Discussion.

The Author introduced the Paper with the aid of a series of lantern slides.

Mr. Ewart S. Andrews observed that the Paper was characterized by the scholarship and originality which the Author had shown in all his previous work.

The Author had been rather critical of Mr. Alfred Hiley, especially in regard to the suggestion that Mr. Hiley had invented the efficiency factor. As a matter of fact, Mr. Hiley had obtained the efficiency factor from a small book written by Mr. Andrews many years ago. It was the obvious result of applying the principle of the conservation of momentum to any problem on impact, and that was undoubtedly the reason why the same factor appeared in the so-called Dutch formula.

The Author had referred only to the Paper which Mr. Hiley had presented to the Institution of Structural Engineers, but a much fuller account of Mr. Hiley's work had appeared in *Engineering* on 29th May, 1925.†

Mr. Andrews considered that Mr. Hiley's work was a landmark in the treatment of the subject of piling formulae—and that opinion was not confined to people in Great Britain. When Professor Karl Terzaghi, M.I.C.E., who was probably the greatest living authority on foundations, was in London some years ago he had spoken with great appreciation of Mr. Hiley's work, and the great Russian engineer, Professor V. Timoshenko, had also been impressed by its value.

The static loading of piles for test purposes was a very difficult matter, especially as the real value of the bearing capacity of piles could not be obtained unless a group of, say, three piles was tested and it was certain that the load was central. That presented considerable difficulty, because if each pile was to carry a load of 100 tons the combined load would be 300 tons. The Author had mentioned that some of his tests had had to be stopped because of the absence of kentledge.

Moreover, if the piles were in groups, it was not satisfactory to test a single pile not surrounded by others.

The Author had referred briefly to the additional resistance of a pile due to the increase in friction for a period after the time of the actual test loading. It was difficult to measure that, but it did give an additional factor of safety.

Mr. Andrews believed that the Author's formula could apply only to pre-formed piles which were driven from the top. It could not apply to many other types, such as the bulbous pile, driven partly by boring and partly by driving at the bottom, so that a bulb was formed on the pile. In other words, he considered that the Author's formula could apply only to impact-driven piles, and not to piles formed partly in situ and partly by impact.

† A. Hiley, "A Rational Pile-driving Formula and its application in piling practice explained." *Engng, Lond.* vol. 119 (1925), p. 657 (29 May 1925).

Mr. Alfred Hiley observed that his long association with the Author had made him realize the intimate knowledge displayed by the latter in the design of retaining walls and buildings, and he thought that the Author had not only a full grasp of static problems and reinforced-concrete design but also was not lacking in a knowledge of what was implied by the far greater field of dynamic impact. He had been astonished by many of the statements made in the Paper, and he could find hardly any points on which he was in accord with the Author's views. They were like intruding neutrons or protons in Mr. Hiley's conception of his arrangement of orderly nuclei, and he felt that he must repel them with all the force he possessed.

The Author's formula seemed to be related to the *Engineering News* formula, but Mr. Hiley, in his shipbuilding experience, had had to arrange for the launching of very large vessels and to be responsible for their safe conduct into the water. He used to say to the contracting department: "I have 2,000 tons coming on this way-end and I want it taken safely. What are you going to do about this and the *Engineering News* formula?" and they assured him that they had immense factors of safety; but he could not, on its showing, accept that. Therefore he had rejected the *Engineering News* formula as inadequate, and had considered that it should be superseded by a better line of reasoning.

The Author seemed to have pinned his faith on only a denominator manipulation to give a result that accorded with certain practical limitations which could be seen clearly from the data with which he had so lavishly adorned his work. That factor, which gave a sort of level line in Fig. 9, Plate 1 and Fig. 16, Plate 2, appeared to Mr. Hiley to partake of a manipulation from the static problems of reinforced-concrete design to arrive more simply at a load which would level out the curves, as had been done clearly and intentionally in Fig 9. It was erroneous to adopt that course, and the Author's proposed formula, with the modification to the denominator to effect his purpose, was not logical or sufficient for the use of engineers who wished to make improvements and to use steel tubes instead of precast piles—as Mr. Hiley had always done to acquire reliable data in the course of his own investigations. The manipulation of the denominator gave a result which seemed to the uninformed sufficiently complex and wide in its application to be accepted as something resulting from great insight and from adequate data collated with skill and accuracy, but he disputed that that was so; but the introduction of x times h as an additional item in the denominator, which could be set into the denominator of equation (1) in the Paper, was the only material difference which the Author had made, and that seemed very trivial and inadequate for the modern mind to accept without more support from closely reasoned causes and effects. Mr. Hiley considered that the Author was, in fact, in error in introducing such manipulation in the denominator which resulted in his desired effect.

He considered also that the Author had failed to appreciate the significance of the temporary set which characterized the elastic movement of any material set in motion in collision. It was not a case for just measuring the permanent sets and then considering it sufficient to estimate the elastic temporary set of the pile alone and where z could be left out. It had never been in his mind, apart from a careful assessment of impact factors, efficiency of blow, etc., to accept them (s and c) as alone sufficient for examining the problem. The temporary set per blow which should always be calibrated was indeed an important factor and entered into everything concerned with elastic phenomena. Wave stress effects had been dealt with in the recent " James Forrest " Lecture by Sir Geoffrey Taylor,[1] who had dealt with all the significant factors in the phenomena of impact, after having studied them with minute care, and had given information bearing upon impact forces and stress effects on which engineers should be guided when forming their views. The Paper contained nothing which showed that the Author had appreciated even the existence of any such investigation, particularly that outstanding one by D. V. Isaacs,[2] as would complete his knowledge about impact.

With regard to the Author's analogy of the wagons in a train, Mr. Hiley considered that the Author had misunderstood the problem. If the Author had confined his research to limits which could be comprehended and dealt with, he would have made his comparison with a train consisting of one engine and one wagon. That train could be run at a moderate speed into the retaining wall, and the wall would probably, if of the Author's design, be able to sustain the blow that it would receive. Measurements could then be made of the energy that was absorbed, of the velocity, and of everything else concerned. All that was required was accurate measurement. In dealing with the phenomena of impact it was necessary to be particularly careful in handling not only the compression sets, which should include the ground itself because of its elasticity, but also the properties of the pile and dolly, which were moderately well known.

Mr. Hiley considered that the Author's conception of the problem was imperfect, as was shown by *Fig. 2*, wherein was shown the rounded contour of the temporary set that was to be measured. Mr. Hiley had never experienced that squat form in practice. It was generally a distinctly peaked form—a straight up-and-down affair, connoting the rapidity of movement, and the vibratory movement invited the most delicate measurement to show its amplitude and also the timed duration and residual movement of the pile.

The Hiley formula had always taken the form :—

$$R = \frac{W.h.z}{s + \frac{c}{2}} + (W + P) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

[1] " The Testing of Materials at High Rates of Loading," J. Instn Civ. Engrs, vol. 26 (1945–46), p. 406 (Oct. 1946).

[2] " Reinforced Concrete Pile Formulæ," J. Inst. Civ. Engrs, Australia, vol. 3 (1931), p. 305.

That was first developed in *Engineering* in 1922[1], and acknowledgements for some timely help from Mr. Ewart Andrews, M.I.C.E., could be found in a continuance of the subject in the issue of that Journal dated 12 June, 1925.

A critique of that formula by Professor P. Raes of Ghent University was published in the *Annales de l'Associaion des Ingénieurs sortis des Ecoles Speciales de Gand* in 1927, In that study of the problem now at issue the French nomenclature, using M to denote *mouton* (hammer), was employed, and where a long dolly was utilized, either of which might prolong the time enough to see the hammer separate or bounce from contact with the pile before its downward movement was ended, the disposable energy would become depleted and the efficiency decreased.

That aspect of the efficiency was thoroughly investigated when considering the use of dollies, in an article by Mr. C. Percy Taylor, M.I.C.E. and Mr. B. Buxton, published in *Concrete and Constructional Engineering* in November 1937.

The Brix formula, which was a modification of the Dutch formula to take into account only the energy given to the pile, was occasionally appropriate for cases where the efficiency tended to be less than the normal.

Again, where the resistance to penetration was very slight and in the limit might not much exceed the dead weight of pile plus hammer, the adoption of the same term by the Author was seemingly to distinguish a very different motive, and especially of no tolerance for "efficiency of blow," as so obviously implied on p. 50, *ante*.

Mr. Hiley had been at pains to show that the value of z was by no means a constant amount, and in his "Pile-driving Calculations with Notes on Driving Forces and Ground Resistance," published in *Structural Engineer* in July and August, 1930, it was explained why not more than one-half of the weight of pile should be taken into account when refusal was met on the pile-point entering a hard rock bed. For that condition, therefore, the efficiency of blow might become much higher than for common driving conditions and the effective force exerted sensibly greater.

On the other hand, where long sets took place, the bracketed value of $(W + P)$ should be included when computing the resistance R.

Failure to do that in the example given on p. 52, *ante*, had led the Author to a wrong conclusion, for it could be shown that the estimate of resistance above and below the line did not differ by more than 1 ton in 16 tons resistance where the blows per foot numbered about 5.

