SECTION II

LABORATORY INVESTIGATIONS

GENERAL REPORT

W. KJELLMAN (Sweden)

For a long time two groups of questions have been predominant in soil mechanics laboratory work, namely problems regarding the consolidation of soils, and problems regarding the shear strength of soils.

The methods of determining the consolidation characteristics of a soil specimen are already more or less standardized. Their results are well applicable to ordinary settlement analysis in which, as a rule, a moderate degree of accuracy is sufficient. In this field two chief problems remain unsolved: the phenomenon of secondary settlement and the influence upon the consolidation process of the rate of loading. The reports received by this conference contain nothing that could strengthen our vague conception of these two phenomena. Three reports only treat of consolidation questions, two of them (IIc 2 and IIc 3) describing good laboratory technique, and one (IIc 1) dealing with a very special kind of soil.

The methods of determining the shear strength characteristics of a soil sample are in certain respects different in different countries. As shown below, the reliability of their results is subject to serious doubt. When, however, these results are applied in stability analysis, a rather high degree of accuracy is usually desirable for economic reasons. Therefore, in recent years the shear strength questions have attracted an increasing interest. The conference has received no less than twenty reports upon such questions. This discussion will concentrate upon the fundamentals of shear strength investigations.

For determining the shear strength the triaxial test is being more and more used instead of the unconfined compression test and the direct shear test. When a quick triaxial test is performed on a specimen saturated with water, it seems obvious, that the shear strength should be independent of the magnitude of the applied allsided pressure (= minimum total principal stress). According to some reports (IId 1, IId 14 and IIg 8) however, the strength increases with this pressure. Golder and Skempton (IId 2) state this to be the case with undisturbed silt, which they believe to have a tendency to expand under shear, like a dense sand. Such expansion would certainly give the silt a good strength, but it would be no reason for the strength to increase with the all-sided pressure. According to the same authors the shear strength of clay shales and siltstones also increases with the all-sided pressure — thanks to a good direct grain-to-grain contact in these soils. If this is true, the compressibility of the grain skeleton of such soils, as measured in a consolidation test, must be about as small as that of water.

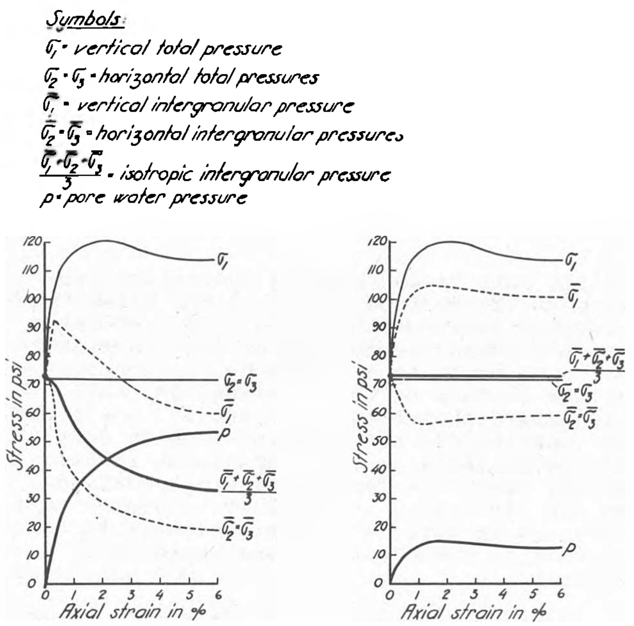
Test of this kind can easily become misleading on account of small leakages or small amounts of air being trapped in the specimen during the sampling operation or the assembling in the apparatus. Therefore it is important, that the constancy of the volume of the specimen be checked accurately and continuously throughout the test.

In the slow direct shear test the increase of the shear stress (in any case if continued until rupture) implies an increase of the isotropic pressure (= the average principal stress) which of course causes consolidation. But even if the shear stress were increased without increasing the isotropic pressure, consolidation would occur. This is explained by Geuze (IIe 3) roughly in the following way, previously mentioned by Hvorslef (1). The pure shear stress system can be defined as the increase of one principal stress and the simultaneous and equally great decrease of the other, the third one being kept constant. Now, in clays which were never heavily pre-consolidated, the contraction caused by a certain stress increase is much greater than the expansion caused by an equally great stress decrease. Thus, the application of pure shear stress in a slow test with normal clay would cause consolidation.

In a quick test, where no consolidation can occur if the specimen is saturated with water, the application of pure shear stress must, in place thereof, increase the pore pressure, e.g. decrease the intergranular isotropic pressure. Now, any applied total stress system can be divided into an isotropic total pressure, (which of course in a quick test has no influence upon the intergranular pressures) and a pure shear stress. Consequently, any stress system applied to an undrained and air-free mass of normal clay will decrease the intergranular isotropic pressures.

