Correspondence.

Mr. Bell. Mr. A. L. Bell desired to congratulate the Authors upon their experiments, their careful analysis of the results obtained, and the conclusions which they had placed before The Institution. Mr. Crosthwaite had made all his experiments with dry sand. While this was a material with which engineers seldom had to deal in practice, it was the only natural material to which Rankine's theory was properly applicable and, as all the formulas which were subjected to examination were identical (with one exception, namely, Darwin's for a surcharged wall), presumably the same limitation applied to all. The diagrams showed clearly that, if these formulas were used, it was necessary, in order to reconcile the results of calculations with the experimental results, to assign to ϕ a value greater than the angle of repose; but an assurance would be very desirable that the sand was perfectly dry throughout all the tests. The effect of moisture had been very strikingly brought out by the late Mr. G. Wilson's experiments on sand published in the Proceedings.[1] Those experiments (of which the results were given below) showed that moisture in sand caused marked variation in the ratio of the intensities of the conjugate pressures:—

	Ratio.
Dry sand	0·319
Sand with 6 per cent. of water (by weight)	0·221
„ 12 „ „ „	0·212
„ 17 „ „ „	0·280

The minimum value of the ratio was found when the sand contained about 9 per cent. of moisture. Attention to the complete elimination of moisture was specially important when experiments were on a small scale, for the effect upon the results caused by cohesion due to moisture would be greater than when the experiments were on a larger scale. He might make his meaning clearer by pointing out that sand, with a horizontal surface and very slightly moist, would stand in a vertical cliff of small height :—

$$h_0 = \frac{2k}{w} \cot\left(\frac{\pi}{4} - \frac{\alpha}{2}\right),$$

the symbols being those used in his Paper on "The Lateral Pressure and Resistance of Clay."[2] Such a condition would clearly vitiate

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

[2] *Ibid.*, vol. cxcix, p. 233.

experiments upon a small scale more seriously than those upon a large scale, the same material being used in both cases. Mr. Crosthwaite had indicated that the sand was dry, but had he taken any special precautions to ensure this? It was very necessary to do so. If there was any cohesion due to moisture, the resultant pressure would act, not at two-thirds of the depth, but at a somewhat lower point; also the intensity of pressure at any depth h would be:— Mr. Bell.

$$wh \tan^2\left(\frac{\pi}{4} - \frac{\alpha}{2}\right) - 2k \tan\left(\frac{\pi}{4} - \frac{\alpha}{2}\right),$$

an intensity less than that given by Rankine's formula, provided k had a real value. If there was no cohesion k would be zero, and the formula became identical with Rankine's. He was of opinion that the method of experiment illustrated in *Fig. 6* of Mr. Crosthwaite's Paper was admirable and, with perfectly dry sand, should give results of great value. He did not think the apparatus would yield satisfactory results with clay, which had tensile strength and could stand with an unsupported vertical face. That would be true even if the experiments were carried out on a much larger scale. So far as clay was concerned he agreed with Mr. Crosthwaite's view that no real advance would be made so long as calculations for that material were based upon an angle of repose. Clay had no ascertainable angle of repose. The theories quoted by Mr. Crosthwaite did not apply to it, and it was impossible to find an angle which, when inserted in the formulas, would yield true results. True results could not be arrived at in the case of clay if cohesion were left out of consideration. He did not concur with the conclusion that the plane of rupture was merely a convenient mathematical fiction and that it had no existence in granular material. The conclusion which he had drawn from his own experiments was that granular material did shear upon the plane bisecting the angle between the vertical and the surface of repose and, unless his memory was at fault, that had been confirmed by experiments made with superimposed layers of sand of different colours. Although the greatest tendency to shear was upon that plane, shearing would take place simultaneously upon a number of other planes radiating from the hinge when the door of the experimental box (Darwin's) was allowed to fall forward, and the released sand would immediately fall to the slope of repose or to a flatter angle. No question of weathering entered into the matter when dry sand was used. What he had said above in regard to cohesion applied also to Dr. Fulton's experiments, for the sand was damp, and garden soil had, no doubt, considerable cohesive strength. He had himself succeeded in

Mr. Bell. obtaining a vertical face about 8 feet high in slightly moist sand. He felt sure that the resultant in that case did not act at two-thirds of the depth.

