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Poisson's ratio of concrete: a comparison of dynamic and static measurements*

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Contribution by Professor J. W. H. King, M.Sc., M.I.C.E., M.I.Struct.E.

A rapid perusal of Mr Simmons's paper revealed several findings so different from those ascertained in my own laboratory at Queen Mary College, mainly by Whittaker, that I consider some query justified.

The first outstanding point is Table 1, wherein not only is Poisson's ratio deemed to vary in the same material under similar conditions, but a value of over 0·5 is found in column μ_2, and μ_1 and μ_2 have no consistency in their relationship to one another. μ_2 has been found using a value of E_D which was apparently acceptable for finding μ_1, yet it clearly cannot produce sense out of equation (3), as imaginary values for $\frac{V}{2f_L l}$ arise. It seems fairly evident then that the determination of G is mainly at fault, since equation (5) is quite sound.

Two points where errors may have arisen come to mind. Firstly, equation (4) is not quite accurate, as it should include a second order term covering cross-sectional distortion of the specimen under torque which has an importance which increases with the harmonic number of the resonance picked up, and the breadth to length ratio of the specimens. There is also a similar, usually smaller, lateral motion correction (due to Poisson's ratio effects) which operates for longitudinal resonances, so that both E_D and G_D are possibly in error since these corrections have been ignored. Although these corrections to G_D and E_D are small, their importance on the finally derived value of μ_2 is about tenfold, and they must have been responsible, in part at least, for the rather surprising acceptance of two values of μ. To quote μ to three decimal places is hardly justifiable under these circumstances. Another factor which may or may not be present is brought to mind by the simple statement in the second paragraph of page 62 that resonance was established by the maximum pick-up output. There are always many spurious resonances present, many of large magnitude, and it is essential to have some device which will show that a pure sine wave is both fed in and picked up. This may in fact have been done, but the paper gives us no information on this point.

I also feel that the method used for torsion as shown in Figure 1 of the paper was open to criticism in practice, although theoretically it should be efficient. For instance, vertical motions induced by the driver would record as part horizontal at all times when the side of the specimens adjacent to the pick-up was not strictly vertical, and might have affected pick-up maxima appreciably by imposing spurious resonances on top of the torsion effects, and so displacing the observed peak resonance frequency if determined purely on a maximum signal basis. Whatever the reason, I must state, most emphatically, that I cannot agree to a μ_1 and μ_2—both should be the same, and have been so determined at Queen Mary College.

Coming now to Figure 8, I must place on record that Whittaker found, before Takabayashi's work was published, that statistically, on over 700 specimens, $E_D = 0{\cdot}95\ E_s + 1{\cdot}27$ with a standard deviation of $\pm 0{\cdot}2$ (in lb/in$^2 \times 10^6$). This, when plotted, is very close to the relationship $E_D = E_s + 1$ over the relevant range. Whittaker covered a range of E_D from $3{\cdot}0 \times 10^6$ to $7{\cdot}8 \times 10^6$, with a definite decrease in scatter of the higher values. There is no trend to equality between the moduli at the high end, and I cannot agree with such an inter-

*Pages 61-68.

†Mr Simmons is now in the Technical Department of the Royal Aeronautical Society.

pretation from points exhibiting such wide scatter as those of Figure 8. No doubt much of the scatter is due to measurement of E_s, which is liable to errors both from strain and load measurement, but I cannot but wonder if E_D is partly to blame also, for both that and the two values of μ.

Altogether, I fear that the combination of Table 1 and Figure 8 has left me suspicious of the soundness of the paper as a whole, which is to be regretted.

