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REPLY TO RÜSCH, JUNGWIRTH, AND HILSDORF'S DISCUSSION OF THE PAPER "ON THE CHOICE OF CREEP FUNCTION FOR STANDARD RECOMMENDATIONS ON PRACTICAL ANALYSIS OF STRUCTURES" *

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The authors welcome the Discussion by H. Rüsch et al., for it raises several important questions on which, unfortunately, no agreement has yet been reached by specialists in the field.

Comparison of the Proposed C.E.B. Creep Function with Test Data

Effect of Vertical Shifting

The figure of the discussers does not correspond to their proposed C.E.B. creep function (Ref. 4). The correct plot is shown in Fig. 6 and it is seen that the deviations from test data are unacceptably large. They are also greater than those in Fig. 1 of the paper, which pertains to the best possible fit by a function of the type proposed for C.E.B. recommendations.

In the figure of the Discussion the creep curves have been vertically shifted, which gives the appearance of a better agreement with test data, but implies a very strong age-dependence of the associated (not the actual) elastic modulus E. By deleting the time range from 0.01 day to 1 day, the associated values of 1/E have been obscured. In Fig. 7 the curves of the discussers are extended to 0.01 day and the 1/E-values obtained by taking the strain at 0.01 day are also plotted.

It is claimed in the Discussion that the disagreement for loadings of duration of less than 1 day "is of no value to the engineering practice". However, this is not true. To be sure, for long-time structural creep effects the detailed shape of creep curves up to 1 day (Fig. 8), as well as the strain increment from 0.01 day to 1 day, is indeed unimportant when the concrete is more than 7 days old at loading. For long-time predictions it does not matter much when only the short-time strain, 1/E, is arbitrarily distorted (see shifts a or b in Fig. 8, yielding curves 423 or 723). However, the total strain due to load at 1 day and beyond is very important. When 1/E is changed by shifting the whole creep curve (shifts c or d in Fig.

* CCR 5, 129 (1975)

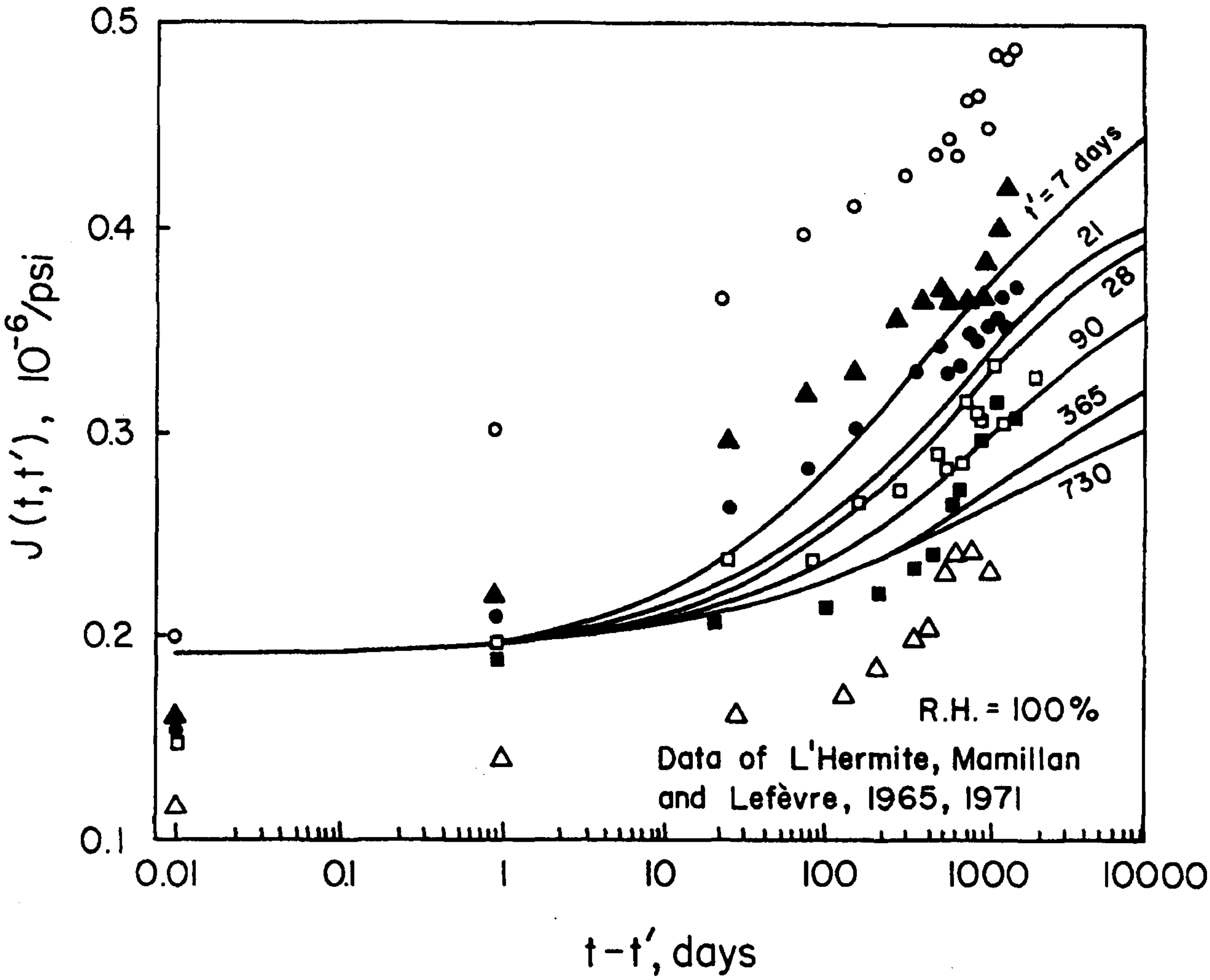

Fig. 6 The Plot of Creep Curves Exactly as Proposed for the C.E.B. Recommendations, Compared with Test Data

8, yielding curves 456 or 789), rather different total long-time creep strain may be obtained, which may result in a gross error in the predictions of long-time creep effects. It is the latter type of distorsion that was done in the figure of the Discussion.

By shifting the creep curves, the discussers transfer the age-dependence into E and assume that in E the age-dependence does not matter. But this is only true when the change in E is small (up to roughly 7%). It has been demonstrated by computer calculations (Ref. 16) that often the time-variation of E does have considerable effect on the theoretical predictions of creep effects. Nevertheless, since the discussers say that they "leave it up to the authors to check", it will be useful to do so by means of a simple example. According to the principle of superposition, strain $\epsilon(t)$ caused at age t by stress history $\sigma(t)$ that has begun at age t_0 is

$$\epsilon(t) = \int_{t_0}^{t} \left[\frac{1}{E(t')} + C(t,t')\right] d\sigma(t') \qquad (11)$$

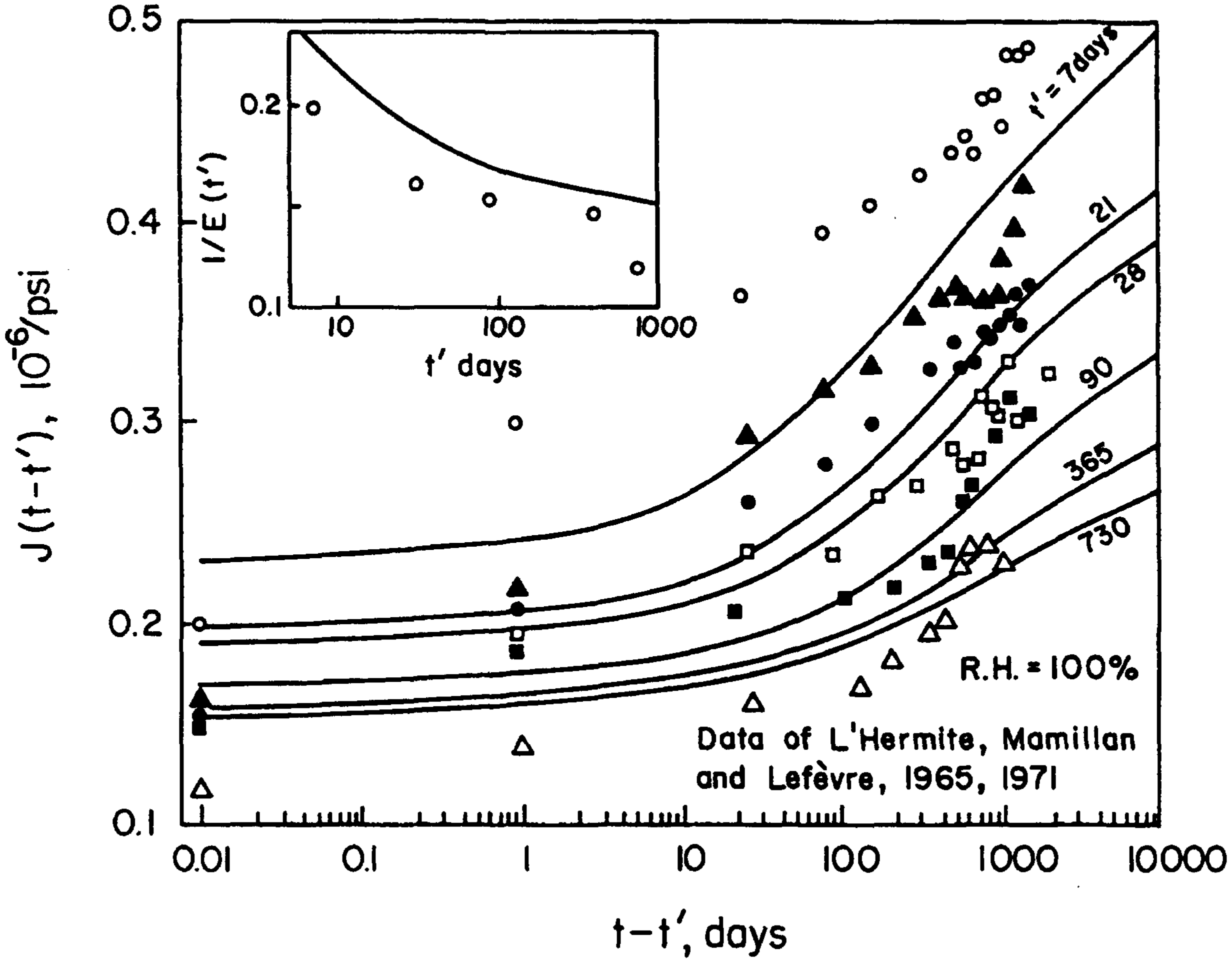

Fig. 7 Extension of the Vertically Shifted Creep Curves from the Discussion into Short Times, Intended to Show the Associated Variation of Elastic Modulus E

