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Reply

Response to A. Carpinteri, B. Chiaia, P. Cornetti and S. Puzzi's Comments on "Is the cause of size effect on structural strength fractal or energetic-statistical?"

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Abstract

Carpinteri et al.'s discussion is very welcome for it gives an opportunity to clarify long-running disagreements on the problem of size effect, important to several engineering fields. However, the discussion misinterprets many points of Ba ant and Yavari's paper and attempts to raise new issues. This response presents recent experimental results contradicting applicability of Carpinteri's "multifractal scaling law" (MFSL), and refutes the discussers' arguments on their proposed concepts of "fractal mechanics", on the statistical size effect, on the validity of mathematical derivation of MFSL and its asymptotic slope, and on various other aspects of scaling of quasibrittle failure.

1. Introduction

A factual discussion of the differences between the energetic or energetic-statistical theory and the fractal theories has been overdue for a long time. Therefore, we sincerely welcome the discussion of our paper by the group of researchers from the Politecnico di Torino. However, since the discussion misinterprets many points from our paper and also attempts to raise many new issues, a detailed response is called for.

2. Response to discussers' "introduction" and to their comparisons with energetic and energetic-statistical theories of size effect

Carpinteri, Chiaia, Cornetti and Puzzi (henceforth called the discussers) begin by pointing out that the hypothesis of fractal origin of the experimentally observed size effect on structural strength, and the

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"multifractal scaling law" (MFSL) in particular, have been promulgated by the senior discusser (Carpinteri) in "more than 60 peer-reviewed papers published in the most prestigious journals" and "in the most important conferences throughout the world". This is true. We leave it up to the reader to decide whether it invalidates our critique.

2.1. Experiments showing inapplicability of MSFL to reinforced concrete structures

The discussers write that "the experimental tests, as evidenced from the first publications in 1992, have often supported the soundness of the fractal approach and of the MFSL". This statement would be accurate only if one considered only older experiments on not too large unreinforced (plain) concrete structures, and only those failing at macro-crack initiation (which requires the structure geometry to be positive [1]). The senior discusser has long claimed that the MFSL applied to all quasibrittle failures of concrete structures except notched test specimens. This claim is not true, and we are compelled to make it clear by giving a synopsis of the main experimental evidence, as available today.

The discussers ignore the difference between the size effects of two types: type 1, which occurs in unnotched structures (of positive geometry) [1,2] at fracture initiation from a boundary layer of cracking in structures, and type 2, which occurs in unnotched or notched structures that reach maximum load only after large stable crack growth. Their fractal geometric arguments do not distinguish between these two cases, and so they assume that the MFSL applies to both. Yet a very different size effect law is required for each [1–3].

The discussers accept that the size effect in notched structures is energetic and different from MFSL. In reality, though, a notch acts similarly to a crack, and is almost perfectly equivalent to a pre-existing (initial) crack when the crack-bridging cohesive stresses in this crack have been reduced to zero by prior load cycling and fatigue, as typical in practice. So, if they accept one case to be energetic, why don't they accept the other? Both cases lead to size effect laws with exactly the same asymptotes [2,3], and even if the crack-bridging stresses have not been reduced to zero by prior load cycles and fatigue, there is only a small and negligible difference in the sharpness of the transition between the asymptotes (see [4,2,3] for the difference between the type 2 and type 3 size effect laws). Another reason why both cases are essentially equivalent is that, if the crack propagation (in a non-homogeneously stressed structure) is stable, the crack path is almost completely dictated by the laws of mechanics (maximization of entropy production or energy release rate), and does not vary perceptibly because of material randomness, nor fractality of fracturing or damage. So, it is known in advance where approximately the crack will go, which is almost the same situation as for notches.

These facts become clearer by considering the widely studied case of diagonal shear fracture of longitudinally reinforced concrete beams without stirrups. As confirmed by many experiments as well as finite element simulations, the diagonal cracks have essentially similar paths in small and large beams and about the same relative length at maximum load, extending through about 80% of the cross section depth (see the bold curves ending by a circle in Fig. 1). There is no chance that the crack would propagate with a much steeper or much milder average slope for one size than for another, start at very dissimilar positions, or have a very different relative length at the moment of failure (faint curves ending by a cross in Fig. 1). Thus the fracture process zones at maximum loads of small and large geometrically similar beams are found to lie at the same homologous locations (in Fig. 1), as if a precut notch had a tip at that location. The senior discusser has since 1994 insisted repeatedly, in numerous papers, that the MFSL is applicable to all unnotched structures, including the aforementioned case of beam shear. Initially, the experimental evidence and its statistical analysis was not unambiguous in this regard. Broader-range test data for normal concretes, of a size range sufficiently broader than the width of the scatter band, were at that time unavailable, or geometrical scaling of test specimens was not adhered to. This situation, and the excessive scatter of old test data, were the reasons why, in the senior discusser's widely circulated 1995 report (the discussers' Ref. [8]), the MFSL seemed to fit various old data equally well as, and in some cases better than, the energetic size effect law (SEL) of type 2 (Eq. (5) in Ref. [5]).

For the shear failure of reinforced concrete beams (with non-reduced aggregate sizes), the situation in 1995 is documented in Fig. 2, which shows all the classical test data on beam shear reported between 1962 and 1994 [6–10]. Note that the closeness of fits of these data by the SEL (energetic size effect law of type 2) and by the MFSL is not very different. Although, in our opinion, the fits by SEL in Fig. 2 are somewhat better, the only clear conclusion that can be drawn exclusively from these data or those compiled in the senior discusser's 1995

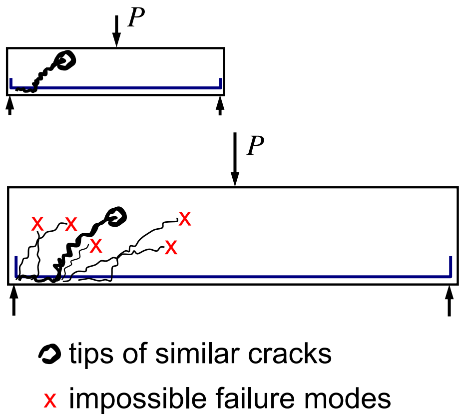

Fig. 1. Crack paths and lengths at maximum load in shear failure of small and large geometrically similar longitudinally reinforced concrete beams. Bold curves ending by a circle show the real cracks. Light curves ending by × are impossible cracks.

