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Soil Properties and their Measurement

Mesure et Propriétés des Sols

Fundamental and Natural Properties—Propriétés Fondamentales et Naturelles

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 T-K. Tan *China*
 T. W. Lambe *U.S.A.*
 J. A. J. Salas *Spain*
 E. C. W. A. Geuze *Netherlands*
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I. Th. Rosenquist

General Reporter, Division 1 / Rapporteur général, Division 1

The Chairman

Je déclare ouverte la première réunion du congrès international de mécanique des sols.

Je remercie le comité d'organisation de m'avoir fait l'honneur de me confier la présidence de la première séance.

Je donne la parole à notre rapporteur général, Monsieur Rosenquist.

The General Reporter

When I received the invitation to act as General Reporter for this Division it was a great honour and pleasure to accept.

The physical, chemical and mineralogical properties of the materials dealt with in soil mechanics are of extreme importance for the fundamental understanding of the subjects we are dealing with in the scientific and in the engineering approach. The large number of papers presented in Division 1 is a proof of the growing understanding of these subjects.

Although there have been changes in the subjects covered by Division 1 since the last conference in Zurich, the reduction of topics has not led to any reduction in the number of papers. We may anticipate that even if the restriction of subjects is more severe for the next conference, there may be an increasing number of papers presented under the heading 'Fundamental Properties'.

In my General Report presented in the Proceedings I have pointed out the necessity of combining topics from the extended field of the natural science in order to elucidate the engineering problems. We shall welcome this development with the greatest pleasure.

It is not sufficient that we can treat sliding phenomena, swelling, shrinkage and frost heave with a masterly mathematical apparatus. Only if we take up the study of the fundamental factors will the science of soil mechanics pass from the descriptive to the exact state. The necessity of correlating different sciences in order to understand the mechanical properties of soils has raised the same difficulties as in many older sciences, such as chemistry, physics and biology; namely, the problems of literature. Many valuable works have been

presented in papers which may have passed unnoticed by the group of applied scientists who should primarily be interested in the results. Language difficulties add to this problem. For instance, a general knowledge of Soviet literature is fairly low in West European countries and in America and, I presume, *vice versa*. In some cases it is found that authors of papers presented to this conference were unaware of results obtained in other countries by scientists working with related problems but in a different connection and bearing a very different title. I believe that the literature question should be considered as a very important one. If some kind of international literature service could be arranged so that the engineers and scientists working at the different geotechnical research centres could be given notice as to important results obtained in related fields of science, it would be a very great help for the development of our subjects. Indeed, I would say that it is absolutely necessary to arrange some kind of literature service with abstracts, otherwise our important branch of applied science may stagnate. The lack of knowledge of foreign languages depends upon the education in the schools of the different countries which cannot be improved by us. Translations of abstracts and papers into English and Russian will certainly make progress more easy and rapid.

When I first received the papers for Division 1 I found it impossible to deal with them all as, due to the great field covered, many subjects were outside my own field of research. This made it impossible for me to give all of them fair treatment, therefore, the Division has been made into two sub-Divisions. For Division 1a the different subjects are further divided under different headings, namely, the geological and regional factors, the fundamental properties, the swelling and shrinkage, the hydraulic properties, frost problems and, finally, different measuring devices.

The majority of the interesting papers invite discussion, but I find that the regional and geological factors are not of such a type that could concentrate the discussion sufficiently: therefore, I propose that the discussion should concentrate mainly on the common features shown by the rest of the papers, namely, the behaviour of water in soils. The extensive research which has been carried out in a long series of laboratories all over the world during the last few years has given us a better picture of the nature of the forces acting between the clay minerals and the liquid phases in the fine grained soils. It is necessary to understand the forces between the minerals and the water in order to understand the static problems in soil mechanics. In the same way it is necessary to understand the movement of water in order to understand the dynamic problems. In a large proportion of the papers presented to this conference the mobility and transport of water in one way or another is a fundamental factor. This holds in the case of swelling and shrinkage, in frost investigations and electro-osmotic properties. It seems most appropriate, therefore, to discuss the mobility of water in the fine-grained soils.

In my report I have distinguished between the following cases: (1) The flow problems of the more or less fixed water when the hydraulic gradients are small, and for small shearing stresses in soils. (2) The different kinds of water transport caused by differences in thermodynamic potential. (3) Water transport in frozen soils during the processes of freezing. (4) The transport of water molecules in completely frozen soils.

I have suggested that we might extend the discussion, if time permits, to the consideration of water movement in relation to two important geotechnical phenomena, namely, creep in natural slopes and long-term settlement due to secondary consolidation. It is a fact that these processes involve a transport of water without any marked hydraulic gradients. It seems as if some lack of thermodynamic equilibrium causes diffusion phenomena which move water molecules from highly

stressed zones to zones of lower stress. Such a discussion should be directed to the fundamental process in these two problems rather than to the consideration of observations in practice or the mathematical treatment of the problems, since the latter will be covered by discussion in other divisions. If we could bring clarity to these problems this conference would be of greater importance to soil mechanics than most of the previous conferences.

Some British friends yesterday said that I was turning this session into a session of physics rather than one of soil mechanics; I admit that it is true, but I find it necessary in order to understand the material.

The great progress made in several branches of natural science and engineering during the last century is mainly due to the co-operation between some people working with purely theoretical problems, without looking upon the possible application or value of their research, and others working on the engineering line, where everything must be considered from economic and technical points of view. Our field provides a brilliant opportunity for such research. Both types of work are necessary, and I hope that the research centres and governments will remember that seemingly useless results and publications are the foundations for the engineering development of tomorrow. I plead most sincerely with the directors of research and the leaders of establishments to permit their colleagues and employees the highest degree of freedom and the highest possible financial support, even if at the moment they can find no use for the results obtained.

H. B. SEED (U.S.A.)

I was most interested in the investigation of strength loss due to remoulding a sensitive clay described by S. MURAYAMA and S. HATA (1a/19) and the apparatus for applying repeated deformations to the clay. This equipment is apparently intended to stimulate the repeated effects of traffic on a soil.

For the past few years somewhat similar investigations have been conducted at the University of California and it might be pertinent to make a brief comment concerning the effect of the frequency of repeated stress applications on soil strength. Unlike the investigations of Murayama and Hata, the tests at the University of California have been conducted on compacted clays possessing no sensitivity due to soil structure. However, after a period of several weeks many of the specimens do possess low sensitivity due to thixotropic strength gain after compaction.

For such soils frequency of load application may have a significant effect on the resulting deformation characteristics of the soil. Figs. 1 and 2 show the results of a series of tests conducted on a silty clay (LL=37, PL=23) compacted to a degree of saturation of about 92 per cent. In these tests specimens were placed in triaxial compression cells as for a normal type of unconsolidated-undrained test, the confining pressure of 1 kg/cm² was applied, but instead of gradually increasing the deviator stress to failure, a deviator stress of 0.8 kg/cm² was repeatedly applied and removed, each application having a duration of 0.2 sec.

For the test series shown in Fig. 1 two identical specimens were subjected to repetitions of the same deviator stress at a frequency of 20 applications per minute: on one specimen the applications were continuous until about 10,000 repetitions had been applied; on the other, 10 repetitions of stress were applied and then the specimen was allowed to rest for one day before the applications were continued. It will be seen that after this period of rest the specimen deformed very much less than did the specimen loaded continuously; this effect would appear to be due to a loss of thixotropic strength resulting from the initial deformation of the specimen during the first 10 applications of stress and a subsequent regain of this thixotropic

strength during the one day period of rest. As a result, the specimen possessed greater resistance to deformation than when the repeated loading was continuous.

Since there appears to be a significant strength regain during periods of rest following deformation of a compacted clay

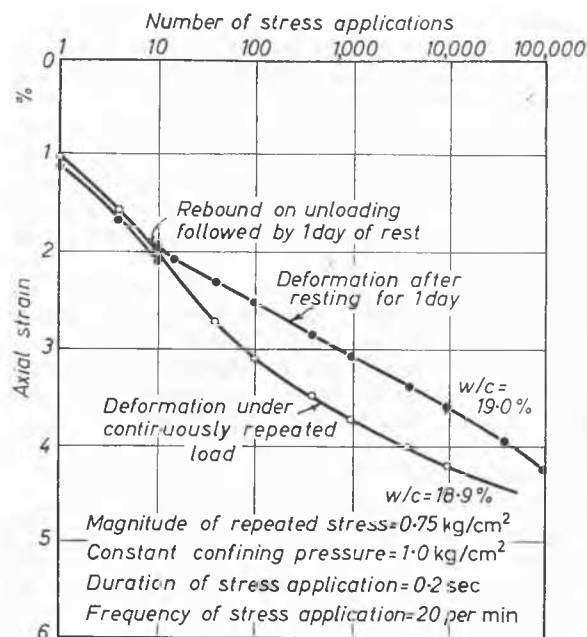


Fig. 1 Effect of period of rest on deformation of silty clay under repeated loading

Effet de la durée du repos sur la déformation d'une argile limoneuse sous charges répétées

possessing appreciable thixotropic characteristics, it would seem logical to expect some influence of frequency of stress application on the deformation of soils possessing thixotropic strength when they are subjected to repeated loading. A comparison of the deformations of samples of the same silty

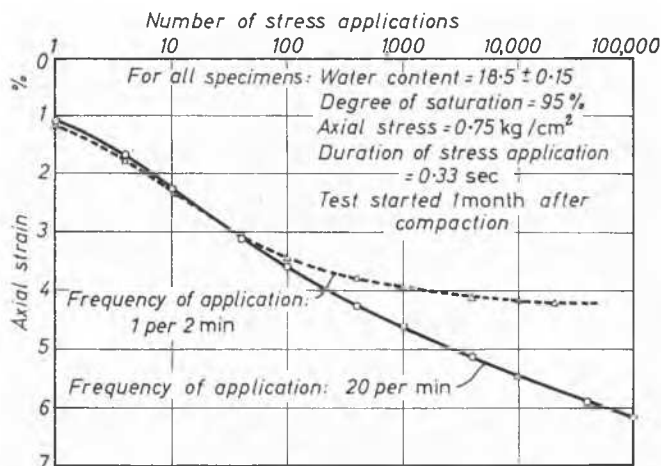


Fig. 2 Effect of frequency of repeated stress application on deformation of silty clay at high degree of saturation

Effet de la fréquence des sollicitations sur la déformation d'une argile limoneuse à un degré avancé de saturation

clay when subjected to repeated applications of a constant stress of the same magnitude and duration but with frequencies of application of 20 per minute and 1 per two minutes is shown in Fig. 2. It will be seen that for such a variation in frequencies there is a significant change in deformation characteristics.

For samples of the same clay possessing little or no thixotropic characteristics, variations in frequency had practically no effect on the results of similar tests.

Such effects are not likely to be observed on sensitive clays such as that tested by Murayama and Hata, since the effects of structural breakdown are likely to overshadow any effects of thixotropic regain between stress applications. However, in assessing the influence of traffic loading on the deformation of compacted clays the frequency of stress applications may well be a significant factor.

T-K. TAN (China)

I am very glad to see that in the last few years much progress has been made in the application of rheology to soil mechanics. We are indebted to the Russian scientists for presenting many valuable data on the rheological properties of soils.

