Correspondence.

Mr. A. L. BELL observed that there was much to be said for approaching the question of pressure upon clay primarily from the point of view of settlement, and the formula that the Author had put forward, $C = \left(\frac{P}{m} + I\right)^6$, would doubtless receive careful consideration from engineers. No reasoned basis for that formula was advanced, and it would be interesting to know how it had been arrived at. Mr. Bell.

The design of the testing apparatus was excellent.

Examination of the plotted graphs (*Figs. 3* to *11*) was made difficult by the fact that there was nothing to show immediately which of the two vertical scales in each figure applied to either set of graphs. He would like to suggest that the Author in his reply should explain, step by step, how he had plotted the graphs and why, in *Fig. 10*, for example, P should be divided by 67·15 and not by some other amount.

It had long been known that clay was slightly elastic. According to the Author it would, under pressure, pass firstly into the ductile state and secondly, with greater intensity of pressure, into the plastic state. Mr. Bell had never held the view that a state which could be called ductile intervened between the elastic and plastic states, and he was of the opinion that when deformation (along planes of maximum shearing stress) had exceeded the elastic limit of complete recovery the material had entered the plastic condition, and would continue to yield so long as the pressure continued to act, or until the material acquired by induration greater resisting power.

The earth-pressure formula

$$P = wh \tan^4\left(\frac{\pi}{4} + \frac{\theta}{2}\right) + 2K \tan^3\left(\frac{\pi}{4} + \frac{\theta}{2}\right) + 2K \tan\left(\frac{\pi}{4} + \frac{\theta}{2}\right)$$

was a general formula for cohesive material (with a level upper surface). K represented the capacity for resisting shear before the material had been subjected to pressure. The dry granular material (to which alone Rankine's formula was correctly applicable) had, when loaded, no resistance to shear: in that case K became zero, the terms involving it became zero, and, as pointed out by the Author, the remaining term, $wh \tan^4\left(\frac{\pi}{4} + \frac{\theta}{2}\right)$, was identical with

Mr. Bell.

Rankine's, $wh\left(\frac{1+\sin\theta}{1-\sin\theta}\right)^2$. Proceeding one step further and applying either formula to a material (water) which had no permanent power of resisting shear, θ would become zero and only wh (familiar to hydraulic engineers) would remain.

For security of structures he would prefer to rely upon a rational theory of foundations (or upon empirical formulas of proved merit) rather than upon the results of loading tests as ordinarily carried out on the site of construction. Some years ago he had had occasion to consider a case where, before erection began, loading tests had indicated a supporting power in excess of that to be imposed by the completed structure. On completion of the work the foundation failed to carry the load and serious settlement took place. It was not unusual, though erroneous, to assume that only the material immediately under the base of the test load was carrying the load. In reality, part of the load on a clay foundation (in virtue of its capacity to resist shear when not under pressure) was borne by the material outside the test area. No one could state the width of that additional strip of supporting material, but it was probably dependent solely upon the nature of the material and not at all upon the size of the test area. If it were true that a 9-inch diameter plunger was really supported, not by a circle of clay 9 inches in diameter, but by one, say, 10 inches in diameter, that would make a great deal of difference when the results were applied to the area supporting the actual structure. Supposing that to be 50 feet in diameter, the supporting area would (if the suggested view were correct) be only 50 feet 1 inch in diameter. The smaller the test area in relation to the size of the real structure, the greater would be the error due to that cause.

There could be little doubt that, as the Author had pointed out, induration caused a widening of the supporting base. As the loading was increased that widened area would, with further settlement, again increase, and so on indefinitely until stability was reached. If the Author's formula, after adequate trial, enabled engineers correctly to estimate the settlement which would take place on clay foundations for any intensity of loading it would be of great value.

Mr. Booth.

Mr. J. J. Booth, of Auckland, N.Z., considered that the Paper marked a definite advance in a subject which, owing to the difficulties involved, had received less attention than it deserved. It was of particular interest to him, as he had attempted to derive a relation between the settlement and the load per unit area when he had been engaged on the construction of foundations in swampy country, about 9 years previously. The methods that he had employed were, however, too crude to allow of any general conclusions being drawn from the results, and their usefulness had been confined to the particular work in hand at the time. He disagreed with the Author regarding the degree of confinement which existed in the average footing after construction. The tests described showed that considerable care had been taken to see that lateral pressure at the foot of the plunger was maintained, but to state that "it is usual carefully to reinstate the material around the footings" described a condition on which he would prefer not to place too much dependence. It was sometimes impossible to withdraw the boxing from around a footing until the concrete had set, and the best that could often be ensured was a well-rammed refilling of the void. Mr. Booth.

In order to apply the law of induration to the practical design of footings the values of m and I would have to be determined for each case, and it would be necessary to make similar tests to those made by the Author at different depths below the free surface and on each particular site. Could the Author state the load per square foot ordinarily allowed on the clay on which he had taken the tests? It was apparent from the tests that the amount of settlement which could take place for the same load per unit of area could vary to a remarkable degree even in normal structures, and that, except where footings could be arranged at the same depth and to take approximately the same loads, the old method of designing footings for the same load per square foot would have to give way to more exact methods, the advantage being found not so much in a saving of cost as in the elimination of unnecessary stresses from parts of the structure which were never designed to take them.

Mr. R. C. HARRIS, of Toronto, observed that, from p. 622, the Author's investigations on clay showed that "When subjected to pressure it parts with some of its water, according to the pressure"; also, that it "has a varying water-content at different depths, due to the pressure of its superincumbent weight." He himself was chiefly interested in the determination of the reaction of clay, such, for instance, as the London clay, on being exposed in a tunnel at a considerable depth. It was well known that such a clay, although it might be described as dry, had a tendency to swell on being exposed, and that if a tunnel in it were not lined directly after excavation, heavy pressure would be exerted on the shoring and the tunnel bore would begin to diminish in size. That swelling might be accounted for if, on exposure and consequent release of pressure on the face, moisture were reabsorbed from the atmosphere, a result which, he thought, might reasonably be looked for. Assuming that to be so, he would expect in the case of sewer-tunnels lined with brick or concrete, in which he was chiefly interested, that moisture would be absorbed by the clay through the lining, resulting in swelling Mr. Harris.