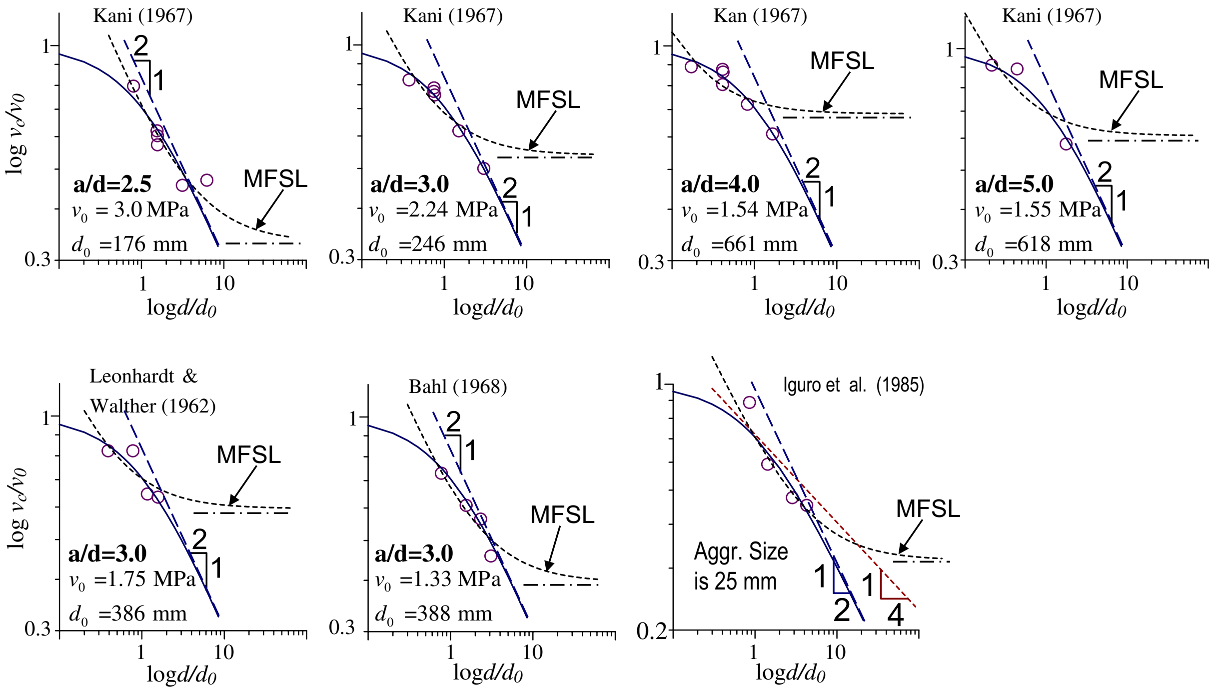

Fig. 2. Comprehensive set of classical test data on the size effect on the strength of reinforced concrete beams failing by shear, and their optimum fits by the type 2 energetic size effect law (SEL) and by the MFSL.

report is that a size effect exists, but not which formula is correct. So, by 1995, one had to rely either on fracture mechanics arguments, or on nonlocal finite element simulation. Although experimental evidence was by 1995 available from reduced-scale model tests of geometrically scaled concrete beams [11] (Fig. 3, left), most practicing engineers (as well as the senior discusser) were unwilling to accept it because the maximum aggregate size ($d_a = 4.8$ mm) was deemed to be too small.

Now, 12 years later, two test series carried out at the University of Toronto (summarized in [12,13]) make the experimental evidence clearer; see Fig. 3 (middle and right). These large-size tests were approximately geometrically scaled and had a broader size range than those in Fig. 2. They were conducted on beams with normal aggregate size, and reached the beam depth of $d = 1.89$ m.

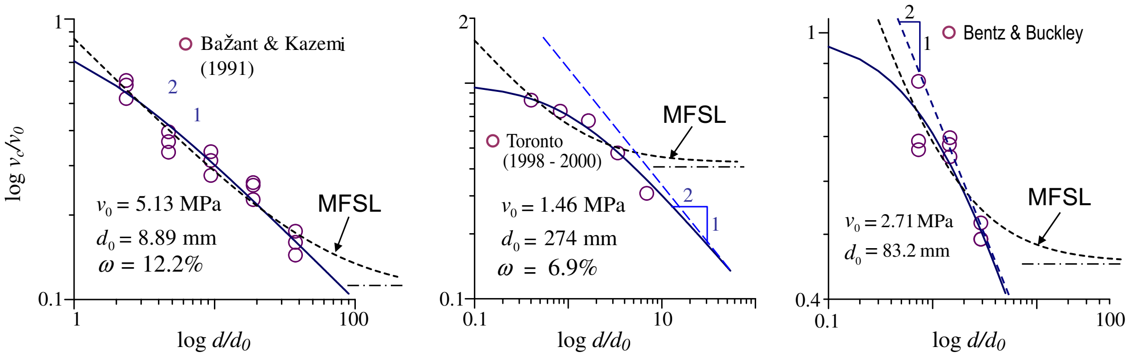

Fig. 3. Data from scaled tests with sufficient range of the size effect on nominal strength v_c in shear failure of longitudinally reinforced concrete beams with stirrups (coordinate: logarithm of dimensionless beam depth; v_0 is the ACI normalization factor of v_c); and fits by the size effect law for failure after large stable crack growth. Left: Northwestern University model tests on concrete with reduced aggregate size; middle and right: two different recent series of University of Toronto tests ranging up to the beam depth of 1.79 m (for details see 12 and 13).