The concept of thick bound water layers has for decades past

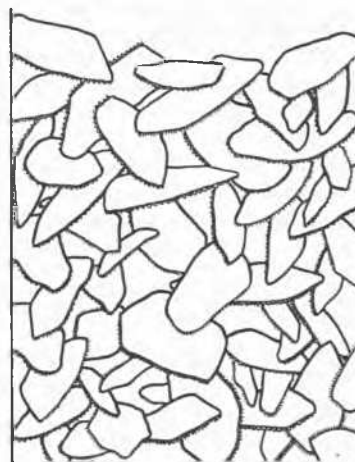


Fig. 3 Schematic picture of clay network
Schéma de la structure d'une argile

influenced the way of thinking in soil mechanics. In the last few years x-ray diffraction patterns have shown that the thickness of the adsorbed bound water layers for montmorillonite does not exceed 10Å and for illites and kaolinites it may even be less. Determination of the heat of wetting which is small and dependent on the nature and quantity of adsorbed cations provides further proof against the concept of thick adsorbed layers. The behaviour of soils towards electrolytes cannot adequately be explained if only bound water is considered.

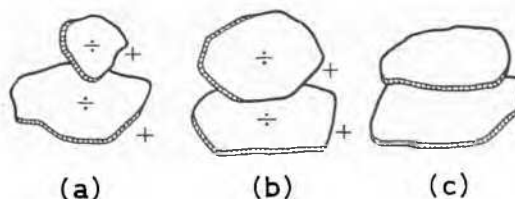
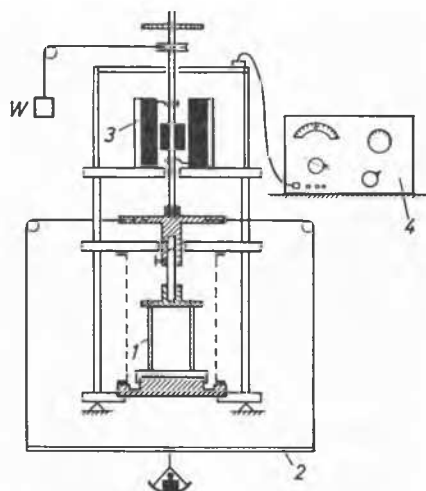


Fig. 4 Types of bonds between clay particles
Types d'adhésion entre les grains de l'argile

I am of the opinion that the clay particles form a three-dimensional network interconnected at several points as in Fig. 3 and that between the flat surfaces of the particles mutual repulsion exists.

The mutual contact between the clay particles may be subdivided into three groups as in Fig. 4: first, corner to flat surface contact; second, edge to flat surface contact; and the third, flat surface to flat surface contact.

In addition to the excellent papers presented by N. J. DENISOV and B. F. RELTOV (1a/5), and S. S. VIALOV and A. M. SKIBITSKY (1a/29) I wish to state that the bonding forces between clay particles may be due to: (1) Coulomb attraction forces between the positively charged edges and negatively



- | | |
|------------------------------|-------------------------------------|
| 1. Soil sample | 2. Loading |
| 3. Inductive measuring gauge | 4. Electronical measuring apparatus |

Fig. 5 Schematic arrangement of torsion plastometer
Schéma d'un plasticimètre à torsion

charged flat sides of the clay particles forming mutual contacts; (2) long-range non-polar London-Van der Waals forces; (3) bonding by cations; and (4) hydrogen bridges.

In montmorillonites the first and third type of contacts are possible, whereas the second type of contacts likely will not occur. This is due to the geometrical shape of the montmorillonite particles. The first type of contact which mainly occurs

at high water contents (corner-flat surface) is weak, as the contact area is very small. Moreover the flat surface is strongly hydrated and the bonding forces cannot be strong as the distance between the mutual particles in the points becomes considerable and the Coulomb and Van der Waals-London forces decrease rapidly with the increase of the distance: therefore montmorillonite soils can very easily be transferred into readily flowing slurries. For this group of soils it is known that even the third type of contact (flat to flat surface) is weak as a result of the thick water layers (10\AA).

Due to the geometrical shape of their particles, the second type (edge-flat surface) bond also may be formed in kaolinites and illites; as the flat surfaces are less hydrated than in montmorillonites the bonds (here three types) are much stronger and therefore these soil groups have better mechanical properties than montmorillonites.

Since I presented this hypothesis in 1953-54, further experimental evidence has been given in the literature concerning the positive charge of the edges of clay particles.

On the basis of this network hypothesis it is possible to give an interpretation of the rheological behaviour of clays. As was shown in 1953 by Haefeli, Geuze and myself, the clay under shear may show instantaneous deformation followed by a retarded deformation and ultimately continuous flow. The apparatus for conducting these experiments is shown in Fig. 5 and the results in Fig. 6. Further, it has been found that flow occurs already at very low stresses exceeding a small yield value. The instantaneous deformation may be due to a bending of the thin plate-like clay particles and to an increase of repulsion between the surfaces of the clay particles. Retarded deformation may be due to the visco-elastic properties of the clay particles themselves and to the migration of water molecules. If the stresses remain sufficiently small—less than a certain yield value f_2 —then no bonds will be broken, and all deformations are recoverable. If this yield value is exceeded, then the bonds will be loosened but may be formed again at other places in the network. This 'jumping of bonds' as it is called is accompanied by a transfer of stresses to more rigid parts of the skeleton which, in turn, will also yield; thus it is

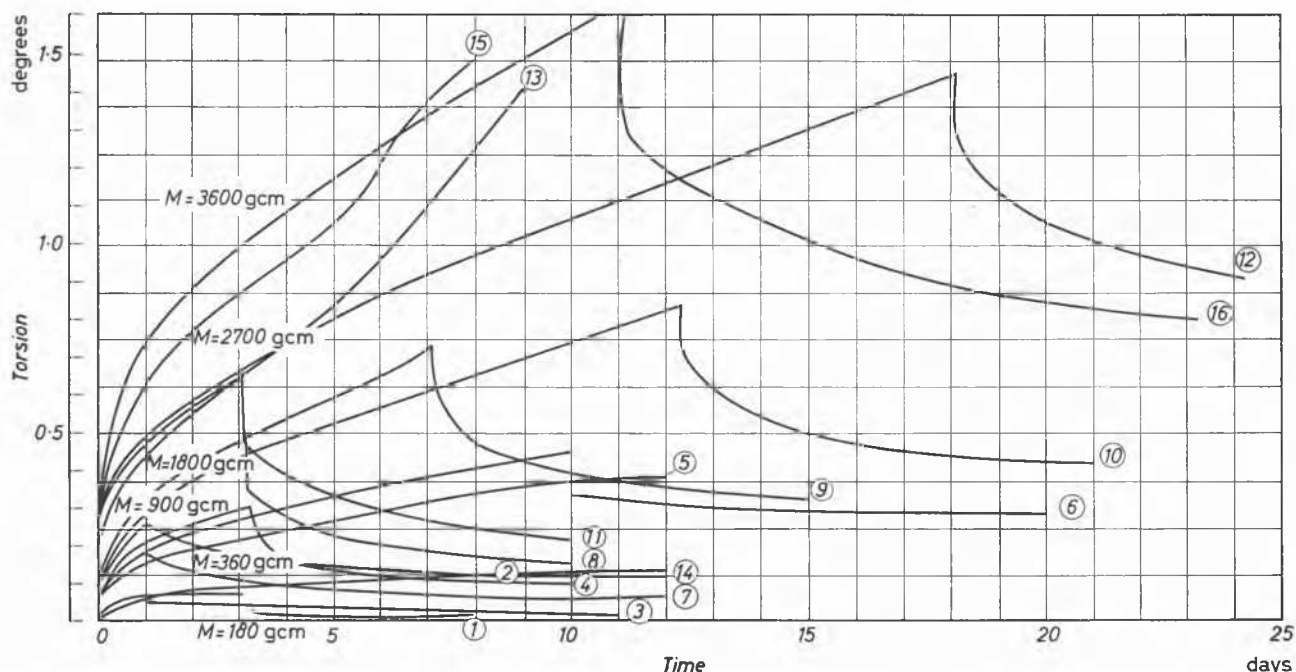


Fig. 6 Deformation/time diagram
Graphique temps/déformation

possible that the substances remain statistically equivalent in spite of the change of spatial configuration of the clay particles. In this stage of the deformation process it is possible to measure reproducible physical quantities, and the stress-strain relationship can be formulated in operator forms and integral equations.

When the stresses become too large and exceed a certain value, called f_3 , then structural disintegrations occur and may lead to failure. In this last stage of the mechanical process a mathematical description becomes too intricate.

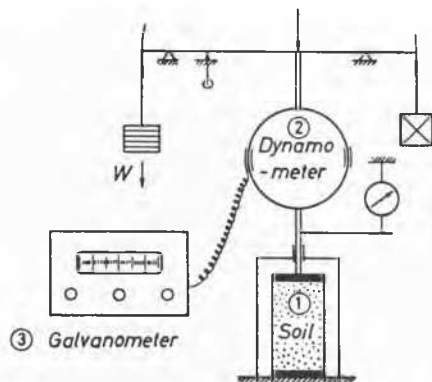


Fig. 7 Scheme compression plastometer
Schéma d'un plasticimètre à compression

In the case of unfrozen clays, S. S. Vialov and A. M. Skibitsky have measured decelerating creep. In this respect I wish to remark that these scientists have possibly measured a complicated inter-relationship between the following time effects: (1) time effect due to shear; (2) time effect due to consolidation; (3) additional time effects due to the change in state of stress; and (4) additional time effects due to hardening processes, which may have a large influence in their experiments.

In the case of fat clays continuous flow has been measured by

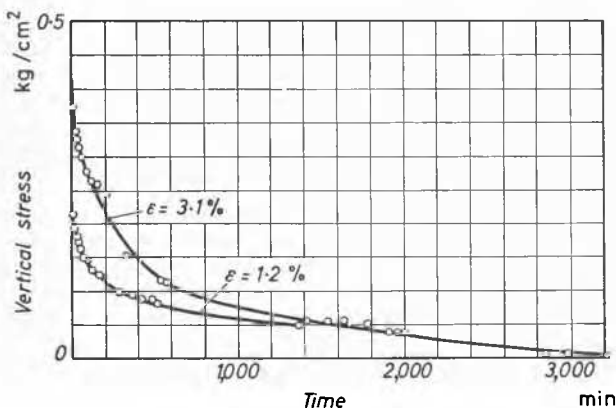


Fig. 8 Relaxation of stresses
Relaxation des contraintes

Haefeli, Geuze and myself, and in order to provide further confirmation of this experimental fact I have studied the relaxation of stresses of cylindrical samples under constant deformation. The phenomenon of continuous flow is supported by the fact that the stresses relax and even may decrease to zero.

In the apparatus shown in Fig. 7 the soil is given constant deformation and the relaxation of the internal stresses is measured. The result is given in Fig. 8, which shows that the stress decreases with time and may even decrease to zero.

NOTE: For the significance of the yield-values f_2 , f_3 see for instance: Geuze, E. C. W. A. and Tan Tjong-Kie (1953), *Proc. 2nd Int. Congr. on Rheology, Oxford*, p. 247. London; Butterworth.

