DISCUSSIONS AND CLOSURES

Discussion of “Shear Compression Failure in Reinforced Concrete Deep Beams” by Prodomos D. Zararis

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Sundaramoorthy Rajasekaran1 and K. Nalinaa2
1Professor of Infrastructure Engineering, PSG College of Technology, Coimbatore, 641004 Tamilnadu, India. Email: sekaran@hotmail.com
2Senior Lecturer in Civil Engineering, PSG College of Technology, Coimbatore, 641004 Tamilnadu, India. Email: knalinaa@hotmail.com

The author is to be commended for developing a theory to find the compression depth as well as the shear strength of deep beams with or without web reinforcement. He has also compared the values obtained by the formula derived with the values available in the literature. However, the discussers would like to make a few points.

In today’s highly integrated world, when solutions to problems are cross-disciplinary in nature, soft computing promises to become a powerful means of obtaining solutions to problems quickly yet accurately and acceptably. Soft computing refers to computational tools whose distinguishing characteristic is that they provide approximate solutions to approximately formulated problems. Fuzzy logic, neural networks, probabilistic reasoning, expert systems, and genetic algorithms are some of the constituents of soft computing. In this context, a multilayer feed-forward artificial neural network can be applied to predict the ultimate shear strength of reinforced concrete deep beams. Artificial neural network (ANN) is an interconnected network of processing elements which have the ability to be trained to map a given input into the desired output. If the training data use the results of beam experiments, the neural network (NN) gives the shear strength without making any assumption about the behavior of the beams.

Sanad and Saka (2001) applied back propagation neural network (BPN) considering ten input neurons and one output neuron for the prediction of the shear strength of deep beams. One hundred and one of the experimental data were used to train the network and ten data were used for testing the network.

One of the main issues in BPN is the determination of the structure of the network in the number of hidden layers and the number of hidden neurons in each hidden layer. In most reported applications, these are determined based on experience. If the network has more hidden neurons than necessary, the resulting network is overparameterized and thus not parsimonious. To counter this, the sequential learning approach with a single hidden neuron has been applied to various practical problems. This method allows for simultaneous building up and training of neurons. The classical Gram–Schmidt (Bathe 1996) orthogonalization technique is used at each training step to build a set of orthogonal bases. Already the senior discusser has used the sequential learning neural network (SLNN) and shown that it is more efficient than BPN since the SLNN architecture uses only one hidden neuron instead of many hidden neurons in BPN. An equation was also derived to find the shear capacity of deep beams and it compares very well with experimental results.

The discussers have investigated three types of problems given by the author in his paper: (1) deep beams without shear reinforcement; (2) deep beams with web reinforcement; and (3) deep beams with both horizontal and vertical web reinforcement. Hence three types of SLNN architectures are used to predict the shear strength.

Deep Beams without Shear Reinforcement

Out of 52 data given by the author in Table 1 of the paper, odd-numbered data are selected for training and even-numbered data are taken for testing. SLNN consists of 5 input neurons for \( f_p', b, d, a/d, p \) with one bias neuron, one hidden neuron, and one output neuron \( V_u \). It uses sigmoidal learning law for hidden layer and linear learning law for both input and output layers. For more details of SLNN architecture, the reader may refer to the paper by Zhang and Morris (1998) and Rajasekaran and Amalraj (2002a,b). The architecture is shown in Fig. 1 of this discussion. It is to be noted that all the inputs and outputs are normalized such that they lie between 0 and 1 as shown below

\[
\xi = \frac{(X_i - X_{i,min})}{(X_{i,max} - X_{i,min})}
\]

where \( X_i, X_{i,max}, X_{i,min} \) are given in Table 1. The network is trained for 50,000 epochs with learning rate of 0.6 and gamma value of 0.000001. After training, once the weights connecting input to hidden neuron and hidden to output neuron are stabilized (see Fig. 1) the network is ready for inferring. Fig. 2 shows the comparison of values obtained from SLNN with the values obtained from experiments and good comparison is obtained. Table 2 and Fig. 3 show the correlation between values obtained by SLNN, experiments, and Eq. (10) of the original paper. In Figs. 2 and 3 the normalized values of ultimate shear (kN)/750 (kN) are plotted for all the data. The comparison of SLNN with experiments is quite good with a correlation coefficient of 0.9725, whereas the correlation of experiments with Eq. (10) is 0.97206. It is observed that SLNN can predict the ultimate shear strength of deep beams without web reinforcement quite accurately.