Fig. 3 shows the optimum fits of the two Toronto test series by the SEL (solid curves) and by the MFSL (dashed curves). Also shown is the optimum fit of the older reduced-scale model tests [11]. Now note that the SEL fits the data points well, while the MFSL does not. In particular, note that the final asymptote of slope $-1/2$, corresponding to the asymptotic power law $d^{-1/2}$, is supported by the data. The final slope of $-1/2$ further implies that the discussers' intuitive argument about the loss of disorder at a large enough scale is unrealistic (it may be noted that ACI Committee 446, chaired by W. Gerstle, voted unanimously in 1993 that any size effect formula to be considered for revising code provisions for beam shear must terminate with the large-size asymptotic slope of $-1/2$).

From the ACI database [14] (shown in Fig. 1 of our paper [5] and in [12]), a clear statistical trend can be extracted using refined statistical analysis. This database collects 398 beam test results obtained in hundreds of laboratories throughout the world (and is an extension of the 1984 Northwestern University database with 296 test results reported in [15]). A simple statistical regression of all the points in this database, attempted previously by several authors, cannot give a meaningful trend of the size effect on the nominal (or average) beam shear strength v_c because other important influencing parameters, such as the steel ratio ρ_w, the relative shear span a/d and the maximum aggregate size d_a, vary in that database arbitrarily and highly non-uniformly with respect to beam size d.

Such primitive statistical techniques showed that power laws of various exponents, the MFSL, and the energetic size effect law, all fit the database almost equally well (or equally badly) [16]. Therefore, special statistical techniques are needed to extract any meaningful and unbiased information by purely statistical means. This objective necessitates suppressing the bias implied by lack of statistical design of parameter sampling. A suitable statistical technique has recently been introduced in [17] (and summarized in [18]). It does provide a clear size effect trend; see Fig. 4 (top, and bottom left). Here the range of beam depths d in the database is subdivided into five equal-ratio size intervals (Fig. 4). They range from 3 to 6 in., from 6 to 12 in., from 12 to 24 in., from 24 to 48 in., and from 48 to 96 in. (1 in. = 25.4 mm). The borders between the size intervals are chosen to form a geometric (rather than arithmetic) progression because what matters for size effect is the ratio of sizes, not their difference (note that, e.g., from $d = 4$ to $4 + 20$ in., the size effect is strong, from 400 to $400 + 20$ in. negligible). The averages and the distributions of the values of ρ_w, a/d and d_a in these intervals of the ACI database are very different. Because, as generally agreed, the effect of the required concrete strength f'_c is adequately captured by assuming the shear strength of cross section, v_c, to be proportional to $\sqrt{f'_c}$, the ratio $y = v_c/\sqrt{f'_c}$, where v_c and f'_c are both given in psi (1 psi = 6895 Pa) may be considered to depend only on ρ_w, a/d and d_a.

To filter out the effect of influencing parameters other than d, one must, within each interval of d, gradually (step by step) restrict the relevant values or influencing parameters ρ_w, a/d and d_a by adjusting the upper and lower limits until the averages of these relevant values within each interval of d would become, for each interval of d, about the same (within a given tolerance). To ensure statistically unbiased treatment, no data point

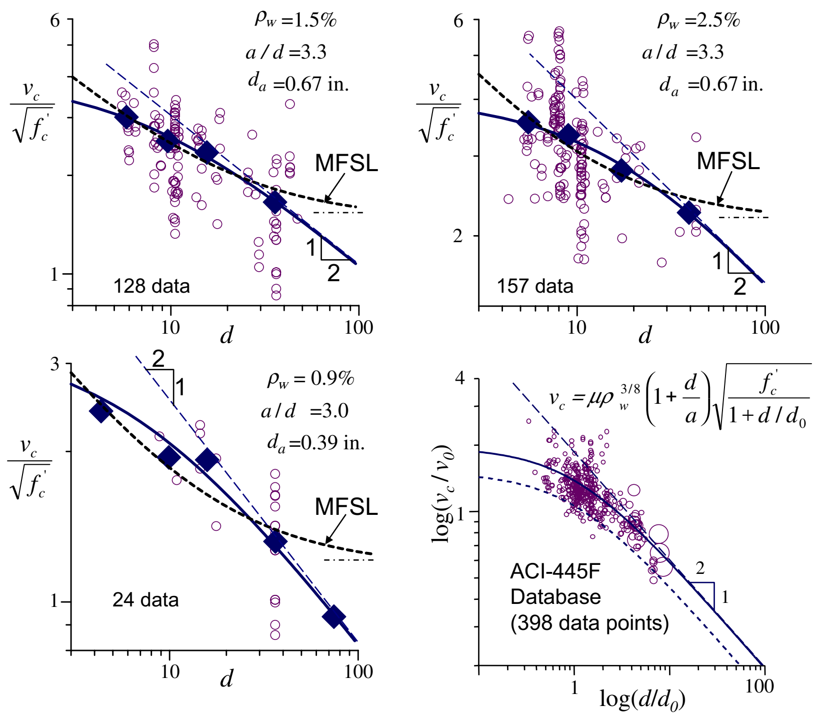

Fig. 4. Top left: data extracted from the ACI beam shear database obtained by a statistically unbiased algorithm that restricts the ranges of the steel ratio, shear span and maximum aggregate size so that their mean values be about the same in each interval shown; and fit of the interval centroids of these data (diamond points) by the size effect law for failure after large stable crack growth. Top right and bottom left: the same but for higher and lower steel ratios. Bottom right: optimum fit (in transformed coordinates) of the entire ACI database on the shear strength of longitudinally reinforced three-point loaded concrete beams by a formula based on size effect law of type 2 [12] (the dashed curves represent the optimum fit by the MFSL, and the circle size is proportional to weight in regression).

within the upper and lower limits of each interval of d for each influencing parameter may be left out, and no data point outside those limits may be included. A computer optimization algorithm has been written for extracting such relevant data from the ACI database. The algorithm has been run to extract three subsets of the ACI database, corresponding to three different average values, $\rho_w \approx 1.5\%$, 2.5% and 0.9%.

