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Influence of aggregate volume concentration upon the workability of concrete and some predictions from the viscosity–elasticity analogy

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The argument produced by Dr Hobbs is based on so many false premises and his conclusions are so misleading that we feel it is necessary to examine the whole of his paper in detail.

After quoting the well-known equation for the Bingham model, Dr Hobbs says that “a single-point test may provide useful information regarding the workability of concrete if” any one of three conditions is fulfilled.

His first condition is “(a) a high rate of shear occurs both in the single-point test and when the concrete is placed on the site”. As he points out, the ‘high rate of shear’ must be high enough for $\tau_0 \leq \mu \dot{\gamma}$, but he does not seem to realize that this is not the case in practice. Practical concretes are characterized by flow curves that *cross* at rates of shear that occur in practice. Furthermore, non-laminar flow would be produced at rates of shear well below those required to satisfy this relationship.

His second condition is “(b) the same rate of shear occurs both in the single-point test and when the concrete is placed on the site”; he seeks to justify this by giving a reference to a paper by one of us⁽¹⁾ in which a similar statement is made, but rather surprisingly he omits to include the information that that statement is followed by a major qualification. That qualification is: “The danger is that the proposed

imitation of practical conditions may not be good enough and if it is not, the test is capable of ranking samples in reverse order or in what appears to be a random order. In addition, of course, each practical set of circumstances may require its own test so the first possibility is not of real practical interest”.

His third condition is “(c) τ_0 is broadly related to μ ”. It is not easy to decide what is meant by ‘broadly’ in a scientific paper. However, even if there is a good correlation between τ_0 and μ , which may occur for a restricted range of mixes, it still does not follow that a single-point characterization is adequate. For a correlation coefficient significant at better than the 0.001 level, the band may still be so wide that, for a given value of μ , values of τ_0 may differ by a factor of 2 or 3, so that again concretes whose flow curves cross in the important practical range of shear rates may be produced. In general, the correlation coefficient is of the order of 0.2 (Figure 1), so that τ_0 and μ are not even ‘broadly’ related.

Thus it is clear that none of Dr Hobbs’ three conditions applies, even approximately.

Dr Hobbs next quotes the Einstein equation, which applies for $V_a < 0.05$, as a preliminary to considering concrete in which $V_a \approx 0.7$. When we pointed out privately to Dr Hobbs, some eighteen months ago, the incorrectness of his assumption of Newtonian behaviour and the irrelevance of the Einstein equation to the system now under consideration, we also drew his attention to the work of Simha and many

* Pages 191 to 202 of *MCR* 97.

Figure I: Relationship between τ_o and μ (correlation coefficient = 0.19).

others who had been concerned with modifications to take into account some of the factors that had been ignored by Einstein in his derivation. The papers by Frisch and Simha⁽²⁾ and Hinch⁽³⁾, referred to in passing by Dr Hobbs, are concerned with colloidal suspensions and polymer solutions. A brief examination of one of these should indicate to any reader that the subject is a very complex one, not likely to be advanced by the consideration of a much oversimplified model.

The assumption that “the cement-paste fraction in concrete is a Newtonian fluid” is, of course, wrong and is not acceptable even for the purposes of permitting the development of an approximate theory. Cement paste is a very complicated rheological system and, although it is known that it approximates to the Bingham model at low shear rates, at higher shear rates it undergoes a structural breakdown which requires at least four constants to describe it. This has been known for over 20 years and is further evidence to destroy Dr Hobbs’ belief that he can simplify matters by assuming high shear rates.

There does not seem to be much point in arguing that “if rigid aggregate particles are ‘suspended’ in the cement paste, the resulting concrete (sic) will still behave as a Newtonian fluid, provided such factors as collisions, turbulence etc are unimportant . . .” because later in the paper there is an attempt to apply the theory presented to real concretes in which there will be particle/particle contact and interference. In any case, the statement is not true; for example, a ‘concrete’ made by substituting a Newtonian sugar solution of appropriate viscosity for the cement paste fraction exhibited pseudo-plastic behaviour (Figure II).