Dr. Brightmore. Dr. A. W. BRIGHTMORE first wished to thank Mr. Crosthwaite for his magnanimous reference to what little help he might have rendered in connection with the Paper. He considered that it was rather a slur on the profession of Civil Engineering that even at the present day there were so few reliable experimental data for the design of so common a structure as a retaining-wall. It had been suspected by some investigators that, in connection with earth-pressures, it was the internal friction which was the important consideration and not the angle of repose; but in the absence of satisfactory experimental data on the internal friction under the actual conditions obtaining, the angle of repose had often been used in default of reliable data on internal friction. The result had been that the pressures as found, e.g., by Rankine's formula, had given too high results for the pressures on walls. To correct this it had been assumed that the friction at the back of the wall was an active force, which would then, of course, tend to increase the stability of the wall and reduce the effect of the calculated pressure on it. This acted very well when the surface of the earth pressing against the wall was level with the top of the wall, or when the wall was subjected only to the pressure of earth sloping away from it, that was, when the wall was negatively surcharged. Mr. Crosthwaite's experiments showed that, taking the angle of repose instead of the angle of internal friction, and taking friction on the back of the wall into account, the calculated results agreed remarkably well with the experimental values under the two conditions named. It was not until positively surcharged walls were experimented upon that it became evident that friction on the back of the wall was not sufficient to bring the calculations on the assumption referred to into accord with the results of experiment in the case of sand.

This led Mr. Crosthwaite to suspect that friction at the back of the wall was not an active but a passive force; and on this assumption, and using the angle of internal friction that would fit in with the experimental results with a level surface of sand, he found that the calculated values of the pressures on the wall agreed with the experimental results still more closely than with the old assumption. This was further confirmed by the experiments made by Mr. Crosthwaite on surcharged walls. Taking the same values for the internal friction, as in the case of a level surface of sand, and neglecting friction on the back of the wall, he found a wonderful agreement between the pressures so calculated and the experimental results. To test the assumption of the absence of friction at the back of the wall, Mr. Crosthwaite had measured, by a very elegant device, the pressures at a vertical plane in the body of the sand, both with a level and a sloping surface. Dr. Brightmore.

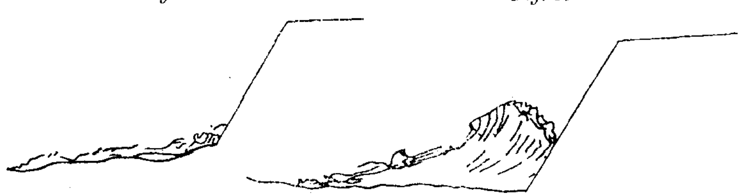
Comparing these with the measured pressures on the wall, the results agreed within 14 per cent., the results being slightly greater in the case of sand against sand than sand against the wall. This slight difference was probably due to the tensile strength due to the interlocking of the grains of sand in the former case. As there could be no friction on a vertical plane, at all events with a level surface of sand, this appeared to support the assumption that friction at the back of the wall did not come in; which was another way of saying that the line of action of the pressure at the back of the wall on a vertical plane was practically horizontal. Taking the case of a surcharged wall, if a plane were taken parallel to the surcharge, at any point in that plane there was acting there the weight of the sand above the point; but this was not the only component of the stress at the point. For the back of the wall exerted a force on the sand above the imaginary plane parallel to the surface of surcharge, which counteracted the tendency of the sand above the imaginary plane to slide, and this force was resisted by an opposite stress exerted by the sand.

