Bažant, Z.P. (2004). Discussion of "Shear database for reinforced concrete members without shear reinforcement," by K.-H. Reineck, D.A. Kuchma, K.S. Kim and S. Marx, *ACI Structural Journal* 101 (Feb.), 139-140 (with companion discussion by Q. Yu, pp. 141–143).

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Shear Database for Reinforced Concrete Members without Shear Reinforcement. Paper by Karl-Heinz Reineck, Daniel A. Kuchma, Kang Su Kim, and Sina Marx

Discussion by Zdeněk P. Bažant

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By assembling the largest database so far, the authors have made a useful contribution. The data selection, however, is not unbiased and the procedure proposed for the use of the database in model evaluation is not correct. This would lead to deceptive conclusions on the size effect, which is the focus of this discussion. The points that follow briefly explain why.

1. *Statistics of model errors*—In an apparent effort to bypass the least-square regression, the authors use some peculiar statistics. The problem of evaluating or minimizing the errors of the model, which represents a statistical regression problem necessitating the use of the method of least-squares, is treated by means of population (or ensemble) statistics. This is done in Eq. (15) by defining the so-called model-safety factor $\gamma = Y_i/y_i$ $(i = 1, 2, \ldots n)$. For the sake of brevity, the authors' notation is here simplified as $\gamma_{mod} = \gamma_i$, $V_{u,test} = Y_i$, and $V_{u,cal} = y_i$, and subscript i is attached to label the individual data points in the database; Y_i = beam shear strength for test number i, and y_i = the corresponding value calculated from the model (design code formula). In Eq. (16) to (18), Fig. 7 to 9, and Table 5 and 6, the authors introduce and evaluate the standard deviation s and coefficient of variation v of the set (or population) of all γ_i values, which are defined as follows

$$s = \left\{\frac{1}{n-1}\left[\sum_{i=1}^{n}\left(\frac{Y_i}{y_i}\right)^2 - \frac{1}{n}\left(\sum_{i=1}^{n}\frac{Y_i}{y_i}\right)^2\right]\right\}^{1/2}, \quad v = \frac{s}{m} \qquad (21)$$

where $m = (1/n)\Sigma_{i=1}^{n}(Y_i/y_i)$ = mean of all γ_i. Consider now *that the model formula* giving $y_i = V_{u,cal}$ as a function of beam size, concrete strength, and steel ratio is multiplied by any constant factor c, that is, all y_i are replaced by cy_i. Then the standard deviation and the coefficient of variation of the set of γ_i values change as follows

$$s = \left\{\frac{1}{n-1}\left[\sum_{i=1}^{n}\left(\frac{Y_i}{cy_i}\right)^2 - \frac{1}{n}\left(\sum_{i=1}^{n}\frac{Y_i}{cy_i}\right)^2\right]\right\}^{1/2} \qquad (22)$$

$$= \frac{1}{c}\left\{\frac{1}{n-1}\left[\sum_{i=1}^{n}\left(\frac{Y_i}{y_i}\right)^2 - \frac{1}{n}\left(\sum_{i=1}^{n}\frac{Y_i}{y_i}\right)^2\right]\right\}^{1/2} \qquad (23)$$

$$v = \left\{\frac{n}{n-1}\left[\frac{n\sum_{i=1}^{n}\left(\frac{Y_i}{cy_i}\right)^2}{\left(\sum_{i=1}^{n}\frac{Y_i}{cy_i}\right)^2} - 1\right]\right\}^{1/2} \qquad (24)$$

$$= \left\{\frac{n}{n-1}\left[\frac{n\sum_{i=1}^{n}\left(\frac{Y_i}{y_i}\right)^2}{\left(\sum_{i=1}^{n}\frac{Y_i}{y_i}\right)^2} - 1\right]\right\}^{1/2} \qquad (25)$$

