Correspondence.

Lieut.-Col. E. P. ANDERSON remarked that further confirmatory experiments on the largest possible scale were needed, and they might well deal also with soil conditions less ideal than those under which the Author's valuable observations had been made. Experiments on the distribution of pressure through soil and ballast over a horizontal surface had been made about 10 or 12 years ago by Mr. H. S. Sales for the Indian Railway Bridge Committee. The apparatus used [1] was a "hydraulic capsule," and the method would appear to be also applicable to the measurement of pressures against an unyielding vertical or other plane, by setting similar capsules of adequate size in suitable positions. For example, if they were set at different levels against the back of a newly completed retaining-wall before the filling was started, a series of readings could then be taken with the filling at different levels and ending with the full working-load. It would seem that useful results might thus be obtained, provided of course that the wall was heavy enough not to yield in any way. Even if the buried capsules had ultimately to be abandoned, their cost would be infinitesimal compared with the economies which might result from more exact knowledge in the design of retaining-walls. The work of the Bridge Stress Committee provided a good example of the economic value of such research, and he ventured to think that equally good results might be obtained from co-ordinated research on retaining-walls, both in the laboratory and by actual observations on new walls in some such manner as that indicated. Lieut.-Col. Anderson.

Professor J. D. CORMACK considered that the Paper was a valuable record of accurate experiment and a very complete discussion of the results. It was deserving of very careful study, and the conclusions seem definite in the case of the material and wall surfaces used. They must, however, be applied with circumspection to cases where the materials and wall surfaces might have very different characteristics, and when the manner of yielding was different. To discuss it at length would require many pages and more time than he had at his disposal, but there were certain comparisons of different theories, or variations of the same theory, that might be helpful. Professor Cormack.

(i) The Author stated that he had not checked the solution given by Mr. Rice-Oxley. Professor Cormack had given that solution to his classes for nearly 30 years. The special case where

[1] Indian Railway Board Technical Paper No. 211, Appendix A, p. 33.

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Professor Cormack.

$\alpha > \beta$ was dealt with hereafter, and it was shown that the results were in agreement with the Author's analysis.

(ii) For purposes of comparison consideration would be given to the horizontal force P. For a given batter when there was no second plane of rupture, $P = r \sin(\alpha + z_1)$, so that r might be found if desired.

(iii) Taking the notation of *Fig. 4*, the general expression, in the case where there was no second plane of rupture in the material, was

$$r = \frac{w(\text{AB})^2}{2} \cos(\alpha - i) \frac{\sin(\alpha + \gamma)\cos(\gamma + z_2)}{\cos(\gamma + i)\sin(\alpha + \gamma + z_1 + z_2)} \quad . \quad (1)$$

If α was varied AB was varied; but v, the perpendicular from B to the earth surface, was constant. The expression for P was then

$$P = \frac{wv^2}{2} \frac{\cos(\alpha + z_1)}{\cos(\alpha - i)} \times \frac{\sin(\alpha + \gamma)\cos(\gamma + z_2)}{\cos(\gamma + i)\sin(\alpha + \gamma + z_1 + z_2)} \quad . \quad (2)$$

(iv) *Wall back vertical.*—When the wall was vertical, height h, $\alpha = 0$, and earth slope i, and if $z_1 = i$ and $z_2 = \phi$, the angle of the plane of rupture in the material might be shown to be $\gamma = \frac{1}{2}\left(\frac{\pi}{2} - \phi + \epsilon - i\right)$ where $\sin\epsilon \sin\phi = \sin i$, and the maximum value of P was then

$$\frac{wh^2}{2} \cos^2 i \times f, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where

$$\begin{aligned} f &= \frac{\cos i - \sqrt{(\cos^2 i - \cos^2\phi)}}{\cos i + \sqrt{(\cos^2 i - \cos^2\phi)}}, \\ &= \frac{\cos(\epsilon - i) - \sin\phi}{\cos(\epsilon - i) + \sin\phi}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4) \\ &= \frac{\sin(\epsilon - i)}{\sin(\epsilon + i)}. \end{aligned}$$

The first form of f was the usual Rankine expression. The second form showed clearly the effect of surcharge, and when $i = 0$, $\epsilon = 0$. The third form was convenient for calculation.

In the case where $\alpha = 0$, $i = 0$ and $z_1 = \phi$, Professor Boussinesq,[1] taking $\gamma = \frac{\pi}{4} - \frac{\phi}{2}$, had obtained a formula which might be reduced to

$$P = \tfrac{1}{2} wh^2 \frac{1 - \sin\phi}{1 + 2\sin\phi} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

[1] Minutes of Proceedings Inst. C.E., vol. lxv, p. 218.

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The maximum pressure wedge theory in that case gave Professor Cormack.

$$\tan \gamma = \frac{\cos \phi}{\frac{1}{\sqrt{2}} + \sin \phi} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

not $\frac{\cos \phi}{1 + \sin \phi}$ as given by Boussinesq. The result given by Dr. A. Morley in the reference quoted was correct.

A simple method of calculation was to find γ from the correct expression for $\tan \gamma$, and insert in the equation

$$P = \tfrac{1}{2} wh^2 \tan \gamma \frac{\cos (\gamma + \phi)}{\sin (\gamma + 2\phi)} \cos \phi \quad . \quad . \quad . \quad (7)$$

the form of (2) for this case.

(v) To get the second plane of rupture, assume that the angle of the second plane of rupture was β, where $\alpha > \beta$ (*Fig. 20*). Let the friction angle on BC and BE be ϕ. Equation (2) became

$$P = \frac{wv^2}{2} \frac{\cos (\beta + \phi)}{\cos (\beta - i)} \frac{\sin (\beta + \gamma) \cos (\gamma + \phi)}{\cos (\gamma + i) \sin (\beta + \gamma + 2\phi)} \quad . \quad (8)$$

For the wedge angle $\gamma + \beta$ the maximum value of the horizontal thrust was obtained by putting $\frac{dP}{d\gamma} = 0, \frac{dP}{d\beta} = 0$. The solution was

$$\gamma = \tfrac{1}{2}\left(\frac{\pi}{2} - \phi + \epsilon - i\right) : \beta = \tfrac{1}{2}\left(\frac{\pi}{2} - \phi - (\epsilon - i)\right). \quad . \quad (9)$$

These values were the same as the Author obtained from his analysis. Inserting them in equation (8), the maximum value of P on the second plane of rupture was given by $P = \frac{wv^2}{2} \times f$ or $\frac{wh^2}{2} \cos {}^2 i = f$, where f had the value given in equation (4), v was the length of the perpendicular from B to the earth surface, and h was the vertical height of the earth surface above B. The horizontal pressure on the second plane of rupture was the same as the horizontal pressure on the vertical plane through B.

This was a simple form of "Working-Rule III." Equation (2) therefore included the revised wedge theory, Parts I and II.

When there was a second plane of rupture, and $\alpha > \beta$ the procedure recommended by the Author corresponded to that adopted by Rankine for walls with small batters. The procedure seemed justifiable if (*Fig. 26*) the resultant of the total pressure on BA and the

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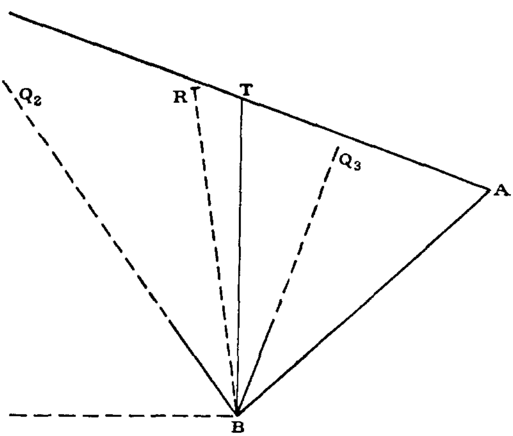
Professor Cormack.

weight of the wedge BAE had on the wall back BE an obliquity less than z_1.