The centroids of the extracted relevant y-data points within each of the aforementioned 5 intervals of d are shown as the bold diamond points in Fig. 4 (top left and right, and bottom left) in the plot of $\log(v_c/\sqrt{f_c'})$ versus $\log d$. For $\rho_w \approx 1.5\%$ and 2.5%, the last of the five intervals of d (which is the case of very large beams) had to be left empty because the ACI database does not contain within that interval sufficient data points giving these average values of ρ_w. The subsets of extracted relevant data points are shown as the faint empty circles. For $\rho_w \approx 1.5\%$, the algorithm delivered a subset of 128 relevant data points for which the precise values of the averages within each interval of d were $\rho_w = 1.51\%$, 1.50%, 1.50%, 1.50%, with the corresponding averages $a/d = 3.45$, 3.33, 3.33, 3.23, and $d_a = 0.66$, 0.67, 0.66, 0.65 in. For $\rho_w \approx 2.5\%$, the algorithm delivered a subset of 157 relevant data points for which the precise values of the averages within each interval of d were $\rho_w = 2.55\%$, 2.51%, 2.48%, 2.44%, with the corresponding averages $a/d = 3.33$, 3.33, 3.34, 3.33, and $d_a = 0.67$, 0.67, 0.66, 0.67 in. For $\rho_w \approx 0.9\%$, the algorithm delivered a subset of only 24 relevant data points for which the precise values of the averages within each interval of d were $\rho_w = 0.91\%$, 0.94%, 0.94%, 0.91%, 0.74%, with the corresponding averages $a/d = 2.94$, 2.94, 2.94, 2.94, 2.86, and $d_a = 0.39$, 0.39, 0.39, 0.39, 0.39 in.

Under the assumption that the statistical weight of each size interval centroid in Fig. 4 is the same, the foregoing procedure [17] is now used to obtain the optimum least-square fit of these 4 or 5 centroids with the SEL (type 2 energetic size effect law, Eq. (5) in [5]), which is written here as $v_c/\sqrt{f_c'} = C(1 + d/d_0)^{-1/2}$ where C, d_0 = free constants to be found by the fitting algorithm (note that Eq. 5 of our paper [5] has a misprint: the exponent should be $-1/2$, not 1/2).

The fits of the diamond points (solid curves in Fig. 4 top left and right, and bottom left) are seen to be quite satisfactory; they have very small coefficients of variation of errors, $\omega = 2.5\%$, 1.7% and 5.1%, respectively (standard deviation of errors divided by data mean). The trends of the diamond points are seen to be quite systematic. Note all the three trends show a negative curvature, agreeing with the SEL and contradicting the MFSL. The terminal trends of the centroids agree with the asymptote of slope $-1/2$, characterizing the SEL [15,16,12,19], and give no hint of an approach to the asymptote of slope 0, characterizing the MFSL. Finally note that the trends of the centroids disagree also with the Weibull statistical theory, which would require a straight line of slope cca $-1/12$.

Another way to obtain from the database a meaningful result is to conduct a multivariate nonlinear regression with a rational size effect formula in which the strong influences of ρ_w, a/d and d_a on the parameters are taken realistically into account. These influences have been incorporated into the coefficients of SEL (see the formula in Fig. 4 bottom right). This led to the optimum fit of the entire ACI database of 398 points shown in Fig. 4 (right) [12], plotted in transformed coordinates of the formula. Despite scatter, the trend of the energetic size effect is clearly confirmed. The MFSL cannot be plotted against the entire ACI database because the influences of ρ_w, d/a and d_a on its coefficient are not known and are impossible to determine by the discussers' 'fractal mechanics'. Nevertheless, the positive curvature of the MFSL disagrees with the trend in Fig. 4 (right).

Since the MFSL cannot apply to reinforced concrete beams failing after large crack growth, it was misleading, in numerous papers of the senior discusser, to present the MFSL and the SEL as two competing models. The former applies only to type 1 size effect (at small sizes), and the latter only to type 2 size effect. Models that apply to different situations cannot be in competition.

The discussers are silent about two other inconsistencies. When applying the MFSL to type 2 failures, they ignore the rate of energy release caused by a large crack growing stably before the maximum load. Yet the fact that this energy release causes a size effect is undeniable and easily understood (see the discussion about Fig. 3 (right) in [5], and point 1 on page 564 of [20]). Even if the fractal source of size effect for type 2 failures were accepted, the size effect of the energy release would have to be superposed on it because it is inevitable. Likewise, in applying the MFSL to type 1 failures, the discussers ignore both the size effect due to stress redistribution engendered by a finite fracture process zone (or boundary layer of cracking), and the Weibull statistical size effect, which is undeniable in the case of failures at crack initiation in a material of random strength.

The discussers are further silent about the fact that the dependence of MFSL coefficients on structural geometry is not predicted by their fractal mechanics. By contrast, the energetic size effect law (of type 1 as well as 2) for failures at crack initiation does predict this dependence, through the limit values of the energy release rate derivatives (Eq. (6) in [5]).