This shows that, according to the authors' strange approach to statistics, the standard deviation s of model safety factors γ_i can be made as small as desired if the formula of the model is multiplied with a large enough factor c, and that the coefficient of variation γ_i does not change with such multiplication. Such statistics are incorrect and misleading. No model can be fitted to the database by minimizing such an expression for s (or s^2, or v^2). The authors' statement "A statistical evaluation of the distribution of γ_{mod} can be used to evaluate the safety and accuracy of these empirical relationships [that is, those for shear capacity] *in design practice*" *is generally not true;*

2. *Special case of constant model*—The aforementioned problem is not manifested conspicuously in the authors' Fig. 7 to 9 because the model is represented in each interval of the varied parameter by an independent constant value (horizontal line) of the existing formula $V_c = 2\sqrt{f_c'}\, b_w d$ in Eq. (11-3) of ACI 318. *Because the value of the model is considered* as an independent constant within each interval, it is not necessary to conduct least-square regression based on minimizing s^2. Only in this special case, but not in general, can one simply use population statistics, determining the optimum fit by the model as the mean of the ordinates, separately within each interval. But this is impossible for fitting a smooth formula to the statistical trend of data as a function of some variable;

Consequently, in the special case of a model representing a horizontal line (as shown in each interval in the authors' Fig. 7 to 9), the aforementioned problem with an arbitrary multiplier does not arise. In that case, the authors' definitions of s and v are not meaningless. Neither are they meaningless if these definitions are used to evaluate s and v after the optimum fit by the model has already been determined by the standard method of least squares. Nevertheless, even in those cases, these definitions are generally not unbiased because they are not based on the method of least squares—a method shown by Gauss to be the only method ensuring that the optimum fit and standard error agree with the actual mean and standard deviation of the statistical distribution of the fitted variable. Consequently, the estimates of the 5 and 95% probability cutoffs in Eq. (16) and (18) do not generally give the correct values;

3. *The way to deal with relative errors*—The authors' motivation for introducing the ratio γ has apparently been the view that what matter are not the absolute errors ΔV_c but the relative errors $\Delta V_c/V_c$. While this view is justified, it is generally appropriate, even in that case (and especially in the

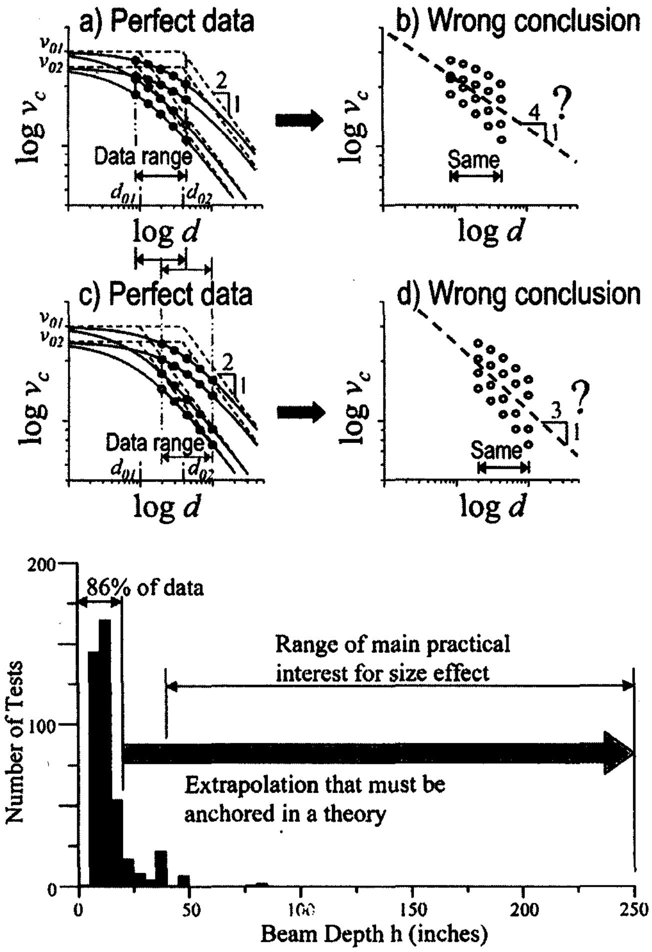

Fig. A—Top: Example of fallacious statistical analysis: (a,c) Hypothetical perfect data generated so as to match exactly the size effect law for four different concretes; and (b,d) incorrect inference made by regression of the combined data set. Bottom: Histogram of the number of tests in ACI 445 database as a function of beam depth d*, demonstrating necessity of extrapolating on the basis of a sound theory.*

