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The rheology of cement pastes and fresh mortars*

by A. A. Nessim, B.Sc.(Eng.), Ph.D. and R. L. Wajda, Dipl. Ing., A.M.I.C.E.

Contribution by G. H. Tattersall, M.Sc., Ph.D., F.Inst.P.
University of Sheffield

In the absence of details of the dimensions of their viscometer, it is not possible to make exact comparisons between Nessim and Wajda's results and mine. It is interesting to see, however, that the general form is the same, in that their Figure 3 is similar to one I reported⁽¹⁾, and they have carried out similar breakdown experiments and plotted the results in the same way.

Their suggestion that two straight lines are needed for the exponential plot is at variance with my results,⁽¹⁾ as they point out, and also with those obtained by Kyte in some unpublished work on lime pastes, but is similar to my findings⁽²⁾ for Weltmann's results⁽³⁾ on printing ink. Weltmann had obtained a straight-line relationship over a very limited range by plotting T against $\ln t$, but by plotting according to my equation, $T - T_E = (T_O - T_E) \exp(-Bt)$, the results for the whole range fell on two intersecting straight lines. It was interesting that Weltmann's results on oils⁽⁴⁾ also gave two intersecting straight lines when plotted by my method, whereas plotting by her method gave curves of various shapes⁽²⁾. At that time, I suggested that perhaps the line of steeper slope represented breakdown while that of lesser slope represented the effect of a gradual increase in temperature; Nessim and Wajda's report that in their experiments the temperature increased by no more than 1.6 degC seems not to support that suggestion. The phenomenon is one that needs explaining and I am at present carrying out some work that might throw some light on the matter. I certainly cannot agree with Nessim and Wajda's contention that, for the results they give, a single exponential equation is a reasonable approximation.

Nessim and Wajda have used Weltmann's technique⁽³⁾ of running down-curves after a specified time

of breakdown. I pointed out⁽²⁾ that for the one case for which Weltmann gave full results, a pigment suspension D , her results were invalid because the early points (which were the important ones) for the first down-curve did not lie on the line; Nessim and Wajda's result, that the down-curves from high rates of shear were non-linear, shows a somewhat similar difficulty.

In considering the effect of rate of shear on breakdown, it seems to me that rather than study the reduction in plastic viscosity to various arbitrary values, it is better to investigate the relationship between B and the rate of shear. My simple theory suggested that B should be proportional to w^2 where w is angular velocity, but I found⁽¹⁾ that a better straight line was obtained when B was plotted against $w(w - w_1)$ where w_1 is a constant.

There are several points on which I should like further information.

What was the reason for using a double-gap instrument instead of the simpler single gap?

What were the gap dimensions? This information is of course very important; it is necessary (a) to permit calculations of rates of shear for comparisons with earlier similar work and (b) to permit a consideration of the degree of uniformity of the rate of shear across the gap.

What method of filling was used?

What method of temperature control was used? A range of ± 5 degF seems quite unnecessarily high.

The exponential relationships between water content and plastic viscosity and yield value, respectively, agree with mine in form and also with the relationships that may be determined at low rates of shear when cement paste behaves approximately as a Bingham plastic⁽⁵⁾.

It is worth remarking that, since cement paste does not seem to show signs of build-up on standing (Nessim and Wajda's results again confirming mine),

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it cannot properly be classified as thixotropic, as I have pointed out earlier⁽²⁾.

The fact that the rate of energy input to the paste at constant rate of shear decreases as the paste becomes thinner, is obvious from the most elementary considerations, and no experimental results are needed to prove it. No deductions about the rate of energy input during vibration of concrete can be made from this alone; it might, however, be noted that in 1951 Venkataramaiah showed the rate of energy input to a vibrating table passed through a maximum if the amplitude was kept constant⁽⁶⁾.

The arguments advanced for a single figure of energy input as a measure of workability are unsound. Cement paste alone needs at least four constants to describe its flow properties and concrete is likely to be more complicated. No single-point test will ever be adequate except possibly over a very limited range of circumstances. If this had been more generally appreciated, a great deal of the time and energy that have been spent in the past on empirical tests for workability could have been saved.

Finally, it is certainly not justified to argue that simply because two phenomena both happen to obey exponential laws, they are in some way related.

Contribution by J. G. D. Morgan, B.Sc.
Blaw Knox Limited

The paper is of particular interest to myself in view of some parallel work I have been carrying out into the flow of fresh concrete.

Nessim and Wajda's work sheds further valuable light onto the thixotropic properties of cement pastes and extends the work of Tattersall⁽¹⁾ to mortars and to a wider range of water/cement mixtures.