Finally, considering the efficiency as suitable to average conditions of driving, the example quoted on p. 48, *ante*, of a 14-inch by 14-inch pile which actually carried 100 tons with a $\frac{3}{8}$-inch settlement, reference to Tables No. 32–33 in Mr. R. V. Allin's "Resistance of Piles to Penetration" would confirm the value as very close to the figure stated.

The speculations indulged in by the Author as to loadings for piles in clay appeared very questionable where calculations for the resistance by

[1] *Loc. cit.* p. 55, *ante*.

the use of the new piling formula would necessarily show discrepancies commensurate with its deficiencies.

Lastly, Mr. Hiley would like to make it plain that the value of R obtained by his formula (1) related to the ultimate resistance opposing penetration, and not to some smaller fraction of the ultimate value at which the plastic yield of the ground was comparatively small or only about to commence.

The nature of the probable normal settlement for piles in distinguishable classes of ground, subjected to percentages of loadings, clear of the commencement of plastic yield, was indicated by graphs given in Appendix III of Mr. Hiley's Papers in *Structural Engineer* in January 1938, on ' Apparatus for New Foundations " and in the Journal of The Malayan Association of The Institution of Civil Engineer, in April 1937, on " The Bearing Capacity of Foundations " (*Fig. 26* in Section III). From the latter, a suitable factor of safety could be selected without difficulty which would allow of a working load being carried without settlement, and its value could be said to range between $1\frac{1}{2}$ and 4 in meeting every-day requirements, whereas, if the Author's new formula were to be depended upon, considerable confusion would inevitably accompany the attempt to fix upon safe limits, owing to the fact that the commencement of plastic yield would naturaly range between 30 per cent. and 90 per cent. of the ultimate resistance as overcome in ground varying between soft clay and decomposed bedrock.

The Author had made a *faux pas,* but it was undeniable that all could learn from their blunders.

Mr. J. P. Porter observed that the Author had based his formula and his rejection of existing dynamic formulae upon Test (*a*) (p. 6) which stated, in effect, that if a pile was driven to a given depth in any subsoil and received blows of varying intensity, the ground reaction would be the same whatever the energy of the blow. So stated, the Author's test appeared to be directly contrary to Newton's Third Law. Mr. Porter considered that the Author's object in the first test could best be achieved by the application of a suitable factor of safety to the value of R, but such a factor of safety was not an integral part of a purely dynamic formula.

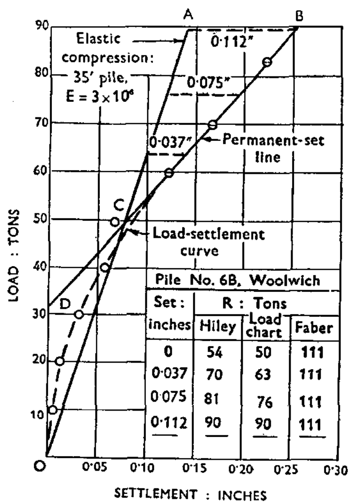
He felt compelled to accept the Author's direct challenge to the Hiley formula, as the first occasion on which that formula had been quoted in the Proceedings of The Institution was in Mr. Porter's reply, in 1927, to the discussion on his Paper entitled " Bridge Foundations on Transported Chalk." [1]

To demonstrate the validity of the Hiley formula, he proposed to use a diagram (*Fig. 18*) based on Fig. 4, Plate 1, which gave the load settlement curve for pile No. 6B at Woolwich. That was the only pile in ballast for which full driving results and full static loading results were

[1] Min. Proc. Instn Civ. Engrs, vol. 224 (1926–27, Part 2), p. 242.

given. In his calculations the Author had taken Mr. Hiley's efficiency factor η as $\frac{M}{M+P}$, but reference to Mr. Hiley's writings[1] would show that it was $\frac{M+Pe^2}{M+P}$, and that made a certain amount of difference. The Author had also taken a constant value for c, the temporary compression, whereas it was clear from Table I that c varied approximately as R. Mr. Hiley's original values for c were based on a value of Young's modulus of $E = 2{,}000{,}000$ lb. per square inch for concrete piles, but it would be more accurate to take E as 3,000,000 lb. per square inch for modern high-quality concrete. If it were assumed that $\frac{c}{2} = KR$, the Hiley formula could be re-written as $KR^2 - Rs - Wh = 0$. Using that form of equation, it was not necessary to assume a preliminary value for R, as $K = \frac{c}{2pA}$ could be calculated from Table I. For pile No. 6B, the value of K was approximately 0·003, and Mr. Porter had taken the loss of efficiency due to winch-drag as 10 per cent. Using those adjustments, he had obtained amended Hiley values of R (shown in *Fig. 18*), ranging from 54 tons for $s = 0$ to

Fig. 18.



Comparison of values of R from Hiley and Faber formulae and load-settlement chart.

90 tons for $s = 0{\cdot}112$ inch, in comparison with the values of 44 tons to 84 tons obtained if the Author's coefficients were used and winch-drag was

[1] *Loc. cit.* p. 55, *ante*, and "Pile Driving Calculations," *Structural Engineer*, July–August, 1930.

neglected. On the other hand, the Author's formula gave $R = 111$ tons at all values of s.

Mr. Porter next proposed to derive confirmation of the Hiley values of R from *Fig. 18*, in which the line DCB was a tangent to that curve and the line OCA was the elastic compression of the pile for $E = 3,000,000$ lb. per square inch. Mr. Porter suggested that the line DCB represented the settlement rate of an incompressible pile which had been loaded beyond the supporting value of the ground, represented by the point D. In *Fig. 18*, point D corresponded to a loading of 24 tons per square foot, which was the Rankine value for ballast at a depth of 35 feet if $\theta = 34$ degrees. Such a value appeared reasonable for the loose water-bearing ballast in middle reaches of the Thames estuary. The intercepts between OCA and the vertical represented the recoverable elastic strains imposed, and the horizontal intercepts between CA and CB represented the permanent sets which remained after the removal of static loading. Corresponding with the permanent sets given in the Paper for pile No. 6B at Woolwich, namely 0, 0·037 inch, 0·075 inch, and 0·112 inch, the values of R due to static loading were 50 tons, 63 tons, 76 tons, and 90 tons respectively. Those values agreed within practical limits with the amended Hiley values and, incidentally, they were at variance with the Author's value of 111 tons for all values of s. Mr. Porter considered, therefore, that *Fig. 18*, which was based on Fig. 4, supported the Hiley formula rather than the Author's formula.

With regard to the factor of safety to be applied to the Hiley formula, four widely varying values of R, the ground reaction during driving, had been derived from varying energy factors, and Mr. Porter agreed with the Author that the carrying capacity under static loading could not vary with the driving factors selected. The common practice of applying a standard factor of safety to the results obtained from driving conditions specified in accordance with the designer's judgment was obviously incorrect, and it was necessary to decide how to interpret the results obtained from the Hiley formula. If *Fig. 18* were examined, it would be found that the point C represented the largest static load which the pile would carry without any permanent set. The value R_0 of the reaction for zero set could be determined on the site from the Hiley formula if the hammer fall was reduced gradually until the pile just failed to move. For pile No. 6B, the value of R_0 was 54 tons from the Hiley formula and 50 tons from *Fig. 18*. On that basis, Mr. Porter suggested that, for piles driven into ballast or sand, the safe carrying capacity might be taken as 0·9 R_0, subject to confirmation by borings that the ballast extended beyond the toe of the pile for a depth equal to four times the pile "diameter."

Calculations based on the Author's results for piles driven into clay suggested that, pending further driving and loading tests, a factor of safety of 2 should be applied to the value of R_0 calculated from the Hiley formula. Such a factor of safety was necessary to take care of the difference between the carrying capacity in clay under conditions of static loading and the momentary driving resistance R.

One final and important inference might be drawn from the Hiley formula. The depth of driving and the value of R_0 were determined by the weight of the hammer and the new type of packing used. Therefore, the value of R_0 and the carrying capacity of a pile might be increased, where the subsoil remained uniform in character, by the use of a heavier hammer; where the subsoil could offer sufficient resistance to driving, the value of R_0 and the carrying capacity of a pile were limited only by the ability of the pile to resist the imposed driving stresses.

Mr. G. B. R. Pimm observed that the Paper contained a great deal of information from which important conclusions could be drawn. But if it were to be judged from the point of view of whether or not it fulfilled the promise contained in the title, it became necessary to ask the following three questions: (1) Had the Author produced a new formula? (2) Was the new formula, if one had been produced, a good formula? (3) Was a new formula really necessary?

With regard to the first question, in equation (15) the Author had derived a value for x, and if that value were inserted in (10), and both sides were squared and the two sides were transposed (all perfectly legitimate operations), the result would be $M.h_r = \frac{R^2.1}{2d^2E}$, or $R = \sqrt{M.h_r\left(\frac{2d^2E}{1}\right)}$. The term $h_r - \frac{d}{7}$ had disappeared. Mr. Pimm did not know why d came into that term at all, and he was sure that Members would wish to know a great deal more about the denominator 7; but it did not matter in the particular instance in question because the whole term had disappeared, and equation (10) was simply the Rankine formula for refusal. For conditions other than refusal the quantity $h - \frac{d}{7}$ appeared again, in the form of a correction for the height of the drop, and with regard to the need for that Mr. Pimm was in complete agreement with the Author.