Mr. Harris. of the clay and increase of pressure on the sewer-walls until the pressure due to the superincumbent earth load was balanced. Professors C. R. Young and W. B. Dunbar, of the University of Toronto, had written a bulletin[1] which would give an idea of the problem that he had in mind, and he would be very glad to receive the Author's comments on the matter.

Mr. Husbands. Mr. H. W. S. HUSBANDS observed that the Author's experiments, and especially his apparatus, made very useful additions to the knowledge of the behaviour of clay, but the so-called laws appeared to be too vague for practical applications. It was stated that clay passed through elastic, ductile, and plastic stages when under pressure, but the boundaries of those stages were not clearly defined, and, short of failure, there was nothing to indicate when the plastic stage was reached. The Author stated that the settlement would be too great, but did not say how great. That would appear to be the case with the Magistrates' Courts mentioned in Table III, p. 643, where the settlement was no less than 18 inches, though the load was less than ½ ton per square foot, but there was no suggestion that the plastic stage had been reached, and there was no sign of failure, not even a crack. That was the more extraordinary in that the loading was bound to vary and that the saturation-level sometimes fell as low as 8 feet below the original foundation-level, but, in spite of the site being drained, the clay could hardly have dried out, as if it had done so the result would have been very different, unless the clay were very much better behaved than that usually met with. Mr. Harold Berridge had shown the enormous pressures exerted by clay taking up water after having dried out,[2] and that explained what had happened to some bridges which Mr. Husbands had built in India on clay underlying black-soil. The foundations were stepped and widths adjusted to make the pressure as even as possible, and where the streams were running all the year round the bridges stood, but where the streams dried up in the hot weather and the saturation-level fell below the foundations they failed. Mr. Husbands had had an opportunity of inspecting some of those bridges 10 years later, and, although the masonry was obviously good, it was shattered in all directions as though it had been through a violent earthquake.

If foundation-pressure were to be based on some allowed settlement, it would be asked whether buildings could be so designed as

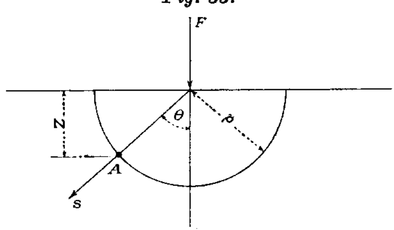
[1] "Observed Soil-Pressures on the Deep Sewers of the North Toronto System," University of Toronto School of Engineering Research, Bulletin No. 145. (Toronto, 1935.)

[2] "The Colloidal Nature and Water Content of Clays," *Engineering*, vol. cxxx (1930), pp. 5–7, 61–63.

to equalize the pressure on foundations and so the settlement, or whether vertical settlement-joints would have to be provided. Surely, however, the suggestion was unsound? According to the Author's formula, as worked out in *Figs. 17*, p. 640, the London blue clay with a settlement of 1 inch would support as much as 6,180 lbs. per square foot at the surface and only 9,152 lbs. per square foot at a depth of 20 feet, but with a 3-inch settlement it would stand 9,280 lbs. per square foot at the surface; therefore, if, having ascertained the constants for the particular clay and tentatively fixed the width and depth of the foundation, the supporting capacity for a 1-inch settlement were subsequently found to be less than the load, all that would be necessary would be to increase the allowed settlement to 2 or 3 inches and the foundation would be adequate. In conclusion, he felt that the Author's proposals had been put forward too soon, but hoped, nevertheless, that the Author would continue his researches until he could evolve some formula of more general application. Mr. Husbands.

Mr. DIMITRI P. KRYNINE, of Yale University, shared the Author's point of view so far as the similarity of clay and metals was concerned. He wished to emphasize that the similarity referred to was not of general character, but was only in the province of stress-distribution under load. Sands, clays, stones, and metals might be visualized as masses, and the stress-distribution in all such masses obeyed a law which might be expressed by a formula. Numerical coefficients or parameters in that formula were different in the case of different materials, however, and those coefficients for clays and metals were Mr. Krynine.

Fig. 33.



strikingly similar. If the boundary-plane of an infinite elastic body (such as a metal, for practical purposes) were loaded with a concentrated force F, the stress at a point A (*Fig. 33*) would be:

$$s = \frac{3F}{2\pi\rho^2} \cdot \cos\theta = \frac{3F}{2\pi z^2} \cdot \cos^3\theta \quad . \quad . \quad . \quad . \quad (1)$$

or, in a generalized form,

$$s = \frac{n \cdot F}{2\pi z^2} \cdot \cos^n \theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Mr. Krynine. In the case of clay the coefficient n in formula (2) was slightly over 3, whilst for sand it was much higher. Formula (1), or the "Boussinesq formula"[1] in a simplified form, was valid in the case of an incompressible body (Poisson's ratio = 2). It had been recommended by the Committee of the American Society of Civil Engineers on Earths and Foundations[2] for determining the vertical pressure at a point within a loaded earth mass.