Deep Beams with Shear Reinforcement (Stirrups)

Out of 65 data available in Table 2 of the original paper odd-numbered data (33 in total) are used for training and even-numbered data (32 in total) are used for testing. SLNN consists of 7 input neurons \( f_p', b, d, a/d, p, p_v, f_p \) with one bias neuron, one hidden neuron, and one output neuron \( V_u \). The network is trained for 50,000 epochs as before with the same learning rate and gamma value. Fig. 4 shows the error versus log (iteration) and one can see that monotonic convergence is obtained. Fig. 5 shows the comparison of values obtained by SLNN with those obtained from experiments and Fig. 6 shows the correlation of
values obtained from SLNN, experiments, and Eq. (10) of the original paper. In Figs. 5 and 6 the normalized values of ultimate shear (kN)/750 kN are plotted for all the data. The correlation coefficients are given in Table 2. Even in this case, good correlation is obtained between SLNN and experimental values. The correlation coefficient of values of experiments with SLNN is 0.948753, whereas the coefficient with Eq. (10) in the paper is 0.96356. Even in this case, it is observed that SLNN can predict the ultimate shear strength of deep beams with web reinforcement quite accurately.

Deep Beams with Vertical and Horizontal Web Reinforcement

Out of 21 data available in Table 3 of the paper, 7 data (3,6,9,12,15,18,21) are used for testing and the remaining data are used for training. In this case, SLNN uses 8 input neurons ($f'_c, b, d, a/d, \rho, \rho_v, \rho_h, f_{yv}$) with one bias neuron, one hidden neuron and one output neuron ($V_u$). The network is trained as before and Fig. 7 shows the comparison of values obtained by SLNN with experimental values. Fig. 8 shows the correlation of values obtained by SLNN, experiments, and Eq. (10) of the original paper. In Figs. 7 and 8 the normalized values of ultimate shear (kN)/750(kN) are plotted for all the data. The correlation coefficients are given in Table 2. It is seen that correlation coefficient of experimental values with SLNN values is 0.9910, and with Eq. (10) of the paper the SLNN is 0.9813; hence it can be concluded that SLNN can predict the ultimate shear capacity of deep beams with vertical and horizontal web reinforcement.

Using the weights of synapses obtained from Fig. 1, the ultimate shear capacity of deep beams can be given in terms of other values in equation form as:

For deep beams without web reinforcement

$$\xi_9 = \frac{29.238}{1 + e^{-I}}$$

where $I$ is given as

$$I = 2.07589\xi_1 + 2.35\xi_2 + 2.13405\xi_3 - 2.98302\xi_4 + 0.86719\xi_5 - 6.5519$$

(2)

For deep beams with vertical stirrups

$$\xi_9 = \frac{19.1427}{1 + e^{-J}}$$

where $J$ is given as

$$J = 1.4485\xi_1 + 1.7002\xi_2 + 0.96717\xi_3 - 0.70348\xi_4 + 0.4\xi_5 + 0.3569\xi_6 - 1.342586\xi_8 - 5.37008$$

(3)

Table 1. Inputs and Outputs (Max and Min Values Considered)

<table>
<thead>
<tr>
<th>Variable</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>Output</th>
</tr>
</thead>
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<tr>
<td>$X_i$</td>
<td>$f'_c$ (MPa)</td>
<td>$b$ (cm)</td>
<td>$d$ (cm)</td>
<td>$a/d$ (shear span/depth)</td>
<td>$\rho$</td>
<td>$\rho_v$</td>
<td>$\rho_h$</td>
<td>$f_{yv}$ (MPa)</td>
<td>$V_u$ (kN)</td>
</tr>
<tr>
<td>$X_i$, max</td>
<td>100</td>
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<td>120</td>
<td>3</td>
<td>5</td>
<td>2</td>
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</tr>
<tr>
<td>$X_i$, min</td>
<td>10</td>
<td>5</td>
<td>10</td>
<td>1</td>
<td>0.5</td>
<td>0</td>
<td>0</td>
<td>250</td>
<td>0</td>
</tr>
</tbody>
</table>
Table 2. Correlation Coefficients of Sequential Learning Neural Network (SLNN) with Eq. (10) and Experimental Values