Thus the stress exerted by the sand was the resultant of the weight above it and the stress it exerted parallel to the surface towards the wall. The resultant of those two component stresses would be a principal stress, inclined to the back of the wall, and that being the line of action of the greater principal stress at the point, it would also represent the trace of the plane on which the smaller principal stress acted. The direction of that smaller principal stress would, therefore, be inclined downwards somewhat below the horizontal; consequently the stress on a vertical plane at the back of the wall would not be far from the horizontal. In conclusion Dr. Brightmore would like to express his admiration for the experimental method adopted by Mr. Crosthwaite of placing his wall on rollers, by which means he was enabled accurately to counterbalance such small resistances that came in. It was a method which could be rendered applicable to experiments on a large scale and with any material, and would enable the angle of internal friction to be ascertained under the actual conditions that the material would be subjected to, which was a very material point.

Mr. JOHN C. FERGUSSON pointed out that, when the angle of repose of loose gravel or sand, standing at an angle of 31 to 35 degrees was Mr. Fergusson.

Mr. Fergusson. compared with the angle of repose of the same gravel or sand in situ, standing at 80 to 90 degrees, it would be noticed that the conditions of pressure and stability of the loose disintegrated materials and of the same materials standing in a bank were as unequal as their separate angles of repose. Anyone might observe, in gravel-pits, the faces of old workings which had stood for years nearly perpendicular and which would remain standing in the same position, until they were disturbed, or weathered by frost and rain; their stability depended on the adhesion of the materials caused by settlement and concentration during past ages, since they had been water-borne and deposited in their present bed. That important factor of adhesion in gravel and in other materials, when reposing in their natural bed, seemed to have been overlooked or disregarded by professors of Civil Engineering. The text-books gave the engineer, for working out the required strength of retaining-walls, a formula which depended solely

Fig. 2. *Fig. 3.*

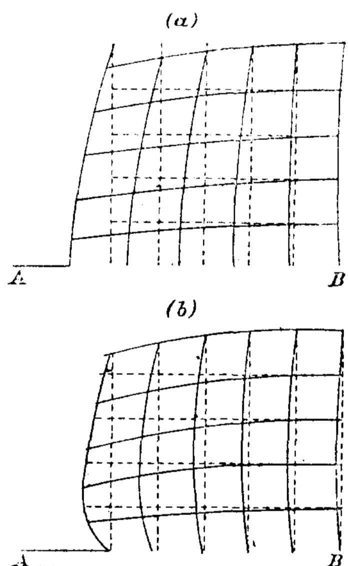


on the angle of repose of the loose material—a factor of small importance to an engineer who was called upon to erect a wall to support or guard the face of sound material in situ. As the foregoing statement might be challenged, he would explain that he had found in practice that it was a labour to break down a high face of gravel, whether he hydrauliced it with "giants," as was the practice in hydraulic gold-mining, or blasted it with explosives, or broke it down from the top. These remarks referred to ordinary gravel and not to cemented gravels. It was important to note that when a bank of gravel fell or "caved," the line of fracture between the fallen mass and the standing bank was nearly vertical; it certainly did not lie parallel to the angle of repose of the disintegrated material. *Fig. 2* showed the floor level of a gravel pit after it has been "cleaned up." *Fig. 3* showed the bank in situ, after a "cave" had taken place. The fallen mass would assume diverse shapes, but it would invariably show that it had dropped vertically and there would be little indication of a lateral or horizontal thrust, excepting when it had slid down along a steep bed-rock.

The plan adopted by Dr. Fulton of using the door of the box as a lever, and the necessity of making a reduction of the readings on the extensometer with calculations and diagrams, instead of reading the pressures direct from a scale, presented great difficulties to an engineer and would cause him much uncertainty. The pressure applied in tamping the material when the box was full would not be felt through the full depth of the gravel, at the bottom of the door, although it might be magnified at the top of the door and show an erroneous reading on the scale of the extensometer, because the door was being used as a lever. Mr. Fergusson.

