Earth Pressure, Retaining Walls, Tunnels and Shafts in Soils

Poussée des terres, murs de soutènement, tunnels et puits dans les sols

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Mr. *R. Briske*, Germany
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Mr. *S. Packshaw*, Great Britain
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General Reporter Session 7 – Rapporteur général Session 7

The General Reporter

The period of five years since our last Conference has seen a number of changes and developments in earth pressure problems; including, I am glad to say, several valuable field measurements, which have been published as case records. Most of these developments are important and I have tried to outline them in my general report. This morning we have, as you will see, eleven names of people who wish to take part in the discussion and therefore I shall reduce my opening remarks to a minimum, making a few comments only on 4 topics: sheet piles, earth pressure distribution, tunnels and creep.

It seems that the subject which has attracted most attention, during the past 5 years, has been that of anchored sheet pile walls. At the last conference Prof. *Tschebotarioff* had proposed a new design procedure. In 1952 Dr. *Rowe* of Great Britain, published a important paper on the subject. In the same year Prof. *J. Verdeyen* of Belgium also published a paper, and in a paper to the Conference we have Mr. *Brinch Hansen* of Denmark, putting forward a further procedure. Immediately before the conference, Mr. *Briske* of Germany has published a considerable monograph on the subject, which I have not yet had an opportunity to read. Now I may remind you that the usually accepted method of designing sheet pile walls, is known as the fixed end method, described in very many places, for example in *Terzaghi*'s "Theoretical Soil Mechanics". In contrast to this method Prof. *Tschebotarioff* has suggested the use of a simplified equivalent beam method and Dr. *Rowe* proposes designing, from the stability point of view by the free end method, and then applying moment reduction factors depending on flexiblity of the piling and also on the nature of soil.

I may say that, although not yet in print, Prof. *Terzaghi* has to some extent accepted *Rowe*'s procedure and shortly will be publishing a paper which unifies *Rowe*'s approach for sands and all soil types. Prof. *Verdeyen* suggests a modified pressure distribution from that normally used and Mr. *Brinch Hansen* has put forward on extremely interesting method of design, based on plastic equilibrium. Now I think there is no question that we can possibly decide, this morning, which of these various new methods is the best, and experience in the field and also experience in the design office will be the final test to decide which of these methods, or indeed whether the old method itself, is the most preferable. Nevertheless I think we can look forward to an interesting discussion since most of the authors, whom I have just mentioned, are in fact taking part in the discussion.

The second topic on which I wish to comment concerns pressure distribution. The experiments by Dr. *Rowe* indicate that the distribution of active pressure on sheet pile walls is in accordance with the classical theory, provided that the anchor is allowed to yield a little. The movement which he found in his tests to be sufficient to reduce any original, non-linear distribution to the linear form is very small; so small in fact that it would appear from this tests that we could say in practice that this small yield is almost bound to occur, and hence we are bound to use the classical linear distribution of active pressure. This is a topic which I hope will come up for debate. In a similar way some full-scale field observations made by the Building Research Station, reported by Mr. *Ward* and myself, in a deep cofferdam in soft clay in the Thames estuary, indicate that the pressure distribution from the clay on the piling, might well have been according to classical theory. Although the actual distribution of load in the trusts was far from linear we were able to show that by taking into account the flexibility of the piling and the deformations which occurred during construction, there was no need to postulate any variation from the classical pressure distribution. Therefore it seems to me that we must recognize that there is an increasing amount of evidence that, in the past, rather too much attention may have been paid to the possibility of deviations from pressure distributions of the classical form.

The third topic I wish to mention is tunnels. For quite a long time we have had no papers dealing with tunnels. I therefore am particularly glad to see that several excellent papers have been recently published in France, and also in the Conference Proceedings. I draw attention particularly to some extremely interesting measurements made in two tunnels in London, and reported by Dr. *Cooling* and Mr. *Ward*, which show that the pressure exerted on these tunnels was of the order of 70% of the overburden pressure; the tunnels being at a depth of about 100 feet. Allowing for variations, it would seem to me that one would have to assume, for safety, the full overburden pressure. Such measurements are invaluable as, in my opinion, we have no reliable theories for predicting such pressures on tunnels in clay. The French papers which I mentioned referred to measurements which were made in rocks, and were carried out mostly under the general direction of Mr. *Mayer*. They seem to me to be very important. There is also, in that connection, a valuable theoretical paper by *Terzaghi* and *Richardt*, which will be found in Géotechnique, which shows the great significance of the natural stresses existing in the rock stratum before tunneling.

The last topic I wish to mention is "creep". It has been obvious for many years that creep should be quite an important factor in earth pressure calculations. Yet there has been a rather conspicuous absence of any data on this subject. I refer particularly to field data, because, with due respect, I do not see how laboratory creep tests can tell us a great deal about what is going to happen in practice. We know too little about the problem yet. Moreover in many cases, and I think that it may be in the majority of cases, creep is not taking place in the field at a constant water content. Yet, the laboratory tests have been carried out under this condition. For example, if we have a sheet pile wall, and if as is commonly the case, some filling is placed on top of the clay to bring the ground level up to a specified height, then during the course of the few years following construction the clay will tend to consolidate and get stronger. This may far more than offset any creep effect. Nevertheless the importance of creep cannot be denied and therefore I welcome particularly the paper in the conference by Prof. *Haefeli* on the pressures developed on the abutment of one of the concrete bridges here in Switzerland. The pressures developed on the abutment of this bridge have been measured and are extremely high; these pressures being developed as the result of creep of the soil in which the abutments of the bridge are placed. I personally would be very interested to know whether this particular experience is in any way exceptional. If it is not exceptional, then much of our design concerning bridge abutments is wrong, because these forces which Prof. *Haefeli* has measured, as I mentioned, are in fact very considerable.

I will not take up any more of your time except that I would like to say, as a General Reporter, how very much indeed I was helped in my task by Dr. *von Moos*, and I would like to express my gratitude to him.

Le rapporteur général récapitule brièvement les divers travaux exécutés au cours des années écoulées sur lesquels portera la discussion, à savoir: l'ancrage des rideaux de palplanches, la poussée des terres, les tunnels et le fluage.

Prof. G. P. Tschebotarioff

The General Report mentions the understandably persistent attempts of several investigators aimed at correlating the lateral pressure of plastic clay to its shearing strength, as determined by the convenient and inexpensive *short* duration tests rapidly carried to failure. This approach, which I call the "strength method", in its present form largely neglects the effects of slow plastic deformations of the clay. That the effects of such "creep" on the solution of other soil engineering problems can be considerable, has been repeatedly emphasized during this Conference.

Another method of design, the "neutral earth pressure ratio method" (*Tschebotarioff*, 1951, pp. 488–492) has for starting point "consolidated plastic equilibrium" conditions. Relevant data is as yet incomplete, but a program of research is now in progress at Princeton University. Meanwhile, the "neutral ratio method" should not be ignored, since even in its present incomplete stage of development this method in several cases has proved to be in better agreement with field measurements than the "strength methods", for instance the method advocated by *Peck* (1943). This was the case on the Rotterdam tunnel approach (*Tschebotarioff*, 1951, pp. 281–286) and is the case in Chicago as shown on Table 1, which amplifies a table by *Wu* and *Berman* (1953). That *Peck*'s "strength method" would give uneconomical values at greater depths of excavation was forecast in 1948 by *Philip Brown*. For shallow depths it is unsafe (*Brown*, 1948; *Tschebotarioff*, 1951, pp. 276–281).

Table 1 Comparison between Design and Maximum Measured Strut Loads for Maximum Depth of Excavation H = 63 Feet in Chicago Clay
Comparaison des valeurs maxima – calculées et mesurées – des efforts dans les étançons pour une profondeur maximum de fouille H = 63 pieds dans l'argile de Chicago

Strut No.	*Elevation (ft.)*	*Design loads (P_s)* "Strength" method *(after Peck, Ref. 10)*		*Max.* measured *Strut load (P)*	*Computed loads (P_n)* "neutral" method *(Ref.7, p.489; d=0.31 H)*	
		Kips/ft.	$R_s = P_s/P$	*(Kips/ft.)*	*Kips/ft.*	$R_n = P_n/P$
1	+14.0	—	—	—	—	—
2	0.0	59.8	3.60	16.3	20.7	1.27
3	9.5	26.7	1.32	20.25	21.4	1.06
4	16.0	38.8	1.80	21.6	28.0	1.30
5	25.0	45.0	1.19	37.9	41.3	1.09
6	36.0	55.0	1.23	44.7	42.5	0.95
Total		225.3	1.53	140.75	153.9	1.09

The agreement between the "neutral ratio" design method and field measurements appears to be best in cuts involving soft clay and in deep cuts through medium clay—that is in cases with a high value of the ratio of imposed stress to the strength of the clay, so that a plastic state is created.