(vi) It would be interesting to have further particulars of the experiments on steel-backed walls; especially whether a second plane of rupture was found, and if so the values of β for various angles a. For complete verification of the wedge theory its results should be in accord with experiments on wall-backs for which z_1 was considerably less than ϕ. If $z_1 > \phi$ the result would be the same as $z_1 = \phi$, for there would be slip, earth to earth, on a plane very close to the wall-back.

(vii) If the ellipse of stress was applied to earth-pressures the major axis BR (*Fig. 44*) lay as shown at an angle $\frac{\epsilon - i}{2}$ to the vertical

Fig. 44.



BT, and the planes on which the friction angle or obliquity was ϕ (the maximum obliquity for equilibrium) were inclined at angles RBQ_2 and RBQ_3, each equal to $\frac{\pi}{4} - \frac{\phi}{2}$. Those planes were therefore inclined $\frac{\pi}{4} - \frac{\phi}{2} + \frac{\epsilon - i}{2}$ and $\frac{\pi}{4} - \frac{\phi}{2} - \frac{\epsilon - i}{2}$ to the vertical, and were the planes found by the Author and found from equation (2) above. The principal axes were $X = wh \cos i \frac{\sin \epsilon}{\sin(\epsilon + i)} (1 \pm \sin \phi)$, and BR was the major axis with the + sign and taking $w = 1$. The fundamental basis of the ellipse of stress was that at a point the shear stress must be of equal intensity on two planes at right angles. The ellipse of stress gave the same result as the wedge theory, where the conditions

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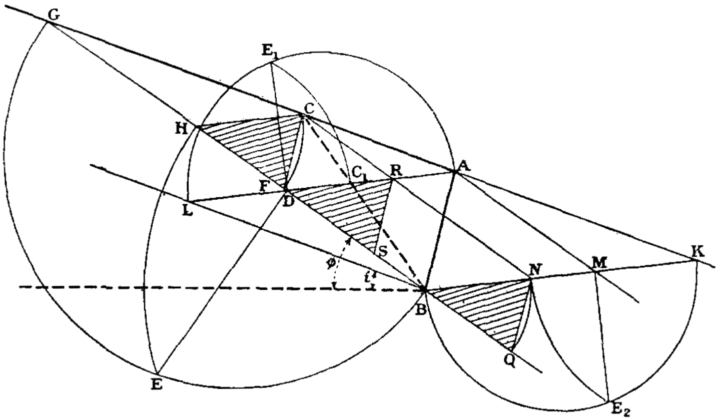
were satisfied and the obliquities were proper to the positions of the planes, *e.g.* (1) $a = 0$, $z_2 = \phi$, $i = 0$, $z_1 = 0$; (2) $a = 0$, $z_2 = \phi$, $i = i$, $z_1 = i$; (3) $\beta = \frac{\pi}{4} - \frac{\phi}{2} - \frac{\epsilon - i}{2}$, $z_2 = \phi$, $i = i$, $z_1 = \phi$. Professor Cormack.

(viii) With reference to Rule I, Rebhann's construction gave a simple graphical means of determining r and P. It was shown in *Fig. 45*, and with it two alternative constructions. The second construction was as a rule preferable, and easily proved. That all gave the same result was checked by the points CR and N being on

Fig. 45.



one line, CN, parallel to BG. The three constructions were as follows:—

Rebhann's Construction.—AB the wall back and AG the earth surface. Draw BG at ϕ to the horizontal. Make BAD $= \phi + z_1$. On BG as diameter describe a circle and draw DE perpendicular to BG, meeting the circle in E. Mark off BH $=$ BE and draw HC parallel to AD, meeting the earth surface in C. Make HF $=$ HC. Then the weight of the wedge CHF (to scale), 1 foot thick, is the total pressure r on the wall back AB per foot run of the wall. BC is the plane of rupture.

Second Construction.—Draw AB, AG and BG and AD as before. Also draw through B a line making i with the horizontal and meeting AD produced in L. On AL as diameter describe a circle. At D erect a perpendicular to AL meeting the circle in E_1. Make $LC_1 = LE_1$ and make $DR = AC_1$. Make DS $=$ DR. Then DRS is the pressure wedge, and BC_1 is the plane of rupture. C_1 is found to be on BC.

Third Construction.—Draw AB, GA produced, BG produced as before.

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Professor Cormack.

Make the angle ABK $= \phi + z_1$, meeting GA produced in K. On BK as diameter describe a circle. Draw AM making an angle ϕ with the horizontal and meeting BK in M. Erect ME_2 perpendicular to BK, meeting the circle in E_2. Mark off KN $=$ KE_2. On GB produced make BQ $=$ BN. Then NBQ is the wedge. Through N draw a line parallel to BG, meeting AG in C. Then BC is the plane of rupture.

With reference to Rule III, when there were two planes of rupture on which the friction angles were ϕ, β could be calculated from equation (9) or by the use of the ellipse-of-stress construction. Laying off BA making angle β with the vertical, the wall-back lay to the right of AB. Proceeding by one of the constructions and making the angle ABK or BAD $= 2\phi$, the total pressure on AB was found. It made an angle ϕ with the normal to AB, and combining it with the weight of the wedge BAE, the total pressure on BE was found, and the horizontal pressure could be obtained if required.

With reference to the statement on p. 123 that when i had the limiting value ϕ, r could no longer be found graphically by the construction used, it was interesting to note that the foregoing constructions did not fail in this limiting case. In the second construction, as i approached ϕ, C_1 approached D and $AC_1 =$ AD in the limit. The pressure wedge RDS had its sides RD and DS each equal to AD. In the third construction, as i approached ϕ, K approached M and N approached M, and in the limit the pressure wedge had its sides BN and BQ increased to BM. If the whole pressure, r, was found, $P = r \cos(\alpha + z_1)$.

Making the same assumptions as the Author, the position of the centre of pressure might be found from the formula

$$x = \frac{1}{3}\frac{\sin(\alpha+\gamma+\psi+\phi)\{\cos(\alpha-i)\sin\gamma-\sin\alpha\cos(\gamma+i)\}}{\cos\phi\cos(\alpha+\psi)\cos(\alpha-i)-\cos\psi\cos(\gamma+\phi)\cos(\gamma+i)},$$

to which form the expression given by the Author might be reduced. The maximum pressure wedge theory and the ellipse of stress gave the same results for P in the three following cases, the values of z_1 being those appropriate for the ellipse. The angle of the plane of rupture was the same in these cases for both theories, and it was to be expected that the wedge theory would give the same value for x as the ellipse of stress. That was found to be correct, and in each case $x = \frac{1}{3}$.

Case I. $i = 0,\ \alpha = 0,\ z_1 = 0,\ z_2 = \phi,\ \gamma = \frac{\pi}{4} - \frac{\phi}{2}$.

Case II. $i = i,\ \alpha = 0,\ z_1 = i,\ z_2 = \phi,\ \gamma = \frac{\pi}{4} - \frac{\phi}{2} + \frac{\epsilon - i}{2}$.

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Case III. $i=i, \alpha=\frac{\pi}{4}-\frac{\phi}{2}-\frac{\epsilon-i}{2}, z_1=\phi, z_2=\phi, \gamma=\frac{\pi}{4}-\frac{\phi}{2}+\frac{\epsilon-i}{2}$. Professor Cormack.

So far as sliding on the base was concerned, the position of the centre of pressure was not a matter of importance; but when questions of stability and the distribution of stress on the base were concerned, the position of the centre of pressure was of great importance.

When a wall slipped, a moving mass of earth resulted, in which there was an "arching" effect, and the surface of rupture was probably not a plane. It seemed that there was at present no adequate theory and that reliance must be placed on the results of experiment to give satisfactory rules.