<table>
<thead>
<tr>
<th>Model</th>
<th>SLNN</th>
<th>Eq. (10) of Writers</th>
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</thead>
<tbody>
<tr>
<td>Eq. 10 of</td>
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<td></td>
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<tr>
<td>Paper</td>
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<td></td>
</tr>
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<td>Experimental</td>
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<tr>
<td>2</td>
<td>0.9487</td>
<td>0.96356</td>
</tr>
<tr>
<td>3</td>
<td>0.9910</td>
<td>0.9883</td>
</tr>
</tbody>
</table>

Fig. 3. Correlation of values obtained from sequential learning neural network (SLNN), experiment, and Eq. (10) of the paper (model 1)

Fig. 4. Error versus log (iteration, model 2)

Fig. 5. Comparison of sequential learning neural network (SLNN) with experimental values (model 2)

Fig. 6. Correlation of values obtained from sequential learning neural network (SLNN), experiments, and Eq. (10) of the paper (model 2)

Fig. 7. Comparison of sequential learning neural network (SLNN) with experimental values (model 3)

Fig. 8. Correlation of values obtained from sequential learning neural network (SLNN), experiments, and Eq. (10) of the paper (model 3)
For deep beams with both vertical and horizontal reinforcement

\[ \xi_9 = \frac{1.906107}{1 + e^{-K}} \]

where \( K \) is given as

\[ K = 4.8906\xi_1 + 0.3535\xi_2 + 0.63596\xi_3 - 0.12765\xi_4 - 1.0755\xi_5 
+ 1.2263\xi_6 + 0.0825\xi_7 - 0.8538\xi_8 - 2.5369 \]  

In Eqs. (2)–(4), \( \xi_i \) = normalized value of \( X_i \) and is calculated as per Eq. (1) if \( X_i \) is known. \( X_i \) denotes: (1) cylinder compressive strength, (MPa); (2) breadth of the beam (cm); (3) effective depth of the beam (cm); (4) shear span (cm)/depth (cm); (5) % reinforcement of total horizontal steel; (6) % reinforcement of transverse steel; (7) % reinforcement of horizontal steel; (8) yield strength of vertical steel (MPa); and (9) shear capacity (kN) depending on the values of \( i \) from 1 to 9.

For example, if the cylinder compressive strength is 24 MPa (see Table 1 for \( X_{i,\text{max}} \) and \( X_{i,\text{min}} \) as 100 MPa and 10 MPa), \( \xi_1 \) is calculated as

\[ \xi_1 = \frac{24 - 10}{100 - 10} = 0.155 \]  

Eqs. (2)–(4) are applicable in the range of values shown in Table 1 (e.g., \( V_I = 0–750 \text{kN} \)). The coefficients in Eq. (2) are the weights of synapses connecting the input to hidden and hidden to output neurons as shown in Fig. 1.

Even though SLNN involves 50,000 epochs, the computer time in Pentium III, 933 MHz with 256 MB RAM is only 5 minutes, and the performance is quite good. It has been found that SLNN generally optimizes at a smaller number of hidden neurons, say one neuron in the hidden layer as compared to many hidden neurons in BPN. It has also been found that the values predicted by SLNN (with one hidden neuron) model are found to be close to the experimental values. The number of computational steps involved is much less than BPN. The architecture can very easily be extended to multiple outputs.

The discussers thank the management and Dr. S. Vijayarangan, Principal, PSG College of Technology, for giving necessary facilities for carrying out the research work reported in this discussion. They also thank the anonymous reviewers for their suggestions for improving the standard of the manuscript.

References


The writer thanks the discussers for their comments and interest in the paper.

The discussers present and use the sequential learning neural network (SLNN) method for finding the shear capacity of reinforced concrete deep beams. The comparison of SLNN with the experimental results shows a very good agreement. I have no doubt that SLNN provides a powerful technique to obtain problem solutions using existing experimental data.

I am obviously glad (although expected) that SLNN using the large amount of the existing experimental data verifies the accuracy of the analytical method presented in my paper. However, I would like to make the following important points:

1. Methods based on statistical nature (such as SLNN) have also appeared earlier in the open literature. These methods, however, rely only on the experimental results to provide a “solution” to the problem and can not go as deeply into the matter of the subject as analytical methods. Based on an analytical method, important theoretical aspects such as the failure mechanism, the cause of failure, and in general the system of internal forces acting in the structural member can be formally investigated. It is similarly essential that the analysis result in a simple formula that is easy to use in practice and involves only the necessary factors for the particular subject. For instance, in the present case Eq. (10) includes all but only the factors influencing the shear strength of deep beams (i.e., beams with a shear span to depth ratio \( \alpha/d < 2.5 \)), and not the factors influencing the shear strength of slender beams (i.e., beams with a ratio \( \alpha/d > 2.5 \)). The shear strength of slender beams is given also by a simple formula in other papers (Zararis and Papadakis 2001; Zararris 2003).