If, instead of using a crude averaging procedure for

considering shear stresses and shear rates, Dr Hobbs were to attempt to consider his simple model in detail, and try to provide a diagram showing the balance of forces on particles, he would find that his model is quite inadequate. Indeed, this might have been indicated, as suggested above, by a consideration of the paper of Frisch and Simha or of the work of Murata and Kikukawa⁽⁴⁾ which he quotes later on. All Dr Hobbs’ equations 7 to 9 are wrong and equations 10 stand some chance of being right only because he has introduced the arbitrary and unknown function $f(V_a)$. In fact, it is the introduction of this function that makes it possible to fit his equation 10a to the data of Vand⁽⁵⁾ and Eilers⁽⁶⁾. Of course, the same result could

Figure II: Relationship between speed and torque for a ‘concrete’ in which a Newtonian sugar solution of appropriate viscosity was used instead of the cement paste.

have been obtained by ordinary curve-fitting techniques without the preceding argument. So that preceding argument is not only wrong, it is irrelevant.

Dr Hobbs' equation 10b can be expressed in the form of a polynomial and this is the form used not only by Vand and Eilers but also by many other authors.

It requires considerable faith in the value of extrapolation to believe that empirical relationships derived from data on a system containing spheres of diameter 100 to 160 μm or bituminous droplets of diameter less than 5 μm are likely to be of value for application to concrete. In addition, there is no a priori reason for supposing that the $V_{a,\text{max}}$ obtained from the curves will be the same as $V_{a,\text{max}}$ obtained from separate compacting experiments. The fact that Vand's curves give values of about 0.5 for spheres suggests that it is not, because a value much nearer 0.7 would be expected from compacting experiments. Dr Hobbs himself finds that the presence even of water makes a difference to his experimentally determined values, but then assumes that the presence of cement paste will have no effect.

Dr Hobbs asks us to accept "concrete viscosities of 30, 20 and 10 poise to describe concrete with low, medium and high workabilities" but does not quote any evidence to justify his choice. They appear to have been selected quite arbitrarily to lie within the range of values plotted in his Figure 3, which has been derived from his own equations and a quite uncritical acceptance of values of cement paste plastic viscosity given by Ish-Shalom and Greenberg⁽⁷⁾. It is not easy to compare Ish-Shalom and Greenberg's results directly with those of other workers because their water/cement ratios are higher and are in the range where other workers found serious problems of sedimentation during the course of the measurement. However, it has been pointed out⁽⁸⁾ that their results are somewhat lower than might be expected.

Criticism may be made of previously reported measurements of plastic viscosity of concrete, and published values have varied widely but, so far as we are aware, nobody has ever suggested values as low as those assumed by Dr Hobbs. Murata and Kikukawa⁽⁴⁾, whose paper he quotes later on, found values ranging from about 530 to 400 poise for a 20 mm concrete and values from 90 to 60 poise for a 15 mm concrete.

Reply by the author

I thank Dr Tattersall and Dr Dimond for their comments on my paper. I maintain my view that the arguments and conclusions of my paper are substantially correct and in my reply I will show that the

Of course, the actual choice of values assumed for η_p , η_c and $V_{a,\text{max}}$ has a great influence on the values of V_a calculated from equations 12 and hence on the values of calculated aggregate/cement ratios used for comparison with the data of Road Note 4. For example, for a mix of very low workability, Grading 1, and 0.4 water/cement ratio, Dr Hobbs assumes $\eta_p = 0.39$ poise, $\eta_c = 30$ poise and $V_{a,\text{max}} = 74.3$ and obtains a figure for aggregate/cement ratio of 4.0 (i.e. the first entry in his Table 2). A change of assumed $V_{a,\text{max}}$ to his 'dry' figure of 68.7 results in a calculated aggregate/cement ratio of 3.4. If a better choice of viscosity values is made, e.g. $\eta_p = 40$ poise, $\eta_c = 400$ poise, and used with Dr Hobbs' assumed $V_{a,\text{max}}$ of 74.3, the calculated aggregate/cement ratio becomes 2.0. Therefore the agreement claimed by Dr Hobbs does not exist. Although he comments that "Changes in aggregate grading, however, influence the predicted aggregate/cement values in the opposite sense to the aggregate/cement values quoted in Road Note 4", he does not seem to attach a great deal of importance to that result. In fact, its importance is such that on its own it is sufficient to condemn the whole of his theory. It is clear then that the claim "it is concluded that the assumption of Newtonian behaviour is broadly in agreement with the known 'workability' behaviour of concrete" is quite without foundation.