Dr. T. E. N. FARGHER stated that some years ago he had an opportunity of conducting some research work on earth-pressure, and carried out many experiments, but lack of time had prevented him from making a complete analysis of the results. The Paper raised many interesting points, but he would confine his remarks on it to two things, namely, the "angle of friction" and the "plane of rupture." The Author had pointed out clearly, particularly in his Paper to the Royal Society, the effect of dilatancy and the essential differences between loosely-packed sand and compacted sand. The experiments described in the present Paper had been made on loosely-packed sand, and Dr. Fargher entirely agreed as to the difficulty of measuring the coefficient of friction of sand on sand for such material. Indeed, he did not consider that there was such a figure, since before one mass of loosely-packed sand could slide over another, a considerable rearrangement of the grains must take place at the plane of ultimate shearing, and not until the sand near that plane had ceased to be in the loosely-packed state did true shearing take place. When he made the experiment described in the last paragraph on p. 112, he found that there was a distinct movement of the top half of the box at a very low force, and that, as that force increased, so did the movement, until, at a final maximum force, complete failure occurred. The preliminary movement varied with the intensity of the load on the sheared surface and the length of the box, but amounted to as much as 2 inches in one experiment with a box only 8 inches long. When the sand was tamped into the box, the preliminary movement was practically nothing, since no rearrangement of the grains could take place without complete failure. The force required to cause complete failure in the "loosely-packed" sand gave an angle of friction of 31 degrees 5 minutes (the mean of fifty-eight readings) the extreme values being 29 degrees 41 minutes and 32 degrees 40 minutes. The value for the "compacted" sand varied much more, owing to the difficulty of reproducing similar

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Dr. Fargher. conditions, but averaged 38 degrees 40 minutes for failure, though a lower force would probably have been required to cause continued motion. The high figure in this case was probably due to artificial arches formed by ramming the sand into the small confines of the box.

As the preliminary movement appeared to be of considerable importance, it was investigated by photography, using a glass-sided box. *Figs*. *46–48*, which were diagrams made from the photographs, showed the box with vertical lines of dyed sand against the glass.

Fig. 46.

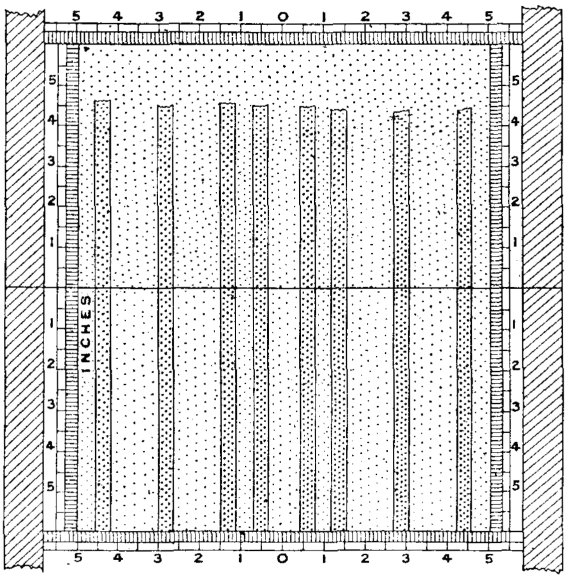

Fig. 47 showed the effect of moving the top half of the box about 0·3 inch towards the left. It would be noticed that the end lines were badly distorted near the shearing surface, and were just beginning to shear. The lines in the centre were distorted for quite a distance from the shearing plane, but were continuous and showed no signs of breaking. *Fig. 48* showed complete shearing just beginning. The top of the box had moved 0·53 inch, the total length of the box being 12 inches. It would be noticed that the end lines were displaced to the extent of their own width, and the effect of the rearrangement of the grains in the centre could be seen to extend to several inches above the plane of shearing, as indicated by the bends in the lines. These investigations clearly explained the preliminary movement in the friction box, but immediately introduced

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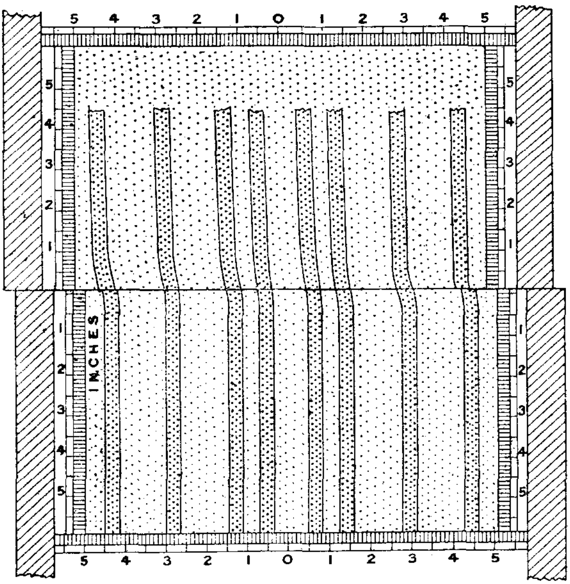
a factor, which, when applied to loosely-packed sand, appeared to him to rule out any theory which depended upon the idea of a wedge of sand sliding down a plane, the reaction of which, R, made an angle z_2 with the normal equal to the angle of friction ϕ (*Fig. 4*). This angle ϕ, representing the maximum resistance to movement, could not be reached, as shown above, until a very large internal rearrangement of the grains (at least $\frac{1}{2}$ inch in 12 inches) had taken place, which in the case of a wall 20 feet high would be at least 10 inches and probably more, since the rearrangement in that case would extend much farther on each side of the final plane of rupture. The horizontal force on the Dr. Fargher.

Fig. 47.



wall would be greater than that suggested in the Paper if the angle z_2 was less than the actual angle of friction ϕ, and the full retarding force of the friction on the surface of rupture, corresponding to ϕ, could not be attained unless the wall moved forward so much that in a practical case it would already have failed. The Author's method of conducting his experiments allowed just that movement to take place, and it appears to Dr. Fargher that the recurring cycle of results obtained by the Author was due to that effect, that was to say, the largest force was due to the preliminary arrangement of the grains, and then the force decreased as the true angle of friction was approached. When the movement of the wall had

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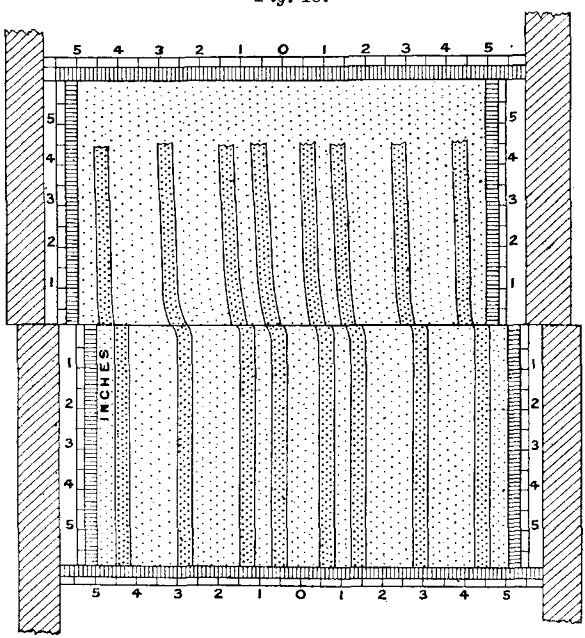
Dr. Fargher. reached a certain limit, a new surface of rearrangement had to form, since the original one had been left behind, and the cycle recurred. For the reasons stated above, he could not agree with the Author, so far as loosely-packed sand was concerned, in his remark on p. 113, that the minimum angle of friction was after movement had taken place, though he agreed that that would be the case in compacted sand.

The Author assumed that the surface of rupture was a plane of no appreciable thickness. *Figs. 46–48* indicated clearly that until

Fig. 48.



quite a large movement had taken place there was no such thing as a plane of rupture, but rather a zone of rearrangement of the granular structure, and that the resistance to movement during this rearrangement was much less than that provided by the final state of slipping down a plane. The value of z_2 in the Author's wedge theory should therefore be much less than the "coefficient of friction" ϕ obtained by complete shear; and since the action of the sand corresponding to the friction box was what he was using in his formula, he should use the value corresponding to the preliminary movements of his friction box (p. 112) rather than the angle corresponding to the shear or the angle of repose. It might be pointed out that if Dr.

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Fargher's suggestions were correct, few retaining-walls designed on the Rankine formula, which had no factor of safety, would be standing to-day; but he did not think that sand in the loosely-packed state was ever met with except in experiments. The fact that proper consolidation of the backing of walls was always insisted upon in specifications automatically prevented any possibility of that state existing. Dr. Fargher.