2. There is a potential drawback in methods based directly on experimental data. The application of such a method requires a relatively sufficient number of valid experimental data. I wonder how an accurate solution can be derived by a method based on a statistical elaboration of experimental results if there is only a small number of data, and in the worst case, if some of the data are wrong, or even as would occur in our case (i.e., shear strength of deep beams) if the failure of some of the test beams is due to another reason, for instance due to bending.

Reference


An innovative reinforced concrete (R/C) beam-column joint model is presented in this paper. The model is capable of simulating inelastic connection behavior resulting from reinforcement bond slip (anchorage failure of beam and column longitudinal reinforcement) and joint shear deformation (failure of the joint core), which is demonstrated in the paper by comparing simulated (model) versus observed (laboratory test) responses of four R/C beam-column connection subassemblies. Based on this, the concluding remarks in the paper indicate that the connection model can well represent the fundamental response characteristics for R/C beam-column joints subjected to "moderate" shear demands. However, given some of the paper's introductory comments regarding poor performance of joints with little transverse reinforcement (or with only moderate amounts of transverse reinforcement), one may be tempted to employ the model in a variety of situations (ranging from low to high joint shear demands and from very little to considerable joint transverse reinforcement). For this wide range of practical cases, then, it is interesting to note how applicable is the proposed constitutive model for the joint shear panel component of the beam-column connection element. In particular, the authors used the modified compression field theory (MCFT) of Vecchio and Collins (1986) to define the envelope of the joint shear stress versus joint shear strain history (as a function of material properties, joint geometry, and joint reinforcement layout), with some experimental data to guide the treatment of response under cyclic loading.

The discussers have found in the literature a total of 50 laboratory tests conducted on R/C beam-column cruciform (interior) connection subassemblies that eventually failed due to joint shear when subjected to cyclic lateral loading and where the researchers provided both average joint shear stress and average joint shear strain data (Meinheit and Jirsa 1981; Kitayama et al. 1988; Watanabe et al. 1988; Leon 1990; Endoh et al. 1991; Fujii and Morita 1991; Kurose et al. 1991; Kitayama et al. 1992; Noguchi and Kashiwazaki 1992; Raffaelli and Wight 1995; Morita et al. 1999; Owada 2000; Teng and Zhou 2003; and Shin and LaFave 2004a). The experiments typically show four distinct regions of joint shear stress versus strain behavior, which approximate a joint envelope response starting from the origin and comprising linear segments connecting three key points. These key points correspond to joint shear cracking, reinforcement yielding, and joint shear strength (Shin and LaFave 2004b). In the case of joint shear failure without any beam hinging, the joint reinforcement typically yields near the second key point; in the case of joint shear failure occurring after some beam hinging, the second key point typically corresponds to the onset of longitudinal beam reinforcing bar yielding (i.e., in some cases of joints with moderately high shear demand, increases in joint core dilation are "triggered" by the onset of beam bar yielding). In all cases, joint shear failure (when a joint simply undergoes large deformations but cannot resist any higher joint shear stress) occurs at the joint shear strength point, with the envelope then typically showing a decrease in joint shear stress with increasing joint shear strain (due to joint reinforcement yielding, joint shell concrete spalling, joint core concrete crushing, etc.).

For each of the 50 R/C experimental subassemblies described above, the discussers applied the MCFT to the joint region (shear panel), using the procedures and assumptions described by the authors in their paper, to determine analytical key point ordinates representing joint shear stress versus strain behavior. Fig. 1 illustrates the relationship between analytical-to-experimental maximum joint shear stress ratio and provided-to-required joint reinforcement ratio. Experimental maximum joint shear stress (which corresponds to reaching the joint shear strength) is simply determined by dividing the maximum experimental joint shear force by the product of column depth and effective joint width (defined here as the average of beam and column widths). Required joint reinforcement is just taken as the cross-sectional area of joint transverse (hoop and crosstie) reinforcement that would be required per ACI 318-02 (ACI Committee 318 2002).