Mr. A. MALLOCK noted that the Authors compared their experimental results with the deductions from certain theories which themselves were founded, not on a dynamic solution of the real problem, involving, as it did, the elasticity and viscosity of the material, as well as the coefficient of friction, but on assumptions from which the former properties were excluded. Dr. Thomas Young had once remarked that it was often easier to give a correct solution than to show in detail the mistakes in intricate work which led to wrong conclusions; and the subject of earth-pressure on retaining-walls was a case in point. If the boxes which the Authors used were really rigid (in the text-book sense) and the end flap or door did not alter its position, the total horizontal thrust would be $wh^2/2$, just as it would be if the filling material were a fluid of the same density; and the centre of pressure would be at one-third of the height. If the door yielded very slightly, the material, in virtue of the pressure due to its own weight, would descend vertically and expand laterally as an elastic solid. The horizontal thrust on the door would be thereby decreased by an amount to be measured by the elastic reaction against distortion. The problem was the same as the distributiou of stress and strain in a vertical cliff (*Figs. 4*). In most earthy material, and especially in such a Mr. Mallock.

Figs. 4.



(a) *No Friction along A.B.*
(b) *No Slipping along A.B.*

Mr. Mallock. material as dry sand, the elastic limits were extremely small, and if the motion of the door was sufficiently great to allow any slipping to take place, no further reduction in the latera lthrust would result. Hence (as Mr. Crosthwaite correctly stated) the horizontal force would be something between $wh^2/2$ and $c \times wh^2/2$ where c was less than unity. In the case of sand (and many other materials) any sort of stirring, vibration, rain, or inflow of water might rapidly change the value of c from its minimum (elastic limit just reached) to unity (practically no distortion). Mr. Crosthwaite's experimental determination of c was open to the objection that it involved a movement of the door exceeding the elastic limits of distortion. A better result might have been obtained by first determining the elastic limits, and then measuring the horizontal thrust required to keep plungers (placed in the side of the box at different levels) stationary within a small fraction of those limits.

Mr. Mitchell. Mr. JAMES MITCHELL wished to express his appreciation of the skill and labour involved in the experiments carried out by the Authors, and of the trouble they had taken to present their conclusions on a very difficult subject. Dealing first with Dr. Fulton's Paper, it was stated (p. 289) that, in measuring the angle of repose of the materials dealt with by him, "the value of μ was very nearly constant with varying pressures, and this was taken as an indication that cohesion might safely be neglected." On p. 303, however, it was stated that, with the 7-feet heights of surcharge, the observed horizontal pressure was less than that required by the wedge formula, whereas, with the 5-feet heights, the observed pressure was greater than that under the wedge formula, and it was added that this "might be explained by additional cohesion under the increased pressure of the greater loading." On p. 286 reference was made to the disturbing effect of arching in the backing, and on p. 303 it was stated that "Certainly any tendency to 'bridge' will become accentuated as the height of filling increases." Throughout the experiments, the centre of pressure was assumed to be at one-third of the height above the base, but the existence of arching, or of cohesion varying as the pressure, would affect the position of the centre of pressure—both probably tending to raise it. Arching would probably also exert an important influence on the direction of the resultant pressure. The results of the experiments were held by the Author to justify the conclusion that the position of the resultant pressure might, under certain conditions, "vary from 0·33 to 0·364 of the inner face of the wall, measured from its base." In the case of a wall 30 feet high, each unit in the latter coefficient represented $\frac{1}{8}$ inch, a degree of precision which was far from justified, considering the many uncertainties still involved in the problem. Mr. Mitchell.