From other photographs taken it would appear that, on the experimental scale at least, the "surface of rupture" was not necessarily a plane. In these experiments the door holding up the sand at the right-hand side of the box was allowed to move away from the sand during the exposure of the negative. The positions of the surfaces of rupture were shown in *Figs. 49* (p. 206).

There might be scale effects from the small size of the apparatus, though they represented larger masses of sand than those used by the Author. Dr. Fargher felt, however, that the assumption that the surface of rupture was a plane in large-scale work should be thoroughly investigated, even for dry granular material, before any definite conclusions were drawn. That it was not a plane surface in materials with a more complicated structure, such as clay, was well known.

Dr. A. L. Higgins observed that the Author's valuable contribution to the subject would well be appreciated by all who knew the difficulties concomitant with such experimental work. Apart from the principle of dilatancy, it appeared (from the practical aspect) to verify the work of Mr. Jacob Feld,[1] even with due reservation as to the position of the resultant thrust upon the wall. It also emphasized the limitations of the general application of Rankine's method, which, ignoring wall friction, introduced it in varying degrees both with batter-angle and surcharge. Rankine's theory doubtless obtained within the mass, but was inapplicable to vertical planes subject to perceptible bodily movement. In general terms, the Author's experiments would suggest the use of Poncelet's application of the wedge theory to retaining-walls. Dilatancy, or some kindred effect, doubtless existed in a mass of fine granular material, and normally it would be more pronounced near the free surface. But in the Author's experiments with the moving wall an exaggerated condition of dilatancy was created by movement, and that was presumably the cause of the observed cycle in the positions of the planes of rupture. The repetition of the steeper plane would suggest that under experiment movement had ultimately re-established the initial state of local aggregation. It was surprising what regularity Dr. Higgins.

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[1] "Lateral Earth Pressure, etc.," Trans. Am. Soc. C.E., vol. 86 (1923), p. 1448.

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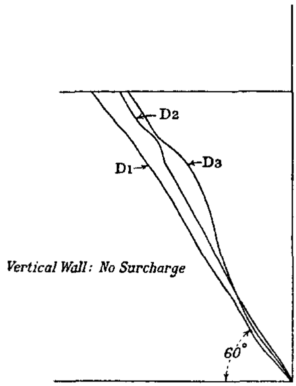
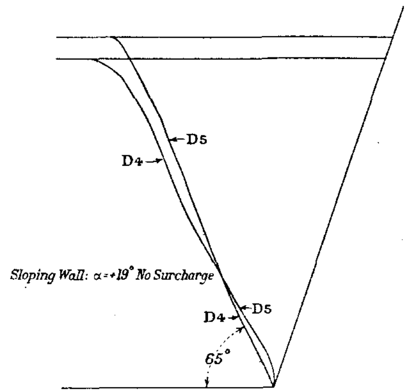
Dr. Higgins.

Figs. 49.

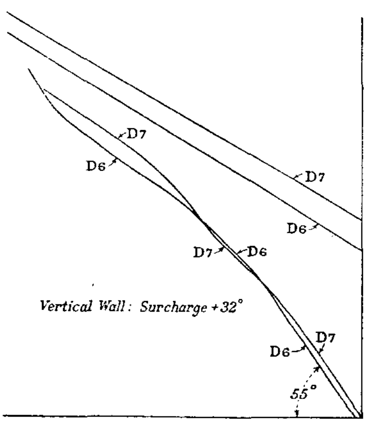


MOVEMENT OF WALL CONTINUED WITH SAME SAND.

Surface of Rupture curved slightly away from Wall at top. Note also each successive movement brings Surface closer to Wall, as would be expected by idea of preliminary arrangement of Grains.

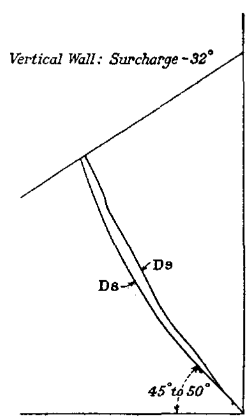


EXPERIMENT REPEATED WITH FRESH SAND.



EXPERIMENT REPEATED WITH FRESH SAND.

Surface of Rupture curves back nearly parallel to Surcharge.



EXPERIMENT REPEATED WITH FRESH SAND.

Surface of Rupture curved towards Wall.

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in results was obtainable in a mass of dry sand of known screening Dr. Higgins. and uniform aggregation. However, in his opinion, there was no justification for the Author's use of the minimum value ϕ as representative of the friction along the plane of rupture; and, as would be seen in *Fig. 8*, the experimental value was almost equidistant from the curves for ϕ and Φ near the initial position γ_1 of the plane of rupture, which alone should obtain in the purely statical problem of a retaining-wall. An initial movement—fractional of those observed—could account for the point appearing above the lower curve. Apart from its scientific value there was little practical qualification for differentiating between the friction angles used in regard to the plane of rupture or the assumed plane surface of a retaining-wall. In that connection the methods of determining the friction angles were very interesting, but the method shown in *Fig. 19* seemed to be open to question, in that artificial means were used presumably to maintain the surface of rupture, BE. Of immediate interest to himself was the further evidence of the fact that an "active" plane of rupture existed, and that under certain conditions it would approximate to that following from Coulomb's or Rankine's theories—namely, at $45° + \phi/2$ to the horizontal for a vertical supporting plane and a horizontal free surface. That fact had been demonstrated in the recent experiments of Professor Takabeya.[1] The existence of Rankine's plane of rupture might be deduced from the experiments of Professor F. H. Hummel and Mr. E. J. Finnan [2] and the late Dr. G. Wilson,[3] the movements in the mass being infinitesimally small and the active pressures low on the one hand and very high on the other. He would be interested to know whether the Author intended to apply the principle of dilatancy to the case of "passive" pressures, and whether that would explain the "transition" to a passive plane of rupture; for the existence of that plane might be premised, and something more was required to explain why the supporting pressures in a granular mass were inordinately higher than those following from Rankine's rule for foundations. He found that these were more nearly represented by $p = wx\left(\frac{1 + \sin\phi}{1 - \sin\phi}\right)^4$ at a depth x in sand of unit weight w with a horizontal free surface. The general application of the method to the various cases of retaining-walls was a monumental piece of work, and the Author was to be congratulated on the results of his researches.

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[1] *Engineering*, vol. cxxxiii (1932), p. 149, Fig. 13.

[2] Minutes of Proceedings Inst. C.E., vol. ccxii (1921), p. 369.

[3] *Ibid.*, vol. cxlix (1902), p. 208.

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Mr. Krynine.

Mr. Dimitri P. Krynine, of Yale University, congratulated the Author on a Paper which presented many interesting new ideas. In the wedge theory the Author postulated: (1) that the face BC (*Fig. 4*) was plane; (2) that that plane passed through the bottom of the wall; (3) that the experimental results were not influenced by the physical nature of an artificial floor (base-plate) placed at the level of the bottom of the experimental wall. The sliding surface in natural landslides was indubitably curved; and many continental engineers considered that to be true for retaining-walls also, especially if the filling material possessed some cohesion. It was, however, not certain that the surface BC was actually plane, even in the case of cohesionless sand. That the wedge passed through the bottom of the wall seemed at first glance to be self-evident in cases both of active and passive thrust. Professor P. Forchheimer, however, had made some tentative experiments [1] by pushing a vertical wall against a horizontal layer of sand, and had found that the surface of rupture did not pass through the bottom of the wall. Dr. Terzaghi had explained [2] this phenomenon by the action of the artificial floor. If for the floor there could have been substituted an unlimited mass of soil, the results, according to Dr. Terzaghi, might have been different. Mr. Krynine affirmed his belief that the analogy given by the Author on p. 143 (*Fig. 25*) was convincing proof of the necessity for a study of the generally ignored influence of the base-plate.