Based on the figure, from the standpoint of joint shear strength the MCFT works particularly well for those R/C connections that have joint reinforcement satisfying (or at least nearly satisfying) ACI 318-02. However, for connections with relatively small areas of joint reinforcement, the MCFT approach consistently gives much smaller joint shear strength values than were actually obtained from the laboratory experiments. This "conservatism" indicates that, in typical R/C beam-column connections, joint shear strength is not nearly as sensitive to the amount of joint transverse reinforcement as the MCFT would predict. (In connections with floor slabs and/or transverse beams that can provide additional confinement to the joint, the disparity between the predicted and actual joint shear strengths could be even greater.) This may have implications if and when the model is used for joints with low to moderate levels of transverse reinforcement. For example, by underestimating the joint shear strength of connections in an R/C frame, excessive attention could be paid to the joints (assuming that they may perform more poorly than they really will), perhaps at the expense of inadvertently ignoring other important possible failure modes. Overall, it appears from the figure that the MCFT works fairly well to define the joint shear strength of R/C beam-
column connections containing more than about two-thirds of the minimum joint transverse reinforcement required by ACI 318-02.

Other observations made by the discussers from a detailed examination of the R/C joint MCFT results versus experimental results were that the predicted joint shear strains upon reaching the reinforcement yielding point were typically somewhat greater than the experimental values, whereas the predicted joint shear strains upon reaching the joint shear strength point were typically somewhat less than the experimental values. This latter observation agrees with the authors’ contention that part of the experimentally measured joint shear distortion can in fact sometimes be related to longitudinal beam bar anchorage distress rather than entirely to shear deformation of the joint core. Lastly, since the MCFT cannot compute any further joint shear (envelope) response after the joint concrete reaches compression failure (the joint shear strength point), it may be interesting to note (for purposes of complete model calibration and verification) that the subassembly experimental descending slopes of the joint shear stress versus strain behavior (after achieving the joint shear strength) were typically found to be in the range of 10 to 20% of the average (secant) ascending slope from the origin to the point of joint shear strength.

In summary, then, the discussers feel that the proposed constitutive model for the joint shear panel component of the R/C beam-column connection element presented by the authors in their paper may actually work quite well up to fairly high levels of joint shear demand (for well-detailed joints with adequate transverse reinforcement), while the model may not work nearly as well (possibly erroneously predicting joint failure at even moderate joint shear demands) for joints that are not necessarily well detailed with sufficient transverse reinforcement. Finally, the discussers are very much in agreement with the authors that proper robust simulation of R/C frame response to seismic loading should include some sort of explicit treatment of inelastic joint effects on both stiffness and strength.

References

ACI Committee 318. (2002). Building code requirements for reinforced concrete (ACI 318-02) and commentary (ACI 318R-02), American Concrete Institute, Farmington Hills, Mich.


Closure to “Modeling Reinforced-Concrete Beam-Column Joints Subjected to Cyclic Loading”

by Laura N. Lowes and Arash Altoontash

Laura N. Lowes1; Arash Altoontash2, and Nilanjan Mitra3

1Assistant Professor, Univ. of Washington, Dept. of Civil and Environmental Engineering, 233C More Hall, Box 352700, Seattle, WA 98195-2700. E-mail: lowes@u.washington.edu
2PhD Candidate, Stanford Univ., Dept. of Civil and Environmental Engineering, MS 4020, Stanford, CA 94305-4020. E-mail: alntash@stanford.edu
3Graduate Student Researcher, Univ. of Washington, Dept. of Civil and Environmental Engineering, Box 352700, Seattle, WA 98195-2700. E-mail: Mitra@u.washington.edu

