SECTION V

EARTH PRESSURE; STABILITY AND DISPLACEMENTS OF RETAINING CONSTRUCTIONS

GENERAL REPORT

A.E. BRETTING (Denmark)

V a. EARTH PRESSURE AGAINST RIGID VERTICAL WALLS. (5 papers).

H. JANSSON, A. WICKERT and A. RINKERT have in their paper: "Earth Pressure against Retaining Walls" reported a series of earth pressure tests on a rather large scale.

The backfilling material was crushed stone with a great angle of internal friction.

The tests conform very well to the classical earth pressure theory of Coulomb, which is not surprising when the character of the mate rial is taken into consideration.

The angle of friction was found to be 40°, and an unsignificant movement of the wall was sufficient to reduce the pressure to active pressure.

H.EPSTEIN in his paper: "Reduction of Lateral Cohesive Soil Pressure on Quaywalls by Use of Sand Dikes" reports on considerable reduction in pressure obtained by the use of sand dikes behind the wall.

Theoretical considerations are compared with the results of experiments executed at Princeton University, which will be discussed more in detail under subsection V b.

The practical importance of these results is evident.

The reduction in pressure from fluid backfills and superimposed surcharges may reach as high as 70%. Triangular form of dike is preferred in practice.

R.B.PECK, H.O. IRELAND and C.Y.TENG in their paper: "A Study of Retaining Wall Failures" state on the basis of information collected by questionnaires sent to the American Railways that relatively few retaining walls are failing and that the cause, in the failure, generally would be a misjudgement of foundation conditions. In most cases of failure the foundation rests on clay, which is overloaded by the weight of the backfill, or the backfill itself consists of clay. Classical earth pressure theories in such cases are evidently insufficient.

V b. EARTH PRESSURE AGAINST FLEXIBLE VERTICAL WALLS. (12 papers).

The most outstanding papers in this subsection are: TSCHEBOTARIOFF and BROWN: "Lateral Earth Pressure as a Problem of Deformation or of Rupture"
and TSCHEBOTARIOFF and WEICH: "Effect of Boundary Conditions on Lateral Earth Pressures", which shall here be treated in common.

The experiments executed at Princeton University at a considerable scale give an important contribution to our knowledge of earth pressure on flexible walls.

The tests seem to have been executed with great care, and the methods for measuring the strains in the wall are very ingenious.

It is however to be regretted that no direct readings of the instruments are given,and that several facts about the dimensions of the model (as f. inst. the dimension of the wall for the setup with combined active and passive earth pressure) are not revealed. Equally the conditions of drainage during the consolidation of the fluid clay are not given.

It is therefore not possible for the reader to make an independent opinion of the correctness of the conclusions drawn, which are in several points very different from the conceptions which many reputable engineers have hitherto had of these questions.

It seems that the testing conditions only will correspond to certain special types of quay walls, as f.inst. walls of steel sheet piling anchored to anchor plates in the backfill. Otherwise the considerable release of the anchor which has been given would not be justified. By the types of quay walls often used in Europe, where a stiff relieving platform resting on piles is serving as anchorage, the displacement of the anchored point of the wall should probably be much less. It would have been interesting if readings after consolidation but before the release of the anchor had been given.

The distribution of pressure on the wall seems to be determined with very little accuracy, and it seems doubtful if the conclusion of the authors that no arching effect is present could really be justified by the procedure chosen.

The pressure could in principle be found by differentiating the curve of moments two times. If one really tries to do this the results will be so obviously wrong that no conclusions can be drawn. If the curve is smoothed out before differentiating, the result will depend entirely on the way in which the smoothing is done.

The authors seem to be quite aware of this fact and have therefore chosen to use two linear equations for the determinations of the pressure. Evidently this can not give any idea of the real distribution (fig. 8, test 12 A).

In other cases (fixed supports) the 3 factors K_A, K_{30} and K_B are used (Proceedings of American Society of Civil Engineers, January 1948, pag. 23), and also this method can evidently only give an approximate idea of the distribution.

By the comparison between ordinary methods of calculation and the results of the tests it seems as if an angle of internal friction for sand of 30° has been employed, although the sand used by tests has according to experiments an angle of friction of 32°-36°. This could doubtless account for a part of the differences which are found.

An interesting observation is the reduction of the total pressure during consolidation of the fluid clay. When the authors seem to mean, that conventional stress-strain equations have no meaning in this case, this opinion can not be supported. It does not seem surprising that the total pressure of earth and water decreases during consolidation. In reality the surcharge is at the beginning carried exclusively by extra pore water pressure, which diminishes during consolidation and is replaced by an effective grain pressure, which is less than the replaced pore water pressure.

The effective horizontal pressure therefore is increasing and the internal friction of the soil will probably at every moment be fully mobilized on account of the great deformations, which are needed to consolidate the fluid clay.

When the wall is released in a horizontal direction the deformation will continue with shear of the same sign as has been produced during consolidation and the shearing stress, which was already fully mobilized, cannot increase further. Consequently the horizontal pressure on the wall will remain unaltered.

If, however, such angular deformations of the backfill are produced by the movement of the wall that they tend to diminish the already existing shearing stress in the clay or even to alter the sign of the shearing stresses it seems possible, that a redistribution of the pressure on the wall can occur, resulting in an increase of the pressure in some parts and a decrease in other parts. Such deformations seem possible f.inst. in the upper part of the wall if the anchor is not released.

According to the above mentioned explanation it should be expected, that for clay the angle of internal friction should correspond to the horizontal pressure at rest. If the corresponding factor is taken at 0,5 it is found that $\varphi = 19^{\circ},5$, which seems to be quite near the value of 17° indicated for direct test.

Taking it that the total pressure is decreasing during consolidation by a stiff wall it is evident that the elastic strain of the wall must be reduced and that the clay gets at a certain compression which can slightly reduce the shearing stresses in the clay.

The rebound of the wall seems to be favored by the fact that the clay at the upper end of the wall, which is not vertical, will sink from the wall.

One important question has not been discussed in these papers, namely the stress allowed in the flexible wall and the factor of safety in general.

If these stresses are taken as generally allowed (f.inst. for steel 1200 - 1500 kg/cm2) there will be a factor of safety against the yielding-point of the steel of abt. 2, whereas the factor of safety for the earth structure will probably be considerably less. (As also mentioned in the paper of Epstein).

It seems still open to discussion whether it can not be expected that greater deformations of the wall after passing the yield-point of the steel will produce considerable redistribution of the earth pressure (arching) so that stability is obtained with a satisfactory factor of safety against rupture, even if the stresses for ordinary loading conditions seem too near the yielding-point. As to the safety of anchorage and the passive earth pressure these must of course be sufficient.

The results of the tests have been compared with structures designed according to Danish regulations, which seem to suppose the greatest reduction of earth pressure.