The "neutral method" *is* consistent with a frequently observed triangular shaped distribution of plastic clay pressure, but the strength methods are not. Deviations from the basic triangular form should be attributed to: (1) effect of horizontal shearing stresses along underlying soil at the level of the excavation (*Tschebotarioff*, 1951, p. 489), and (2) continuity effects of the piling itself (*Skempton* and *Ward*, 1952).

This discussion proposes for consideration the use of the simplified fixed earth support "equivalent beam" method (Fig. 1 b) as a basis for design (*Tschebotarioff*, 1951, pp. 505–510). This method was developed from the Princeton tests with different bulkhead flexibilities, covering 70% of what is designated on Fig. 1 of the General Report as the "normal range of flexibility for steel sheet pile walls". It gives a bending moment curve of the type marked (2) on Fig. 1 a.

This method has been found to give values closely corresponding to actual field conditions by Oberbaurat Dr.-Ing. *Wiegmann* who has just completed extensive full-scale measurements in the harbor of Bremen, both in sand and in stiff clay. The point of contraflexure was located at the dredge line; the maximum bending moment computed by the writer's method agreed within 4% with the one actually measured (*Wiegmann*, 1953).

The Princeton[1]) 1948 tests showed that greater bending moment reductions were possible under static conditions for more flexible bulkheads (*Tschebotarioff*, 1949, Fig. 42). However, such reductions were not included in the writer's design recommendations because of lack of field data concerning the stability of the reduction phenomena in the presence of wave action and of other vibratory effects. Stiffer sheet piling was not excluded from the writer's design recommendations because such piling *had* been designed according to the Danish Rules and had withstood the test of practical experience.

In stating that the writer's design recommendations are on the unsafe side for stiff sheet piling both the General Report and *Terzaghi* (1953, p. 31) refer only to *Rowe*'s original paper (1952), according to Fig. 8 of which the writer's method gave bending moments some 25% smaller than the Danish Rules. The writer has however shown (*Tschebotarioff*, 1952) that this is an error and that the reverse is true—the Danish Rules give bending moments some 25% smaller than the writer's method. *Rowe*'s original Fig. 8 (1952) was corrected accordingly (*Tschebotarioff*, 1952).

The writer's simplified "equivalent beam" method can be adapted—by varying coefficient f''' in: $K_A = 0.33 f'''$, as shown on Table 2—to reflect field imponderables (such as shocks) or to *increase* bending moments of curves type (2) to correspond to moments of curves type (3) and even type (1)—(Fig. 1 a)—if so desired for stiffer bulkheads of the remaining 30% of the "normal range of flexibility".

The procedure suggested is similar on a point of technique to *P. W. Rowe*'s, but the order of operations is reversed: *P. W. Rowe* used curves of type (1) as a starting point and then *reduced* the maximum bending moments to correspond to curves (3) and (2)—(Fig. 1 a).

Table 2 is conservative and is based on present field data (*Tschebotarioff*, 1951, pp. 510–514). It can easily be amplified to reflect results of future field measurements.

P. W. Rowe's outstanding work is welcomed both as a con-

[1]) Not "Princetown" tests as stated in the General Report.

Table 2 Recommended Values for Coefficient f''' in: $K_A = 0.33 f'''$ (see Fig. 1 b)
Valeurs proposées du coefficient f''' dans la formule: $K_A = 0.33 f'''$ (voir Fig. 1 b)

	$\varrho = \frac{H_T^4}{EI}$ *(per foot of wall)*	*Sheltered Location*		*Strong Ocean Wave Action*	
		Clean sands	*Silty sands*	*Clean sands*	*Silty sands*
(Flexible)	$\geqq$ 20 in²/lbs.	0.9	1.4	1.2	2.0
(Stiff)	> 20 in²/lbs. < 6 in²/lbs.	1.2	1.8	1.6	2.6
(Very Stiff)	< 6 in²/lbs.	1.4	2.2	1.9	3.2

firmation of the writer's explanation of the causes of bending moment decrease with increased flexibility and of other findings (*Tschebotarioff*, 1951, pp. 298–301, Fig. 10–41; *Tschebotarioff*, 1949, Fig. 49), and as a development of the writer's own investigations. The writer accepts the use of the coefficient "ϱ" which *Rowe* introduced to define bulkhead flexibility. Consistent units should however be used (Table 2).

One should recall *Terzaghi*'s statement at the 1948 Rotterdam Conference that it would be premature to accept "any" of the writer's conclusions "... unless and until they are confirmed by the results of pressure measurements on full-sized sheet pile bulkheads...". *Terzaghi* now (1953) accepts *Rowe*'s findings, although *Rowe*'s tests were performed at a smaller model scale than those at Princeton, confirmed the writer's 1948 conclusions, and used bending strain measuring techniques identical to those used in 1944–48 at Princeton—(*not* pressure measurements)—to establish the bending moment reduction curves. This recognition of model testing and of novel techniques is gratifying. However, the suggestion to use *P. W. Rowe*'s bending moment reduction curves as the direct basis for field design carries the reversal of 1948 too far towards the opposite unjustifiable extreme. These curves reflect purely laboratory static conditions and are not adapted to reflect field conditions. Attention is therefore drawn to the method proposed by this discussion. Space limitations preclude further comments, which will be given elsewhere (*Tschebotarioff*, 1954). Space limitations preclude further comments, which will be given elsewhere (*Tschebotarioff*, 1954). It will be shown that *Rowe*'s own data (1952) proves that the writer's original (1949 and 1951) design proposal Fig. 1 b cannot be "on the unsafe side"—under static loading— even for very stiff walls built of customary steel sheeting profiles, of timber, or of good quality reinforced concrete.

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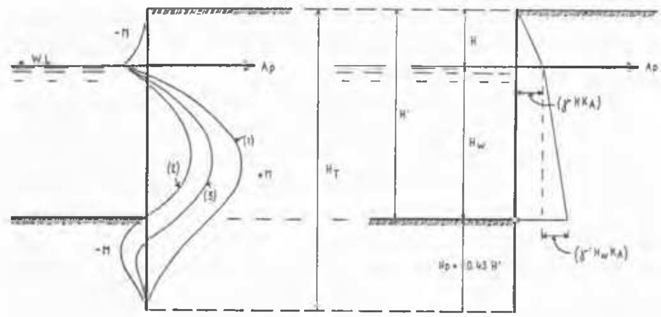
Dans la première partie de sa discussion l'auteur compare les avantages offerts par sa méthode du coefficient de pression neutre et la méthode de résistance pour le calcul de la pression latérale des argiles contre des rideaux de palplanches entretoisées. La méthode de résistance calcule la pression latérale dans les argiles sur la base de la résistance au cisaillement déterminée par des essais rapides, tandis que la méthode du coefficient de pression neutre se



a b

Fig. 1 (*a*) Influence of Degree of Sheet Pile Fixation below the Dredge Line on the Shape of the Bending Moment Curves
Influence du degré d'encastrement au pied des palplanches sur la courbe des moments fléchissants

(*b*) *Tschebotarioff*'s Simplified "Fixed Earth Support" Method for Full Fixation in Sand—Curve (2)
La méthode simplifiée «fixe au pied» proposée par *Tschebotarioff* pour encastrement maximum dans le sable – courbe (2)

rapporte à un état d'équilibre plastique après consolidation. Des essais en cette matière sont actuellement en cours à l'Université de Princeton.

Dans la deuxième partie l'auteur décrit sa méthode simplifiée de fixation au pied, qu'il appelle méthode de poutre équivalente, pour le calcul des rideaux de palplanches ancrés. L'auteur établit une comparaison entre les mesures effectuées dans les chantiers et les résultats obtenus en appliquant d'autres méthodes (*Rowe*, etc.) et, finalement répond aux critiques qui ont été avancées.

Mr. K. Mortensen

In his General Report Dr. *Skempton* strongly advocates the design method proposed by *Rowe* for flexible anchored sheet walls. Let me say at once that I consider *Rowe*'s tests the most brilliant of their kind, and I wish to congratulate Dr. *Rowe* on his excellent work. I think, it deserves very great attention. However, his proposed design method seems not quite satisfactory, neither from a theoretical point of view, nor in the light of general experience.