For the triaxial test Skempton (IId 3) deduces the pore pressure and the intergranular stresses as functions of the total stressses applied and the ratio between the expandability and the compressibility of the grain skeleton. (Along these lines he also proposes that the true cohesion and the true angle of internal friction be derived from the unconfined compression test). Taylor (IId 13) has obtained interesting results in triaxial tests, where the specimen, after having been consolidated under an isotropic pressure, was exposed to an increasing axial compression strain, drainage being prevented and pore pressure measured. From these results the left diagram of figure 1 has been prepared, showing that the intergranular isotropic pressure decreased very much with increasing strain. For the sake of comparison, the right diagram of figure 1 shows, what would have happened, if the grain skeleton had obeyed Hooke's law instead of behaving as explained by Hvorslef (1).

In tests of this kind the true cohesion according to Hvorslef (1) should remain constant, because the pore ratio is constant, and the true internal friction should decrease, because the intergranular isotropic pressure decreases. Thus we arrive at the puzzling conclusion that an undrained air-free clay looses part of its inherent shear strength, as soon as one tries to measure or make use of it. The loss is probably influenced i.a. by any change in the principal directions. Therefore the remaining shear strength is probably a function



Consolidated-quick triaxial test on clay.
Left: As the grain skeleton really behaves, according to Taylor.
Right: If the grain skeleton obeyed Hooke's law.

FIG. 1

not only of the values of the principal stresses under which consolidation took place, but also of the orientation of the sliding surface relative to these stresses.

On account of these circumstances the problem of determining the shear strength seems to be more intricate than hitherto believed. Further complications are implied by those losses of strength, which the specimen has probably suffered before testing on account of the stress changes during the sampling operation, and when being pushed out of the liner. Therefore it is important, that the test results be checked by comparison with the results from stress computations of actual slides. Four reports contain such comparisons. Tschebotarioff and Bayliss (IId 5) found that the unconfined compression test yields correct values on the shear strength, and so did Skempton (IIa 2). On the other hand Odenstad (IIIc 3) and Carlson (IIIb 3) found that on samples from greater depths this test gives much too low values.

In the unconfined compression test and the usual triaxial test, where dilatation occurs in two principal directions, the loss of strength mentioned above is probably greater than in practice where dilatation is prevented in one direction. Therefore it would be interesting to test clay triaxially under the latter condition. This could be done in an apparatus constructed by Buisson (IId 15), where all three principal stresses can be varied at will. Unfortunately, however, friction between the specimen and the walls of this apparatus seems to be all too great.

It seems appropriate to keep apart the influence of the isotropic pressure from the influence of the pure shear stress also in regard to another problem, namely the critical density of sand. In the direct shear test such distinction cannot be done and the isotropic pressure is not even known, and therefore this test seems unsuitable for the study of critical density. For the first-mentioned reason Geuze (IId 8) also rejects the normal performance of the triaxial test; he states that to be correct one should keep the normal stress constant in the future plane of rupture, but from practical reasons he prefers to do so in the 45°-plane. However, none of these performances seems correct for tests on critical density. The proper way would be to keep the isotropic pressure constant and to prevent dilatation in one principal direction. An apparatus, in which such a test could be done, exists (2).

In recent years the methods of electro-osmotic drainage of soils and electro-chemical hardening of clays have attracted an increasing interest. The law governing the electro-osmotic transport of water through a soil has been known for some time. Now Leo Casagrande (IIf 1) has determined the necessary coefficients for different soils, viz. the electro-osmotic permeability, which was proved to be fairly constant, and the electric resistance, which was found to decrease with decreasing grain size. Thus the simple case, where the water levels at the anode and the cathode are kept equal and constant, can be calculated. As pointed out by Bernatzik in a subsequent discussion, no consolidation should occur in this case. In Casagrande's test the water level was kept a little lower at the cathode than at the anode, but this cannot explain the strange consolidation obtained, which therefore must be due to other phenomena, probably of an electro-chemical character. The results obtained by Dawson and McDonald (IIf 3) and by Geuze, de Bruyn and Joustra (IIf 2) are also difficult to interpret, because they contain influences of both electro-osmosis and electro-chemical hardening and the static load.

LITERATURE

1) Hvorslef: Ueber die Festigkeitseigenschaften gestörter bindiger Böden, p. 23.
2) Hvorslef: Ueber die Festigkeitseigenschaften gestörter bindiger Böden, p. 78.
3) Kjellman: Report on an apparatus for investigation of soils, Proc.Int. Conf.Soil Mech. 1936, Volume II, p. 16-20.

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SUB-SECTION II a

GENERAL

II a 5

DISCUSSION (BY LETTER) OF PAPER IIa 10

R.G. HENNES (U.S.A.)

This description of sample preparation at Northwestern University should interest especially those who find it necessary to prepare test specimens of glacial till. The use of the rotary needle cutter and the air jet in conjunction with the soil lathe appears to be an ingenious step in the handling of difficult soil types.

The simplicity and effectiveness of the author's mitre box offer considerable inducement for adopting a square cross-section for unconfined compression test specimens. The paper does not explore the conditions under which "a specimen of square cross-section is satisfactory". These would seem to depend largely upon the vulnerability of the structure of a specific soil to any cutting operations performed at its surface. Other things being equal a circular cross-section should be preferable because a lesser percentage of its cross-sectional area would be affected by disturbance near the surface of the specimen. In soils where this factor becomes important, one might also question the advantage of running four compression tests on small specimens rather than one test on a large specimen; provided that the same ratio of length to diameter were maintained in both instances. Comparative test data bearing upon these issues would be of interest.