T. W. LAMBE (U.S.A.)

First I should like to compliment I. Th. Rosenquist on an excellent job of reporting. He has emphasized the importance and requested that we discuss moisture in fine-grain soils—the role of this moisture in soil behaviour and its movement. Our Soils Group at M.I.T. has studied the nature of moisture in clay and I should like to summarize some of our present thoughts.

Our adsorption data agree with the results of the heat of wetting data cited by the General Reporter. These data indicate a layer of orientated water next to the mineral surface, the water molecules being held by strong electrical forces from stray charges in the mineral and from the exchangeable ions; the adsorbed layer is, however, very thin. For example, our results on kaolinite show this water layer to be about 2 molecules thick and about 1 to 2 per cent in water content. Beyond this adsorbed layer essentially all the moisture in a natural clay is within the colloidal diffused double layer, and its behaviour follows the Gouy-Chapman theory.

In pure clays I feel that shear strength is due to attractive forces between mineral particles; therefore, in my opinion there is no fundamental difference from the point of view of mechanism between friction and cohesion. Since the strength appears to be due to attractive forces it is directly related to the contact area between particles and, inversely, is related to the distance between these particles: both depend on intergranular pressure. If we increase that, we increase the contact area between the particles; this increases the strength and we decrease inter-particle spacing; this increases shear strength. If we change the intergranular pressure by reducing it, I do not believe that the changes in orientation are reversible; therefore its contribution to shear strength is reasonably permanent. Particle spacing is I think generally related to intergranular pressure and if we relieve that its strength decreases.

Clays desire moisture to satisfy the adsorption tendency of this very thin layer and to complete the double layer; they will therefore suck in moisture and will fully develop their double layer, which means a reduction of the ion concentration in the double layer. The osmotic concept is in complete agreement with this concept. As clays take on moisture by adsorption the particles move farther apart and the strength goes down.

I question the widely held concept of water bonding, that is, attributing cohesion to tension in the water. During desiccation I feel that water tension plays a vital role, and I do not question that; I do, however, question that the clay strength is due primarily to tension in the water film and in support of the contention that this concept is erroneous I would raise two points. First, we have seen cohesion in clays even where we have measured positive pore pressures and, secondly, the highest cohesion is when the clay is completely dry. The water content is therefore zero and we cannot attribute the cohesion of the strong clay to the water.

With regard to moisture in frozen soils, as the soil freezes it takes moisture from the double layer. This results in an increase in ionic concentration and water flows in from the bottom to satisfy this condition. It is osmotic pressure. I believe that moisture transfer mechanism in freezing soil is the same as that which occurs when soil swells.

J. A. J. SALAS (Spain)

I should like to draw your attention to the discrepancy between two conceptions which are sometimes used as equivalents or as if directly related, namely, the amount of fixed water and the film of adsorbed water.

The expression 'film of adsorbed water' is often employed in a very loose fashion, confusing the actual layer of adsorbed water, which is the external sheet of the Helmholtz double layer, and the equivalent double layer. The first is diffuse and is

liable to give rise to misunderstanding when speaking about its thickness. The second is a mere convention—sometimes defined by the tangent to the Gouy potential curve at the point of intersection with the surface of the particle. The amount of fixed water is, on the other hand, related to the adsorption phenomenon; but it is also related to the roughness of the particle surface, with the terraced pattern of its ends and with its irregularities of shape. These dead spaces caused by the roughness of the surface of the clay may quite possibly be one of the causes of the secondary compression, and this leads us to a consideration of the nature of the contacts between the particles of the clay.

During the last few years there seem to have developed two lines of thought on the nature of these contacts. They are sometimes called the 'Effective Contact School' and the 'Wide Spacing School'. In my opinion these definitions are completely inadequate. On the one hand they suggest that those who defend the second school consider that there is no effective contact between the particles. Without any doubt there exists an effective contact between the particles of clay as there exists transmission of stresses. For example, the contact between the crankshaft of an engine and its bearings is effective—very effective, but not direct: there exists between them a layer of lubricant to be taken into consideration.

From these deductions I would propose that the following definitions of 'direct contact' and 'effective gap' might be more proper, considering that the second school characterizes itself by the importance it gives to the study of the gaps believed by it to exist between the particles. This school is not sure they deserve the designation wide or narrow, but believes, although not being 'as deep as a well or as wide as a church door', that they embody a major part of the secret of the compressibility of clay, also of its cohesion and that of its shearing strength.

Those who, until now, have worked in the demonstration of this idea have endeavoured to simplify the phenomenon to the extreme: they must be excused for that as it is the reasonable way of the experimental method. Nonetheless, they never pretended that the results of their tests were representative except of only one part of the behaviour of a natural soil: this assertion is clearly shown in all their studies. For example, to take one of the most typical, G. M. Bolt's article, published in *Géotechnique* in June 1956; the title itself states that the subject to be discussed is 'Physicochemical analysis of the Compressibility of Pure Clays'. In the study which Serratosa and I made and presented at the Zurich Conference, we employed pure and homoionic clays which had previously been extracted and electrolysed so that the maximum size of the particles was 1.2μ . The problem of the behaviour of natural soils is evidently different, but when studying this it is important that we should have a knowledge of the behaviour of the pure clays.

A promising way for relating one with the other was shown by Karl Terzaghi, in a letter to the Editor of *Géotechnique* in December 1956. I daresay that in this problem, as well as in that of the permeability and other related problems, the observation of the structure of the soil can be very usefully made with the techniques and concepts of W. Kubiena, late Professor of the Landwirtschaftsschule of Vienna. To refer only to an essential division, Kubiena distinguishes between two types of structures, the 'pectic' and the 'peptic'. In the first the clay is coagulated and forms clusters of the shape described by Terzaghi in his letter to *Géotechnique*. In the second one the clay is peptized and covers the coarse particles of soil with a continuous film through which they make contact with each other. Within each of these basic classes of structure Kubiena makes other subdivisions, the study of which can be very important to the subject of our discussion.

E. C. W. A. GEUZE (Netherlands)

Referring to the General Report, I should like to make some comments on the suggestions concerning the movement of water in relation to creep and the long-term or secular settlements. The choice of both subjects is significant, and it tells us quite a good deal about the General Reporter's point of view concerning the importance of physicochemical properties of fine-grained soils containing clay minerals. It may be of interest to him to know the Dutch attitude towards the related mechanical properties of these soils as expressed by Keverling Buisman in 1939, and I should like to quote some lines from his book *Grondmechanica*:

Modern clay research has led us to assume the water molecules next to the clay mineral particles to be directed towards these and to be bound to these by electrical forces; those at greater distances would be less strongly bound than those next to them. The latter would be so rigidly fixed that this absorbed water would behave as a rigid or at least as a fluid of extremely high viscosity. In trying to visualize the process of squeezing out water from the pores, as will occur in a loaded, saturated mass of clay or peat, the settlement diagrams (obtained from consolidation tests) lead us to the conviction that the highly viscous fluid is subject to a plastic deformation and that the so-called 'free water' is thereby expelled from the pores, followed by a slow creep with gradually decreasing speed. It is quite possible that free water will be enclosed by zones of high viscosity and therefore will have to overcome a high resistance before it will be squeezed out.

In this way the late Keverling Buisman was the first investigator to study the remarkable phenomenon of the secular consolidation effect following from his long-term consolidation tests, and he was also the first who tried to explain this phenomenon about 20 years ago on the basis of the mutual forces acting between the soil particles and the pore water.

On the strength of this hypothesis I tried an indirect approach to the problem by measuring pore pressures at the bottom end of a consolidation sample, an enclosed volume of water being used as the pressure medium. These measurements were carried out at a high precision and utmost care was taken to prevent any movement of water from or into the bottom of the sample. A great number of these tests were carried out and the results led to the following conclusions: First, secular settlements will occur after the pore pressure built up by a step-load has decreased to the lowest measurable limit, that is, to about 0.2 per cent of the applied vertical stress. Secondly, following step-loads will build up maximum pore pressures of decreasing magnitude. Thirdly, the higher the initial density, the maximum pore pressure will accordingly decrease. These findings indicate that movement of water may occur at extremely low rates and be maintained for considerable periods of time, as for instance when measured over 1 year at hydraulic gradients, which could hardly be realized in an ordinary permeability test.

The second point, however, is even more significant. If the maximum pore pressure remains below the applied stress, this means that part of the load is not transmitted to the pore water. Therefore I had to assume that either one of two main causes or both would be responsible for this behaviour: (a) frictional forces activated between directly contacting coarse particles; and (b) viscous forces activated in and between conglomerates of clay mineral particles.

As the material used for the tests contained both constituents, clay mineral particles being present in a quantity of about 20 per cent of the dry weight, I was led to assume that both elements would be responsible for the residual inter-particle forces. Some indication as to their respective effects could be derived from the build-up of pore pressure. A sudden drop of pressure would be registered directly after the load had been applied, followed by a gradual increase of pressure lasting for more than one hour. This behaviour could well be ascribed to the combined effect of the frictional and viscous forces, activated by a

re-arrangement of particles at a relatively high rate during the initial phase of compression.

At higher initial densities this phenomenon becomes less pronounced as greater frictional and viscous forces are mobilized in order to re-arrange the particles. However, it would be of considerable practical importance if we were able to determine the nature of these residual stresses, considering their effect on the mechanical behaviour of natural deposits.

In this connection the General Reporter has raised a point of considerable interest on the nature of the bound water. According to his view it would behave as a 'pseudo-plastic' material, that is, as a plastic material without a definite yield value. From the findings of Tan Tjong-Kie and myself, which were included in my General Report at the Zurich Conference, it would follow that the behaviour of the clay material is characterized by a yield value insofar as a perfect elastic behaviour is found below a certain shearing stress of some tenths of g/cm² in a remoulded clay. The elastic range is followed by one exhibiting increasing deformation at repeated shear-stress loads, but still lacking viscous properties, and finally a purely viscous range. As our tests were carried out in such a way that deformation occurred under the influence of a pure shear-stress system, the occurrence of a perfectly elastic behaviour indicates the existence of bonds of an elastic nature.

On the basis of the concept put forward by the General Reporter, the number and strength of these bonds should increase with increasing density. The results obtained by pore pressure measurements in consolidation tests supply indirect proof of this phenomenon, as less pressure will be transferred to the 'free' pore water at increasing densities. As an increased number of contacts between the coarse-grained particles may lead to the same effect, I have started investigations into very fine-grained artificial materials, including the effects of the nature of the fluid and its electrolytes. The usual consolidation tests are not suitable for this purpose, as shearing stresses will occur at zero-lateral deformation, therefore, the tests are conducted at an all-sided equal pressure.

We have to consider the fact that although we may exclude the occurrence of shearing stresses for the material in bulk, the resistance of the material to pure compression will still largely depend on the shearing resistance of the bonds between the particles. Proof of the fact that shearing deformations will occur in a consolidation test as well as compression in a normal shear-test was given in my paper to the 1948 Conference.

R. PARRY (U.K.)

Some of the properties of normally consolidated clay observed by P. L. NEWLAND and B. H. ALLELY (1a/20) may be due to pore pressure behaviour. If a sample is normally consolidated in the laboratory and tested undrained in the triaxial cell, an increase in pore pressure occurs. For some clays this pore pressure is still increasing at failure. This is true for London clay which is an active clay and has a PL of 26, a LL of 76 and a clay content of about 50 per cent; whereas in the case of Weald clay, which has a PL of 20, a LL of 46 and a clay content of about 40 per cent, there is very little pore pressure change at failure.