in which C(t,t') = specific creep = creep strain (total strain minus instantaneous strain) at time t caused by a unit stress acting since time t'. If the actual function E(t') is replaced by some arbitrary function $E_a(t')$ without changing C(t,t') (see the vertical shift c or d in Fig. 8), the error committed in the final strain is

$$\text{Error }(\epsilon) = \left[\frac{1}{E_a(t_0)} - \frac{1}{E(t_0)}\right] \sigma(t_0) + \int_{t_0^+}^{\infty} \left[\frac{1}{E_a(t')} - \frac{1}{E(t')}\right] d\sigma(t') \qquad (12)$$

To allow easy integration, one may quite realistically assume (18) that $1/E_a(t') = [1+\alpha(28/t')^{1/3}]/E_0$ where E_0 and α are constants and t' is in days. According to the proposed C.E.B. creep function, E(t') is taken as a constant, $E(t') = E_{28}$. As an example of stress variation, one may consider that stress $\sigma(t_0) = \sigma_0$ induced at age $t_0 = 7$ days gradually relaxes to a final value $\sigma_\infty = 0.25\ \sigma_0$ and that the relaxation curve is roughly similar to $t^{-1/3}$; this yields $\sigma(t') = \sigma_\infty\ [1+3(7/t')^{1/3}]$. Substitution of the fore-

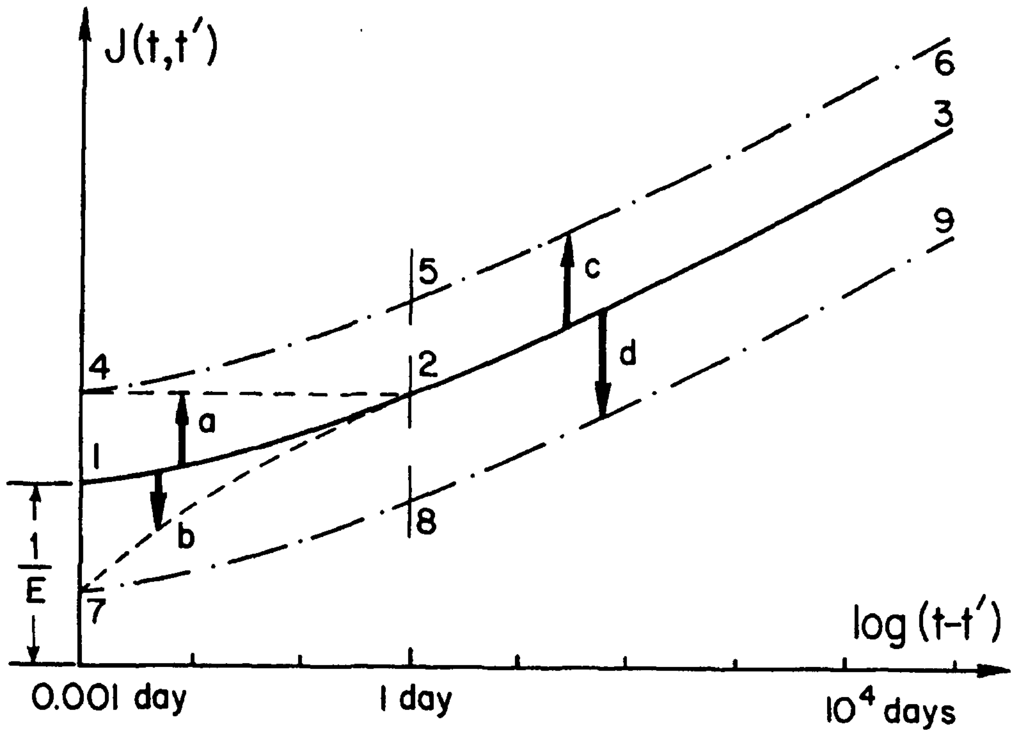

Fig. 8 Admissible (a,b) and Inadmissible (c,d) Distorsions of the Correct Creep Curve (123) with Regard to the Prediction of Long-Time Effects for Concrete More than a Few Days Old

going expressions into Eq. 12 and integration provides 0.74 $\alpha\ \sigma_0/E_0$. The variation of E in Fig. 7 is well described by function $E_a(t')$ with $\alpha = 0.43$ and this provides

$$\text{Error } (\epsilon) \doteq 0.32\ (\sigma_0/E_0) = 32\% \text{ of total strain} \qquad (13)$$

i.e., the error that would be committed in strain by vertical shifting of creep curves in the discussers' figure is in this example about 32% of the total strain causing stress relaxation.

Thus, it is obvious that the creep curves must accurately describe the total strain due to stress. Although the apportionment of the total strain in the elastic and creep parts is insignificant, arbitrary vertical shifting of creep curves leads to a serious error because it alters the total strain.

The variation of the associated elastic modulus such as that in Fig. 7 can, of course, be taken into account using step-wise numerical integration, as the discussers suggest. However, then the "improved Dischinger's method" which they proposed in Ref. 4 cannot be applied and the calculation is much more complex. Aside from that, the proposed C.E.B. recommendation does not tell the designer how to determine the variation of 1/E.

Other Aspects

It is wondered why the range of ages at loading from 7 days to 730 days is labeled "meaningless". Structures are designed for a life of about 40 years and when any long-time creep effect on stress distribution occurs, the stress varies gradually up to 40 years. According to the principle of superposition, the creep caused by all stress increments, even those after 730 days, must be included to reach correct long-time predictions. It is not the question of whether or not the loads applied on the structure will change after 730 days.

It is not understood how the discussers could attribute the better agreement of the creep curves obtained by optimization to a "large number of variable coefficients" and say that this "does not lead to a prediction method". The double power law underlying these curves (Eq. 10 of the paper) involves only four constants, namely E_0, φ_1, m and n, of which one (E_0) defines the basic value of elastic modulus E and two other (φ_1, m) define creep while at the same time defining the age-dependence of E (E represents 1/J for t-t' ≈ 0.01 day). This is the least number of constants one could possibly desire. On the other hand, in the proposed C.E.B. creep function (Eq. 2), the functions f and g are not characterized by any law and to define them at least one discrete value (f_i and g_j) is needed in every decade of log(t-t')— and log t'— scales, which amounts to at least 10 unknown parameters. Moreover, since the adjacent values f_i and g_j are not tied mutually by any law, Eq. 2 cannot be used for extrapolating short-time creep data into long-time creep data, whereas the double power law (Eq. 10) can be used for this purpose very effectively and does lead, therefore, to a prediction method.

The discussers deny that their proposed creep function has been "deduced from one typical creep curve and one typical recovery curve", disregarding data on the age effect. But then it is not clear how the age effect could have been taken into account because a single creep curve and a single recovery curve is sufficient to define the creep function in Eq. 2 uniquely, unless the recovery curve is disregarded even though its use is implied by introducing the notion of reversible creep. (A formulation using as the basic information the recovery curve instead of the creep curves at various ages at loading is disadvantageous for reasons which were stated on page 133 in the second paragraph, which was not commented upon in the Discussion.)

The fact that insufficient agreement of the existing (1970) C.E.B. Recommendation with relaxation data is sometimes found (item 3 in the Disdussion) is due, in the writer's opinion, mainly to the effect of drying. This is a nonlinear effect that cannot be described by any linear creep law based on principle of superposition (19). However, for massive structural members, in which the rate of moisture loss is small or nonexistent, the linear creep law predicts relaxation very accurately, provided that it fits also the creep data (Ref. 15). The writers concur that the effect of specimen size on creep does not agree very well with the existing (1970) C.E.B. Recommendation; but, according to their own studies, improvement of this shortcoming does not necessitate abandoning creep functions of the form of Eq. 1 in the paper.

Separation of Creep in Reversible and Irreversible Components

To make a definition of the reversible component of concrete creep meaningful, the strain which is ultimately recovered after a stress cycle, such as a pulse of constant stress beginning at age t_0 and ending at age t_1, would have to be essentially independent of ages t_0 and t_1. However, there is no test data indicating that for various ages t_0 and t_1 the ultimate recovery strains do not significantly differ. Therefore, reversible creep cannot be uniquely defined. Although it has been suggested that at least after longer creep periods the ultimate creep recovery is almost constant and equal to 0.4 of the instantaneous strain, no data on recovery of long (many year) duration are available, and when the available recovery curves are plotted versus the logarithm of the time elapsed since unloading, no approach to an asymptotic final value is usually apparent (even though it may appear so in the actual time scale).