This is clearly no occasion for detailed quoting of Whittaker's findings in tests which covered variation of mix ratios, aggregate gradings, cements, ages, and wet or dry state, and were very much more comprehensive than Mr Simmons's work, and I will therefore refer those interested to his 1955 Ph.D. thesis entitled " Ultrasonic determination of the physical properties of concrete ", available for reference in the library of the University of London. I can indicate, however, that he found a range of μ from 0·18 to 0·28. For any one concrete it diminished between 5 and 10% with ageing from 3 days to 1 year, whilst it increased with water/cement ratio, and decreased with increase in aggregate/cement ratio. As a broad generality, the stronger concretes had the lower values of μ. The accuracy of his determination of E_D and G_D is assessed as giving a standard deviation of less than 1%, which results inevitably in a standard deviation of μ of the order of 10%. Thus Mr Simmons's values of both μ_1 and μ_2 for concrete are within Whittaker's range, and both exhibit trends of the same type. It would appear therefore that, somewhere, his accuracy of measurement was not as good as he supposed in his dynamic measurements or calculations, thus leading to a fairly consistent error in one of his values of μ, and the false conclusion that there were two values. This might have been realized from a study of Table 1.

Contribution by G. Bradfield

I note that Mr Simmons is puzzled over what has, in fact, a simple explanation. His anomalous results for Poisson's ratio on page 62 are due to preferred orientation in his metal specimens. Reference can be made to discussion of this effect by Pursey and myself[1]* and by Pursey and Cox.[2] It may be reassuring to him to know that we carried out tests several years ago on the elastic constants and anisotropy of concrete beams and found that this anomaly was almost entirely absent in good quality uniform concrete. If, however, a gradient exists in the elastic constant, serious errors can arise in interpreting results from resonance methods both for longitudinal and torsional modes. It is best to use pulse techniques when investigating the elasticity of materials of this type and to propagate in a path normal to the direction of maximum gradient. It will probably be found that shear waves have a different velocity when the plane of polarization is normal to the direction of maximum gradient from that when it is rotated by 90° from this position.

Contribution by R. Jones, B.Sc., Ph.D.

In my comments I shall refer mainly to the results quoted by Mr Simmons for the dynamic modulus of elasticity, E_D, the static modulus, E_s, the pulse velocity, V, and the Poisson's ratio, μ_1.

The difference which Mr Simmons found between μ_1, calculated from measurements of longitudinal resonance and pulse velocity, and μ_2, calculated from longitudinal and torsional resonance, can be ascribed to anisotropy in the case of concrete. However, the larger discrepancies between μ_1 and μ_2 in copper and brass bars are not satisfactorily explained.

RELATIONSHIP BETWEEN E_D AND E_s

There have been several attempts to derive a relationship between E_D and E_s and conflicting relationships have been obtained. A lack of uniformity in the definition and method of measuring E_s may be responsible for some of the discrepancies. For example, recent information from the United States[3] has given results in agreement with those of Mr Simmons when E_s was measured on compression specimens, i.e. with E_D generally greater than E_s. However, in the same experiments on identical concrete, E_s was found to be almost equal to E_D when E_s was derived from bending experiments on the concrete beams.

A further discrepancy may arise when different aggregates are used. Mr Wright, of this Laboratory,* has measured E_D and E_s by the methods given in B.S. 1881 for concretes made with a river gravel coarse aggregate and a crushed rock coarse aggregate. He found results comparable with those of Mr Simmons for concrete containing the river gravel aggregate, but there was a greater difference between E_D and E_s for that made from crushed rock aggregate. It is hoped to publish the full results in this Magazine later.

RELATIONSHIP BETWEEN E_D AND μ_1

Mr Simmons obtained some very interesting results indicating a relationship between E_D and μ_1 as follows:

$$\mu_1 = 0{\cdot}391 - 2{\cdot}4 \times 10^{-8} E_D \ldots\ldots\ldots (1)$$

Such a relationship implies that μ_1 can never exceed 0·391 without E_D becoming negative. To hold throughout the hardening period of the concrete, values of μ_1 up to 0·5 should be permissible and E_D should always be positive.[4] Early results obtained during the hardening period of concrete made with Ham River aggregate have been analysed, and it has been found possible to obtain a relationship which also agrees with Mr Simmons's

*The index numbers refer to the items in the list of references on page 45.