The authors would like to thank LaFave and Shin for their thoughtful discussion and their evaluation of the application of the modified compression field theory (MCFT) (Vecchio and Collins 1986) to predict the shear strength of reinforced concrete beam-column joints. The writers agree with the discussers that it is critical to establish the range of joint design parameters for which application of the MCFT is appropriate. Recent research activities by Mitra and Lowes (2004) have focused on further evaluation of the shear-panel calibration model as well as the bond-slip model and joint element formulation presented in the paper by Lowes and Altoontash.
The results of recent efforts by Mitra and Lowes support the summary conclusion of the discussers: Application of the MCFT may result in underestimation of joint shear strength for joints that have low to moderate volumes of transverse reinforcement. Fig. 1 shows data similar to that presented by the discussers for a series of interior beam-column joint test specimens (Alire 2002; Durrani and Wight 1982; Kitayama et al. 1987; Noguchi and Kashiwazaki 1992; Oka and Shiohara 1992; Otani et al. 1984; Park and Ruitong 1988; Walker 2001). This series of joint tests includes only beam-column joint subassemblies without slabs, out-of-plane beams, and eccentric beams that exhibited joint failure; this series of joint tests includes some of the joint specimens used by the discussers. In Fig. 1, the ratio of joint shear strength as predicted using the MCFT, without modification for cyclic loading, to joint shear strength observed in the laboratory is plotted versus the joint transverse steel ratio, $\rho_s$. The data in Fig. 1 suggest that the MCFT may be used to predict strength, with a relatively high level of uncertainty, for joints that have a transverse steel ratio in excess of approximately 0.006 and that a different model is required to predict the strength of joints with transverse steel ratios less than approximately 0.006. A manuscript currently in preparation by Mitra and Lowes presents a mechanistic model that is appropriate for use in predicting the shear response of joints with low to moderate transverse steel ratios.

References


mits a special separate evaluation of design wind speed.

The ASCE Wind Load Task Committee that is responsible for writing Chapter 6 of the standard guided the development of the map in a significant way. Decisions to use contours or zones, where to break the 90 and 85 mph (40 and 38 mps) zones, and the decision to include all of Maine in the 90 mph zone based on a single station showing lower speeds were all thoroughly discussed with the committee, and the advice of the committee was included in our decisions. Thus, there is significant engineering judgment from a prestigious group of engineers built into the map. Among these is the simplicity of wind speed zones in place of contours, which provides significant benefits in design and in limiting interpretations of map contour locations that can lead to erroneous selection of wind speeds.

Addressing the first numbered issue above relative to inclusion of stations in more than one superstation, extensive analysis performed prior to formation of the final superstation set showed that inclusion of a station in multiple superstations would not change the final result. For example, Figs. 1 and 2, taken from Peterka and Esterday (2001), show 50-year contours for two different sets of superstations in which no station is included in more than one superstation. The contours are very similar to each other and to those of Fig. 3 of Peterka and Shahid (1998). Thus, the inclusion of an individual station in more than one superstation has no significant effect on the outcome.

Fig. 1. First superstation set with no single station in more than one superstation; speeds are at 10 m in open country

Fig. 2. Second superstation set with no single station in more than one superstation; speeds are at 10 m in open country
Figs. 1 and 2, in combination with Fig. 3 in Peterka and Shahid (1998), show the reason for using zones, and not contours, in ASCE 7. A simple reassignment of stations to a different set of superstations can move the contours sufficiently that the precise location of the contours is not well known. The fact that the contours generally lie within approximately one standard deviation of the standard error indicates the strong likelihood that the entire zone (except for local areas that were not intended to be treated) can reasonably be considered to be one value for design purposes.

The second issue cited by the authors is that the analysis by Peterka and Shahid (1998) included areas of dissimilar geography and meteorology in the same superstation. This is correct and is required by the lack of data. Many western states have only a few stations (see the 1998 paper), so that a choice must be made to include multiple areas without data or omit much of the western U.S. from the map entirely. As an example of a local area with different meteorology, several stations in the Central Valley in California have 50-year wind speeds that could be included in a 75 or 80 mph zone. However, there is insufficient data to know where the boundary for the lower speed is, and representing the bounds of that area (if it could be defined) on a national map would not be sufficiently precise to be acceptable.

The third issue by the authors is that legitimate data were not included in the analysis leading to the ASCE 7 map. This comment apparently results from a misinterpretation of our 2001 report (Peterka and Esterday 2001). In that report, we performed an analysis to see how sensitive the 95 mph contour in west Texas, in Figs. 1 and 2, was to the recorded data. We wanted to see if the probability distribution fit to the data was being unduly influenced by the extreme tail of the distribution. By causing the fit to ignore the two largest values out of about 50, the 95 mph contour disappeared, and we concluded that there was not sufficient high-velocity data to be sure that these data were not statistical outliers. Omitting the few highest samples in fitting a distribution is common practice, and has been shown to improve the estimate of extreme winds (Holmes et al. 1990; Dorman 1982b).

The fourth issue cited by the authors is that the contour program used to create the contours did not include geographic or meteorological influences. Since there does not exist a contouring program with built-in intelligence to recognize topographic or meteorological variations, the only other option is to perform contouring manually, including those influences based on whatever information is available. As part of several investigations for site-specific wind speeds in our consulting practice, we have attempted to include local knowledge of winds where available data were insufficient to make a fully objective quantitative assessment. We have found that there are frequently significant discrepancies between limited available objective data and the reports of qualified observers who did not have access to the quantitative data. Thus, such information must be used with extreme caution.