It is illuminating to consider a rate-type stress-strain law for an aging viscoelastic material. Such a law has been shown to be capable of approximating a given creep function with any desired accuracy (1). The components of the reversible strain increments are in the rate-type law expressed as $d\sigma_\mu/E_\mu(t)$, where σ_μ are the hidden stresses (e.g., the stresses in the springs of the Kelvin chain model) and E_μ are the associated elastic moduli. For a definition of the total reversible creep strain to be admissible, it would have to be possible to integrate $d\sigma_\mu/E_\mu(t)$ as $\sigma_\mu/E_\mu(t)$; but this is impossible because, as a result of aging, E_μ is strongly time-variable. In fact, the E_μ-variation is much stronger than that of the instantaneous modulus E. Consequently, there is no physical and mathematical justification for the separation of the reversible component of total creep strain, as introduced in Eq. 1 of the Discussion.

The foregoing arguments do not imply, of course, that a separation of reversible creep could not be a useful practical expedient. Nevertheless, the fact that the creep function in Eq. 2 of the paper compares with the test data on creep (at various t') much poorer than other equally simple creep functions does prove that the separation of the total reversible creep strain is practically useless.

Method of Analysis of Structural Creep Effects

In view of the preceding analysis, it is hard to understand that the proposed C.E.B. creep function could have any other purpose but to tailor the creep description to the "improved Dischinger method" of structural creep analysis. It is true that the proposed C.E.B. creep function can be applied with other methods of analysis; but the "improved Dischinger method" cannot be applied for creep functions of other forms.

Mention has been made of the age-adjusted effective modulus method (Refs. 15 and 16), which represents a refinement of the method originally discovered by H. Trost. Here, one has a method which allows predicting creep effects in structures by a simple elastic analysis using the age-adjusted effective modulus E" in place of the actual elastic modulus. By contrast, in the "improved Dischinger method" one needs formulas based on integration of differential equations, and this is obviously more involved. It is unclear why the discussers claim the opposite. It has also been shown that, aside from greater simplicity, the age-adjusted effective modulus method is much more accurate in comparison with the exact solutions based on principle of superposition (Ref. 16). The preceding facts have recently been independently confirmed at the University of Toronto in an extensive study by Bruegger (20), who compared various methods of analysis in a vast number of carefully documented examples involving essentially all practical creep problems.

The objection has been previously raised that in Trost's approach a table of a certain coefficient is needed for determining E", so that an engineer on an isolated island would be unable to use the method. However, he could not use the "improved Dischinger method" either because he would need a table or graph of the creep function. A table or graph of the coefficient needed does not take more space than the graphs for the creep function itself and could be published simultaneously with it.

Conclusion

From the foregoing analysis it becomes even clearer that the general form of the creep function in the existing (1970) C.E.B. Recommendations is better than the proposed one and should be retained until a truly improved form is found.

"Let the users judge", the concluding call of the discussers, certainly sounds logical. However, the vast majority of engineers in the design offices do not have time to make their own comparisons with test data and with other methods of analysis. They need standard recommendations which they can take for granted. Let the creep specialists in committees judge first.

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REPLY TO JORDAAN AND ENGLAND'S DISCUSSION OF THE PAPER "ON THE CHOICE OF CREEP FUNCTION FOR STANDARD RECOMMENDATIONS ON PRACTICAL ANALYSIS OF STRUCTURES" *

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The authors appreciate the discussion by Jordaan and England but cannot agree with their four objections for the following reasons.

(1) Validity of the principle of superposition for concrete creep is, of course, limited. However, all practical methods in use today, including the improved Dischinger's method and the rate-of-flow method, are described by linear relationships and this automatically implies the principle of superposition as the underlying assumption, whether or not the creep function has been set up by considering the creep curves at various ages at loading, t'. The deviations from the principle of superposition, as mentioned in the discussion, are nonlinear effects and in the authors' opinion it is a misconception when one is trying to correct them by any creep law which is linear. The fit of the test data for unloading is improved by the afore-mentioned methods only at the expense of sacrificing something else, i.e., the fit of unit creep curves at various t'. (This fact is, however, obscured when the creep curves are plotted in the actual rather than the logarithmic time scale.) The only possible remedy is a nonlinear creep law.

The authors also disagree with the statement that the principle of superposition overestimates stress relaxation. Within the working stress range this is found only for relatively small and rapidly drying specimens, the cause being the nonlinearity of the effect of drying on creep; see (21) and Ref. 1 of the paper. This error cannot be corrected by means of a linear creep law.

Using a more accurate computer algorithm, the authors have recalculated the stress relaxation curves from the creep curves for the data of Ross (Ref. 1 of the Discussion) as well as the data of Bureau of Reclamation; see Figs. 10 and 15 in Ref. 15 of the paper. It appeared that the predictions agree as closely as one might desire.

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* CCR 5, 129 (1975)

The error of the principle of superposition of creep curves of virgin concrete in the working stress range is not serious unless not merely the stress but also the strain decreases, as at sudden unloading; but compared to the relaxation regimes this is a case of lesser practical interest for structures.

For these reasons, the authors dispute the claim that "the fits of virgin creep strains for virgin specimens loaded at large ages are largely academic". It should be also noted that the close agreement of Eq. 2 with the experimental data mentioned by the discussers is found only when creep curves are plotted in the actual time scale, which permits only one order of magnitude of the time delays (say, from 10 to 100 days) to be graphically represented. When replotted in the logarithm of creep duration, the same comparisons look unfavorable. There is no reason why the stress redistributions due to creep between 10 and 100 days should be more important than those between 100 or 1000 days, 1000 and 10,000 days, or 1 and 10 days, provided that the creep properties change substantially (due to aging) in each of these spans.

The increase of irrecoverable creep at transient temperature or humidity conditions can be modeled by Eq. 2, as mentioned by the discussers, only to a limited extent, especially when both short and long delays are considered and the opposite effects of humidity during and after its change are taken into account. According to authors' recent (as yet unpublished) analysis of available test data, a better model can be attained when the creep rate derived from Eq. 1 of the paper is multiplied by a factor which grows with the rates of drying shrinkage (or swelling) and thermal shrinkage and decreases with decreasing humidity or temperature. This formulation can reflect the increase of irreversible creep which occurred during or shortly after drying or temperature change and, at the same time, it can correctly model the fact that at a decreased humidity or raised temperature the reversibility of creep is about the same as that for saturated concrete at room temperature, provided that sufficient time needed to achieve internal moisture equilibrium has been allowed.

(2) The term "theoretically exact" does indeed apply here to analysis based on the superposition method. This is justified by the fact that all formulations under consideration are linear and, therefore, imply the principle of superposition as the basic assumption. The only difference is that the method which the discussers call "the superposition method" applies the superposition to the actual creep curves as measured, while other methods (e.g., Eq. 2) are equivalent to applying it to distorted creep curves. Direct comparisons of structural creep calculations with measurements on structures are important; but if they were used as the only basis for validation the method could not be regarded as a general one and could not be applied with confidence to structures other than those measured. To obtain a general method it is essential to base it on a certain well defined creep law and validate this creep law directly by comparisons with appropriate measurements of creep specimens. If a disagreement with measurements on structures is subsequently detected, one must decide whether the error is in creep law or in the method of calculation.

(3) The reply to the question of including the elastic strains and the age-dependence of elastic modulus coincides with that in the reply to a preceding discussion; see Ref. 22, Fig. 8, Eq. 13, and the associated comments.

(4) The discussers objection to the impossibility of decomposing the

total creep in reversible and irreversible components is also answered in Ref. 22. To be sure, summing the infinitesimal reversible increments is always possible but it is of no advantage if the result is not independent of stress history. For a constant load followed by a zero load the recovered strain component can be, of course identified, but it cannot be applied to the cases of other load durations and ages, and of time-varying stress.

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A DISCUSSION OF THE PAPER "A MODEL FOR THE CREEP OF CONCRETE" BY B.B. HOPE AND N.H. BROWN*

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The authors are to be congratulated on providing important original experimental data on changes of pore structure associated with creep and varying water content. While irreversible changes in pore surface areas due to changes in water content and to desorption-sorption cycles have recently been directly evidenced by X-ray scattering measurements (17), the analogous changes of pore structure due to sustained compression have so far been inferred only indirectly from hypotheses on creep mechanism and from the changes of mechanical properties due to creep. The most important result of the authors is a direct demonstration of these changes, and, in particular, of the fact that the fraction of pore volume occupied by the smallest micropores (interlayer space) is increased by creep. This is of considerable value for understanding the mechanism of creep.

As a possible mechanism of creep which could explain this effect, the authors adopt the hypothesis that under sustained stress the solid sheets of cement hydrate undergo severe physical distorsions (bending) and a general reduction in spacing of the sheets. Although the writer agrees with all other conclusions of the authors, he cannot accept the afore-mentioned hypothesis (conclusion 4 of the authors, p. 585).