Of many structures designed according to these regulations (which have no real theoretical basis) very few have been unsatisfactory, when good backfill as sand has been employed. Some failures can be referred to insufficient passive earth pressure, and the regulations are doubtless in this respect too favourable. But in the most respects structures have stood well, and it can hardly be supposed that full earth pressure always occurs. The Princeton tests give for the first stage, immediately after backfilling bending moments which are abt. 50% higher, and in the second stage after vibration and release of anchors abt. 100% higher. Many of the walls are made in reinforced concrete and are rather stiff; but they are generally combined with a relieving platform. It is possible that cracks have occurred, but no failure on this account has been reported during the period of about 40 years. when they have been employed.

B.S. BROWZIN in his paper: "Upon the Deflection and Strength of Anchored Bulkheads" reports on small scale experiments with flexible walls.

The deflections are obtained directly by drawing. The author states that ordinary sheet piles are so stiff that no point of inflexion will occur in the deflection line, and that the method of calculation designed as "fixed earth support" will not suit real conditions. The conclusion is, that the method of calculation with "free earth support" should be preferred.

T.K. HUIZINGA: "Computation of a Quay Wall" gives a method of computation indicated by the late professor Buisman for a quay wall with relieving platform resting on piles.

A certain effect of the piles for relieving the earth pressure on the sheet piling is taken into consideration.

H.Q. GOLDER in his paper: "Measurement of Pressure in Timbering of a Trench in Clay" has reported some carefully executed measurements of the pressure on the timbering. He finds that the earth pressure is not distributed according to classical earth pressure theory after a triangel, but rather parabolically, as also known from other experiences.

R.B. PECK and S.BERMAN in their paper: "Measurements of Pressures against a deep Shaft in Plastic Clay" give results obtained by direct observation in the shaft by dividing the bracing and introducing hydraulic jacks in the joint.

The pressure displayed a marked reduction in the lower part of the shaft. The pressure was only about 46% of that generally assumed for open cuts. It is thus confirmed that a considerable part of the pressure is transferred through shearing stresses to the soil beneath the bottom of the shaft.

W.L. SHILTS, L.D. GRAVES and G.F.DRISCOLL in their paper: "A Report of Field and Laboratory Tests on the Stability of Posts against Lateral Loads" give an extensive series of field and laboratory measurements and indicate the position of the point of rotation as the depth below which there is 0,324 of the total vertical cross sectional area of the imbedded portion.

With this assumption the necessary basis of calculation is found.

J. VERDEYEN in his paper: "The Use of Flat Sheet-Piling in Cellular Construction" mentiones the principles in cellular cofferdam construction and gives the basis of their calculation.

LOUIS BAES in his paper: "Belval ρ Flat Sheet Piles for Cellular Structures" gives a detailed account of resistance and deformation of the flat pile section based on photo-elastic tests and tests on pieces of rolled steel.

V c. EARTH PRESSURE AGAINST UNDERGROUND CONSTRUCTIONS. (4 Papers).

O.K. PECK and RALPH B. PECK in their paper: "Experience with flexible Culverts through Railroad Embankments" report measurements on elastic steel culverts.

After placing the tubes are backfilled with selected material, which is tamped to give the necessary side-support of the tube, which is provisorily stiffened. It is stated that in the end horizontal and vertical pressure will be of approximately the same size. Deformation is given. Only ring stresses need to be considered.

F.K.Th. van ITERSON in his paper: "Earth Pressure in Mining" states that the usual theories of earth pressure and soil mechanics are applicable in deep mining. As found by the theory of elasticity the stresses in the vicinity of underground working are so high, that the rock is crushed, so that the timbering of the works will only have to resist the load of a limited amount of loose rock. Examples of calculations are given.

JACOB FELD in his paper: "Soil Resistance to Moving Pipes and Shafts" has collected extensive data concerning the resistance of shafts and shields etc. He states that the area of the surface of the tube is decisive for the resistance and that this must be considered as being of the same character as the resistance of a viscous flow. It does not seem, however, that any indications are given regarding the viscosity or the dimension of the viscous layer, which should be introduced, neither is any evidence given that the resistance is proportional to the first power of the velocity.

If the theory was absolutely correct, practically no force should be needed if very small velocities were used, which result does not seem to check with practical experience. It must probably be supposed that a certain minimum resistance is present beside the resistance of a more or less viscous flow.

Synopsis Section V.

The most outstanding results obtained in the last years are the tests made at Princeton University and reported in papers of Epstein, Tschebotarioff & Brown and Tschebotarioff & Welch. Here it is stated, that the wellknown arching effect in reality is not present, a conclusion which seems in contradiction to many years experience and to some of the results mentioned in papers by Peck & Berman, Golder and van Iterson.

It is supposed that this result will give rise to much discussion at the conference.

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SUB-SECTION V a

EARTH PRESSURE AGAINST RIGID VERTICAL WALLS

V a 7

DISCUSSION

J.A. RINKERT (Sweden)

As to the report on section Va concerning "earth pressure against rigid vertical walls" presented by Messrs. Jansson-Wickert- Rinkert from Stockholm I am anxious to point out as follows:

It has not been our purpose to verify the Coulomb theory as the reporter seems to have partly misunderstood, neither to examine earth pressures against rigid walls.

As a matter of fact the purpose of the tests was to examine whether earth pressure at rest or active earth pressure is acting on vertical walls of a rigidity which in the utmost of cases really occurs. Our tests in fact showed that only active earth pressure is to be taken into account as a necessary movement of the basis of the wall is of sùch an unsignificant size as proved in our tests.

I believe this result is of importance showing the right way for carrying out vertical walls and moreover in strict agreement with Prof. Terzaghi's recommandation as to the preference of studies of the nature to theories.

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SUB-SECTION V b

EARTH PRESSURE AGAINST FLEXIBLE VERTICAL WALLS

V b 10

DISCUSSION

G.P. TSCHEBOTARIOFF (U.S.A.)

First of all I would like to express my appreciation to Prof. Bretting for having stated in his General Report concerning Section V that: "The most outstanding results obtained in recent years are the tests made at Princeton University". I also thank him for having stated: "The practical importance of these results is self-evident". However, there are a number of very important statements in Professor Bretting's General Report concerning our tests which require correction. I shall discuss first a point to which both Professor Bretting and myself appear to attach the greatest practical importance. This is the question whether there is or there is not an appreciable reduction of bending moments due to arching behind anchored flexible steel sheet pile bulkheads backfilled with sand.

There are at least three very distinct types of arching, which differ considerably from each other by their stability characteristics. The three types should not be confused with each other. The first type occurs when the arch in the soil can form a complete circle, as happens around vertical shafts or horizontal tunnels. This is the most pronounced and stable type of arching since a yield of the shaft or tunnel supports only tends to increase its effectiveness.