But the Rankine formula had three defects. Firstly, it took no account of the height of the drop, and the Author had made a perfectly good correction for that in conditions other than refusal. Secondly, it took no account of the efficiency of the blow. Thirdly, it took no account of the ground quake, which, at all events in Mr. Pimm's experience, was often the most important factor of all.

Attention had already been drawn to the fact that the Author had misquoted the Hiley formula. The efficiency factor given by Mr. Hiley was $\frac{M + Pe^2}{M + P}$, whilst the Author, in his calculations by the Hiley formula, had given it as $\frac{M}{M + P}$.

The Author had asserted that there was no such thing as an alteration in the efficiency of the blow according to the weight-ratio. Had he ever seen the strong man in the market-place who lay down on his back and whose assistant then put a heavy stone on his chest, and a smaller stone on the top of it, which the assistant proceeded to crack with a sledge-hammer? Would the Author say that that feat could be performed just as well if the nether stone was a quarter of the weight?

With regard to the analogy of the railway-trucks, Mr. Pimm had never seen a pile in which the reinforcement rattled like the trucks! Mr. Hiley's analogy of the billiard-balls would do very well if both balls were running on the green cloth, but if one of them was embedded in clay, or even coated with clay, the state of affairs would be altered. Mr. Pimm considered that the defect in the Hiley formula was that it took no account of the quantity of ground which moved with the pile, and which represented in effect an increase in *P*. It was reasonable to suppose that the quantity of earth or clay that moved in that way depended upon the height of the drop, and if that were so, and if the efficiency factor were on a sliding scale, a lower efficiency would be obtained with a higher drop. That was the only correction which was necessary in the Hiley formula.

The other factor which the Author denied, and which Mr. Hiley included, was the quake of the ground. Mr. Hiley had always insisted that $\frac{c}{2}$ should include the elastic compression of the pile, the elastic compression of the packing, and the elastic compression of the ground. The last had been completely ignored in the Author's formula, and might well be the greatest of the three.

Nevertheless the Author had asserted that his formula was right because it agreed with the loading tests, but Mr. Pimm considered that so far from proving that the Author's formula was right, that proved most conclusively that it was wrong.

Mr. Pimm believed that he was one of the first investigators of the take-up. *Fig. 19* (reproduced from a Paper presented by him to The Institution in 1912)[1] depicted the records of the driving of three piles. No. 3 was driven without any break, and gave a smooth curve. In No. 2 there were two short breaks, of a few hours, one just below 25 feet and another lower down, and the effect of the stoppage, in each case, was very marked. In No. 1 there was a much longer break. The pile had to be lengthened and after an interval of 4 months, when driving was resumed, a still more marked difference appeared in the number of blows per foot.

Fig. 20 illustrated a case in which the object was to ascertain the take-up. On the left of the diagram was the toe resistance, which was very

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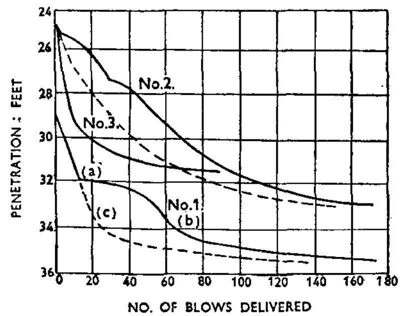
[1] "The Making and Driving of Reinforced-Concrete Piles," Min. Proc. Instn Civ. Engrs, vol. clxxxix (1911–12, Part 3), p. 314.

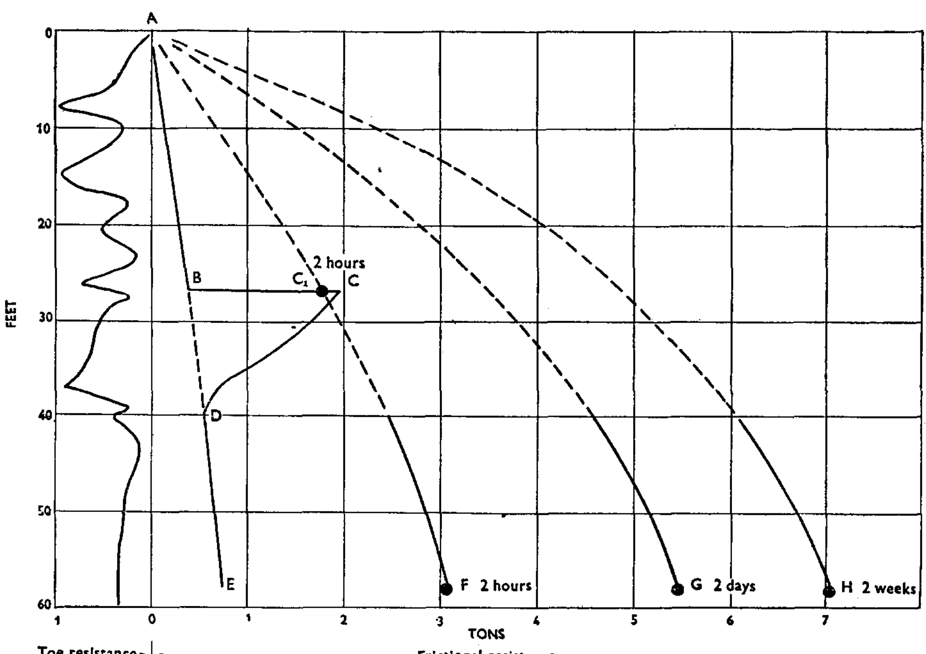
small. The curved line A–E represented the frictional resistance recorded in driving. The portion between B and D was not the actual drive, however, because at B driving was stopped, and after an interval of 2 hours a pulling test was made, the result of this test being shown at C_1. Driving

Fig. 19.



PENETRATION CURVES.

Fig. 20.



was then immediately resumed, and at that point the recorded value of the frictional resistance, as calculated by the Hiley formula and as shown at C, agreed closely with the pulling test. Driving was continued and, as would be expected, the increased frictional resistance due to the stoppage was soon broken down, the remainder of the drive being shown by C–D–E. The drive being completed, a pulling test was applied after an interval of 2 hours, a second after an interval of 2 days, and a third after an interval of 2 weeks, the results being shown at F, G, and H respectively.

Mr. Pimm believed that take-up occurred in any material. However, that was not important because piles were not usually driven a great distance into ballast or sand; they were driven to the ballast or sand, and a short distance into it, but through ground which might be made up ground, soft clay, mud, peat, silt, etc., and it was that material which provided the take-up. He considered that the agreement between the Author's inflated figures for sand and gravel and the loading test was due entirely to the fact that between the two tests a take-up had occurred.

He considered it would have been better if the Author's efforts had been directed towards the necessary improvement of the Hiley formula rather than to the production of a new formula.

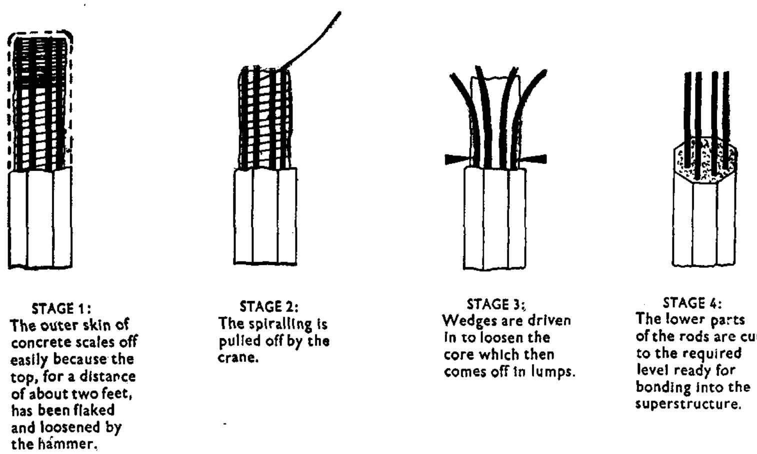
With regard to the question whether the Author's formula was a good formula, he thought that, like the curate's egg, it was good in parts. It was certainly bad in two places, namely the top and the bottom.

With regard to the question as to whether a new piling formula was really necessary, Mr. Pimm considered that there might be a danger of over-rating the value of the impact test as applied to the actual piles. In addition to the important point made by the Author, as to the time element, particularly in clay, and to varying and uncertain degrees in other materials, and the further effect of take-up, the load which could be sustained by an individual pile, however accurately ascertained, might have little relation to the load that could be safely applied to each pile, regarding the piles as units in the foundation as a whole. The effect of grouping and spacing, the thickness of the bearing stratum to which the piles were driven, and the character of the underlying strata, all tended to discount the value of the impact test on the individual piles, and all those factors, with others, had in any case to be taken into account. The most satisfactory course would appear to be to explore the ground conditions, by means which might include an impact test by special testing apparatus. Having in that way located the bearing stratum, and ascertained its bearing value, the piles could then be driven to such depths as the judgement of the engineer might indicate as suitable, having regard to the conditions disclosed.