The structure of the pore space must, of course, change, but it is difficult to imagine that the change could consist mainly in large bending deflections of adjacent sheets, as pictured in Fig. 1 of the paper. One difficulty is of geometric nature and becomes apparent when an array of a number of parallel sheets with interlayer spaces is considered (Fig. 2). Because the overall deformation of this array due to creep is known to be very small (less than 0.001), the large lateral deflection in one sheet would have to be accompanied in the adjacent sheet by an opposite deflection of roughly equal magnitude, which is impossible. On the other hand, if a displacement which is small relative to the spacing of the sheets were assumed, contrary to authors' concept, the accessibility of pores to methanol could not be significantly altered, because the surface forces require a large displacement if they are to change significantly. Large displacement of sheets should be possible on the end segments sticking out of the array of parallel sheets; but such loose segments cannot receive any significant portion of the macroscopic compressive load. An open configuration such as that

* CCR 5, 577 (1975)

at the bottom of Fig. 1 of the authors may exist within the parallel array, as shown by "A" in Fig. 2. However, if closing of such open configurations should result in small macroscopic strain, these configurations would have to be spaced mutually very far apart, and then their closing under compression could not add significantly to the micropore volume. Simply, within an array

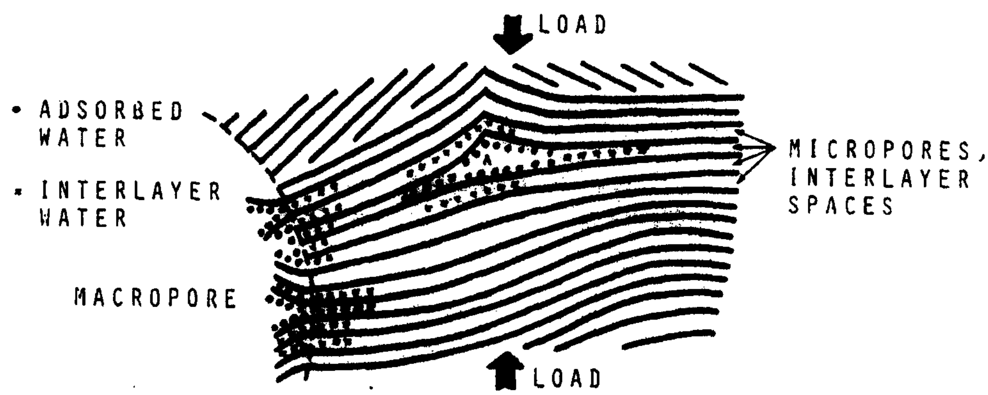

Fig. 2 Idealized Typical Arrangement of a Number of Sheets of Solids (Silicates) in Cement Paste

of a number of parallel sheets, the writer has been unable to picture the configurations from Fig. 1 of the authors in a form that would agree with the observed phenomena.

By means of large lateral deflections of sheets, it is also difficult to explain various other phenomena. E.g., consider a fully dried concrete in which no closing of pore spaces occurs because there is no creep. Then, why after subsequent rewetting the pore spaces would close under load, as is required by the fact that creep which can exceed the swelling on the previous rewetting takes place? Or consider the attainment of full water saturation. This inhibits the closing of the sheets because water has no empty space to go to and must compress in bulk. Then, why the creep at full saturation is about the same as creep of sealed specimens, which are not quite saturated? Furthermore, if the closing of interlayer space under load is the source of the additional compression creep due to drying (as compared with basic creep), why is it that drying causes an additional creep also in shear [or tension], in which roughly half [or nearly all] of interlayer spaces would have to get opened more rather than closed? Also, why not only a decrease but also an increase of water content causes additional creep (in compression, shear, or tension) (18), why any change of water content causes an additional recovery of compression creep (as has been recently demonstrated by experiments (18)), and why any change of temperature causes an additional creep?

The authors' important conclusion regarding pore structure changes under sustained stress can be alternatively explained by the hypothesis that creep is caused by migration (diffusion) of certain components of solids (probably Ca-ions) between load-bearing and load-free domains in the microstructure. This mechanism has been proposed in Ref. 19 and extended on pages 45 through 52 of Ref. 20, refining the preceding, inadequate model quoted by the authors as Ref. 8. The diffusion of solids can take place only through micropore water, and it almost ceases when this water is removed (dried state). When the micropore water itself is in motion, as in the process of drying or wetting, as well as temperature change, there exists a strong nonlinear coupling with the diffusion of solids, such that one diffusion flux accelerates the other diffusion flux (20). This hypothesis can explain all cases of additional creep mentioned in the preceding paragraph. The transport of solids and their reprecipitation in load-free areas can also explain the irreversible changes in the pore structure, including all the phenomena reported by the authors (see Fig. 19 of Ref. 20 and Figs. 5 and 6 of Ref. 19). In particular, noting that the diffusing solids are most likely to precipitate near the entrances to micropores, it is clear that even a minute amount of reprecipitated solids (as implied by the smallness of creep strain) is capable of blocking access to large micropore volumes (see Figs. 5 and 6 of Ref. 19; Fig. 19 of Ref. 20), the case demonstrated in the paper. This can also explain various other irreversibilities associated with sorption and desorption (19,20).

Thus, in the writer's opinion, the hypothesis of diffusion of components of solids coupled with diffusion of water along micropores seems to be a preferrable explanation for the phenomena reported in the paper. The authors' experimental results can also be regarded as an additional confirmation of this hypothesis.

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REPLY TO RUSCH, JUNGWIRTH, AND HILSDORF'S SECOND DISCUSSION OF THE PAPER[1] "ON THE CHOICE OF CREEP FUNCTION FOR STANDARD RECOMMENDATIONS ON PRACTICAL ANALYSIS OF STRUCTURES"

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It is rather unusual to receive a discussion of authors' reply[3], and it is very welcomed as an indication of the interest in this topic and its importance. Since the second discussion makes it clear that some of the questions still persist, the authors are pleased to provide clarification.

On Wylfa Vessel Concrete (First Paragraph of Discussion)

Argyris et al. (21) compared various creep functions with one set of experiments for one particular concrete, namely the Wylfa Vessel concrete (22,23). It is unclear why the discussers try to prove the faults of the product form by refering to a study of this particular set of experiments, for the discussers themselves state in their next to the last paragraph that "an attempt to prove...faults on the basis of one other set of experiments is ...meaningless" and "whoever does so is either inexperienced or unobjective."

The data used by Argyris et al. (21) represent smoothed experimental results (design curves). It should be noted that,from among the data of Browne et al. (22), Argyris et al. tacitly excluded the creep curves for some ages at loading (60 and 180 days, see Fig. 9c). Although this might be statistically questionable, exclusion of some curves in smoothing the data set seems to be justified by the fact that the three curves for t'=28, 60 and 180 days show an increase of creep with age (Fig. 9c) rather than a decrease, which can only be a random feature. It so happens that it is least favorable for the product form if the excluded data curves are those for t'=60 and 180 days.

Nevertheless, let it be assumed for the present that this exclusion is proper (Fig. 9a,b). Then, after studying such data it appears that the curves

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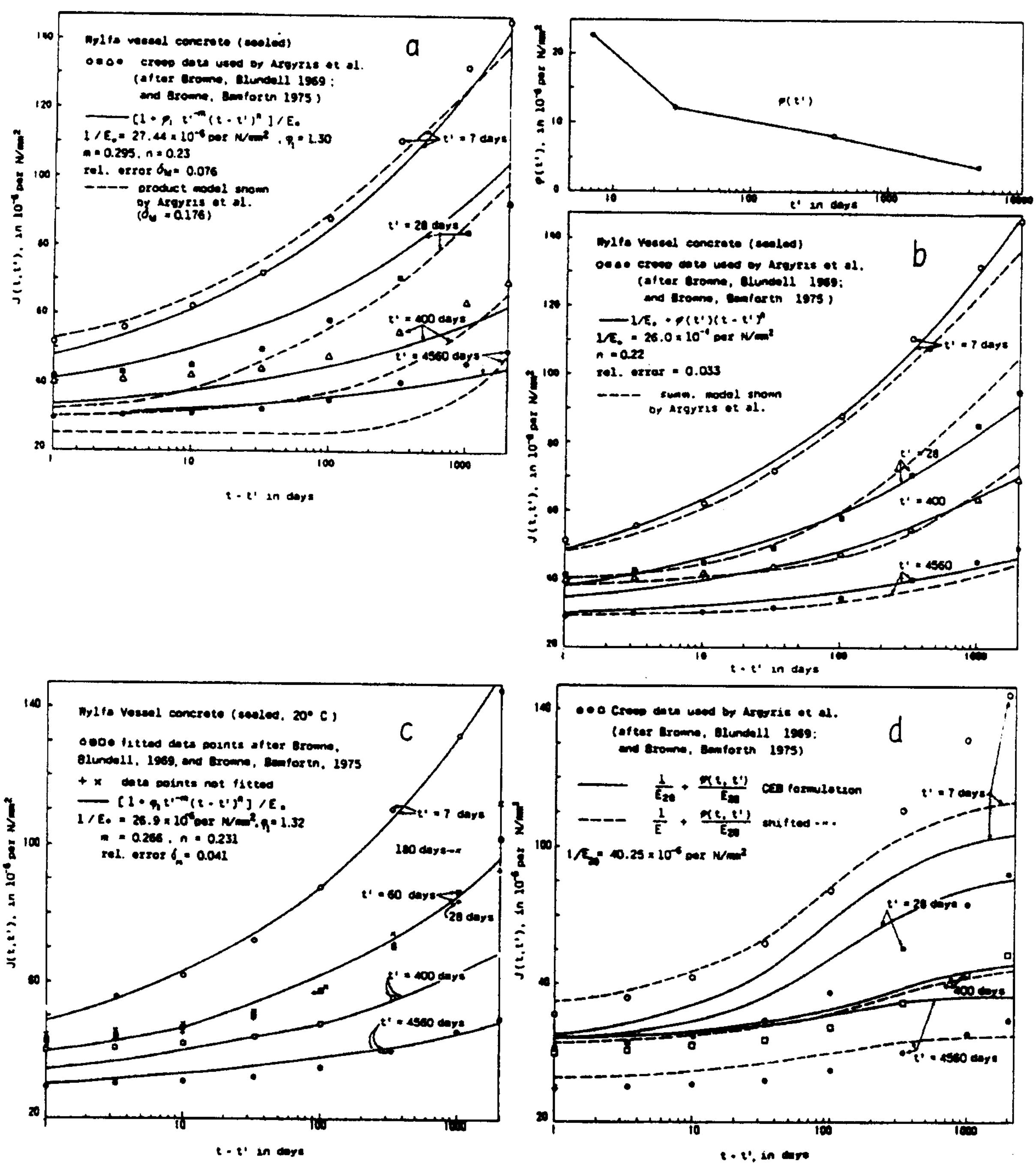

