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REPLY TO HILSDORF AND MÜLLER'S DISCUSSION OF "COMMENTS ON THE USE OF ROSS' HYPERBOLA AND RECENT COMPARISONS OF VARIOUS PRACTICAL CREEP PREDICTION MODELS"*

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Hilsdorf and Müller's detailed discussion is deeply appreciated. They raise several interesting points which call for further analysis.

The discussers claim that a plot of $\bar{t}/C$ versus $\bar{t}$ (Eq. 3) is preferable to the plot of $1/C$ versus $1/\bar{t}$ (Eq. 2). This is not true, for three reasons:

1) As one can verify by numerical examples, both plots yield essentially the same values of a and b.

2) It is not true that determination of the "final" creep value from the plot of $1/C$ versus $1/\bar{t}$ gives too little weight to the long time creep data and too much weight to the short-time creep data. The opposite appears to be true. The y-intercept (i.e., point $1/\bar{t} \rightarrow 0$) is very close (in the horizontal direction) to the points for high $\bar{t}$ (Fig. 4) and is, therefore, influenced by an error, e_1, at points for large $\bar{t}$ (small $1/\bar{t}$) much more than by an error, e_2, at points for small $\bar{t}$ (large $1/\bar{t}$) which lie far from the intercept (see Fig. 4).

3) The plot of $\bar{t}/C$ versus $\bar{t}$ (Eq. 3) may be misleading since it gives an impression that the error is less than it actually is (compare Fig. 7a-d with Fig. 7e-h discussed later). The reason is that this plot, unlike the other one, does not become a horizontal line in the special limit case when C does not vary with time (C ≈ const.); rather, it reduces to a plot of $\bar{t}$ versus $\bar{t}$, i.e., a straight line of slope 1. Therefore, when C increases with time, a large part of the variation in the plot of $\bar{t}/C$ versus $\bar{t}$ is of deterministic nature ($\bar{t}$ as a function of $\bar{t}$) and is not due to a variation of C. Thus, the reason that the plot of $\bar{t}/C$ versus $\bar{t}$ appears to give a better fit is that it superimposes upon the random scatter of creep strain as a function of time a deterministic dependence of $\bar{t}$ versus $\bar{t}$, thereby hiding the misfit of the creep formula and creating an illusion of a good agreement (such as that apparent from Fig. 7a-d below).

The plot which matters most for comparing a creep prediction formula with test data is the plot of $J(t,t')$ versus $\log(t-t')$. Such plots were shown in Fig. 3, and from these plots (as well as Fig. 7) it is clear that, regardless of which plot is used for linear regression, the shape of Ross'

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hyperbola does not agree at all with the available long-time creep data and represents just about the worst possible choice among the previously proposed formulas. Although the use of any empirical formula introduces some degree of bias, the power law, $J(t,t') = a + b(t-t')^{1/8}$ would have been clearly preferable to carry out the extrapolations (which could be accomplished by plotting $J(t,t')$ versus $(t-t')^{1/8}$. The statistics that Hilsdorf and Müller [1, 2] obtained would then change substantially.

The discussers try to argue against the power law. They say that it tends "to overestimate creep after long periods of loading". However, from their phrase "functions such as those suggested by Shank or Bažant et al." it seems that they might be unaware of an important difference between the original form of the power law, as suggested by Straub and Shank, and the new form, as suggested by Bažant et al., in the double power law [23]. Long-time creep is considerably overestimated by the original form in which the power function is not applied to the total creep strain $C(t,t')$ (per unit stress), but only to that part of the creep strain $C_1(t,t')$ that accumulates after an initial short-time loading of approximately 1 hour duration (Figs. 5,6) [23]. These two parts are defined by $C(t,t') = J(t,t') - 1/E_0$ (see Refs. 23, 24) $C_1(t,t') = J(t,t') - 1/E$ where E = conventional elastic modulus and E_0 = instantaneous (true) elastic modulus which corresponds to loading applied at infinitely high rate; E_0 is close to the usual dynamic modulus, and is obtained as the left-hand side horizontal asymptote in the plot of $J(t,t')$ versus $\log(t-t')$ (Fig. 6). The so-called short-time strain $1/E$ contains much creep strain, usually over 30% of $1/E$ value (Fig. 5). Exclusion of this creep strain from the original form of power law greatly reduces the range of applicability. The fact that the left-hand side asymptotic value $1/E$ of the power curve $(t-t')^n$ in Fig. 8b is placed too high forces one to give the power curve a large curvature, i.e., use a higher exponent n, in order to fit the short-time creep data. Exponent n here comes to be about 1/3, while the correct exponent obtained with the correct left-hand side asymptotic value $1/E_0$ (Fig. 6) is about 1/8. The excessively large curvature causes the original form of the power law to pass high above the creep data for longer creep durations (Fig. 6). It was for this reason that power law was judged in older works to be inapplicable to long-time creep. Now it is well known, however, that the power law works quite well (and far better than Ross' hyperbola) even for very large creep durations provided that all short-time creep strain is included in the power law [23]. (The power law is not perfect, of course, and improvements appear to be possible - one is the log-double power law, presently under study by J. C. Chern at Northwestern University).