The second type occurs, when the sand has the opportunity to form a stable arch between unyielding supporting abutments - and I emphasize the word "unyielding". To this category belong the well-known arching phenomena in grain silos, or in backfilled trenches, or over openings between timber sheeting. Also to this category belongs the arching in a horizontal direction behind rigid walls, as established by Professor Terzaghi during his 1927 tests at the Massachusetts Institute of Technology and confirmed by measurements in cuts through sand on the Berlin and the New York subways. This latter type of arching can only develop if the upper support of a wall does not yield whereas the lower one does yield. Perhaps "wedging" is a word which may better describe this particular phenomenon. A redistribution of pressures takes place with an appreciable increase of pressures against the upper half of the wall and a corresponding decrease of pressures against the lower half.

Finally the third type of arching is supposed to develop - I emphasize the word "supposed" - in a vertical direction between the anchor level and the dredge level of an anchored flexible sheet pile bulkhead backfilled with sand. The redistribution of pressures in a vertical direction is then caused mainly by the deflection of the sheet piling between these two levels. It is this type of arching that both the Danish Society of Engineers and Professor Terzaghi considered in their recommendations for computations of a decrease of bending moments in anchored sheet pile bulkheads. I also shared their views originally. However it is this - and only this-type of arching that our Princeton tests have shown to be nonexistant for normal field backfilling conditions. Professor Bretting apparently recognizes this, since he stated in his General Report concerning the Princeton tests: "It seems that the testing conditions only will correspond to certain special types of quay walls, as for instance walls of steel sheet piling, anchored to anchor plates in the backfill". This interpretation by Professor Bretting is approximately correct.

It is therefore all the more surprising that in his Summary of Section V Professor Bretting then proceeded to make the following absolutely erroneous statement concerning the Princeton University tests with specific reference to the papers by Epstein (Va 4); by Tschebotarioff and Brown (Vb 2); and by Tschebotarioff and Welch (Vb 7) - to quote Professor Bretting: "Here it is stated, that the well-known arching effect in reality is not present, a conclusion which seems in contradiction to many years experience and to some of the results mentioned in papers by Peck and Berman (Vb 5), Golder (Vb 1) and Van Iterson (Vc 2)".

No such generalized statement was made in the papers by myself and my associates.

I regret to say that in this respect Professor Bretting has completely misquoted us in his General Report, so that I now have to go formally on record as most emphatically rejecting any interpretation which attributes to us such obviously unwarranted generalizations. In all our papers we have repeatedly emphasized the fact that our findings refer to normally backfilled and anchored flexible bulkheads and that the breakdown of arching observed by us in our tests is caused by normal displacements of the bulkhead which deprived the sand "arches" of stable "abutments". Since Professor Bretting states that our conclusions "seem in contradiction to many years' experience" I would like to ask him to name a single case when arch segments of uncemented sand grains withstood collapse after a yield of one of their abutments.

The paper by Peck and Berman (Vb 5) refers to shafts and the paper by van Iterson (Vc 2) refers to tunnels, that is to stable conditions of arching, which, as already explained, are totally different from those prevailling behind flexible bulkheads. Therefore there cannot be any contradiction between their test results and ours and a statement by Professor Bretting claiming such a contradiction in his Synopsis of Section V is absolutely unjustified. Further, one of the main findings of our tests, that is the decrease of active lateral pressure due to shearing stresses at the dredge line boundary, is not even mentioned by Professor Bretting in connection with our papers, but an analogous finding by Peck and Berman (Vb 5) is emphasized; although in the case described by them it is of a relatively lesser importance than in ours. Similarly it can also be shown that there is no contradiction whatsoever between our results and those of Golder (Vb 1).

Professor Bretting acknowledges that the Danish bulkhead regulations "have no real theoretical basis". To this I should add that neither do they seem to have any real experimental or practical basis, since no measurements appear to have been made to justify them. The fact that bulkheads designed according to these regulations have not failed is fully explained by our findings which disclosed a completely different mechanism of bending moment decrease than the one previously assumed both by the Danish Society of Engineers and by Professor Terzaghi. Apart from the presence of the effect of shearing stresses at the dredge line which reduce the active pressures near it, the residual maximum passive pressures are located much closer to the dredge line than is usually assumed.

The preceding discussion should show that there is no reason for Professor Bretting to question our test results simply because they appear to disagree with observations made elsewhere. The disagreement does not exist.

These remains the question raised by Professor Bretting concerning the accuracy of our measurements and computations. Before demonstrating the reliability of our results, I think it is only proper first to subject to a critical examination the only tests so far performed on which are based opinions contrary to ours concerning the arching of sands in a vertical direction behind flexible sheet pile bulkheads. I refer to the tests by Stroyer reported in 1935.

Fig. 1 shows the apparatus used by Stroyer. It is a three ft. by three ft. box, closed by a metal plate with completely unyielding supports. Deflexion of plate was prevented until completed backfilling. Under similar conditions we also got arching.

However, such conditions cannot arise in the field under normal backfilling procedures. Further, Stroyer found that a slight movement of the sand induced by opening the trap door immediately broke down the arching and induced what he termed a state of "flux". It is an extremely important point which everybody disregarded at the time, myself included.

It simply means, that if a support of an arch moves - as this happens under field conditions simulated by our tests - then all arching should immediately disappear. Thus there is no contradiction between Stroyer's tests and our results.

Fig. 2 illustrates the Danish regulations. This is not a bending moment diagram but a pressure diagram. One can see that after deducting the surcharge effect, there is practically no pressure left at the center of the span.

The diagram does not explain how the active pressures are balanced out below the dredge line by the passive pressures, although this is of the utmost importance. Thus one can see that this diagram represents only an uncertain and incomplete general concept of the problem. It has not been limited to any one type of bulkhead.

The choice by Prof. Bretting of fig. 8, Test 12a, (Vb 2) to illustrate the "unreliability" of our pressure determinations is unjustified, because we ourselves used it only to show an exceptional condition produced by the layered system of clay and sand. The resulting discontinuities in the bending moment curve precluded the use of the "smoothing out" procedure in this case, since not enough points were available for each curve section of the same curvature. This test is not typical of our usual conditions, but is an exception. Typical test results are presented by fig. 3 (fig9 of paper Vb2 , p.85, Vol.II). It was not mentioned by Prof. Bretting.