He agreed in a general way with the Author's conceptions of elasticity, ductility, and plasticity, but considered it necessary to introduce some clarifying statements. At a point h feet deep in a clay mass, there was a vertical pressure $w.h$ and a horizontal pressure which could be expressed as $k. w. h$. The ratio k for a clay mass in its natural state was perhaps about 0·75, whilst for an ideal elastic body it would be 1. If a circular area at the boundary were loaded (*Fig. 34*), the zone I, representing the nucleus of the mass, would carry about $\frac{2}{3}$ of the whole load and would be overstressed vertically. The zone II, which in the case under consideration represented the shell of the mass, carried only about one-third of the load and was overstressed horizontally. The boundary of the two zones was a hyperbolic surface, and along it the ratio k retained its natural value as it was in the whole mass before loading. Furthermore, within zone I there was a disturbed zone III, or "bulb," as the Author called it. If the shell II was strong enough, it held the whole system in equilibrium; what happened in such a case was the consolidation of the vertically-overstressed nucleus I. That process was what the Author termed "induration" or the "ductile stage." If, however, the shell II yielded, plastic flow toward the outside would take place following the direction of shearing surfaces or slip lines. Those lines were known in the theory of elasticity as trajectories of principal shearing stress, and their lower points possessing horizontal tangents were located at the hyperbolic boundary between zones I and II. If zone II yielded, nothing would keep zone I in equilibrium any longer; it would be destroyed and would flow. Evidently the bulb III would also break. That was what the Author termed the "plastic stage."

He agreed completely with the Author's interpretation of the meaning of the "bulb." Since vertical stresses under a foundation were considerable, moisture was squeezed out of the "bulb"; hence its physical properties changed, and it might become quite a foreign body in relation to the rest of the mass. In a field loading test that he had made in East Hartford, Connecticut, a decrease of the

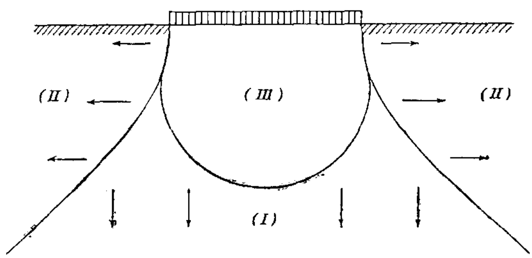
[1] J. Boussinesq, "Application des potentiels a l'étude de l'equilibre et du mouvement des solides élastiques." Paris, 1885.

[2] Proc. Am. Soc. C.E., vol. 59 (1933), p. 781, formulas (7) and (8).

Mr. Krynine.

moisture-content within the bulb from about 33·0 per cent. down to 18·4 per cent. had been observed.[1] He was not prepared, however, to furnish experimental evidence as to the decrease of colloidal content within the bulb. The disturbed zone or "bulb" III (*Fig. 34*) might break and flow, not laterally, but upwards, as shown in *Fig. 21*, p. 642, only in the case of an excessive load and a strong shell II. That might happen, for instance, if the load were applied at the bottom of an excavation in stiff clay; in such a case the overburden increased the strength of the shell II, and according to the law of least resistance, the failure manifested itself in the breaking of the "bulb" III alone. Hence *Fig. 21*, where the load was shown to be applied at the boundary, should be slightly corrected.

Fig. 34.



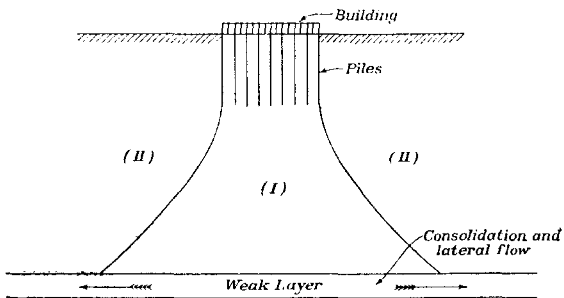
In his opinion the Author had not paid due attention to the fact of the crushing of the "bulb" by the foundation and to its lateral squeezing from underneath the building. Admittedly, if such a lateral squeezing were prevented by the presence of the overburden, as in the case of an excavation, the "bulb" would tend to be squeezed up.

Reverting to the Author's conception of elasticity, Mr. Krynine wished to emphasize that a purely elastic stage in the clay mass did not exist at all. If a clay mass were loaded, a settlement would occur; if the load were removed, an elastic rebound would take place, but it was always smaller than the whole amount of settlement. That proved that the elastic action and the ductile action existed simultaneously, which was perhaps the respect in which clays differed from metals. As the Author rightly stated, all buildings settled, and in the case of clay there was no way to prevent the consolidation or "induration" of zone I. That consolidation was the primary cause of the settlement of a building, even if it had been

[1] D. P. Krynine, "Soil Action under Load Shown by Test," *Engineering News-Record*, 29 December, 1932.

Mr. Krynine. conservatively designed so as to keep zone II stable. If zone II yielded, that would be another cause of settlement. In addition to those two causes of contact settlement, there might be still another cause owing to geological irregularities in deeper strata. *Fig. 35* showed diagrammatically how zone I, even though itself perfectly stable, might sink together with the building solidly founded on piles if there were a weak layer underlying it. If a formula were proposed to express the amount of settlement, admittedly it could not forecast the action of deeper strata, and it would therefore be merely a contact formula. Such was the case of the Author's formula expressing the settlement C for the " ductile state "—in other words, for the case of consolidation of zone I kept firmly in position by zone II. It followed from the preceding discussion that the formula in question, even if right for the case when zone II was stable, did not hold any longer if zone II started to move.

Fig. 35.



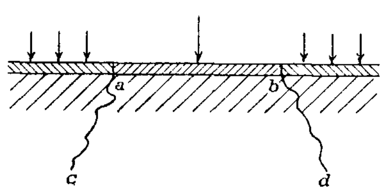
Putting $P = 0$ in the Author's basic formula, the settlement C would become I^6; thus, if the clay mass were not loaded at all there was already a certain settlement. That was theoretically unsound; but since the value of I^6 was very small, that objection could be withdrawn. Furthermore, the formula for the settlement expressed the inter-relationship of one variable, C, and another, P. Hence the parameters m and I, which depended both on the physical properties of the soil and on certain mechanical characteristics of the foundation, should be constant. Otherwise, if m were a variable, the formula in question represented one equation with two unknowns, and hence did not express a categorical law.