All the criticisms relating to the derivation of equations 7 to 12 apply equally to equations 14 to 20. Murata and Kikukawa give values of plastic viscosity and yield value for several 20 mm and 15 mm maximum size concretes, but Dr Hobbs does not test his theory against these results; he chooses instead, from the same paper, the results they obtained on a highly artificial mortar (not "concrete" as stated by Dr Hobbs) made with fly ash and glass balls, and again the measure of agreement obtained is due primarily to suitable adjustment of the empirical constant $V_{a,\text{max}}$.

We make no comment on the viscosity-elasticity analogy except to say that it is irrelevant.

It should now be quite clear that to offer even tentative proposals for a mix design method based on this paper is not just premature, it is ridiculous.

The reason for the inadequacy of Dr Hobbs' theory is given in the fourth of his "four possible explanations" in which he acknowledges that "The theory proposed may not be correct". It isn't.

criticisms raised against my paper are without substance.

The criticisms raised by the contributors may be summarized as follows:

- (1) a single-point test cannot provide useful information regarding the 'workability' of concrete.
- (2) the theory is oversimplified and wrong.
- (3) the volume concentrations for close packing, which are used to test the theory, are incorrect, e.g. the volume concentration for close packing of spheres is about 70% and not about 55%.
- (4) the values chosen for the fluid and plastic viscosities are incorrect.
- (5) the suggested method of mix design is inappropriate.

The contributors' general point, that a complex situation must be dealt with by a complex theory, is an interesting one: do they, I wonder, reject the Kinetic Theory of Gases on the grounds that it is not possible to 'provide a diagram showing the balance of forces on particles' and that crude averaging procedures are used to deal with wide-ranging individual velocities? Do they reject experimental studies on the flow properties of cement paste on the grounds that the complicating factors mentioned in my paper are, as they imply, of overriding importance?

In making my reply I shall deal with criticisms in the order listed above.

SINGLE-POINT TESTING

In my paper I gave three reasons why a single-point test *may* provide useful information regarding the workability of concrete. the Contributors have not shown that reason (a) is incorrect. As far as I am aware, we do not know the rates of shear which occur in practice. the Contributors are therefore attempting to present their opinion which, while possibly true, is certainly not an established fact. On the contrary, there is some evidence which indicates that reason (a) is correct⁽⁹⁾.

The contributors have not shown that reason (c) is incorrect. To substantiate their claim, they present data obtained from turbulent flow tests on concretes of high air content. When the flow is turbulent, viscosity is not an important property; but, when the flow is streamline (steady and stable), the work done is expended solely in overcoming viscosity. In my paper, I attempt to predict the streamline flow behaviour of concretes of low air content. I concluded that the dependence of the predicted 'workability' of a Newtonian concrete is in general agreement with the observed 'workability' of concrete. I therefore suggested that either reason (a) or reason (c) was correct. (The theory presented for a Bingham concrete does not predict that τ_0 is proportional to μ .)

There is a further reason why a single-point test may provide useful information:

(d) when concrete is subject to vibration, a structural breakdown occurs which reduces the yield value τ_0 . [Nessim and Wajda⁽¹⁰⁾, Popovics⁽¹¹⁾, Jones, Brindley and Patel⁽¹²⁾, and Tattersall⁽¹³⁾ (p. 89).]

PREDICTING CONCRETE BEHAVIOUR

My paper deals with the influence of the aggregate upon the workability of concrete. The concrete is treated as a two-phase material consisting of aggregate particles in a cement paste matrix. Two cases are considered – Newtonian and Bingham.

I accept that rigorous prediction of the properties of fresh and hardened concrete is not possible, owing to its complex structure and geometry, and the numerous interacting particles. If attempts are made to predict and understand concrete behaviour, simplifications have to be made. Under shear or under load, the complexity of the shear-stress–shear-rate or the stress–strain distribution in fresh or hardened concrete may well go up as the aggregate volume is increased. However, this does not matter, because we are interested in the bulk properties of concrete and, except possibly for strength, extreme values are of little consequence.

Few attempts have been made to predict the flow properties of concrete, but many attempts have been made to predict the Young's modulus of concrete. In the case of Young's modulus, we assume that the paste is elastic, which it is not, and we assume that the Young's modulus is dependent upon just *three parameters*, which are:

E_a the Young's modulus of the aggregate;

E_p the Young's modulus of the paste; and

V_a the aggregate volume concentration.

We know from observation and simple reasoning that aggregate size has little effect upon E_c . (This is also true for shrinkage and creep, and flow⁽¹⁴⁾.)