Referring to the RILEM report [20], the discussers are not right in criticizing that "a non-zero term is included in the formula" of the SEL. As a matter of fact, the original SEL [19] is cited in [20] as Ref. [53] and is presented in Eq. (11). It is true that "a non-zero term" σ_R (the need for which was pointed out in 1987 [21]) is added, but this is done only for the sake of brevity, and a few lines below it is stated that the residual strength σ_R is usually zero, with two exceptions – unbroken fibers crossing the crack, or transition at large sizes to a residual frictional plastic mechanism as in compression kink bands or the Brazilian test [22]. We can see nothing that could be criticized.

2.2. Discussers' comments on statistical size effect and Weibull theory

The discussers write that Z.P. Bažant (ZPB) "exploited the same tactics he followed to demonstrate that Weibull-type size effect is not applicable to concrete structures". This is a distortion of our position, which must be explained. We certainly do not deny that the Weibull-type statistical size effect on the mean structural strength exists in concrete structures, though not without important limitations. It exists only in those structures that fail (under load control) at the moment of macro-crack initiation and are sufficiently large compared to the aggregate size. Very large unreinforced concrete structures (such as large arch dams or retaining walls failing by flexure) qualify, but reinforced concrete structures failing after large crack growth generally do not. Neither do small plain concrete beams. What needs to be understood is that (aside from other reasons [23,2]) there are four strong reasons for these limitations:

(1) Large cracks grow stably prior to the maximum load, traversing typically 80% of the cross section in the case of beam shear. Since, as already pointed out, the crack path is dictated by mechanics, the fracture process zone at maximum load lies at one precise location, and so a random local strength at other locations in the beam does not matter. In such structures, strength randomness affects only the shape of the probability density function (pdf) of structural strength but not the mean, and causes no appreciable size effect on the mean strength [23]. Here the size effect, seen in experiments, is energetic (non-statistical), caused by the energy release due to stress redistribution engendered by large crack growth.

(2) The contribution to Weibull probability integral is proportional to $r = (\sigma(\boldsymbol{x})/\sigma_{\max})^m$ where $\sigma(\boldsymbol{x})$ = first principal stress at point of coordinate $\boldsymbol{x}$, $\sigma_{\max}$ = maximum of principal stresses within the structure, and m = Weibull modulus ≈ 24 for concrete [24]. Because of stress concentration near the tip of a large crack, the stresses decay rapidly and at points where $\sigma(\boldsymbol{x}) \leqslant 0.9\sigma_{\max}$, one has $r \leqslant 0.08$, and where $\sigma(\boldsymbol{x}) \leqslant 0.5\sigma_{\max}$, one has $r \leqslant 6 \times 10^{-8}$. So, virtually the only non-negligible contribution to the Weibull probability integral for structural strength comes from the fracture process zone, whose size is essentially independent of the structure size, and thus can cause no size effect [23]. Besides, the Weibull probability integral could diverge if the singular elastic stress field of a crack were substituted. Hence, there is no appreciable statistical size effect if a large crack forms before the maximum load.

(3) The third reason, which precludes the use of classical Weibull statistics to both unreinforced and reinforced concrete structures of normal sizes (as well as to most structural parts made of laminates or coarse-grained ceramics), is that the equivalent number [25,26] of representative volume elements (RVE) contained in a normal concrete structure is not large enough (because of insufficient ratio of cross section size to aggregate size, as well as stress non-uniformity).

(4) To obtain acceptable (in fact, barely acceptable) data fits, the classical Weibull theory is often considered to have a non-zero threshold. But recently it was shown [25,26] that the threshold in Weibull theory must always be zero, or else the Maxwell–Boltzmann distribution of interatomic bond strength would be contradicted.

While the discussers object to certain ‘tactics’ against the Weibull statistical theory, they themselves actually never use that theory. They argue that some sort of strength randomness is in some way communicated through fractals. But the way it is supposed to be communicated is to us mathematically incomprehensible.

Since the discussers emphasize the statistical aspect of the material, it is strange that they accept a horizontal large-size asymptote of size effect, as exhibited by MFSL. According to the Weibull statistical theory (and also the chain-of-RVEs model [25,26]), the slope of that asymptote would have to be $-n_d/m$, which is about $-1/12$ for two-dimensional scaling of failure of unreinforced concrete structure (m = Weibull modulus ≈ 24 for concrete, and n_{d} = number of dimensions of fracture scaling).

2.3. Why do the discussers compare MFSL to the universal SEL?

In [5], the universal size effect law (U-SEL) was mentioned only to illustrate a unified description of size effect clarifying the transition between the energetic size effects of type 1 (failures at crack initiation) and type 2 (failures after large crack growth). It has not been mentioned in regard to MFSL because it is irrelevant to the comparison of MFSL with the energetic size effect. Yet the discussers write: “ The reaction of Bažant was a fierce opposition … Therefore he introduced the so-called Universal SEL”. The word “therefore” is misplaced (because the U-SEL was shown on the screen of an opening lecture at that same conference at which the discussers’ Ref. [8] was distributed).

More importantly, none of us has ever mentioned the U-SEL in relation to the MFSL, nor to the fractal theories of the senior discusser. The repeated mentions of this law by the discussers in regard to MFSL only cloud the issue and evade the real problem.

In all the discussers’ statements, the words “Universal SEL” should be replaced by the words “the energetic size effect law for failures at crack initiation”, or simply the words “type 1 size effect law”. Only then the discussers’ commentary would make sense (although it would still be unjustified). The MFSL should be compared only to the type 1 size effect law because it is usable (as an empirical formula for a limited size range) only for small enough structures failing at crack initiation, and not when small or large macro-cracks grow stably before the maximum load.

2.4. Discussers' other points

The discussers appeal to the well-known renormalization group theory as a foundation of their 'multifractal' theory. That is unwarranted. The renormalization group theory does not sanction the 'multifractal theory'. That theory merely describes the transition from one power law scaling to another power law scaling, which is a characteristic not only of the MFSL but also of the energetic and energetic-statistical size effect laws (thus, a foundation in the renormalization group theory could just as well be invoked for the latter). What is crucial is the gradual transition between these two power-law scalings, which spreads over several orders of magnitude depending on strain localization instabilities. On that, the renormalization group theory says nothing.