As the reporter may or may not know, the so-called Danish Rules, which have been used for more than 25 years, take into account, in an empirical way, the same effects of the flexibility which *Rowe* has now studied in the laboratory.

Of course, the Danish Rules can be criticized as has been done by *Tschebotarioff*, but the fact remains that hundreds of kilometers of quay walls have been built all over the world according to this design method. And so far no failure has been recorded apart from a few cases of anchor yield.

Now the fact is that *Rowe*'s method in practically all cases will lead to more expensive structures than the Danish Rules, mainly because it requires a considerably greater driving depth. As no failure due to insufficient driving depth has ever been recorded in the numerous structures built according to the Danish Rules, this must mean that *Rowe*'s method with the safety factors so far proposed is unnecessarily conservative.

Moreover, *Rowe*'s methods, as all empirical methods, has the serious limitation of being applicable only to structures and conditions corresponding to those investigated in the tests. For example, *Rowe*'s method cannot without additional unproven assumptions, be applied to such cases as a sheet wall under a relieving platform or a sheet wall which is fixed in a concrete superstructure.

Finally *Rowe* operates with allowable stresses, which means that he applies his safety factor to the strength of the wall material, whereas in my opinion at least the main part of the safety factor ought to be applied to the soil constants, just as it is done in stability analysis.

The three mentioned drawbacks of *Rowe*'s method, which I can summarize as: overdesign, limitation to test conditions and unsatisfactory application of the safety factor, can all be avoided by using the new design method of Dr. *Brinch Hansen* as described shortly in Proceedings 1953 (vol. II, p. 170) and more fully in his book on Earth Pressure Calculation. His method is based on an extended theory of plasticity applied to the state of failure. Without the use of any empirical constants it leads in cases, where *Rowe*'s method can be used, to approximately the same wall sections and anchor sections as found by *Rowe*, but to the smaller driving depths known from practice to be sufficient.

I wish to conclude my discussion with the following general remarks. For a given structure subjected to earth pressures it would seem appropriate to investigate the safety against ultimate failure as well as the deformations under actual working conditions. Sometimes the deformations will be decisive as is often the case for foundations, although strangely enough the plasticity theory seems to be almost generally accepted here. In bulkheads far greater deformations and movements can be tolerated which means that here the safety against ultimate failure will usually be decisive, so that only the plasticity calculation needs to be carried out.

Therefore it is surprising to see the emphasis now being placed on empirical design methods for sheet walls. It may be due to the fact that until quite recently no satisfactory theory of plasticity or rupture was available for anchored bulkheads. However, such a method has now for the first time been put forward by Dr. *Brinch Hansen.* In my opinion this theory is the most significant advance in theoretical soil mechanics in recent years. It is applicable to practically all two-dimensional earth pressure problems and also to some stability problems such as cellular cofferdams. In all cases where a comparison has been possible this method has led to results in good agreement with practical experience. I am therefore surprised to see that the General Reporter in his written report does not even mention this considerable advance in theoretical design procedures.

L'auteur critique la méthode du Dr *Rowe* pour le calcul des rideaux de palplanches et la compare aux expériences obtenues par la Méthode Danoise. Selon l'auteur, la méthode du Dr *Rowe* est trop conservative, est limitée aux conditions des essais et les facteurs de sécurité ne sont pas judicieusement choisis. L'auteur est d'avis que la méthode préconisée par le Dr *Brinch Hansen* donne des résultats serrant de plus près les résultats obtenus dans des expériences pratiques.

The General Reporter

As to the concluding remarks of the last speaker may I have time, for one minute, to explain, that the absence of any comment in my general report on Dr. *Brinch Hansen*'s book is due to the very simple fact that his book was published five months after my report was in the hands of Dr. *von Moos.* At the time I wrote my report, I had only the very brief paper of Dr. *Brinch Hansen* available. I am sure that now his book is published we shall read it with great interest. I would like to assure Mr. *Mortensen* and my other Danish friends that there is no suggestion in my report of any national preference for Dr. *Rowe* of England to Dr. *Brinch Hansen* of Denmark.

Le rapporteur général rappelle que l'ouvrage du Dr *Brinch Hansen* n'est pas mentionné dans son rapport général pour la bonne raison qu'il a été publié cinq mois après la rédaction du rapport général pour la Session 7. De plus le rapporteur tient à assurer M. *Mortensen* et ses autres amis Danois qu'il accorde une attention impartiale à leurs travaux.

Dr. J. Brinch Hansen

It is a little embarrassing for me to speak just after Mr. *Mortensen*'s enthusiastic remarks about my work; I can assure you that I tried to discourage him, but apparently without success.

In his reference to my paper (Proceedings 1953, vol. II, p. 170) Prof. *Skempton* questions the justifiability of using *Kötter*'s equation for a circular sliding surface. I shall not waste time here proving this point, but if Prof. *Skempton* is interested, he can find the proof in my book: Earth Pressure Calculation.

It is true that Prof. *Skempton* did not see my book until quite recently, but in my paper to the conference (Proceedings 1953, vol. II, p. 170) I have given the calculation of an anchored sheet wall and of a cellular cofferdam as examples of the practical application of my method.

With regard to the linear distribution of the active pressures mentioned by Prof. *Skempton*, this is a natural consequence, if an excessive driving depth and yielding anchors are used. However, not all anchorages yield, and even when they do, the use of a smaller driving depth will actually alter the pressure distribution and lead to a cheaper design.

However, to my mind the essential question in earth pressure design is, whether the structure should be designed so that certain permissible stresses are not exceeded under actual working conditions, or whether it should be designed so as to possess a certain safety against ultimate failure.

In the first case it would probably be necessary to use a good empirical or semi-empirical method such as *Tschebotarioff*'s or *Rowe*'s, but in the second case a theory of plasticity such as my own must be employed.

The only serious shortcoming of a plasticity theory is that it cannot determine the deformations and movements occurring under actual working conditions. However, to limit these deformations is largely a matter of specifying suitable safety factors, and moreover, earth-retaining structures can actually undergo rather large movements without detrimental effects. Therefore I consider the plasticity method the most suitable one for the design of most earth retaining structures.

Of course, not every plasticity theory can be used, but only such theories which, as my own, take due regard to the equations of equilibrium and to the movements of the structure in the state of failure.

I take this opportunity to recommend a closer collaboration between the scientists engaged in soil mechanics and those studying plasticity theories. I have seen plasticity scientists make mathematical assumptions which any soil mechanics engineer would at once reject as being contrary to simple experimental facts. On the other hand, soil mechanics engineers have sometimes made mistakes which could immediately have been discovered by a plasticity scientist. An example of the latter kind was pointed out by Prof. *Lundgreen* in the discussion of the report on his paper (Proceedings 1953, vol. I, p. 409).

May I conclude my discussion with a few general remarks. Successful development of a natural science like soil mechanics is only possible through a suitable combination of theory, tests and field evidence. Therefore, I have been considerably surprised and worried to see a number of leading soil mechanics people becoming increasingly disinterested in theory and advocating exclusively the collecting of test results, field evidence and case histories.

Admittedly, a certain amount of all this is necessary before any useful theory can be developed, but, after more than 25 years, sufficient material must surely be available now for forming a number of suitable working hypotheses and tentative theories. Without these means of correlating the existing evidence and directing further research, we will soon be lost in a jungle of unrelated facts. At the best, soil mechanics will then degenerate into an empirical science, which can be mastered only by a few super-engineers.

As I see it, there is an urgent need just now for a number of new and appropriate theories. They must be tested thoroughly, of course, and be rejected if they cannot be brought into sufficiently good agreement with experimental and practical evidence. However, if no such theoretical attempts are made, further progress in soil mechanics as a practical science will be very slow.

L'auteur répond tout d'abord à différentes questions posées par le rapporteur général. Ensuite il établit une comparaison entre les méthodes empiriques et demi-empiriques pour le calcul de la poussée des terres et les méthodes qui, comme la sienne, se basent sur une théorie de plasticité. Il exprime son regret de constater qu'un grand nombre d'ingénieurs ne manifestent pas d'intérêt pour les recherches théoriques.