Let us now examine practical use of discussers' Eq. 3 to extrapolate to 50 years some very consistent and careful creep measurements, such as those

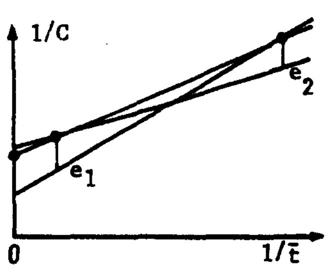

Fig. 4

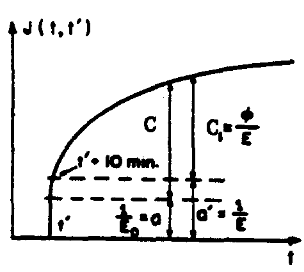

Fig. 5

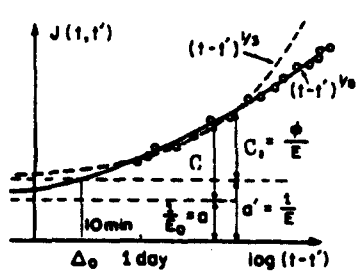

Fig. 6

by Rostasy, et al. [17] (Fig. 7), the duration of which is $\bar{t}$ = 3.7 years. The regression line obtained according to the discussers' method is shown in Fig. 7, and the corresponding Ross' hyperbola in Fig. 7. According to Ross' advice emphasized by the discussers, this hyperbola is made to fit closely the terminal segment of the measured data curve, as seen from Fig. 7g. Suppose now that the measurements terminate either at $\bar{t}$ = 1 month or at $\bar{t}$ = 6 months, instead of 3.7 years. Applying the discussers' method to such limited data and ignoring the data points beyond 1 month or 6 months, respectively, one obtains the regression lines shown in Fig. 7a, b, with the corresponding Ross' hyperbolas shown in Fig. 7e, f (and coefficients a, b of Eq. 3 listed in Figs. 7a-d).

If the discussers' method (Eq. 3) were valid, the Ross' hyperbolas in these three figures would have to yield essentially the same value at $\bar{t}$ = 18260 days = 50 years. They do not, and the discrepancies are huge. Extrapolation of the full 3.7-year data yields a 50 year value that is 2.63 times larger than the value obtained by extrapolation of the 1 month data. The long-time extrapolations drastically change with the duration of measurements, regardless of the manner in which Ross' hyperbola is applied. Therefore, Ross' hyperbola does not appear to be an acceptable approach even when discussers' Eq. 3 is used.

For comparison, Fig. 7 also shows extrapolations with the best formula that the writers presently know (it is called the log-double power law, and represents a gradual transition from the double power law for short and medium times to a logarithmic law for very long times). With this formula, the 50-year extrapolations obtained from the data terminating at 3.7 years, 6 months and 1 month do not differ from each other by more than 9%. With the double power law, the consistency of extrapolations is not much worse. Fig. 7d, h also shows the least square fits of the complete data. The parameters of the log-double power law and the coefficients of variation ω are also listed in Fig. 7e-h.

The discussers offer some justifications for having omitted many of the existing test data from their study. In the writers' opinion such omissions inevitably introduce subjective bias (which seems to have worked in favor of CEB-FIP Model in this case), and are unjustified. If some careful measurements by reputable experimentalists cover, e.g., only a 6 month duration but include, e.g., rather different ages at loading, or different humidity conditions, or different sizes, or different temperatures, or static and pulsating loads, etc., they are relevant and ought to be included. Even if some good data include, e.g., only one-month load duration, and if the creep prediction formula comes, e.g., 100% above this short-time curve, the error ought to be counted in the overall comparison. There exist well documented statistical examples demonstrating how subjective omissions from the data base, i.e., those not made by chance (e.g., by casting a dice), can falsely reduce the coefficient of variation of errors [7].

The discussers further state that in "most experiments" (used by them to calibrate their formulas) "the relative humidity ranged between 50 and 70% at room temperature because this range is of particular practical significance" This premise is not true, however, because it ignores the fact that the humidity effect is very different for different thicknesses D of the cross section, as known from tests as well as theoretical analysis by diffusion theory [23]. A 6 inch (15cm) diameter cylinder in a drying environment loses moisture at about the same rate as a 5 inch (12.5cm) thick slab, but about 4-times faster than a 10 inch (25cm) thick slab, and about 36-times faster than a 30 inch (75cm) thick slab (this fact is not adequately reflected in the CEB-FIP Model Code). Slabs of these thicknesses are quite typical for