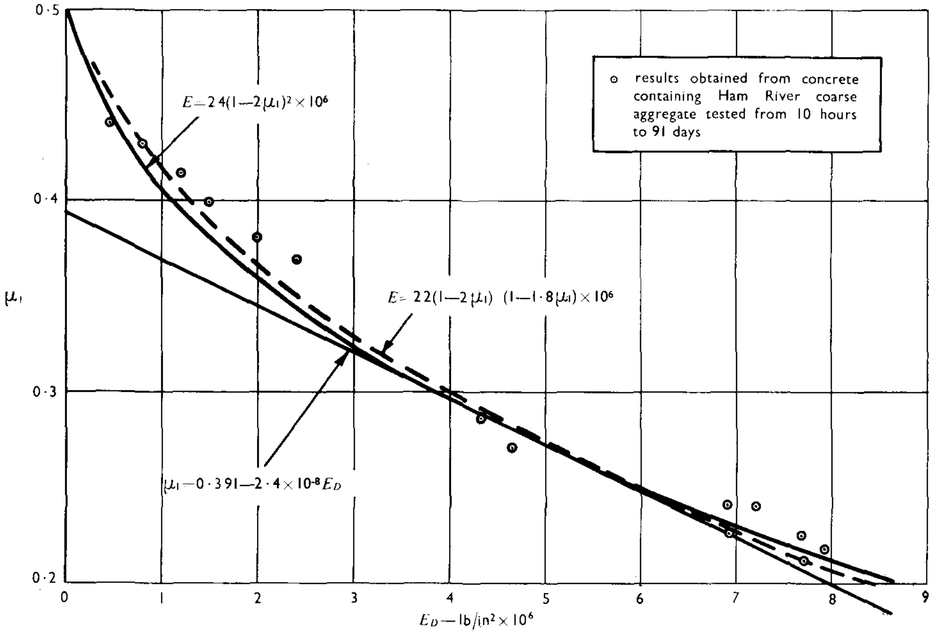

Figure 1: Relationship between dynamic modulus (E_D) and Poisson's ratio (μ_1).

results. This relationship, shown in Figure 1, has the form

$$E_D = A(1 - 2\mu_1)(1 - B\mu_1) \quad \ldots\ldots\ldots (2)$$

where, for Ham River concrete, $A = 24 \times 10^6$ lb/in² and B has a value nearly equal to 2. For most purposes $B = 2$ is adequate, but the qualification that B should be slightly less than 2 arises from a consideration of the pulse velocity (V) in wet concrete.

$$V = \sqrt{\frac{E_D(1-\mu_1)}{\rho(1+\mu_1)(1-2\mu_1)}} \quad \ldots\ldots\ldots (3)$$

Substituting from equation (2),

$$V\sqrt{\rho} = \sqrt{\frac{A(1-B\mu_1)(1-\mu_1)}{(1+\mu_1)}} \quad \ldots\ldots\ldots (4)$$

When $B = 2$, V would approach 0 as μ_1 approaches 0·5 whereas, in practice, V has a small but finite value. However, the objection is not serious because it is very unlikely that the wet concrete behaves elastically as is assumed by equation (3).

Equation (2) is an empirical relationship which could hardly be derived theoretically. However, by the use of simplifying assumptions, the following approximate relationships can be derived between E_D, μ_1, E_A, μ_A, E_M and μ_M.

$$\frac{1}{E_D} = \frac{v_A}{E_A} + \frac{1-v_A}{E_M} \quad \ldots\ldots\ldots\ldots (5)$$

$$\frac{\mu_1}{E_D} = \frac{\mu_A v_A}{E_A} + \frac{(1-v_A)\mu_M}{E_M} \quad \ldots\ldots\ldots (6)$$

where E_A is the elastic modulus of the aggregate
E_M is the dynamic modulus of the mortar
μ_A is the Poisson's ratio of the aggregate
μ_M is the Poisson's ratio of the mortar
v_A is the volume percentage of aggregate

from which

$$\mu_1 = \mu_M + \frac{E_D}{E_A} v_A(\mu_A - \mu_M) \quad \ldots\ldots\ldots (7)$$

