Discussion.

The President, in proposing a vote of thanks to the Author, said he was glad the Paper had been read while he was in the Chair, because the Author and he had been fellow-students in years gone by, and had both found considerable difficulty in understanding earth-pressures. He did not know whether the Author felt that he had now got to the bottom of the difficulty, but at all events he had collected much information which had been put before The Institution in the past, and he had also carried out some valuable original experiments. The President.

Mr. J. S. Beresford regarded the Paper as of importance, not so much because of the figures deduced from the observations, as on account of its raising questions, to some of which he would refer. Sir Benjamin Baker in his Paper, in 1883, cited the case of a heap of sifted road-metal against a wall of pitch-pine blocks 1 foot wide and 4 feet high, the height of the metal being 3 feet 9 inches, with a top width of 5 feet, and a slope of 1·2 to 1, the angle of repose of the metal corresponding to 40°. Sir Benjamin had pointed out that the wall stood, notwithstanding all theory on the subject; and in his calculations he reduced the effect of the lateral pressure of the metal to not more than that of a fluid weighing 10·7 lbs. per cubic foot, although the metal itself was 101 lbs. per cubic foot. Mr. Beresford thought it was rather misleading to consider the pressure as equivalent to the effect of a fluid, for the idea of fluid pressure precluded the existence of friction between the back of wall and material supported. Like Rankine, Sir Benjamin had neglected the effect of friction on the back of the wall. Mr. Beresford had made calculations at the time which showed that, adopting Rankine's theory but making allowance for friction at the back of the wall, taking 0·75 as the coefficient, there was a moment of resistance of 115·5 foot-lbs. to be added to Sir Benjamin's moment of 92 foot-lbs. Thus for $\phi = 40°$, $\frac{1 - \sin\phi}{1 + \sin\phi} = 0{\cdot}217$, making the lateral pressure $\frac{3{\cdot}75^2}{2} \times 101 \times 0{\cdot}217 = 154$ lbs., and overturning moment $154 \times \frac{3{\cdot}75}{3} = 192{\cdot}5$ foot-lbs. Against the latter were the following moments of stability aggregating 207·5 foot-lbs., namely, due Mr. Beresford.

Mr. Beresford. to the weight of the wall, $184 \times 0 \cdot 5 = 92$ foot-lbs., and due to friction down the back of the wall, $154 \times 0 \cdot 75 \times 1 = 115 \cdot 5$ foot-lbs., giving a balance of 15 foot-lbs. to the good. Indirectly, the friction at the back of wall prevented the wall from sliding forward, for, with the narrow base, there would be a tendency to overturning in the first instance, which simultaneously would bring that friction into play, increasing the pressure on the base at the toe by 115·5 lbs. Mr. Beresford had found this element of friction very important in practical cases. He had experimented on the pressure of sand against a wooden shutter, counterpoised to neutralize its weight, with a horizontal base half its height bracketed on outside as a toe, the latter abutting against two pegs in the floor to keep the shutter from being pushed forward. That stood perfectly well, on account of the friction at the back. The objection to laying too much stress upon friction was, that vibration might spoil the effect; but tapping the shutter with a hammer caused no movement. Referring to the Author's experiments, the sand, etc., put into the bucket seemed to have been shaken together and rammed down. In Mr. Beresford's experience, that process changed the usual conditions altogether, and greatly affected the conjugate pressure. According to Rankine, when the surface of a mass of granular material is horizontal and the weight of the unit volume w, then the conjugate pressure at the depth x cannot be less than $wx \dfrac{1 - \sin \phi}{1 + \sin \phi}$ or exceed $wx \dfrac{1 + \sin \phi}{1 - \sin \phi}$. Experience showed that by ramming the material, especially when in a moist condition, the above minimum might be greatly exceeded and the maximum approached; perhaps an extreme case was found in consolidated road metal. He had taken a piece of wire gauze, 24 meshes to the linear inch, measuring 2·85 inches wide, bound at one end round a strong wire to pull by. This was buried to a depth of $4\frac{1}{2}$ inches in damp fine sand $\frac{1}{100}$th to $\frac{1}{200}$th inch diameter, contained in an iron basin 14 inches wide and 6 inches deep. The vertical pulls required to cause the least movement were: with the sand put in loosely, 11 ounces; when the sand was well shaken, $3\frac{1}{4}$ lbs.; when the sand was well rammed all round, $8\frac{1}{3}$ lbs., the average of three trials. He had tried experiments in the same sand with a disk 1·9 inch in diameter, fixed to the end of a brass tube to admit air to the base and prevent any chance of a partial vacuum below when withdrawing from damp sand. With the disk covered by $4\frac{1}{2}$ inches of sand well rammed there was no movement under a pull of 10 lbs.; 2 lbs. over this caused radial cracks at the surface, and a pull of 16 lbs.

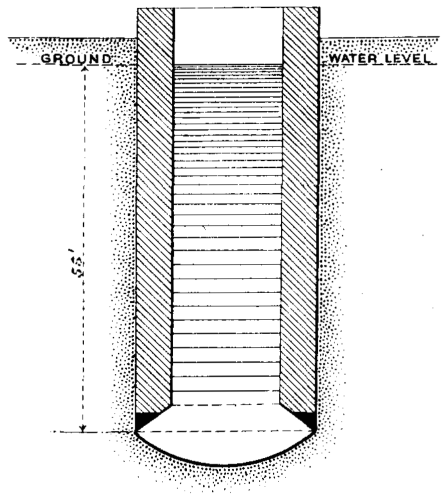
Mr. Beresford.