presence of a trend, as in the authors' Fig. 5 to 7), to introduce a logarithmic transformation of the fitted variable and consider the statistics of $\ln V_c$ instead of γ (Bažant and Yu 2003); the reason is that $\Delta \ln V_c = \Delta V_c / V_c$ for small ΔV_c. In fact, the necessity of such a transformation is obvious from the fact that the data on the size effect trend in the authors' Fig. 4 are clearly heteroskedastic (that is, their variance strongly depends in the regression coordinate—in this case, d) but, after the logarithmic transformation of the regression ordinate, the data became approximately homoskedastic (for example, Ang and Tang [1976]; Mandel [1984]; Plackett [1984]). Only in such transformed coordinates can the standard (unweighted) least-square regression of the data give unbiased estimates of the mean trend, the standard error, and the 5% probability cutoff (Bažant and Yu 2003);

4. *Fallacy in limiting consideration to combined data*—The authors propose the model to be evaluated only by statistical comparison to the database as a whole. This is fallacious. To illustrate it, consider Fig. A (top), which shows the plots of log v_c versus log d (on the left) for two sets of four hypothetical data series with the same range of beam depth d (in logscale), generated so as to *perfectly* match the curves of the size effect law $v_c = v_0(1 + d/d_0)^{-1/2}$, in which the empirical parameters v_0 and d_0 depend on the type of concrete. The set on top is obtained by a frugal investigator, who has modest funding and must, therefore, test smaller (less expensive) beams, and the set at the bottom is obtained by a wealthy investigator, who has greater funding and can, thus, afford to test larger beams. Each investigator conducts the size effect tests for four different concretes, each of which is the same for both investigators (and all other influencing parameters, such as the steel ratio ρ and shear span a/d are also the same for both). The curve of the size effect law for each concrete is different, characterized by different values v_{01}, v_{02}, d_{01}, and d_{02} of the size effect law parameters v_0 and d_0. Assuming that both of these investigators do not know the size effect law and regard these perfect data as one combined database, they see only the data pictures on the right of Fig. A (top). Because of the high scatter of the combined database on the right, each investigator, looking at his combined database, can at best infer a straight line trend in the bilogarithmic plot, which corresponds to a power-law size effect. By statistical regression, the frugal investigator finds the means size effect $v_c \propto d^{-1/4}$, while the wealthy investigator finds the mean size effect $v_c \propto d^{-1/3}$. Thus, because of ignoring the trend of each individual data series, both investigators are led to erroneous conclusions. Their conclusions depend on subjective factors, such as the choice of beam sizes that, in turn, depend on the funding of their sponsors. By changing the size range of their tests, they could have obtained a power law with any exponent between 0 and –1/2;

5. *Correct use of database*—The foregoing example documents that it is incorrect to base the selection of a design formula on statistical fitting of the entire database. It would even be incorrect to use a combined set of the data with a non-negligible size range (which are much fewer, numbering about 10). Correctly, the selection of the basic form of the size effect formula must be based on a sound theory verified by comparisons to individual size effect test series, geometrically scaled, spanning a broad enough size range, and made with one and the same concrete. The entire database should be used only for calibrating the formula after its basic form has already been selected~~, and~~.

6. *Imperative of theoretical support*—The large beam size of main concern represents an enormous extrapolation of the bulk of existing experimental evidence (refer to Fig. A [~~bottom~~]). Such a huge extrapolation cannot be accomplished purely empirically without any theoretical support. Selecting an empirical model to extrapolate the existing test data without the support of a sound theory would fly in the face of ACI President J. M. Izquierdo-Encarnación, whose inaugural address (Izquierdo-Encarnación 2003) had the motto: "Ars sine scientia nihil est" (that is, "Art without science is nothing").