Figure III compares three simple predictions for Young's moduli⁽¹⁵⁾ with the observed behaviour of concrete⁽¹⁶⁾. The simple approaches used do, in my view, improve our understanding of concrete behaviour; the fitting of empirical equations does not. It can be seen from the Figure that neither the stress nor the strain is constant throughout the concrete, both the stress distribution and the strain distribution lying somewhere between these two extremes. The elastic behaviour of concrete is similar to the behaviour of other particle composites.

Let us now run through the argument I use to predict the flow behaviour of concrete. In the case of the Newtonian paste, the following parameters are taken to be important:

η_p the fluid viscosity of the paste;

V_a the aggregate volume concentration; and

$V_{a,max}$ the aggregate volume concentration for close packing.

In the case of the Bingham paste the following parameters are taken to be important:

μ_p the plastic viscosity of the paste;

τ_{0p} the yield value of the paste;

V_a and $V_{a,max}$.

For convenience, I shall consider the Newtonian case only. The simplest assumption we can make is

Figure III: Relationship between Young's modulus and aggregate volume concentration. After Ishai⁽¹⁶⁾. Matrix: cement. Aggregate: Leighton Buzzard sand.

that the applied shear stress is uniform throughout the concrete. This assumption, since $\eta_c \rightarrow 0$ when $V_a \rightarrow V_{a,\max}$, leads to

$$\eta_c = \frac{V_{a,\max} \eta_p}{V_{a,\max} - V_a} \dots \dots \dots (1)$$

This equation gives a poor fit to experimental data (Figure IV). The assumption of plain stress is therefore not correct so, in my paper, a slightly more complicated approach had to be used. I let τ_c be the shear stress applied to the concrete and I let τ_a and τ_p be the average stresses in the aggregate and paste phases respectively.

The contributors suggest that equations 7 to 9 in my paper are wrong. If they look again at these equations, they will see that equation 8 expresses the fact that the shear stresses must balance, that equation 7 expresses the fact that the velocity gradients must balance and that equation 9 is obviously correct in the context which it is used.

It is perfectly true, as the contributors state, that the forces must balance at the paste–aggregate interface, but this does not alter my argument, because I deal with average stresses. Indeed, in the paper, I make use of the fact that the forces must balance at the paste–aggregate interface to obtain an expression for compressive strength.

The contributors criticize the way in which I deduce k_1 . In the paper k_1 was obtained empirically and the viscosity–elasticity analogy was used to show that the approach was sound. Einstein's equation was not used

Figure IV: Effect of aggregate volume concentration upon viscosity.

in the derivation except to show that the author's final equation did not conflict with Einstein's equation when $V_a \rightarrow 0$. In this reply, I shall obtain k_1 in a different way.

k_1 must meet the following conditions.

- (1) When $V_a \rightarrow 0$, equation 10 in my paper must reduce to Einstein's equation; therefore $k_1 \rightarrow 2.5$ when $V_a \rightarrow 0$.
- (2) When $V_a = V_{a,\max}$, all the applied shear is taken by the aggregate particles; therefore, from equation 9 or 10 in my paper, $k_1 \rightarrow \infty$ when $V_a \rightarrow V_{a,\max}$.
- (3) The average shear stress supported by the aggregate particles goes down as V_a goes up. That this condition is so may be argued as follows.

When 'concrete' containing a single aggregate particle is subject to a shear stress, τ_c , a stress concentration is induced by the aggregate particle (Figure Va). If an additional aggregate particle is now placed close to the first aggregate particle (Figure Vb), the viscosity of the volume C containing this additional aggregate particle is increased and, as a result, supports a greater proportion of the applied shear than volume B, whilst the volume D containing the original aggregate particle supports a smaller shear load than volume A. Thus, as the aggregate volume goes up, the average shear stress supported by the aggregate particles goes down.

Note that, in specifying these conditions, we are taking into account hydrodynamic interactions between the particles. The simplest equation which meets the three conditions is

$$k_1 = \frac{2.5 V_{a,\max}}{V_{a,\max} - V_a} \dots \dots \dots (2)$$

This equation is the same as equations 11 and 19 in my paper.

Figure V: Effect of aggregate volume concentration upon the average shear stress concentration.