the upheaval of an inverted cone of damp sand with a base over $4\frac{1}{2}$ inches in diameter; the weight of the column of sand directly over the disk was only 12 ounces. Damp sand showed a higher coefficient of friction in pulling anything over it than did dry sand, and no doubt shaking down sand increased the friction, because when sand was in the loose state the particles rolled on each other a good deal. Sir George Darwin had pointed out, in his Paper, that when the particles were shaken down they became more fixed and the coefficient of friction was greater, and he made out that Rankine was altogether wrong. When sand was shaken down, he said, the density was increased by 10 per cent. The shaking down, Mr. Beresford assumed, increased conjugate pressure on the two side walls, and that was the reason why Sir George Darwin had got such low results. Everyone knew how difficult it was to drive a pile into wet sand; that was because the lateral compression brought on maximum conjugate pressure and produced great frictional resistance. Shaking and ramming down the sand into a narrow bucket produced a conjugate pressure throughout the mass much in excess of that due to mere weight above any level, the latter being what Rankine assumed. The amount of excess depended on the degree of consolidation, for grains of damp sand did not rebound when rammed, but held like a ratchet, so maintaining the excess conjugate pressure up to the maximum limit. Thus the plunger came down on artificially stiffened sand, capable of arching against vertical pressure in either direction. Even quite dry sand became hard when poked with a stick or shaken down, and offered a lateral pressure much greater than the active conjugate pressure due to the mere weight of the sand. Taking a large area, and going into feet instead of inches, a foundation pressure might be assumed of 9,000 lbs. per square foot in sand weighing 100 lbs. per cubic foot and having an angle of repose of 30°. Then, according to Rankine, as $\frac{1 - \sin 30^\circ}{1 + \sin 30^\circ} = \frac{1}{3}$, the vertical pressure of 9,000 lbs. would produce a horizontal conjugate pressure at the edge of the base of 3,000 lbs. per square foot, and again this would produce an upward vertical conjugate pressure of 1,000 lbs., which would be met by a depth of 10 feet. Rankine assumed that the resistance intensity to displacement of the outside soil corresponded with that of a fluid of the same weight; but the resistance intensity immediately outside the base was not the weight of a definite column but that of an indefinite wedge, a fact which was very pronounced in damp sand, as indicated by Mr. Beresford's own experiment with the disk. The horizontal pressure, when the piston was very small in diameter, soon became

Mr. Beresford. reduced radially. For instance, in most of the experiments the diameter of the plunger was very little over 1 inch, and ½ inch from the plunger this outside pressure was only half what it was at the edge. That did not apply to the case of a long wall, where this pressure did not decrease in the same ratio. Mathematical deductions from Rankine's foundation formula, without regard to the many physical conditions involved, would lead to errors. One of them he had already mentioned. Therefore he felt very doubtful whether, in these experiments, the question of conjugate pressure was raised at all, whether the whole of the small depression in inches was not due to the condensation of the material, which would largely account for its uniformity, because the depression would not be uniform if conjugate pressure had come in under actual conditions. The vertical resistance would have gone on increasing, at a rate something like the square of the depth, or rather in proportion to the volume of the resisting earth cone outside. Considering that the area of plunger was only 1·33 inch in most of the experiments, and taking the penetration of 2 inches, if the bucket held 1,000 cubic inches, the whole of the displacement was only ¼ per cent. of the volume: nothing was said of any bulging up at the surface. He also had made experiments of the kind, but he gave them up because he found that when the plunger came to rest under an applied load and was then gently tapped it went down considerably further: afterwards when turned slightly backwards and forwards it went down rapidly at first then slowly till it sunk no further under this movement. This showed that permanent frictional resistance had not been attained in the first instance. He always revolved it round completely as a final test, to see if he had got to the limiting point, and he found, sometimes, that the secondary depression due to revolving slightly was more than the original penetration. The Paper was valuable because it raised questions of this kind. He had laid on the table photographs showing test-loading of the well-foundations of the new Nadrai aqueduct, which was the biggest in India, if not in the world. The first aqueduct, the well-foundations of which went down to a clay stratum at a depth of about 30 feet, was washed away by an abnormal flood in 1885. The new aqueduct had fifteen arches of 60 feet span, and rested on 268 deep circular wells 20 feet to 12 feet in diameter. The load on the foundations was 6½ to 7½ tons per square foot. The orders were that the depth of the wells should be so regulated as to impose 2½ tons per square foot on the base;

the rest of the load was to be taken by friction. The matter was given to a mathematician to calculate, who inverted formula $\frac{1 - \sin \phi}{1 + \sin \phi}$, and produced such an excessive conjugate pressure that to get a fair result required a very low coefficient of friction on the well. The calculation gave a depth of 55 feet. Mr. Beresford was in charge of the work, and on asking for the calculations he saw the mistake. It was a very important question, as the first aqueduct had failed, and the second was to be about six times as large as the first. It was decided to test-load some of the wells that had gone down suddenly on passing through the clay. One well dropped 13 feet into a crater 16 feet deep in the centre. The loading consisted chiefly of brickwork in weak mortar easily dismantled. 30 feet below, there was 14 feet of clay. The first aqueduct had been founded on this clay. They wanted to get through the clay, which would serve as a deep flooring if there should be an abnormal flood again. Some of the wells went down 16 feet, with heavy gantries on the top. Four (*Fig. 18*) were test-loaded. First, they were cleared out at the bottom to a depth of 4 feet below the cutting edge of the curb to test the lateral friction. As a typical case of a 20-foot well thus tested, the weight of the thick brick steining, deducting that of water displaced, was 300 tons; on adding a load of 205 tons there were distinct signs of movement, the addition of another 400 tons causing it to sink 5 to 7 inches. With the wide clearance made through the clay, the well was surrounded with super-saturated sand throughout its depth of 55 feet below ground-water level, above which there was wide excavation. Taking the effective weight of this sand at 70 lbs. per cubic foot, its internal angle of friction 35°, and the coefficient

Fig. 18.



Mr. Beresford.