The author's detailed descriptions of apparatus and technique are helpful.

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II a 6

DISCUSSION ON CHARACTERISTICS OF SOILS

A. LAZARD (France)

Very many reports by those attending this conference, which are extremely interesting, cannot be completely exploited, because there is almost invariably a lack of one or more details which would enable them to be compared usefully with other reports.

I should like to quote an example: Let us take the investigation of cohesion and friction in clays by triaxial tests (let us say rapid tests, to avoid complication). If there is still a considerable difference of opinion as to how to take into account the water pressure, thus regardless of the operative methods, and if some define true cohesion and true friction as opposed to apparent cohesion and apparent friction, every one is almost agreed on the two following points:

1. The coefficient of friction (whatever its definition) is a fairly constant quantity for a given material.

2. On the other hand cohesion (whatever its definition) is a very variable quantity for a given material.

Now (A) according to a certain theory, cohesion is very directly related to consolidation pressure. Unfortunately, the majority of reports do not give this consolidation pressure. Nevertheless it was probably determined oedometrically.

(B). Every one is likewise in agreement as to the sensitiveness of cohesion to water content. It may be imagined that the law relating cohesion to water content should present different expressions, according as we are dealing with the plastic field, or the elastic field with shrinkage. Now if the reports generally give the limit value of plasticity and if the water content is sometimes higher, sometimes lower, the value of the shrinkage limit (knowledge of which - to me personally - would seem indispensable) has never been given.

(C). Data as to compactness, the differences existing between the water content and:

the optimum content in respect of the compactness employed on the one hand;
the theoretical value of saturation at the same density on the other,

are seldom indicated.

Now a very interesting report by one of those present at this Congress revealed the extraordinary sensitiveness of cohesion to an extremely small variation in water content on the one hand and to the optimum content for the compactness employed on the other. It is consequently difficult to derive the best from a report, if these data are not indicated.

(D). Quite a few other parameters may be necessary, but I do not want to insist on this point and I shall pass to a second example relating to sands.

Very many reports insisted on the importance of critical density. To suppose this phenomenon valid for all sands, it will be necessary henceforth to give the value of this critical density side by side with the density of the sand being investigated. The phenomena changing in pace according as we are on one side of this critical density, or on the other, I imagine we could thus explain the numerous divergencies between the tests reported upon.

I am now concluding by drawing the attention of those present to the necessity of providing the maximum data possible, both physical and mechanical, regarding the soils tested by them, even if in their minds, such data are without any apparent relevance to the phenomena observed or measured by them. I should be pleased if this Congress could make a recommendation on these lines.

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II a 7

STANDARIZATION OF CONVENTIONAL TESTS

W. SCHAAD and L. BJERRUM (Switzerland)

For classification and description of soils standard tests have been introduced everywhere in laboratories of soil mechanics. Exchange of results of scientific research and practical experience is possible only when the soils can be described in terms internationally understood. Determination of liquid limit, plastic limit, sieve analysis, hydrometer analysis etc.etc. are such standard tests.

It is desirable that the apparatuses and methods applied for such tests be standardized in order to make the results independent of where and by whom the tests have been made. However, it has proved extremely difficult to standardize even the most simple tests. And such conventional tests as determination of the liquid limit and plastic limit are made in different ways in different countries. From determinations of liquid limits in different laboratories the authors have found deviations up to 8-12 % in the water contents.

For the International Soil Mechanics Association it must be an important task to standardize apparatuses and methods for the most common standard tests. The authors of this article suggest that also a standard material be introduced, a standard clay just as there is a standard sand. Such standard material should be a rather fat clay (as for instance London clay) made homogeneous and closely examined by the international soil mechanics office. It should be possible for any laboratory to procure a suitable sample with statement of the geotechnical coefficients. In this way it would be possible to check a series of tests as for instance:

1) Liquid limit, plastic limit,
2) Hydrometer analysis,
3) Compression tests on remolded material Permeability,
4) Shear tests on remolded material,
5) Capillarity, hygroscopicity, etc.

Also in connection with tests of pure scientific nature a material of this kind would be of interest. New theories could be compared directly with previous results and collaboration between the different laboratories would be simplified.

Therefore, the authors suggest that the International Soil Mechanics Committee take the initiative to a standardization of the conventional tests comprising the following items:

1) Description of apparatuses, including dimensions and description of the material used,
2) Exact description of testing methods and definition of soil coefficients.
3) Introduction of a standard material with statement of its standard coefficients.

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SUB-SECTION IIb

INDENTIFICATION TESTS

IIb 7

DISCUSSION

J.J. KOLBUSZEWSKI (England)

Mr. Kolbuszewski (Imperial College, University of London) recalls results included in paper IIb 1 and announces the following final results of the research.