It was regrettable that the Author paid no attention to the rigidity of the foundation, since the degree of that rigidity influenced greatly the amount of settlement and its distribution along the loaded area. It was also very regrettable that, in describing the interesting cases of settlement in British Guiana, he had not plotted settlement against time, and had thus deprived the reader of the possibility of stating, even tentatively, what were the actual causes of the phenomena. If the time-settlement curve had been shown to approach an oblique straight line, it indicated a possible motion of zone II. There was no further information in the Paper on the geology of deeper strata, so that Mr. Krynine was obliged to believe that there was a contact (shallow) settlement only. The fact that the Magistrate's Courts took from 10 to 12 years to settle 18 inches, while the Free Library constructed at the same site took more than 20 years to settle $6\frac{1}{2}$ inches or less, could be explained by the motion of zone II in the former case owing to the greater unit load (1,208 lbs. per square foot as against 1,084 lbs. per square foot). Admittedly, that was only a guess, since there was not sufficient evidence of the necessary type in the Paper. Mr. Krynine.

In the opinion of Mr. Krynine, the Author was to be commended for bringing to the attention of the profession interesting cases of settlement, and for emphasizing several sound statements on the behaviour of a loaded clay mass.

Mr. JAMES MITCHELL remarked that, on p. 624, the Author, in upholding the value of bearing-tests conducted on small areas, stated that "the clay experimented upon behaves in a remarkably consistent manner." That was to be expected, if the tests were properly conducted, but it afforded no proof that small-area tests had a value equal to those on large areas. Indeed, on p. 625 it was mentioned that small-area tests evaded the difficulty of the time-factor—a most important one. Referring to the test by Dr. Faber, cited on p. 633, attention was drawn to the fact that pressures were greatest at or near the edge of the plate, and that the effect of a surcharge was to Mr. Mitchell.

Fig. 36.



reduce the excess of pressure. If the surface of the ground outside the 12-inch diameter plate used by Dr. Faber had been covered by plating, as in *Fig. 36,* and surcharged to the same unit intensity as in the case of the loaded disk, the inequality of upward pressure on the latter would disappear. The effect of the load on the disk, when there was no surcharge, would be to cause a tendency to

Mr. Mitchell. rupture along lines somewhat as ac and bd. That would produce an upward force along the edge of the plate, and it was obvious that the influence of that "edge-effect" would become relatively more important as the disk was decreased in area. On p. 632, with reference to a compressive test on clay, it was stated that "The appearance of the indurated material had entirely changed, as though it had been denatured by loss of colloid matter." A few lines further on, after stating that the changes observed could not be entirely accounted for by the reduction of water-content, it was claimed that "The induration of the material was due also to partial loss of colloid matter." It was a matter of ordinary observation that stiff clays could be considerably indurated by pressure, without any apparent loss of water. It would be interesting if the Author would give some of the evidence supporting the statement that clay could be deprived of some of its colloid-content by pressure. On p. 625, it was stated that "Several previous investigators have observed that the friction-angle of clay is indeed small, and yet the loads placed on clay are considerably *in excess of that corresponding to its friction-angle*. This being so, its resistance must be due to some other property." There was no such thing as a "friction-angle of clay." Clays differed enormously in their friction-angle, and, in any particular clay, the friction-angle was subject to great variations due to change of circumstances, one of the principal governing factors being the water-content of the material. The Author claimed (p. 626) that the results of his tests were in close agreement with "the actual settlement of structures erected in the same clay," and on p. 642 reference was made to a Paper on the settlement of two of those structures. In that Paper it was mentioned that the settlements were measured from the top of a wooden post, which itself moved upward and downward to an unknown extent owing to the expansion and contraction of the clay-bed by which it was supported. It did not seem to have been quite clearly established that the differences of level observed did not refer to a rise of the post, rather than a settlement of the buildings. After some years' observations, the bench-mark was changed to a mark on a building, which it was stated was "assumed to be stable," no reason being given for the assumption. Notwithstanding that state of affairs, a Table of average settlements measured to the nearest $\frac{1}{833}$ part of an inch was given in that Paper.

Reference was made on p. 641 to the idea that clay might be reduced to a state of fluidity under certain conditions of pressure which, although not occurring in ordinary foundation-work, might arise in pile-driving. There appeared to be nothing in the results of Mr. Ackermann's experiments which was inconsistent with the view that clays were plastic solids, of varying constitution and characteristics.

Certainly his experiments afforded no support for the idea that in any of them the clay had the property, characteristic of a fluid, that at any point in its mass the pressure was the same in every direction. With reference to *Fig. 21*, p. 642, it was highly probable that a plug of clay, *forming a virtual point*, would be carried down by the loaded structure, and that the plastic upward flow of material, suggested by the Author, would neutralize any tendency to leave a vacant space round the sides of the structure. The bulb in the diagram, however, appeared to be unduly large. **Mr. Mitchell.**

Mr. MAURICE NACHSHEN was of the opinion that, whilst the Author had made a valuable contribution to the knowledge of the behaviour of clay under pressure, his equation $C = \left(\frac{P}{m} + I\right)^6$ could hardly be accepted as the statement of a law of nature, as the title of the Paper might indicate; it might have been better to avoid the use of the word "law" in that title. The Author himself admitted that the above equation only approximately represented the middle portion of a curve, which in that portion approximated to a straight line. It was therefore an empirical expression applicable only within certain limits, which, however, were not clearly stated in the Paper. The Paper would also be increased in value by the inclusion of definitions of the terms "set," "ductile," and "plastic," and also of the actual readings made during the tests, with a description of all relevant circumstances, so that readers might use that information, so carefully collected, to form their own conclusions. **Mr. Nachshen.**

The Author stated that in the equation $C = \left(\frac{P}{m} + I\right)^6$, the coefficient m depended upon the water-content of the mass and the size of the foundation. There was no factor to allow for the thickness of the clay stratum below the point of application of the load, though it seemed obvious that settlement would be more when that thickness was great than when it was small. With regard to the absence from the equation of a factor for time, the Author had said that the readings in the tests had been taken when the settlement was complete at each increment of load. Was it to be assumed that, when the equation was applied to a full-sized foundation, the resulting value for the settlement was the final amount of settlement? If so, engineers would have to seek a means of estimating the length of time before settlement would be complete, which might be many years, and also of estimating the progress of settlement during that period.