There are several additional aids to our double differentiation procedure, apart from the "smoothing out" method, which leaves very little room for the exercise of personal judgement when sufficient points are available. These additional aids are: first the shear must be zero at the points of maximum bending moment; second the slopes of the two branches of the shear curve at anchor level must be equal. Similar checks apply to the pressure curves. It is fully realized that near the anchor level the pressure curve thus determined is subject to appreciable errors if these are expressed in percents of the correct values. Nevertheless the accuracy of determination of the overall shape and values of the pressure curve is satis-

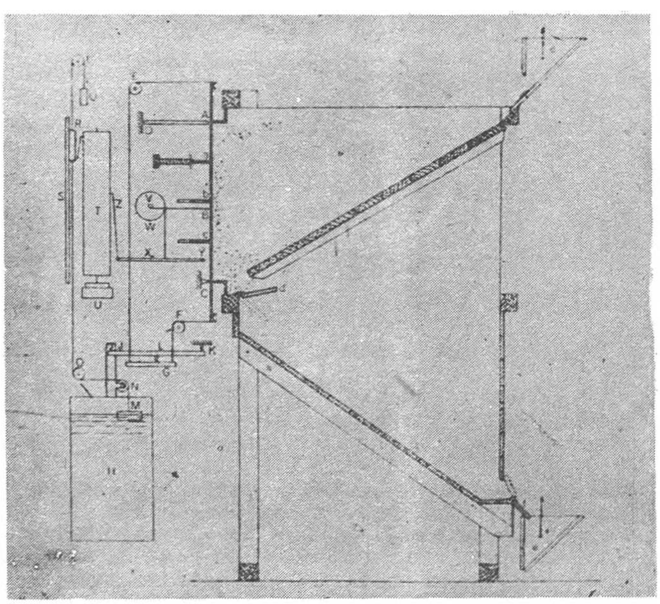
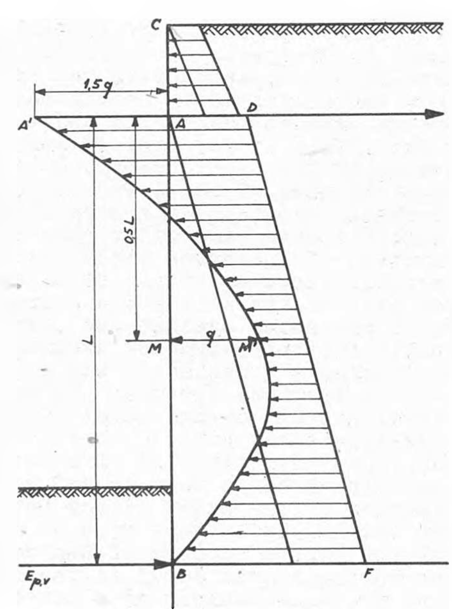


FIG. 1



PRESSURE DISTRIBUTION RECOMMENDED BY THE DANISH SOCIETY OF ENGINEERS

FIG. 2

factory because, after computing the pressure curves, we checked always back by computing the bending moments at three points where they had maximum values. These three points are: the anchor level; the point of maximum moment between anchor level and dredge level; and the point of maximum moment below dredge vel.

Usual agreement was within 6% or 7%; never exceeding a difference of 10%. We have more than 30 test stages from six separate tests with sand, when the results always gave the same shape of the pressure curves.

At the center of the span its determination is particularly accurate and most certainly there is no decrease of pressure there, as there should be, if vertical arching existed.

Professor Bretting expresses a rather unusual criticism by regretting that we did not give the direct readings of our instruments. I am not aware that this has ever been done before by any previous authors of similar papers, since normally space is not available for the purpose. Nevertheless, so as not to leave any doubts in anyone's mind as a result of Prof. Bretting's criticism, I have obtained the agreement of Mr. Glossop and of Mr. Golder, editors of the new bi-annual publication "Geotechnique", to provide space in their next issue for a paper which shall give all the instrument readings and subsequent computations of a typical stage in one of our tests.

It is believed that the actual accuracy of our results is greater than can be obtained at the present time by any other known method of measurement.

Another important point raised by Prof. Bretting concerns the stress-strain relationships of disturbed plastic clays. I was most gratified to see that Professor Bretting has accepted our findings showing that with outward movement of the wall there is no decrease of lateral pressures of re-consolidated plastic clays so long as no rigid horizontal boundary restrained the adjoining clay. Professor Bretting even finds this result quite natural and, in addition advances a partially acceptable explanation for the observed decrease of lateral pressures during consolidation of the clay with no outward wall movement being necessary to achieve this reduction. It is quite possible that the intergranular movements during consolidatioh may fully mobilize the internal friction of the soil as suggested by Professor Bretting; but it is also possible that not yet fully understood physico-chemical phenomena not requiring motion at contact surfaces of soil particles are responsible for the lateral pressure reduction.

However, in any case conventional stress-strain concepts most certainly cannot be applied to this condition, although Professor Bretting claims the contrary. The point is of practical importance, since it is the application of conventional stress-strain concepts to earth pressure problems which led to the widespread erroneous belief according to which lateral outward movements of up to 5% of the height of the wall would be necessary to reduce the lateral pressure of plastic clays to their minimum value. In this respect results very similar to ours have been independently obtained by the Research Department of the Delft Soil Mechanics Laboratory. The active and the neutral pressures of clays appear to be almost identical.

As a result of discussions with Mr. Geuze, Head of that Department, I however, consider it necessary to modify one of the statements in our papers, although that particular statement appears to have been accepted by Prof. Bretting. I refer to our test results which appeared to indicate that the neutral earth pressure ratio of most not overconsolidated inorganic soils did not differ by more than $\pm$ 10% from the value K = 0.50. It would appear that the deviation may be greater, since slow cell tests at the Delft Laboratory gave neutral values as low as K = 0.35 for some clays. This deviation, however, is in the opposite direction from the one usually assumed.

The following additional points are concerned with the General Report of Prof. Bretting:

The sand dyke tests at Princeton University described by Mr. Epstein in the paper No. Va 4, pp 291, Vol.III were concerned with flexible walls and not with rigid walls. The classification given to that paper in the Proceedings is therefore in error.

Every sand surface served as a drainage surface for the fluid clay tests.

Fig. 3 gives values which refer to a test stage <u>prior</u> to the release of the anchor.

The bulkhead is ¼ inch thick. Fig. 3 corresponds to a test when the flexibility of the bulkhead was about twice as great as it should have been for conditions of complete model similarity. Nevertheless no signs of arching developed. Tests with smaller flexibility and different depths of embedment are being performed and will be reported in the closing discussion of my recent paper on this subject in the January 1948 Proceedings of the American Society of Civil Engineers.

None of our tests are intended to simulate the very complex conditions of bulkheads combined with a relieving platform on piles. A test simulating a "sunk wall" will however be performed.

<u>REFERENCE</u>

1) Earth pressure on flexible walls. J. Stroyer, Paper 5024. Journal of the Institution of Civil Engineers, London Volume I 1935-36 pp 94-139 with discussion by K. Terzaghi pp 550 - 557.