I showed in my paper that the equation deduced for the viscosity of a Newtonian suspension, with the correct choice of $V_{a, \max}$, closely fits the experimental data published by Vand⁽⁵⁾ and Eilers⁽⁶⁾. I also showed that the 'sister equation' for Young's modulus closely fits experimental data. *The complicating factors, which the contributors believe are important, which I ignore in my paper, such as collisions, turbulence, Brownian motion, particle rotation, etc., cannot therefore be important, otherwise similar equations would not fit both the viscosity data and the elasticity data.*

The contributors suggest that the same equation for viscosity could be obtained by ordinary curve-fitting techniques, as many other workers have done. Such an approach does not advance our understanding of the subject – the simple treatment which I present does.

The contributors accuse me of being selective in the data which I choose to test my theory; this is not true. I would have been delighted to use experimental data on the effect of aggregate volume concentration upon the rheological properties of concrete to test the theories – as far as I am aware such data do not exist. If I have missed relevant experimental data, this is through ignorance on my part. In the paper by Murata and Kikukawa⁽⁴⁾, to which the contributors refer, τ_{oc} and μ_c are only quoted for four sand contents, which is insufficient to test the theory. I note,

however, that both τ_{oc} and μ_c increase with increasing V_a , as the theory predicts.

PACKING OF SOLID PARTICLES

The contributors question whether the close-packing volumes I use to test my theory are correct. There are many variables which control solid particle packing⁽¹⁷⁾ and, in consequence, the subject is a difficult one. Table I lists the close-packing volumes of single-size spheres with various systematic assemblages (Manegold, Hofmann and Solf⁽¹⁸⁾, Manegold and von Engelhardt⁽¹⁹⁾ and Gray⁽¹⁷⁾). The co-ordination number is the number of other spheres in contact with a selected sphere. It is apparent from the Table that, depending upon the packing distribution, the packing volume can vary widely. In the case of spherical particles of a single size, well compacted by a vibratory technique, McGreary⁽²⁰⁾ found that the volume concentration for close packing ranged from 0.58 to 0.625. The particles tested were steel, tungsten and aluminium. In the case of polystyrene spheres, 1 to 3 mm in size, Spooner (in results communicated privately) obtained a close-packing volume of 0.558.

It has been deduced by Gray⁽¹⁷⁾ from data published by Meldan and Stack⁽²¹⁾, Orr⁽²²⁾ and Evans and Millman⁽²³⁾ that, after simple deposition, spherical particles pack to higher densities than irregular ones, but if the bed is subject to vibration or compaction, the reverse applied. Table II gives the close-packing volumes, after vibration, for 'single-size' fractions of Thames Valley flint aggregate and Mountsorrel crushed granite. Note the lower packing volume for Mountsorrel aggregate.

TABLE I: Close-packing volume of systematic assemblages of single-size spheres.

Co-ordination number	Particle volume concentration
3	23.0
4	34.0
5	40.3
7	56.1
9	68.0
11	71.8

TABLE II: Close-packing volumes for a number of 'single-size fractions'.

Sieve size	Thames Valley	Mountsorrel
9.5 to 19 mm	0.59	0.55
4.8 to 9.5 mm	0.60	0.54
2.4 to 4.8 mm	0.57	—
1.2 to 2.4 mm	0.58	—
600 μm to 1.2 mm	0.59	—
300 to 600 μm	0.64	—
150 to 300 μm	0.64	—

So far I have considered particles of a single size. If a range of particle sizes is used, higher packing volumes are obtained. In the case of the typical gradings used in concrete, the close-packing volume ranges from about 70 to 80%, and in the case of spheres of selected sizes McGreary⁽²⁰⁾ obtained a packing volume of 95.1%.

In the light of the above discussion, I believe that the values of $V_{a,max}$ which I selected to fit the data of Vand⁽⁵⁾ and Eilers⁽⁶⁾, namely 0.48, 0.54 and 0.755, are reasonable close-packing volumes. The low value of 0.48 was used to fit data where the suspension was stirred just before testing⁽⁵⁾. In my view, this stirring will result in a more disordered arrangement of the suspended particles. The high value of 0.755 was used to fit the data obtained by Eilers where the particle diameter varied by a factor of 3.

PASTE AND CONCRETE VISCOSITIES

The values for the fluid and plastic viscosities of cement paste and concrete which I use in my paper may well be incorrect, as the contributors suggest, but this is because established workers in the field have provided little information on the flow properties of normal pastes and concretes. If, for example, one refers to the recent book published by one of the contributors⁽¹³⁾, one finds not a single value quoted for the yield value, plastic viscosity or fluid viscosity of normal pastes and concrete.