Our decision to include large areas of the U.S. within zones with limited data was made with judgment and with the counsel of the ASCE 7 committee. As more data becomes available, wind speeds in those areas can be better defined. It should be noted that the use of large zones has also been incorporated into the Australian wind load standard. It should also be noted that an analysis by Dorman (1982a) concluded that the U.S. nonhurricane wind region contains, at most, two or three statistically significant wind levels [Peterka and Shahid (1998) selected two].

Other criticisms within the paper not included in the list of four are too numerous to comment on here individually. A few are addressed. Tropical storms were purposely included in the database so that their impact on the results near the hurricane coast could be observed. There are so few strong-wind hurricane measurements in the record that the contours, except in south Florida, did not reveal the existence of hurricanes. Since the coastal portion of the nonhurricane map was overlaid by higher-speed hurricane contours from other sources, there is no consequence to this procedure. Another comment related to our inclusion of coastal stations with inland stations in superstations on the east coast. Since almost all nonhurricane storm winds pass from land to water, winds at coastal stations arrive over land so that a coastal station is not significantly different from inland stations. Again, the coastal areas were overlain with higher speed contours, so the impact of this issue is moot.

Locations such as Portland and Ogden, cited in the paper, are local areas included in special wind regions (Portland) or local mountainous regions (Ogden), where special analysis is required (as indicated on the ASCE 7 map).

The paper makes an issue of the waste of materials if a structure is designed for a speed specified too high, and lack of safety if the map design wind speed is too low. The uncertainty in specification of pressure coefficients in ASCE 7 is significantly larger than the current level of uncertainty in wind speed, and wind-tunnel results on buildings under design frequently indicate that the standard is slightly conservative by roughly the magnitude of underdesign claimed in this paper. Design under ASCE 7 provides for “failure” at first significant yield, indicating a significant safety factor against a collapse that would cause injury. Thus, neither of these claims is at the level of importance inferred in the paper.

While in our opinion the current ASCE 7 wind map is based on reasonable analysis, we do believe that additional analysis would be of benefit. There are now 12 years of additional data available, including many stations installed during the 1990s at locations without prior coverage. In addition, there may be sufficient data to permit separation of data into thunderstorm and non-thunderstorm winds to provide a more accurate assessment of extreme winds for recurrence intervals of 1,000 years or more. We believe that analysis including recent wind data would be far more beneficial than a continuing reanalysis of the pre-1991 data addressed in this paper. We also reiterate that the paper only criticizes previous analysis, and does not demonstrate an improved analysis method or suggest an improved map.

References

Closure to “Wind Speeds in ASCE 7 Standard Peak-Gust Map: Assessment”
By Emil Simiu, Roseanne Wilcox, Fahim Sadek, and James J. Filliben

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Emil Simiu1; Roseanne Wilcox2; Fahim Sadek3; and James J. Filliben4
1NIST Fellow, Building and Fire Research Laboratory, National Inst. of Standards and Technology, Gaithersburg, MD 20899.
2Undergraduate Student, Dept. of Physics, Brigham Young Univ., Provo, UT 84601.
3Research Structural Engineer, Building and Fire Research Laboratory, National Inst. of Standards and Technology, Gaithersburg, MD 20899.
4Mathematical Statistician, Statistical Engineering Division, National Inst. of Standards and Technology, Gaithersburg, MD 20899.

We thank Peterka and Esterday (P&E) for their discussion.

As its title clearly indicates, the objective of our paper was to address the issue of whether the basis for the ASCE 7 Standard peak-gust map as developed by Peterka and Shahid (P&S) was acceptable. P&E are in full agreement with us when they note that the ASCE 7 map can be substantially improved. However, this goal cannot be achieved without a careful scrutiny and assessment of the methodology used to develop the map. It is regrettable that no serious scrutiny—one based on detailed knowledge of the methodology and data used by P&S—was performed before the map was published in the ASCE 7 Standard. But this does not mean such scrutiny is not necessary. We performed it only as late as 2001 because it took that long before the authors of the map could provide the requisite methodological details and data in usable form, having indicated in the closure to their paper (Peterka and Shahid 1998) that the data took too much space to be reported.