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A BRIEF STUDY OF THE THRUST OF AN ANCHORING SCREEN CLOSE TO THE WALL IT SUPPORTS

V b 11

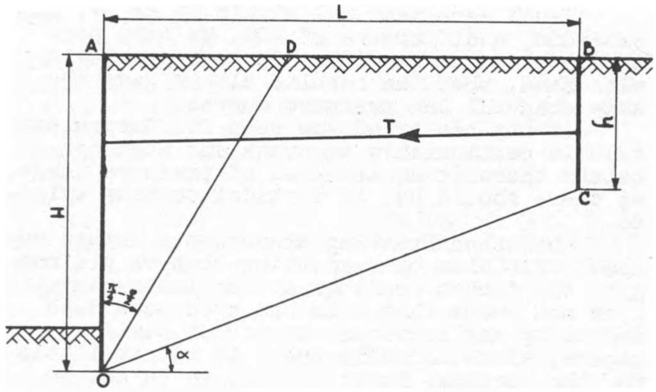
A. COUARD (France)

In previous articles we have examined the thrust of an unlimited terrain in front of the anchoring screen, in other words, the coefficients of thrust, as well as the influence of marginal thrust and the relationship between the height of the screen and the stake at its base. It is possible to examine summarily by an elementary calculation the order of magnitude of the thrust, taking into account the length of the stays.

As reality may show a difference of about 30% from the theoretical result, according as Caquot's effect applies more or less effectively to the method of packing, which affects the maximum angle of intergrowth, it is useless and illusive to look for such precision and strictness in the calculations as would render them impregnable. A further considerable uncertain factor is provided by the heterogenerty of the soil and the customary lack of data at our disposal as to its angle φ and its cohesion. The calculations should therefore not be considered as a guide, starting from the hypothesis that practice and experience suggest should be adopted in every individual case.

Let us take a quay wall with a height H above the point of pressure O, the density of the soil being Δ and its angle of friction φ, let L be the length of the stays and h the height of the anchoring screen assuming it to be continued up to the surface of the solid ground.

Let us join the point of pressure O at the base of the anchoring screen, we shall take the length L minimum is that for which the frinction of the volume O A B C on the plane O C is higher than the component accor-



ding to O C of the weight of this volume, plus the component parallel to the tension of the stay, plus the component perpendicular at O C of the tension T of the stay multiplied by tg φ. One might be tempted not to take into account the prism of thrust O A D, but it is nevertheless a contributory factor on account of its weight of friction O D B C on O C and it would be too pessimistic to neglect it.

We consequently arrive at the formula:

$$(h + H)\ (\operatorname{tg}\varphi - \operatorname{tg}\alpha)\ \Delta L = 2\ T\ (1 + \operatorname{tg}\varphi\ \operatorname{tg}\alpha)$$

and the results obtained in this manner, may be usefully compared with those shown by other methods, before finally fixing the characteristics of the structure.

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FRESH COMMENTS ON THE COEFFICIENTS OF THRUST AND PRESSURE OF PULVERULENT SOILS AS WELL AS ON THE LENGTH OF ANCHORING STAYS

V b 12

A. COUARD (France)

It is rather striking to note that generally quite an illusory exactness is required of notes on calculations, if we compare this exactness with the remarkable lack of precision of the hypotheses on which they are based. This is notably so in problems in which soil pressure and thrust play a part and there is no lack of real experts who - often contradicted by facts - make light of theoretical data on such occasions, whenever they are unduly pessimistic.

Having had an opportunity of carrying out some brief experiments on the thrust of pulverulent banks, we were struck by the anomalies - perhaps apparent, but at all events repetitive- both in the numerical results and in the physical characteristics of the phenomena, which manifested themselves as and when increasing forces were applied.

The experiments having given us the results and the immediate indications, we had expected, we next endeavoured to reconcile the conceptions hitherto accepted with the results of the experiments, instead of forming a new theory, which it would have been presumptuous to hope to find.

The two anomalies regularly observed are:

1. A sliding plane (or a sliding surface, which for the sake of simplicity, we took to be plane, without modifying in the least the order of magnitude of our conclusions) forming an angle with the horizontal suggesting a φ greater to that we measured.

2. A double coefficient of thrust: To compress the bank of dry sand, we took a wooden plank, to which we stuck sandpaper, to ensure

our obtaining $\beta = \varphi$. On applying to this plank a regularly increasing force, we observed first of all slight settling movements of a very low amplitude, followed by clear stability. Then with a relatively considerable force, a movement of the plank by 1 or 2 centimetres, accompanied by a creeping of the sand, delimiting the prism of thrust. Next there followed a clear state of stability, this movement having stopped of its own accord when the force on the plank increased. Then at another and greater value of the force there was a clean break the movement not stopping once it had commenced.

If we compare the numerical results obtained experimentally with the values we expected to get on applying Mr. Ravizé 's coefficients, we find that the final break is produced in accordance with the theory, assuming $\beta = \varphi$, but that the initial break is produced at a value of the force which is too low for $\beta = \varphi$ and too high for $\beta = 0$.

We were then induced, to consider this anomaly simultaneously with that outlined in (1) and to seek a physical explanation in the behaviour of the soil.

We believe that it is Mr. Caquot's theory as to the intergrowth of grains which should be called in.

When the plank commences to press back the soil, it compresses it and the grains become interlocked. When the force increases,the soil has no vertical motion and the friction on the plank has no chance of manifesting itself, until the prism of thrust shifts to the plane of breaking.

At this moment we have φ apparent and $\beta = 0$ but since the fracture has commenced, there is a creeping of the prism, due to the increase in the volume of the sand, which is less compact as the intergrowth is destroyed. φ drops to its real value and β is only fully manifested when the forward movement of the prism is sufficient for the vertical movement of the sand to attain an adequate amplitude. To pass from φ apparent $\beta = 0$ to φ real $\beta = \varphi$ requires an appreciable movement, but one which stops of its own accord.

This hypothesis is confirmed by the angle of the plane of breaking, which quite corresponds to the value of φ apparent. In this way, we would be led to the conclusion that:

If $\varphi < 25^\circ$ there is no need to consider two coefficients of thrust for φ apparent would have a greater coefficient than φ real $\beta = \varphi$ and practically there would only be a single limit of fracture. Moreover, such a small angle of friction sooner indicates a clayey soil, than a pulverulent one and in these conditions we do not know in what way Caquot's effect is manifested.

If $\varphi > 25^\circ$ the difference between the two thrustvalues the greater according as φ is higher and at this succeeding moment when we can accept a slight displacement of the work, or its anchorages, it is necessary to take one or the other of these coefficients.

The same reasoning may be applied to the coefficients of pressure, according to which the soil is in situ or has been filled up, whether it is able to mobilize itself before thrusting on to the sides.