Generally it was found that:

1) A low velocity of fall, as for example, in water leads to high porosity, undependent of the intensity of deposition.
2) A high velocity of fall, as for example from heights of several inches or feet in air, produces a low porosity with low intensity of deposition, but with increasing intensity of deposition the porosity increases progressively until, with a high intensity of deposition corresponding to the free fall of a mass of sand the porosity is of the same order as that obtained by deposition in water.

For any given, relatively high velocity, the effect of increasing intensity of deposition is to inhibit the possibilities of movement of the grains and they are virtually "locked" in the open packing existing at any moment in the extreme top layer of the sample. Generally it could be said that with low velocities of fall there is insufficient energy available for producing a dense packing.

With high velocities there is sufficient energy available for a dense packing to be achieved but with high intensities of deposition there is insufficient time available for this close packing to be achieved owing to the "locking" action, of the newly arrived grains. Only when a high velocity is accompanied by a low intensity which means that the grains behave more or less as indivuduals, can they be forced into position of relatively close packing.

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IIb 8

WRITTEN DISCUSSION ON PAPER IIb 4

P. BAUMANN (U.S.A.)

This paper furnishes valuable information on recent tests conducted by various institutions and agencies at various places in the United States. Of particular aid in similar studies on the capillary rise of water in soils is the "rise function" as shown in equation 3. This is a modification and simplification of equation 2 and permits the expression of time in terms of the percentage m of maximum capillary rise.

It is not quite clear how the maximum capillary rise h_c was arrived at. For example, in Fig. 2 seven curves are shown for materials varying in size between 4.70 mm and .074 mm. The only apparent maximum rise within the day period indicated in Fig. 2 applies to materials 6 and 7, while for the materials 1 to and including 5, the capillary rise appears to still have been in progress after 30 days, and therefore not to have reached the maximum value. It would perhaps be helpful to many readers to have the concept of maximum capillary rise clearly defined.

Of considerable interest is the graph in Fig. 3 showing the degree of saturation and the text referring thereto. Capillarity seems to be predicated on incomplete saturation and conversely to be eliminated by complete saturation. This fact aids in the understanding of capillarity which nevertheless is, for the time being at least, a somewhat mysterious phenomenon.

Of considerable significance in this paper is the statement that capillarymeters did not furnish satisfactory results so far as the soils referred to are concerned. Likewise significant is the finding that for certain liquids the density and viscosity of which are greater than those of water the rate of capillary rise is smaller than that of water.

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SUB-SECTION IIc

CONSOLIDATIONS TESTS

II c 3

WRITTEN DISCUSSION ON PAPER IIc

R.G. HENNES (U.S.A.)

Authentic test data, such as those contained in this paper, are of lasting value to any investigators of the physical properties of soil. This is especially true in the present study, because of the evidence that the tests have been competently performed and carefully reported. These consolidation tests on Crookston clay constitute an important contribution to any further research into the effects of sample disturbance.

Nevertheless, there does remain a need for further investigation to substantiate the hypotheses advanced by the author. The conditions of the tests were not such as to permit complete isolation of variables; consequently the author was forced to make assumptions regarding the magnitude of sidewall friction from observations made elsewhere on a different soil. The relative importance of sidewall friction and sample disturbance is almost the whole issue at stake, and it would appear to be essential that any conclusions should be based upon direct measurement of both of these factors, made on the same sample. This objection is given weight by the data for the floating ring apparatus. The author states that "The lesser degree of friction in tests of 0.75-inch specimens in floating rings accounts for the greater degree of consolidation effected in these tests as compared to that obtained in specimens tested in the same thickness of fixed rings." However, the vertical offset of the curves for floating rings from those for fixed rings of the same thickness in Figure 3 is of greater magnitude than the entire allowance for sidewall friction made in Figure 7. It is true that Curves T_{1-5} and T_{1-7}, in Figure 7, show good agreement when corrected, but it would have been more appropriate to have shown T_1-8 than T_1-7 in this connection, because of the higher initial void ratio of the latter. The divergence of the curves would have been more apparent for the two specimens of identical initial void ratio.

It is unfortunate that floating rings of other thicknesses were not included in the testing program. The additional data might have permitted a closer estimation of sidewall friction, without the very substantial enlargement of the program that would have been required for positive evaluation of this important factor.

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SUB-SECTION IId

TRIAXIAL TESTS

II d 16

DISCUSSION

H.Q. GOLDER (England)

I want to disagree with one or two things which have been said by Mr. Kjellman and certain of my Dutch and Belgian friends and, as I know all these gentlemen very well I won't waste your time by apologising for disagreeing with them.

Mr. Kjellman referred to a soil mechanics axiom. I differ from him here as I have ceased to believe that anything is axiomatic in soil mechanics. I do not believe in anything until it has been proved. If Mr. Kjellman believes in his own axiom it may follow that the shear strength of a clay is reduced as soon as one begins to measure it and therefore it may pay not to try too hard to measure the strength accurately or one may find that the strength has been reduced to zero. Personally, I like things simple and prefer to stick to simple unconfined compression tests whenever possible.