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Shear Database for Reinforced Concrete Members without Shear Reinforcement. Paper by Karl-Heinz Reineck, Daniel A. Kuchma, Kang Su Kim, and Sina Marx

Discussion by Qiang Yu

Graduate Research Assistant, Northwestern University, Evanston, Ill.

The authors deserve praise for compiling the largest database up to now. The data selection, however, is not free of bias and the proposed empirical statistical evaluation would be misleading. The reasons are as follows.

1. *Ignoring previous database and its analysis*—Documenting the size effect on shear strength by the dimension-free (that is, *dimensionsfrei*, or dimensionless) plots in Fig. 4 and 9 is important but not new. A 1984 study (Bažant and Kim 1984), which escaped the authors' notice, showed the same size effect trend on a database of 296 data points, not much smaller than the database assembled by the authors. A database of 461 points, of which 296 were concerned with beams without stirrups (and the rest with stirrups), was published in detailed tables (with basic test parameters) on pp. 264-267 of Bažant and Sun (1987), which apparently went unnoticed by the authors. Despite the absence of some more recent tests with a broad side range by Collins and coworkers, Iguro et al. (1985), Walraven, and Bažant and Kazemi (1991) the trend of the effect of beam size (as well as concrete strength and reinforcement ratio), evidenced in these two old studies, was the same as shown in the paper.

2. *Ignoring the largest beams tested so far*—The Japanese tests (Iguro et al. 1985; Shioya and Akiyama 1994) (Fig. B), in which the largest beam was 3 m deep (still a record), should not have been omitted from the ACI 445 database. Aside from Bažant and Kazemi (1991) (as well as one recent test by Collins and coworkers in Toronto), these important data provide the best experimental support for the fact that, in the plot of log $(V_c/b_w d)$ versus log d, the mean size effect curve attains a large-size asymptote of slope, –1/2 (this is so despite the fact that the these data were originally interpreted by a power law of exponent, –1/4, inspired by Weibull's statistical size effect theory that, however, has latter been shown inapplicable). The objection against including these Japanese data was that the load was distributed uniformly over the beam while all the other tests in the database were done under three-point loading. The design code formula under study, however, is supposed to apply to both types of loading, and the differences in arch action are much less significant than other discrepancies among the tests included in the database. The fact is the these remain the largest beams ever tested, and that they provide strong support for the size effect theory based on fracture mechanics and dimensional analysis (including the aforementioned slope, –1/2).

3. *Exclusion of test data of the highest brittleness*—Although the omission of a single size-effect test series could not change the overall statistics significantly, the omission of the reduced-scale test series from Bažant and Kazemi (1991) (the second of two test series reported in that study, in which the steel bars did not slip globally) is unjustified for other reasons and appears to be motivated by bias. Thanks to using small maximum aggregate size (5 mm), reduced-scale standard deformed bars (procured from the Portland Cement Association) and a concrete of relatively high strength, this test series has achieved so far the highest brittleness number (Bažant and Planas 1998). This is valuable information because high brittleness can engender, on a small scale, the highly brittle response typical of very large beams made with normal concrete, normal aggregate, and normal reinforcement. The tests in Bažant and Planas (1998) were statistically better designed than any other so far—three identical tests made for each size, five sizes covering the whole range in an unbiased manner (that is, uniformly in the log-scale), and one of three broadest size ranges achieved so far (the ratio of the smallest to the largest beam depth was 1:16). The statistical scatter was the smallest and the overall trend was very clear (it happened to agree very well with the size effect law based on quasibrittle fracture mechanics and dimensional analysis). When inclusion of this data series in the ACI 445 database was recently suggested to a subcommittee chaired by the first author, two objections were raised:

a) One objection was that the beam width b must exceed 50 mm (criterion KON3 in Table 3). But why isn't this condition related to the aggregate size? It excludes reduced-scale model testing, which is an indispensable tool for simulating the size effect of very large normal concrete beams. For an aggregate size of 5 mm, the beam width 38 mm used in Bažant and Kazemi (1991) is perfectly adequate. Besides, the beam width is known to have almost no effect on shear strength $v_c = V_c/b_w d$. Criterion KON3 would exclude all reduced-scale testing, the only means to simulate the highly brittle behavior of extremely large beams experimentally. This is a nihilistic position.