Mr. R. Briske

Prototype measurements (e.g. those of Prof. *Duke* in Los Angeles and Dr. *Wiegmann* in Bremen) and various model tests (e.g. those of Mr. *Stroyer*, Prof. *Streck*, Prof. *Ohde*, Prof. *Tschebotarioff* and Dr. *Rowe*) have shown in my opinion that active pressure redistribution is a stable state of stress like flexure, but not a "variable phenomenon". The essential reasons are:

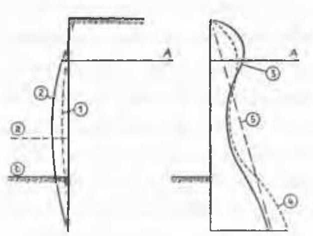


Fig. 2 Deflexion and Pressure Re-Distribution (Dredged Wall)
Déflexion et redistribution des pressions (rideau de palplanches dragué)

(*a*) Stage before last dredging
Stade avant le dernier dragage

(*b*) Stage after last dredging
Stade après le dernier dragage

(1) Deflexion, stage (*a*)
Déflexion, stade (*a*)

(2) Deflexion, stage (*b*)
Déflexion, stade (*b*)

(3) Pressure re-distribution due to (2)
Redistribution des pressions due à (2)

(4) Pressure distribution due to questionable "vertical arching"
Distribution des pressions due à un «effet de voûte vertical» douteux

(5) Triangular pressure distribution
Distribution triangulaire des pressions

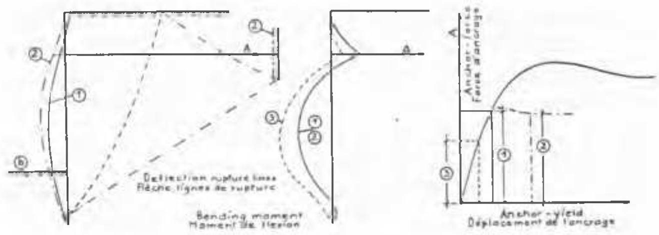


Fig. 3 Equilibrium between Anchor-Force and Bending Moment
Equilibre entre force d'ancrage et moment de flexion

(1) Deflexion, moment, anchor-force and anchor-yield, stage (*b*)
Déflexion, moment, force d'ancrage et déplacement d'ancrage, stade (*b*)

(2) As before, but after later anchor-yield due to creeping, etc.
Comme avant, mais après déplacement d'ancrage dû au fluage, etc.

(3) Bending moment and anchor-force for triangular pressure distribution
Moment de flexion et force d'ancrage pour distribution triangulaire des pressions

Pressure redistribution due to sheet pile deflexion occurs if the deflexion of the wall compared to the top yield (usually this is not the anchor-point) is the last predominant deformation during the construction of the bulkhead (Fig. 2). This is also the reason why it occurs more often in dredged walls than in backfilled ones. Pressure redistribution effects an increase of the anchor-force and a decrease of the bending moment and toe reactions, the resultant of the active pressure curve being higher than the resultant at triangular type pressure distribution. This kind of pressure distribution is in contradiction with the so-called "vertical arching" (i.e. pressure concentration at the top and the toe of the wall) and I doubt that this phenomenon is of any importance in soils.

By every subsequent alteration of the deformation, the previous state of pressure distribution will be annulled. I entirely agree with Dr. *Rowe* on this point, as well as on his tests on flexure, but not on his conclusion that any later anchor-yield during the lifetime of the structure will cause the breakdown of the previous pressure redistribution. Viz., a later yield of the anchorage will effect an increase, or, at least, no decrease of the anchor-force (Fig. 3). In this way pressure redistribution will be stabilized for anchor-force and bending moments depend reciprocally, i.e. there will exist a stable equilibrium between the value of the increased anchor-force and the decreased bending moment. Thus, even a later anchor yield will not effect any diminution of the pressure redistribution if the anchor-force does not decrease.

Since pressure redistribution effects an increased anchor-force, an increased anchor-force effects pressure redistribution, for action equals reaction. Therefore pressure redistributions may arise through the prestressing of the anchorage system up to the value of the anchor-force to which the required pressure redistribution corresponds (see Figs. 2 and 3). At Pier C in Los Angeles Prof. *Duke* found a similar kind of pressure redistribution which was effected by an increased anchor-force caused by earth load upon the tie-rods due to the settlement of deeper earth layers (Fig. 4). But I do not agree with Prof. *Duke* or Prof. *Tschebotarioff*, who point out that no "arching" was found, for this pressure redistribution due to an increased anchor-force does not contradict in principle an "arching" caused by a final predominant bulkhead deflexion.

Pressure redistribution due to deflexion must occur, if the yield point in the wall is reached first (plastic deformation), i.e. if the factors of safety for anchorage and toe are higher

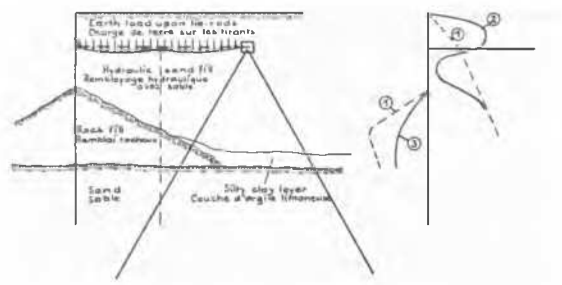


Fig. 4 Pressure Re-Distribution due to Increased Anchor-Force (Prototype Measurements of Prof. *Duke*)
Redistribution des pressions due à la force d'ancrage augmentée (mesures faites sur prototype par *Duke*)

(1) Active and passive pressure before pressure re-distribution
Pressions actives et passives avant la redistribution des pressions

(2) Active pressure re-distribution due to increased anchor-force
Redistribution active des pressions due à la force d'ancrage augmentée

(3) Decreased toe reactions (passive pressure) due to active pressure re-distribution
Réaction diminuée du barrage (pression de butée) due à la redistribution des pressions actives

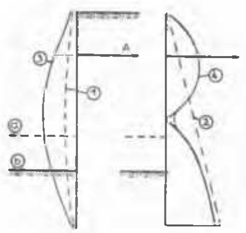


Fig. 5 Pressure Re-Distribution due to Plastic Deflexion of the Wall
Redistribution des pressions due à la déflexion plastique de la paroi

(1) Deflexion stage (*a*); stress near yield point
Déflexion stade (*a*); tension de la limite apparente d'élasticité

(2) Pressure distribution stage (*a*), e.g. triangular
Distribution des pressions stade (*a*), p.e. triangulaire

(3) Deflexion stage (*b*); plastic yield of the wall
Déflexion stade (*b*); déformation plastique de la paroi

(4) Approximate pressure, re-distribution stage (*b*)
Redistribution approximative des pressions, stade (*b*)

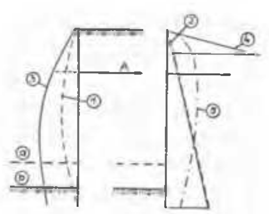


Fig. 6 Pressure Re-Distribution due to Plastic Yield of the Toe
Redistribution des pressions due au déplacement plastique du pied

(1) Deflexion stage (*a*); no passive failure in front of the wall
Déflexion stade (*a*); pas de rupture passive devant le pied de la paroi

(2) Pressure distribution stage (*a*), e.g. triangular
Distribution des pressions stade (*a*), p.e. triangulaire

(3) Deflexion stage (*b*); plastic yield of the toe
Déflexion stade (*b*); déplacement plastique du pied

(4) Pressure distribution after *Brinch Hansen* due to (3)
Distribution des pressions d'après *Brinch Hansen* due à (3)

(5) More probable pressure distribution due to (3)
Distribution plus probable des pressions due à (3)

than those for the sheet pile wall (Fig. 5). In a paper presented to the conference, Dr. *Brinch Hansen* proposes another type of pressure redistribution for the case of the toe yielding first plastically, i.e. if a passive state of failure occurs at the dredged side of the wall (Fig. 6). Both Figs. 5 and 6 show a stable state of stresses. However, if the toe yields, anchor-force and bending moment will increase, since the actual pressure curve will equal that usual in open cuts. In his plasticity theory Dr. *Brinch Hansen* has not taken into account the elasticity laws applicable between the upper passive pressure and the wall deformation. Thus a bulkhead constructed for the case of passive failure usually will be less economical than one constructed for pressure redistribution due to deflexion to which the Danish rules refer. Nevertheless I should like to point out that I agree in general with Dr. *Brinch Hansen* whose new theory as well as that of Prof. *Ohde* confirms the experience of the Danes.

Flexure as well as pressure redistribution or both simultanuously may occur; the choice of bulkhead to be constructed is an economic question. I would like to see Dr. *Rowe* or Prof. *Tschebotarioff* his valuable investigations to cases where pressure redistribution may occur, and I am sure that the theories of Prof. *Ohde* and Dr. *Brinch Hansen* as well as my own considerations will be confirmed.

L'auteur montre que la redistribution des pressions actives ainsi que la flexion dans les rideaux de palplanches ancrés ne sont pas un phénomène variable mais un état stable de contrainte du sol.