Mr. Crosthwaite's Paper described (p. 254) an experiment made with the object of measuring the angle of rupture, and stated that that angle should be assumed by the material when the partition was suddenly removed. The true angle of rupture had, however, been completely masked by the wedge of material broken off and tumbled down the slope. On p. 275 it was suggested that the material under a wall was affected by pressure in a different way from the material of the backing, and that, in the former case, particles squeezed from under the wall forced their way between the particles of the surrounding material. It seemed much more reasonable to assume that, in yielding, the particles in a mass of material extending outwards from the foot of the wall to a distance depending on the nature and condition of the material and the amount and direction of the pressure, would be forced into a state of closer aggregation by the reduction of the spaces between them. With a compressible backing-material precisely the same effect would be produced, at different levels, by the weight of the superincumbent material, and the back of the wall must take its share of the thrust so produced. With regard to the reference (p. 280) to Sir Benjamin Baker's statement that, for loose earth backing, a wall was strong enough if designed to withstand a liquid pressure of 10 to 20 lbs. per cubic foot, it was probable that Sir Benjamin made the statement with a view, not of suggesting that the backing behaved as a liquid, but of emphasizing the difference between the horizontal pressure exerted by the backing, and that which would be exerted by a liquid having the same specific gravity. In any case, it did not appear likely that, in the present state of knowledge, the introduction of an equivalent liquid pressure would tend to simplify the problem. Referring to the Author's conclusions, if there was no such thing as a plane of rupture, it was difficult to understand what was meant by an angle of friction. The statement that the horizontal thrust exerted on a wall by the backing was due to the tendency of a wedge of the backing material to slip, and that, while the friction on one side of the wedge had an important influence on the amount of the thrust, that on the other side had none, did not appear reasonable. It could not be accepted as a statement of what occurred in practice, even had the curves in *Figs. 3, 4* and *5* of the Paper, representing the results of the experiments, and those representing the corresponding wedge theory without wall-friction, agreed more closely than they actually did.

Mr. Mitchell. With respect to both Papers, it was noteworthy that, while each experimenter attempted to measure the overturning effect of the backing, Dr. Fulton considered that his results corroborated the wedge theory, with an allowance for friction between the wall and the backing, whereas Mr. Crosthwaite held that the experiments made by him demonstrated the soundness of the wedge theory with no such allowance. The latter concluded (p. 252) from his loaded-plunger experiments "that walls designed to withstand a thrust from granular materials calculated on Rankine's theory would have a factor of safety ranging from 2½ to 4," while an examination of the figures given for Dr. Fulton's experiments showed that they indicated a much lower factor of safety. The differences between the conclusions arrived at by the two Authors showed how far the problem was from being solved. Their work, however, was calculated to expose some fallacies in the recognized methods of dealing with it, and so to clear the way for better methods. It ought to be clearly recognized that the angle of repose for most materials varied with their physical condition, as regards percentage of moisture, degree of aggregation, etc., and that the angle of repose, measured from a heap or embankment of material, was quite a different thing from that which existed in the same material when used as backing to a wall. The materials used for backing varied through every gradation, from hand-packed stone that exerted no horizontal pressure to soft clay or mud having the properties of a viscous liquid. It appeared probable that, while the theory of slipping wedges might be applicable to some of the materials at the former end of the scale, it was quite inapplicable to those at the other end. Observations of embankment slips indicated that, instead of being plane, the surface of rupture was frequently, if not generally, concave, which pointed to differences in the angle of friction at different depths; and in all probability a somewhat similar state of affairs existed in the backing of a wall.

Mr. Crosthwaite. Mr. CROSTHWAITE, in reply, stated that Mr. Bell might be satisfied that the sand used throughout his experiments was uniformly dry. With regard to the values for C found by the late Mr. G. Wilson, it should be remembered that they were obtained with pressure-gauges which apparently gave very unsatisfactory results with sand. This was referred to in Mr. Bell's Paper [1] in which it was stated that the results with sand were altogether anomalous and irregular. He quite agreed with Mr. Bell that his rolling-wall apparatus might not give satisfactory results with clay, and even with damp sand the

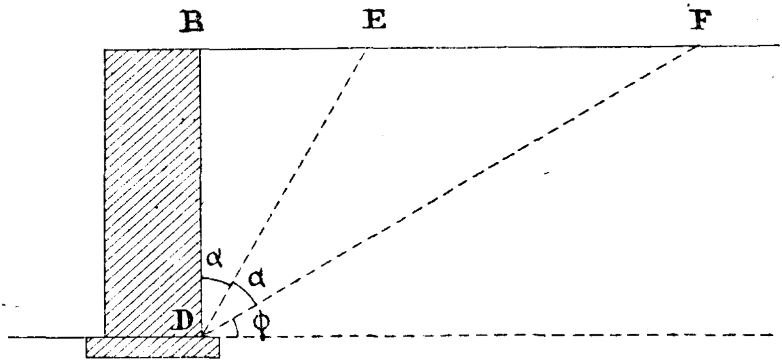
[1] Minutes of Proceedings Inst. C.E., vol. cxcix, p. 233.

experiments would have to be on a much larger scale. With regard to Mr. Bell's dissent from the view that the plane of rupture was merely a mathematical fiction, the only reply the Author could give was that in none of his experiments could he find the slightest indication of its existence. In all cases the movement of the sand attained a maximum close to the wall, gradually decreasing to nothing, whereas if the wedge theory were true there should have been a definite movement along the plane of rupture. Mr. Crosthwaite.