Mr. Beresford. of friction between the sand and the brickwork at 0·7; then $\frac{1 - \sin \phi}{1 + \sin \phi} = 0 \cdot 271$; and the conjugate pressure on 1 foot of circumference would be $70 \times \frac{55^2}{2} \times 0 \cdot 271 = 286{,}921$ lbs., or 12·81 tons. The circumference being 63 feet, the total frictional resistance would be $12 \cdot 81 \times 0 \cdot 7 \times 63 = 565$ tons, as against 505 tons observed. This result showed that the angle of internal friction cannot have exceeded 35°, for a coefficient of 0·7 was not low for the other friction. It was a remarkable fact in connection with the subject of the Paper, that notwithstanding the large horizontal conjugate pressure near the bottom of a deep well supported by lateral friction in super-saturated sand or dry sand, a crater could be dredged out below the curb, the sand sloping down from the outer edge of the curb at the natural angle of repose, and not gushing up the well to the height of some feet, as might be expected on the theory of Rankine's formula for foundations. The wells tested for friction were then hearted with concrete, which was allowed to stand for a month to set, after which there was put on them a load, amounting in some cases to 2,800 tons. The subsidence was only about 1 inch. He thought it was appropriate to mention these tests, which were on a very large scale. He was reluctant to criticize the Paper, but it struck him that, having regard to some of the observations he had made, the Author's results would not serve for the design of works. He considered that unexpected results attributed to high angles of internal friction were due to other causes. The wells he had referred to were 58 feet deep, of which 55 feet were in water-logged sand; hence, in calculating the lateral pressure, deduction had to be made for the buoyancy of the water. He assumed 30 per cent. of interstices in the sand, and allowed for 70 per cent. of solid matter weighing 162 lbs. per cubic foot in air, but only 100 lbs. in water, thus reducing the effective weight of the sand to about 70 lbs. That was a deduction which was not usually made. It was made on the structure which was submerged, not on the surrounding granular material.

Dr. Brightmore. Dr. A. W. Brightmore remarked that the Author had been good enough to thank him for advice when carrying out these experiments. The only advice he remembered giving was that the experiments should be continued, as he found them so instructive. The Author had rather upbraided himself for lack of faith in the theory of the subject, but Dr. Brightmore thought

Dr. Brightmore.

that the way in which the Author had attacked this question, namely, by setting out to calculate the angles of friction by measuring penetrations, put the theory to a very crucial test, because the process necessary to obtain the lateral pressure from the load had to be repeated a second time to find the penetrations. The measurements of penetrations gave very definite results, which, he considered, was their most valuable property. So far as dry and damp sand was concerned, the Author's confidence had been amply justified, because the experiments showed that the angle of internal friction had proved to be constant for any particular sand in a given state of aggregation. But in regard to sand containing larger percentages of water, and also clay, the Author's results confirmed the conclusion that Rankine's formula did not hold good in such cases. It was interesting to inquire exactly where Rankine's formula broke down. It would be recalled that Rankine first wrote down an expression for the maximum angle, θ, which the resultant stress on a plane at a point made with the normal to that plane, and that expression was:—

$$\sin \theta = \frac{p_x - p_y}{p_x + p_y},$$

or

$$\frac{p_y}{p_x} = \frac{1 - \sin \theta}{1 + \sin \theta} = k, \text{ say},$$

and

$$d = p_x \left(\frac{1 - \sin \theta}{1 + \sin \theta}\right)^2 = p_x \times k^2$$

where p_x and p_y were the greater and less principal stresses at the point, and d was the pressure due to the depth of penetration measured in the same unit as p_x. These expressions held for any material. Rankine then introduced the experimental fact that the internal friction on a plane for loose granular material was proportional to the normal pressure on it; thus, in the limiting condition for stability for such materials, ϕ, the angle of internal friction, might be substituted for θ in the above formulas. In the case of clay this simple relation did not hold, but the curves in *Fig. 10* showed that there was, in the case of clay, a corresponding experimental fact which might be introduced to find an equation to apply in that case. If the pressure on the plunger in the experiments were measured in terms of the weight of a cubic foot of the material experimented on, and were denoted by b, and the horizontal pressure under the plunger measured in the same unit were denoted by c, it followed that $\frac{c}{b} = k'$, and $\frac{d}{c} = k''$, where d represented the

Dr. Brightmore. pressure due to the depth of penetration in the same unit, and k' and k'' were the respective values of k in the above formula. Multiplying these two results together gave $\frac{d}{b} = k'k''$. Taking the values of $\sqrt{\frac{d}{b}}$ from *Fig. 10* and plotting them as ordinates, and the corresponding pressures b as abscissae, it was found that the resulting points lay on a straight line for each type of clay experimented on. These straight lines were not horizontal as in the case of dry or damp sand, but inclined to the axes. If the equations to be straight lines were written $y = \alpha + \beta x$, or

$$\sqrt{\frac{d}{b}} = \alpha + \beta b,$$

the values of α and β, determined by plotting the results as just described, were as follows:—

Experiment No	α	β
65	0·015	$\frac{1}{700}$
68	0·014	$\frac{1}{1950}$
67	0·0175	$\frac{1}{2680}$
66	0·016	$\frac{1}{4140}$

From the above it followed that the equations to the experimental curves in *Fig. 10* were of the form

$$d = b\,(\alpha + \beta b)^2.$$

Working out the results from this formula and comparing them with the experimental results in *Fig. 10*, the figures given in the Table (p. 175) were obtained.

The limiting value of the above equation would be when $\alpha + \beta b = \pm 1$. In the case of dry or damp sand the equation reduced to $d = \alpha^2 b$, and α then equalled k. It would be noted that the calculated results agreed closely with the results of the experiments. He put forward that equation tentatively, as representing the results of the Author's experiments on clay. Further experiments might show that it was not of general application. So far as he could see, the Author's experiments relating to clay did not help in finding the lateral pressures on a wall. That seemed to him to

Dr. Brightmore.