Fig. 9 Comparisons of Various Creep Functions with Test Data of Browne et al. (22,23).

for the product form, as indicated by Argyris et al. (21), are far from optimum fits. Using Marquardt optimization algorithm, the creep data have been fitted (26) by the product form $1/E_0 + \varphi(t') F(t-t')$ (Eq. 1 of the paper) of two special types: (a) the double power law (Eq. 10 of the paper), and (b) a more general form of the type $J(t,t')=1/E_0+\varphi(t')(t-t')^n$ where $\varphi(t')$ is an arbitrary function. The fits are drawn as solid lines in Fig. 9a, b and it is seen that the more general form gives a better fit. However, each of these fits is much closer than that indicated by Argyris et al. (21); their relative errors δ_M (root mean square error in J divided by root mean square of J) are as small as 0.076 and 0.033, respectively, while the fits shown by Argyris et al. (21) have δ_M = 0.176 for the product form and δ_M = 0.147 for the summation form. The errors in both cases are in fact so small that any effort for further improvement is meaningless in view of the random scatter which is apparent from the reversed sequence of the creep data for t'=28, 60 and 180 days (Fig. 9c). Furthermore, in addition to the scatter with regard to t', the measurements exhibited also considerable scatter with regard to t-t'. This is not apparent from the smoothed data (22) used by Argyris et al. (21), but it is clear from Ref. 24, in which the same data were published in greater detail and averages of measured values were indicated. These averages differ appreciably from the data (design curves) (22) used by Argyris et al. (Fig. 9a,b); but they are less smooth, which lends some degree of justification for preferring the data from Ref. 22.

Comparison of these data with the prediction by the discussers' formulation now adopted by C.E.B. (European Concrete Committee) (25) is shown in Fig. 9d (29). The comparison is made both for constant E, which is the case to be considered in accord with C.E.B. recommendations during the period under load, and for arbitrarily variable E. In the latter case a large vertical shift of creep curves is necessary to achieve an acceptable fit, just like that in Fig. 1 of the first discussion[4]; the fallacies in such vertical shift are discussed in the first reply and also later in this reply.

The same data (22,23) have also been fitted excluding the curves for t'=28 and 180 days, instead of those for t'=60 and 180 days (Fig. 9c). In this case the double power law fitted extremely well (see Fig. 9c), giving a relative error of only 0.041, while the fits for the summation form and for the new C.E.B. formulation became even worse than those in Fig. 9a,b. Since the power-type dependence on age t' agrees with most other data, it seems to be more appropriate to exclude the curves for t'=28 and 180 days (rather than those for 60 and 180 days) if any such data smoothing is carried out.

Consequently, the authors are afraid that the discussers' interpretation of the example given by Argyris et al. (21) might not be complete. Indeed, the data on Wylfa Vessel concrete (22,23) show again the product form to be vastly superior.

On Section 1.- Inadmissible Shifting of Creep Curves

In their last paragraph of Sec. 1, the discussers state that "Eqs. 1 and 2 have been used in presenting the data in Fig. 1" of their first discussion. However, the fact that Eq. 1 is written with a variable elastic modulus $E_c(t_0)$ does not mean that $E_c(t_0)$ can be given an arbitrary value, and especially not such unreasonable values (Eq. 3 of second discussion) as those implied by Fig. 1 of first discussion (E_c=E). Furthermore, even if realistic values of $E_c(t_0)$ were considered (Fig. 2 of second discussion), new C.E.B. recommendations (1976) do not indicate how these values should be determined. Anyhow, consideration of the E-variation during the period under load is not intended in C.E.B. recommendations, as the discussers admit below their Eq. 1. Undoubtedly, the reason is that the "improved Dischinger method", whose applicability is contingent upon the use of the summation form for the creep function, would become too complicated in case of variable E.

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[4]CCR 5, 631-634 (1975)

However, the point is not whether one "correctly assumes $E_c(t_0) \neq E_{c_{28}}$," as the discussers state in conclusion on Sec. 1. Rather, the point is whether the total strain $J(t,t')$ produced by unit stress (sum of elastic and creep strains) is predicted correctly by the formula used for the creep function (for $t-t' \geq 1$ day). How the total strain is subdivided into instantaneous (elastic) strain and creep strain is of little importance in most structural calculations, for it is well known that if the age at loading is t_0, then the values of creep function at stress durations $t-t' < 0.1\ t_0$ are irrelevant for long-time response (provided the loading is steady). This means that the stress relaxation predicted on the basis of creep curve 123 from Fig. 8 of the first reply is the same as that predicted on the basis of creep curve 423 or 723. Thus, part of strain called creep strain can in fact be adjusted at will by a vertical shift of the whole creep curve (for $t-t' > 1$ day; see Fig. 8 of first discussion), but only if the shift is compensated for by a fictitious variation of E to be used in calculations so as to keep the total strain unchanged. Without such a shift-compensating variation of E (Eq. 3 of second discussion), there is no way to cancel the 32% error found in the example of stress relaxation in the first reply. As long as the user does not intend to complicate his creep calculations by taking into account during the period under load the shift-compensating variation of E according to Eq. 3 of second discussion, the vertical shifting of creep curves which was used in Fig. 1 of the first discussion to obtain a better fit is i n a d m i s s i b l e because it would imply altered values of total strain or $J(t,t')$.

The fictitious variation of E which would have to be used in conjunction with the discusser's shifted curves in order to preserve the same $J(t,t')$-values was figured out by the leftward extensions of the shifted creep curves, as shown in Fig. 7 of first reply. The discussers apparently thought that these $E(t)$-values were proposed in the first reply, although this was not the case. Thus, the curve which is labeled "Bažant et al." in Fig. 2 of the second discussion and is reproduced here as Fig. 10 should actually be labeled as is shown in Fig. 10.

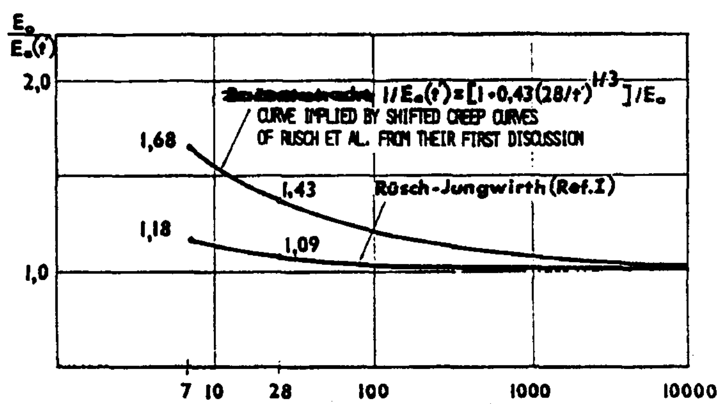

Fig. 10 Reproduction of Fig. 2 of the Discussion with Corrected Text.

On Section 2.- Effect of Time Dependence of Elastic Strain on Relaxation

The shifting of creep curves in Fig. 1 of the first discussion would be of no practical consequence if it had little effect in practical calculations. The example of stress relaxation in the first reply was intended to show that there exist some practically important cases where this is not so.

Strictly speaking, one ought to compare relaxation predictions based on (I) actual creep curves (Fig. 6 of first reply), and on (II) shifted creep curves (Fig. 1 of first discussion and Fig. 7 of first reply). However, calculations may be simplified by noting that the difference between these two cases results solely from the differences in elastic modulus E and can be evaluated from Eq. 12 of first reply. To make the calculations in a simple way which the reader can check without a computer, the shift-compensating fictitious variation of E(t) was approximated by a formula (as quoted in Fig. 2 of second discussion). Then an example was solved to show what is the difference in the prediction of stress relaxation when this shift-compensating E(t)-variation is considered, as it ought to be, and when it is neglected, which would be dictated by practicality of design office calculations and would not be disallowed by the C.E.B. recommendations. Again, to keep the calculations simple, it was chosen to compare the strains corresponding to a typical chosen relaxation curve rather than the stresses corresponding to constant strain. (This is possible because the percentage error in both cases is in fact about the same; see the sequel.) For convenience, a typical relaxation curve was described by a formula, quoted in Eq. 3 and Fig. 1 of the second discussion. The discussers may have overlooked the intended purpose of calculating the effect of E(t)-variation, as stated on pages 636-637 of the first reply.