b) The second objection was that the reinforcement bars were not precisely scaled geometrically and that they must have slipped locally along part of the bar length. This, however, was the case to the same or higher degree for virtually all the other data in the ACI 445 database (including the latest Toronto data).

4. *Huge extrapolation necessitating theoretical support*—A further reason why the data from Bažant and Kazemi (1991) should not have been excluded is that the large beam sizes of main concern represent an enormous extrapolation of the bulk of the existing experimental evidence. The size effect is of main practical concern for beam depths ranging from 1 m to many meters; however, 86% of all the tests in the database pertain to beam depths less than 0.5 m, 98% less than 1.1 m, and 100% less than 1.9 m. Such enormous extrapolation must be anchored in a rational theory, and the reduced-scale tests in Bažant and Kazemi (1991) provide the most consistent data available for verifying a theory in the range of high brittleness, which is symptomatic of the large-size beams of the greatest concern. These data happen to give the clearest experimental evidence for the fracture mechanics explanation of size effect. Their exclusion would skew the results away from fracture mechanics.

5. *Importance of fitting individual broad-range test series*—Another important reason why the data from Bažant and Kazemi (1991) should not have been excluded is that the

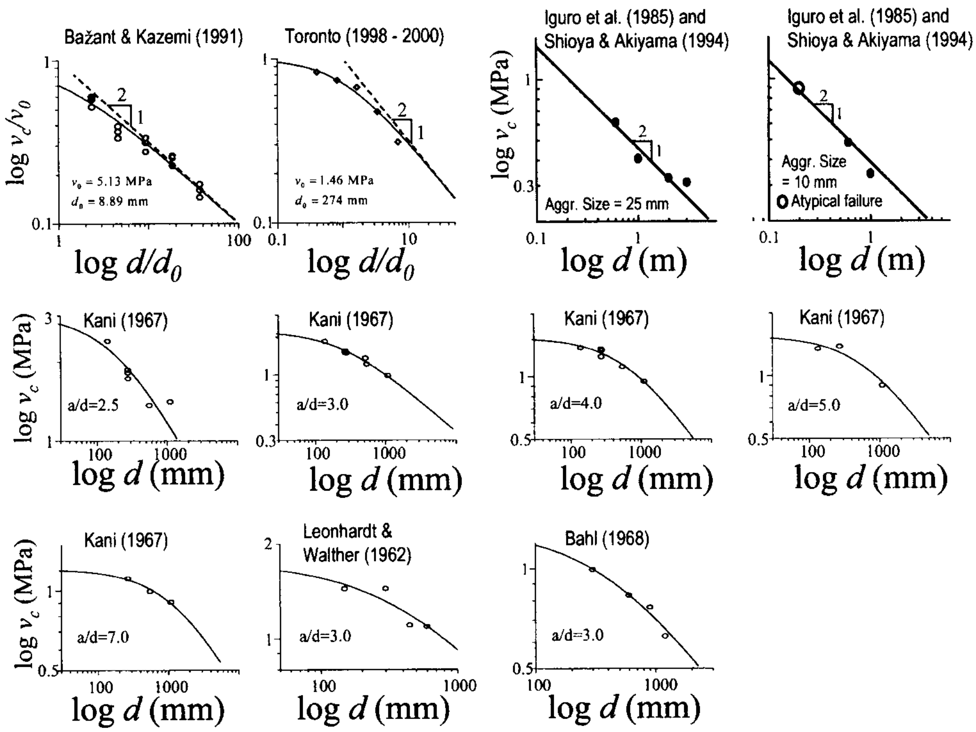

Fig. B—Two diagrams on top left: existing test series of broad size range, 1:16; and rest: all other existing test series of non-negligible size range.