As the concrete hardens, μ_M decreases and E_D increases. Initially E_D is considerably smaller than E_A and the value of μ_1 should be approximately that of the corresponding mortar; this is approximately true. As the concrete hardens, μ_M approaches a constant value (normally less than μ_A) and E_D becomes more comparable with E_A. In the hardened state, therefore, the relationship between μ_1 and E_D may be expected to depend upon v_A, E_A, μ_A and μ_M. However, as v_A is changed, E_D changes and a different mortar is used so that μ_M is also changed. For example, if more aggregate is used, a wetter mortar with a higher value of μ_M is required and μ_1 is increased. However, E_D is also increased and the relationship between E_D and μ_1 may not be influenced appreciably by changing the percentage of coarse aggregate, as is evident from Mr Simmons's results.

However, a change in the type of coarse aggregate may be expected to influence the relationship between E_D and μ_1. Unpublished results obtained by Dr Kaplan at the Road Research Laboratory from concretes containing a number of different types of coarse aggregate have shown that the relationship between E_D and μ_1 is considerably affected by aggregate type. For example, if a relationship of the form of equation (2) ($B = 2$) is fitted to results from a concrete containing limestone coarse aggregate, the value of A is almost doubled. It is hoped at a later date to publish Dr Kaplan's results in this Magazine.

Mr Simmons expressed an opinion that "in general, any factor that increases the elastic modulus or strength of the concrete will tend to decrease the values of μ_1 and μ_2". This may be true for the limited number of variables he has considered but it is certainly not true in general. A change of coarse aggregate can produce increases in both E_D and μ_1, and a decrease in the moisture content will certainly produce a decrease in both E_D and μ_1. Consequently, equation (1) or (2) can be expected to have only a limited application.

RELATIONSHIP BETWEEN V AND E_D

The modification suggested to equation (1) will automatically cause a change in the relationship between V and E_D given by Mr Simmons. The amended version (with $B = 2$) reads:

$$V\sqrt{\rho} = \sqrt{\frac{(E_D A)^{\frac{1}{2}}\left[1 + \left(\frac{E_D}{A}\right)^{\frac{1}{2}}\right]}{\left[3 - \left(\frac{E_D}{A}\right)^{\frac{1}{2}}\right]}} \quad \ldots\ldots\ldots(8)$$

Once again it should be emphasized that this relationship will give a different quantitative relationship between V and E_D for change of coarse aggregate, moisture content and probably other factors. Dr Kaplan's results have also given an almost linear relationship between V and E_D for a particular type of concrete in wet test specimens; the quantitative values depended upon the type of coarse aggregate.

APPLICATION OF RESULTS

The analysis given by Mr Simmons presents a very intriguing conclusion. It implies that for a particular concrete a measurement of pulse velocity can yield both dynamic modulus and Poisson's ratio, provided the appropriate constants in the equations have been obtained. At first sight this is rather incredible and further work will be required to define how far it is justified. The work has an obvious application in the evaluation of stresses from the measurement of strains on structural members where the loading is dynamic.

The work may eventually be of great value in the use of pulse methods as a means of assessing quality. At present there are two main approaches, the first based on E_D as a criterion of quality and the second based on V. Mr Simmons's work may provide a useful link, provided the complicating factors, such as change of moisture content, can be evaluated.

Reply by the author

Considering first the determination of the longitudinal and torsional resonant frequencies, the experimental methods used are by now fairly well known and it was not felt necessary to include full details in the written paper. In view of this lack of detail, therefore, it seems hardly justifiable that the experimental technique should be criticized by Professor King. However, it should be pointed out that considerable care was taken to ensure that it was the true longitudinal and torsional resonant frequencies of the specimens that were measured.

The electro-magnetic driver in the resonance tests was excited from an oscillator which gave an output of sine wave form with less than 0·5% of harmonic distortion. The wave form of the pick-up signal was also found to be of undistorted sine wave form. Furthermore, signals from the oscillator and from the pick-up were fed to the x and y plates, respectively, of an oscilloscope; thus it was always possible to check that the beam was vibrating at the same frequency as the input and not at some harmonic thereof.