Dr. P. W. Rowe

Sheet pile walls may be designed to stand at a chosen safe working stress ("elastic method") or to stand at a given factor of safety against ultimate failure ("plastic method"). In the first instance this is a matter of choice rather than of scientific fact, as in any structure.

The large scale experiments by Prof. *Tschebotarioff* are in agreement with the proposed working stress design using moment/flexibility curves, and no evidence of disagreement of any kind arose at the Conference regarding the influence of pile flexibility in sands. Mr. *Briske*, however, considers that there are cases when further moment reduction due to active pressure redistribution should be allowed with yielding anchors. With a deep anchor point the bending of the wall causes the top of the pile to move a distance χ into the retained sand. Mr. *Briske* then considers, from theoretical calculations, that an outward movement of the anchor point by a distance χ is necessary to produce a triangular type pressure redistribution. This would be true if sand had a linear stress strain relationship and were "elastic". In fact very small strain reversals in a shear test reduce the shear stress to zero. Therefore designers must be guided more by the results of model tests.

Deep seated, prestressed anchorages may be regarded as requiring the largest outward movement before breakdown of the active pressure redistribution, and when the degree of moment reduction that can be allowed in the field for this case is established, it is easily incorporated in the flexibility design method by reducing the stiff wall moment.

Dr. *Brinch Hansen* has written a book on Earth Pressures in which he calculates the position of the centre of pressure of active and passive pressures due to various types of wall movement. This is likely to prove a valuable contribution to this subject.

The theory does not allow the calculation of pressures for various *degrees* of wall movement so that it leads logically to the analysis of structures at failure only. In the application to sheet pile wall design, the wall toe is considered to be failing, and the steel in the wall to have passed first yield giving a plastic hinge. Under these conditions the active pressure redistribution is assumed neglecting the expansion of the sand behind the wall and the resulting cyclic slips. A small factor of safety in penetration is used which allows the wall to stand after several inches of outward movement at the toe. The influence of this movement and large wall deflexions on the settlement of the backfill is not considered.

Mr. *K. Mortensen* has criticised the flexibility design method on three points:

(1) It is semi-empirical.

(2) It does not indicate the safety against ultimate failure.

(3) It is uneconomical.

The replies to these criticisms are as follows:

(1) An "elastic" mathematical solution has been made which gives good agreement. It is hoped to publish this shortly.

(2) By using "structural" curves to the yield stress of the wall, the ultimate plastic type of design is achieved. In addition the use of the yield stress rather than some unknown stress between yield and ultimate, seems preferable.

(3) This arises mainly because the factor of safety in penetration proposed by the writer is larger than that used by Dr. *Brinch Hansen.* This is a matter of individual decision, not of science, and cannot be a criticism of the flexibility *method.* The method may still be used with a lower factor of safety in penetration. Prof. *Terzaghi* proposes an even higher safety factor than the writer.

Finally there are the following considerations to be made in the choice of method.

(1) The "ultimate" method does not give any guidance as to the choice of type of wall, i.e. concrete, steel, or make of steel section.

(2) The "ultimate" method gives the same design for a wall penetrating a loose sand of $\varphi = 30°$ as for a loose silt also with $\varphi = 30°$. Since the compressibility of the silt is very much greater the working moment would in fact be larger.

(3) The "ultimate" method does not indicate the distribution of bending moment over the wall. This will be important in the development of prestressed concrete walls.

L'auteur répond aux critiques de MM. *Mortensen, Brinch Hansen* et *Briske* sur les avantages respectifs de la «méthode élastique» et de la «méthode plastique».

Mr. S. Packshaw

I should like to make a few brief remarks about my own experience in the practical application of Dr. *Rowe*'s methods. During the last few months I used Dr. *Rowe*'s methods on a number of occasions whenever the soil approximated to sand or other non-cohesive material. At the same time the design was also carried out by the conventional fixed earth support method and a record of the results by the two methods was kept for purpose of comparison, because although the fixed earth support method is not on a strictly correct theoretical basis, it has nevertheless been used with apparently satisfactory results for very many years, and to that extent it can be regarded as a check on Dr. *Rowe*'s method. As a rule the difference between the results obtained by the two methods has not been very great. However, the practical application of Dr. *Rowe*'s method has confirmed that one of its great advantages is that it establishes a quantitative relation between the flexural properties of the wall and the bending moment what it will have to withstand. The method shows that at the stresses in two different sections of piling are not simply in inverse proportion to their section moduli. Thus it often permits the designer to choose a lighter and therefore more flexible section than he would have otherwise dared to use. It also demonstrates the advantages of using steel of somewhat higher strength than the standard grade, because it brings a lighter and more flexible section within the range of permissible stress. A designer who is concerned with only one material, for instance steel, can simplify the use of Dr. *Rowe*'s method by preparing a set of graphs applicable to that particular material and to the range of sections which are commercially at his disposal. At present the use of Dr. *Rowe*'s method is limited to non-cohesive soils, but I hear that he is continuing his research with the object of obtaining data for other types of soil. I think that when this information is available it is quite likely that his methods will come into general use unless, perhaps, all methods derived from the analysis of working stresses are eventually discarded in favour of methods based on the theory of plasticity.

L'auteur compare l'application de la méthode du Dr *Rowe* et des méthodes conventionnelles dans des cas pratiques et dans des sols pulvérulents.

Les différences qu'il a relevées sont légères. Cependant la méthode du Dr *Rowe* fournit des indications quantitatives qui permettent de construire plus économiquement.

Mr. P. S. Cockle

My remarks refer to the paper by Messrs. *Cooling* and *Ward* (Proceedings 1953, vol. II, p. 162) with especial reference to the "Earth Pressures on Tunnels in Clay".

It is noted that all the experiments described are for tunnels driven in London Clay between 90 and 109 feet deep from ground level to axis, and that *Skempton* (Jour. Inst. C. E., Vol. 20, No. 5, 1943) states that in the last case the compressive strength of the clay was 80 lbs./sq.in. and (in the General Discussion) that London Clay at depths of about 100 feet has a shear strength of the order of 50 lbs./sq.in.

These are equivalent to the vertical pressures exerted by from 90 to 115 feet of overburden and indicate that at depths equal to or greater than these the clay, having been loaded to more than its unconfined compression strength would flow into any cavity or, we may suppose, gradually adjust itself to give a uniform fluid pressure (with correction for depth-variation) around any tunnel lining of reasonably regular profile.

In appreciably shallower tunnels on the other hand, some residual arching would remain and the normal pressure on the extrados would be reduced. The late Mr. *G. L. Groves* (in the Inst. C. E. Journal referred to) hinted at the existence of a critical depth, separating these two conditions.

It is thus quite logical, in view of local variations in the strength of the clay, that *Cooling* and *Ward* should obtain average pressures ranging from 45% to 100% and *Skempton* 100% of the full overburden pressure.

It is a pity that all these measurements should have been taken just at this depth; the results are inconclusive and it is suggested that much would be learned if opportunity should arise to repeat the experiments at depths of say, 60 and 150 feet, to establish whether such a critical depth or zone, in relation to the strength of the clay, actually exists.

As regards the considerable variation in the observed pressures around the circumference of the 25-ft. tunnels, whilst, as Dr. *Skempton* suggests, this may be due to the proximity

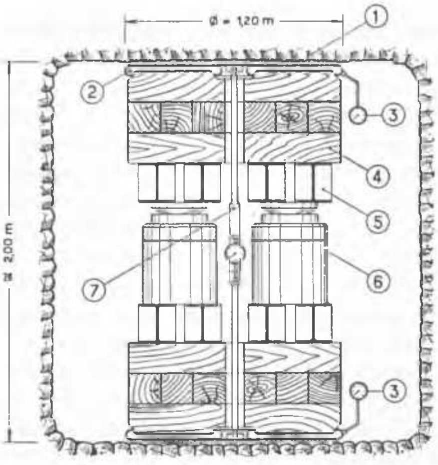


Fig. 7 Arrangement of Loading Tests in Rock
Dispositif pour essais au vérin en galeries

(1) Mortar
Mortier
(2) Metallic cushions filled with oil
Coussins métalliques remplis d'huile
(3) Pressure gauges
Manomètre
(4) Timber packing
Pilotis
(5) Broad-flange steel distribution joists
Poutrelles Grey
(6) Hydraulic jack
Vérin
(7) Micrometer stick for measuring height changes under load
Dispositif pour la mesure des variations de diamètre

of the adjoining tunnels, there may be another cause at work. It is known that some of the tunnels at this site were driven partly in the London Clay and partly in the Shepherd's Plaid Clay of the Woolwich and Reading beds, which immediately underly the London Clay; this clay has the reputation of great hardness combined with treacherousness due to slipping on cleavage planes, and the angle from which the load may come is quite unpredictable.