Experiment No.	Penetration.	
	Calculated from above Equation.	Experimental. *Fig. 10.*
	Inches.	Inches.
65	1·2	1·2
65	6·1	6·1
68	1·2	1·2
68	3·0	2·9
68	6·2	6·6
67	1·5	1·6
67	4·3	4·0
67	7·2	7·2
66	1·1	1·2
66	3·5	3·2

be not of much importance, because the pressure on a wall differed from the pressure in a foundation, in that the foundation, generally speaking, was not subject to any surface or drainage variations, as often obtained in the case of material behind a wall. In the case of a retaining wall, it was not the pressure exerted by the clay in any particular condition which was wanted, but the maximum pressure which the clay might exert on the wall, due to any circumstance that might reasonably arise, for instance, cracks in dry weather, filled by water afterwards, which might cause slight slips in the material behind the wall. Therefore the angle of slip should be determined which would cause the greatest pressure to come on the wall. But allowance should be made for the friction on the back of the wall, which tended to increase its stability.

Mr. Wentworth-Sheilds.

Mr. F. E. Wentworth-Sheilds thought the Author was to be congratulated on having carried out a piece of research, which would—perhaps not immediately, but some day—be exceedingly useful to the engineer who had to design large retaining-walls. It would appear to give the supporting power of various materials under various circumstances. It was exceedingly interesting in this respect—that it brought to engineers' attention Rankine's theory, often neglected, that the supporting power of an earthy material varied with the depth at which the foundation was placed. Engineers commonly assumed that clay or sand would bear a

Mr. Went-worth-Sheilds. certain load per square foot without regard to the depth below the surface at which the load was imposed. The various experiments had borne out Rankine's theory and brought this fact into prominence. He did not know how far that theory could be pushed. For instance, *Fig. 4* of the Paper, in which the first experiments on Leighton Buzzard sand were given, showed that the sand would bear about $3\frac{1}{2}$ tons per square foot with a penetration of only 4 inches. Presumably, if Rankine's law held good, with a penetration of a foot that sand would bear 10 tons to the square foot, and with a penetration of 10 feet it would bear 100 tons. He imagined that if 100 tons per square foot were put on Leighton Buzzard sand, something would happen. At all events, these experiments and the discussion would call attention to a fact which the designers of retaining-walls often lost sight of, namely, that it was really not so important to design a retaining-wall which would not overturn, as it was to design one which would not slide forward. In nine text-books out of ten, he thought he might say, the reader was warned to design a wall so that the resultant of the lateral pressure of the backing and of the weight of the wall itself should fall within the middle third of the base, with the view to ensure that the wall should not overturn. Sir Benjamin Baker recognized long ago, and others who had had to do with big walls, and especially with big wall-failures, realized that it was the rarest thing possible for such a wall to overturn; when a wall did overturn, it did so not forwards, about its toe, but about its heel, backwards. That being so, practical experience showed that walls rarely failed by crushing the material under their toe, and the careful calculations to avoid that class of failure were scarcely worth making. The failure which needed most to be guarded against was the sliding of the wall forward bodily; there engineers were badly in need of a great deal of research work and experiments to enable them to determine whether such failure was likely to take place. Research was wanted, in which the experiments under discussion would probably help, to enable engineers to determine the actual lateral pressure of the material, whatever it might be, against a wall; also to determine resistance of the material in front of the toe. Most of all, data were needed as to the friction between the wall and the material on which it rested, because, after all, that was the great thing on which one depended for the stability of any large retaining-wall. While saying that the Author's experiments called attention to the large weight which almost any substance would bear if the load were imposed at a considerable depth from the surface, he thought

Mr. Went-worth-Sheilds.

it was important to emphasize that the material must be confined, in other words, the material must be packed close round the wall or column, or whatever it might be, which was bearing the load, so that the particles of sand or clay underneath that wall should not be allowed to escape laterally. He had himself noticed, in his experience at Southampton, that a very large pressure could be put on a wall or a column in comparatively weak ground, provided the column or wall was built solid against the sides of the trench in which it was placed. It was a common practice among engineers, when building a wall in a timbered trench, to fill the trench with concrete, and then draw out the timber runners leaving the space they had occupied to be filled in by nature as best it might. If the wall had to bear considerable weight it was most important that no such void should be left, and if it were necessary to draw out the timber runners, then they should, if possible, be drawn before the concrete set, so that the concrete could pack itself tightly against the earth by which it was surrounded. If that were done, experience proved that a very large weight could be safely carried by the wall. This bore out what the Author had stated, namely, that the bearing power of earth did not depend on what was underneath the load, but on what surrounded the load; and the importance of packing the material around the wall or column which imposed that load became evident. He would like to point out, too, that these experiments brought out a slight imperfection in the interesting formula and calculations given by Mr. Bell, according to which a "stiff" clay should bear $3\frac{1}{2}$ tons per square foot without any penetration whatever from a loaded plunger. Under increments of load that plunger would produce penetrations following a straight-line law, i.e., the penetration would be proportional to the increment of load. The Author's experiments (*Fig. 10*) did not bear that out. All those clays showed slight penetration by the plunger, even with only a slight load upon them. That he regarded as important, as otherwise one might be tempted to place too much faith in Mr. Bell's formula. But it was interesting that the curve which the clay experiments showed, approximated somewhat to the horizontal line, meeting the sloping line which would be given by plotting results from Mr. Bell's formula. It was to be wished that the plunger experiments might be carried out on an even larger scale, using a longer plunger, and higher pressures. The difficulty with such an experiment would be that friction on the sides of the plunger would probably come into play. Engineers knew from pile-driving what a large amount of support was due to friction on the sides of

Mr. Wentworth-Sheilds. the pile. That friction would of course come into play if the Author's experiments were made on a very large scale. Still, he thought that as soon as the Research Council could be persuaded to give more money for investigations, some such research should be organized, and he hoped that Mr. Crosthwaite would have the management of it, because his experience would be very valuable. He would like to see any further researches which might be carried out directed towards finding out the actual lateral pressure of the material behind a wall, and the friction between the wall and the earth on which it rested. If those two items could be determined, the design of large walls would not be the haphazard work which, unfortunately, it now was.