Considering now longitudinal resonance, the relationship given by equation (2) of the original paper

$$E_D = 4 f_L{}^2 l^2 \rho \quad \ldots\ldots\ldots\ldots\ldots(2)$$

ignores the second order effects due to the lateral vibrations of the bar, but the results quoted do, in fact, include this correction. Its magnitude is quite small, however, and for specimens the size of the concrete beams the correction to f_L is generally less than 0·2%. The lateral inertia correction is considered in detail in my thesis[5] where it is shown that, if the correction is not applied, the observed harmonic frequencies become increasingly different from exact integral multiples of the fundamental as the harmonic number is increased. On applying the correction this discrepancy is virtually eliminated.

For the fundamental torsional mode the second order term is very small and has been neglected in the calculations. This is considered justifiable since tests show that, for specimens the size of the concrete beams, the observed harmonic frequencies in torsional vibration are sensibly exact integral multiples of the fundamental. Tests reported by Pickett[6] also confirm that the use of the constant R in equation (4) is sufficient to allow for the effects of the distortion of the cross-sections of the bar.

$$G_D = 4 R f_T{}^2 l^2 \rho \quad \ldots\ldots\ldots\ldots\ldots(4)$$

Theoretically, the method used for torsional vibration of the beam (shown in Figure 1 of the original paper) is not ideal since it could also induce a flexural mode. This can, of course, be avoided by using a pair of drivers symmetrically placed at the same end of the beam, but tests showed that this was unnecessary for the work in question since, with a little care, the harmonics of the flexural mode could be avoided.

If a spurious resonance had occurred at a frequency near to the true torsional resonant frequency so that the observed output maxima were displaced, then the general form of the resonance curve would be expected to be distorted. Tests showed, however, that the output curve was quite symmetrical about the maximum at resonance (this was also the case with longitudinal resonance), and for better accuracy the resonant frequency was determined not by measuring the frequency at resonance, but by taking the mean of observations at equal outputs a little to either side of resonance.

The method of measuring the frequency of the oscillator output was by means of a frequency monitor capable of determining frequency to an accuracy of $\pm 0{\cdot}005\%$ and its performance was constantly checked by means of a standard crystal oscillator incorporated in the equipment. It is considered that, allowing for any possible errors, the resonant frequency determination could be carried out to an accuracy of better than $\pm 0{\cdot}02\%$. Thus with values of μ_2 of the order of 0·2, the accuracy of determination of μ_2 from equation (6), which was incorrectly printed in the original paper, and should read

$$\mu_2 = \frac{1}{2R}\left(\frac{f_L}{f_T}\right)^2 - 1 \ldots\ldots\ldots\ldots\ldots (6)$$

was within $\pm 0{\cdot}6\%$ which is considerably better than the value of 10% quoted for Whittaker's results. It is, therefore, considered perfectly justifiable to quote μ_2 to three decimal places. Similar reasoning shows that it is equally justifiable to quote μ_1 to three decimal places. Furthermore, experimental evidence quoted in the original paper confirms this. On page 65 it was stated that when different specimens from the same batch of concrete were tested, the mean standard deviations in μ_1 and μ_2 were 0·005 and 0·004 respectively (i.e. about 2%). Thus, since specimens made from the same batch of concrete are known to show appreciable variations in properties, it is reasonable to suppose that the measurement of the individual values of μ_1 and μ_2 was made to a considerably closer degree of accuracy than 2%.

Contrary to Professor King's suggestion, a study of the results given for metals in Table 1 confirms, rather than casts doubt upon, the reliability of the experimental technique. The results given for steel are clearly consistent and in agreement with accepted values. Steel, however, very rarely shows any appreciable degree of anisotropy, whereas copper and brass may be markedly anisotropic. For example, results quoted by Barrett[7] show that for single crystals of copper it is possible for *E to be less than G*, both being measured in the same direction (this would give a value of $\frac{E}{2G} - 1$ of less than $-0{\cdot}5$). For specimens other than single crystals, such startling results are unlikely, but preferred orientation may often be present, causing an appreciable degree of anisotropy, the extent depending on the nature of the material, the manner of production of the specimen and the degree of heat treatment or cold working. These differences in the properties of metals in different directions have been known for many years and several references are available quoting experimental results.