The "stress corresponding to overburden pressure" appears to require closer definition, since the hydrostatic pressure to which it relates must vary by more than 25% at different points around the circumference from crown to invert. It is, nevertheless, shown in Fig. 7 (Proceedings 1953, vol. II, p. 165) as a constant, viz. the radius of a circle representing $2\frac{1}{2}$ tons/sq.in.

One comment may be made from the design angle as distinct from the academic view-point. *Skempton* in the above paper, noted that the maximum recorded stress in the iron was 70% greater than the average owing to minor bending effects. Since it is the practice to form a two-ring opening in iron tunnels in London without lintol or jamb-plate, and this may be done many years after the original construction, this figure of 70% should be increased for design purposes to at least 100%.

It may be convenient to list the main cases which arise as below.

1. Shallow Tunnels

Appreciable bending stresses will occur:

(*a*) where the cover is small and there is a tendency to heave the crown;
(*b*) where a concentrated load at or near the crown can cause the side-walls to spread if the passive resistance of the ground is insufficient to take the thrust.

A recent example was a proposal to build a block of flats with basement over twin 21 feet diameter station tunnels which had originally 12 feet of cover. The limit of permissible excavation and the subsequent safe loading were to be specified.

2. Tunnels of Medium Depth

Where, although the active pressures around the circumference may not be uniform, the passive resistance of the ground is sufficient to prevent deformation of the lining.

3. Deep Tunnels

Where the ground is stressed to a state of plasticity and acts like a fluid, the pressure being equal to the overburden.

It may be noted that the effect of opening a fresh cavity in the clay in the vicinity of an existing tunnel may be to induce very severe bending stresses in the lining.

L'auteur constate que les essais de MM. *Cooling* et *Ward* ont été fait à des profondeurs critiques pour lesquelles la pression de surcharge est du même ordre que celle de la résistance à la compression sans contrainte latérale. Il serait recommendable de faire des essais dans des tunnels où la pression de surcharge est nettement supérieure ou inférieure à la résistance du matériau. Il se pourrait que dans des tunnels construits à des profondeurs moindres une décompression résulte de l'effet d'arc. Les variations frappantes de la pression dans la zone périphérique du tunnel pourraient être attribuées d'après l'exemple de MM. *Cooling* et *Ward*, à la présence de couches géologiques plus solides. L'auteur estime également qu'il serait bon de définir les contraintes correspondant à la pression de surcharge de manière plus précise que ne l'ont fait MM. *Cooling* et *Ward* à la Fig. 7 de leur mémoire (Comptes Rendus 1953, vol. II, p. 162). Finalement l'auteur dresse la liste des différents cas où un moment de flexion se développe sur un tunnel.

Dr. L. F. Cooling

I want to make a few brief remarks on the tunnel investigations which are being carried out by the Building Research Station of Great Britain. This work was begun early in 1952 and is still in progress, but since the observations given in our paper (Proceedings 1953, vol. II, p. 162) only cover the period up to July 1952, I thought it might be of interest to describe in general terms some of the results obtained since that date.

I shall only have time to refer to the 9 feet diameter water tunnel. The axis of this tunnel is 90 feet below ground level in London clay and it is lined with special wedge-shaped concrete segments which are jacked into place. The tunnel was completed in May 1952 and remained empty until March 1953 when it was filled with water up to ground level. It was emptied again in May 1953 and is now in the process of being refilled, when the head of water will be increased to that of the adjoining reservoir. Throughout this period tests have been made by my colleague Mr. *W. H. Ward*, in collaboration with the engineers of the Metropolitan Water Board, to measure (*a*) the earth pressure on the tunnel, (*b*) compression thrust in the ring, (*c*) distortions of the tunnel.

Fig. 10 in the paper shows typical results of thrust measurements with the tunnel empty. During this time diameters were measured directly with the micrometer stick. At first the distortion increased with time and then settled down to a steady value. After seven months, the vertical diameter had decreased by about 0.05 in. and the horizontal diameter had increased by about 0.02 in.

To enable measurements of the diameter to be taken when the tunnel was filled with water, a special strain gauge using the vibrating-wire principle, was designed. The pressures and thrusts were measured with vibrating-wire load gauges and all the instruments could be read remotely from the top of the shaft. The results of these measurements showed that the radial pressures decreased in proportion to the increased water pressure, but that the change in earth pressure was less than that of the water pressure.

Diameters increased proportionately with increasing water pressure and the horizontal diameters tended to increase more than the vertical diameters. The greatest increase in diameter was of the order of 0.01 in. for a water head of 100 feet. The results obtained in this later work have given us additional confidence in the suitability of the vibrating-wire type of load and strain gauges. Even under the 90 feet head of water only two or three instruments out of the fifty installed were so badly affected as to be unserviceable.

Finally, I agree with Dr. *Skempton* that it is a little premature to draw too many deductions from the results, but I would claim that a satisfactory first stage has been reached, that of having a reliable method of measurement. Every opportunity will be taken to obtain measurements at other sites, and with the accumulation of data it may be possible to approach the theoretical problem with more confidence.

L'auteur donne quelques résultats complémentaires sur la galerie d'amenée faisant l'objet du mémoire présenté en collaboration avec M. *Ward* (Comptes Rendus 1953, vol. II, p. 162). Le diamètre horizontal mesuré dans le tunnel vide a augmenté, tandis que le diamètre vertical a diminué. Des mesures en tunnel ont été effectuées pendant et après la mise en service à l'aide de comparateurs à fils vibrants; on a constaté que les diamètres augmentaient plus dans l'horizontale que dans la verticale.

Prof. K. Terzaghi

One of the most important topics covered by this session is the design of anchored bulkheads. In 1935 Mr. *Paul Baumann* of Los Angeles published a paper on the failure of a bulkhead in Southern California in which he pointed out that the then current procedure for the design of anchored bulkheads with fixed end support involves a serious fallacy. This procedure involved the assumption that the conditions of end support depend exclusively on the depth of sheetpile penetration whereas in reality they are also a function of the flexural rigidity of the sheetpiles and of the compressibility of the material which provides the support. The recent experimental investigations by Dr. *P. W. Rowe* confirmed Mr. *Baumann*'s findings.

As a consequence of this situation and at the present state of our knowledge, the conditions of end support can only be ascertained by means of model tests similar to those performed by Dr. *Rowe* or on the basis of the theory of horizontal subgrade reaction. The experimental procedure involves all the hazards associated with extrapolation from small scale tests with model piles embedded in artificially deposited, homogeneous materials. On the other hand, the theoretical approach has two serious shortcomings. First of all, the theories of subgrade reaction are based on highly artificial concepts, and second, the equations contain the coefficient of subgrade reaction which is one of the most controversial items in the inventory of theoretical soil mechanics.

In 1921, Mr. *K. Hayashi* published a book in which he derived the equations for the bending moments in beams resting on an elastic subgrade. In this book he considered an extraordinary variety of loading conditions. However, in connection with the coefficient of subgrade reaction, which appears in all his equations, he stated very briefly that the value of this coefficient should be determined by means of a loading test. He did not seem to realize that a loading test performed on a given soil may furnish for this coefficient almost any value depending on the size of the loaded area. Eleven years later I published a paper in which I discussed the various factors which must be considered in the approximate evaluation of the coefficient of subgrade reaction and the inevitable uncertainties involved. Yet this paper received no attention whatsoever and still today there are many engineers who believe that the coefficient of subgrade reaction is a constant for any given soil, comparable to the compression index or the void ratio.

In the realm of the coefficient of horizontal subgrade reaction, conditions are still worse. Within the last ten years numerical values for this coefficient have been published which were derived from the results of horizontal loading tests on individual piles. Yet these values have been used as a basis for computing the distribution of the passive earth pressure on the buried portion of continuous rows of sheetpiles. The authors of the papers did not seem even to suspect that the value of the coefficient of the subgrade reaction at any given point on the face of a buried wall depends not only on the type of soil and the depth at which the point is located, but also to a large extent on the height and width of the wall. On account of the prevailing confusion the theoretical procedures for the investigation of the conditions of end support of anchored bulkheads cannot be relied upon, unless and until the existing misconceptions concerning the evaluation of the coefficient of horizontal subgrade reaction are radically eliminated.