Nevertheless, it is reassuring to see that the discussers obtain the same value ($0.32\ \sigma_0/E_0$) when they calculate in their own way the effect of E(t)-variation (cases (a) and (b) below Eq. 3 of the second discussion). However, it should be noted again that this is not how much the new C.E.B. formulation differs from some method "proposed by the authors" (case (b)), but how much it differs from Eq. 3 implied by the shifted creep curves in Fig. 1 of the first discussion. The error of 32% is the error caused when the creep curves are arbitrarily shifted without compensating for it by means of a change in E(t). Thus, the authors are afraid that the argument below Eq. 3 of the second discussion does not address the point.

Discussers' calculation of cases (c) and (d) demonstrates that the effect of the actual variation of E(t) compared to the assumption of constant E is relatively small ($0.09\sigma_0/E_0$), which the authors have not disputed.

The discussers state at the bottom of the page below Eq. 3 that "the error due to the time dependence of E becomes of even less signigicance", referring to the fact that the "creep strains which may be twice the elastic strains have been neglected" (in the example calculated). As a matter of fact, however, they have not been neglected. Rather, creep functions of the same long-time creep component and different elastic components have been compared. Thus, there is no reason to expect an error of lesser significance.

The discussers also add in this respect that "an error in strain is not equal to an error in stress". However, this is not true, as far as linearity of the creep law is assumed. For a linear creep law, the histories of stress and strain are related as $\epsilon(t)=\tilde{E}^{-1}\sigma(t)$ where $\tilde{E}^{-1}$ is the Volterra integral operator of creep (Ref. 1 of the paper). Consider an error in stress $\delta\sigma(t)=k\sigma(t)$ where k is a small number. Then owing to the linearity of operator $\tilde{E}^{-1}$, the relative error in $\epsilon(t)$ is $\tilde{E}^{-1}[k\sigma(t)]/E^{-1}\sigma(t) = kE^{-1}\sigma(t)/\epsilon(t)=k\epsilon(t)\epsilon(t)=k$. Hence, the relative error is the same. (For a time-dependent error $\delta\sigma(t)$, the comparison is more complicated and requires defining a suitable norm of the error; but the same result is obtained for the norm of the error.)

On Sections 3 and 4.- Fitting of Creep Functions to Experimental Data and Deduction of Creep Functions from Experimental Data

The discussers state repeatedly that their formulation, now adopted by C.E.B. (25), has been deduced from a "multitude of experimental data" and that it "describes the behavior of average types of concrete" (Sec. 3, end of 2nd par.; Sec. 4, lines 3 and 12; Sec. 6, line 7; Sec. 7, 2nd par.). However, the writers are aware of no publication showing h o w . In the writers' opinion, this would require showing comparisons of the new C.E.B. formulation with the relevant sets of data available in the literature. The only data comparisons shown in Ref. 3 of the second discussion with respect to both time and age at loading are Fig. 3, which is a creep recovery curve (not indicating, incidentally, any approach to some "final" value); and the creep curves for various t' in Figs. 4 and 5 and for a single t' in Figs. 9-11, in which no comparison of data with creep function is shown. The new C.E.B. creep function has been compared there only with the old C.E.B. creep function and with one measured deflection curve of a certain bridge, but not with any test data on both age and load duration effects. No more comparisons are given in the book quoted by discussers as Ref. 2. Although one of the writers raised the questions now discussed while serving as ACI representative on a C.E.B. Working Group on creep (since 1971), he was unable to receive any more comparisons with test data. Thus, it seems as if the new C.E.B. creep function has in fact not been compared with any extensive data set on the effect of both time and age at loading. Yet, the time curves of J(t,t') for various t' are the most fundamental characteristic of creep because every structure which is suffering stress changes due to creep is aging in the process.

By contrast, the following data sets, involving broad ranges of both t-t' and t', have been fitted (27) by the product form: 1) L'Hermite and Mamillan's data, 2) Dworshak Dam, 3) Shasta Dam, 4) Ross Dam, 5) Canyon Ferry Dam, 6) Gable and Thomass' data, 7) A. D. Ross' data, 8) Wylfa Vessel data by Browne et al. (26). (For various humidities many further comparisons are made for an extension of double power law in Ref. 28.)

Justification of any creep function should involve two steps: (a) Show that the mathematical form selected is capable of individually representing well any of the relevant test data available in the literature; and (b) determine the dependence of the coefficients in this creep function upon the type of concrete, and estimate the random differences from various test data within each particular type of concrete. It appears that the first step, which is essential for chosing the right mathematical form, has been omitted in deriving the new C.E.B. creep function. The fact that the creep parameters of double power law found by fitting differ from concrete to concrete is not surprising, and the dependence of these parameters on the type of concrete can be established. The main point is that the product form is capable of representing well various test data, while the summation form, now adopted by C.E.B., is not.

It has been also objected that only the general form of the creep function has been analyzed in the paper. Suppose, however, that the actual creep curves as proposed at that time for C.E.B. recommendations were considered, being found to disagree with test data. From experience, a possible response would then be to merely modify the creep curves keeping the same basic form, again without full-scope comparisons. Then another paper would have to be written, comparing these modified curves to full-scope test data, etc. It was for saving years of delay and the labor of doing this that the general form of creep function was considered in the paper. The purpose was to show that no matter how the creep curves are modified, better fits than the optimum ones shown in the paper cannot be obtained, unless the summation form itself, along with the "improved Dischinger method", is abandoned.

At the end of Section 3, the discussers point out that in double power law "the limiting value of J for t-t'=0 is erroneous". This seems to be a misinterpretation. The value of J for t-t'=0 is beyond the range of validity; it merely represents the left-hand asymptote of the creep curve plotted in log(t-t')-scale. What only matters is that the elastic modulus E, obtained as 1/J for $t-t' \approx 10^{-3}$ day, and even the dynamic modulus E_{dyn}, obtained as 1/J for $t-t' \approx 10^{-7}$ day, is represented by the double power law quite well.

The writers have been aware that superposition of the creep curves for double power law sometimes yields unrealistic shapes of recovery curves (Fig. 3 of second discussion). However, creep recovery is beyond the range of applicability of any linear creep law. Furthermore, the C.E.B. formulation itself represents recovery inadequately; see discussion of Fig. 11 in the next section.

It should be also noted that a reversal of recovery curve (Fig. 3 of second discussion) is not theoretically impossible. Indeed, there exist some recovery experiments which show just that (see Fig. 11c,i; and also Ref. 42), although majority of recovery tests follows a different trend (see Fig. 11).

On Section 5.- The Magnitude of Delayed Elasticity

The discussers refer again in Section 5 as well as 4 to creep recovery data which have been used to characterize the delayed elastic part of strain. As has been already mentioned in the second paragraph on p. 133 of the paper, it is inappropriate to use recovery data for determining J(t,t') because the principle of superposition, assumed in C.E.B. recommendations, does not apply when strain decreases (as in creep recovery), although it does apply when only stress decreases (which covers most practical situations). To fit creep recovery data, a nonlinear creep law would be necessary.

However, let us for a while disregard with the discussers this fact. The C.E.B. formulation is based on these two hypotheses: (I) The creep recovery curves are bounded; and (II) the ultimate recovered strain is independent of age at loading, t', and age at unloading, t_1. These hypotheses appear to be true when the creep recovery curves are plotted in actual time scale for $t-t^1$, as has usually been done in the past. The trouble is, however, that if the scale on the paper ends by 1 day, then the plot looks as if an asymptote were to be reached at 2 days; if the scale ends on the paper by 100 days, then the plot looks as if an asymptote were to be reached at 200 days, and so on. So, the "asympotic" value can be manipulated largely at will. Thus, the plots in actual $t-t_1$ scale obscure the creep recovery curve for times $t-t_1$ which fall out of a chosen limited time range. This would not matter if only $t-t_1$, say, from 0.1 to 1 day or from 10 to 100 days, were of interest; but if a creep function covering the full time range from 1 minute to 40 years is of interest, then plots in log $(t-t_1)$-scale must be considered. Such plots have been constructed (29) from nearly all relevant recovery data available in the literature (30-44), and they are shown in Fig. 11 (30-41). It is evident from these plots that in most cases the creep recovery curves are essentially straight in log$(t-t_1)$-scale and normally do not approach any asymptote, especially not within a 100-day or 1-year recovery period as has been previously assumed. (Actually, creep recovery data were plotted on p. 55 of Ref. 3 of