Mr. R. D. Brown observed that the Author had informed him about a year ago that he was trying to make a new formula for pile-driving. He had expected something very good, and he had not been disappointed. He considered that the Hiley formula had always been of doubtful value, because of the reliance which had to be placed upon the values of c, which ranged from very small to very large. The authority behind Table I had never, to his mind, been entirely satisfactory. He was satisfied that the Author's formula was an improvement.

He suggested that The Institution should appoint a committee to standardize many things in connexion with piles, such as the pile itself, the method of testing, the method of loading, and so on. Members should supply information to the committee on those points, and a very large number of test loads should be accumulated, which should ultimately enable the committee to draw up a table of known safe loads in relation to sets. A very simple test load or method of testing should also be standardized. Very little reliance would then have to be placed upon any formula.

Fig. 21.



Standard Octagonal Piles used in Bare-Headed driving showing method of Stripping.

The first step in simplification was the pile itself, and *Fig. 21* illustrated the top of an octagonal pile, which had been first used in a work in which Mr. Brown had been engaged in 1910. Since then he had driven many thousands of that type.

The first blow knocked off the outer skin, which ultimately peeled off down to about 2 feet; but that 2 feet had been added to the pile in manufacture, and so it became the cushion or dolly and thus eliminated a number (seven or eight) of the variables which entered into any formula.

In good practice all squares in concrete had splayed corners. If the corners were splayed sufficiently an octagonal pile would result, the spirals of which contractors should have no difficulty in making by a machine. Octagonal piles with spiral reinforcement could always be made more cheaply and more quickly than any other form of pile.

The Institution might standardize the pile and the method of loading it or testing it, and should publish ultimately a very considerable number of results which should be fitted into the Author's formula, which was very simple and straightforward. There were at least thirty-three variables in piling and, as there were only twenty-six letters in the English alphabet, assuming the possibility of devising a formula which would embrace all those variables it would thus be necessary to borrow letters from the Greek alphabet! He suggested that it was beyond the power of the human mind to invent such a formula.

Mr. H. Donovan Lee observed that it did not seem to have been established that Mr. Hiley's formula was substantially incorrect; in fact, he believed that the reverse was possible from a brief calculation of some of the comparisons.

Some confusion was apparent in the Paper between the resistance to penetration and the ultimate supporting value. The value of r, as determined in the Paper, was the ultimate supporting value, but in the Author's notation it had been called the resistance to penetration. The difference might be either quite small or enormous, as had been shown by Mr. Pimm.

The Author had proposed two rather important tests of his piling formula, but Mr. Lee considered that they should not be accepted too readily, or that the Author should not feel certain about them, until they had been considered further. Test (b) should not be applied at all to cohesive soil, and some examples which had been quoted made it appear doubtful whether test (a) was really fair.

It was a pity that anyone should bring forward to-day the Dutch formula, which was understood to have been in use in 1820, and attempt to show how unsuitable they were for conditions for which the inventors could hardly have expected them to be used. Those formulae had been derived for the driving of timber piles to fairly large sets, with fairly large hammer-drops, but the Author had applied them to concrete piles and, in one or two cases, to drops of only 12 inches.

It should not be overlooked that the popularity of the *Engineering News*, or Wellington, formula in the United States in the past (and it seemed to be fairly popular still) had been due largely to its suitability for timber piles driven under conditions very different from those in the Author's example (b).

Mr. Lee agreed that one disadvantage of the Hiley formula had been the apparent need of forecasting the driving stress and the need to use tables to evaluate the temporary compressions. It was not sufficiently realized that it was not necessary to use tables in applying the Hiley formula. A few years ago, Mr. Lee had evolved the following formula (not previously published) which could be adapted to represent the Hiley formula directly, and the approximations which were made were largely self-compensating :—

$$R = \frac{M^2h}{\left[S + \left(\frac{Mh}{A(3s+1)}\right)\right]\left[M + QP\right]}$$

There was nothing empirical about the formula : it was essentially the Hiley formula in a form in which Mr. Hiley might not have seen it before.

With the same objective, Mr. George E. Thompson, A.M.I.C.E., had recently proposed the formula :—

$$R = \frac{Mh}{\left(1 + \frac{P}{M}\right)\ \left(S + \frac{C}{2}\right)}$$

where $\frac{C}{2} = \frac{1}{4}$ inch $=$ Constant.

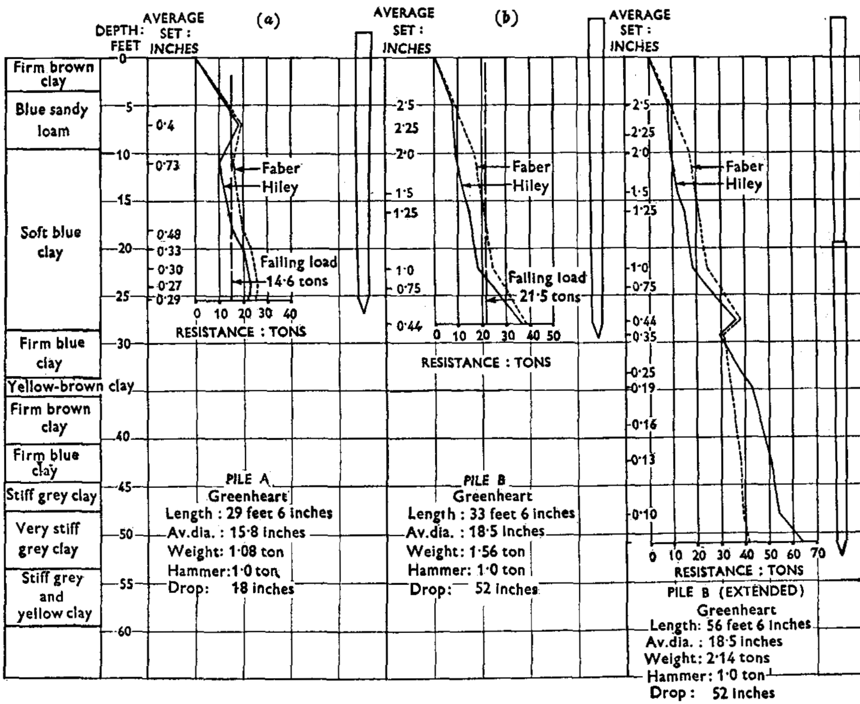
Mr. Lee considered that the analogy of the impact of a locomotive with a goods train was rather difficult to reconcile with what was known of the speed of the stress-wave travelling down a pile, and he agreed that a better idea was given by the suggestion of only one truck being used. By imagining a fairly well fixed buffer stop, and that the locomotive arrived at about 20–50 miles per hour, a better idea might be obtained.

It was difficult to believe that adding a long dolly which, in the case in question, was virtually a follower, did not reduce the efficiency of the blow. Mr. Lee believed that it was common knowledge that it did reduce the efficiency of the blow, and he had never heard anyone suggest that it did not do so. There must, he thought, be another explanation.

In the Paper, the resistance to penetration appeared to have been confused with the safe supporting value and much recent work on soil mechanics suggested the need for caution in that respect. A pile might be driven to a good set and a test load imposed for a short time. Everything might be all right then, but the position might be very different within three or five years. He thought that the Author would agree with that, and that some very important qualification was needed in the Paper with regard to using the impact piling formula to estimate the permanent safe loads of piles, either on clay or bearing in strata overlying clay.

Mr. Maurice Nachshen observed that *Figs. 22* (*a*) and (*b*) related to two round greenheart piles driven in very soft clay. The full line on each graph was the plot of the resistances by the Hiley formula, whilst the dotted line showed the resistances by the Author's formula. For Pile A, the Hiley formula gave a final resistance of 22 tons and the Author's formula 25 tons, of which 5 tons was the frictional increase or take-up. The pile failed at a load of 14·6 tons, which was rather disquieting, as it was less than two-thirds of the result shown by either formula. Pile B was longer and of greater diameter. By the Hiley formula, the resistance was 36 tons and by the Author's formula 38 tons, of which 6 tons was the frictional take-up. The failing load was 21·5 tons—less than two-thirds of the value shown by either formula. The record showed that the tests had been carefully made and the piles had carried nearly all the failing load with comparatively little settlement

Fig. 22.



—in the first case $\frac{1}{3}$ inch and in the second case $\frac{2}{3}$ inch—and had then gone down very quickly. There was no question but that they failed.

In each case, instead of a frictional take-up or increase there had been a falling-off, and Mr. Nachshen believed that the resistance that had been built up at the toe of the pile during driving had dissipated itself during the time taken in testing; evidently it was necessary to be very careful about piles driven in soft material and not tested by loading.

After pile B had been tested, 6 inches was cut off the top, another length of timber was put on the top of the pile, and the driving was continued. The results were shown in *Figs. 22 (c)*. The upper half of the driving diagram was a reproduction of the middle diagram. The new driving started at the kink. The resistance, as calculated by either formula, decreased on redriving by 7 tons, but that was small compared with the difference between the calculated resistance after the first driving and the resistance shown by test loading. That difference was 14·5 tons (Hiley) or 16·5 tons (Faber).