Another subject of outstanding importance which was included in the programme of this session consists of the mechanics and practical implications of creep. In connexion with creep, distinction should be made between skin and mass creep.

The term skin creep refers to the soil movements which occur within the zone of seasonal variations of temperature and moisture. The depth of this zone rarely exceeds ten feet, and it can be considerably smaller. The creep, is due to alternate expansion and contraction. In his classical book "Morphologische Analyse" published in 1924, the late *Walther Penck* has convincingly demonstrated that on any slope with a dip of more than about 5°, the toplayer of soil moves slowly, like a continental glacier, towards the foot of the slope where the material accumulates unless it is removed by erosion. Skin creep is almost exclusively responsible for the gradual transformation of mountain chains with sharp-crested ridges characteristic of regions of uplift into peneplains with a gently undulating surface.

In open cuts through deeply weathered rock, the boundary between creep layer and the underlying stationary material is commonly well defined. In the mountains west of Rio de Janeiro, composed of deeply weathered gneiss, I have noticed that the creep layer has a color slightly different from that of the underlying material and its permeability is considerably lower. I have taken advantage of the low permeability of the creep layer in the design of storage dams by eliminating the grouted cutoff because the presence of the creep layer alone could be counted upon to reduce the losses due to seepage to a tolerable value. The surface of contact between creep layer and the underlying weathered rock was locally paved with small, angular quartz fragments which must have moved in the course of time into their present location. Yet the rate of creep was imperceptible as it is on the majority of existing slopes all over the globe. However, there are exceptions to this rule.

In one instance I have established a target with a concrete foundation, the base of which was located within the creep layer on a relatively gentle slope in the temperate zone. There were no visible indications of creep movements. Yet within a year the target had moved over a distance of two inches. In other instances exceptionally rapid surface creep has produced a conspicuous displacement of retaining walls. The conditions responsible for exceptionally high rates of skin creep are not yet known. Therefore, caution is indicated and well-documented case records are urgently needed.

The second main category of creep movements, mass creep, is due to the action of the force of gravity on the materials underlying slopes. Mass creep is due to the fact that the shearing stresses may produce slowly increasing shear deformations even in the event that their intensity is considerably smaller than the shearing resistance of the materials involved. In connexion with mass creep, distinction must be made between creep in scree material and in rock.

The creep phenomena described by *Haefeli* at this conference occurred in scree material. Creep phenomena in rock have already been described by *M. Lugeon* in 1922 in a paper "Sur le balancement des couches" and, at a later date, in his book "Barrages et Géologie". However, the universal occurrence of mass creep on rock slopes was not recognized until the Austrian geologist, *O. Ampferer* and, after him, *J. Stiny* published the results of their studies and observations in the eastern Alps. *Ampferer* arrived at the conclusion that many of the known rock deformations and displacements in the eastern Alps, which had previously been ascribed to tectonic movements, have in reality been produced by mass creep and that our conceptions of the tectonics of the eastern Alps require a radical revision on the basis of the creep concept.

The most impressive and, from an engineering point of view, the most important manifestation of mass creep consists in the descent of vast slices or bodies of rock from the slopes of deep valleys along more or less well-defined shear zones. Morphologically the results of such movements have the characteristics of vast landslides, but it is very doubtful whether the movements ever took place at a perceptible rate because the stress conditions for failure were very slowly approached. If a tunnel crosses the boundary between the stationary and the displaced material, or if a dam site is located at the foot of a displaced body of rock, serious construction difficulties are likely to be encountered.

On account of the extraordinary variety of creep phenomena and of their practical implications, creep research is a very promising field for the cooperation between geologist and engineer. So far this field has hardly been touched.

Le Prof. *Terzaghi* traite tout d'abord la question du calcul des cloisons étanches ancrées. Ce calcul ne doit pas prendre en considération uniquement la longueur de la cloison étanche, mais il doit englober les propriétés du matériau de construction et du sol. Ces calculs peuvent être effectués sur la base d'essais sur modèles réduits, cependant l'extrapolation soulèvera des difficultés. On peut également avoir recours à la théorie de la réaction horizontale du sous-sol, mais dans ce cas, il convient tout d'abord de déterminer expérimentalement la réaction horizontale du sous-sol. Cette valeur ne dépend pas seulement du sous-sol et de la profondeur, mais aussi des dimensions de la paroi étanche.

Le Prof. *Terzaghi* distingue deux phénomènes de fluage: l'un est un mouvement de fluage superficiel, l'autre un mouvement de fluage de la masse. Le premier se manifeste dans les zones exposées aux variations saisonnières de la température et de l'humidité, et est dû à des phases de contraction et de dilatation. Les zones de fluage superficiel se différencient, parfois, par leur couleur et par une plus faible perméabilité. En général le mouvement de ces zones est lent, toutefois des mouvements rapides ont été observés.

Le fluage de la masse est causé par la force de gravité. La contrainte développée produit une déformation croissante bien que la résistance au cisaillement du matériau ne soit pas dépassée. On observe des mouvements de fluage de la masse dans les matériaux détritiques et dans la roche. Morphologiquement le fluage dans les matériaux détritiques offre l'aspect d'un glissement mais se différencie par un mouvement beaucoup plus lent. Du point de vue de l'ingénieur ce sont les surfaces de contact entre les zones de fluage et les zones stables qui sont importantes.

Prof. R. Haefeli

Prof. *R. Haefeli* replies as follows to Prof. *A. W. Skempton*'s request for more information (see Proceedings 1953, vol. II, p. 359, 5) concerning the determination of the angle of creep on the basis of measurements of the pressure exerted on the strut:

Unfortunately it appeared, after the closing of the Conference, that a small error had slipped into our paper on "The Behaviour of the Concrete Bridge Built at Klosters by the Rhaetian Railway Company, Switzerland, Under the Influence of Soil Creep Pressure" (Proceedings 1953, vol. II, p. 178). The maximum pressure of 1650 t—as mentioned in the paper—on the basis of which the dimensions of the strut were calculated is higher than the computed creep pressure; it was chosen in consideration of the possible resistance offered by the right abutment. On the other hand, the following values were calculated for the pressure on the strut due to the creep pressure on the left abutment:

for $\tan \varphi s = 0.70 : N \sim 860$ t
for $\tan \varphi s = 0.85 : N \sim 1010$ t
for $\tan \varphi s = 1.00 : N \sim 1160$ t

The maximum value for the pressure exerted on the strut, established on the basis of measurements, varies with the values assumed for the creep movement within the concrete, i.e. between 500 and 860 t (see curves R_1 and R_2 Fig. 7, p. 178). From the comparison between the computed and the measured pressures on the strut, the following limiting values are arrived at for the determinative friction coefficient:

for $R_{1\,max} = 500$ t; $\tan \varphi s = 0.35$ (and not 0.3 as indicated on page 178)
for $R_{2\,max} = 860$ t; $\tan \varphi s = 0.7$ (and not 0.6 as indicated on page 178).

Thus the agreement between measurement and computation is better than that stated in the mentioned paper (see vol. II, p. 178). Nevertheless the friction values thus obtained are substantially smaller than those arrived at in the laboratory in standard shear tests. This may be explained—as mentioned in the paper—by the relationship between friction and slide velocity, i.e. by the extraordinarily small values of the creep velocity.

L'auteur rectifie une erreur qui s'est glissée dans son mémoire (voir Comptes Rendus 1953, vol. II, p. 359). La valeur de tg, selon le fluage du béton, est de 0.35, respectivement 0.7, et non pas de 0.3 et 0.6 comme indiqué précédemment. La concordance entre le calcul et les mesures est donc plus rapprochée.

Messrs. M. Rocha, J. Laginha Serafim and A. F. Silveira

In the design and construction of large dams and of tunnels which are to withstand high internal pressure, the problem arises of predicting the rock deformability by "in-situ" tests (see also Proceedings 1953, vol. III, p. 167).

The paper under discussion presents the comparison of measurements carried out on different rocks along a tunnel. Two different testing methods were used, namely local loads applied by means of jacks and rubber cushions intended as a means of uniformly distributing the load over a circular area of 0.05 m^2, and load tests in tunnels (2.3 m in diameter) filled with water under pressure. The writer states that the jack tests merely provide information on a superficial shell of the rock the structure of which had been disturbed by the excavating and blasting work.

The Laboratório Nacional de Engenharia Civil, Lisbon, has carried out a number of similar tests to determine the properties of the rock, either as an aid in the design of tunnel linings or as means of evaluating the modulus of elasticity of dam foundations.