In the same way, the establishment of the fact, that the thrust of an anchorage depends on the soil which it consolidates, shows, that the coefficient of thrust to be taken into account varies linearly with the length of trusses outside the prism of thrust, up to a limit value corresponding to the maximum thrust, for a given depth of the anchoring screen, which readily enables curves to be drawn which show thrust as a function of H, h and L.

These hypotheses permit us to obtain a satisfactory agreement between experimental results and theoretical forecasts, but the fact must not be neglected that the penalty to be paid for permitted economics by an exact calculation is the necessity of appreciating just as precisely the characteristics of the soil which is legitimately considerable. In this respect a certain degree of practice is not without its use.

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V b 13 WRITTEN DISCUSSION ON PAPERS Vb 2 AND Vb 7

J. BRINCH HANSEN (Denmark)

The Princeton tests of the earth pressure on flexible bulkheads are no doubt the best, the most extensive and the most carefully performed tests of their kind.

Against the testing installation with its ingenious measuring devices any severe criticism can hardly be made. It is only to be regretted that no instruments were devised and used for direct measurement of the lateral earth pressures on the bulkheads, as the indirect methods used for this purpose seem to give rather inaccurate results.

Setting aside methods which determine only 2 or 3 points of the pressure curve, the remaining method used by the investigators comprises a double differentiation of the moment curve. In order to do this a smooth moment curve must be drawn between the observed values, and in the same way the ensuing shear curve must be smoothed out.

This procedure is not only very susceptible to errors in the observed moments; but in addition, it involves a considerable personal judgment and the result seems to be strongly influenced by the preconceived ideas of the investigator.

Take f.inst. fig. 9 of the paper Vb 2 (Proceedings Vol. II.p.85). In stage B the investigators have found a triangular pressure distribution above anchor level. If this were true the relation between the moments at anchor level and at half anchor depth should be abt. 8:1, but the moment curve clearly shows a relation of 4:1, indicating that the actual pressure must be roughly constant above anchor level.

Below anchor level the moment curve is drawn as a straight line for at least 1/5 of the distance between anchor level and dredge level, and consequently an unbiased investigator ought to have found zero or very small pressures immediately below anchor level. Nevertheless, the pressure curve in fig. 9 does not show the slightest decrease of pressure at this point.

Through calculations which I have carried out by means of the plasticity theory I have found a pressure redistribution - as compared with Coulomb's triangle - with a decrease somewhat above the point, where the bulkhead has its greatest deflection, and a corresponding increase still higher up.

In the case of the Princeton test recorded in fig. 9, the calculated decrease would roughly coincide with the observed (but not recorded) pressure decrease immediately below anchor level. I am aware, however, that another explanation may be offered for this phenomenon, namely that the anchors are acting as a kind of relieving platform, carrying in part the fill above. I should like to ask the investigators whether the testing arrangement was such that they would consider the latter explanation the more plausible.

By most of the Princeton tests a considerable pressure decrease is found immediately above dredge level, whereas by my plasticity calculations I have not been able to find any appreciable decrease at this point. Moreover, it seems to me that the decrease found by the investigators may very well for the greater part be simply a result of the smoothing-out of the moment- and shear curves. Even if the real pressure curve forms an angle or makes a jump at dredge level, the said smooting-out may easily lead to a pressure curve approximately of the shape found by the investigators.

Although thus the details of the pressure distribution cannot be said to be ascertained with sufficient accuracy by the Princeton tests, it is clear, nevertheless, that so pronounced a pressure redistribution, as is presumed by the so-called Danish Method, is not developed generally, at least not by normal bulkhead deflections.

On the other hand, it is an established fact that the many bulkhead structures, that have been designed and constructed in accordance with the Danish Method, have stood at least equally well as other structures. The same applies to the bulkheads, built by Christiani & Nielsen prior to the appearance of this method. These were designed for earth pressures according to Coulomb's theory, but with working stresses 2-3 times as great as those ordinarily allowed.

The fact that such methods have proved satisfactory in practice is probably in part due to the actual earth pressures being smaller and to a certain degree, more favourably distributed than generally assumed. In this connection it should be mentioned that the major part of the reduction indicated in fig. 11 of the paper Vb 7 (Proceedings Vol. III. p. 313), will be accounted for, if full wall friction is assumed and the angle of internal friction is put at 32°-36° as found by direct tests (Proceedings ASCE, Jan.1948. p.19). The theoretical K-value for the horizontal pressure will then be 0.24-0.20.

However, this is clearly not sufficient to reduce the bulkhead dimensions to the values that have proved satisfactory in practice, if ordinary allowable stresses must be used. Therefore, the logical conclusion seems to be, that considerably higher stresses can safely be allowed in bulkhead structures. One reason for this may be that the acting forces are largely static and of comparatively well-known magnitude, so that the risk of overloading is small. E. Epstein (Proceedings ASCE. Jan.1948. p.70) arrives at the same conclusion on the motive that the factors of safety for wall and earth ought to be approximately equal. It should finally be noted that it is essentially the same conclusion, to which Christiani & Nielsen came as early as 1905.

With regard to further Princeton tests I should like to express the wish, that the test conditions as well as the direct measurements will be published as fully as possible in order to enable others to make their own conclusions which may, or may not, coincide with those of the investigators themselves.

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SUB-SECTION V c

EARTH PRESSURE AGAINST UNDERGROUND CONSTRUCTIONS

V c 4

WRITTEN DISCUSSION ON PAPER Vc 3

G.P. TSCHEBOTARIOFF (U.S.A.)

I. INTRODUCTION

The present discussion will be limited to the analysis of the "neutral earth pressure ratio k_n" as given by the authors, and of its significance in relation to the classical earth pressure theories which are used by many contributors to the Proceedings of this Conference. This same ratio is referred to elsewhere as the "earth pressure at rest ratio" 1), 4) or the "consolidated equilibrium ratio" 12),14)

The writer is in general agreement with the authors' statement that this ratio is "an exclusively experimental value which should only be determined from field experiments in a laboratory of Soil Mechanics". However the authors then give the following actual numerical values for different soils apparently based on some of the very early experiments by Terzaghi:

dense sand k_n = 0.40 to 0.50.

loose sand k_n = 0.45 to 0.50.

clay k_n = 0.60 to 0.75.

Since the above values are not in full agreement with the writer's own experimental data and with the results reported from other sources, a brief discussion of the entire question of the "neutral" or "at rest" earth pressure ratios appears indicated. The knowledge of the value of this ratio is of particular importance in the case of disturbed clays, since for such soils the "at rest" and the "active" earth pressure ratios appear to be almost identical.