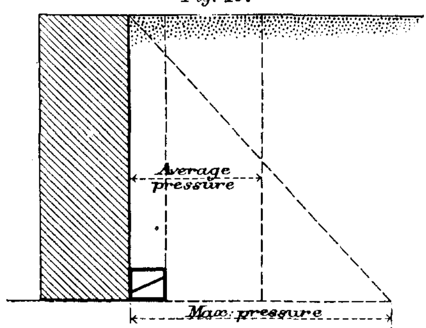
Mr. Cotterell. Mr. A. P. I. COTTERELL ventured to think that the Author might have started his experiments after an experience which he and Mr. Cotterell had had in connection with alluvial clays in South Wales. In that case the Author had occasion to question the safety of Rankine's formula, and pointed out that the formula might answer very well in the case of sands and granular material, but that it was altogether fallacious and dangerous when dealing—as in this instance—with a soft clay that could easily work up into a slurry. The banks of the River Usk, which ran alongside the particular work in question, took an angle of repose, even after being washed by a somewhat rapid current, of about 8°, and Mr. Cotterell thought that the angle of internal friction of the clay where it was not touched by tidal water would be at least as high as that, but upon this point the Author was doubtful if it could be taken for granted. Hence the need of experiments arose. The Author's experiments were so simple that almost anyone could carry them out; but of course the Author would be the first to admit that from the fact of their being on a limited scale, they might not quite tally with experiments on a larger scale. He would have liked to see the effect of the plunger upon a free area, rather than on the confined area afforded by a bucket 11 inches in diameter. That view seemed to him to be borne out by one or two remarks the Author made as to the arching effect that might take place in a limited area and help to resist the pressure of the plunger. In the case of the bucket, there was a possibility that such an effect would increase the apparent angle of friction. The same thing applied to the special experiment described on p. 145, in which the Author surrounded the plunger with small piles, and found that the penetration was considerably reduced. There it seemed clear that the limitation of the area caused by the circle of small piles introduced some factor other than the resisting power of the sand. But one of the most interesting parts of the Paper was, surely, that showing the difference in the behaviour of clay, as compared with sand. In *Fig. 11* the curve was seen to be completely altered. Instead of flattening out, as it did in the case of sand as the pressure grew greater, it became steeper, which recalled—if he remembered rightly—a similar discrepancy obtained by Mr. A. L. Bell. Mr. Bell had started with an initial stress which clay was capable of bearing as compared with sand, and then went on with an angle of repose which might be considerably less than was shown by these experiments. At all events, it suggested what the Author himself pointed to, namely, the existence of a factor of cohesion, which appeared to be broken down almost completely, according to the diagram, as the pressure became greater. On the other hand, it was satisfactory to find that the Author admitted the safety of Rankine's theory. Mr. Cotterell had used it for a number of years and had always found it safe to use; and after hearing this Paper he would have still greater confidence in it. In the case of bridge wing-walls, he remembered using an old rule of increasing the thickness of the retaining-wall by 1 foot for every 10 feet in depth, with an outside batter of 1 in 8. And he had never found that go wrong, though he believed it gave dimensions considerably less than were commonly used. Of course, much depended on the character of the material forming the backing. With regard to the depth of foundations and the Author's experiments upon them, it was very interesting to find that the plunger when put down a large hole made scarcely any additional impression when the depth at which it started was equivalent to the calculated retaining power of the particular material. As bearing this out, he remembered particularly a case of some concrete tanks that had to be founded in peat on a moor at a depth of 17 feet 3 inches, with a maximum pressure of 0·44 tons per square foot. The tanks were sunk as open cylinders, the men excavating inside them. When they had been completed to the required depth no further movement took place, although when the tanks were loaded with water and superstructure there was every reason to expect it. After a year or so no change took place in the level. He thought the experiments were reassuring, especially as the Author frankly said he had started with a bias, if anything, against the theory that Rankine had put forward. Mr. Cotterell.

Mr. W. F. Muckle remarked that on reading Rankine's theory it would appear that he had considered a particle of earth contained in a mass. The little cube in *Fig. 19* would represent the particle which Rankine described, and as that observer said, Mr. Muckle.

N 2

Mr. Muckle. it would be acted upon by the vertical column of earth above it, producing lateral pressure on either side of the cube, by the cube sliding along some diagonal plane. This lateral pressure was due to the weight of the column of earth above the particle. The weight of this column of earth should produce the pressure indicated by Rankine's law. It might be stated that this column alone should then produce the pressure. But Rankine specifically stated that the top surface was of unlimited extent, and therefore there were other columns behind the one under consideration. What he particularly wished to bring out was, that in designing walls engineers had been too prone to consider that the pressure was produced by a wedge sliding down some plane, generally called the plane of rupture, and that had given their minds a bias in the wrong direction. If, for a few moments, they confined their attention to the lateral pressure produced by a little cube at the base of the wall, corresponding with Rankine's particle, they would possibly arrive at a somewhat different understanding of the matter. That view was borne out to some extent by the Paper on retaining-walls by the late Sir Benjamin Baker. Describing some experiments made by Lieutenant Hope at Chatham, about 20 or 30 years earlier, Sir Benjamin said Lieutenant Hope placed a board behind the wall, so as to include the wedge of earth described in Coulomb's theory. What he tried to do was to produce the actual wedge of earth sliding down a free plane, such as a board placed at the required angle, and to see if that would produce the pressure; but he found "that the lateral pressure was quite as much when the board was at the angle of repose, $1\frac{1}{2}$ to 1, as when it was at half the angle. There was hardly any difference whether the board was horizontal or at a slope of $\frac{1}{2}$ to 1, or at any intermediate slope."[1] It would be agreed that this was in keeping with the idea he had described, of the cube at the base of the wall. In the case of a certain wall with which he

Fig. 19.



[1] Minutes of Proceedings Inst. C.E., vol. lxv, p. 208.