If a sample is isotropically consolidated under external stresses σ_c' and loaded undrained to failure there is a pore pressure increase of Δu_1 . If it is then unloaded there is a further pore pressure change Δu_2 which is usually small and may be positive or negative. After unloading the external stress condition is identical to that after consolidation. At the same time, however, there is a high positive pore pressure. If further consolidation is permitted, this pore pressure will dissipate giving a lower water content. Therefore, by doing an undrained test in this way the sample can become in effect

over-consolidated. If on the other hand, the sample is reloaded to failure directly after unloading, a further increase in pore pressure occurs. It becomes greater than its previous failure value, as shown in Fig. 9. A similar diagram to this was given in 1953 by Bishop and Henkel.

If we consider the Mohr-Hvorslev failure criterion, the compressive strength for a particular water content may be expressed

$$\begin{aligned}\sigma_1 - \sigma_3 &= A + B\sigma_3' \\ &= A + B(\sigma_c' - \Delta u)\end{aligned}$$

where A is a constant containing the true cohesion and friction parameters and B is a constant containing the true friction parameter. As the pore pressure is higher on reloading, it follows from this equation that the strength will be lower. It is

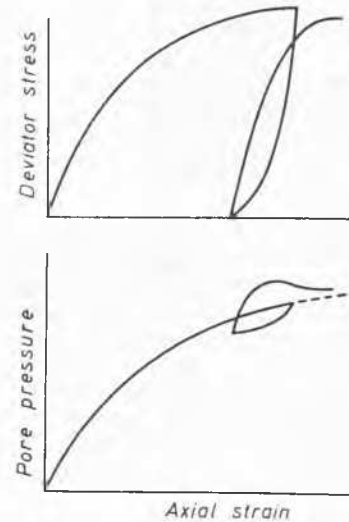


Fig. 9

probable that each successive unload-reload cycle gives a small and progressively decreasing increase in pore pressure and hence a drop in strength. A drop in strength is also observed if deformation of the sample is continued past failure during the first loading. This is due to the progressive increase in pore pressure.

The process of remoulding is one of repeated loading and unloading, so that a progressive drop in strength occurs. The sensitivity for a completely remoulded sample may be expressed as follows:

$$S = \frac{A + B(\sigma_c' - \Delta u_1)}{A + B\left(\sigma_c' - \sum_{r=1}^{\infty} \Delta u_r\right)}$$

where Δu_r is the pore pressure increase during any loading or unloading. Probably the same reduction in strength would occur if a sufficiently large shear strain was applied during the first loading. Although these principles have been illustrated for isotropic consolidation and triaxial loading, they apply equally to one-dimensional consolidation and vane testing.

H. M. RAEDSCHELDERS (Belgium)

It was with much interest that I read the paper of R. GANDHAHL and W. BERGAU (1a/8) concerning the measurement of the frozen zone in soil. I think that the principal merit of both proposed methods is in the simplicity of the apparatus. In this connection I should like to discuss the method used at the Belgian Government's Institute of Soil Mechanics.

The observations comprise measuring the temperature in the soil at different depths which makes it possible to follow very closely the rate of penetration of the frost, and to determine at each moment the 0°C . level. Use is made of small electrical carbon resistances, which are in common use in radio practice. These resistances have a negative temperature coefficient (high resistance at low temperature and small resistance at high

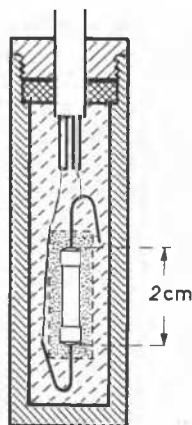


Fig. 10

temperature). The resistances have to be hermetically enclosed in a brass tube and insulated from the tube and the surroundings by oiled paper and bitumen (Fig. 10). The type used for our tests has a resistance at 20°C . of about 11,000 ohms. At -10°C . a change of 1°C . corresponds to a change of electrical resistance of about 1100 ohms, and a change of 1°C . at a temperature of $+20^{\circ}\text{C}$. corresponds to a change in electrical resistance of about 400 ohms. Since the maker gives no information on the subject each gauge has to be

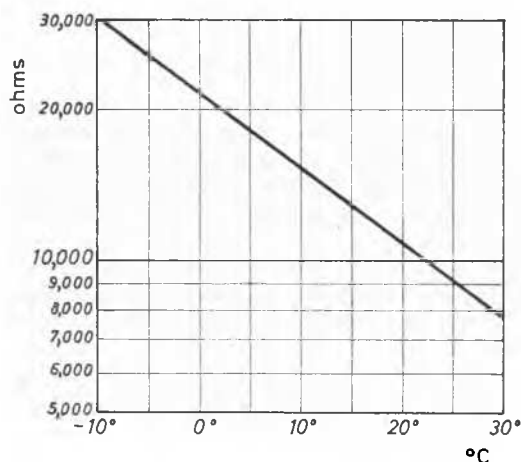


Fig. 11

calibrated. The variation of resistance as a function of temperature is indeed different for all gauges, but for each of them it is nevertheless strictly constant with time.

An example of a calibration curve is given in Fig. 11. In the observed range of -10°C . to $+30^{\circ}\text{C}$. we found a linear relationship between the logarithm of the electrical resistance and the temperature. The variation in resistance is measured with a simple Wheatstone bridge and a practical accuracy of 0.1°C . is obtained. The apparatus is very cheap, one electrical resistance costing only about 8d.

An example of an observation performed by the Government Institute of Soil Mechanics is given in Fig. 12. It represents the temperature of the subgrade under a concrete

slab on the experimental section of the Highway Brussels-Ostend at Hekelgem. The concrete has a thickness of 20 cm. Curve 31/1/1955 represents the temperature of the subgrade before a freezing period. The curve 18/2/1955 shows the beginning of a long period of cold weather in which temperatures reached about -4°C . under the concrete shown by curve 24/2/1955. The period of thaw began on 27/2/1955, and

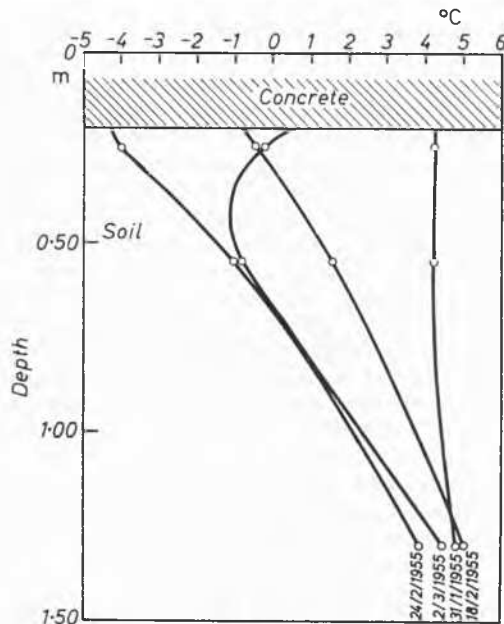


Fig. 12

the curve 2/3/1955 shows the temperature in the subgrade after 4 days.

I think that the simplicity of our measuring method will permit complete information to be obtained concerning the penetration of frost into the soil as a function of time and depth, for some very interesting problems under investigation.

N. A. TSYTOVICH (U.S.S.R.)

Experiments have shown (TSYTOVICH, 1947; ANANYAN, 1952; ROSA, 1952; Collections 1, 2 and 3) that any dispersed frozen soils, such as frozen clay, contain some, and at times considerable, amounts of unfrozen water in dynamical equilibrium with external factors, e.g. temperature, pressure, as shown in Paper 1a/28.

There is a quite definite relationship between the amount of unfrozen water and the degree of negative temperature, which can be expressed by means of curves characteristic for each type of soil (Fig. 1 in 1a/28). The influence of the negative temperature can be judged from the following. According to our experiments (Collections 1, 2 and 3) pure sand at -0.2°C . contains only about 0.3 per cent unfrozen water, while sandy clay has 18 per cent, and clay 42 per cent. With a temperature of -10°C . in the same soils there was found only one-half that amount of unfrozen water. It should be noted that even with a temperature of -10°C . frozen clay was found to contain more than 20 per cent of dry weight of unfrozen water which is important when evaluating the physical and mechanical properties of this type of soil. It should be also noted that dispersed clay contains unfrozen water even at the temperature of liquid air.

As the temperature increases the amount of unfrozen water contained also increases. This is caused by the melting of ice in the pores at a temperature below freezing point which is a very interesting and important fact.

The magnitude of the outside pressure manifests itself in a pronounced way in the region of intensive phase transformations of water into ice at temperatures from zero to -5°C . Thus, according to our experiments (TSYTOVICH, 1947) at -1.7°C clay contained 42 per cent unfrozen water, but when subjected to a pressure of 2 kg/cm^2 it held 58 per cent at the same temperature. With high density soils this effect decreases, which is explained by the greater cohesion of pore water in these soils.

As the following investigations have shown (Collections 1, 2 and 3) the reasons why the water does not freeze in the pores of dispersed soils are as follows: (a) electromolecular attraction between mineral particles and water molecules which increases the thinner the layer of pore water; (b) ionic attraction of water molecules in the diffusion layer which appears only at low temperatures of up to -0.5°C ; and partly (c) capillary effects at temperatures near zero of the order of -0.1°C . The most important of these is the orientation and attraction of water molecules by mineral particles of soil.

When the amount of unfrozen water in frozen soil and the external factors are no longer in equilibrium excess pressures are produced which cause movement of water. One cause of the appearance of these excess pressures is the temperature gradient governing the film mechanism of movement of unfrozen water towards the freezing boundary where the films of water, as part of these turn into ice, become thinner and have a greater force of attraction. The increase in the moisture content in frozen soils on further cooling has been established in our laboratories (TSYTOVICH, 1947) as well as in the field by Tyutyunov, according to which the moisture content for frozen sandy clay between December and April at a depth of 35 cm increased by 15.3 per cent of the total weight.

The paper by our collaborators 1a/29, Fig. 2a, gives convincing data on the displacement of water under the action of pressure gradients.

As shown by ANANYAN (1952) and IVANOV (Collections 1, 2 and 3) the movement of unfrozen water takes place under the action of the difference of potential of an electric field, which may be utilized in electro-osmotic dewatering of frozen soils. Experiments further show that water movement takes place at a definite magnitude of the acting pressure or of gradient of pressures which we shall call the 'initial' gradient. If the gradient, be it of temperature, pressure or electro-osmosis, is less than the 'initial' gradient then there will be no water movement.

The importance of the initial gradient of pressure in unfrozen soils was first noticed by N. Pusyrevski in 1934 and later by ROSA (1952). Investigations concerning frozen soils were carried out in our laboratories (TSYTOVICH, 1947; ANANYAN, 1952; Collections 1, 2 and 3). According to our experiments it appeared, for instance, that when freezing dense KINEL clays with a temperature gradient of $-1^{\circ}/\text{cm}$ there was no water movement, while with a temperature gradient above $-5^{\circ}/\text{cm}$ there was intensive movement towards the freezing boundary producing frost agglomerations of soil.