II. VALUES OF THE "NEUTRAL" OR "AT REST" EARTH PRESSURE RATIOS

a) Sands. Values of the lateral earth pressure "at rest" for sands were reported by Terzaghi in 1920. The apparatus used was illustrated by Figure 4, page 633 of Ref. 1. The values of the lateral pressure "at rest" were determined from the resistance offered by a steel tape pulled through a rectangular mass of sand bounded by rigid surfaces on all sides, i.e. the load also was applied to the sand surface by means of a fully rigid piston. The "at rest" ratio of lateral to vertical pressures is given on page 635 of Ref. 1 and is stated to equal k_n = 0.42 both for poured in horizontal layers and for compacted layers of sand.

This value of k_n = 0.42 is smaller than the values obtained by the writer in the "Lateral Earth Pressure Meter" 14). During these Princeton tests the "at rest" or "consolidated equilibrium" ratio of pressures was found to approximately, (i.e. within ± 10%), equal k_n = 0.50 for all of the soil backfills tested. Our experiments with the Lateral Earth Pressure Meter have shown that the use of a rigid piston for the application of the vertical pressure decreases the observed lateral pressure by restraining lateral deformations of the soil (see Ref. 14, page 310, Fig. 3). The writer therefore believes that the k_n = 0.42 value established by Terzaghi in 1920 for sand is somewhat too low since appreciable lateral restraint existed in his apparatus both at the upper and at the lower rigid boundaries, the height of the layer of sand tested being approximately equal to only one third of its width.

A study of Terzaghi's later publications appears to indicate that he does not attach considerable importance to the exact values of this coefficient since the "k_n" value given by him in 1920 for loose sand was 0.42 (see Ref. 1, page 635); in 1934 (Ref. 3, page 140) he gives this value as being about 0.4 for loose sand backfill; in 1936 (see Ref. 4, page 279) he gives the values for loose sand as varying from 0.45 to 0.50. Also in 1936 Terzaghi gives for dense sand "k_n" values of 0.40 to 0.45; in 1920 (Ref. 1, page 635) this value was given by him as being equal to 0.42; but in 1934 (Ref. 3, page 140) Terzaghi stated that for compacted backfill the hydrostatic pressure k corresponding to the pressure acting in its original position on the wall (e.g. the "at rest" ratio k_n) may have any value between 0.35 to 0.7 depending on how the fill was made. Thus there are a number of numerical contradictions in the statements by the same distinguished author on this matter. This entire subject therefore requires re-examination.

In this connection it may be interesting to note the results of lateral earth pressure measurements reported by Gersevanoff in 1936 5). With a type of apparatus which was not specified the value of k_n = 0.41 for loose sand was obtained. It would appear from other references that this test was performed in an similar to the one used by Terzaghi. 1). However in the same Reference 5) Gersevanoff gives the results of a test with compacted sand performed in a confined triaxial (e.g. cell) apparatus. At low normal pressures the ratio k_n was equal to 1.00, but with increasing load it dropped to k_n = 0.50 and the curve flattened out at that value (see Figure 3, Ref. 5). This is in agreement with our Princeton values. There is no indication from the test results given by that diagram that the curve is a true hyperbola with a final value of k_n = 0.36, as appears to have been assumed by Gersevanoff.

b) Clay Fills and Disturbed Plastic Clays. In 1925 Terzaghi reported the results of measurements with clay soils placed in a completely disturbed and fluid condition and then reconsolidated in the apparatus shown on Figure 2, page 743, Ref. 2. The measurements were performed as described on page 798 of Ref. 2. The vertical pressure was applied to the clay through sand filters which were placed both at the upper and at the lower boundaries of the clay and which therefore exercised less restraint on lateral deformations of the clay than did the rigid pistons in the preceeding sand tests. Separate tests were made by pulling out, after completed consolidation of the clay, steel tapes which had been placed in the center of the layer prior to consolidation. In one test the tape was vertical, in the other it was horizontal. By taking the ratio of the resistance of the two tapes, Terzaghi obtained for one clay the value k_n = 0.70 and for the other clay k_n = 0.75. This is much higher than the values of approximately k = 0.50 (within ± 10 %) obtained by the writer by direct measurements on disturbed consolidated clays in the Lateral Earth Pressure Meter (Ref. 14, page 311, Fig.5) In this connection it should be noted that the method of measuring soil pressures by means of friction tapes can give enormous scatterings of results. Thus, by examining Figs. 1, 2 and 3 on pages 58 and 59 of Ref. 7 it may be seen that the differences between readings obtained by this method in most cases appreciably exceed 100% of the lower values. Further, the writer believes that in Terzaghi's 1925 experiments the vertical pressures were likely to have been measured too low since during the consolidation of the fluid clay surrounding the rigid horizontal tape smaller than the average vertical pressures are liable to develop on the lower face of the tape.

Therefore the writer believes that the "at rest" values of k_n = 0.72 and k_n = 0.75 reported by Terzaghi in 1925 for re-consolidated disturbed clay fills are too high because of deficiencies in the set-up which he himself described as "primitive" (Ref. 1, p. 634).

c) Undisturbed Natural Clays. With this type of brittle material Terzaghi does not seem to have made any direct measurements so far; in 1943 (Ref. 9) he states that for a normally consolidated bed of clay the ratio k_n "is likely to range between 2/3 and 7/8" (e.g. between 0.66 and 0.87) At the same time (1943) his immediate associate, R.B. Peck, went even further and stated on p. 1018 of Ref. 10 that the coefficient of earth pressure at rest "for clays in their natural state is not yet known". The writer also has not performed so far any direct measurements with undisturbed clays, but a number of tests with the confined triaxial (or cell) apparatus reported by other authors are of interest in this connection. Thus Gersevanoff reported in 1936 on page 49 Figures 4 and 5 of Ref. 5 the results of tests with two types of undisturbed loam which show that the ratio "k_n" gradually increased from zero with increasing vertical pressure until the curve flattened out at a value of $k_n = 0.55$ in Figure 4 and approached the value of $k_n = 0.50$ in Figure 5. This again is very close to our Princeton values. The values of 0.62 and 0.65 reported by Gersevanoff on these diagrams were obtained by assuming that the curve is a true hyperbola although the readings shown on the diagrams do not justify this assumption. It would appear from other references that it was influenced by Terzaghi's earlier results.