From the kink downward the results given by the two formulae diverged more widely. The Hiley formula was more sensitive to changes in set and seemed to reflect the increased strength of the ground penetrated. It showed an increase of 35 tons (29 to 64 tons) in the further 22 feet of driving, whereas the Author's formula showed only a small gradual increase of 10 tons (31 to 41 tons). That did not seem to be logical, because to drive that further 22 feet down required 2,920 blows of a 1-ton hammer falling 4 feet 4 inches, as compared with 222 similar blows required to drive the original pile B down to 28 feet; yet only about 10 tons had been added to the resistance. It was unfortunate that the redriven pile could not be tested. He had found that the value of p that would make the Author's formula fit the test results was about 8. Perhaps, instead of having one value of p, it would be necessary to have a series of values for different clays. A hint to that effect had been given in the Paper.

Mr. Nachshen had also applied the two formulae to a number of piles driven to ballast and for which loading tests had been made. In the average, the Author's formula gave values slightly higher than the test loads, whereas the Hiley formula always gave a value slightly lower; the difference, however, was only about 10–15 per cent.

He wished to plead for a standard form of loading test. It was very difficult to determine the failing load, and some standard form of test and method of interpretation would be very desirable.

He considered that the Paper was a very valuable contribution to knowledge of how to interpret pile-driving results.

*** **Mr. A. S. E. Ackermann** observed that in his experiments with clay he had found that the water-content of clay had an enormous effect on what he had termed the pressure of fluidity of clay. (The pressure of fluidity of plastic metals had also been found.) For example, with London clay, when the water-content was 26 per cent., the pressure of fluidity was 1·6 kilograms per square centimetre, but when the water-content was reduced to 22 per cent., the pressure of fluidity was increased to 7·2 kilograms per square centimetre—or four-and-a-half fold. Therefore he could not see how any formula which did not take the water-content of the clay into account could be satisfactory. No reference to water-content had been made in the Paper.

Instead of the arrangement shown in *Fig. 2*, the Author might have

*** This and the following contributions were submitted in writing.—SEC. I.C.E.

used Mr. Ackermann's pile-set gauge (not patented !) which had been described in the *Journal of Scientific Instruments* in about 1923. By its means a completely automatic curve could be obtained and the time calculated.

The " take-up " in Mr. Ackermann's experiments was found to be affected by the foot of the pile. If that were flat, the clay squeezed out sideways and tended to leave contact with the side of the pile (cylindrical) and thus the load supported was comparatively low ; whereas, with a long point to the pile, the clay was displaced more gradually and therefore remained in more intimate contact with the side of the pile.

Take-up was also affected by the length of the pile. Supposing the pile to be 20 feet long, then when it was completely driven the top (say) 4 feet of clay had been enlarged and rubbed smooth by 16 feet of the pile having been pushed through the 4 feet. If the pile were 40 feet long the top 4 feet would be still more enlarged and smoothed.

The Author had stated (p. 18) that the 2-inch square pile had a foot 3 inches square and $3\frac{3}{4}$ inches long. That was almost the same as the disks of duralumin which Mr. Ackermann had used, except that the thickness of the latter was only a few millimetres in order to eliminate all friction. Was the length of the pile-foot made $3\frac{3}{4}$ inches to act as a guide and prevent lateral movement ? If so, by making the plane of the disk truly at right-angles to the axis of the pile, no such wandering of the pile would occur. Otherwise the length of $3\frac{3}{4}$ inches seemed to be a complication.

In Mr. Ackermann's five Papers (1919–1923) to the Society of Engineers (Inc.). he had used the term " monkey ", but he had since learned that " hammer " was the better term, and that " monkey " referred to the trip gear which released the hammer. Hence the expression : " Don't monkey with that " (as it would cause something to happen).

Mr. J. S. Henzell observed that the Author had asked why a piling formula should not give the same result, or ultimate resistance, for different drops, and why one particular drop should be right and all others wrong. Mr. Henzell suggested that the higher the drop, and the greater the impactive differential above refusal, the greater was the accuracy of the rational piling formula (other qualifications, such as strength of the pile, permitting) and the further question could be asked : what happened when the drop fell to refusal ?

Ignoring the obsolete piling formulae quoted, the Author's comparison was with the Hiley formula. He had pointed out that his denominator was in the form *s* plus *xh*, and had stated that no other formula was in that form. But, ignoring the " efficiency of the blow," the denominator of the Hiley formula was also in that form, in effect if not in terms. The merit of the Hiley formula was that it allowed for the (effective) elastic compression of pile cap, pile, and soil, to a degree depending on the effective blow, which was clearly conditioned by the drop. The allowance, realistically measured on a pile and not just abstracted from an arbitrary tabular handbook, as was so often the case, covered most of the intangibles, such as localized elastic hysteresis, in the pile head which affected stress rather than resistance. Not that the localized stress was not important. Some engineers, in fact, drove piles to the limit of strength and ignored formulae for resistance, preferring to trust tests for proof of strength, or intuition.

The Author refused to recognize an "efficiency of the blow" and consequently, the weight of the pile did not appear in his formula. In that he was certainly consistent. But his only direct evidence was the experiment, reported on p. 52, *ante*, of a follower driven at Blackpool. There, it was stated, a reinforced-concrete follower, by reducing the "efficiency of the blow," should have reduced the driving set also, but did not. Of that experiment, questions should be asked. For 10 feet running the set was given as five blows per foot—exactly five. Were the sets measured "foot by foot," a foot at a time, or by an overall check from zero, exactly fifty blows for the 10 feet ? If the "nearest" round figure, a foot at a time, was noted, was the change with the follower more like 5·4 to 4·6 ? Was the Author present on the site ? The drop had not been given, but was presumably small.

Certain compensating factors might have operated to offset the expected diminution of set when driving with the follower. As the Author had stated, the first pile was driven with a timber dolly which smashed, the dolly cushioning the blow, but the follower pile, used instead of a dolly, had no such cushioning effect, receiving the full blow. Through the soft ground, it is possible that the addition of the follower, bringing the combined pile weight to 3 tons per square foot of ground area, had some effect in bearing. In driving followers for timber piling on one occasion, Mr. Henzell had noticed a definite diminution of set, using a light hammer, but he had no recorded evidence to suggest whether or not the diminution was as much as would be indicated by the momentum theory.

Considering the case of two circular piles under the same hammer and drop in the same site, both 15 inches diameter and 50 feet long, one of reinforced concrete and the other 15-inch OD steel tube $\frac{3}{8}$-inch thick with a closed end, the first weighed 4·2 tons and the second 1·3 ton. Shape and volume were identical, and skin friction was not very different in that case. Did the Author believe those piles would drive to identical sets ? And what of the popular American steel pile, driven without a mandrel, of 8- or 10-gauge sheet fluted to offer strength against handling and driving, and weighing not much more than half a ton for the above dimensions. (That type was generally tapered, but the comparison held.) Would those three piles drive to identical sets under the same hammer and drop ? American opinion was emphatic that they did not. That, however, had been rather taken for granted. With the Author equally emphatic, the answer to that question should be thoroughly checked, and American experience offered the widest range of evidence, particularly with so many long steel piles driven in follower sections, both tube and H.

The Author's analogy of locomotion, *à la* goods-wagon, in the blow upon a pile-cap could not be accepted without proof; the Paper did not mention steam and pneumatic hammers, with their dynamic driving characteristics.

Although the Hiley formula originated in Great Britain, where pile-driving was limited in scope and magnitude, yet it had gained wide recognition in the United States, where piles of every kind, in single lengths up to 200 feet and 35 tons in weight, were driven with more varied equipment under far more difficult conditions. But the Hiley formula should not be applied blindly, with tabular insertions from a text-book. It should be used with understanding in each practical case. It required more skill in use than would be expected from the engineer who only occasionally drove piles. For him, certainly, the Hiley formula was dangerous, as were all pile-driving formulae, and a simpler form was necessary for occasional use in the average range of British conditions dealt with in the Paper. For those it was possible that the Faber formula really predicted the ultimate driving resistance, and on that basis it should be tested.

Driving resistance, however, was not necessarily the value of a pile, nor was test load the measure of safe bearing of a single pile, much less of a pile-group.

Mr. G. P. Manning observed that four methods of approach to the problem of assessing the safe bearing capacity of a driven pile were available, namely :—

(1) Driving by impact, measuring the effect produced by the blows and interpreting that in terms of a formula—Saunder's rule, the Dutch rule, the Hiley formula, the Author's formula, or some other formula. The Paper was mostly confined to that one aspect.

(2) Driving by impact, but relying on past experience and using a hammer formula (which was really a restricted or inverted piling formula).

Example: Considering a 12-inch by 12-inch by 30-foot reinforced-concrete pile in ground known from experience to be good enough for 45 tons safe load, first a suitable monkey —2 tons—and a suitable drop—3 feet—are selected. Using the Dutch rule, a set of $\frac{1}{8}$ inch is found for ten blows.

A formula had been used, but had been restricted within narrow limits within which it was known to be reliable.

Mr. Manning did not know how many piles of that type his firm had made and driven in the past 30 years, but they numbered possibly 100,000.

(3) By following the ideal conception of using bigger and bigger

monkeys until a monkey (or hydraulic jack) equal to the static resistance was obtained which would push the pile down gently without impact. There were dangers and difficulties to be overcome, but a move in that direction was long overdue. In fact time was being wasted in developing piling formulae when it was the pile-driving plant that should be developed.

