course may be compensated by a large factor of safety but a more objective treatment of the problem would be welcome. In this respect I wish to draw attention to the experimental fact that there exists a certain yield-stress in clays above which accelerating flow occurs which may be greater than can be tolerated and ultimately lead to failure. Below this yield-value, called f_3 by GEUZE and TAN (1953), TAN (1957), only uniform flow has been observed with continuous re-adjustment of the particles. Because of the complexities of the disintegrating phenomena in an elastoviscous plastic body such as clay, it is more objective to base our limit equilibrium theories on f_3 rather than on an ultimate failure stress (TAN, 1953). If in a region of an earth mass a certain limiting combination of stresses is exceeded, then local structural disintegrations will occur with gradual transfer of stresses to the neighbouring regions. In combinations with other unfavourable factors this time process may lead to an ultimate failure of the slope.

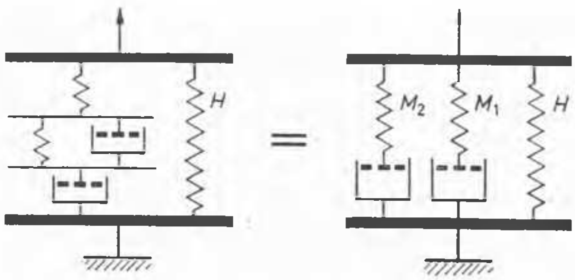
The determination of the viscosity as a function of the shearing stress under various hydrostatic stresses is recommended in order to get a better insight into the mechanism of long-term stability in earthwork engineering. This can be carried out with the help of compression plastometers of the triaxial type, as I have shown in my discussion in the fourth session. The experimental procedure to study the flow under constant loads has been used by HAEFELI and SCHAERER (1946), HAEFELI (1953), and TAN (1954) and is also proposed by M. GOLDSTEIN and G. TER-STEPANIAN in Paper 6/11. It is reasonable to assume that the shearing-stress-viscosity relationship of shear-hardening clays will not be so simple as it has been found for fat clays by Geuze and Tan. The choice of the admissible yield value as a function of the effective hydrostatic stress is dependent on the nature of the earth work construction; for instance a viscosity of 10^{13} poise for a railway embankment is inadmissible due to the large flow effects. Therefore the analysis of long-term stability is inseparable from the study of the rheological properties of clays.

Another important factor which should be studied in the laboratory is the chemical stability of the substance. In this respect a mineralogical analysis in combination with the determination of the physicochemical properties may be very helpful. A high base exchange capacity for instance is a strong indication of the great possibility of chemical change and it is advisable to study the mechanical properties of the clay as a function of the adsorption complex (TAN, 1954).

The $C=0$ hypothesis of course is very interesting as an empirical method of design, but it does not take account of the viscoelastic and plastic properties of the clays.

Following Bikovsky, M. Goldstein has tried to make an interesting approach to the mechanism of long-term strength of clays. Based on a rheological model and on some experiments he proposes the hypothesis that the rupture of clay samples would occur at constant values of the strain, independent of the duration of loading. It may be pointed out that the author's experiments have been carried out on hard and brittle clay samples with a PI of about 15 per cent, water contents from 20 to 23 per cent and a deformation of 3 per cent at the moment of failure. As a parallel to M. Goldstein's experiments it may be mentioned that brittle loess samples with a water content of 12 per cent show failure as soon as a certain deformation is reached. However I have as yet never observed this regularity from my experiments on non-brittle clays with higher water contents.

A model which is valid for the states of stress before the yield value f_3 is reached has been constructed by TAN (1954) and is shown in Fig. 46. This model shows reversible deformations with complete recovery as long as the Hookean spring H is not loosed and exhibits continuous flow after the yielding of this spring. An analogous model is shown in Fig. 45, and a generalized model is given in Fig. 46. It is possible to describe the complete shearing process by this model. As soon as the spring is loosed, uniform continuous flow will occur; this is valid for stresses below the yield value f_3. As soon as f_3 is exceeded then the Maxwell elements M gradually become decoupled, the number of active dashpots will decrease and the viscosity decreases as a result of structural disintegrations. For stiff and brittle clays the viscosity may be very large and some of the Maxwell elements may quite well be replaced by springs



(a) (b)

Fig. 45 (a) Rheological model for clays; (b) model analogous to (a)
(a) Schéma rhéloogique pour argiles; (b) modèle semblable à (a)

and the approximate model then obtained is similar to that of M. Goldstein.