A year ago I would have agreed with Mr. Kjellman that in a quick triaxial test as carried out in England there would be no angle of shearing resistance, although the Dutch and Belgians consistently measured such angles. On looking through some hundreds of test results with Professor Skempton, however, we discovered that there were some soils which gave a definite angle of shearing resistance.

Mr. Kjellman has suggested that this may be due to the fact that we get air in the sample during the test. I do not think that this is true. Since I wrote the paper describing these tests I have had two further examples, one a very good one. A borehole was put down to a depth of 60-ft in soft estuarine clay. The Liquid Limit gradually decreased with the depth. On two samples, one 10-ft lower than the other, the upper sample had a Liquid Limit of 33% and no angle of shearing resistance was found. The lower sample had a Liquid Limit of 27% and an angle of shearing resistance of 20° or more. We had had other cases of a similar sort. I see no reason why the technique employed, which was the same in each case, should give an angle of shearing resistance greater than zero in one case and equal to zero in the other if this was not a real difference. I am of the opinion that a Liquid Limit of about 30% there is a change in the properties of the material which accounts for this difference.

I cannot agree with Mr. Kjellman that an expansion which would cause tension in the pore-water would not result in an increase of shear resistance with lateral pressure. When the pore-water goes into tension the whole of the lateral pressure becomes effective and therefore an angle of shearing resistance must be observed. If the whole of the lateral pressure is not effective then there will not be a tension in the pore-water. The amount of movement of expansion required to cause tension in the water is very small.

On the point of the compressibility of the clay shales I can confirm that the compressibility is low and I will later send the values to Mr. Kjellman and he can work out for himself whether his theory is correct.

Now on the question of the Dutch and Belgain Triaxial - also called the "cell" test - which, I think, is a better name, I have a few comments to make.

I do not think that the same results would be obtained if the tests were carried out in the same way as we do in England. It seems to me that the result obtained is a function of the rate at which the test is carried out, and of the permeability of the material. In a way this test corresponds to the Immediate Shear Test as it was carried out in England, but which has now been more or less abandoned in favour of triaxial compression tests, as one was never sure in the shear test whether the result obtained was a property of the material or a function of the testing technique.
I think the same criticism applies to the Dutch "cell" test owing to the fact that some consolidation takes place during the test.

I gather from talks with my Dutch and Belgian friends that they are inclined now to agree with this point of view to some extent, but they claim that the results of the "cell" test, since they are always carried out in the same manner, give them an index to the properties of the material. Regarded simply as an index test I can see their value but I cannot see that they are measuring a fundamental property of the soil.

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II d 17

DISCUSSION

A.W. SKEMPTON (England)

I wish to mention two points, first the problem of the changes in effective pressure during an undrained triaxial test on saturated clay, and secondly the relation between shear strength and stability analyses in clays. In his opening discussion Mr. Kjellman referred to the tests of Prof. Taylor which show that the average effective pressure in a sample of clay decreases during an undrained triaxial test. This is, at first sight, rather surprising: but it can also be predicted theoretically . x)

Now the simple explanation of the constant strength in an undrained triaxial test (i.e. independent of lateral pressure σ_3) is that the applied lateral pressure do not influence the effective stress. This is only a partial truth. In fact, the effective stress normal to the shear plane decreases during the test, but it is found that this decrease is a constant, for any given sample, irrespective of the applied lateral pressure.

The second point is of more practical value, concerning shear strength and stability analysis. I shall outline a few of the more important features in the procedure used in England. As a first step undisturbed samples are obtained throughout the depth influenced by construction. If the soil is a saturated clay (LL more than 30) we carry out unconfined compression tests and use as the criterion of failure:

$$(\sigma_1 - \sigma_3) = 2c$$
$$\phi = 0$$

where ϕ is the angle of shearing resistance and 2c is the compression strength, c being the shear strength. I wish to emphasize that we do not imply, by this criterion, that the clay has no true internal friction. On the contrary many clays have true angles of internal friction equal to 20° or 30°. Yet, without exception, they show zero angle of shearing resistance when tested under conditions of no water content change.

Using the above criterion of failure the stability analysis is simple (see my paper Ie 6 for an outline of the methods). This procedure has been checked in about a dozen field jobs in England (see Skempton and Golder Paper IVd 2), by Peck in the Chicago Subway, by Tschebotarioff (Paper to this Conference) and others.

Now this analysis will give us the factor of safety only under conditions of no water content change. In the course of time, owing to changes in stresses caused by construction, the water contents will change. These must be considered, by making slow shear tests, but I cannot go into that point in this discussion. I do wish however, to make it clear that we

x) A.W. Skempton. Paper IIId 3.

do not consider the $\phi = 0$ analysis with the unconfined compression strength to be the only necessary consideration. This method has its limitations, and these I have discussed in detail in my paper (Ie 6).

Up till now, I have been considering only non-fissured clays. With stiff-fissured clays the problems are more difficult. A vertical cut 20 ft. or 30 ft. deep can be made in the London Clay and remain stable for several weeks or months. I have an old print showing the constuction of a retaining wall, by Robert Stephenson in 1840, where the clay was cut almost vertically and the wall built in brickwork in front of this face. Yet after periods of 20, 40 or 70 years many retaining walls fail.