It should be noted that for light soils the magnitude of the initial temperature gradient causing water movement is very small. Furthermore, electro-osmotic movement of water in frozen soils also comes about only with a certain initial value of the electric field gradient (ANANYAN, 1952; Collections 1, 2 and 3).

Such experiments show that the velocity of water movement is very small, and the coefficient of water penetration or filtration is of the order of 10^{-12} cm/sec .

The results now being developed have a very important practical application in the estimation of the depth of the frozen soil and of its thawing out when the transformation of water in the whole volume of soil masses being frozen or thawed out must be taken into account, as well as in the

evaluation of mechanical properties of frozen soil with special reference to its liability to sliding under load.

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D. M. MILOVIĆ (Yugoslavia)

I should like to compare the findings of A. J. L. BOLOGNESI and O. MORETTO (1a/3) with some of my own results, obtained on Yugoslav loess.

Paper 1a/3 contributes to better knowledge of loess properties, showing that there can be very different characteristics, especially concerning its strength and potential possibility for settlement.

In Yugoslavia there are also a great deal of such loess materials with different properties as regards their strength and settlement, caused by different geological conditions under

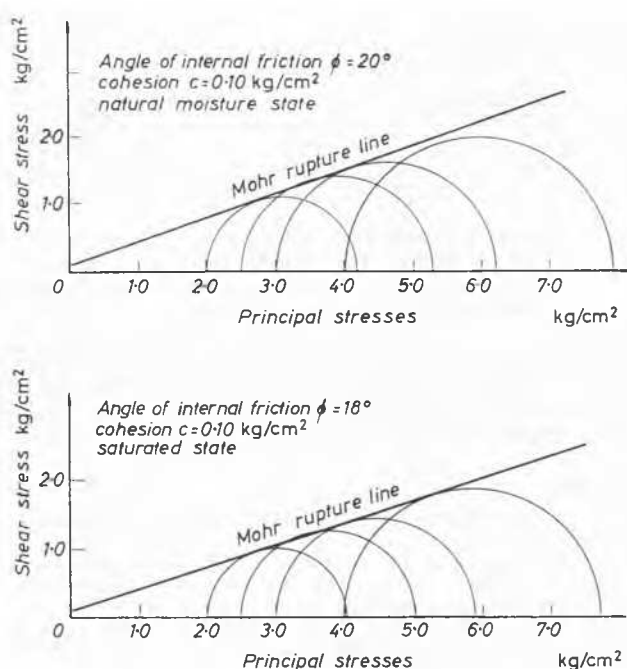


Fig. 13 Mohr diagrams for triaxial drained consolidated compression test—Belgrade land loess

Graphiques de Mohr pour un essai de compression triaxiale avec drainage et consolidation

which they were transported and deposited—such as 'land loess' and 'swamp loess'.

According to my research on Yugoslav loess, which has similar or even the same properties as the loess mentioned in Paper 1a/3 (with content of about 10 per cent of fine sand particles and 10 per cent of clay particles, with natural moisture of about 20 to 22 per cent, LL 30 to 40 and natural porosity 1.05 to 0.98), the following values were obtained: (1) angle of internal friction $\phi = 20^{\circ}$ and cohesion $c = 0.10\text{ kg/cm}^2$ on an undisturbed sample in natural moisture state, consolidated and drained; (2) the angle $\phi = 18^{\circ}$ and cohesion $c = 0.10\text{ kg/cm}^2$ on an undisturbed sample in saturated state, consolidated and drained.

In Casagrande's shear box the angles are somewhat bigger while the cohesion is practically the same (Fig. 13).

Consolidation tests carried out on our loess in the Institute for Testing Materials and Constructions in Belgrade have shown a good agreement with the results obtained by Bolognesi and Moretto, i.e. that even at considerably great natural moisture ($w=23$ to 24) but at minor porosity in the natural state (0.77 to 0.81), settlements are not too high (Fig. 14, curve 2).

Generally speaking, one may conclude that the settlements of the loess, under the two simultaneously fulfilled conditions, that is, both at low density, in other words at high porosity, and at saturation, have considerably great values.

On the other hand, I should like to stress the necessity of distinguishing loess materials which are and which are not dangerous with regard to settlement, and to share the authors'

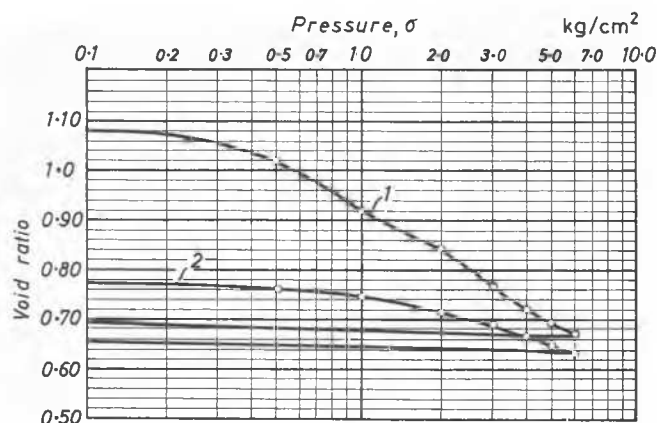


Fig. 14 Results of consolidation tests for two different kinds of loess (Belgrade and Zrenjanin loess)

Résultats des essais de consolidation pour deux types de loess différents (Belgrade et Zrenjanin)

opinion that, for some kinds of loess materials the allowable pressure appears to depend only on the ultimate bearing capacity, settlements being unimportant when the load is limited by the introduction of the factor of safety.

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B. AISENSTEIN (Israel)

Dans leur article sur le loess en Argentine (1a/3), les auteurs écrivent que la dimension des grains était à peu près la même pour le loess éolien et le loess remanié. Or, nous avons aussi dans le Neguev qui est situé au sud d'Israël deux espèces de loess: le loess éolien et le loess remanié et déposés par des torrents. Nous avons remarqué que la coupe granulométrique a une forme différente pour chacune des deux espèces; elle est du type simple pour le loess apporté par le vent et du type composé pour le loess remanié. Dans ce dernier cas on a même pu calculer, d'une manière approchée les proportions relatives de loess primaire et d'argile alluviale qui y a été apportée.

Il semble en plus que les minéraux argileux ne sont pas les

mêmes dans les deux cas; l'illite prédomine dans le loess éolien tandis que la proportion de montmorillonite augmente dans le loess remanié.

Il serait intéressant de savoir si des observations analogues ont été faites pour les loess d'Argentine, ou de tout autre pays. De cette manière on pourrait voir s'il existe une relation entre l'origine géologique du loess, sa structure minéralogique et ses propriétés mécaniques.

F. L. CASSEL (U.K.)

I have a few remarks to make on the physical and engineering properties of chalk. As the authors of Paper 1a/16 say, very little has been published on the geotechnical properties of the chalk of southern England. This is not so surprising as most of the chalk at outcrops or at the depth of foundation occurs in the form of a rock, though a relatively soft rock. However, the safe bearing capacity of this chalk for the usual methods of construction is never much in doubt or, indeed, the factor of safety is very high.

From the engineer's point of view the variations which are due to the transformation of this soft rock into other types are, however, of the greatest interest, and they certainly merit more attention than has been given to them in the past. The hypothesis about the genesis of some of them (I suppose the explanations given in the paper for the formation of the putty chalk are put forward as a hypothesis) may be correct or not: I do not feel competent to pronounce on this aspect, but there are certain points which, in my opinion, are against it.

There is a third well distinguished type of chalk which is frequently encountered above the chalk rock. It is the rubble chalk, a loose accumulation of hard, well-rounded chalk particles of gravel size. These are probably re-deposited products of erosion of putty chalk, from which the clay-like matrix has been washed away.

The most interesting variation however, without doubt, is the putty chalk. Its thickness is by no means limited to the 30 ft. mentioned in the paper. I described in a discussion at the Zurich Conference (Vol. III, p. 173) two occurrences where the thickness of this formation over a large area exceeded 60 ft. and 70 ft., and the sound chalk rock had not been reached at that depth. Whether this can be brought into agreement with the frost-and-thaw theory or, alternatively, the pressure theory appears to be doubtful to me. Usually the thickness of the putty chalk is of course much smaller.

A. C. MEIGH and K. R. EARLY (1a/16) have omitted to mention in their paper the index properties of the plastic matrix, which forms, in cases of my experience, between 70 and 90 per cent of the total mass. Guthlac Wilson has given values for the putty chalk of $LL=27-28$ and $PL=21-24$ per cent. I have found similar values after sorting out all large hard particles as $LL=29-34$ per cent, $PL=25-29$ per cent, $PI=2-5$. A characteristic feature of the soil is that the water content of the hard rock-like particles, which appear dry, is nearly always higher by about 3 to 6 per cent than that of the soft, wet appearing matrix. The description in the paper as cohesionless calcitic silt is quite significant but not quite correct. I had compared it with a saturated loamy sand. In any case, in spite of the clay-like appearance and feel, it has very little cohesion and a high angle of shearing resistance.

It is always fully saturated, as is most of the chalk rock. I have described the compressibility as very low. The compressibility of the putty chalk was, however, about ten times that of the rock tested by the authors (mean $M_v=0.0044$ sq. ft. per ton) while Guthlac Wilson's values were again ten times higher (mean $M_v=0.04$). I have proved the low compressibility once on an outcrop near Reading by a loading test up to 6 tons per sq. ft. The settlement after a first elastic or plastic

adaptation at 2 tons per sq. ft. was nil. There is one feature which is very important, and is actually the reason I described this type of soil in Zurich and for my comments today. Piles driven into the putty chalk are liable to settle for an indefinite time under vibratory loads due to plastic deformation and dislocation of the matrix. For this reason it would seem that foundations on this formation should always be considered from the point of view of raft construction if the solid rock is out of reach.

As the same chalk formation covers a large region of northern France, I suppose our French neighbours must have had similar experiences when exploring the properties of this stratum. It would be of the greatest interest if some comment on the content of the paper and on the properties of putty chalk mentioned by me could come from their side.

T. K. CHAPLIN (U.K.)

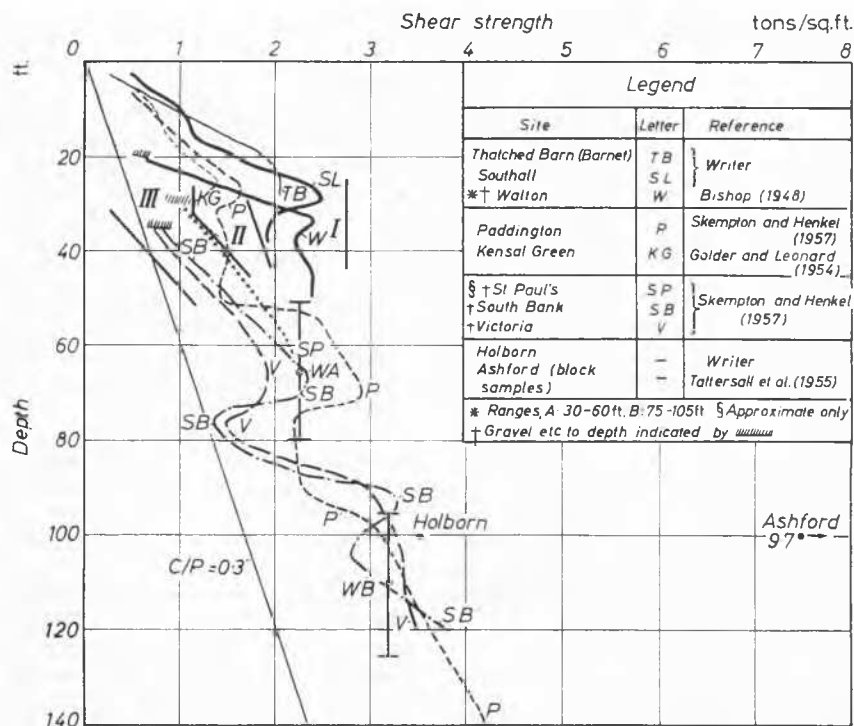
As the General Reporter has classed London clay as being of special geotechnical interest, may I discuss some points about its strength in relation to Papers 1a/25 and 1a/18 by A. W. SKEMPTON and D. J. HENKEL and J. MOUM and I. TH. ROSENQUIST.