Very interesting results were reported in 1936 by Ir. E.C.W.A. Geuze who showed by Figures 5 and 6 of his unpublished report Ref. 6 that before the vertical pressure in a confined triaxial cell apparatus reached the value of the pre-consolidation load the "k" values were appreciably smaller than the values after the pre-consolidation load was exceeded. The values reported on Figures 5 and 6 of Ref. 6 were obtained from rapid tests. Therefore they were influenced by the unequalized excess pore pressures and may have been higher than "k_n" values as defined above. In this connection the writer was very interested to see from the paper No. IIf 2 by Ir. E.C.W.A. Geuze on page 154 and 155 of Volume III of the Proceedings of this Conference, that, after completed consolidation, the lateral pressure values of three specimens from an undisturbed sample of heavy clay with traces of peat were slightly <u>smaller</u> than 0.50 and were equal to $k_n = 0.47$; $\overline{k_n = 0.48}$; and $k_n = 0.38$, respectively; whereas prior to this and immediately after the initial application of the load the k values were 0.64; 0.57; and 0.58 respectively. The low "k_n" value of the third sample may have been caused by a somewhat excessive percentage of peat, since the "k" values of peat soils appear to be usually lower than the corresponding values for inorganic soils.

It should be further noted that in 1943 (Ref. 11, page 1045, Fig. 41) Housel reported the results of measurements on Detroit sewer tunnels through clay from which the final "consolidated equilibrium" ratio of lateral to vertical pressure can be computed as being equal to 0.66. However, it is probable that the measured pressure values partially included seepage forces directed towards the tunnel and that therefore the above value does not represent a true "k_n" ratio of natural intergranular pressures and is higher than this ratio.

The results of all these tests therefore confirm the writer in the belief that for a natural state of consolidation of any clay, that is, for vertical pressures equal to or greater than the past pre-consolidation load, the intergranular "at rest" or "consolidated equilibrium" pressure ratios are of the same order of dimension as for sands, i.e. $k_n = 0{,}50$, as found during his experiments at Princeton.

III. <u>EFFECT OF SAMPLE EXPANSION ON THE "AT REST" AND ON THE "ACTIVE" EARTH PRESSURE RATIOS OF CLAYS.</u>

The writer was very much interested by the statement of the authors that: "according to Keverling Buisman IV), however, the neutral pressure in cohesive soils approaches the active pressure". This is equivalent to saying that expansion of a clay sample does not change its lateral pressure, and therefore the point is of considerable practical importance. Unfortunately, due to language and to war-time communication difficulties, details concerning the work of Buisman and of his accociates in The Netherlands are not well known on this side of the Atlantic.

During the writer's own experiments at Princeton in 1943-48, similar findings were independently made in so far as naturally consolidated clay backfills were concerned (see Ref. 12; p. 85). The writer however went further in interpreting the practical significance of this finding, - (see p. 86, Ref. 12 and p. 30 of The January 1948 Proceedings Am. Soc. C.E.) - and concluded that there should be no numerical relationship between the laboratory shearing strength at failure of naturally consolidated plastic, - (i.e. with no brittle inner structure), - clay samples and both their "consolidated equilibrium" and their "active" lateral pressure coefficients. These findings were frequently received with surprise by engineers familiar only with some of the previous contrary authoritative statements concerning the effect of motion of supports on earth pressure against them. Many of these statements, however, contradict each other and should therefore briefly be reviewed.

In Ref. No. 4, Terzaghi wrote in 1936: "The fundamental assumptions of Rankine's earth pressure theory are incompatible with the known relation between stress and strain in soils, including sand. Therefore the use of this theory should be discontinued". Nevertheless, in subsequent publications, e. g. in 1943, the Rankine theory and its modifications were used by Terzaghi and by his close associate, R.B. Peck, for the analysis of actual pressure measurements performed on the Chicago Subway 9), 10).

No reasons for this complete reversal of attitude were given. At the same time the opinion was expressed by Terzaghi (also in 1943) that an outward movement of over 5% of the height of the retaining wall might be necessary in order fully to mobilize the shearing strength of a clay backfill, (Ref. 8, p. 96). No movements of this magnitude actually occurred on the Chicago subway cuts, nevertheless the laboratory shearing stress at failure of the clay was used for the analysis of the results. Further, when the sheeting was allowed to yield, the effect was exactly the opposite to the one to be expected on the basis of the preceeding assertion. Thus on p. 1035 of Ref. 10 Peck states that "the magnitude of the pressure remains substantially unchanged for a yield up to 1% . Beyond this value, the lateral pressure again increases". In the writer's opinion this result is due to the breakdown of the brittle inner structure of the natural clay which breakdown is caused by the induced expansion of the clay.

The obvious contradictions between all these data made the writer surmise that the agreement reported in 1943 by Peck 10) between the lateral pressures as measured in the Chicago Subway cuts and as computed from the formulas he developed on the basis of the strength at failure of clay specimens may have serious limitations. A supplementary analysis of Peck's 1943 data performed in 1947 by one of the writer's students, Philip Brown, appears to confirm this point of view. Fig. 8 on page 37 of Ref. 13 shows that there is no agreement between Peck's computed and measured results at depths of cuts smaller than the maximum attained on that job. Whether there would or would not be any agreement at greater depths or cuts remains an open question which requires solution by further research.

IV. CONCLUSIONS

1) The values of the "neutral earth pressure ratio k_n" given by the authors for consolidated clays are based on old tests performed with inadequante facilities. These values are too high. The corresponding values for sand are in part too low.
2) Buisman's findings concerning the identity of the neutral and of the active pressure ratios of clays agree with the independent later findings of the writer in respect to disturbed and re-consolidated clays.
3) There appears to be very little difference between both neutral and active "consolidated equilibrium k_n" values of all inorganic soils except in the pressure range where the active values are decreased by the resistance to deformation of the brittle inner structure of a natural undisturbed clay soil or where the neutral values are increased by backfill compaction.
4) The use of so-called "classical earth pressure theories" in conjunction with the "laboratory" strengths of soils at failure does not appear justified for most cohesive earth pressure problems since they are problems of deformation and not of rupture. The limitations of these theories should therefore be carefully reconsidered. The "cell" type of test as described by contributors to the Proceedings of this Conference from the Netherlands and Belgium, with possible modifications in technique, should prove a valuable accessory to that end.

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CLOSING DISCUSSION

Prof. K. TERZAGHI (U.S.A.)

The problem of the lateral earth pressure on earth-retaining structures was the first one, in the field of soil mechanics, which attracted analytical minds and efforts to solve the problem extend over a period of more than 150 years. Nevertheless the paper and discussions show that it is still far from being completely solved. Foremost among the controversial issues we find the intensity and distribution of the earth pressure on flexible bulkheads.

During tne last few years Prof. Tschebotarioff has made extensive laboratory tests at Princeton University with the intention of solving the problem. However the subject of his investigations appears to be as elusive as are the elastic properties of clay in an undisturbed state. As a consequence the results of his tests still leave a wide margin for interpretation and it would be premature to accept any of the conclusions of the experimenters unless and until they are confirmed by the results of pressure measurements on full-sized sheet pile bulkheads driven into very different soils and acted upon by very different backfills. As a matter of fact it is doubtful whether a definite rule regarding the pressure distribution on flexible bulkheads can be established at all which would be valid under all the conditions to be encountered in practice.

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