He found noteworthy the theory of cyclical variations, but hesitated to attribute their cause exclusively to a modification in the coefficient of internal friction as such. There were other views on the law of changes of the coefficient of the internal friction, for example, the theory of Dr. Terzaghi.[3] It would be interesting to know the distances between the points 1, 2, 3, 4, 5, and 6 in *Fig. 3*, since in practice only very small displacements of the wall were of importance. The second part of the wedge theory (when there were two planes of rupture) deserved special consideration. It was not clear, however, why the value of the batter (α) of the wall did not enter into formulas (1) and (2) on p. 148.

In connection with the possibility of the existence of two planes of rupture, the necessity of careful observation of the top surface of the sand, quantitative as far as possible, should be emphasized, for numerical records of changes of the sand level might advance a fruitful line of conclusions on the subject. A qualitative observation alone might be very useful sometimes. An earlier valuable

[1] Zeit. österr Ing.- und Arch.-Ver., vol. xxxiv (1882), p. 111; Neues Jahrb. für Mineralogie, 1893.

[2] "Erdbaumechanik," pp. 331–33. Leipzig, 1925.

[3] Trans. Am. Soc. C.E., vol. 86, pp. 1525 *et seq.*

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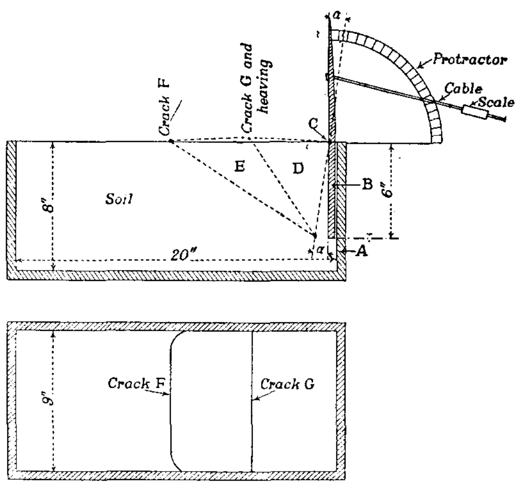
Paper by the Author [1] furnished an analogy. By observing the sand surface in an apparatus for determining the coefficient of friction by rotating a disk, important conclusions concerning the value of closeness of packing had been drawn. Another analogy might be found in Mr. Krynine's laboratory practice. A box A (*Figs. 50*) was constructed for pushing out a wedge of soil and thus determining the coefficients of friction and cohesion. A rigid metallic plate B, with polished surface turned about a horizontal axis C; the bearings of the axis were supported by the walls of the box. The box was filled with soil, and the plate was turned about the hinge C. For observing the action within the box, one of the lateral walls was made of glass. On starting the experiment, the soil Mr. Krynine.

Figs. 50.



in front of the plate became compressed and stretched. When the angle α reached a certain value (3 degrees to 5 degrees, sometimes more) the separation of the wedge could be observed. It was particularly clear in the case of slightly moist sand. It had been expected that a wedge would be obtained similar to the wedge of passive thrust; but by closely observing the top surface, a heaving and a crack just in the middle of the wedge were discovered. The experiment resulted in pushing out not one wedge, but two, D and E, of equal volume.

The Author found that the value of the angle of repose of his sand fluctuated between $30\frac{1}{2}$ and $35\frac{3}{4}$ degrees, even when the surface of the sand was plane. He observed also that the sand surface was

[1] Proc. R. S., *loc. cit.*

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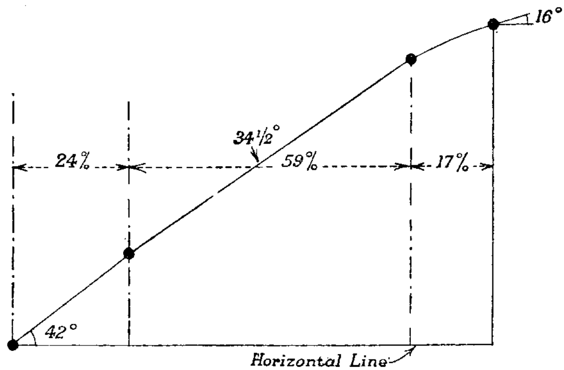
Mr. Krynine. usually rounded near the bottom; the slope at the bottom occasionally reached 38 degrees, thus being considerably more than in the middle. This observation was highly noteworthy. Engineers were accustomed to believe that cohesionless sand and similar materials in equilibrium revealed plane surfaces forming the so-called angle of repose with the horizon. Strictly speaking, that was not the case. The surface of a sand slope in a careful laboratory experiment was actually steeper at the bottom and more nearly horizontal at the top (a phenomenon first described by Professor F. Auerbach [1]). The results of one of his experiments were represented in *Fig. 51.* On about 24 per cent. of its length the slope was steeper, and on about 17 per cent. less inclined, than in the middle. The average value of the angle of repose in that case was $34\frac{1}{2}$ degrees,

Fig. 51.



being 16 degrees at the top and reaching 42 degrees at the bottom. Thus more than one-third of the length of the slope in the case of *Fig. 51* was abnormal in steepness. No adequate explanation of that fact existed. Professor Auerbach believed that limit conditions (*Randwirkung*) were responsible for it: in Mr. Krynine's opinion the arching effect might be the actual cause. The arching perhaps manifested itself more strongly against the floor and was quite negligible in the higher sections of the slope.

Several hundred papers had been written on the lateral pressure against retaining-walls. This problem was a vital one; international efforts were perhaps needed in order to arrive at some definite basis for the design of such walls. The resolution of another important problem of soil engineering, the settlement of buildings, should

[1] *Annalen der Physik,* 4th series, vol. 5 (1901), p. 182.

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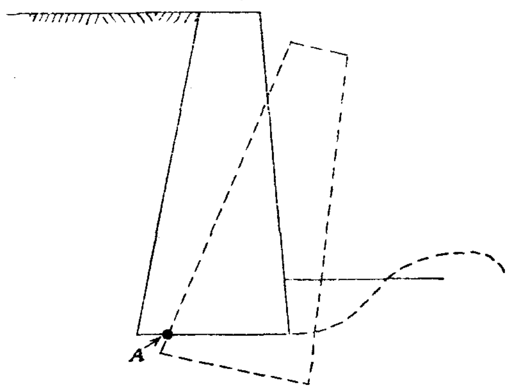
likewise receive international consideration. An International Association similar, for example, to that of Road Congresses, could consider all such soil problems, to the benefit of the engineering profession throughout the world. Mr. Krynine.

Mr. G. P. MANNING remarked, with regard to possible variation of the pressure caused by the direction of movement of the wall, which in the experiments was horizontal, that from observations of actual walls which had failed he was convinced that the cause in every case had been, not under-estimation of the earth thrust, but over-estimation of the bearing power of the foundation. The movement of the wall was primarily rotation about some point A as in *Fig. 52*, but that was immediately followed by bodily forward movement and sinkage. Thus the top of the wall moved faster than the bottom, and the whole wall dropped. The rotation of the wall might reduce the vertical Mr. Manning.

Fig. 52.



component of the active pressure of sand, and it seemed that the bodily downward movement would still further reduce it. Could the Author devise an experiment to test that point ? In clay soils, where the backing came forward in a more or less solid lump, the effect of the downward movement of the wall must be considerable. With regard to L-walls of the type in *Fig. 53*, it was usual to regard the vertical dotted line as the virtual back of the wall, and to assume a horizontal pressure on that line acting at one-third of the height, H, while the weight of the shaded area was taken as the weight of the wall. From the Author's remarks on p. 136, he understood that that method was absolutely correct if the line XY corresponded with the forward angle of rupture and very nearly correct if line XY lay near that angle. Perhaps the Author would confirm that.