Tattersall found that "there is little or no change in the yield value during breakdown", whereas the present paper indicates that the yield value is profoundly modified by breakdown, i.e. if Tattersall is correct the first up-curve (Figure 3) would be seen to be only part of a large hysteresis loop, and if the readings had been extended below 10 rev/min, the yield value would have been the same for all up-curves and down-curves, indicating a constant yield point (see Tattersall's Figure 1). I would be interested to hear if the apparatus permitted readings to be taken in this region.

The results for plastic viscosity at constant rates of shear are also of interest. Figures 5-7 of the paper show that the apparent plastic viscosity decreases with time after the first application of the shear and decreases to an approximately constant value if the shearing is applied long enough.

In this instance, is it not perhaps an over-simplification to treat cement paste as a thixotropic Bingham plastic, like printing ink, when it is well known that it

possesses visco-elastic properties which are exhibited in the phenomenon of "stress relaxation"? It will now be shown that equation 5 of the paper may be arrived at purely by consideration of stress relaxation.

Forslind⁽⁷⁾ states that the instantaneous stress in the relaxing system may be defined by the relation (Nessim and Wajda's nomenclature)

$$\frac{dF}{dt} = G \frac{dv}{dx} - \frac{F}{t_{rel}} \dots \dots \dots (I)$$

where t_{rel} is the relaxation time and G is the elastic modulus of shear.

The viscosity for a steady progressive deformation is given by

$$\eta = G t_{rel} \dots \dots \dots (II)$$

which gives

$$\frac{dv}{dx} = \frac{1}{G} \frac{dF}{dt} + \frac{F}{\eta} \dots \dots \dots (III)$$

If the yield point f (which we will assume constant) is taken into account, the authors' equation 2 becomes

$$\frac{1}{\eta} (F - f) = \frac{dv}{dx} - \frac{1}{G} \frac{dF}{dt} \dots \dots \dots (IV)$$

The solution to this is (see reference 8)

$$F = \exp\left(-\frac{G}{\eta} t\right) \times \left\{ F_o + \int_0^t \left(G \frac{dv}{dx} + \frac{G}{\eta} f\right) \exp\left(\frac{G}{\eta} t\right) dt \right\} \dots (V)$$

where F_o is the initial stress.

For constant rates of shear

$$\left(G \frac{dv}{dx} + \frac{G}{\eta} f\right) \text{ is constant, equal to say, } c.$$

Therefore equation V becomes

$$F = \eta c + (F_o - \eta c) \exp\left(-\frac{G}{\eta} t\right) \dots \dots \dots (VI)$$

Hence $\frac{F - \eta c}{F_o - \eta c} = \exp(-Bt) \dots \dots \dots (VII)$

where $B = G/\eta$.

At equilibrium we have $dF/dt = 0$

Therefore $F_E = \eta c$

Hence $\frac{F - F_E}{F_o - F_E} = \exp(-Bt) \dots \dots \dots (VIII)$

which leads to equation 5 of the paper, but derived purely by consideration of visco-elasticity.

I have tried to show that some of the phenomena observed by Nessim and Wajda are not necessarily caused by breakdown. I do not regard the vibration of concrete as a purely thixotropic process, but a process in which mechanical shear forces, sufficient to overcome the yield point, are induced in the cement paste after which, of course, the rate of rotation is largely determined by the rheology of the cement paste, the size and shape of the aggregate material and the flow forces which are applied.

Reply by the authors

We thank Messrs Tattersall and Morgan for their comments and would like to reply as follows. First of all, in Dr Tattersall's work^(1, 2), the paste was subjected to the shear action $4\frac{1}{2}$ min after the first drop of water was added, whilst the corresponding period in our work was 12 min. This, when considered in conjunction with the fact that the variation of the rate of shear across the gap of the viscometer used by Dr Tattersall was considerably greater than that obtained with our double-gap viscometer, would make a direct comparison of the results difficult. (The ratio of the maximum rate of shear across the gap to the minimum value was 1.33 in Dr Tattersall's viscometer as compared with 1.08 in ours.)

The suggestion that two straight lines are needed for the exponential plot was confirmed by tests carried out by the senior author at the Building Research Station using a single-gap viscometer with grooved surfaces. In this work, a camera was used to record the torque variation, in order to minimize the human error.

In stating that "a reasonable approximation to the breakdown curve can still be obtained if one exponential curve is used to represent the whole curve", we were anxious to keep the expression as simple as possible. This seemed necessary in view of the fact that the breakdown expression would have to be used in any satisfactory theoretical approach to the problem of concrete vibration. A comprehensive approach to the vibration problem appears to be fairly complex even when a constant viscosity is assumed.⁽⁹⁾ With this in mind, the degree of approximation involved, illustrated in Figure 9*, would seem to be acceptable.