Laboratory tests on drill cores and on prismatic specimens are carried out, as well as "in-situ" tunnel tests. The latter are either water-pressure tests, like those described in the paper, or jack loading tests in which the load was distributed over two areas of 1 m^2, thus far more than the loaded area reported by the writer. These tests are carried out in tunnels (2 m in diameter), opened some 20 m inwards. The loads are applied either vertically or horizontally, by two 300 ton jacks, being distributed on the two opposite areas of 1 m^2 each by means of two circular oil-filled metallic cushions (Fig. 7). So far the maximum loads have reached about 300 tons, corresponding to a unit load of 30 kg.cm^{-2} on the rock.

The deformations are measured at the central part of the loaded areas by means of a special rod gauge which detects the outward displacement of the opposite tunnel walls, either vertically or horizontally.

Tests are carried out before and after grouting, so it is necessary to provide the tunnels with brickwork lining to enable grout to reach even the boundary zones around the tunnel.

From all the results obtained up to now from both shales and granites the following conclusions may be drawn:

(*a*) The comparison of the values obtained from the different types of tests shows that a general agreement exists between the results of the tests made after grouting, with both jacks and water-pressure, and those obtained from the testing of prismatic samples and drill cores, providing that the latter are numerous and properly selected.

(*b*) A correlation could be established between the percentage of recovery obtained in boreholes and the magnitude of the modulus of elasticity.

From the jack load tests carried out before and after grouting an improvement of the elastic properties of the rock was found to take place. Thus, for rocks which were very weathered but not much cracked, or whose cracks were filled by settled materials, the value of the modulus of elasticity has been found to increase by some 25%, this amount reaching 100% or even more for rocks which were not much weathered but badly cracked.

(*c*) Water-pressure tests are not reliable unless the tunnel is provided with a waterproof course or with a rubber lining to prevent water from entering the cracks of the rock, and to avoid any objectionable pressure rise in the cracks, which would lead to both erroneous results and erroneous deviations in the readings taken along different diameters.

In one of the tunnels a displacement could even be detected whose direction was opposite to the predicted one; this was ascribed to high-pressure water having penetrated into the cracks.

On présente les conclusions tirées d'essais de déformabilité des roches, tant en laboratoire (échantillons) que sur place, avant et après injection (essais aux vérins et à l'eau sous pression).

On a constaté une assez bonne concordance des différents résultats. Après les injections on a obtenu une augmentation importante du module d'élasticité. Les essais à l'eau sous pression exigent l'étanchement des galeries.

M. Ch. Schaerer

M. *Ch. Schaerer* se réfère au mémoire 7/6 sur le comportement sous l'action du fluage des terres du pont en béton construit à Klosters (Suisse) par la Compagnie des Chemins de Fer Rhétiques (Comptes Rendus 1953, vol. II, p. 175). Le Rapporteur général, M. *Skempton*, a suggéré aux auteurs de compléter les données relatives aux caractéristiques géologiques du sous-sol en mouvement et, notamment, de préciser à quelle valeur de l'angle φ correspond l'effort mesuré dans l'étançon.

Rappelons qu'il s'agit d'un éboulement constitué en majeure partie par des dolomites désagrégées, matériau pour lequel les indices moyens suivants furent déterminés au Laboratoire (en 1942) sur des échantillons prélevés dans un puits, à une profondeur de 4,5 m (sable argileux).

Poids volumétrique	γ_e^* 1,77–1,95 t/m^3
Teneur en eau naturelle	w^* 5,90–9,60%
Angle de cisaillement	tg φ 0,97–0,76
Perméabilité in situ	K'_{10} 4,10^{-4} cm/s

Comme le relève le rapporteur général, les efforts semblent avoir atteint leur limite et le mouvement de fluage du terrain se poursuit selon un régime permanent.

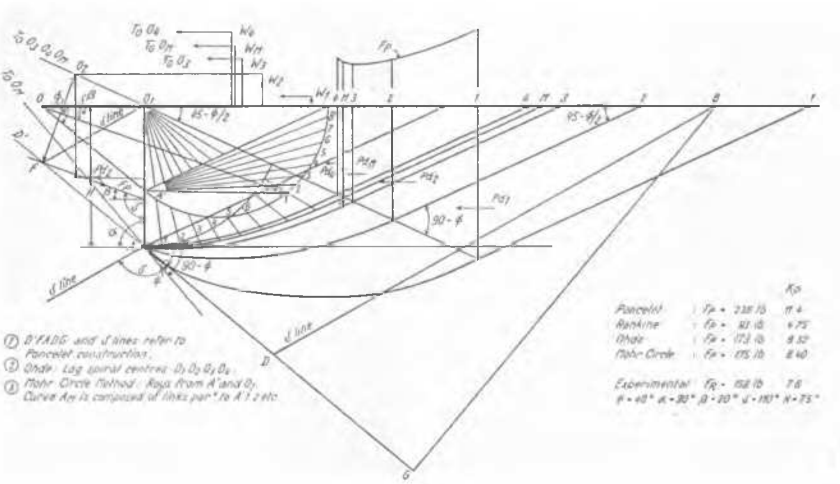


Fig. 8

The author gives some additional results regarding the subsoil of the Klosters Bridge (see Proceedings 1953, vol. II, p. 175). As pointed out by the General Reporter, the strains seem to have reached the upper limit and the creep movement in the soil appears to progress at an unvarying rate.

Mr. C. F. Trigg

Many methods are available for the evaluation of active and passive earth pressure coefficients. These are mainly graphical and are based on *Coulomb*'s Wedge Theory or are "trial and error" methods using curved slip surfaces.

A new procedure is suggested, which is equally applicable to both active and passive problems. This traces the critical slip plane (for the two-dimensional non arching case) by means of lines drawn parallel to the shear planes forming a fan from the tangent point of the *Mohr* circle. The limiting shear planes in the fan are determined from the directions of the principal stresses at the wall face and at the surface of the backfill.

Fig. 8 shows the method compared with those of *Ohde* and *Poncelet*. The experimental value includes only the horizontal component of the passive thrust.

Fig. 9B shows its application to a sloping wall with friction. The failure curve consists of three separate components; $b'c'$ being obtained from the tilted circle while $c'd'$ is drawn from the horizontal *Mohr* circle. As the wall friction β decreases to zero, $b'c'$ approaches a straight line. The limiting condition is shown at Fig. 9A.

Fig. 10 shows the method compared with *Rebhann*'s construction for the active case.

Many combinations of wall conditions and backfill slopes have been investigated and, comparing the results obtained by this method with other values indicates close correlation with

(*a*) *Ohde*'s minimum value obtained by "trial and error" for the passive case;

(*b*) *Rebhann*'s value for the active case.

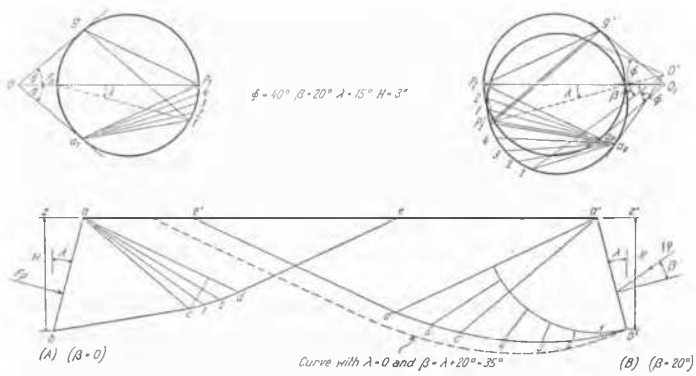


Fig. 9

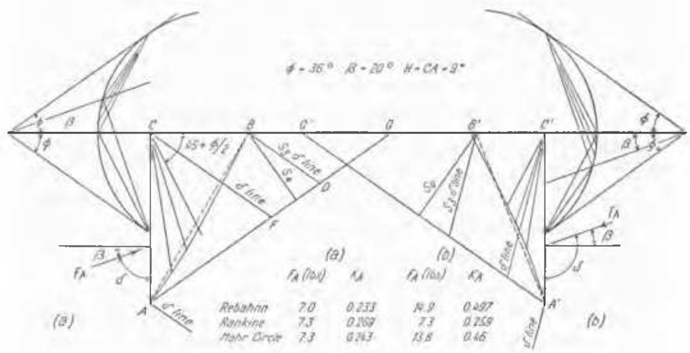


Fig. 10

L'auteur expose une méthode générale pour le calcul de la poussée des terres. Sa méthode est basée sur l'inclinaison du plan de cisaillement dans les cercles de *Mohr*.