Obviously based on this model he proposes a method for determining the time of failure of clay samples by studying the continuous flow under several constant loadings. It is not quite clear how this prediction can be made from these stages of continuous flow, as his model only describes decelerating flow before failure.

I wish to congratulate G. Ter-Stepanian for presenting an unconventional method for the estimation of the depth creep

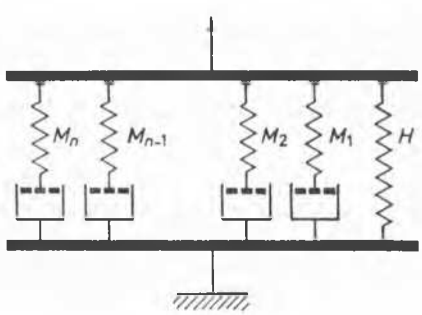


Fig. 46 Generalized model, in which structural disintegrations are described by gradual decoupling of the Maxwell elements
Schéma généralisé dans lequel les désintégrations structurelles sont illustrées par un de couplage graduel des éléments de Maxwell

in slopes, and it may be recommended to combine field creep measurements of slopes with the measurements of the rheological quantities in the laboratory.

References

GEUZE, E. C. W. A. and TAN, T-K. (1953). *Proc. 2nd Int. Congr. on Rheology*, p. 247, Oxford; Butterworth
— — (1953) *Rheological properties of clays*, Rotterdam: D. van Sijn & Zonen
HAEFELI, R (1953). *Proc. 3rd International Conference on Soil Mechanics and Foundation Engineering*, Vol. 3, p. 238
— and SCHAERER, Ch. (1946). *Schweiz. Bauztg.*, **128**
TAN, T-K. (1953). *Proc. 2nd Int. Congr. on Rheology*, p. 245, disc. Oxford; Butterworth
— (1954). Investigations on the rheological properties of clays, *Dr thesis Delft Techan. Un.*
— (1957). *Structure mechanics of clays*, Publ. Soil mech. Lab., Inst. Civ. Eng. and Arch., Academia Sinica, Harbin, China

J. G. ZEITLEN (Israel)

The General Reporter has remarked, with respect to Paper 6/3 (M. BAR-SHANY, G. KORLATH and J. G. ZEITLIN), that the authors place a great deal of reliance in cohesive strength. This comment is applicable only to design values used to approximate strength in the special series of stability studies included in the paper, and is not justified as the general view of the authors. The selection of design shear strength of clays, particularly in view of the discussions during this conference, deserves further explanation.

As may be found by detailed reading of those sections of Paper 6/3 dealing with Engineering Properties and Design, it has been determined that the cohesive strength tends towards zero with saturation of the clay at low normal loads. This effect is taken into account in the shear values suggested for foundation clays tested undrained ($C=0$, $\phi=23$ degrees), as well as in upstream slopes where steep faces are avoided in recognition of the danger of shallow slides. Even for deeper slide analyses, where pore pressures are considered together with drained shear values, the cohesion value for embankment material is only 0·2 kg/cm^2. It must be appreciated that the cohesion values shown are simply graphic intercepts, for the convenience of using a linear shear strength function, whereas test data indicate a curved envelope, dropping towards zero cohesion at low normal pressures.

The concept that cohesion is not a basic property of the clay but a function of the stress conditions in the soil is one to which the writer subscribes, but it is felt that this phenomenon is far more complicated for clays than simply applying a negative pore pressure to a fixed friction angle. Basic reasons for considering this approach to be over-simplified are: the difficulties of determining pore pressure and friction angle values in present test techniques; neglect of 'interlocking'; importance of time and creep functions; particle surface and chemical activity; and the effect of stress history on such factors as particle orientation. It is hoped that present clay shear strength research, including the programmes at the Israel Institute of Technology and by the Water Planning for Israel, Ltd., will help in understanding better the behaviour of the fat clays.

The soil engineer engaged in embankment or foundation designs concerned with clay soils is faced with a very practical problem, in that he must often design structures of limited size in situations where exhaustive shear strength determinations complete with accurate pore pressure determinations under all the possible test conditions are not economically justified. He should keep the latest explanations of shear strength behaviour in mind, but will still have to depend on a limited number of tests, under far from comprehensive conditions, in order to decide on the design shear values. He must consider the field conditions to be expected, and will have to establish his requirements for test-saturation, drainage, pressure range, time of shearing, type of apparatus—so as to come close to the actual situation if possible, and use his judgment of the similarities and differences in order to select proper results for use in his analyses.