Terzaghi in 1936 gave an explanation of this effect. Our field investigations in London (carried out by the Building Research Station) show that his conceptions of softening along fissure planes is correct.

It is, however, essential to get some idea of the rate of this softening process. Yesterday Mr. Cassel criticised my attempts in this direction without, however, making any constructive suggestions. I have (Paper Ic 6) shown some empirical curves for London Clay. As a matter of course it would not be excepted that these should apply to a very different material, such as the Lias Clay with which Mr. Cassel was concerned. It is also obvious, as he mentioned, that local conditions of topography and drainage influence the rate of softening. Yet I believe that depth of the slip surface and type of clay are the most important factors and further attempts should be made be to obtain at least rough estimates of the rate of softening, as a function of these factors, for various stiff fissured clays.

Finally, I wish to point out that no softening will take place in the clay beneath a foundation. The fissures have not an opportunity to open. The proof of this is the fact that in London many buildings have been standing for centuries, with foundation pressure of 2 or 3 ton/ft.^{2}; a value far in excess of that possible with the softened strength as found in the clay behind retaining walls and in the banks of cuttings.

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II d 18

DISCUSSION

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

One can agree with the General Reporter's opinion on the proper execution of a tri-axial test for the study of critical density properties. The apparatus which allows tests to be performed with zero dilatation in one principal direction, as used by Mr. Kjellman (Proc. Int. Conf. Soil Mech. 1936, Volume II, p. 16-20) does not seem suitable for a series of tests on critical density on account of its intricateness. This explains the use of the cylindrical type of tri-axial apparatus, which is now commonly accepted throughout the world, not only for tests on sands but also for more or less cohesive soils.

The main object of the author's paper was however to promote the modified performance of variation of principal stresses, such that normal stresses on planes with an angle approximately equal to those of rupture would vary as little as possible as to their magnitude and their direction. Experiments show, that the 45°-plane gives satisfactory results. Volume changes are then approximately equal to those obtained with $(45^\circ - \varphi/2)$ planes. The largest deviation is obtained with the normal performance of the tri-axial test.

Mr. Kjellman's objection does not however invalidate this main argument, which in the author's opinion also applies to the type of test as executed by himself.

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II d 19

DISCUSSION

D.W. TAYLOR (U.S.A.)

Mr. Don. W. Taylor makes a comment on the unconfined compression test:

I believe strongly in unconfined compression tests. That seems possibly common, but I have to mention that there are several variables who play some part in the charges of the soil. And now we are coming to make bad mistakes considering all these variables taking them in the same condition. Variables must be understood well enough but I am a little afraid that most of the tasks in connection with entirely compression tests contribute to an overloading of variables which have to be studied.

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II d 20

DISCUSSION (BY LETTER) ON PAPER IId 13

W.J. TURNBULL (U.S.A.)

This paper presents a unique method of interpreting the shearing strength of soils from undrained cylindrical compression tests in which the pore water pressure within the sample is measured throughout the test. The principal item of new data, made possible by the pore pressure measurement, is the ratio of the shearing strength to the intergranular major principal stress (S/σ_1), and is shown plotted on Figures 1, 2, 3, and 8. The study is of great value in that it points a way to the selection of shearing strength for clays which may prove to be fundamentally more nearly the true values than those now determined by current methods of testing and interpretation.

The concept that the ratio S/σ_1 is the same for a given soil regardless of the initial consolidating pressure is well demonstrated by Figure 3; however, mucht additional data on other soils are desirable to make a firm conclusion to this effect. The author makes the following statement with respect to the σ_1/σ_3 ratio: "Possibly the majority of cases encountered would have pre-shear σ_1/σ_3 ratios greater than 2.4, but this cannot be assured". It is felt that the implication of this statement concerning natural deposits of clay is considerably in doubt; however, as indicated by the author, proof is lacking.

The procedure of applying the data obtained in estimating the shearing strength at various depths in natural deposits of clay which have not been precompressed appears to have considerable merit. However, shearing strength so obtained for natural deposits which have σ_1/σ_3 ratios less than say 2.0 may be unduly conservative. The author's procedure in applying his method of interpretation of the shearing strength of precompresses clays appears to be logical if acceptance of the interpretation in clays not precompresses is made.

In order to gain additional information on the method demonstrated by the author, it would be desirable to compare the range in shearing strengths by this method with that obtained by other methods currently in use. Based on the knowledge gained by past experience in correlation of present methods with behavior in the prototype, it would be possible to determine whether or not the test and interpretation proposed by the author are superior or otherwise to current methods for routine design testing.

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SUB-SECTION II e

DIRECT SHEAR TESTS

II e 7

DISCUSSION (BY LETTER) ON PAPER IIe 8

W.J. TURNBULL (U.S.A.)

Several features of this machine to test soils in double shear are quite different than those found on most shear machines in general use. Certain of these features appear undesirable; however, their adverse effect on the shear strength of a soil may be no greater than factors occurring in other shear machines and test procedures. No data are available to permit a comparison of the results obtained with the machine described in this paper and other types of machine.