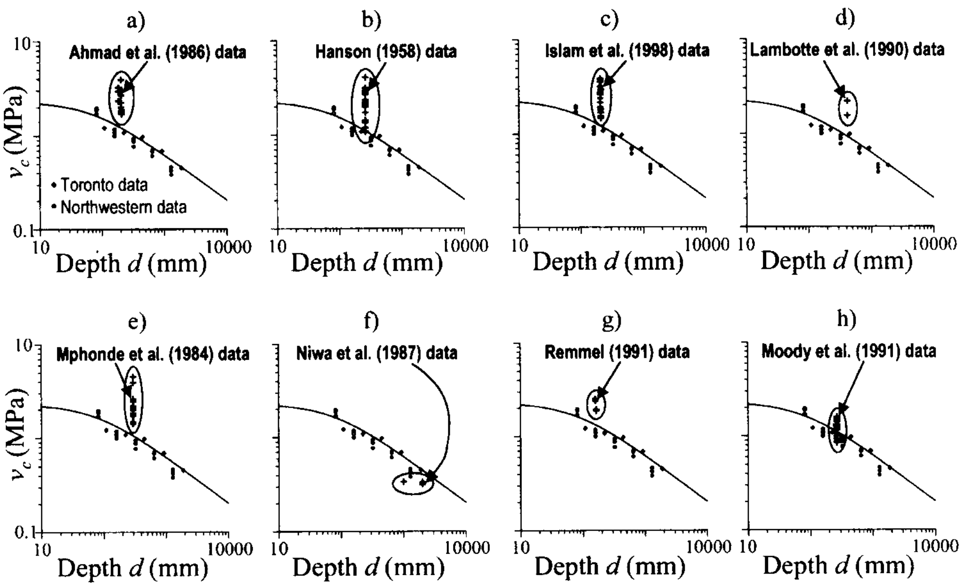

Fig. C—Some of many data contaminating size effect trend of database due to variation of uncertain factors other than size; size effect curve and broad range data from Toronto and Northwestern tests are shown for comparison (Northwestern tests, shown in actual scale in Fig. B, are here shifted, to match).

selection of the best code formula (or model) for the size effect must not be based on the statistics for the entire database, as proposed in the paper. This database is contaminated by highly scattered influences caused by the variation of poorly understood parameters other than size, and this obfuscates the size effect trend (Fig. C). Rather, the selection must be based on the ability of a (theoretically justified) formula to closely fit each of those few available individual data series that had a significant size range (Fig. B), with all other influencing parameters being constant or approximately constant, and here the use of the data from Bažant and Kazemi (1991) would make a significant difference. The entire database is of course very useful, but it should be employed *only* for calibrating the formula of the best form, *after* that form has already been identified on the basis of a theory verified by properly designed individual size effect test series, which must include reduced-scale model tests, to achieve high brittleness numbers (Bažant and Planas 1998).

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AUTHORS' CLOSURE

The authors would like to thank the discussers for their contributions and thus making public their jointly published views, which were previously distributed widely in ACI prior to the authors' response. Therefore, this response jointly covers similar statements of both discussers where applicable.

In their extensive discussions, the discussers focus on the size effect, but they overlook that this was not the aim of the paper. The main aim was to present the shear database for members without shear reinforcement and to demonstrate its relevance by comparing ACI 318 Eq. (11-3) with the selected results derived from the database. Therefore, the following response will concentrate on the points relevant for the database.