For the sake of accuracy, various marginal comments of the discussers need to be corrected. E.g., the discussers write "Although some scepticism ... was outlined ... by Bažant, Gettu, Jirásek, Planas and Xi, the other members of the Committee did not take a position and, in other papers, expressed their independent point of view ... in favour of the MFSL" (actually, not 'members', but only one member, van Mier, who expressed that point of view, in a rather non-specific way).

3. Response to "slope of the MFSL asymptote"

3.1. The concept of "fractal mechanics"

The discussers' concept of the so-called "fractal mechanics" is interesting but not well defined. As explained in [5], it involves some simplistic extensions of linear elasticity. The arguments presented in [27], and also those offered by the discussers, conflict with some universal principles of mechanics. Note the following points, to wit.

In an axially loaded bar, the strain is a normalized displacement, i.e., the ratio of the total relative displacement to the original length of the bar. But this can have nothing to do with the cross section of the bar, as implied in [27,28]. Thus the one-dimensional definition of strain proposed in [28, Eq. (3)] is not rigorously justified. In the prototypal one-dimensional problem of an axially loaded bar, one can define a "fractal normal stress", similar to what was defined for a cohesive theory in [29], but such a definition is limited to forces in a fixed direction. If it is desired to introduce a normal stress definition as the density of force on a fractal cross section, what should be modified is the stress–strain relation, and not the strain. In that sense, the constitutive equations become scale dependent, but not the strain.

The recent works of the senior discusser's team go even further and, without any sensible justification, define the strain as the fractional derivative of the displacement field. What they present as a "formal derivation" [28, see the paragraph after Eq. (18)] looks to us as a collection of various undefined quantities with undefined connections.

In this regard, note that linear elasticity can be rigorously derived from nonlinear elasticity by linearization about a given deformation mapping [30]. The linearized strain represents the linearization of deformation gradient and thus happens to depend on the first derivatives of the displacement field. The deformation gradient (which is the derivative map of the deformation mapping) maps an infinitesimal line element in the reference configuration to its deformed form in the current configuration.

Now it is unclear what a fractional derivative would mean in this fundamental context. Defining a quantity as the fractional derivative of the displacement field is meaningless. The possibility of some new measures of strain is not excluded, but any such measure would have to be based on sound geometric arguments.

Another surprising aspect in the so-called "fractal mechanics" [27] is the differential equation of equilibrium, which is expressed in terms of some fractional divergence of an undefined stress tensor. In classical continuum mechanics, the local balance of linear momentum is obtained by localization of the global balance of linear momentum away from discontinuities. This yields the Cauchy theorem and the local balance of linear momentum (or equilibrium equations), i.e. $\mathrm{div}\boldsymbol{\sigma} + \rho\mathbf{b} = \rho\mathbf{a}$ (where $\boldsymbol{\sigma}$ = stress tensor, $\mathbf{b}$ = body force, $\mathbf{a}$ =

acceleration, $\rho =$ mass density). However, the discussers' definition of a "fractal stress tensor" is unclear, and so is their replacement of the divergence operator with a fractional operator. It is also unclear how the stress could be defined on a lacunar fractal if the energy is supposed to dissipate on an invasive fractal.

3.2. Problems with MFSL asymptotic slope −1/2

Trying to defend the small-size asymptotic slope of MFSL, the discussers say nothing about one simple but strong objection to its value $-1/2$. Since the fractal dimension δ of the cracking morphology is not constant but varies from one material to another, how can the exponent $n = -1/2$ of the small-size power-law asymptote of MFSL be treated as independent δ?

Consider that δ varies and approaches the Euclidean dimension δ_{Eu}. In the discussers' view and in the derivation of MFSL ([31] or discussers' Ref. [2]), exponent n remains equal to $-1/2$ even when, for example, $\delta = \delta_{\mathrm{Eu}} + 10^{-9}$ (where $\delta_{\mathrm{Eu}} =$ Euclidean dimension). Then n is supposed to jump discontinuously to 0 as $\delta = \delta_{\mathrm{Eu}}$.

In a credible theory, n would have to approach 0 continuously, i.e., $\lim_{\delta\to\delta_{\mathrm{Eu}}} n = 0$ should hold. The discontinuity of n as a function of δ is physically unacceptable. Thus the arguments in [31] that led to asserting that $d_\sigma = 1/2$ are hazy and, to us, mathematically incomprehensible.

In regard to their Eq. (1), the discussers claim that "at the smaller scales … continuum mechanics holds", and "damage is diffused and one obtains $d_\epsilon = 0$". This cannot be true. At the smaller scale, the discreteness of the microstructure of the material, for instance concrete, cannot be ignored, and thus the continuum mechanics approximation of concrete as such cannot hold.

Furthermore, the discussers' claim that "the maximum value for $d_{\mathscr{G}}$ is 1/2" looks to us as nothing but a questionable conjecture. In fact, the variation of the critical energy release rate $\mathscr{G}$ in the sense of an R-curve is not a physical fact but merely an artifice allowing approximations by linear elastic fracture mechanics. According to the cohesive crack model, which is a physically more realistic approach, the critical $\mathscr{G}$, i.e., the fracture energy, is a constant, yet the R-curve (representing the variation of the critical J-integral near the crack tip) can be predicted by this model (and so can the variation of this integral when the crack front is close enough to the boundary to interact).

The discussers' response to our statement in [5] that the "the defects of *maximum* size cannot have the same probability distribution of a as the ensemble of all defects (as considered in Eqs. (22)–(32) of [31]) but could have only one of the three possible extreme value distributions (Fréchet, Gumbel or Weibull)" sidetracks the issue that we raised. The Fréchet distribution would, of course, be acceptable (provided that the defect size distribution had a Pareto tail), but that is not what we criticized.