The Author had referred to several American publications, but attention should also be drawn to the Report of the Special Committee on Earths and Foundations of the American Society of Civil

Mr. Nachshen. Engineers [1] as, in his opinion, that Report contained the most rational approach to the subject of settlement in clay that he had yet encountered.

The Author's reference on pp. 636–637 to pile-driving confirmed the view that piles driven into clay did not follow the dynamic laws assumed in most pile-driving formulas. It seemed clear that a temporary condition of stress was set up during driving, which later disappeared, leaving the pile supported only by friction and by bearing on its shoe, where a bulb of pressure might form.

He hoped that the very important subject of soil mechanics would receive more attention in Great Britain than it had done hitherto. Such investigators as had worked on the subject had individually been brilliant, and their results valuable, but they had largely been hampered by lack of means, time, and opportunity from developing the subject fully. Great work could be done under the guidance and encouragement of an Institution Committee, which might co-operate with the already existing committee of the American Society of Civil Engineers. Besides organizing laboratory work, such a committee could encourage the making of systematic measurements on actual works which were being constructed all over the world by Members of The Institution. The analysis of such a fund of information might result in placing soil mechanics on a comparable footing to the mechanics of constructional materials.

Mr. Searle. Mr. A. B. Searle observed that much work had been done on the flow of plastic clay under pressure, but most of it related only to cases where the clay was contained in impervious vessels, such as press-boxes, pipes, and extrusion machines. For that limited condition, Dr. E. C. Bingham's work [2] was commonly regarded as classical as it led to the law of plastic flow :

$$\mu = K\frac{V}{t(P-f)}, \quad \text{or} \quad V = \frac{\mu t}{K}(P-f),$$

where

μ denoted the mobility,
f ,, ,, yield value,
V ,, ,, volume extruded during time t,
P ,, ,, pressure,
K ,, a constant.

That law showed that plastic masses rich in clay behaved almost like non-plastic solids up to a certain critical pressure (a yield-point corresponding to Ackermann's " pressure of fluidity "), after which,

[1] Proc. Am. Soc. C.E., vol. 59 (1933), pp. 777–820; *see also* Engineering Abstracts 57, 70.—Sec. Inst. C.E.

[2] E. C. Bingham, U.S. Bureau of Standards Scientific Paper No. 278; *Jour. Amer. Ceramic Soc.*, vol. 7 (1924), p. 430, etc.

Mr. Searle.

under the excess of pressure, they flowed like liquids. In other words, the pressure producing the flow was only the excess of pressure above the yield-pressure. That had been confirmed by Dr. H. Chatley.[1]

The upper curves in several of the Author's graphs closely resembled those obtained by Messrs. Bingham, Hall, Green, and others by plotting the volume of clay extruded in unit time against the pressure per unit area of clay, but the Author's equation omitted all reference to time. Yet, by judging from general experience in the manufacture of bricks, tubes, and other articles of clay by extrusion under pressure, as well as by more fundamental considerations, time was an essential factor; it might, of course, be included (undisclosed) in the factor m. On the other hand, as the time-factor was chiefly concerned with the early stages of the application of the pressure (prior to the critical pressure being reached), the time-factor might be of minor importance in cases such as those considered by the Author. The use of a porous container —which was practically what the Author had employed—altered the conditions by providing a means of escape for some of the water and probably some of the colloidal clay. It thus led to a fresh set of conditions, and introduced the factor of induration with which the Author had dealt so ingeniously and so clearly. It was surprising that that apparently simple change should convert a simple formula into one involving a sixth power, but all who spent much time in investigating clays were familiar with equally unexpected results.

The comparison with metals was probably less helpful than might be thought, because the changes which took place during the deformation or extrusion of metals were very rapid, whereas plastic clay only behaved normally when the pressure was applied comparatively slowly. Hence, unless the conditions were more strictly analogous than seemed probable, the applicability to clay-paste of a law deduced from the behaviour of metals appeared to be rather in the nature of a coincidence.

It would be exceedingly interesting if the Author would investigate the relation of Bingham's formula with his own, because there ought surely to be some definite relation between them. Apart from that, he deserved the thanks of all who had to deal with buildings on clay foundations because he had shown that his formula was applicable in many instances, and, for that reason, it appeared to be of great value.

Professor Terzaghi.

Professor K. TERZAGHI observed that the apparatus shown in *Fig. 1*, p. 624, had been designed to measure the settlement produced by a

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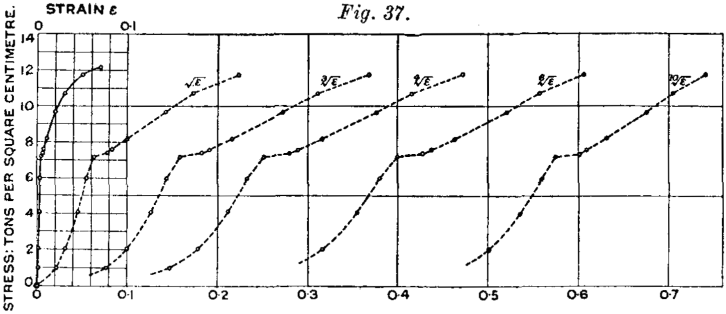
[1] *Engineering Society of China*, vol. 23 (1923–24), Paper No. 2.

Professor Terzaghi.