One of the principal features of this machine is that remolded specimens can be compacted directly in the shear rings. Thus, the possibility of disturbance in a specimen caused by cutting to certain dimensions to fit shear devices is avoided. This feature may be a marked advantage over other types, but the effect of residual stresses should be examined. When a soil is compacted to a high density in a relatively rigid container, residual stresses remain after compaction. The soil is compacted with the spacer rings and housing in place. The removal of the spacer rings and housing undoubtedly permits some re-adjustment of residual stresses on the planes between the shear rings. The magnitude of the effect of the residual stresses on the shearing strength is not known. Residual stresses due to compaction directly in the shear apparatus would not occur in specimens that were cut and then fitted in the shear rings. The latter is true for the usual type of direct shear test.

The author points out that there is an undesirable effect on the normal loads caused by friction on the walls of the cylinder, and states that this effect is reduced by the procedure used in preparing the specimens. Presumably, this is accomplished by preconsolidating the specimens and allowing ample time for complete consolidation under the normal load. It is difficult to see how uniform consolidation can take place throughout the 6-in. depth. It would seem that more load would be transferred to the walls with increasing depth and that the resulting density would decrease from top to bottom of the specimen.

After preconsolidation has been completed, the spacing rings are removed and the normal load is applied under which the shear test is to be conducted. Additional vertical movement takes place under this normal load and it is probable that most of the change occurs where the spacing rings have been removed and the specimen is not confined laterally. Thus, the density and structure of the soil along this plane, which is the plane on which the shear failure will occur, may be quite different than the average density of the sample.

The primary purpose of the machine is to test compacted specimens of soil. A special procedure is required to adapt the machine to test undisturbed samples. The undisturbed specimens are of a smaller diameter and are surrounded by plaster of Paris which is cut between the shear rings by a special instrument. It seems that most of the consolidation caused by the normal load would take place at or near the point where the plaster has been removed rather than uniformly throughout the specimen.

Due to the great thickness of the test specimen an unusually long time is required for preconsolidation. This is somewhat objectionable, as a number of sets of shear rings and accessories would be required to permit a number of tests to be performed in a reasonable length of time.

It is desired to emphasize the fact that sufficient data are not available to permit an evaluation of this machine with respect to other direct shear machines.

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SUB-SECTION II f

ELECTROSMOSIS

II f 4

ELECTRICAL TREATMENT OF SOILS

W. SCHAAD (Switzerland)

The contributions of L. Casagrande, E.C.W.A. Geuze, C.M.A. de Bruyn and K. Joustra to the field of application of electrosmosis for the electrical treatment of soils show that by laboratory tests considerable improvements of soils are obtainable. If all these results may be transmitted to practical application can not yet be confirmed definitely, possessing only little experience on large-scale experiments. Until now only some large drainage experiments have been carried out in practice. Therefore questions on economical application of electrical consolidation and allied procedures with regard to energy consumption and the time available for treatment are not yet solved. Theoretically the time factor could be reduced by using electrode-nets of narrow spaces where all electrodes work at the same time. If realisation of such applications is possible will have to be determined by future investigations on large-scale experiments. However the possibility of consolidation procedures for heavy clay-masses would represent a progress, such masses being practically unconsolidable until now.

With regard to the physical research of electrosmosis the fact must be considered that the formulas deduced by Helmholtz neglect several remarkable influences. Electrical current does not only produce electrosmosis. Parallel to it electrolysis takes place as a result of the galvanic current in the capillaries. It causes the decomposition of the liquid. Further the conductivity of the soil particles is neglected. The former and later experiments of numerous physicists, (e.g. Quincke, Jllig and Schönfeldt) on the electrosmotic flow through capillaries and diaphragms show, that this flow decreases with increasing diameter of capillaries or voids. The formulas of Helmholtz mentioned in L. Casagrandes report give the opposite effect. From a capillary of infinite diameter an infinite electrosmotic discharge would result. This effect is contradictory to every test result.

The use of the formulas for the determination of the relation between diameter of capillaries and transported charges also leads to results contradictory to experience. A more accurate consideration of the phenomena leads to a division of the electric current into three phases already mentioned by Smoluchowski. The current is composed by a galvanic phase of current transporting the ions in both directions (anode-cathode, cathode-anode), a surface current transporting the charges of double-layer and a third phase flowing through the solid particles. Therefore the form of velocity distribution in the capillary is not yet clear and will have to be found by physical researches and reflexions.

In laboratory tests the disturbance of electrosmotic effect by electrolytic decomposition of the liquid and electrodes is considerable. During the tests polarisation of the electrodes, increase or decrease of electrical resistance and exchange of ions between liquid and soil and even inversion and oscillations of flow were observed. Acidity and basic concentration increases during long permeability tests (e.g. electrosmotic rise tests) and change the electrocinetic potential of double-layer and thereby the coefficient of electrosmotic permeability. These relations have already been observed by the physicists occupied by former investigations on electrosmosis as well as in our Laboratory working with soils. Cruse stated already in 1905 on occasion of his electrosmotic investigations on porous diaphragms that the ratio of discharge and intensity of current rises up to a maximum and falls after. In this connection a paper of Perrin (Mécanisme de l'électrisation de contact et solution colloidales 1904) must be mentioned, containing a complete and detailed investigation on the contact electrisation and showing the parallelism between electrosmosis and colloidal flocculation. Only some of them will be mentioned here.