How did the Faber formula face up to that aspect ?

$$\frac{R}{M} = \left(\frac{h - \frac{d}{z}}{s + xh}\right)$$

When M increased and approached the value of R, h and s would approach zero.

$$\frac{R}{M} \text{ (in limit)} = 1$$

but
$$\left(\frac{h - \frac{d}{z}}{s + xh}\right) = \left(\frac{O - \frac{d}{z}}{O + O}\right) = -\infty$$

Thus the formula broke down in the limit, but it might be approximately correct over some range of practical values.

(4) By pure soil mechanics. If a pile were pushed or driven down through specified strata of soil, was it possible to calculate the path of escape of the soil particles which were displaced, and to calculate from the measured physical properties of the soil what the static resistance would be at any depth ?

In the present state of ignorance it certainly was not possible to arrive at any exact figures, but contemplation of the problem from that angle would greatly help towards a general understanding.

The Author had the rudiments of that idea as he had suggested different formulae for clays and gravels, but Mr. Manning would go farther and suggest a different assessment for all types of soil—clay, silt, fine sand (wet or dry), coarse sand, fine ballast, coarse ballast, and mixtures of those soils.

Most of the disagreements and arguments over pile-driving and pile-testing were due to the fact that individuals had restricted their viewpoint to one, or at most two, of those aspects.

A balanced estimate of safe bearing capacity and allowable bearing pressure would be arrived at only by approaching the problem from all four aspects.

Groups of Piles.—The major loads in any large structure were not carried on single isolated piles, but on groups of piles. Therefore it was essential that any piling formula for general use should indicate the combined strength of a group.

Denoting by R_n the combined strength of a group of N piles,
by N the number of piles in the group, and
by R_1 the strength of one pile,

the following expression could be derived :—

$$R_n = N \times R_1 \times f_n,$$

where f_n denoted a factor depending on the type of the pile, the type of ground, the spacing of piles in a group, etc.

Any piling formula which did not include a value for f_n failed to solve the major problem of supporting a large heavy structure.

When driving groups of piles in plastic clay, not only did the ground rise, but also it pulled up with it the piles already driven. Ground-heaves up to 2 feet 6 inches high had been observed. If that ground movement were imagined to be entirely deep-seated all was well, but some of it took place during the early stages of driving the last piles in the group and, if that meant that a shallow ground movement was gripping the heads of the driven piles and pulling their points up several inches above the point to which they had been driven, the phenomenon could not be regarded with equanimity and a low value of f_n was indicated.

Mr. Manning considered that the Author's formula was deficient in several vital factors and was unlikely to be accurate except over a limited range of site conditions.

The Author had claimed that "no other formula is in this form." The Romans drove piles and, as they obviously knew more about masonry and mass concrete than most modern "experts," it was only reasonable to assume that they had some criterion to which they drove their piles. Therefore research over many centuries, in many languages, both live and dead, would be needed before that assertion could be established.

Mr. Savile Packshaw observed that the Author had rendered a service to the engineering profession by drawing attention to the difficulties inherent in predicting the bearing capacity of clay from a dynamic pile formula, but he was not at all sure whether such a formula could be adapted to allow for the phenomena which the Author had described as plasticity and take-up. If, however, an empirical change was to be made, it should be applied to the set, so that it would correspond to the ultimate reduction in the strength of the ground, and not the xh term in the equation. Although the Author had not said so, the xh term had to be assumed to take account of the energy lost in the temporary elastic compression of the pile. A great deal of caution and a thorough knowledge of the ground conditions would, in any case, be necessary in assessing the value of the plasticity and take-up coefficients. In some types of clays, particularly soft clays, the increase of side friction or adhesion with time might be negligible, so that there would be no take-up ; on the other hand, there might be a considerable reduction in the resistance of the ground at the foot of the pile. When the pile was first driven part of the stress was carried by the water which filled the voids in the clay and had not had time to escape. When the water-content had had time to adjust itself to the new conditions, the whole load had to be borne by the soil particles, which might be incapable of doing so. Cases had occurred of piles driven into soft clay which carried a modest load quite well for a fortnight and then suddenly settled a couple of feet. Obviously there was no take-up there but, on the contrary, a considerable take-off.

In such circumstances, there was no alternative to a properly conducted test-load. Some engineers held the view that a pile formula should not be used even in hard clays ; it had even been advocated that pile formulae should not be used at all. Mr. Packshaw did not agree with those sweeping assertions, provided, of course, that a rational formula, such as Hiley's, were used and were checked by a test-loading whenever practicable. The other formulae cited by the Author, and another twenty or so which Mr. Packshaw could mention, hardly came into consideration, as they were crude and empirical and often incorporated arbitrary factors of safety which made them neither rational nor reliable.

The Author had rightly drawn attention to the apparent inconsistency of the Hiley formula when applied to different hammer-strokes, and had pointed out that the temporary compression was a function of $\sqrt{h}$. Mr. Packshaw assumed that the term xh in the Author's formula represented the temporary compression, and in that case he considered that it should be proportional to $\sqrt{h}$, and not to h. He realized that that would upset the perfection of the arithmetical agreement which the Author had achieved, but he thought it would be done without introducing errors greater than those which were inseparable from any pile formula known at present. Moreover, the inconsistencies of the Hiley formula were not as great as might appear at first sight. Graphs of the bearing capacities given in Tables V and VII showed that they reached a maximum when h was about 4 feet, and that there was very little variation, amounting to only a few per cent. over the whole normal range of hammer-strokes—namely from 3 feet up to 4 feet or 4 feet 6 inches. Strokes of 12 inches or 24 inches were of little practical interest. Moreover, he believed that those Hiley coefficients which were determined experimentally were also the results of tests within that range of strokes. Once again, the error might be regarded as being no greater than the uncertainties resulting from the basic assumptions of the formula.

Mr. Packshaw could not follow the Author's reasoning in the analogy of the railway engine striking a stationary goods train. Nor could he agree that the efficiency of the blow—the effect of the relative hammer and pile weights—should be discarded altogether. He admitted that in some respects the efficiency of the blow was a source of contradiction in the Hiley formula. For instance, imagining two piles driven to refusal through soft clay and into a hard layer of ballast under identical conditions, the piles themselves possessing precisely the same elastic properties and dimensions, but one made from a material very much lighter than the other; the lighter pile would penetrate farther per blow during its passage through the soft clay, but when it stopped in the hard ballast it would have nearly enough the same carrying capacity as the heavier pile, whereas by the Hiley formula there should be a considerable difference. On the other hand, if a pile had reached refusal notwithstanding a very long hammer-stroke, it could nearly always be restarted and driven down to a greater depth and to an increased resistance if a larger hammer with a moderate stroke were substituted. It would have been interesting if the Author had carried out such a substitution on one of his contracts. Mr. Packshaw did not regard the evidence cited on p. 52, *ante*, as entirely conclusive, as the pile was, in effect, being driven by a very long dolly. Perhaps the importance of the efficiency diminished with a decreasing set. So far as the Author's numerous load-settlement tests were concerned, it was, unfortunately, very difficult to estimate the ultimate resistance from graphs of that nature, as they very rarely showed a well-defined maximum. Some of the piles would, however, have carried greater loads if the tests had been continued, whilst others were already settling badly under constant loads left on overnight. Similar graphs on a time basis, each increment of load being allowed enough time to develop its final settlement, would have been helpful.

Other points that had attracted Mr. Packshaw's attention were the small variation in ultimate resistance which the Author's formula gave for piles driven to refusal with a very wide range of stroke; the examples on p. 27, *ante*, showed an increase of only 10 per cent. when the stroke was doubled and of 15 per cent. when it was quadrupled. The Author had also shown (p. 39, *ante*) that his formula was influenced very little by the drag on the free drop of the hammer. If the Author's example were worked out for a hypothetical case in which the drag reduced the effectiveness of the blow by 50 per cent., it was found that the reduction in resistance was only 14 per cent. It would be more logical to reduce the hammer-mass, and not to interfere with the drop. In advocating a reduction of the stroke, Mr. Hiley probably had had in mind a reduction in kinetic energy.

The road which was being followed in endeavours to predict the bearing capacity of a pile was as old as the history of civil engineering. Unfortunately it had been the same road all the time, and the basic principles of most of the formulæ had remained unaltered. Mr. Packshaw had felt for some time that the Hiley formula had taken engineers as far as could be expected from any dynamic pile formula, and he still considered that, if its limitations for silts and soft clays were recognized, it would give useful and practical information in the great majority of cases. He sympathized with the Author's statement that it had taken him years to free his mind from preconceived ideas; but if a new pile formula were to be evolved, some entirely fresh line of approach appeared to be necessary. It might help if the fact were recognized that bearing capacity was something determined by the ground rather than by the pile, and if engineers were satisfied to treat the pile merely as a column of homogeneous material which transmitted the load to the ground. That was all a pile needed to do, and it was all it could do.

Mr. Guthlac Wilson observed that he agreed with the Author's statement (p. 6) that all known formulae gave different results when applied to the same case, but he had regretfully concluded that the Author's formula had to be included in the same class as all the others. That was not unnatural, as there was no reason why the resistance of a pile to dynamic blows should bear any particular relation to its resistance to a static load. But for that inconvenient fact, he would agree with the Author's criterion that the formula should give the same result independently of the drop chosen.