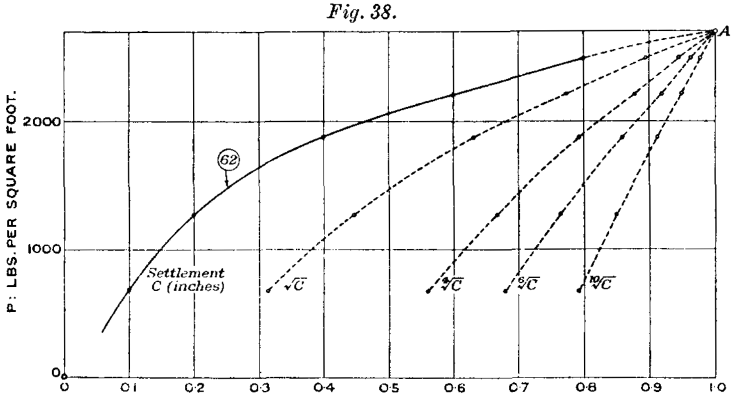
concentrated load acting on the bottom of a drill-hole. Since it was desired to produce complete consolidation of the clay under the loaded area, it would be advisable to accelerate the process by using a piston with a permeable bottom, similar to the piston of the compression apparatus that he himself had employed.[1]

Fig. 37.



The plain curve in *Fig. 37* represented the stress–strain curve for a Siemens-Martin alloy steel. The abscissæ of the dotted curves gave the nth root of the strain, for $n = 2$, 3, 4, 6 and 10. For each curve, the abscissa-scale was so chosen that the curve remained within the boundaries of the diagram. In a similar manner the

Fig. 38.



abscissæ of the dotted curves in *Fig. 38* represented the nth roots of the abscissæ of the plain curve, which was identical with the settlement curve 62 in *Fig. 5* (p. 628); the scales for the curves were so chosen that all the curves, including the original, passed through

[1] K. Terzaghi, " Festigkeitseigenschaften der Schüttungen, Sedimente und Gele." ; in Auerbach and Hort, " Handbuch der physikalischen und technischen Mechanik," vol. iv, Part 1. Leipzig, 1931.

a common point A. The facts represented in *Figs. 37* and *38* expressed the *purely geometrical* law, valid for any curve with a variable radius of curvature, that the difference between the *n*th-root curve and a straight line decreased with increasing values of *n*. From an examination of the different *n*th-root curves in *Figs. 37* and *38*, no reason was apparent for giving to the sixth root the preference over the fourth or the tenth. Considering the arbitrary character of the Author's choice of the sixth root, a general validity for the formula based on that choice could not be expected. **Professor Terzaghi.**

Under the heading "Law of Induration" (p. 625) the Author mentioned four different changes, (*a*) to (*d*), resulting from the slow application of an unbalanced pressure. All the changes which really occurred, such as the closer aggregation of the grains and the increase in strength (cohesion), represented the inevitable consequence of the process of consolidation, namely, the gradual increase of the density of the clay due to a migration of part of its water-content to the surrounding mass. The differential equation of that process and an analysis of all its physical consequences had been published by Professor Terzaghi 10 years ago.[1] Since then he had made or supervised several hundred consolidation-tests, yet he had never observed the escape of colloidal constituents from the clay subject to consolidation, except in minute quantities. According to his opinion the slime which had been observed by the Author in his tests had originated in the following manner. Because of the pressure exerted on the plunger, clay had been forced into the narrow slit between the plunger and the tube. At the same time, part of the excess water of the "bulb of pressure" had been forced through the slit into the tube, whereby it disintegrated the clay present in the slit or in its immediate vicinity and assumed the slimy appearance mentioned by the Author.

In *Fig. 13*, p. 635, the Author showed a curve which represented the relation between the depth and the reciprocal of the water-ratio. The shape of that curve seemed to express the experimental fact that the water-content of a clay decreased with increasing pressure according to an approximately logarithmic curve. If the pressure–water-content curve for a clay were known, the relation which should exist between the water-content and the depth below the surface of the entire deposit could easily be derived by a graphical procedure. Professor Terzaghi had determined in the laboratory many pressure–water-content curves and had compared the corresponding depth–water-content curves with reality. During the first years of his investigations he had been puzzled by the fact that there was

[1] K. Terzaghi, "Erdbaumechanik," pp. 170–183. Vienna, 1925.

Professor Terzaghi.

very seldom any resemblance between the anticipated and the real depth–water-content curve. Later he had became accustomed to that fact. Although many of his drill-holes had been carried to a depth of more than 100 feet, an appreciable decrease of the water-content with the depth, comparable to that which should be expected, had been found to be exceptional. The following data were selected at random from his records. A certain clay had been described by the drill-man as stiff and homogeneous. The upper surface of the layer was located at a depth of about 30 feet below the bottom of a river. The water-content (as a percentage of the weight of the solid substance) obtained from tests on undisturbed samples was as followed:

Depth below the bottom of the river: feet	30	31	37	40	46	51	60
Water-content: per cent.	29·9	26·2	24·2	25·2	27·3	24·3	30·9

According to the results of the soil tests, the clay encountered at a depth of 60 feet was practically identical with that at 30 feet. In another case he had secured samples from a stratum of glacial clay 100 feet below the surface. The superimposed strata of sand and silt represented a surcharge of about 3 tons per square foot, which had acted on the clay during a period of at least several thousand years. That excluded the hypothesis of incomplete consolidation. The water-content of the sample in its natural state was 67 per cent., but the water-content of a disturbed sample of the same clay consolidated in the laboratory under a pressure of 3 tons per square foot was only 46 per cent. An attempt to explain the properties of the clay responsible for the apparent lack of a relationship between the depth and the water-content of natural clay deposits had been published by Mr. A. Casagrande.[1]

Twenty years ago, when Professor Terzaghi had begun his systematic investigations of the behaviour of clay under pressure, he also had believed the problem to be very simple. However, the more he learnt about the subject by continuous personal contacts with the material in the laboratory and in the field, the more he became convinced that there was "a long jump from a block of steel to a lump of clay," not to speak of the long jump from a lump of clay to a natural stratified or lenticular soil-deposit.