First, at the sites where gravel overlies the clay, namely, Victoria, South Bank and St. Paul's in central London, and Walton, Middlesex (BISHOP, 1948a and b), the effects of softening by unloading and possibly weathering persist for roughly

30, 30, 30 and 13 ft. of clay respectively. The strength of the clay is low under the gravel and only increases slightly with depth. MOUM and the General Reporter in Paper 1a/18 show that weathering (oxidation) may actually strengthen clay by increasing its LL and PL at constant moisture content. Could this apply to the London clay? Since visible weathering—brown discoloration—is only visible at any of the sites for a few inches in the clay under the gravel, one suspects that weathering may perhaps not be very important; but could there be any other reason why the strength at Walton, only, rises so rapidly to a notably high value? In the lower reaches of the River Thames, as at Victoria, South Bank and St. Paul's, flooding during each winter would have lasted for a much longer period than higher up the valley as at Walton, and so there would have been some opportunity for softening to penetrate deeply. This would occur while the erosion surface of the London clay was exposed before the flood plain and terrace gravels were laid down. The depth of softening is unlikely to be related to the present depth of gravel, which is very variable locally.

Secondly, the effect of sampling disturbance has not been mentioned in Paper 1a/25 by A. W. SKEMPTON and D. J. HENKEL, although TATTERSALL, WAKELING and WARD (1955)* have shown that block samples from a tunnel may give three times the strength of bore hole samples from the same depth. Such clay may have a failure strain of $1\frac{1}{2}$ to 2 per cent, and if the

* See list of references



Zone	Area	Sites
I	North and west outskirts of London	Thatched Barn, Southall, Walton, Ashford
II	Inner north-west London	Paddington, Kensal Green
III	Central London	St. Paul's, South Bank, Victoria, Holborn

Fig. 15 Strength profiles of London clay from bore hole samples

Résistances de la London clay en fonction de la profondeur, d'après échantillons obtenus par forage

bore hole sampling causes as little strain as that, no imagination is needed to see why bore hole samples are much weaker than block samples.

Comparing the strengths at sites mentioned by Skempton and Henkel with other sites at Walton, Southall, Barnet and Kensal Green, I have found that the strength at about 35 ft. below ground level increases steadily with greater distance away from central London, that is, Victoria, South Bank and St. Paul's, through Inner London—Paddington and Kensal Green, towards the north and western outskirts of London—Barnet, Southall and Walton. The reloading and absence of weathering associated with gravel terraces appear not to affect the order.

Thirdly, the measured strengths of locally weak zones at certain depths in central London are only just over the calculated $c/p=0.3$ values for normally consolidated clay. It may be noted here that the lower part of the Paddington, Victoria and Thatched Barn curves roughly correspond to an increase parallel to the $c/p=0.3$ line from a shear strength of perhaps 1.7 ton/sq. ft. at 25 ft. depth. This last, by coincidence, happens to be the average strength for the four sites where no gravel overlies the London clay, i.e. Southall, Thatched Barn, Paddington and Kensal Green.

Fourthly, the drop in strength which occurs at four sites out of seven when a shear strength of 2 to 2½ ton/sq. ft. is reached, and when about 1½ ton/sq. ft. is reached at the other two.

Fifthly, at Paddington, Victoria and South Bank, a sudden drop in shear strength occurs between about 80 and 85 ft. depth. For Victoria and South Bank this is the same feature as is noted in the previous paragraph.

Finally, the comparatively even increase in shear strength until the first peak is reached. This reflects the weathering (which is almost absent under the gravel terraces) and softening, perhaps also the infiltration of surface water and water from the chalk below under artesian pressures. It is remarkable that, although the Victoria and South Bank borings showed very little brown (weathered) clay overlying the 'blue' (unweathered) clay, the strength there near the clay surface is still low. Evidently the influence of weathering as such is still small by comparison with the hysteresis loop for strength on loading and unloading, demonstrated by BISHOP (1948a, Fig. 2) and again by Skempton and Henkel.

May I also draw attention to the close relation between Paper 1a/11 by P. B. HEIDEMA and Paper 6/6 by L. BERNELL. They both agree that measuring shrinkage is better than measuring the PI directly in routine tests on soils of low PI. Such tests are suggested as most suitable to soils of PI below 30, which corresponds to a bar-linear shrinkage of about 16 per cent. Even without a correlation for an individual soil, Heidema's results show that over half the sites give mean PL within ± 4 per cent of the equation

$$PL = \frac{\text{Bar-linear shrinkage}}{0.55}$$

that is, within a PL error of ± 1 at PI of around 25. Of course, there is a correspondingly lower error at lower PI.

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A. G. STRMAC (Yugoslavia)

I should like to comment on Paper 1a/9 by H. J. GIBBS and W. G. HOLTZ and to congratulate the authors on their valuable and practical contribution which has helped to fill a gap which has existed for quite a long time.

We have ourselves experienced the deficiency of the standard penetration test while working, that is, testing strata almost devoid of any overburden pressure. This was particularly true in one case where we were called in to give our opinion on the foundation of a pavilion on the new fairground at Zagreb. The building had to be erected on a layer of fine to medium sand which was some 3 to 4 m thick, and in some places thicker. We considered the standard penetration test to be the most appropriate and fastest method of obtaining results, and to obtain the desired information which would provide some quantitative results to be used in the determination of the allowable pressures.

We carried out the standard penetration test from the excavation, which meant that there was practically no overburden pressure present. Although we considered the sand layer to be in a fairly dense state, according to our visual inspection, the number of blows of the standard penetration test was very low—some 2 to 5 blows per foot, indicating a layer in a very loose to loose state. Judging these results to be illogical, we made a few plate bearing tests which produced moduli of elasticity of 250 to 400 kg/cm². Since these results were in agreement with that which we could see and judge, we relied on them and based our final report on them.

In Fig. 7 of their report GIBBS and HOLTZ have clearly shown that the overburden pressure plays a very important role indeed, and considerably influences the results. The Terzaghi and Peck curve gives very conservative results for layers without or with a small overburden pressure. Our plate bearing tests have also shown and proved the tested sand layer to be in a much denser state than that indicated by the standard penetration test.

The new relationship between penetration resistance and relative density for cohesionless sands established by the authors will certainly help and contribute to the better and more realistic application of the standard penetration test.

J. K. ALDERMAN (U.K.)

I want to direct my comments to Paper 1a/9 by H. J. GIBBS and W. G. HOLTZ on the calibration of the standard penetrometer.

This paper is valuable in indicating what variables affect our results; but it is rather surprising to find such a great disagreement between the normal curves given by Terzaghi and Peck and those derived by the authors, particularly for very low overburden pressures. I wish, therefore, to bring to the notice of the meeting the results obtained from two recent field investigations.

From a number of field tests on a sand and fine gravel stratum it was found that within 5 ft. of the surface values of N greater than 35 were obtained. According to the figures given by Gibbs and Holtz, however, this is impossible, as the value of N for a sand of maximum density is given as 20 at the surface rising to 35 at a depth of 15 ft. We have, therefore, an example where the curves by Gibbs and Holtz do not apply.

In a second investigation, when studying the relative density of a sand at a depth of 10 to 15 ft., values of N greater than 100 were obtained; this sand had a grading similar to the one used by the authors. These results are in complete disagreement with those given in the paper, and in view of this discrepancy it is as well to examine the variables introduced by the authors, to see whether some other point has been missed.

A large percentage of our sands and sand deposits have been over-consolidated during their geological history, and therefore the present overburden is much lower than at previous times. This point has not been studied by the authors, and I suggest that it is a possible explanation of the difference between the laboratory tests and the field results. Before any value for N can be reasonably estimated we should know the over-consoli-

dation ratio. This fact has been confirmed by a number of tests carried out at Manchester during which we calibrated a slightly smaller type of dynamic sampling tube. It was found that not only did N value vary with overburden pressure, but it also varied with the over-consolidation ratio.

A. J. DA COSTA NUNES (Brazil)

Monsieur le Président, Messieurs: nous avons entendu, aujourd'hui, une série de communications à propos de la résistance à la pénétration normalisée de l'échantillonneur qui a déjà été étudiée d'une façon assez complète par Messieurs Terzaghi et Peck.

Au Brésil, nous avons fait procéder à des sondages par des firmes spécialisées et, avec les mêmes équipements, on trouve quelquefois, dans les mêmes terrains, des résultats discordants. Nous avons constaté, à plusieurs reprises, que la façon d'exécuter les essais de pénétration avait une importance assez considérable. Nous avons même trouvé des différences d'environ 50 pour cent dans la résistance à la pénétration suivant la vitesse de l'essai. Si on commence à compter les coups après avoir pénétré à une certaine profondeur ou si on commence tout de suite, on obtient des différences de résistance à la pénétration assez marquées.

Notre expérience nous permet donc d'affirmer que les essais de pénétration normalisés ne sont pas encore assez normalisés. Je vous remercie.

D. CRONEY (U.K.)

I would like to reply to one point made by T. W. Lambe. He said that he doubted if there was any relationship between shear strength and soil suction or tension, and he added that the fact that an oven-dry soil without any moisture content had a very high shear strength supported this view.

I am sure that Lambe is fully aware that the tension or suction of soil water increases rapidly as that soil dries, and has a very high value at oven-dryness, which could well account for the high shear strength. A soil dried arbitrarily at 100° to 110° C. contains sufficient water to provide these high suction forces.

G. STEFANOFF (Bulgaria) (read by the General Reporter)

In most countries the LL is being determined according to Atterberg's method by means of A. Casagrande's apparatus. This method has proved to have essential advantages: (1) precision, (2) simplicity of apparatus and manipulation, and (3) results are little dependent on human errors. There are, however, certain disadvantages.

First, it is inconvenient and even unfit for exceedingly sandy soils. With the latter, in order to cut a groove, the spattle has to overcome a shearing resistance in the soil which is greater than the adhesion and friction between the soil and the metal surface of the brass cup, so that it carries away the whole soil sample. In this case the LL cannot be determined. It is usually accepted equal to the PL, that is to say, the $PI I_w = 0$. In most of these cases the PI is indeed small, although not necessarily equal to zero. I have established that due to the above-mentioned reason the LL of 6.3 per cent of nearly 3000 successive samples (BRENNER and LOHMEYER, 1938) has not been determined. Only in the case of 36 per cent of the sandy loams (with 10 to 30 per cent clay fraction under 5μ) and 12 per cent of the loamy sands (with 3 to 10 per cent clay fraction under 5μ) could the LL be determined (STEFANOFF, 1955).