No exception could be taken to the Author's second criterion that the calculated result should accord with static test loadings. It was, however, necessary to have a clear mind as to what was the criterion of failure in a static test loading. It did not seem that any of the tests reported by the Author had been carried to failure. A loading test on a pile should be conducted in the following manner :—

(i) The load should be applied in suitable increments.
(ii) No increment should be added until the pile had ceased to settle under the effect of the previous increment; that should be checked by the plotting of a time-settlement graph.
(iii) A graph of ultimate settlement versus load should also be plotted, that was to say, the value of the settlement at which the pile became stable after the application of any increment should be plotted against the total load then carried by the pile.

Mr. Wilson suggested that failure should be defined as the load at which the graph of load versus ultimate settlement became parallel to the axis of the settlement. For practical reasons, since great settlements could not be tolerated, the load at failure might be considered to be the load which caused a settlement of 20 per cent. of the diameter of the pile.

The Author's small oak test-pile at Dolphin Square carried 1·9 ton, or 80 per cent. more than his formula value on a settlement of only 0·15 inch. At that load, there was no sign of the load settlement graph becoming parallel to the axis of settlement and the total settlement was less than 10 per cent. of the diameter of the pile.

In his other tests, the Author appeared to have been satisfied if a pile loaded to the value given by his formula did not fail. If he had wished to prove anything, he should have loaded all his test-piles to failure.

With regard to piles founded in ballast, the Author's formula, like other formulae, would provide a useful field method of obtaining rough uniformity of resistance. It seemed possible that it might, in addition, indicate a lower limit for static failure, and that would be useful, if not always economic.

For some time it had been recognized by those who had made a special study of the problem of piles in clay that the point driving resistance bore no relation to the static point resistance and that the former was much the greater, whereas the side friction during driving was much less than the frictional resistance to static loading. The Author's convenient colloquial terms, " dash-pot effect " and " take-up," would be of assistance to the general understanding of those points. There was no reason to believe, however, that the division of static resistance between the point and the side friction would be as stated by the Author. Mr. Wilson considered it probable that the point resistance would be considerably less and the side friction considerably more. A better means of estimating the bearing capacity of a pile in clay would be to determine the shear strength of the material in the remoulded condition and to apply that to the surface of the pile.

The Author, in reply, observed that he was sorry that his old friends Ewart S. Andrews and Gower Pimm had found no merit in the Paper, or had expressed none. He had hoped that his collection of data on piles of such different weights, sizes, and materials, driven with monkeys of such different weights and drops, some into sand or ballast and some into clay, might at least be deemed to have added to knowledge, and would be of value for the purpose of examining the Author's or any other formula, old or new, and even for helping to establish the perfect formula of the future.

For, just as Hiley's work represented a milestone in the development of piling knowledge, and possibly the Author's contribution added something further, yet development and learning would, the Author hoped, not stop there, but would go on for ever—and in that development he hoped his record of carefully conducted tests and records might find its sphere of usefulness.

The matters on which the Author differed from Mr. Hiley's valuable formula were really quite few and simple, and it should be possible to consider those in a spirit of scientific calm in which the only thing concerned was elucidation of the truth. Whether the result was a new formula or a correction or development of an earlier one was a question which the Author would leave to Mr. Pimm.

There were two ways in which that question could be approached. The first was to devise a formula from first principles. But that implied a complete and accurate knowledge of a great many things, of which the following were only a few :—

(*a*) How much energy is lost in heat at the pile-head ?

(*b*) How much is lost in friction on the pile sides as the compression wave travels to the point and back again ?

(*c*) How much is absorbed by causing movement and consolidation of soil particles at the foot, whether in clay, or sand, or other soil ?

(*d*) How much is lost in the packing ?

(*e*) How much is lost in hysteresis in compressing and releasing various portions of the pile consecutively ?

The Author's view was that knowledge of all those matters was inadequate to enable the problems to be dealt with from first principles and that it would be better to approach the problem with a completely open mind and a mass of experimental data and let the latter speak for themselves, confining one's self to finding a formula which seemed to fit the data best.

That was the more necessary in that particular science, because the efficiency of driving was frequently very low. Clearly the energy input was Mh and the useful work done was Rs.

Taking, for example, the Siemen's pile referred to in Table XVI, and assuming for the moment that the Author's estimate for R of 111 tons was near enough for that purpose : then

h drop inches	s set	R tons	Mh energy input inch-tons	RS useful work	Efficiency of driving $\frac{Rs}{Mh}$: per cent.
48	0·1125	111	96	12·5	13
36	0·075	111	72	8·33	11·6
24	0·0375	111	48	4·16	8·7
12	0	111	24	0	0

If the Author's estimate of 111 tons was considered too high (as some contributors had suggested) the efficiency would be even lower.

It seemed from that that when sets were small the losses might easily amount to 90 per cent. of the energy output, and it therefore became very dangerous to attempt to estimate the losses under items (*a*) to (*e*) (and many others) from first principles, and deducting them from Mh, arrive at Rs, and (knowing s) at R.

In those circumstances, the Author considered that the safer way was to estimate R by static tests, and then to find a formula that agreed with it, not presuming to say whether the term to be added to s should be independent of h (Hiley) or vary with h (the Author), or vary with $\sqrt{h}$ (as suggested by Mr. Packshaw).

The Author discovered, in fact, that if the term to be added to s were made to vary with h, a consistent result was obtained ; if it were made to vary with $\sqrt{h}$, the resultant value of R would not be independent of

h; and if it were made constant (as in the Hiley formula) the latter effect was accentuated.

The Author was much too modest to attempt to answer the question *why should it vary with h;* he confined his statement by saying that in all his tests, it was clearly shown that the answer could be made consistent only if the term to be added to s_1 varied with h. The problem obviously included immensely complicated ones if tackled from first principles, but quite simple answers were obtained if the tests were allowed to speak for themselves with no *a priori* knowledge of what they *ought* to say. The chief matters on which the Author differed from the Hiley formula as he understood it were:

(1) He differentiated between clay and ballast, whereas, as far as he could see, the Hiley formula and tabular values did not so differentiate.

(2) He considered that Hiley's term $\frac{C}{2}$ should vary with h, and be independent of the hardness of driving, whereas from Hiley's table it was clear that Hiley considered that it should vary with the hardness of driving and not with h.

(3) He considered that the Hiley "efficiency of blow" term $\left(\frac{M}{M+P}\right)$ or $\left(\frac{M+e^2P}{M+P}\right)$ overstated the effect of P.

Considering those in detail. In regard to (1), was it not the universal experience that piles driven into ballast would stand a far greater load than piles founded on clay?

In regard to (2), Mr. Pimm had stated that "the Rankine formula took no account of the height of the drop, and the Author had made a perfectly good correction for that in conditions other than refusal."

Mr. Packshaw had also stated: "The Author had rightly drawn attention to the apparent inconsistency of the Hiley formula when applied to different hammer-strokes." The Author gratefully acknowledged that small measure of appreciation.

In regard to "hardness of driving" (see the Hiley table reproduced as Table I in the Paper), Dr. Glanville had stated (on p. 6 of his Paper [1]) that:

(4) "For long piles, or for short ones with light hammers and stiff cushions, the maximum stress at the head is dependent only on the conditions at the head and *is the same for all sets*."

That was to say, it was independent of the hardness of driving.

With regard to (3), Hiley himself had given the value of e as 0·25 for timber piles or concrete piles with helmet; whence $e^2 = 0{\cdot}0625$.

Considering the case when $M = P$; then

[1] *Loc. cit.*, p. 11, *ante*.

$$\frac{M}{M+P} = 0{\cdot}5$$

and $$\frac{M + e^2P}{M+P} = \frac{1 + 0{\cdot}0625}{2} = 0{\cdot}53$$

so that there was only about 6 per cent. difference, Mr. Porter had stated that the Author had given only the first expression ; but both were given (see p. 11, *ante*) and the difference was usually rather unimportant.

Dr. Glanville had shown clearly that on impact the head received the blow, which was then transmitted by a pressure-wave to the toe and back again, That took time (the speed was known). Consequently the head was stressed before the toe, and, as Dr. Glanville had stated, the "stress at the head is dependent only on the conditions at the head."

The Author's explanation was his analogy of the railway train of trucks. The condition for a compression-wave was that the pile should be deemed to have both its mass and its elasticity distributed along its length, as in the train analogy.

The mathematical theory developed in Dr. Glanville's Paper had that as its basis.

It was also clear that, as concrete weighed about four times as much as timber, if two piles, one of timber and one of reinforced concrete, of the same length and size, were driven to the same stratum, then, if the timber pile weighed 1 ton, the reinforced-concrete pile would weigh 4 tons.

Supposing that the monkey weighed 2 tons, then $\frac{M}{M+P}$ for the timber pile would be

$$\frac{2}{2+1} = 0{\cdot}66$$

and for the concrete pile

$$\frac{2}{2+4} = 0{\cdot}33$$

so that the concrete pile should carry half the load of the timber pile in the same stratum.