Mr. Walker.

Mr. E. G. Walker observed that the results obtained with the Author's experimental apparatus ought to be given greater weight than those of many tests which had been made in the past, because of the arrangements which he had adopted. The inconsistencies

[1] "The Structure of Clay and its Importance in Foundation Engineering." Journal Boston Soc. C.E., vol. xix (1932), pp. 168–221.

which had characterized a large number of field observations of bearing pressures on soils made in the past had arisen in considerable measure from defective experimental methods which had introduced large errors by the disturbance of the soil from its natural state. Although the ill-effects of that disturbance had been recognized, experimenters generally had only succeeded in partially eliminating it. The Author had succeeded to a greater degree than most. Mr. Walker suggested, however, that under some circumstances the conditions of testing were still not completely satisfactory. From *Figs. 1* and the description of the apparatus on p. 623, it would appear that the surface upon which the plunger pressed was practically co-planar with the bottom edge of the bore-tube. If any downward pressure should have been applied in placing that tube, a disturbance of the original structure of the clay might result in the formation of a ring of disturbed material around the plunger. The extent of any interference with the measurements that such a ring would cause would depend upon the stiffness of the clay. Mr. Walker.

The Author had concluded (p. 621) from his experiments that the behaviour of clay under load might be likened to that of metals and other materials having a high degree of elasticity. There was certainly an analogy between them, and it had been recognized by other modern experimenters, as the Author stated later. From the evidence so far available, however, it seemed unreasonable to push that analogy too far. The Author agreed that the properties of clay depended upon a number of variables, all of which tended to non-homogeneity to a degree which was absent in metals.

The difficulty in reconciling the terms that were applied by writers on ceramic matters with the proper scientific definitions of those terms was a very real one, and the Author's quotation from a well-known writer on ceramics was an apt illustration of that difficulty. It seemed reasonable to suggest that such terms as "plasticity," "ductility," and "elasticity" could only be defined clearly in the first place with reference to materials that had definitely elastic properties. Mr. Walker considered that the definitions given by Dr. Arthur Morley [1] were as clear as any that had been put forward. They were :—

> "Plasticity.—A material may be said to be *perfectly* plastic when no strain disappears when it is relieved of stress."
>
> "Ductility is that property of a material which allows of its being drawn out by tension to a smaller section. . . ."

On the basis of those definitions, the meaning of the statement on the ductility of clay quoted on p. 622 was obscure. Unfortunately

[1] "Strength of Materials," 7th edition, p. 34. London, 1928.

Mr. Walker. slackness in the use of terms which were capable of close definition was widespread. It would be of great advantage in the investigation of the complex phenomena of the behaviour of soils under load if a greater rigidity in defining and using terms could be achieved.

The suitable size and shape of a test area for bearing-tests was a question upon which there had been a certain amount of controversy. Although the small standard area of 12 inches by 12 inches should suffice for comparative results (for example, for measuring the variations of supporting-power from point to point over a finite site or over a range of depths), the evidence which was adduced in the Paper, as well as the experimental results of Professor Terzaghi and others, which showed the variation of intensity of pressure over extended areas of varying dimensions and shapes, pointed to the conclusion that a proper working value could only be obtained by making such allowances as had been shown to be appropriate.

The main interest in the Paper naturally centred around the load–deformation formula

$$C = \left(\frac{P}{m} + I\right)^6,$$

which was also written in the form

$$\sqrt[6]{C} = \frac{1}{m} \cdot P + I\,;$$

that was to say, there was a straight-line law connecting $\sqrt[6]{C}$ and P. That formula was very different from anything which had previously been proposed, and it would be of considerable interest to learn of the steps of its development. From the plottings given in *Figs. 3* to *10* it would seem that in many cases the straight-line relationship was only a relatively wide approximation. There seemed no *a priori* ground for expecting so high an index as 6. Presumably the method that had been employed for developing the formula had been to plot observations and deduce therefrom an empirical law, choosing naturally the simplest form that would agree with the observations. The application of that process in the present case might well have led to the masking, by the use of a high root-index, of the effect of other important factors upon which the load–subsidence relation depended. The higher the root-index, n, in the formula

$$\sqrt[n]{C} = \frac{1}{m} \cdot P + I$$

the closer would plottings of $\sqrt[n]{C}$ and P be likely to follow a straight line, because the range of variation of $\sqrt[n]{C}$ over any specific range of values of P would be so much less. Mr. Walker believed that, although the fixed index 6 no doubt fitted the Author's observations better than any other value, the rational relationship between C and P would be found to be more complex than appeared from the empirical formula. The Author defined m as a coefficient, and I as "a constant representing elasticity and set" expressed in the same units as C. It was difficult to follow that definition. If the Author were to be followed in his analogy between the behaviour of clay and elastic solids, there would have to be a proper dimensional relation between the terms of his equation. Such a relation could not exist unless m and I were assumed to be functions of considerable complexity. Mr. Walker.

It was greatly to be hoped that measurements such as those already made by the Author would be continued and extended. Although vast sums were expended annually in constructing foundations in all kinds of circumstances, quantitative observations which could be used in combination with others for general information were very rarely available. Contributions such as the Paper under discussion were of considerable value in advancing the knowledge of the more practical aspects of the behaviour of soils under load.