In the case of sheet piling tied back to anchor-blocks, it was

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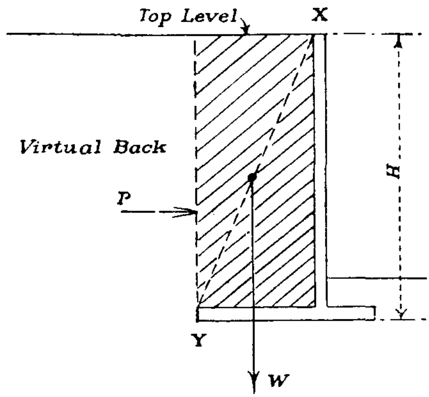
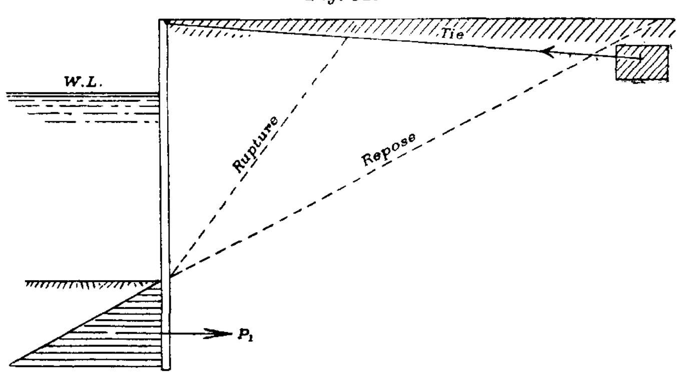
Mr. Manning. sometimes thought that the anchor-blocks should be placed behind the plane of rupture (where the plane of rupture would be for a self-supporting wall), others thought it should be placed behind the angle of repose, as in *Fig. 53*. Anchors of that type always seemed to

Fig. 53.



shift, however far back they were placed. It seemed obvious that the presence of anchors or deadmen under heavy lateral tension must affect the lines of rupture. There was urgent need for some measurements of the counterpressure P_1. Could the Author

Fig. 54.



arrange to measure it for sand under water? That was more important than the question of the anchor-blocks, as the type of sheet-pile wall shown in *Fig. 54* was found to be inferior to the type held by an earth-plate supported on raking piles.

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Mr. Manning.

The most important point which he wished to put forward referred mainly to clay soils. From observations of excavations in clay he had come to the conclusion that clay in situ, although it might appear homogeneous to the eye, was really not so. The pressure necessary to make a given impression in a vertical direction varied with the depth, and much less pressure was necessary to make a similar impression horizontally. In other words, the weight of the clay above had compacted the clay below in a vertical direction, but the small lateral pressure had effected a much smaller degree of compacting horizontally. The variation of the degree of compacting with the depth and direction was very interesting from the point of view of driving piles, particularly driving groups of piles; but that was rather away from the subject of the Paper. How would the Author propose to reproduce those conditions in a small laboratory model? Mr. Manning failed to see how it could be done satisfactorily. Small models filled with sand might, however, be useful in finding pressures in coal-bunkers, etc.

Mr. Ravier.

Mr. LOUIS RAVIER stated that he had read the Paper with great interest, having himself made experiments on earth-pressure. The Author had been so fortunate as to have official support, and therefore better means and more time than Mr. Ravier, and instead of making partial studies and observations and trying new forms of walls, the Author had been able to make an admirably complete study of earth-pressure, with the very important result of confirming the accuracy of Résal's formulas in many cases, and the way in which they must be modified in others. His theory of two planes of rupture by which he explained a part of the facts was particularly ingenious and fruitful. Mr. Ravier considered it was very satisfactory to note the Author's confidence in the accuracy of model experiments, because they agreed with the wedge theory and with Resal's results, and that could not be true on a small scale and not true for larger walls. Model experiments might therefore give engineers confidence in trying new forms of walls. He had found it much easier to make the experiments with gravel than with sand, with which he had had much trouble owing to the cohesion when it was not perfectly dry. He thought gravel was preferable for rapid experiments with limited resources, and for trying new forms of walls. With gravel, also, troublesome leakages were avoided without anything like rubber tissue at the joints.

The Author had found that when a wall began to move forward the pressure behind increased, while Mr. Ravier, and Dr. Terzaghi also, had found that it diminished; but he thought the cases were different. The Author had obtained his result with a wall remaining vertical when moving, and without adding overload.

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Mr. Ravier. Mr. Ravier had obtained his result with a wall which moved only from the top, the base forming a pivot, and with a surcharge, and then the increase of the pressure behind the wall was less than proportional to the increase of the surcharge, but the diminution, in comparison with the proportionality, was smaller when (owing to the use of stronger scale springs) the wall advanced less from the top.

Mr. Robertson. Mr. R. G. Robertson remarked that the Paper gave the results of a large number of tests, from which important conclusions might be drawn with a certainty which formerly was rather the result of intuition than of accepted reasoning. In all the diagrams in which the forces were given they were stated in pounds, and he presumed that except where otherwise expressly stated they were the measured forces on the model wall shown on p. 105. *Figs. 11* and *12*, showing the variation in the forces on a wall with varying slopes of the back of the wall, afforded information which he believed was given here for the first time. Taking for comparison a sheet-pile wall and calculating the over-turning moment about the foot, for a vertical wall without surcharge it was equal to that due to a fluid weighing 28 lbs. per cubic foot. If the wall had a backward batter of 10 degrees, the moment was reduced by 25 per cent. If it had a backward batter of 20 degrees, the moment was reduced by 50 per cent. It was interesting to recall that in the older designs of dock and lock walls the walls often leaned backwards. If the wall leaned forward 20 degrees, the moment was increased by no less than 60 per cent. For the L-shaped wall on p. 131, the horizontal force was 10 per cent. greater than the force on a wall with a vertical back, and the downward force was greater than the weight of sand vertically above the step by an amount which was two-thirds of the maximum friction on the back of the wall.

Failures of walls backed with granular material were seldom reported. In a search through the "Proceedings," he had found only one which was described by Sir Benjamin Baker; the failure was due to a surcharged filling tipped on a sloping rock base, and was cured by stepping the base in level benches. A recent failure where the backing was gravel ballast might be of interest. The wall was a small wing wall, projecting from the upper part of a bridge-pier, *Fig. 55*. The wall was cantilevered out from the bridge-pier and had no foundations. It retained only the upper part of the embankment, the lower slope being formed by material flowing out beneath the wall. When the embankment was tipped, the wall failed, and an explanation could perhaps be found in the action of the friction on the plane of rupture. With a settlement of the backing while the wall was rigid, friction on the plane of rupture could be imagined to act in an opposite direction to that assumed when it was the wedge

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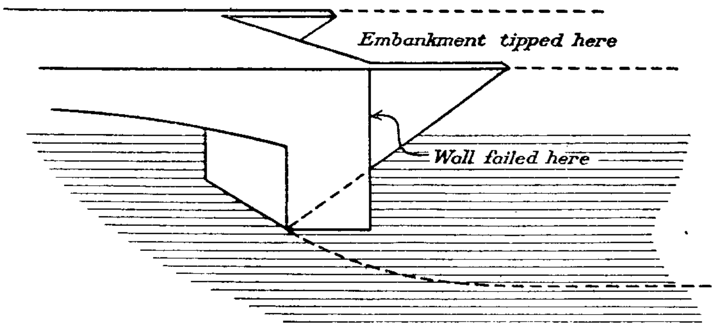
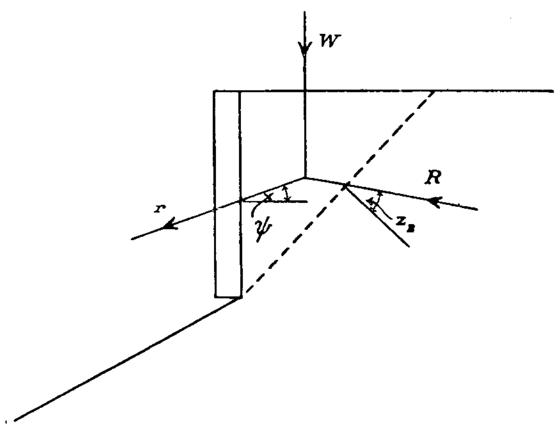
of material that was settling (*Fig. 56*). In the "Proceedings" many failures were recorded in cohesive soils described as "plastic shale," "peat," "clay," "sandy clay," and "yellow clay." When dealing Mr. Robertson.

Fig. 55.



with those materials, therefore, it seemed that, until the Author had given as clear an insight into the properties of cohesive soils as he had given into the properties of granular soils, reliance must be

Fig. 56.



placed on comparative tests of the hardness or otherwise of the actual material behind the wall, as measured by some such apparatus as that described by Mr. H. G. Lloyd.[1]

[1] *Ante*, p. 188.