What we criticized as incorrect is that the derivation of the MFSL in [31] considered, for the maxima, the *same distribution* as for the whole ensemble of defects, and particularly not the Fréchet distribution (note that a consistent statistical theory of crack propagation using the Fréchet extreme value distribution was presented in Section 12.6 of [1]).

Stochastic simulations with the nonlocal Weibull statistical theory or with the probabilistic nonlocal damage mechanics [24,32, e.g.] demonstrate that the small-size asymptotic power law of size effect of type 1 can have an exponent different from $-1/2$. Since no analytical solutions of boundary value problems with the fractal theory have been presented by the discussers, it would be welcome to see at least some numerical solution of the fractal field equations of some boundary value problem, and its comparisons with experiments. We expect that such solutions would show that the small-size asymptotic exponent of size effect is not restricted to $-1/2$, even under the hypotheses of their 'fractal theory'.

A fundamental model for the statistical aspect of the size effect at crack initiation (type 1) in a heterogeneous material is the chain-of-RVEs model [25,26,33]. This is a weakest-link model that, in contrast to Weibull theory, has a finite, rather than infinite, number of links, each of which is imagined to correspond to one RVE. Based on the Maxwell–Boltzmann statistics of atomic energies, each RVE of a heterogeneous brittle material must have a Gaussian strength distribution onto which a power-law tail with a zero threshold is necessarily grafted at the probability of the order of 0.001. For large sizes, this model asymptotically reduces to the classical Weibull theory, while for small sizes it yields the correct (experimentally and computationally confirmed) deviation from the Weibull power-law size effect (whose exponent is $-n_d/m$). This is the same as predicted by the energetic analysis of size effect (the reason is that the RVE is considered to have a finite size and the equivalent number of RVEs is finite). The predicted small-size asymptotic slope of size effect (plotted in a logarithmic scale) is, in general, different from $-1/2$ [25,26].

The MFSL, with its initial asymptotic slope fixed as $-1/2$, and the chain-of-RVEs model (whose mean coincides with the energetic-statistical size effect law and also with the mean of nonlocal Weibull theory), are mutually exclusive. They cannot both be correct. Eventually, the science and engineering community will have to choose.

The discussers try to justify the small-size asymptotic slope of $-1/2$ by claiming that "the maximum defect size is proportional, on the average, to the structural scale". It is not clear whether the discussers consider the defects to be the initial microscopic defects, i.e., microcracks (smaller than about the maximum inhomogeneity sizes), or the macrocracks produced by load. There are two points to consider in this regard.

First consider the initial microcracks, or flaws. The distribution of their sizes is supposed to be an objective material property, not alterable by human will once the concrete is cast. So how can it depend on the structural scale, e.g., the depth of a reinforced concrete beam, which is a subjective property chosen by the engineer at will?

On the other hand, if the maximum 'defect' is considered to be the macro-crack produced by loading, the size of which at maximum load does depend on the beam depth, then there is a different problem. Wouldn't macrocrack formation cause stress redistribution with energy release? Wouldn't that energy release inevitably cause an energetic, rather than fractal, size effect? Wouldn't that size effect have to be taken into account? Isn't it true that the energy release increases with structure size roughly quadratically, while the energy dissipated by the crack increases approximately linearly? [2, e.g.].

Alas, the argumentation relative to the maximum defect size is not mathematically comprehensible to us.

3.3. Criticisms of MFSL derivation whose refutation was not attempted

Aside from the discussers' points rebutted later in Section 4, the interested reader should note that the authors made no attempt to refute the following problematic and mathematically invalid steps in the 'derivation' of MFSL, as identified in [5]: seven of the nine points mentioned in item 1 on page 26 of [5], all the three points in item 2, and all of item 4.

So it must be concluded that the MFSL does not logically follow from the hypothesis of fracture or damage fractality. It is merely an empirical formula which is good enough only for the type 1 size effect (at crack initiation), and only for sizes not so large that Weibull statistical size effect would intervene. In that range, the MFSL is equivalent to a special case of the energetic and energetic-statistical size effects. For very small sizes, though, the MFSL has a questionable asymptote, and for very large sizes it fails to capture the inclined asymptote of the power-law size effect of Weibull statistical theory, which must occur for failures at crack initiation.

Although MFSL has the same asymptotes as the earlier CEB-FIP formula [34] $\left(A+\sqrt{B/D}\right)$, it certainly cannot be claimed that it provides a theoretical foundation for that formula, which was introduced purely empirically.

4. Responses to points made in "further considerations"

(1) We find this point to be mere playing with words. Careful reading of [5] makes it clear what we meant by 'fractals' and by 'energetic-statistical'.
(2) Mathematically, a multi-fractal is not what the discussers call 'multi-fractal', as explained in [5].
(3) The requirement for "six orders of magnitude" of scales [35] (see also item 8 on page 564 of [20]) is necessary to ensure that the scaling property be *unambiguously* fractal, i.e, that it cannot be described equally well by some non-fractal theory (e.g., autocorrelated statistical roughness). The fractal scaling property must be verified over a sufficiently broad range of scales, since not everything with apparently fractal scaling in a narrow range of scales is properly modelled as a fractal. Of course, the two limits of this interval depend on the microstructure scale and on the macroscopic characteristic length of the problem.