Secondly, after long service the spattle and the brass cup become worn, which leads to a change in the standard dimensions. If the height of the spattle is worn by 1 mm, the width of the groove increases from 2 to 4.1 mm. Experiments have

shown that this leads to an apparent increase of the LL by 15 per cent water content.

Thirdly, the closing of the groove and therefore the LL itself, as is well known, depends on the stiffness of the base (hard rubber plate on telephone directory and others).

In some countries (the U.S.S.R. and others) recently, in order to eliminate these disadvantages, LL is being determined by means of a standard cone. This method is similar to that of the Swedish cone, the difference being that the weight is fixed at 76 g, and the angle of the cone at 30° (VASSILIEV, 1949, 1953). The device was introduced by Vassiliev and its operation has since been improved. In the Bulgarian soil mechanics laboratories the cone shown in Fig. 16 is being used (STEFANOFF, 1955): with this device all the defects of the classical method are removed. The basic disadvantage of this method however is that it changes Atterberg's well known definition of the LL: the LL obtained with the cone being lower than that obtained by the classical method. It is necessary to



Fig. 16

know the exact relationship existing between the LL obtained with the two methods in order to be able to compare the results of the laboratory tests in different countries.

I have shown (STEFANOFF, 1955) that the relationship between water content and penetration of the cone is a logarithmic one, just as the relationship between water content and the number of blows. The result is that the relationship between LL determined according to the two methods is linear. This has been confirmed by 84 tests where the LL are between 28 and 90 per cent. An extremely close correlation between the results obtained according to the two methods has been established (Pearson's correlation coefficient (SMITH and DUNCAN, 1944) is $r = 0.964 \pm 0.024$). The equation of the straight line which expresses the relationship between the two LL's is:

$$L_w^A \approx 1.449 L_w^C - 7.4 \quad \dots (1)$$

or

$$L_w^C \approx 0.690 L_w^A + 5.1 \quad \dots (1a)$$

where L_w^A is the LL according to Atterberg,
 L_w^C is the LL obtained with the standard cone.

When we know one LL the other can be determined with an average error of 2 to 3 per cent water content.

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General Reporter

In my opening address I said that if we could bring clarity to the problems concerning the transport of water and the nature of water in the soils, this conference would be of greater importance to soil mechanics than most of the previous conferences. However, at the end of the discussion we have to conclude that there is still a great uncertainty about how water behaves. The discussion has given the impression that we do not know sufficient about the physical basis of this subject. There are great similarities but also some differences between the views held by T-K. Tan and E. C. W. A. Geuze and those contained in my general report. It seems obvious that further research is necessary and we cannot expect to reach a conclusion today. I hope, however, that what has been presented here will promote initiative at different laboratories, and that in the future we shall have further results in this respect. I think that the officers of the next conference will have a very difficult job in picking out the papers to be presented under the heading of 'Fundamental Properties'.

I should like to thank all those who have taken part in the discussion, and look forward to reading those contributions when they are printed.

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

I have followed the discussions of this morning with great interest. The General Reporter had expressed the hope that the discussions may lead to a clearer understanding of the physical properties of the water occupying very small voids. However, summarizing the discussion, he arrived at the conclusion that our knowledge of the performance of water in small voids is still in a rather hopelessly controversial state. This fact is by no means surprising, because the molecular interaction between water and a solid at the boundaries between these two substances is not yet clearly understood.

When I made the first consolidation tests on clay in 1921 I was impressed by the fact that the rate of flow of the water through the voids of the clay decreased much more rapidly with decreasing porosity than Darcy's law would lead one to anticipate. In order to account for this phenomenon I was compelled to assume that each mineral particle is surrounded by a layer of adsorbed water with a thickness of 0.2μ , the viscosity of which increases rapidly from that of normal water at the outer boundary of the layer to that of hard asphalt at the contact with the solid. The observed phenomena and my tentative explanation of their causes were published in 1924 in the *Zeitschrift für Angewandte Mathematik und Mechanik* (Vol. 4, pp. 107-133). In a paper which I presented in 1926 at the Fourth Colloid Symposium in Cambridge, Mass., I expressed the opinion that the abnormally high viscosity of the boundary layer may be the result of the dampening effect of the attraction of the molecules of the solid phase on the movements of the adjacent water molecules, comparable to the effect of lowering the temperature. With increasing distance from the interface the intensity of the dampening effect decreases and at a distance of 0.2μ from the interface it becomes imperceptible. This hypothesis appeared to be corroborated by the observation published by W. D. Harkins in 1921, that the

density of water contained in the voids of charcoal is very much higher than that of normal water. During the meeting which was attended by most of the leading colloid chemists of the United States of America, the paper was discussed, but no objections against my explanation of the observed phenomena were raised.

During the following three decades, concepts concerning the constitution of the adsorbed layer have changed several times. We know today that the interaction between solid and water is far more complex than I assumed in 1924, but a convincing explanation of the abnormal properties of the water contained in very narrow voids is not yet in sight. Therefore we have to postpone final conclusions concerning this subject, at least until the next International Conference.

A. BELLUIGI (Italy)

My contribution deals with the non-steady case of electro-osmotic dewatering and consolidation of soils.

In a saturated isotropic and homogeneous soil mass a direct electric current I is applied, which causes the water flow \bar{q} . During consolidation, a water loss Q per unit of volume takes place (zero in the case of the steady state).

In the succeeding deductions the symbols mean:

- I = electric current density
- \bar{q} = discharge per unit of area
- Q = water loss per unit of volume
- k = permeability coefficient (Darcy)
- k_e = electro-osmotic flow coefficient (Schaad-Haefeli)
- u = hydraulic pressure
- γ_w = specific weight of water
- ρ = specific resistance of saturated soil
- t = time

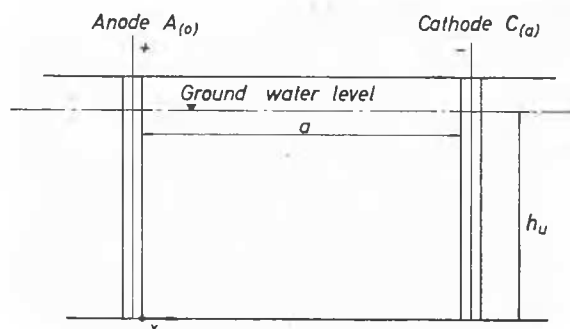


Fig. 17 Electro-geo-osmotic drainage (diagrammatic)
 Drainage electro-geo-osmotique (graphiques)

Assuming the following expressions to be valid:

$$\bar{q} = -k \cdot \text{grad} \frac{u}{\gamma_w} + \mu \cdot I, \quad \mu = k_e \cdot \rho$$

$$Q = \text{div } q, \quad \text{div } I = 0$$

$$Q = \frac{k}{c_v \cdot \gamma_w} \cdot \frac{\partial u}{\partial t}, \quad c_v = \text{Terzaghi's coefficient of consolidation}$$

there will be:

$$\Delta u = \frac{1}{c_v} \cdot \frac{\partial u}{\partial t}$$

We consider the 'linear case' with two vertical parallel electrodes, where $A(o)$ = anode and $C(a)$ = cathode (see Fig. 17).

At the beginning of the electro-osmotic process h_u is everywhere equal to h_o (hydraulic head) and $u = u_o$, and if Q_c

(cathodic discharge) will maintain steady hydraulic head h_o then:

$$Q_c = \mu \cdot I$$

If $h = h_u - h_o$

$$\bar{q} = -\frac{k}{\gamma_w} \cdot \frac{\partial h}{\partial x} + \mu \cdot I$$

$$Q = -k \cdot c_v^{-1} \cdot \frac{\partial h}{\partial t}$$

$\frac{\partial^2 h}{\partial x^2} = \frac{1}{c_v} \cdot \frac{\partial h}{\partial t}$, with the following boundary conditions:

$$h(\pm \infty, t) = 0$$

$$h(+o, t) = h(-o, t)$$

$$\left(\frac{\partial h}{\partial x}\right)_{+o} - \left(\frac{\partial h}{\partial x}\right)_{-o} = \frac{\mu I}{k}$$

$$Q_c = q_{a-o} - q_{a+o} = \mu \cdot I - k \left\{ \left(\frac{\partial h}{\partial x}\right)_{a-o} - \left(\frac{\partial h}{\partial x}\right)_{a+o} \right\}$$

The problem can be solved by the Laplace transformation method and we will mark the transformation of all the magnitudes considered by a bar:

$$\bar{f}(x, s) = \int_0^\infty e^{-st} f(x, t) dt$$

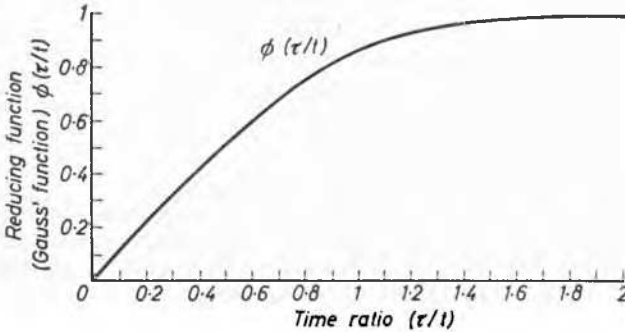


Fig. 18 Reduction of electro-osmotic drainage rate
Diminution de la vitesse de drainage electro-osmotique

There results:

$$\bar{h}''(x) = \frac{s}{c_v} \cdot \bar{h}$$

a differential equation with well known solution. Thus we obtain all the \bar{h} 's for $x < o$, $0 < x < a$ and for $x > a$. Applying the transformation to Q_c and successively the inverse transformation, the following expression results:

$$Q_c = \frac{2}{(\pi)^{\frac{1}{2}}} \cdot \mu \cdot I \cdot \int_0^{(\tau/t)^{\frac{1}{2}}} e^{-U^2} dU$$

wherein: $\tau = (a^2/4c_v)$ means the time parameter. The result shows that the cathodic water expelled per unit of time diminishes with respect to its stationary value in the ratio $\phi(\tau/t) = \int e^{-U^2} dU$ (well known Gauss function) the more τ/t increases (see Fig. 18).

At first the dewatering process is similar to the stationary case, where $Q_c = \mu \cdot I$. Conversely for large values of t with regard to τ there results:

$$Q_c = \frac{\mu \cdot I \cdot a}{\pi(t \cdot c_v)^{\frac{1}{2}}}$$

with rather slow decrease.

Introducing $\psi = \int_0^\tau Q dt$, $T = I \cdot t$ the total quantity ψ of water

expelled with relation to T shows similar behaviour to Q_c as a function of I . Further ψ is independent of the coefficient of consolidation.

The examination of the space distribution of ψ (with relation to the abscissa x) shows that the cathodic water discharge partly comes from the space beyond the anode $-\infty < x < o$, and partly from the space between the electrodes $o < x < a$.

When $t \leq \tau$, Q_c flows equally from these two regions and at first shows a distinct maximum at the anode.

When $t \gg \tau$, the water extraction takes place in the zones far from the electrodes.

In practice the electro-osmotic drainage occurs within the electrodic zone when t does not exceed τ by some multiple.