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Professor Southwell.

Professor R. V. Southwell felt that, in view of the close agreement between theory and experiment which was revealed by the Author's measurements of resultant force (*Figs. 11* and *12*), it became an urgent question why he should have obtained relatively anomalous results in his determinations of the position of the centre of pressure (*Figs. 13* and *13a*). The theory suggested on pp. 121–124 was based upon a rather questionable assumption, and (as the Author emphasized, p. 124) it gave results which were not confirmed by experiment except within a limited range. Professor Southwell had no better theory to propound; but he offered the following remarks which might perhaps suggest a line for further investigation.

An essential feature of the Author's "Revised Wedge Theory" was its concentration on resultant forces: no restriction was imposed upon the height of the centre of pressure, other than what was implied by the requirement that the lines of action of r and R (*Fig. 4*) must intersect on the line of action of W. But was the theory applicable only to a wedge of sand having its vertex at B, or could it be applied to a smaller wedge contained between the wall and a new plane C′B′, parallel to CB, but cutting AB in some intermediate point B′? In assuming that the line of action of r was inclined to the wall at some known limiting angle z_1, the Author had tacitly assumed that limiting friction had been reached at all points in AB, and from that it must follow that the resultant force exerted by the smaller wedge must have the same inclination to the wall: if limiting friction had also been reached at all points on C′B′, the resultant action on that plane must, by a similar argument, be parallel to R. But the triangle of forces for the wedge ABC, drawn on the right of *Fig. 4*, would equally apply to the similar but smaller wedge A′B′C′; only the scale was altered, and it would follow that the forces acting on A′B′C′ which corresponded with the forces r and R on ABC would be reduced in the same proportion as the weight of the wedge. Hence the resultant force on any part AB′ of the wall AB must have (i) the direction of r, and (ii) a magnitude proportional to $(AB')^2$; only a linear distribution of stress on AB could satisfy that requirement, and a linear distribution would involve a centre of pressure at a height $\frac{1}{3}$AB above B. To discard the assumption that limiting friction acted all along BA would be to lose the theoretical expression for H_m which was so satisfactorily confirmed by the Author's experimental observations. The only alternative that Professor Southwell could see was to assume that, of all planes parallel to CB, only that plane which passed through the base B (that was, the plane CB itself) had limiting friction at every point. It would seem too contradictory of the whole idea underlying the Author's theory to assume that an inclination greater than

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z_2 could be attained; but there was nothing impossible in the idea of an inclination smaller than z_2 to some parts (at least) of the plane C′B′. Referring again to the triangle of forces on the right of *Fig. 4*, it was clear that a smaller inclination to CB of the vector lettered R would (since the weight of the wedge and the direction of the vector lettered r were definite) involve an increase in the magnitude of the vector lettered r. Thus the smaller inclination (which alone seemed possible) would involve a relative increase of pressure on the upper part of the retaining-wall, that was, centre of pressure above the one-third point. In *Figs. 13* and *13a*, most of the observed positions were shown as lying above the one-third point; on the foregoing argument that was explained if it might be asserted that limiting friction was not attained on every plane parallel to CB. Had the Author's apparatus any feature which might be expected to produce that result? It might, for example, be expected that limiting friction would be attained first on the plane CB, if his model wall (which might be regarded as completely rigid) had been constrained to move so that its displacement was greater at B than at any other point in BA. In that event an explanation would be lacking only in those few observations in *Figs. 13* and *13a* which gave centres of pressure *below* the one-third point. An argument along similar lines might perhaps be employed to explain the experimental results which were described in the last paragraph on p. 121. Professor Southwell.

The foregoing remarks might have some bearing on the question of possible "scale effect." The Author's remarks on that question (p. 144) struck Professor Southwell as unnecessarily hesitant, since it was surely safe to assert that *if* the factors which determined H were limited to the density and frictional angle of the material and to the linear scale of the model, then the usual argument "from dimensions" showed that H must be given by an expression of the form of equation (3), p. 133. In other words, if the model was a strict copy of the full-scale wall, an observed scale effect could be explained only on the ground that H was influenced by some factor (e.g., grain size of the "sand") which was not included in the foregoing list. But it was essential that the wall should be a scale model, as regarded both dimensions and the freedom of movement which it possessed; and the only feature of the Author's experiments which Professor Southwell could conceive to render the results of doubtful application to practice was the rigidity of his model wall. He would expect that an actual wall would yield in such a way as to permit the attainment of limiting friction on all planes parallel to CB; and if that were so, the argument which he had developed above indicated that the centre of pressure would in practice be relatively low. That, of course, would make for safety; so that the final

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Professor Southwell.

conclusion to be drawn from these remarks (if they had any application) was that the Author's rules were likely to err, if at all, in the right direction. He would like to add his congratulations on this successful issue of a very arduous and difficult piece of work.

Mr. Veal.

Mr. T. H. P. VEAL observed that the Paper showed how the magnitude, direction, and position of the point of application of the force r on the face of a wall might be determined, but did not indicate how that force was distributed over a vertical or inclined face. In the design of a reinforced-concrete wall with counterforts it was necessary to know that. The force was obviously a function of the weight of the wedge, which was again a function of the second power of the height of the wall. Consequently the distribution of the force appeared to be parabolic. The evidence given by the Author showing that the point of action of r was at about 0·4 of the height, measured from the foot of the wall, tended to confirm that assumption. In designing reinforced-concrete retaining-walls of the counterfort type, it had been customary to assume a triangular distribution of the force, and, as the Author stated, to assume the point of action of the force at one-third of the height, measured from the foot of the wall. The reduction in the intensity of pressure, from a maximum value at the foot of the wall to zero at the top, resulted in corresponding reductions in the bending-moments in the vertical slab, treated as a continuous or simply supported beam. To provide for that reduction in the bending-moment the spacing of the horizontal reinforcing bars was increased from a minimum value at the foot of the wall to a certain maximum value at a height determined by assuming that the pressure fell off uniformly. If, as appeared to be reasonable, the reduction in the pressure-intensity followed a parabolic curve, and was not linear, the spacing of the reinforcing bars in the majority of retaining-walls of that type that had been designed had been increased too much near their bases. It was not suggested that most existing walls were unsafe, but that the error introduced by assuming a triangular distribution of the force r on the face of the wall was not on the safe side, and that in the design of such walls in future it would be more correct to assume a parabolic distribution.

Mr. Walker.

Mr. E. G. WALKER thought that one of the most interesting facts brought out by the Author's experiments was the existence of the cycle of variation of the plane of rupture. The existence of that cycle would explain the variations found in practice in the pressures of dry granular materials and the differences in the recorded measurements of their friction angles. The description of the apparatus for the measurement of friction angles given on pp. 111 and 112, recalled to him some similar experiments which he made a few years ago on

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Mr. Walker.

the angle of repose of cement clinker. He was then concerned with the design of retaining-walls and structures for the storage of clinker in large quantities, and accordingly thought it desirable to obtain the angle of repose to as high a degree of accuracy as possible. At the time there was available to him a large storage heap, containing several thousand tons, which had been tipped from a gantry 15 or 20 feet above the ground, so that the material had taken its natural slope. Measurements were made with an apparatus similar in principle to that described in the Paper, and were averaged at a number of suitable points around the slope. There was a curious variation in the readings which was not explainable at the time. The measured angles ranged from 28 degrees 30 minutes to 34 degrees 0 minutes, and the following table gave the averages at different parts of the heap :—

1. 34° 0′	5. 31° 0′
2. 33° 0′	6. 30° 45′
3. 32° 45′	7. 30° 30′
4. 32° 10′	8. 28° 30′