1. The authors disagree with Bažant's comments that the authors' model evaluation is "not correct" or "misleading," and that "they use peculiar statistics" or a "strange approach of statistics." Firstly, the ratio of test value to calculated value, that is, the model safety factor $\gamma_{mod} = V_{u,test}/V_{u,cal}$, has traditionally been used for a long time, as demonstrated by many peer-reviewed papers in established technical journals. Secondly, and likewise, it is a theoretically well-founded method in statistics and safety theory of structures. The authors firstly refer, for example, to MacGregor's well-known textbook (1988), Section 2-4, where the variability of resistance is dealt with by "a histogram (Fig. 2-3) for the ratio of beam moment capacities observed in tests M_{test} to the nominal strength M_n computed by the designer." Subsequently, the resistance factor ϕ is then derived using this ratio. The model safety factor can be understood as the reverse of an ϕ-factor, whereby it only covers a part of the uncertainty covered by the strength reduction factor of $\phi = 0.75$ for shear. If $\gamma_{mod} = V_{u,test}/V_{u,cal}$ is less than 1.0, then a design equation does not attain the required safety margin and the value ϕ is less than the required 0.75.

The authors also recommend Schneider's (1997) "Introduction to Safety and Reliability of Structures," where in Section 3.3.2 the model uncertainties are dealt with, and where he would read the following:

"...deviations between analysis and tests are to be expected. This fact is considered by a model variable M that may be determined from tests. The test results r_{exp} are divided by the corresponding results r_{mod} obtained using the resistance model: $m = r_{exp}/r_{mod}$. From a number of tests a histogram for M is obtained. Also the mean value and the standard deviation s_M may be calculated."

The author's approach is consistent with the above.

Many further references can be given that this is a theoretically sound method for deriving code expressions for the resistance of structures or structural members, especially due to the work of several CEB and now *fib* Committees working over many years on this topic: refer to, for example, CEB Bulletins 219 (1993) and 224 (1995), and, specifically, König and Fischer (1995) in this context of assessing design relations for members without shear reinforcement.

From the final statements of Point 1 by Bažant, the authors only can confirm the well-known fact that the coefficient of variation does not change if the calculated values are multiplied by a factor, and this allows a comparison of relations with different average values. The authors reject Bažant's assertion that they falsified results by making "the standard deviation of the model safety factor as small as desired." The standard deviation and the coefficient of variation for Eq. (11-3) of ACI 318 were just calculated according to well-known formula, and there is no mention at all in the paper of applying a factor. Multiplying by a factor of approximately 1.5 (not "a large enough factor") may only be required when different proposals are compared in order to bring all proposals to the same level of, for example, the average value. The statement, "No model can be fitted to the database by minimizing such an expression" is clearly out of place because nothing about "minimizing" and "fitting" can be found in the paper.

Point 2 has also been answered by this because the authors also did not "conduct least square regression" or "minimize" anything or carry out an "optimum fit." Figure 7 to 9 demonstrate by simply carrying out separate statistical evaluations for different ranges that the overall statistical values for the whole dataset do not provide a sufficient insight into the performance of a resistance model, for example, high model safety factors for low d does not make up for unsafe values for high d.

Point 3, again, has nothing to do with the paper because the authors clearly did not deal with relative errors ΔV, but looked at the model safety factor $\gamma_{mod} = V_{u,test}/V_{u,cal}$, as explained previously.

Point 4 discusses at length a pretended proposal by the authors (see first sentence), but this statement is obviously not true, so that no detailed discussion is required. The authors did not "only" look to the database as a whole but also evaluated the performance of Eq. (11-3) in the selected ranges. It needs no logarithmic plots to see that high safety factors in the range of low d do not cover the deficiencies for high d.

The authors also do not believe in statistical comparisons only, but they wished to improve the basis for the empirical methods, used until now in all codes for deriving code expressions for especially the shear capacity of members without shear reinforcement. Likewise, however, this improved database can be used for comparisons with theories and models for which experimental evidence is vital. The authors are satisfied that at least this improvement of the database for members without transverse reinforcement is acknowledged by both discussers as "useful contribution" or even "deserving praise."