Further theoretical deductions allow the limits of drainage to be determined with relation to a given electric current and to given physical characteristics of the soil like resistivity, coefficient of consolidation and others.

The author is indebted to Dipl. Ing. W. Schaad, Consulting Eng., Zurich, for his advice.

M. E. BUISSON (France)

Dans leurs communication, S. MURAYAMA et S. HATA (1a/19) exposent les résultats qu'ils ont obtenus en remaniant le sol dans une seule direction et en répétant les remaniements. Il serait intéressant que cette détermination se fasse d'une façon comparable dans les divers laboratoires pour déterminer la sensibilité de l'argile. Cette notion est en effet pleine d'intérêt et permet de comprendre bien des phénomènes d'abaissement de résistance de l'argile par cisaillement.

En ce qui nous concerne, et après l'avoir mis approximativement sous forme cubique, nous plaçons l'échantillon à remanier sous une machine de compression, et nous aplatissons le cube de façon que sa hauteur soit diminuée d'environ 15 pour cent. Nous tournons l'échantillon à 90° et nous recommençons dans les deux autres directions principales du cube. On opère rapidement pour empêcher l'évaporation.

Une autre méthode consiste à malaxer le sol à la main en lui incorporant éventuellement le minimum d'eau. On prélève un échantillon cylindrique après malaxage et on mesure sa teneur en eau. Le remaniement après malaxage, sans ajout, conduit à une teneur en eau inférieure. La résistance est alors obtenue par interpolation.

Il semble qu'en vérité, et suivant les ouvrages, on puisse se contenter d'un remaniement suivant une seule direction de compression, ou suivant plusieurs directions. De toutes façons, l'intensité du remaniement n'est pas précisée. Les résultats diffèrent quelque peu, mais pas d'une façon régulière. Il serait donc intéressant d'étudier de près le mode opératoire et de le normaliser.

A. J. L. BOLOGNESI and O. MORETTO (Argentina)

In answer to B. Aisenstein's comments on our paper (1a/3), we wish to point out that this paper was purposely entitled Properties and Behaviour of Silty Soils Originated from Loess Formations because we limited it to the types of re-deposited modified loess that belong to the ML group. We do not question the existence of other types of soils derived from loess with a larger percentage of clay particles than the ones described. On the contrary, we state specifically that the area we have studied is covered by very thick deposits of materials ranging from true wind-blown loess to very active clays.

W. SCHAAD (Switzerland)

The contributions to the conference in the field of electro-osmotic treatment of soils reveal a steadily increasing interest

in the range of practical application of these methods. At the same time this interest stimulates the improvement of theoretical bases.

The publication of G. M. LOMIZÉ, A. V. NETUSHIL and B. A. RZHANITZIN (1a/15) represents a valuable contribution to electro-osmotic drainage of soils.

Their expressions 5 to 9 give a new derivation of the electro-osmotic permeability coefficient k_e treating the relationship between porosity, grain size and this coefficient. One may state that the basic form of these expressions is of the same form as the Helmholtz formula from which may be derived:

$$k_e \sim \text{const.} \frac{\zeta}{\eta}$$

ζ = electrokinetic particle potential

η = viscosity

Since the electrokinetic particle potential is directly proportional to the cation charge σ of the double layer the above expression may be replaced by:

$$k_e \sim \text{const.} \frac{\sigma}{\eta}$$

and the basic type of formulae developed by Lomizé is obtained.

The electro-osmotic activity as defined by Lomizé is identical with the specific electro-osmotic pressure height:

$$h_e = \frac{k_e}{k} \quad (k = \text{hydraulic permeability coefficient})$$

defined by SCHAAD and HAEFELI, 1947 and 1948, in addition the expressions 14 and 15 have already been published (Schaad-Haefeli) and are not new. At the same time the Laplace equation for combined hydraulic and electro-osmotic ground water flow was also published by the latter authors:

$$k \Delta \phi_h + D_B \Delta \phi_e = -4\pi\epsilon$$

where:

ϕ_h = hydraulic potential

ϕ_e = electric potential

D_B = dielectric constant of saturated soil

ϵ = electrical charge density per unit volume

It was then stated that on the basis of this equation the theoretical treatment of electro-osmotic percolation may be undertaken in the same manner and the same methods are applicable as for hydraulic ground water flow, since both show the same type of differential equation (Laplace).

This fact enables treatment of electro-osmotic flow in porous media by means of analogy, e.g. electric model test method as in the field of hydraulic ground water flow.

The analogue studies of NETUSHIL represent an interesting contribution to the combination of vacuum well point system with electro-osmotic treatment, and are illustrated by the practical application for building and trench excavations described by RZHANITZIN.

The publication demonstrates that electro-osmotic treatment and combination of well point systems with electro-osmotic dewatering methods represents effective means for the drainage of fine-grained soils.

The coulometric coefficient treated by A. PIASKOWSKI (1a/22) is of the same form as the old WIEDEMANN's coefficient (1852-56). In the case of the hydraulic coefficient being zero the coulometric coefficient represents the inverse of Wiedemann's coefficient:

$$c = \frac{Q}{J} = \frac{Q \cdot t}{J \cdot t} = k_e \cdot \rho = \mu \quad (\text{Belluigi, see written contribution})$$

where: Q = electro-osmotic discharge

J = current strength

t = time

ρ = resistivity of soil

These coefficients are very useful for checking the effectiveness of practical applications of electro-osmotic drainage.

The further statements of influence of the change of cation charges confirm the former observations of PERRIN (1904) showing that the particle potentials of double layers depend in a high degree on the ionic nature of liquid phase. Since the electro-osmotic permeability coefficient k_e is proportional to the electrokinetic particle potential ζ respectively to the cation charge σ the proportion of the influences of base exchange on k_e is the same as to the influence on ζ .

The inter-relation of the electro-osmotic flow coefficient k_e and the soil properties on the one hand, as well as the connection between the classic laws of electro-osmosis (Helmholtz-Perrin-Smoluchowski) and the law published in 1946-48 (Schaad-Haefeli) on the other hand represents a central point of interest in research and has been treated by the author in a paper that appeared in *Annali di Geofisica*.

Starting from the Helmholtz formula the author formerly mentioned (1948) that regarding the boundary conditions of this law the formula leads to results contrary to experience. In the past ten years an analysis of this formula:

$$u_c = \zeta \frac{D}{4\pi\eta} \cdot E$$

u_c = capillary speed

ζ = electrokinetic particle potential

D = dielectric constant of fluid

η = viscosity

E = electrical field strength

has been undertaken, considering not only the electro-osmotic flow, but also the electrolytic part of current transported simultaneously and in combination with the electro-osmotic phenomenon and causing cation exchange.

By means of special considerations one may show that the Helmholtz formula can be transformed to the expression:

$$k_e = n \cdot \lambda \left(\frac{\delta \cdot a}{\rho_L \cdot \eta} \right)^{\frac{1}{2}}$$

n = porosity

λ = transformation factor from c.g.s. to pract. units

δ = thickness of double layer

a = capillary radius

ρ_L = specific resistance of fluid

The electrokinetic particle potential ζ results from this consideration:

$$\zeta = \frac{4\pi}{D} \left(\frac{a \cdot \delta \cdot \eta}{\rho_L} \right)^{\frac{1}{2}}$$

which shows that the particle potential ζ as defined by Helmholtz would depend on the capillary size, incompatible with its original definition.

Introducing the electrolytic action combined with the electro-osmotic phenomenon another particle potential ξ was introduced. As expected, it is independent of geometric magnitudes of capillary:

$$\xi = \frac{4\pi\delta}{D} \left(\frac{\eta}{\rho_L} \right)^{\frac{1}{2}}$$

The current J_w transported by electro-osmosis is obtained:

$$J_w = \frac{J}{1 + \frac{2\pi}{\xi \cdot D} \left(\frac{\eta}{\rho_L} \right)^{\frac{1}{2}} \cdot a} = \frac{J}{1 + \frac{a}{2\delta}} = \beta_e \cdot J$$

where β_e means a coefficient of electro-osmotic action and J the total current transported by electro-osmosis and electrolysis. The expression demonstrates that by increasing the pore diameter $2a$ the electro-osmotic transport diminishes to zero and by decreasing the diameter to zero theoretically the whole amount of current is transported by electro-osmosis.

The analysis leads to the following conclusions:

By considering the electro-osmotic flow as a partial electric surface current always combined with electrolytic current, the contradictions resulting from application of the classical theory, when applied to special problems, e.g. the electro-osmotic rise as a function of capillary diameter, disappear. With increase of capillary diameter, the electro-osmotic effect decreases in accordance with experience, while from the classical theory the opposite effect results.

Taking into consideration the electrolytic part of the current the modified electrokinetic particle potential becomes constant, a real physical magnitude independent of the capillary diameter as must be expected from the original definition of the particle potential.

Assuming the electro-osmotic part of flow to be some sort of film motion, the modified electrokinetic potentials show an order of magnitude of volts and seem to represent simple contact potentials similar to galvanic potential differences, and with these results the inconsistencies of the classical theory seem to vanish.

It must be emphasized that for the range of *practical application* of electro-osmotic procedures the inter-relations of the diverse influences on electro-osmotic permeability coefficient like cation exchange, porosity, etc., do not affect very much the relevant magnitudes, since other influences like cavities, fissures and heterogeneities of soils influence the electro-osmotic permeability much more than these inter-relations, in the same way as for the hydraulic permeability of ground water flow.

For this reason in a practical case the electro-osmotic permeability coefficient, the electro-osmotic pressure height (activity) or the coulometric coefficient must be determined on samples, if possible undisturbed ones, and cannot be checked from the grain size or other soil coefficients. Even knowledge of coefficients determined on samples does not guarantee a successful result of application, since influences of natural heterogeneities of large soil masses are not included. Therefore practical applications necessitate preliminary field tests. The experience shows that for electro-osmotic action the same holds as for hydraulic percolation, i.e. the fact that only *orders* of magnitudes of water expelled, drainage rate, energy consumption, drainage time, etc., can be determined, and if this check of the order is confirmed by application, one must be satisfied and the result is a very good one.

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P. SŁIWA (Poland)

I have analysed the results of about 10,000 physical and mechanical investigations on cohesive soils from several soil mechanics laboratories. Advantage has also been taken of all matter available in the literature.

It has been ascertained that there are statistical relationships among all properties of soils which may be classified into two types:

(1) Conformities, practically speaking, straight lined: for example the connection between the Atterberg limits and the content of clay fraction, between unit weight, porosity and water content, between plasticity index and compressibility, etc.

(2) Conformities determined by the equation of the logarithmic curve: for instance the connection between shearing resistance and water content (the angle of friction, cohesion and water content), compressibility and water content, etc.

In all these connections there appear three parameters of physical and mechanical properties of cohesive soils which are fundamental from a practical point of view. These are: the mineralogical character, grain-size characteristics and natural water content.

These connections may be explained by the theory of the structure of clay soils, and may be found useful in determining soil properties for practical purposes. The three above-mentioned fundamental parameters: mineralogical character, grain-size characteristics and natural water content may in practice be estimated by means of the simplest methods, which can be carried out in field as well as in laboratory investigations. In this way it is possible to simplify to a large degree the laboratory investigation of soils to a method which amounts to taking advantage of established graphical or analytical relationships.