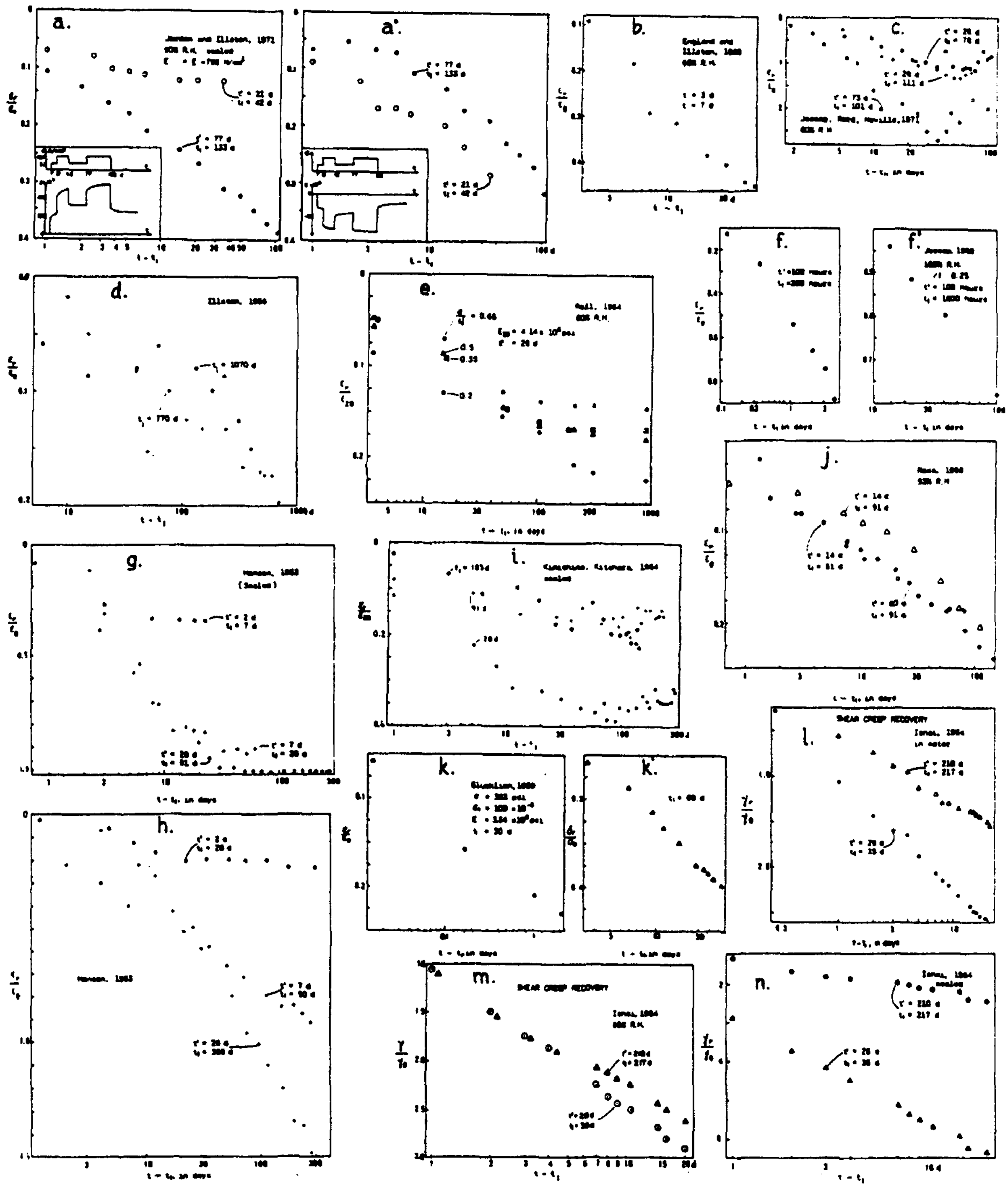

Fig. 11 Summary of Basic Experimental Data on Creep Recovery Available in the Literature (30-41)

the second discussion in log-time; the data point band was also steadily inclined up to the last point, and it is unclear why a horizontal asymptote was drawn there right behind the last point.) Furthermore, it is seen from Fig. 11 that creep recovery within a one-year period may range from 0.14 to 0.9 of the elastic strain.

Thus, hypotheses (I) and (II) on which the new C.E.B. formulation rests are tenable only for a rather limited time range, and for the full time range of interest they appear to be quite illusory.

In terms of rheological models, the new C.E.B. formulation with the "improved Dischinger method" corresponds to the well-known Maxwell model, in which the viscosity coefficient grows with age and the spring represents the effective modulus for the sum of the elastic strain and the final value of "delayed elastic strain". However, it is known from viscoelasticity that for a broader range of creep durations this model is an oversimplification for all real materials whose creep law is linear in stress.

In Ref. 28, optimization of data fits with the function $\varphi(t')F(t-t') + g(t)-g(t')$ has been reported. This function involves both the product form and the summation form (Eqs. 1 and 2 of the paper) as special cases. The optimum fits have not been appreciably better than those for $\varphi(t')F(t-t')$ alone and the flow term, $g(t)-g(t')$, came out to be negligible for the optimum fits. Thus, the flow term, which is basic to the "improved Dischinger method", appears to be generally a concept of dubious usefulness.

This conclusion agrees with the fact that in microstructure of cemment paste and concrete no viscous (or inelastic) strain can occur without producing elastic microstresses at the same time, while in a Maxwell model the viscous deformation of the dashpot representing the flow term does occur freely, without producing stress in any spring.

From the point of view of mathematical analysis and approximation theory, it is known that if a function of two variables, such as $J(t,t')$, is to be approximated by means of functions of one variable, it is normally much better to assume a product rather than a sum of functions of one variable. In fact, the product form corresponds to the well-known technique of separation of variables and represents the first term of the widely used expansion in a series of products, such as $\Sigma_\mu f_\mu(t')g_\mu(t-t')$ with $\mu = 1,2,\ldots$.

On Section 6.- Advantages and Disadvantages of Both Methods

The discussers state that the new C.E.B. creep function "is not of value only to one analytical method such as Dischinger method" (Sec. 6). The point to note, however, is that the "improved Dischinger method" is inapplicable for other formulations of creep, and the writers question whether this motivation was involved in selecting the new C.E.B. formulation. The Dischinger method, while hardly ever used in English and French speaking countries, has taken deep root in Central European countries. From this point of view, of course, the Trost method or its refinement, the age-adjusted effective modulus method, has the disadvantage of being new (although it is formally equivalent to effective modulus method which has been prevalent in English speaking countries). Yet, this method is applicable to any creep function, is simpler to use, and is more accurate. The only disadvantage, in the eyes of some, is that this method requires a table or graph of a certain coefficient; but such a graph does not take more space than a graph of the creep function (25).

The argument for the product form and against the summation form has so far been based on: (a) comparisons with experimental creep curves, (b) lack of thermodynamic justification of separating reversible and irreversible creep strain for an aging material, and (c) the preceeding critique of the concept of "delayed elastic strain". Recently, two further arguments have appeared: (d) a stochastic process model has been developed as an extension of double power law, and it yielded quite realistic empirical distributions of extrapolated long-term creep values (4); (e) the form of the creep function has been deduced from the fact that the viscoelastic properties of cement gel are essentially constant and the age dependence is due to the growth of the volume fraction of cement gel (4). This led to a certain power-type law for creep rate ("triple power law"), which seems to be reasonably approximated, for not-too-young concrete, by the double power law. It is also noteworthy that the triple power law (46) has time-dependent E_0 and that the theory (46) indicates that, for the double power law approximation, E_0 ought to reduce to a constant, as data fitting has already shown (27).

On Section 7.- Contribution by Other Discussers

It is unclear why the discussers question the fact that in the data of Hummel et al. the creep curves for concrete loaded at 28 and 90 days of age are rather close. This property is not "in contrast to most other test series"; see the multitude of test data plotted in Ref. 15 or Ref. 1 of the paper. The reason for the small difference is that log 28 and log 90 differ little compared with the full range of log t'.

In the penultimate paragraph of the second discussion, the question of being "inexperienced and unobjective" is raised. While the experience of the discussers is certainly above question, the important point is that of objectivity. It has been the pervading concern of the writers to rely on objective methods of evaluation; i.e., to use quantitative methods such as optimization techniques, to plot creep curves in log-time scales which do not obscure disagreement for short and long times, to show comparisons with all relevant data sets available in the literature, to avoid the temptation of presenting the fits in a manner which seems to indicate better agreement than there actually is, etc. The reader must make the final judgment on whether an objective approach has been taken.

Reply to the discussers' comments on the data comparison from Haas' discussion is left to that discusser. The writers are disappointed that the discussers "sternly object against the manner" in which the C.E.B. formulation has been discussed, and if the writers have done anything other than raise objective criticism, they offer their most sincere apologies. They would also be most happy to reply to further discussions.

Conclusion

It has now become still more firmly established that the newly adopted C.E.B. formulation (25) of the effects of load duration and age at loading needs to be revised. Until this is done, designers are well advised to use with regard to these effects the previous (1970) C.E.B. formulation.

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DISCUSSIONS

DISCUSSION OF L. F. NIELSEN'S PAPER*
"ON THE APPLICABILITY OF MODIFIED DISCHINGER EQUATIONS"

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It is very welcome that L. F. Nielsen has contributed to the discussion of the practical formulation for creep in concrete structures which is currently unfolding in the literature. He should be congratulated for improving the improved Dischinger equation which he previously devised and which has been proposed by Rusch et al. for C.E.B. International Recommendations. Nielsen's new linear second-order differential equation with variable coefficients considerably reduces the disagreement of the improved Dischinger equation with creep test data. At the same time, however, it must be pointed out that some of the conclusions of the writer appear to be unwarranted.

1. Separation of Reversible Creep.- The author asserts that his updated Dischinger equation shows the separation of the reversible part of creep to be in agreement with experimental data. However, the logic of this assertion is unclear because his Eq. 4a, as well as its simplified form (Eq. 4b) and the general expression in Eq. 2, does not include any term which would be recoverable upon unloading. To ensure reversibility, a linear creep component must have the form $f(t-\tau)$ where t is the current time, τ is the time of applying the stress, and f is a monotonically increasing bounded function. Although the function of the form $f(\psi_t - \psi_\tau)$ in Eq. 4a has some resemblance to the reversible creep term, it does not yield full creep recovery as t increases. Component $m_\tau f(t, \tau)$ in Eq. 2 has a form which is as general as $c(t, \tau)$ and includes both reversible and irreversible components. Nielsen's use of the term "reversible" does not conform to the notion of reversibility known from thermodynamics.

Thus, since the author's equation for creep does not really separate reversible and irreversible creep strains, it is not clear how the author can conclude on the basis of his analysis that the separation of reversible and irreversible creep is very useful.