Of particular interest is the paper by Bradfield and Pursey (quoted by Bradfield in his contribution) giving results of tests on bars of copper and silver. They determined Poisson's ratio in a similar manner to the determination of μ_2 in the present paper and found values for copper varying between 0·271 and 0·781, and for silver between 0·185 and 0·802, the actual value depending on the type of heat treatment given to the specimen. Only when the prior metallurgical processing had been correct and when the specimens had been thoroughly annealed did the values of this "apparent Poisson's ratio" agree with the value determined by other methods.

Equation (5)

$$\frac{E}{2G} - 1 = \mu \ldots\ldots\ldots\ldots\ldots (5)$$

will only give the true value of Poisson's ratio either if the material is isotropic or, if it is anisotropic, if the appropriate values of E and G to which the relationship still applies are taken. If in a material E varies by 10 or 20% according to the direction considered, and this is not impossible in a material such as brass, then clearly the apparent Poisson's ratio determined from equation (5) can be as much as double the true value. Professor King states that a value of μ_2 greater than 0·5 is inconsistent with the pulse velocity relationship of equation (3)

$$\frac{V}{2f_L l} = \sqrt{\frac{(1 - \mu)}{(1 + \mu)(1 - 2\mu)}} \ldots\ldots\ldots (3)$$

This is obvous, but it is, of course, quite irrelevant, since the apparent Poisson's ratio μ_2 is not the value appropriate to that equation.

The paper by Pursey and Cox—the author is grateful to Mr Bradfield for drawing his attention to this reference—is also interesting in that it shows theoretically that, for a slightly anisotropic material, a low value of μ_2 (in relation to the average value of Poisson's ratio) will be accompanied by a low value of E_D, and that as E_D increases, so too will μ_2. This trend is evident from the experimental results shown for copper and brass in Table 1.

With these large discrepancies possible between μ_1 and μ_2 for metal specimens, it is not surprising that there are also differences, though only about 20%, with concrete. Whether or not this can again be ascribed to anisotropy is not absolutely clear, since authorities still differ on this subject but, as Bradfield suggests, a gradient or variation of the properties of the concrete within the specimen may also have an effect. However, the fact remains, and unpublished work by Dr Jones confirms, that μ_1 and μ_2 for concrete specimens are not necessarily equal. The fact that they have been found equal at Queen Mary College does not invalidate the results obtained by other investigators.

The remainder of the experimental results on concrete quoted by Professor King are interesting since they confirm the author's suggestion that, in general, factors which increase the strength of concrete will tend to decrease the value of μ. The range of values quoted for μ and the effects of age, water/cement ratio and mix proportions on μ are all similar to the results reported in the original paper. It is gratifying to see, therefore, that this rather more recent work, with one additional variable (cement type), confirms the results already obtained by the author.

The relationship between E_D and E_s shown on Figure 8 of the original paper clearly shows a trend towards equality at higher values of E, and, considering the data at values of E_D greater than 5×10^6 lb/in^2, the standard deviation is $\pm 0{\cdot}3$ (in lb/in$^2 \times 10^6$). This is a slightly lower degree of accuracy than obtained by Whittaker, but, as mentioned in the original paper, this may be due to the rather short gauge length used in measuring strain. The rather different relationship found between E_D and E_s at Queen Mary College may well be due to a difference in conditions such as, for example, type of aggregate, rather than any faulty experimentation.

I should like to thank Dr Jones for his very useful contribution and for the extension of the idea (originally due to a suggestion by Mr R. H. Elvery of the Department of Civil and Municipal Engineering at University College, London) of the determination of the dynamic modulus from measurements of pulse velocity and density.