That did not conform with the Author's experience.

In an excellent article in 1946, based on much research, W. H. Rabe [1] suggested an "efficiency of blow" term of $\frac{M}{M+\frac{P}{2}}$, which lay half-way between $\frac{M}{M+P}$ and unity. That again showed that the Author's view, that $\frac{M}{M+P}$ gave too low a value and over emphasized P, was shared by others having experience and research facilities.

[1] *Engng News-Rec.*, 26 Dec. 1946

The Author hoped that in his reply to the Correspondence, to be published in a supplement to the October 1947 Journal of The Institution, he would be able to give the results of further work on that subject.

He had not suggested, as Mr. Andrews had asserted, that "Mr. Hiley had invented the efficiency factor." He had only suggested that Mr. Hiley had incorporated it, and the Author believed that it gave too great an emphasis to P and that the correct value lay nearer to unity.

The Author agreed with Mr. Andrews and other contributors that Mr. Hiley's work was a landmark in the treatment of the subject, but he still considered that a few improvements were possible, some of which he had ventured to suggest. He realized that it was too much to hope that those suggestions would receive instant recognition. He recalled that a Paper presented by him to The Institution, in 1927,[1] had been derided by nearly every contributor to the discussion upon it (but not by the late Professor W. C. Unwin, Past-President I.C.E.); yet it was accepted to-day as common knowledge.

The problem of "groups of piles" referred to by Mr. Andrews and others was beyond the scope of the present Paper. The Author agreed that his formula applied only to driven piles.

Mr. Porter had calculated corrected values for the Siemens pile from the Hiley formula as giving:

50 tons with $h = 12$
63 ,, ,, $h = 24$
76 ,, ,, $h = 36$
90 ,, ,, $h = 48$,

if the Author had understood Mr. Porter correctly, as against a constant value of 111 tons by the Author's formula. The Author's difficulty, shared by many, was that there seemed to be no particular reason why one of Mr. Porter's values should be more correct than the others; which should be adopted? Mr. Porter had suggested taking 90 per cent. of the lowest value ($h = 12$ inches). Other contributors had expressed the view that 12-inch drops were unpractical, and adopted a drop of 3 feet or 4 feet. But that seemed very unsatisfactory in a formula which was claimed to be rational. How could the safe load on a pile driven to a depth in a particular stratum, say a ballast layer, depend on whether it were driven with a 3-foot drop or a 5-foot drop?

Again, Mr. Porter had suggested taking 90 per cent. of the 50-ton value. But Mr. Hiley had advocated a factor of safety of 3, if the Author had understood him correctly. (See the example in the last page of the Appendix to Dr. Glanville's Paper,[2] in which the Hiley formula was used and the driving resistance was taken as 90 tons for a pile with a load-bearing capacity of 30 tons.)

[1] "Plastic Yield, Shrinkage, and Other Problems of Concrete, and their Effect on Design," Min. Proc. Instn Civ. Engrs, vol. 225 (1927–28, Part I), p. 27.

[2] *Loc. cit.*, p. 11, *ante*.

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The Author considered that Mr. Pimm was not correct in stating that the Author had misquoted Hiley's "efficiency factor" (which Mr. Andrews had stated was his) by not giving the full term $\frac{M + Pe^2}{M + P}$. It was given in full on p. 11, *ante*, but the difference between the two was generally less than 6 per cent., as the Author had shown, so that the point was perhaps unimportant.

The Author was grateful to Mr. Pimm for proving that his formula agreed with Rankine in the refusal condition. That testimony was appreciated and the Author would not wish to be in better company. He also thanked Mr. Pimm for the statement that Rankine "took no account of the efficiency of the blow." In other words, Rankine had taken it as approximating to unity—as the Author had done as a result of his researches. Again the Author appreciated the company in which he found himself. But Mr. Pimm, in asserting that the Author's formula ignored "ground quake," was surely under a misapprehension. The Author's formula had been devised to agree with the *results* of the testing of piles under practical conditions.

The deduction of $\frac{d}{z}$ from the numerator (a small deduction) and the addition of xh to the denominator (generally a larger addition), allowed for *all* losses in driving, including ground quake. Was it to be presumed that the Author's piles were not subject to ground quake as much as Mr. Pimm's? Hence a formula derived from those actual test results had to be deemed to make due allowance for it.

Mr. Pimm had stated: "For conditions other than refusal the quantity $h - \frac{d}{7}$ appeared again, in the form of a correction for the height of the drop, and with regard to the need for that Mr. Pimm was in complete agreement with the Author."

But earlier he had asked to know more about the denominator 7. That was purely experimental. Taking, for example, the Siemens pile No. 6B:

$$h_r = \frac{d}{z} \times \frac{M}{M - xR} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

whence

$$z = \frac{d}{h_r} \times \frac{M}{M - xR}$$

When driving hard,

$$h_r = 12 \text{ inches (see } \textit{Fig. 8}\text{)},$$

whence

$$z = \frac{14}{12} \times \frac{2}{2 - 0{\cdot}015 \times 111}$$

$$= \frac{28}{12 \times 0.335} = 7.$$

When driving soft,

$$h_r = 2.85 \text{ inches (see } \textit{Fig. 8}\text{)},$$

whence

$$z = \frac{14}{2.85} \times \frac{2}{2 - 0.014 \times 40}$$

$$= \frac{28}{2.85 \times 1.4} = 7.$$

Similarly, for hundreds of other piles z was found to lie near the value of 7.

The Author was grateful to Mr. Pimm for *Fig. 20*, showing a take-up of 7 tons on a 60-foot pile, if the Author had correctly understood him. Unfortunately Mr. Pimm had not stated the size of the pile, so that it was impossible to say what that represented in tons per square foot.

The Author was also grateful for Mr. Brown's demonstration, with which he was in entire agreement. Mr. Brown's great experience of driving reinforced-concrete piles in connexion with the Manchester Ship Canal and elsewhere made his contribution of especial value.

With regard to Mr. Donovan Lee's statement that: "it was a pity that anyone should bring forward to-day the Dutch formula, which was understood to have been in use in 1820," perhaps Mr. Lee did not know that one of the most experienced reinforced-concrete contractors used it to the present day; surely that was a justification for commenting upon it. Mr. Manning had stated that, "Using the Dutch rule, a set of $\frac{7}{8}$ inch is found for ten blows"; that suggested that he also used it, though doubtless with great intelligence and discretion.

The Author was especially grateful for Mr. Nachshen's contribution. The Author and Mr. Nachshen had been in correspondence and had exchanged some figures, as a result of which the Author believed that Mr. Nachshen might have somewhat underestimated x and overestimated R in his calculation by the Author's formula. But the agreement between the Author and Mr. Hiley on the short piles was still quite good. On the long pile, the Author's value was a good deal lower than Mr. Hiley's value. Mr. Nachshen appeared to have concluded that the Author was therefore necessarily in error; but the alternative explanation seemed just as feasible.

Unfortunately the long pile was not test-loaded and so afforded no direct evidence. But since, on Mr. Nachshen's own showing, the short piles failed well *under* the results obtained by either the Hiley or the Author's formula, was it not likely that the Author's result for the longer pile, being the lower, also accorded best with the test result?

Although it was disconcerting that the short piles failed at less than the calculated value, Mr. Nachshen had mentioned that the clay was "very soft." One explanation might be that the Author's p for that particular clay should be more than 4, as Mr. Nachshen had suggested. The Author had mentioned that as a matter needing further research. It was, however, comforting that the factor of safety of 2 would have prevented failure from occurring with either of the piles referred to.

Mr. Nachshen's contribution was valuable and the Author hoped that more on similar lines would be forthcoming.

The Author thanked Mr. Manning for his critical observations, but he considered that Mr. Manning had slipped up on his point (3). If M became equal to R (so that $\frac{R}{M} = 1$), the set would not be zero, but the pile would run away. Nor would h then be zero, as work would be expended in the falling of the monkey. But the whole condition differed from that under discussion, because the Paper related to *pile-driving* and not to forcing down a pile by a steady load.

If, however, M were taken as something just short of R, then there would have to be a drop h, and that would produce a set s; Mr. Manning has adduced no evidence that the formula would give the correct estimate of load-carrying ability.

The Author was well aware of the "group of piles" problem, but that lay outside the scope of the Paper.

The Author was grateful to Mr. Packshaw for his thoughtful contribution. Several contributors had referred to the fact that the driving resistance and the carrying capacity of a pile were not the same. Perhaps the Author should have made that clearer.

In the case of clay, the true driving resistance was, in his view, given by

$$R = \frac{M\left(h - \frac{d}{z}\right)}{s + xh}$$

But that was not the load the pile would carry under static load conditions owing to plastic flow. The latter was given by

$$W = \frac{M\left(h - \frac{d}{z}\right)}{s + pxh} + \text{Take-up}$$

where p depended on the properties of the clay and was about 4 for London clay. The take-up depended on the time element. No doubt it would have been clearer if separate symbols had been used, R_1 denoting the ultimate static load allowing for take-up.

With high-friction materials, such as sand with little loam, the Author believed that there was little difference between R and W; in other words, p approximated to unity.

He hoped and believed that the discussion would, in time, be found to be of considerable value to the younger members of the profession.