With regard to Dr Tattersall's comments that the early points of Weltmann's first down-curve do not lie on a straight line, we found in our work that at the rate of shear associated with the speed of 50 rev/min, the early points did lie on the straight line and the results given in Figure 4 are valid. When a similar series of tests was carried out at 70 rev/min, the upper part of the down-curve was curved because the rate of speed reduction originally adopted was not fast enough and some breakdown was taking place under constant rate of shear. This difficulty was overcome by decreasing the viscometer speed at a faster rate. This second series, as stated in the paper, confirmed the results obtained in the first series as far as the disappearance of the yield value is concerned.

In considering the effect of the rate of shear on breakdown, the relationship between B and the rate of shear referred to by Dr Tattersall had in fact been studied⁽¹⁰⁾, but no conclusive results were obtained. On the other hand, the investigation of the effect of the rate of shear on the time required to achieve a certain breakdown level yielded satisfactory and con-

sistent results. The power of -3.2 to which the angular speed ω is raised was determined from 20 different plots. The mean of the 20 values obtained was -3.23 with a standard deviation of 0.20.

We shall now deal with the points on which further information is required in the same sequence in which they are presented by Dr Tattersall.

(1) The governing factor in choosing a double-gap viscometer was the ease of introduction of relatively thick pastes into the gap. The reason for this is that, in the double-gap viscometer, the force exerted on the middle cylinder while it is being pushed into the paste is distributed over the actual cross-sectional area of its walls only, whereas, in the case of the simple single-gap viscometer, this force would be distributed over the entire base area of the bob, and consequently a much greater force is required to cause the paste to flow into the gap.

Among the other advantages associated with the double-gap viscometer as compared with the single-gap type, the following may be mentioned.

The end effects are reduced.

The amount of paste used is considerably less than that required for a single-gap viscometer of comparable dimensions.

The fact that most of the material to be tested is contained in the gaps between the cylinders tends to reduce any adverse effects due to sedimentation or slight non-uniformity in mixing.

(2) The relevant radii for the outer gap were 3.500 in. and 3.375 in. and for the inner gap the corresponding values were 3.187 in. and 3.074 in., giving an outer gap width of 0.125 in. and an inner gap width of 0.113 in. The ratio of the radii for each gap was 0.964 and the average rate of shear in reciprocal seconds was $2.88 N$ where N is the viscometer speed in rev/min.

(3) To fill the viscometer with paste, a specially designed funnel⁽¹⁰⁾ which fitted over the combined gap was used, together with a spatula to extrude the paste into the gap. The funnel was semi-circular in plan and consisted of a long vertical inner leaf connected to a shorter outer leaf which had a wide horizontal collar.

(4) All the tests were carried out at room temperature and no tests were undertaken when the room temperature was outside the specified range. This should not affect the results in view of Bachman's finding⁽¹¹⁾ that a variation in room temperature from 18 to 20°C produced an insignificant change in the flow curves of lime suspensions. Bachman states that the "temperature affects the viscosity of the water, the variations of which are unimportant in relation to the total plasticity".

As for the classification of the cement paste as a thixotropic material, it was found that when the paste was allowed to rest, after breakdown was complete there was a tendency for the broken structure to re-

*The Figure numbers refer to those in the original paper.

build itself very slowly. But whether this rebuilding was due to the chemical reactions acting alone, or to a combination of chemical reactions and thixotropic build-up, was difficult to ascertain. If the assumption is made, however, that in the absence of the chemical reactions the broken-down paste would eventually regain its original plastic viscosity if allowed to rest for a sufficiently long time, the breakdown would then be a reversible isothermal process, and the paste could then be classified as a thixotropic material in the broad sense of the word.

With reference to the variation of the total energy input with time, the curves in Figure 12 were produced primarily to show that the rate of energy input to the paste is not constant and to drive home the fact that the stiffer the paste the higher is the total energy input at any given time. In Venkatramaiah's work⁽¹²⁾, the energy absorbed by the concrete (or rather the cement paste matrix) was determined as the difference between two quantities of comparable magnitudes, whereas we measured the energy input directly because the electrical and frictional losses do not affect the value of the torque.

Since the compaction of concrete by vibration is essentially a breakdown-and-flow process which is similar to that achieved in the paste in the rotational viscometer, it would seem reasonable to assume that the rate of energy input to the concrete under vibration increases with the stiffness of the mix. In these circumstances, the comments in the paper regarding the Vebe tests would seem applicable.

Dr Tattersall objects to the use of the total energy input as a measure of workability. Perhaps it should be pointed out that the suggested method is not the same as a single-point measurement because the final value of the input energy depends on the whole torque-time curve and not on any single value of the torque. Furthermore, W_T was shown to be proportional to the initial plastic viscosity over a wide range of consistencies. Although we agree that workability depends on a number of parameters, each of these parameters must have a certain effect on the energy consumption and would therefore be taken indirectly into consideration by the suggested method of measuring workability.