Different writers' views seemed to vary greatly as to the plane of rupture, and the Author would draw attention to one statement taken from a well-known American authority[1] :—

"If the wall is suddenly removed a portion of the volume BDE (*Fig.* 5) will at once fall; the remainder will assume its natural slope gradually. The pressure upon the back of the wall is assumed to be due to the volume which

Fig. 5.



will at once fall, and this volume is assumed to be separated from the remainder by a plane surface DE, called the plane of rupture, along which it slides in the form of a compact prism. The angle BDE is called the angle of rupture. The less cohesion there is in the material the more closely will the results of the experiment agree with this hypothesis."

It was to test the truth of this statement that the experiments described on p. 258 were made, and it was to Mr. Crosthwaite's explanation thereof that Mr. Mitchell took exception.

He had never intended to imply that Sir Benjamin Baker had suggested that a loose granular material behaved as a liquid, but had

[1] G. J. Fiebeger, *Civil Engineering*, New York, 1905.

Mr. Crosthwaite. merely quoted his rule in order to show to what extent it was confirmed by the experiments. Mr. Mitchell referred to the conclusions drawn by Mr. Crosthwaite in his former Paper that walls designed by Rankine's theory would have a factor of safety ranging from $2\frac{1}{2}$ to 4, a conclusion that was drawn before he had made any experiments to determine the actual horizontal thrust, and when he had only the results of his plunger experiments from which to draw conclusions. At that time he had no idea that granular materials had three distinct angles of internal friction. This was no doubt disconcerting, but it appeared to be a fact. He agreed with Mr. Mitchell that the angle of repose was quite distinct from the angle of internal friction, which varied with the way the material was put together, namely, whether hand-packed, consolidated, or thrown together loosely, and this was the very point upon which he joined issue with the writers of current text-books, who used for their calculations the angle of repose, and dragged in wall-friction in a fruitless attempt to make the results of their calculations agree with observation.

Dr. Fulton. Dr. FULTON, in reply, referring to Mr. Mitchell's observations, remarked that his experiments had been made for the purpose of measuring moments, not pressures. To connect with the pressures obtained by calculations based on the Rankine and wedge theories it was necessary to convert the pressures into moments by making use of the assumptions fundamental to each. These depended mainly on the adoption of the correct angle of friction or repose and on the direction of the resultant pressure. He had adopted what he considered the proper factor, namely, the internal friction of the material, and the most reliable means of obtaining it. Apart from using the angle thus deduced as the angle of surcharge, the experiments were quite independent of these assumptions. It was only because fair agreement seemed to exist between the observed moments and the moments calculated by the wedge theory at the smaller heights—though there was a much greater discrepancy using the Rankine assumptions—that it was thought advisable to try to explain the discrepancy from both at the larger heights of the observed moments. It was stated that it might be due to experimental error, in that possibly cohesion did vary with pressure and should not have been ignored, or the conditions were, to a certain extent unsuitable and "arching" took place, though every effort was made to avoid it. As to the variation in the position of the resultant from 0·33 to 0·364 of the face of the wall from the base, this was an assumption applied to the wedge values only, and was taken, as stated in the Paper, from Professor Cain's investigation. Dr. Fulton.

He believed that the principal difficulty in obtaining correct conclusions was in the adoption of a true angle of friction. While it was the case that the surface of rupture of compacted material was not a plane and that its slope at parts was very steep, it should be noted that in the experiments these conditions were purposely avoided, loose material being used with as low an angle of repose as possible in order to obtain the greatest turning moment.