The AUTHOR, in reply to the Correspondence, dealt first with opinions which had been expressed as to the suggested analogy of clay and metals under test. He was aware of the views held by some authorities that certain materials under test passed directly from the elastic to the plastic states, omitting any intermediate state. The ductile stage was considered by some to be a combination of the elastic and the plastic states. In regard to clay, the elastic range was exceedingly small. Deformation in the purely plastic state was, by definition, permanent. If, upon removal of the load, a small partial recovery occurred, that showed that the clay, or at least part of it, still possessed some degree of elasticity. The Author.

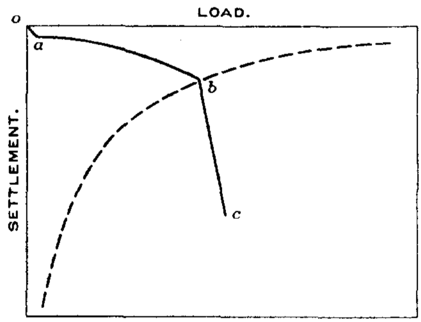
Mr. Krynine did not consider that a purely elastic state occurred in a clay mass. That state, however, could be observed by screwing a small disk and rod similar to a screw pile into an undisturbed bed of clay and then placing independent and small increment-loads on the clay surface. Although the loads would produce permanent deformations of the clay at the surface, delicate observations of the rod and disk would demonstrate that at a depth of, say, 3 feet, the clay would, within small limits, show a complete recovery. *Fig. 37*, p. 680, shown by Professor Terzaghi, indicated more clearly than the Figures in the Paper that the curves for metal during the so-called ductile stage had a striking similarity to those he had put forward for clay, but it was apparent from the interesting though divergent opinions expressed that the suggested analogy would have to remain unsettled for the present.

He agreed with Mr. Bell that the results of loading tests as

The Author. ordinarily carried out on the site were not at present satisfactory. It was for that reason that he had found it necessary to develop an improved method of testing. Indeed it would be correct to state that any progress that he might have achieved was due, in a large measure, to the gradual improvement of his apparatus. That experience was not uncommon in other branches of research work.

Mr. Krynine's difficulties as to varying strata at different depths could be overcome by conducting tests at varying depths. With a 12-inch diameter plunger, he had found that tests could be safely made at each 6-foot increase in depth without disturbance of the clay by the preceding test. Permanent deformation of the British Guiana clay occurred to a depth of approximately three times the diameter of the loaded area.

Fig. 39.



With regard to the actual supporting area of the clay being somewhat larger than the nominal area, his investigations had shown that the increase of 1 inch on a 9-inch diameter base was approximately correct, giving an increase in effective bearing area of 23 per cent.

The boundaries or limits of the different stages might be clear from *Fig. 39*, in which o–a represented the elastic stage, a–b the ductile stage, and b–c the plastic stage. For small areas, say 1 inch in diameter, the plastic stage would be reached after a small settlement of, say, $\frac{1}{2}$ inch. For a test area 12 inches in diameter, the same stage might be reached with a settlement of about 3 inches, whilst for a foundation base it might only be reached with a settlement of 18 inches or more. With the testing apparatus described, that critical point could be recognized by a change in the curve. Some writers had termed the last stage "progressive settlement," no doubt because of the change which occurred in the time–settlement curve.

The point b corresponded to Mr. A. S. E. Ackermann's "critical pressure." The Author.

The safe load often adopted on the British Guiana clay was 7 cwt. per square foot.

The increase of pressure caused by the swelling of clay due to a change in its moisture-content, referred to by Mr. Harris, was a problem well known to bridge-engineers. In the case cited by Mr. Booth, there would seem to be little doubt that the pressure exerted on the sewer-walls would be greater after the material had been disturbed, on account of an increase in its plasticity.

Replying to Mr. Husbands, it was not considered that the plastic state had been reached in the settlement of the Magistrates' Courts, otherwise it would have been reflected in the time-settlement curve. That could be seen on reference to Mr. Hodge's Paper.[1] In British Guiana, all time-settlement curves were affected by occasional periods of drought. His statements regarding the average settlement of that building were quoted from Mr. Hodge's Paper, and he agreed with Mr. Mitchell that such exactitude as the giving of settlements to $\frac{1}{833}$ of an inch might be considered unnecessary.

He had plotted a large number of time-settlement curves, but he did not think that the value of the Paper, which dealt mainly with the relation of load and settlement, would be materially enhanced by their inclusion, as he could not see the necessity of introducing the time-factor into the problem, provided that the settlement was kept within the limits given above. The actual amount of settlement to be adopted in design would, of course, depend upon a study of the conditions governing each particular case, and the class of structure to be erected, and it should be realized that the settlement of a structure should be measured from the time the building was commenced, and not from the date of its completion, as was commonly the case.

The index 6 had been adopted in the general equation because, in the plotting of a large number of curves, experience had shown that if a lower index were used, the plotted points deviated from a straight line. He agreed that an index of 10 might be used, provided the values of m and I were also adjusted, but what Professor Terzaghi had really done in *Fig. 38* (p. 680) was to take a value of C equal to unity. Whatever value was assigned to n, $\sqrt[n]{C}$ equalled unity so that each line passed through a common point. When it was desired to analyse a number of curves of the same type, comparisons were greatly facilitated if they could be reduced to a straight-line equation. What

[1] See footnote (1), p. 642.

The Author. really mattered was whether the method of analysis used gave sufficiently accurate results.

He agreed that the analyses of disturbed samples of clay gave unreliable results, and it was for that reason that he had been induced to conduct tests in situ rather than to make them in the laboratory.

It was satisfactory to note that Professor Terzaghi considered that there was such a large difference between a lump of clay and a natural stratified deposit, as he had read some of Professor Terzaghi's valuable contributions to the subject, from which he had observed that much work had been done in testing in the laboratory the "lump of clay" which had been obtained from the "stratified deposit," and it was his contention that laboratory tests on clay were not altogether reliable, due to changes which occurred in its physical properties; he was glad to find that his conclusions were confirmed by the experience of Professor Terzaghi and Mr. Walker.