The angles which the Author had obtained in his experiments were given on p. 113 as ranging from 30 degrees 12 minutes to 35 degrees 40 minutes, so that the ranges in the two sets of experiments were not very different. That might have some bearing upon the subject of scale effect, which was referred to on p. 144. Bearing in mind the inconclusive results of so many experiments that had been made in the past upon model retaining-walls, Mr. Walker was inclined to be somewhat sceptical of the value of tests made with very small apparatus. He had therefore followed with great interest the Author's argument in favour of the small-scale experiments. The correspondence between the results of his own measurements on granular cement clinker and those of the Author's experiments on sand would appear to favour the contention that scale effect in that case did not exist—at any rate, not to any appreciable extent. In terms of linear dimensions the grains of the sand used by the Author on his 2-inch wall would more or less correspond with the clinker nodules on, say, a 15-foot or 20-foot retaining-wall. In comparing the two cases it would appear that there should be almost identical angles of repose and almost identical cycles of variation of that angle. That would lead to the conclusion that the forces developed were likely to be proportionate, or in other words, that there was no measurable scale effect. From the standpoint of practical design, the cycle introduced a complication, since, in order to apply the curves, a number of tentative calculations must be made in place of the one simple calculation that had heretofore

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Mr. Walker. been sufficient. The difficulty was much increased when it came to the study of the stepped walls which formed the majority of the large walls constructed in practice. The Author's plan of experimenting with a model was no doubt the best way of obtaining results, but it was unfortunately only open to a few, and in most cases it was necessary to investigate a design on the drawing-board without having recourse to experiments. It would seem to be necessary, therefore, to endeavour to extend the Author's work in the direction of developing suitable approximate methods of handling the more complicated retaining-wall sections so as to enable practical advantage to be taken of the new results which had been brought to light by the Author's investigations.

Too much stress could not be laid upon the fact that the Author's experiments dealt only with granular materials. Unfortunately, in the majority of cases in which retaining-walls and similar structures were required, the materials to be supported were of very different character. In such cases the investigation of the distribution of forces must be a more complex matter than in those which the Author had studied, and it would appear doubtful whether it could be carried out successfully by the aid of small-scale models. It seemed likely that some other line of approach to the problem might have to be devised.

The Author. The AUTHOR, in reply, observed that several correspondents had referred to the "scale effect" and the need for experiments on a larger wall. He welcomed Professor Southwell's clear statement on the validity of small-scale tests, and Mr. Veal's evidence supporting that view, but perhaps it was necessary again to point out that those arguments did not apply to cohesive soil; small-scale experiments on cohesive soil might be entirely misleading. Colonel Anderson had suggested the use of "hydraulic capsules" in large-scale measurements, but all "capsules" (and there were many different types) were essentially liable to errors produced by arching. The formation of an arch over a hole in the base of a box, or the diaphragm of a capsule, had been demonstrated by the Author at the meeting by means of a mechanical lantern slide; it was illustrated in *Fig. 13* of his Paper to the Royal Society.[1] He believed that capsules could only furnish reliable data in positions where the granular material was moving past them, as for example in the sides of grain silos. To test their reliability Dr. Terzaghi had placed a large number in his very large experimental apparatus at the Massachusetts Institute, and he reported[2] that they "were found to be utterly unreliable."

[1] Proc. R. S., *loc. cit.*
[2] *Engineering News-Record*, vol. 109 (1932), p. 369.

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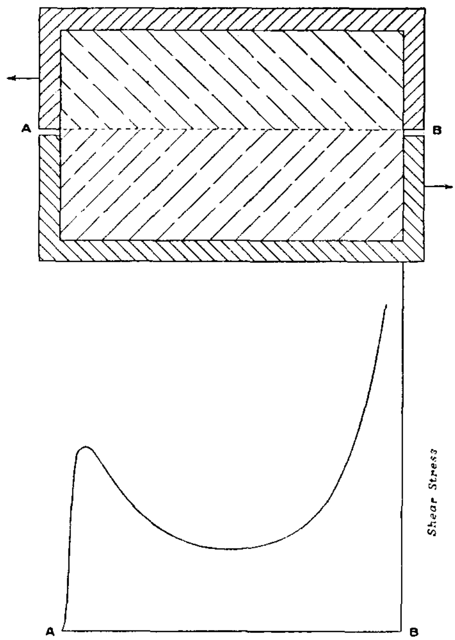
The Author was glad to note that Professor Cormack's calculation agreed with his own; that was to be expected, as they were based on the same assumptions; for the revised wedge theory only differed from the old theory by the removal of errors, the definition of the limits $(\alpha < \beta)$ within which it was applicable, and the extension of the theory to the numerous cases when $\alpha > \beta$. The Author.

Dr. Fargher's interesting experiments illustrated very well the defects of that type of experiment. It was now known that the stress

Figs. 57.



distribution in such tests was very complex, and in particular that the shear stress was not in the least uniform along the dividing plane. The sort of distribution was shown in *Figs. 57*, which was copied from tests made on celluloid by Professor Coker, and agreed with Dr. Fargher's results in indicating higher stresses at each end, and lower stress in the centre. From another point of view the defect in that test might be regarded as an example of the old difficulty with "grips." Tests on metal were rejected if the fracture occurred too close to the grips, but in this sand test the grips (the boxes) were actually touching, and fracture always

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The Author. occurred within the grips. The curvature of the columns shown in *Figs. 47* and *48* was the natural result of the complex stress-distribution. It was clear, therefore, that no satisfactory measurements of shear stress could be made in that way, and that the Figures did not represent the motion of one mass of free sand over another. The Author had watched the plane of rupture in free sand through a low-power microscope, and had found that the dividing line between moving and stationary sand was only two or three grains thick.

Dr. Higgins had criticized the method of finding the friction angle illustrated by *Fig. 19*, suggesting that "artificial means were used . . . to maintain the surface of rupture BE." That was a complete misunderstanding; the surface BE maintained itself, however far the wall was moved. The Author could not at present experiment on the pressures caused by moving the wall towards the sand, as he was fully occupied in investigating the pressures caused by clay.

He was convinced that in his own experiments the base-plate had no effect on the results, but agreed with Mr. Krynine that it would have an effect if the wall were moved towards the sand. The reason why a did not appear in the formulas on p. 148 was that those referred (as stated in the heading to Appendix I) to cases where there were *two* planes of rupture. When there was a second plane of rupture the inclination of the wall had no influence on the horizontal force; it did influence the vertical force on the wall, as explained in Rule III, p. 138. He agreed with Mr. Krynine as to the valuable information which might be gained by observing the traces of the rupture planes on the surface of the sand.

It was clearly desirable to make tests on walls which moved in the manner suggested by Mr. Manning (*Fig. 52*), and there would not appear to be any difficulty in arranging the apparatus to carry them out. Walls of the type shown in *Fig. 53* were dealt with on p. 130. The strength of anchor-blocks (*Fig. 54*) would be difficult to measure experimentally, but perhaps not impossible. The Author hoped that other investigators would take up these questions now that a satisfactory method of measurement had been found, and he was glad to learn that an experimental wall was being constructed at University College, Southampton.

Mr. Ravier had suggested the use of gravel instead of sand. The Author had used fine gravel for demonstration purposes with success, but it was important to keep the size of the grains small compared with the height of the wall. The diminution of pressure observed by Dr. Terzaghi was due to the initial ramming of the sand, not to the manner in which the wall moved. Mr. Robertson's explanation of the fracture of a wing wall was interesting, and appeared to be adequate to account for the failure.

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The Author.

The meaning of the variations observed in the height of the centre of pressure had been referred to by several correspondents. The Author entirely agreed with Professor Southwell's explanation of the direction of the forces. If those directions were drawn, they would be found to represent precisely what the Author called vertical arching. There seemed to be little doubt that that was the cause of the displacement of the centre of pressure from the hydraulic position, one-third up the wall.

He was in close agreement with the views expressed by Mr. Walker, except on one point. There was no need for " a number of tentative calculations " for any plane wall. The working-rules proposed in the Paper allowed for the maximum pressure occurring in the cycle. There appeared to be no serious difficulty in " developing suitable approximate methods of handling complicated retaining-wall sections." The easiest plan would probably be to make a considerable number of model walls (such as those shown in *Figs. 17*), covering the shapes in general use, and to tabulate the results. Approximate figures of sufficient accuracy for other designs could be obtained by interpolation.

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