As far as I am aware, Ish-Shalom and Greenberg⁽⁷⁾ are the only workers who have carried out flow tests on cement pastes with water/cement ratios covering the range used in practice. It may well be that their values are on the low side. If we estimate μ_p from the mortar tests reported by Nessim and Wajda⁽¹⁰⁾ (Figure 13 of reference 10, $w/c = 0.42$), a value of about 3 poise is obtained after the first few seconds of test. This is an order of magnitude less than the values suggested by the contributors, but an order of magnitude greater than the comparable value obtained by Ish-Shalom and Greenberg⁽⁷⁾.

If the magnitudes of the ratio η_c/η_p used in my paper are correct, the data in Tables 2 and 3 of the paper remain unchanged. This will be the case if, say, η_p ranges from 3.9 poise when $w/c = 0.4$ to 0.58 poise

when $w/c = 0.8$ and if η_c for concretes of low, medium and high workability are 300, 200 and 100 poise respectively.

AN ALTERNATIVE METHOD OF MIX DESIGN?

The contributors suggest that to offer even tentative proposals for a mix design method based on the paper is ridiculous. I believe that the method proposed is sensible.

If we accept that the starting point for mix design is a knowledge of $V_{a,max}$, the following observations can be made:

- to produce a workable concrete, V_a must be less than $V_{a,max}$;
- the greater the difference between V_a and $V_{a,max}$, the higher the concrete workability;
- to produce concretes of similar workabilities, $V_{a,max} - V_a$ must be increased as the water/cement ratio is reduced;
- for the same V_a , the workability of concretes containing crushed rock aggregate is lower than that for concretes containing natural sand and gravel (see Table II).
- for the same V_a , the workability of mortars is lower than for comparable concretes (see Table 1 of my paper).

In Table 6 of the paper, the 'theory' is used to predict V_a for different values of $V_{a,max}$, water/cement ratio and concrete viscosity. I state in the paper that the predicted values can only be used as a rough guide (the aggregate/cement ratios in Road Note 4 are, also, only an approximate guide) because firstly I realize that the theory and predictions are approximate and secondly I realize that the $V_{a,max}$ as measured in the paper is not necessarily the same as $V_{a,max}$ within fresh concrete. If I had managed to predict concrete behaviour correctly, there would be no need 'to make and test a trial mix'. Nevertheless, readers who are familiar with aggregate volume concentrations will recognize the predicted values as sensible both for $w/c = 0.40$ and for $w/c = 0.80$ (see also Table III and Tables 2 and 3 in the paper).

FINALLY, on a non-technical note, impartial readers

TABLE III: Predicted aggregate/cement ratios for workable concrete. Aggregate density 2.6 g/cm³.

Aggregate volume concentration for close packing, $V_{a,max}$ (%)	Predicted aggregate/cement ratio					
	$w/c = 0.40$			$w/c = 0.80$		
	Low workability	Medium workability	High workability	Low workability	Medium workability	High workability
68	3.3	3.1	2.6	6.0	5.9	5.6
72	3.8	3.5	2.9	7.1	6.9	6.6
76	4.4	4.0	3.2	8.7	8.5	7.3
80	5.2	4.6	3.5	10.6	10.3	9.5

will by now be aware that there are considerable differences in opinion on this subject, a fact also borne out by discussion at recent meetings and symposia. This discussion should bring these differences

into the open – which is healthy – and I hope that other workers will be stimulated to join the debate*, perhaps even to do some work on an important subject.

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CORRIGENDA

The following corrections apply to the original paper.

In equation 10a, the denominator of the term in brackets *should read*: $1 - V_a$

In equation 16, the denominator of the term on the right-hand side *should read*: μ_c

From Table 4, “(mN s/m²) or” *should be deleted*.

In Table 5, the equation for Young's modulus of porous material, $E_{p,v}$ *should read*:

$$E_p \left[\frac{(1 - V_v)^2}{1 + V_v^2 + 0.5V_v} \right]$$

and the previous solution for Concrete shrinkage, S_c *should read*:

$$S_p \left[\frac{1 - V_a}{1 + V_a} \right] + \left[\frac{2V_a S_a}{1 + V_a} \right]$$

*Preferably by submitting entirely new papers, rather than continuing the present discussion. Ed.