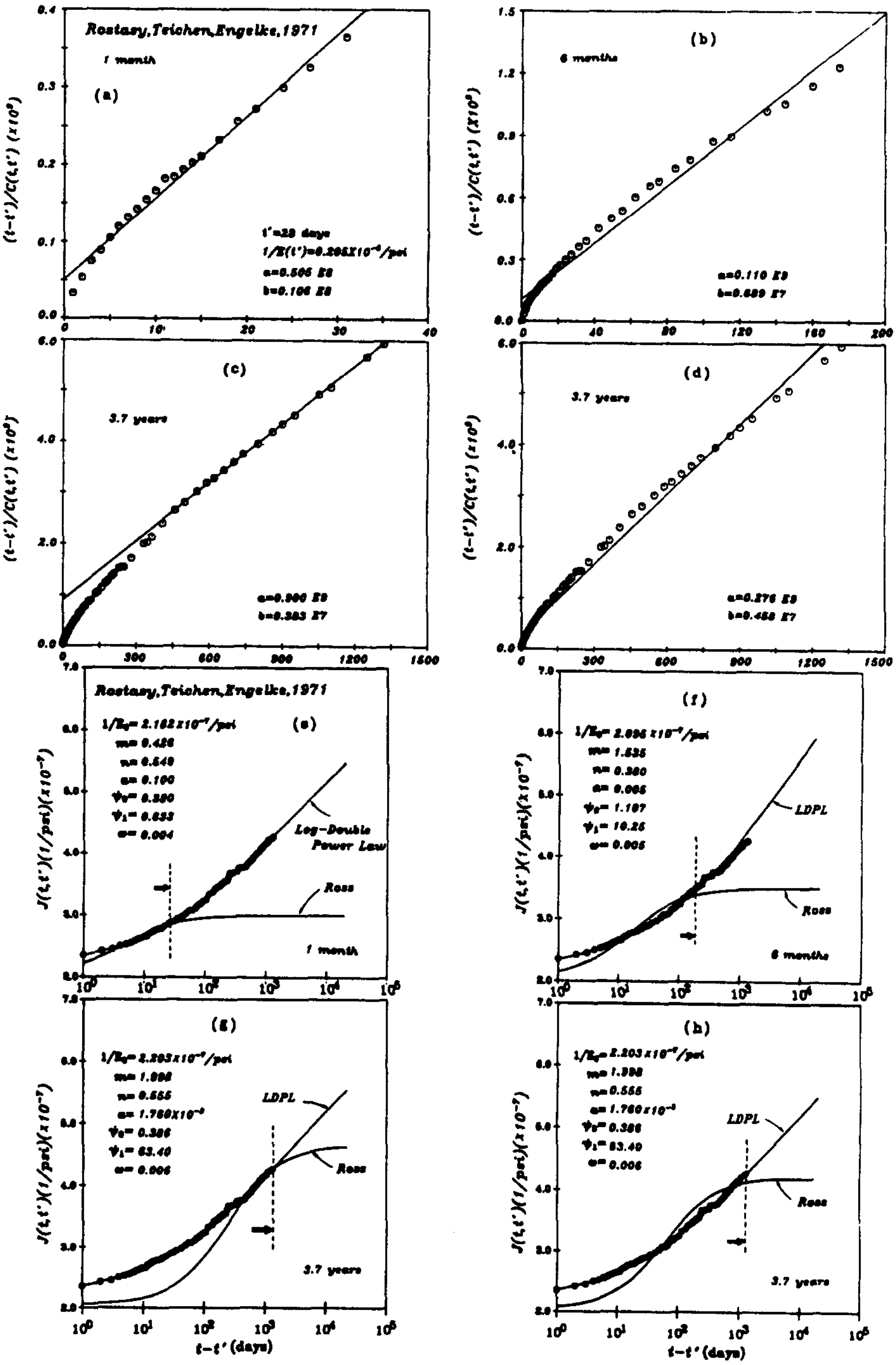
Rostasy,Teichen,Engelke,1971
(a)
1 month
(b)
6 months
(c)
3.7 years
(d)
3.7 years
Rostasy,Teichen,Engelke,1971
(e)
Log-Double Power Law
Ross
1 month
(f)
LDPL
Ross
6 months
(g)
LDPL
Ross
3.7 years
(h)
LDPL
Ross
3.7 years
t-t' (days)
t-t' (days)

Fig. 7

structures to which the CEB-FIP Model Code is intended to apply, e.g., the critical cross sections of large span bridges. From the mean drying rate (rate of loss of water) of a member of any thickness D at a certain environmental humidity h one can easily determine an equivalent environmental humidity h_{eq} which would give about the same creep for a 6 inch cylinder (see, e.g., Fig. 2 in Ref. 25). Thus, one can find that a 10 inch thick slab and a 30 inch thick slab exposed to h = 65% creep, over a long time period, about the same as a 6 inch diameter cylinder exposed to h_{eq} = 77% and h_{eq} = 90%, respectively. For a 90% relative humidity, the creep of standard 6 inch cylinders is much closer to the creep of a sealed specimen than to the creep of a cylinder exposed to a 65% relative humidity, and for 77% the creep is roughly the average of these two cases. Thus, unless good creep data were available for very thick specimens (which is not the case), the discussers should not omit from their comparisons the creep data for high humidities and sealed specimens, even if they intend the CEB formulation to be used only for non-massive structures, such as large span bridges.

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