2. Creep Recovery Data.- The author makes use of two widespread concepts of creep recovery of concrete (p. 153): (a) The limiting value of the delayed elastic strain is nearly a constant, i.e., independent of ages of concrete at loading and unloading; (b) the delayed elastic strain develops substantially more rapidly than the irreversible creep. These two concepts have been

*CCR 7, 149 (1977)

perpetuated through the recent literature, but they were actually based on a rather limited examination of test data. A study of numerous recovery data at Northwestern University (31) revealed that they in fact do not support these two concepts. When plotted in log-time scales, the creep recovery curves have usually the shape of straight lines and in most cases they do not approach any final value (horizontal asymptote). Normally, the recovery curve in log-time continues to be a straight line for many months and probably even years. The diagrams for numerous data are shown in Ref. 31.

3. Variation of Elastic Modulus.- Nielsen correctly points out that the poor agreement with test data which was demonstrated in some recent papers by this author can be explained by insufficient variation of the elastic modulus. However, it is necessary to point out also that a variation of the elastic modulus which is needed to bring the Dischinger-type methods in agreement with test data is unrealistically large, i.e., it far exceeds the actual growth of the elastic modulus with time (31) because a vertical shifting of creep curves is needed to fit long-time creep values (see dashed lines in Nielsen's Fig. 4 and 5). With reference to new code formulations, such as the newly introduced German DIN specifications, proposed to be incorporated into the C.E.B. International Recommendations, it has to be mentioned that these recommendations do not include any rule as to how a variation of elastic modulus should be considered in creep analysis. These recommendations, in fact, do not disallow the use of a constant elastic modulus, and this is what would be generally done in applying them in a design office. On p. 158, Nielsen himself mentions the desirability of $E = E_0$ = constant.

4. Extrapolation of Creep Data.- It must be emphasized that an acceptable fit of short-time creep data is sometimes needed not only for accurate calculation results but also for extrapolating short-time creep data into long times. Formulations such as the improved Dischinger method may obviously not be used to extrapolate short-time creep data because they cannot fit both short-time and long-time creep at the same time. By contrast, the recently proposed double power law gives a good fit of creep data not only for long creep durations but also for very short times; it gives correct values of the elastic modulus at various ages, and even of the dynamic modulus of elasticity. Such a formulation is much more suitable for extrapolating short-time creep measurements.

5. Consolidation Term.- The author's proposal for inclusion of a term which represents the additional nonlinear creep that is sometimes observed on virgin specimens is interesting, but a more extensive experimental verification would be needed. It is certainly disturbing that for one concrete, such as A. D. Ross' concrete, the so-called consolidation term appears to be significant, while for another concrete, namely the Shasta Dam concrete, this term appears to be zero. This is not surprising because the creep data and stress relaxation data reported for this concrete exhibit an exact agreement with the principle of superposition (i.e., superposition of "virgin" creep curves). These data represent perhaps the most extensive set of creep and relaxation measurements available, as far as the range of times and ages is concerned. In the writer's opinion, the arguments leading to consolidation term and superposition of "non-virgin" creep curves are vague and lack solid test support.

6. Simplicity of Creep Analysis.- It is important that practical recommendations are sufficiently simple, and this argument has recently been often raised. Regarding simplicity, everybody would probably agree with the following list of the methods of creep analysis in the order of decreasing simplicity:

1) Quasi-elastic analysis; this includes the classical effective modulus

method, as well as the recent age-adjusted effective modulus method, a refinement of Trost's method.

2) First-order differential equation with constant coefficients; this includes the rate-of-creep method which is due to Glanville (1930) and is known in Germany and some other countries as Dischinger method; and the improved Dischinger equation with a constant elastic modulus.

3) First-order differential equation with variable coefficients; this includes the same formulations as item 2, but with a variable elastic modulus.

4) Second-order differential equation; this includes Nielsen's updated Dischinger equation, the rate-of-flow equation as recently proposed by Illston, as well as the creep laws of Arutiunian, Aleksandrovskii, and many others.

5) Integral-type formulation.

The updated Dischinger equation (Nielsen s Eq. 3) is certainly one of the more complicated formulations for creep structural analysis. All of the Dischinger-type formulations are far more complicated than the quasi-elastic calculations based on some sort of effective modulus.

In this light, it is curious that so much effort is being expended on formulating creep laws that allow reducing the basic, integral-type, formulation to a differential equation. It is certainly simpler to use just a quasi-elastic analysis, such as the age-adjusted effective modulus method, which can be applied for any type of the creep function and thus allows concentrating exclusively on the best possible description of creep curves. Compared to Dischinger-type formulations, this method gives results which are in better agreement with the exact solution based on the principle of superposition, whose validity is implied in all linear formulations, including all Dischinger-type methods under discussion.

7. Concluding Remark.- One fact which makes resolution of the present disagreement about creep formulations for concrete more complex is the randomness of creep data and the influence of a great number of factors which are difficult to quantify. Clearly, there exists no method which would be best in every situation. However, in spite of that, it is possible to identify methods which are on the average better than others. The formulations which have recently been criticized by this writer are certainly not incorrect. The point is that one could do better. Although no dramatic improvement can be expected, it is still worthwhile to choose the best available formulation. That would not eliminate the problems with creep-induced cracking and deflection in concrete structures, but it would alleviate them to some extent. Thus, if there exists a formulation which clearly gives at least a somewhat improved agreement with test data and which is not more complicated than other approaches available, it should be adopted, even though it may mean breaking in some countries the long tradition of using Dischinger-type (rate-of-creep type) formulations. In this light, the writer is pleased to see that Nielsen by his effort in effect admits the need of doing better than the improved Dischinger equation, which currently forms the basis of the German DIN specifications and is proposed for being co-opted for the new C.E.B. International Recommendations.

References

31. Z. P. Bažant, E. Osman, "Reply to Second Rusch, Jungwith and Hilsdorf's Discussion of Nielsen's Ref. 25," Cem. and Conc. Res., 7, 119-130, (1977).

CEMENT and CONCRETE RESEARCH. Vol. 8, pp. 129-130, 1978. Pergamon Press, Inc
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ADDENDUM TO REPLY[1] TO RUSCH, JUNGWIRTH AND HILSDORF'S SECOND DISCUSSION OF THE PAPER "ON THE CHOICE OF CREEP FUNCTION FOR STANDARD RECOMMENDATIONS ON PRACTICAL ANALYSIS OF STRUCTURES"

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This addendum is published in response to a suggestion by Dr. K. Willam, Stuttgart, to whom the writers are obliged for pointing out that the reference to Argyris, Pister, and Willam's report[3] in the first section of preceding reply[1] was incomplete and could have been misinterpreted. In that report the term "product model" for creep did not have the same meaning as previously used in the literature. It actually referred to a degenerate form of the creep memory function, i.e., to the exponential series representation of aging material response to a unit stress impulse (see Eqs. 2.39 and 3.36 of that report), and not to a creep function chosen at the outset in the form of a product $\varphi(t')F(t-t')$ (although this form can be obtained from Argyris et al.'s exponential series by integration). This takes, however, nothing away from the conclusion[1] that the product form in the form of the double power law agrees with Wylfa vessel test data distinctly better than does the summation form, although the differences between the two fits are not significant in view of experimental scatter.

Furthermore, in Fig. 9a (p. 120), $\delta_M = 0.176$ should read $\delta_M^C = 0.176$ because in Argyris et al.'s report[3] the definition of the relative root-mean square error was not the same. There, δ_M^C referred to the "time-dependent part" of strain, while in the writers' reply[1] to Rusch et al.'s second discussion, δ_M was based on the total strain caused by stress. Thus, the δ_M-values and δ_M^C -values are not comparable.

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[3] Ref. 21 in the reply to second discussion, p. 128, CCR 7 (1977); see also J. H. Argyris, K. S. Pister, J. Szimmat, K. J. Willam, "Unified Concepts of Constitutive Modelling and Numerical Solution Methods for Concrete Creep Problems", Comp. Meth. Appl. Mech. Eng., Vol. 10, 199-246 (1977).

According to a private communication by Argyris et al., their δ_M-value based on total strain is 0.109 for what they call product model (the afore-mentioned exponential series, dashed line fits in Fig. 9a), and 0.093 for their summation model (not shown in Fig. 9; see Argyris et al.'s report[3]). It is noteworthy that the value 0.093(of which 0.084 corresponds to the creep part and 0.009 to the elastic part) is not much worse that the value δ_M = 0.076 for the product model in Fig. 9a. Argyris et al.[3] obtained this δ_M-value by adding the initial values from their fit of the elastic curve (Fig. 3.6) to their fits for the "time-dependent part" of strain from their Fig. 3.11. However, although this does provide identically defined δ_M-values, the results are not directly comparable because the data were not fitted in the same manner.

The afore-mentioned degenerate forms of creep function, which are equivalent to a rate-type creep formulation, greatly reduce time and storage requirements in computer analysis for creep. The product form (e.g., the double power law) is not of this form. However, this is no disadvantage because a simple subroutine converting any creep function into a degenerate form (based on Maxwell of Kelvin chain models) is available and the degenerate form obtained is so close that it is graphically undistinguishable from the double power law. This subroutine forms an internal part of a program for creep analysis of concrete structures and automatically converts the input function J(t,t') into a degenerate form. This enables one to deal on input with functions given by only a few parameters, as in double power law. Alternatively, conversion of double power law into a degenerate creep function can also be accomplished by an explicit formula (see Ref. 18 of first reply).