The necessity for theoretical support of a design formula (expressed in Points 5 and 6 by Bažant and Point 4 by Yu) is surely undisputed in principle, but practically it only holds if generally agreed models are available for the design of members under the combined action of bending moments and axial forces. Yet even this agreement on the flexural design was only reached approximately 50 years ago, which means that the big success of structural concrete in the first 50 years of its use was based more on empirical methods, perhaps combined with some theoretical considerations. Should the practitioners have waited these 50 years the researchers obviously needed and should they have stopped using concrete, only because of an "imperative of theoretical support"? Both discussers should reconsider their absolute demand on the primacy of theories and humbly accept that the enormous success of concrete was in the first instance not solely based on theories but perhaps rather more on engineering judgement.

Especially in the case of models and theories for the ultimate shear capacity of members without transverse reinforcement, such an agreed state on the theoretical treatment is still not reached today, as can be taken from the state-of-the-art reports by Joint ACI-ASCE Committee 445 (1998) as well as CEB TG 2.7 in CEB Bulletin 237 (1998). This was also confirmed by the recent comparisons of design proposals for ACI 318 carried out by ACI Subcommittee 445-F, which yielded large discrepancies between different theoretically-based proposals. Among these were the seven proposals of both discussers themselves, which showed an enormous scatter and encompassed almost the whole range of all other proposals although only based on the one theory favored by the discussers. In view of such variation within methods based on only one theory, the absolute claim that design proposals "must be based on a theory" cannot be justified, and this is even more true in view of the fact that there are other theories with contradictory relations and predictions.

Point 1 by Yu is not true because the tests reported in the cited reference were considered in the database. The purpose of the paper was not to write a historic review on earlier published data collections.

In Point 2, Yu overlooks that all available design proposals are based on tests of simple beams under one or two point loads. The tests on beams under distributed loading were then used to confirm the location of the section for which the design is carried out. There are too few tests under distributed loading to justify a statistical and empirical approach. In addition, the evaluation of such tests requires the determination of the location of failure to realistically and safely assess the loads carried by the shear transfer actions across the failure crack. There are only a few proposals for this location, which contradict each other; the usually-in-codes assumed distance d from the support is not a realistic value, although a safe assumption.

The authors strongly reject Yu's accusation in his first sentence of Point 3 that the authors were motivated by bias. The reasons why any test should not be considered in the evaluations are clearly explained in the paper, and for the beams by Bažant and Kazemi, it was solely the Point a) of 3 that the width of some tests was smaller than 50 mm (2 in.). The selected lower limits for the dimensions b and ha of test beams are practical limits for beams built according to ACI 318 or surely any code, as was agreed upon without much discussion by the three committees in *fib*, ACI, and DIN, in which the first author was a member of.

In Point 4, Yu addresses the extrapolation of an empirically derived design proposal beyond the available data. This is certainly a problem with all empirically derived design formulae in codes and, therefore, code makers point out the limits of the design equation and the range of data they are based on in the code explanations or in accompanying handbooks. If a designer faces a case beyond this range, he or she will reconsider his design; if, for example, he or she wanted to design a 5 m-thick slab without shear reinforcement, which is highly unlikely to ever be tested, he will at least consider to place minimum shear reinforcement.

Under Point 5, Yu firstly characterizes the authors' database as "contaminated by highly scattered influences"; the authors wonder how Yu characterizes beams and slabs concreted in structures outside the laboratories, for which codes are meant and which surely exhibit even a far higher scatter of parameters than the database? Secondly, the discusser seems to be preoccupied by the dominance of the size effect, but the influence, for example, of the reinforcement ratio is likewise important. This is a fairly well understood parameter because the longitudinal reinforcement controls the crack widths and especially that of the failure crack in members without shear reinforcement.

The proposal by Yu (and also Bažant, refer to Point 5) to base a design formula on individual test series is simply dangerous as can be taken from the scatter in the diagrams, where different laboratories reported completely different test results for beams with the same dimensions and materials. The idea that only individual test series should support theories can only be understood as such: that the discusser believes strongly in only one or his own theory and ignores contradictory theories and models, as already explained previously.

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