Mr. Muckle. was acquainted, on some dock works, when the trench for the wall had been excavated nearly to the bottom, it was found that the cross-strutting of the timbering, which was massive work very well put together—12-inch by 12-inch cross struts fixed at fairly close intervals—was bent 3 or 4 inches out of the straight, which seemed to be unique. The reason was afterwards found to be that the wall had been excavated for on the site of an old creek. Examination of the foreshore in the neighbourhood of the creek showed that the mud round the district lay at a natural slope of about 7 to 1. According to Rankine, the pressure would be found to be intense, and it accounted for the bending of the timbers. This was another point in favour of considering a cube, and not a wedge sliding down a slope. Supposing the whole of the material at the back of the wall, down to within a couple of feet of the bottom, were fairly good, and right at the bottom there was a small stratum of weak material, having a very small angle of repose, the effect would be to cause greater pressure than would be anticipated from borings showing the general nature of the strata. The Author's experiments proved, as far as experiments of such a nature could do, the truth of Rankine's theory, and he wished to say that when Rankine mentioned the angle of repose, it perhaps did not mean absolutely the angle of repose, and it was inculcated with some other factors which were in Sir Benjamin Baker's mind when he said the angle of repose was the physical constant of the material which would give the lateral pressure accurately, and in the same Paper Mr. Airy propounded the same idea. On p. 158 occurred the remark, "So far as the Author knows, no engineer takes Rankine's value for the thrust and multiplies it by, say, three or four for safety; and this means that he accepts, as a matter of fact, that Rankine's calculation provides a factor of safety." This seemed to Mr. Muckle to be hardly correct, because the factor of safety—namely, against overturning—was provided by keeping the resultant force a certain distance within the wall, and as long as the foundation was of a perfectly rigid material, and the wall was constructed of rigid material, it was a physical impossibility for the wall to overturn. The most important feature of the present Paper was that the Author had provided very simple and ingenious apparatus, which was capable of producing good practical results. For instance, if one sank a trial hole at the back of the position of a proposed quay wall, one could descend to the bottom of it and measure accurately, with this apparatus, the resistance of the material to sliding and cohesion. Consequently it would be possible to state exactly the angle ϕ required in Rankine's expression

Mr. Muckle. to give the lateral thrust, the material being actually experimented on as it lay bare *in situ*. Also, after digging the trench and before putting in the wall, one could probe along the bottom with the apparatus, and thus find what the angle of repose should be: and if the design required amendment, it could be done before actually carrying out the work. In clay, however, the apparatus would not help, and the only thing to be said, was that although, as regards materials of granular nature and with a small amount of cohesion satisfactory results could be obtained, yet in clay there was a factor of greater cohesion which made it very difficult to deal with. In passing along railways, he had noticed the curious way in which the material had sunk in at the back where a slip had occurred. It seemed as if the piece which was breaking away was rotating about an axis, and behaving somewhat in the manner of a semi-fluid, such as treacle. If the Author would continue his experiments with the object of finding the cohesive property of clay alone, without the friction, then he could combine it with friction and so possibly arrive at figures for clay.

Dr. Lapworth. Dr. HERBERT LAPWORTH said that the Author, who was prevented by illness from being present, had asked him to lay before the Institution two points which were not included in the Paper. The discrepancy between the measured angles of repose, as obtained by the Author, and the angle of internal friction as calculated from his experiments, had led him to seek some reason for this; and it had occurred to him to test whether there might not be some relationship between this angle of internal friction and the angle of rupture, the plane of rupture bisecting the angle contained between the angle of repose and the vertical. Table X (p. 183) showed how the angle of rupture derived from the measured angle of repose compared with the maximum angle of internal friction obtained from the plunger experiments. It would be seen that the two agreed very closely, the average error being $0° 51'$. The Author therefore suggested that in applying Rankine's formulas to sands, it would seem that the angle of rupture should be used in place of the angle of repose. Dr. Lapworth had examined the Author's results for clay, in order to see what the law was, and had tried several kinds of curves. He had found that the simple parabola gave the best results. Whether it was the true law, he was unable to say, but it was so near that one could not say it was wrong. He would like to add an expression of his admiration for the Author's work, which was extremely valuable: not so much perhaps for the actual figures, as for the establishment of laws. That there were

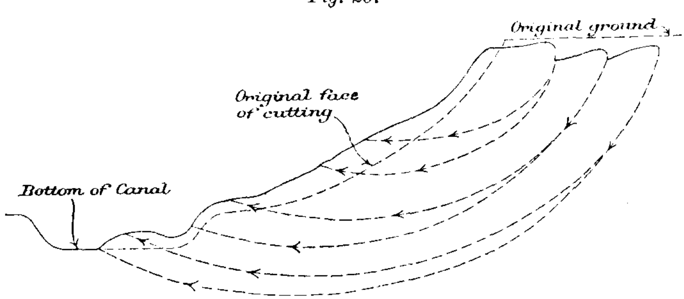
Dr. Lapworth.

TABLE X.

Material.	Angle of Repose.		Angle of Rupture.		Maximum Value of ϕ.	
	°	′	°	′	°	′
Leighton Buzzard sand—						
Damp	52	28	71	14	67	48
Dry	43	4	66	32	64	47
Bournemouth sand	47	21	68	40	65	32
Garden earth	46	12	68	6	60	51
Ashes and cinders—						
Through $\frac{1}{8}$-inch sieve	53	48	71	54	72	59
Through 30 × 30 mesh sieve	47	30	68	45	68	8
Mean			69	12	68	21
Difference			0° 51′			

definite laws here he thought was clearly brought out. The results for sand were perfectly consistent throughout, and for clay throughout, and they seemed to harmonize with results which had been obtained on public works. Finally, one point which had interested

Fig. 20.



him very much was the Author's suggestion that if this parabolic law were true for clay, there must be a height or depth at which clay would refuse to stand, and the Author had instanced the case of the subsidences in the Panama Canal. But he did not point out

Dr. Lapworth. what Dr. Lapworth considered to be one of the most interesting points about the Panama Canal movements, namely, that one of the serious troubles there was not merely slips of the ordinary kind, but movements such as those indicated by the sketch (*Fig. 20*), in which masses of material cracked, slid on the planes of rupture, and bulged up the bottom of the canal, showing, as the Author had stated, that the depth of the cutting was greater than the material could stand.