As mentioned in the paper, only a limited period of time was available for the entire programme and thus there were a number of variables which had to be eliminated. Probably the most important of these was aggregate type, and thus it is most interesting to hear of similar tests in which different aggregates were used. In spite of the extent to which non-destructive testing methods are used in this country, it seems that only a very limited amount of the work is ever published. It is to be hoped, therefore, that this necessarily incomplete paper may provoke the publishing of other data on this and related topics.

The difference reported in the paper between the values of μ_1 and μ_2 for the copper and brass specimens has been dealt with in the reply to Professor King's remarks, and the comments by Mr G. Bradfield of the National Physical Laboratory add suitable confirmation to the data. The remaining points raised by Dr Jones will, therefore, be considered.

The extension of the experimental relationship between μ_1 and E_D to values of E_D well below 4×10^6 lb/in^2 is of considerable interest. The linear relationship was originally chosen on the grounds of simplicity and it was not intended that it should be used beyond the limits of the variables used in the tests or of the experimental data given in Figure 6. The additional data at low values of E_D certainly imply other than a straight line relationship, and the suggestion of an equation of the form $E_D = A(1 - 2\mu_1)^2$ is both simple and convenient. It may be noted, however, that between $E_D = 3 \times 10^6$ and

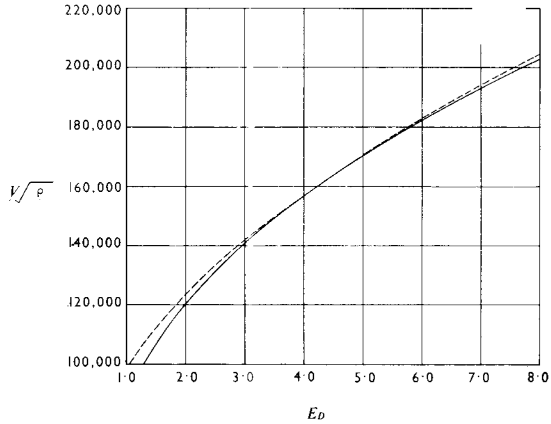

Figure II: Relationship between $V\sqrt{\rho}$ and E_D.
—— *equation* (8) *of original paper*
- - - - - *equation* (8) *of Dr Jones's, contribution with* $A = 24$
E_D—*lb/in*$^2 \times 10^6$
ρ —*lb/ft*^{3}
V —*ft/sec*

7×10^6 lb/in^2 (the limits of the original data), the difference between the quadratic function and the original linear relationship is almost negligible. Similarly, over the same range of E_D, the new relationship between $V\sqrt{\rho}$ and E_D (equation (8) in the comments by Dr Jones) gives values of E_D less than 2% different from those obtained from equation (8) of the original paper. The amended version is, however, clearly preferable owing to its wider range of applicability and since the correct choice of the constant A may allow for the effects of variables such as aggregate type. The discrepancy between the two equations naturally increases as E_D becomes less than 3×10^6 lb/in^2 as can be seen from Figure II. This Figure should be used in preference to Figure 9 of the original paper since the labelling of the axes may have been misleading and since the appropriate units were not stated.

As all the specimens used in the investigation were subjected to standard compaction and were cured and stored under standard conditions (in water at 60°F), the effect of treating these as variables is at present unknown. The type of compaction and curing common on civil engineering sites might produce a rather different relationship between μ_1 and E_D, and thus the empirical equation relating $V\sqrt{\rho}$ and E_D might not be applicable under these circumstances. A few simple tests should, however, clarify this situation.

The effect of the drying out of the concrete specimens was not mentioned in the paper, although a few tests have been made on its effect. As Dr Jones says, reduction in moisture content causes a reduction in both E_D and μ_1. This is contrary to the relationship of equation (7)

$$\mu_1 = 0{\cdot}391 - 2{\cdot}4 \times 10^{-8} E_D \quad \ldots\ldots\ldots (7)$$

but the differences are generally not large in comparison with the general scatter of the test data, and it is not thought that this will have any serious effect on the determination of E_D from $V\sqrt{\rho}$.

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