It is further contended that it is not practicable to measure the workability of concrete by evaluating a number of variables, however desirable this may be theoretically. The method described in the paper can be used to measure the consistence of grouts and mortars on site if certain modifications are made to the apparatus. In the absence of a better alternative, it is considered inevitable that a single figure will be taken as a measure of workability.

Finally the suggestion that a close relationship exists between the rate of shear and the acceleration of the vibrator and also between the paste viscosity and compaction, appears to be justified not just because the

two phenomena may be similarly related mathematically but primarily because the compaction of concrete is physically dependent upon the viscosity of the cement paste matrix whilst, as was pointed out in the original paper, shear stresses must arise in the mix as a result of the existence of differential accelerations.

In reply to Mr Morgan's comment on the variation of the yield value with time, we should like to state that the minimum speed for the double-gap viscometer was $10\frac{1}{2}$ rev/min and no readings could be taken below this speed. In the experiments carried out by the senior author at the Building Research Station, however, it was possible to take readings for various viscometer speeds down to zero. The results thus obtained were similar to those shown in Figure 3. It ought to be pointed out, perhaps, that in view of the fact that most of the breakdown takes place during the time taken to run the first up-curve, the first and subsequent down-curves are well within the constant yield value range in Figure 4. In other words, there is no contradiction between the results given in Figures 3 and 4.

It is worth noting that the decrease of the plastic viscosity to an approximately constant value when the shearing action is continued for a long enough period of time (provided the rate of shear is above a certain critical value), is in agreement with the results given in the Institution of Civil Engineers' first interim report⁽¹³⁾ on concrete vibration. In this work, it was established that there is a critical acceleration below which the cube strength falls off rapidly and above which there is little increase in strength. Here again, if the rate of shear is assumed to be a direct function of the acceleration, while the cube strength (and hence the compaction) is assumed to be a function of the plastic viscosity, it will be seen that the curves in Figure 7 are in agreement with the results obtained from the work on vibration. The ineffectiveness of small accelerations in the compaction of concrete is probably due to the fact that there is little breakdown at low rates of shear, which in turn results in the material being too viscous for full compaction to be attained.

It is interesting to note that Mr Morgan has derived Dr Tattersall's equation by considering stress relaxation alone. The reduction of the plastic viscosity with time under shear is referred to in the paper as "breakdown" and, in the absence of a more detailed study of this phenomenon, it is difficult to ascertain whether it is in fact caused by a stress relaxation or some other phenomenon, but this should have no adverse effects on the validity of the conclusions we drew on the behaviour of the paste.

REFERENCES

1. TATTERSALL, G. H. The rheology of Portland cement pastes. *British Journal of Applied Physics*. Vol. 6. May 1955. pp. 165-167.

2. TATTERSALL, G. H. Structural breakdown of cement pastes at constant rate of shear. *Nature*. Vol. 175, No. 4447. 22 January 1955. p. 166.
3. WELTMANN, R. N. Breakdown of thixotropic structure as a function of time. *Journal of Applied Physics*. Vol. 14, No. 7. 1943. pp. 343-350.
4. WELTMANN, R. N. Thixotropic behaviour of oils. *Industrial and Engineering Chemistry, Analytical Edition*. Vol. 15, No. 7. 1943. pp. 424-429.
5. TATTERSALL, G. H. *Rheology of cement pastes*. Thesis submitted to the University of London for the degree of M.Sc. 1954. pp.
6. VENKATRAMAIAH, s. Work done in compacting a known weight of concrete by vibration. *Magazine of Concrete Research*. Vol. 2, No. 5. January 1951. pp. 89-96.
7. FORSLIND, E. Fresh concrete. Chapter 7 of *Building materials—their elasticity and inelasticity*. Edited by M. Reiner. Amsterdam, North Holland Publishing Company, 1954. pp. 253-289.
8. REINER, M. *Deformation and flow—an elementary introduction to theoretical rheology*. Second edition. London, H. K. Lewis & Company Limited, 1960. pp. 346.
9. SAUL, A. G. A. *Suggested mechanical principles underlying the vibration of concrete*. London, Cement and Concrete Association, October 1958. pp. 36. Technical Report TRA/303.
10. NESSIM, A. A. *Rheology of cement pastes and fresh mortars*. Thesis submitted to the University of London for the degree of Ph.D. 1965. pp. 391.
11. BLACKMAN, A. Apparatus and methods for measuring the plastic qualities of lime. *Zement, Kalk, Gips*. Vol. 7, No. 5. May 1954. pp. 197-205.
12. VENKATRAMAIAH, s. Thesis submitted to the University of London for the degree of M.Sc. 1950.
13. INSTITUTION OF CIVIL ENGINEERS. Investigation on the vibration of concrete. Interim Report No. 1. *Journal of the Institution of Civil Engineers*. Vol. 5, No. 5. March 1937. pp. 435-449.