The Author. The AUTHOR, before replying to the discussion, desired to express his thanks for the kind and considerate manner in which the Paper had been received. He agreed with Mr. Beresford, that in the case of the wooden wall supporting road-metal, quoted from Sir Benjamin Baker's Paper, the friction between the filling and the back of the wall should be taken into account, and that should apply to all experiments with model walls, unless this friction can be eliminated as in Darwin's experiments. The Author, however, did not agree that the friction on the back of the wall would increase the pressure on the foundations at the toe. The friction was a passive force that resisted motion of the back of the wall upwards, but not an active force tending to press the wall downwards; and if it did not do this it could not increase the pressure on the foundations. Neither could he accept Mr. Beresford's statement that:—"According to Rankine, when the surface of a mass of granular material is horizontal, and the weight of the unit volume w, then the congugate pressure at the depth x cannot be less than $w\,x\,\frac{1-\sin\phi}{1+\sin\phi}$, or greater than $w\,x\,\frac{1+\sin\phi}{1-\sin\phi}$." So far as the Author understood Rankine, he said that the conjugate pressure at the depth x *is* $w\,x\,\frac{1-\sin\phi}{1+\sin\phi}$. The conjugate pressure could not possibly be $w\,x\,\frac{1+\sin\phi}{1-\sin\phi}$; for were it so the horizontal pressure would be greater than the vertical pressure for all values of ϕ greater than 0°; which for $\phi = 90^\circ$ would be infinite, obviously an impossible result. Referring to Sir George Darwin's experiments, Mr. Beresford had pointed out that these showed that when the sand was shaken the density was increased by 10 per cent., and the coefficient of friction was greater. Commenting on this, Mr. Beresford assumed that "the shaking down increased the conjugate pressure on the two side walls, and that was the reason Sir George Darwin got such low results." This conclusion did not seem to be correct, for if the shaking increased the coefficient of friction it

The Author.

decreased $\frac{1 - \sin \phi}{1 + \sin \phi}$, and therefore correspondingly decreased the conjugate pressure. Also, if it had increased the conjugate pressure, Darwin should have got high, and not low results, as he was measuring the pressure. As regards the objection that experiments with the Author's small plunger were not strictly comparable with what occurred in front of a long wall, for in the case of a small plunger the pressure rapidly diminished with the distance from the plunger, while in front of a wall it did not diminish so quickly. This might be the case; but it was difficult to say how the horizontal pressure in front of a wall varied with the distance from the face. The Author would however point out that the size of the plunger did not seem to alter the character of the results, for the penetration curves for his experiments on clay were practically the same as those of Messrs. Coode, Matthews, Fitzmaurice and Wilson, made on plungers 110 and 1,730 times the size of that employed by the Author. Mr. Beresford had expressed a doubt whether if, in the Author's experiments, the question of conjugate pressure came in at all, and suggested that the whole of the small depression might be due to the condensation of the material. The Author understood Mr. Beresford to mean that the penetration curves were uniform because the load was carried by the compressed material below the plunger. It appeared however to him that this conclusion was completely negatived by the experiments shown in *Fig. 14*. If the plunger was carried only by the underlying material the penetration should be the same whether the loading was commenced with the plunger resting on the surface or at the bottom of a hole, but inspection of the penetration curves showed that when the loading was commenced below the safe depth there was practically no further penetration, and consequently no consolidation of the material below. Whilst appearing to accept Rankine's theory he could not understand why Mr. Beresford should suggest that the penetration should vary as the square of the depth, or as the volume of the cone of resisting earth, that is, as the cube of the depth. If this conclusion were true Rankine's whole theory would collapse, for Rankine proved that the penetration must vary directly as the depth. In fact, it was to test the correctness of this conclusion that the Author's investigation was carried out. Mr. Beresford had made experiments similar to those of the Author, but had given them up, as he found that when the plunger had come to rest under the applied

The Author. load, and was then gently tapped, it went down considerably further. Mr. Beresford had, however, answered this objection in his earlier remarks when he said, "The objection to laying too much stress upon friction was that vibration might spoil the effect." The object of the experiments was to measure this friction, which Mr. Beresford had broken down by tapping and spoiled his results. It was obvious that in sand experiments vibration should be avoided as much as possible, for once the sand was got in motion, it was difficult to see how any static theory could apply. Mr. Beresford's experiments on sinking of cylinders in India were of considerable interest. He had drawn attention to the fact that notwithstanding the horizontal pressure, a large excavation could be made below the bottom of the cylinder, as in *Fig. 18*, the sides of which would stand at the angle of repose, not gushing up into the cylinder as might be expected from Rankine's theory. This was easily explained by the formation of an inverted arch or groyne under the cylinder, just as in the Author's experiment shown in *Fig. 15*, the sand was not forced out through the hole in the bottom of the box.

The analyses of the clay curves made by Dr. Brightmore were most interesting, for he had devised an equation that covered both the cases of sand and clay. They pointed to the probability of there being some distinct physical property characteristic of each, one of which was wanting in coarsely granular materials, and it would be of great interest to determine what this property peculiar to clay is. The Author was in agreement with Dr. Brightmore that the experiments did not assist in determining the lateral pressure against walls, for they negatived all existing earth-pressure theories as regards clay. The example mentioned by Dr. Brightmore of wedges of dry clay slipping along cracks filled with water and so exerting pressure against a wall seemed to the Author to be outside the domain of ordinary earth-pressures, for the wedge of earth might be regarded as a solid body sliding down the inclined plane formed by the crack. The coefficient of friction between the clay surfaces thoroughly lubricated by water would probably be altogether different from the internal coefficient of friction of the mass, and therefore no theory based on this internal friction could apply. The conditions approximated more to the case of clays interstratified with beds of sand or gypsum quoted in the Paper. It seemed very probable that such conditions might produce pressures enormously greater than would be calculated from any earth theory. The problem seemed to be indeterminate unless the coefficient of friction between the lubricated clays, which will vary with the extent of lubrication, were ascertained. Such cases seemed to the Author to show the necessity for designing walls that had to support clay with a large factor of safety. Should a wall designed according to Rankine fail in such circumstances his theory would be abused, while in reality it was never intended for application under such conditions. The Author.