(4) Our paper [5] certainly did not exclude the possibility of modelling certain aspects of fracture mechanics by fractals. What [5] warns about is the danger of introducing some ill-defined fractal-motivated concepts into mechanics. Furthermore, the discussers pointed out the similarity of Eq. 15 in an earlier Carpinteri's paper to Eq. (10) in [29], which describes the asymptotic stress distribution around a fractal crack (they actually write ''Eq. (20)'' but the number ''20'' was most likely a misprint). This comment, however, is surprising since Eq. (10) was not presented in [29] as original. In all the papers by Yavari and co-workers on this subject [29,36–40], proper reference has been made to those who studied this problem for the first time, i.e. Mosolov [41], and Gol'dshtein and Mosolov [42,43]. However, none of these original sources has been cited in the long series of papers by the senior discusser.
In this context, we wish to emphasize that, aside from Mosolov and Gol'dstein's pioneering contributions, we see, despite our critiques, valuable contributions in the early and recent works on fractal aspects of fracture by many other researchers (see, e.g. [44–53] and other works cited in [5]).

(5) There is no such confusion in [5]. Careful reading reveals that crack fractality and the so-called ligament fractality were properly distinguished. Besides, the related sentence ''after more than ten years, Bažant still seems to confuse lacunar fractals with invasive fractals'' (in the penultimate paragraph of the discussers' Section 2) is baseless.

(6) In principle, one can work with an entity having a zero volume and a finite fractal measure, and associate with it a fractal mass density. Our argument in [5] does not exclude this possibility. The point is the following. Would it be meaningful to work with a body of zero volume within the setting of classical continuum mechanics? The answer is no, if one wants to use the established continuum concepts. For example, if a body has the fractal dimension of 2.55 (locally, in the neighborhood of a point) it makes no sense to think of the ''stress'' and ''strain'', or even mass density, as a field in the classical sense, since these quantities cannot be defined everywhere in the ambient space.

(7) This comment is surprising because, in subsection 4.4.3 of [5], we give no definition of stress. We explicitly say that ''One may be *tempted* to define a fractal traction...'' but we do not use the equation following this statement, and we explain in detail why one must be very careful in defining the stress on fractal surfaces, and why the concepts of the so-called ''fractal mechanics'' are unjustified and should be avoided. Besides, [29] discussed similar problems and again made no use of any ''fractal stress tensor''. In that work, it was explained that, in one-dimensional problems, one can define fractal densities of force although their naive extension to three-dimensional problems is impossible.

(8) Any definition of stress and strain should be based on sound mathematical arguments, but those used by the senior discusser are not of such a kind. For a given system, one should start with a proper description of kinematics and define appropriate strain measures. This is not done in the so-called ''fractal mechanics'' in [27]. The existence of Cauchy stress is a consequence of balance of linear momentum and explicitly depends on smoothness of the surfaces on which traction acts. In any well-defined ''fractal mechanics'', one needs to address this issue and define a proper measure of stress. Again, this is not done in [27].

(9) Eq. (12) in [27] is a constitutive equation. This equation, whatever one may want to call it, uses a meaningless quantity, and this was the point made in [5]. Fractal derivatives have been applied in physics – for example, in describing anomalous diffusion, in characterizing viscoelasticity, and also in describing dissipation in the framework of Lagrangian mechanics. We certainly did not imply in [5] that fractional derivatives could not be useful. Our point simply is that one cannot simply take a fractional derivative of a displacement field and call it a ''fractal strain''.

(10) This statement of ours is attacked outside its context. Exponent β, of course, does not characterize fractality, but if β would not reduce to 1 then standard continuum mechanics could not be the limit case of Carpinteri et al.'s ''fractal mechanics'', which is a fundamental requirement.

(11) The point is not whether exponent N is used in [31] as some sort of a measure of disorder, but whether it is realistic to use it for that purpose. The author provides no justification, and the reason for its introduction in the MFSL derivation in [31] is indeed unclear.

In conclusion, it must be reasserted that ''the 'MFSL' has been based on a series of hypotheses but does not follow from these hypotheses by a valid mathematical procedure''.

4.1. Closing comments

The tone of the discussion and the personal emotive comments it contains suggest that our attempt at a factual criticism has offended the feelings of the discussers. If that is so, we are truly sorry. It was definitely not our intention to cause that. What is needed is an unemotional scientific debate of the salient differences between competing viewpoints.

Although the discussion and our response are focussed on concrete structures, further debate of size effects will be important for all the fields of engineering affected by quasibrittle (or cohesive) fracture. This for example includes the design of large load-bearing composite parts for aircraft and ship, predictions of the load capacity of sea ice and the forces it exerts on obstacles, estimates of the danger of landslides, snow avalanches, and rock burst in mines, safety of nuclear containments and waste storage, and reliability of micro-electronic components and nano-devices.

Introducing the size effect into design codes for concrete structures, which affect how many thousands of concrete structures are built, is a necessity. One reason why progress has been delayed for 15 years is the unresolved scientific conflict between the energetic-statistical and fractal theories of size effect. Reaching a consensus on the theory is essential since it is costly to test a very large structure to destruction and outright prohibitive to conduct a statistically significant number of such tests. Thus it is not surprising that, in the case of shear of reinforced concrete beams [12,16,18,54], 86% of all available test data pertain to beam depths less than 0.5 m, and 99% to depths less than 1.1 m, while, by contrast, the world-record box girder of the Babeldaob-Koror Bridge in Palau, whose fatal collapse in 1996 was marked by a large inclined shear–compression fracture emanating from the support, was 14.2 m deep. In extrapolations of the laboratory test data to such a structure size, the SEL and the MFSL curves in Figs. 2–4 differ by factors between 2.4 and 3.7, the MFSL being on the unconservative side. Clearly, the present debate has grave engineering consequences [55].

That a serious problem exists is also clear from past experience with catastrophic collapses of large concrete structures involving fracture. According to the statistics reported in [56,57], the frequency of collapses of very large structures has been more than 1 in a thousand (per lifetime), while for normal size concrete structures it has been about one in a million, which is what is generally required [58,56] to ensure that concrete structures (or aircrafts, ships, nuclear plants, etc.) would not add significantly to other hazards that people inevitably face. One in a thousand is intolerable, and a way to move forward in the ongoing polemic must be found.

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