1) Les liquides ionisants sont ceux où les corps s'électrisent fortement par contact.
2) L'addition graduelle d'acides monovalents diminue la charge d'une paroi négative, puis, le souvent, la charge positivement.
3) Tous les acides monovalents agissent de même, à concentration égale en ions H^+ c'est donc l'ion H^+ qui agit ainsi. Il agit déjà nettement à des concentrations tres faibles; son action grandit avec la concentration mais de plus en plus lentement.
4) Des énoncés symétriques sont applicables à l'ion OH^- caractéristique des bases.

By these rules of the phenomenon the results found by the Delft Soil Mechanics Laboratory confirmed by our own experiences are easily explicable by the discharge and charge of electrocinetic potential ξ of the double-layer. The accessory phenomena influence very much the course of electrosmotic rise, acidity being continuously changed and concentration increasing during long tests.

These influences are less evident and less important in large-scale and field experiments than in laboratory tests. Experiences with filter wells showed, that several days of flow of current did not change the electrosmotic discharge of the well point. For transmission of laboratory tests to field applications, to calculations we should therefore apply the values found in the beginning of the test, which is not yet influenced very much by the accessory phenomena mentioned above. Thus the electrosmotic flow near the origin of the diagram of electrosmotic rise (Subsection IIf2, Fig. 7) is suited best for determinating the k_E-value. The Delft Laboratory obtained the theoretical formula of rise:

$$h = \frac{k_E}{k} \cdot U \cdot \frac{\left(e^{\frac{k.O}{F.d}\cdot t} - 1\right)}{e^{\frac{k.O}{F.d}\cdot t}}$$

which can be written in a simpler form:

$$h = \frac{k_E}{k} \cdot U \cdot \left(1 - e^{-\frac{k.O}{F.d}\, t}\right)$$

By differentiation we find the velocity of rise:

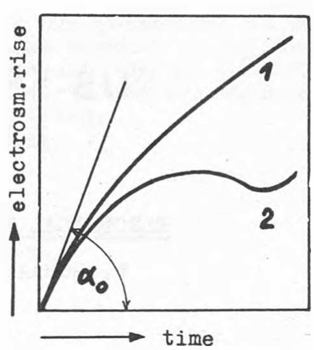
$$\frac{dh}{dt} = k_E \cdot U \cdot \frac{O}{F.d} \cdot e^{-\frac{k.O}{F.d}\, t}$$

And from introducing t=o the velocity of rise in the beginning of the test results:

$$tg\alpha_0 = \left(\frac{dh}{dt}\right)_0 = k_E \cdot \frac{U}{d} \cdot \frac{O}{F}$$

$$k_E = \frac{F}{O}\cdot\frac{d}{U}\, tg\alpha_0 = \frac{F}{O.E}\, tg\alpha_0$$

Wherein α_0 means the slope angle of the electrosmotic rise curve (see Fig. 1) at the origin.



1 = Theoretical curve

2 = Experimental curve

FIG. 1

From this method the advantage results, that the determination of k_E becomes independent from the permeability coefficient k and gives a value of k_E just in the beginning of the test and lest influenced by accessory phenomena. Application of long tests will give caracteristic curves depending on the material of electrodes and on the allied processes if not special constructive tricks for prevention from polarisation, concentration, decomposition of the liquid etc. are introduced.

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II f 5 WRITTEN DISCUSSION ON PAPER IIf 3

W.J.TURNBULL (U.S.A.)

The contents of this paper, although meager, demonstrate one of the most important studies in soil mechanics. The paper serves to reemphasize the need for information on the possibilities in the use of electric current to speed up the consolidation of clays involved in foundation construction.

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SUB-SECTION II g

MISCELLANEOUS

II g 15

DISCUSSION

M. BUISSON

Mr. M. Buisson makes some remarks about the application of straingages on laboratory investigations on walls of model scale.

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II g 16

WRITTEN DISCUSSION ON PAPER IIg 13

R.G. HENNES (U.S.A.)

Although the specialized nature of the examples given in this paper appear to limit its range of practical applications, the basic concept is clever and will appeal especially to teachers of soil mechanics. Classroom discussions of the theory of consolidation would be clarified by simulating ground water flow by means of viscous flow models. In such demonstrations, and in practical applications as well, the assumption of a constant value of a_v will sometimes be undesirable, and may be avoided by using tubes of variable cross-section. Thus less excess water could be made available as the consolidation process develops. Especially attractive is the author's suggestion for varying the rate of load application by feeding water into compression reservoir tubes at predetermined rates. By means of this and similar extensions of the fundamental idea, the viscous flow tube model might become a useful adjunct to the consolidation test in a variety of practical applications.

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