The Author was unable to answer the question put by Mr. Wentworth-Sheilds as to what would happen if sand was loaded to 100 tons per square foot, but he saw no reason why the straight-line law should not be followed until the quartz grains began to crush or flow under the pressure. Short of this there seemed to be no reason why anything should happen. He would very much like to see his experiments carried further, and it would be an easy matter for anyone carrying out works to do so. The Author thought Mr. Wentworth-Sheild's interpretation of the text-book reference to the inner third of the base was not quite correct. The idea of confining the resultant to the inner third of the base was to ensure that there should be no tension in the masonry at the back of the wall, and not to provide against overturning, though of course incidentally it provided a factor of safety against overturning. If it were desired that there should be no tension, as should be the case in a block wall laid without cement joints, and the wall were designed so that the resultant came just to $\frac{1}{3}$ of the thickness from the outer edge, then there would be no factor of safety against tension, for the least increase pressure would drive the resultant outside the middle third, and there would be tension if the stresses in the masonry followed a straight line law. The Author agreed that the question of walls sliding forward was most important and that investigations in this direction were very desirable. As regards the failure of walls by rotating round the heel, it appeared to the Author that rotation round the heel could only come after the toe had slid forward, for the backing would prevent it earlier. With reference to a very natural suggestion, if the experiments were carried to greater pressures and depths friction on the plunger might vitiate the results, that might be the case with very wet clays and very dry sands, but with sound clays and damp sands it was found that friction did not come in. The material escaping from under the plunger carried with it the superincumbent material, leaving an annular ring round the plunger. This must have ensured that in these experiments at least friction did not affect the result.

In the case of pile-driving, it was difficult to say how much of the blow was absorbed by friction. It must be remembered that when a pile was being driven an enormous mass of sand must

The Author. be put in motion by each blow, and this must absorb a large part of the energy of the falling ram. It was generally assumed that the bearing capacity of a pile was due to the friction on its sides, but as Mr. Wentworth-Sheilds had pointed out, if the straight-line law held good to high pressures, the bearing capacity of a pile driven to a depth of 20 feet or 30 feet must be enormous, apart altogether from the side friction. This could easily be tested by sinking a tube, say, 10 feet into the ground, and driving a pile inside it, after the tube had been cleaned out. The number of blows required to drive it, say, an additional foot could then be compared with that required to drive a similar pile, that had been driven from the surface, the same distance at the same depth. The difference in the number of blows should indicate what energy had been absorbed by friction of 10 feet of the pile in the second experiment.

It was correct that the experiments were the outcome of a case the Author had to investigate with Mr. Cotterell in South Wales, and the Author thought they amply justified the fears he then entertained that Rankine's theories would not apply to clays that were little better than slurry. As regards the small size of the experiments and the objection that arching might interfere with the results, the Author thought they showed that where the penetrations were kept within the limits prescribed at the beginning of the Paper, the sides of the bucket did not affect the result. With regard to the pile experiments, it was only when the piles were driven in a circle of 4 inches diameter that the bearing capacity of the sand was materially increased. The nearer the piles were to the plunger, and the more numerous, the more completely was the sand enclosed and the greater the bearing capacity. If sand were enclosed and thoroughly rammed into a cast-iron cylinder it would carry almost any weight without settlement.

Reference had been made by Mr. Muckle to Lieutenant Hope's experiments, when he tried to produce an actual wedge of earth by placing boards at the required angle to carry the wall backing. It appeared to the Author that unless the boards were at a greater slope than the angle of friction between them and the backing, they could not affect the resulting pressure against the wall, for at a flatter angle the earth would not slip on them. Dr. Lapworth had referred to the coincidence that in granular material tested by the Author the angle of rupture was practically identical, except in the case of garden earth, with the maximum values of ϕ obtained from the materials when thoroughly consolidated.

In the text-books it was stated that when a wall suddenly failed the backing first slipped along a plane, called the plane of rupture, much steeper than the angle of repose, and that the backing, when left to itself, under the action of the weather, would in time assume the true angle of repose. Now it seemed to the Author that anyone seeing such a thing occur, and having no preconceived notions about what the angle of repose should be, would have at once said that the angle of the slip was the angle of repose. The Author thought his experiments showed that this was the true explanation, namely that slipping took place along a plane inclined to the horizontal at the angle of internal friction, or the angle of repose for the backing in its consolidated state. This explanation, however, did not fit in with the accepted angle of repose, and the wedge theory had to be invented to bring the two into line, introducing the plane of rupture, which appeared to the Author to be a mere fiction that would never have been heard of had people always appreciated the distinction between the angle of repose and the angle of internal friction, and the very large difference there might be between the two. The Author.

Correspondence.

Mr. A. L. Bell observed that the method which the Author had followed was to load a vertical plunger resting upon the surface of the material to be tested, to note the depth of penetration under varying loads, and then, from Rankine's well-known formula for the maximum supporting power of dry granular material, to determine values of ϕ appropriate to the observed results. This method was excellent so long as experiments were limited to the dry granular material postulated by Rankine; but, in Mr. Bell's opinion, it would not lead to correct conclusions if applied to cohesive material which did not obey the primary law of resistance to shear which formed the basis of Rankine's theory. Moisture in sand caused the grains to adhere to each other, and this had the effect of increasing the supporting power at or near the surface. Anyone who had walked on a hot summer day upon a sandy beach at the time of ebb, could not have failed to notice how much more pleasant it was to walk upon the firm damp sand which had been covered by the tide than upon the dry sand above high-water mark. Rankine's formula (No. 4 in the Paper) correctly showed that the dry sand above high-water mark had no supporting power at the surface, but it was evidently inapplicable to the damp sand, Mr. Bell.