P&E’s belief that our paper has not “understood the improvement of the gust map in reducing sampling error over the earlier fastest mile wind map” is in our opinion due to their failure to realize that reduction of sampling errors is only part of a larger story. As explained in detail in the paper, what the authors have actually done is to reduce typically modest sampling errors by introducing typically large bias errors. Sampling errors can be reduced to almost zero by analyzing a sample consisting of many extreme data recorded over a very large territorial expanse. It is clear, however, that this would be inadequate both statistically and for structural engineering purposes. In a future wind map sampling errors should be reduced to the extent possible, but without overwhelming such reductions by unacceptable—and in many cases unnecessary—bias errors inherent in P&S’s work.

P&E believe that “an unstated implication in the paper seems to be that there is sufficient existing data to permit wind speeds to be specified.” There are two situations that need to be addressed separately. In the first situation a reasonable amount of good data are available. We stated that the minimum that can be expected of the analyst is that such data be used. For example, it is in our opinion unacceptable to disregard excellent data samples available for about 100 stations in easily accessible fastest-mile databases, as P&S have chosen to do. Using them would be a useful step toward developing an improved wind map. On the other hand, knowingly using data that are demonstrably inappropriate for the location of concern is not helpful. We gave examples of such misuse of data (see, e.g., superstation 999, p. 429, or superstation 99927, p. 340). P&E have not explained why the wind climate over open terrain in Vermont should be influenced by data taken in New York City’s Central Park, surrounded as it is by tall buildings on all sides, and located in a region whose wind climate is not the same as Vermont’s. Surely the fact that in a number of cases such practices may turn out not to be too harmful is not a sufficient reason for lack of attention to the composition of data samples.

In the second situation, where there are no sufficient data for the wind speed to be specified, good judgment needs to be used conservatively, and the help of NOAA or other meteorologists needs to be sought. Using data just for the sake of using them, even though they are unrepresentative, is not in our view an acceptable practice. The fact that insufficient data are available for Alaska, say, would not justify the use for Alaska of data from Washington, Oregon, or California. Nor, for that matter, should New York Central Park data be used for central Massachusetts, or central Massachusetts data for New Jersey, as P&S have done. To summarize, we find unacceptable the statement by P&E that including “areas of dissimilar geography and meteorology in the same station is correct and required by the lack of data.” As P&E correctly state, the ASCE 7 map includes a statement on the exclusion from the map of winds in terrain of complex topography. Yet, to use one example from our paper, P&S include, for the description of the Portland, Oregon, climate, data measured at Yakima, where “local topography is complex, resulting in marked variations of wind within short distances.”

Similarly, P&E argue in effect that what data they use or, equivalently, how they chose to create their superstations, doesn’t really matter. We do not share this view. In this case also adding the wrong data to a data set is not justifiable. An approach implying robustness of the wind climate to data changes and shuffling, and which may be harmless in some instances, can be quite harmful in others. Our paper provides many examples of situations where this is indeed the case.

P&E take issue with our criticism of the fact that “individual stations were included in multiple superstations.” Systematic inclusion of the same stations in more than one superstation is in our opinion unacceptable. It leads to an equalizing of wind climates that masks actual differences between the climates of various locations. Justifying this practice by shuffling stations around to show that it doesn’t matter what data are used may work in some instances but will not work in others, as our paper demonstrates.

The map need not be all on one page, as P&S suggest to justify their failure to account for significant wind climate differences between various regions. In fact, in the current version of the ASCE Standard the wind map is represented on five pages. Nor are contours the only alternative format to specifying one wind speed over a vast area, as P&E suggest: the current standard uses wind speed descriptions for individual zip codes for large areas of the country.

P&E assert that there is significant engineering judgment from a prestigious group of engineers built into the map. The following example shows that input from and approval by a standards committee is far from guaranteeing that a wind map is correct. The ANSI A58.1-1972 Standard indicated a 50-year basic wind speed at Corpus Christi of 31.2 m/s (70 mph) at 9.1 m (30 ft) above ground in open terrain, when in fact Corpus Christi was hit in the period 1916–1970 by three hurricanes with wind speeds of up to 53.6 m/s (120 mph) at 7 m (23 ft) above ground in open terrain. The fact that a group of prestigious engineers had input into and voted in favor of that map did not make it right. A similar statement holds for P&S’s map. The fact that two incorrect maps were
approved by prestigious engineers says less about those engineers than about the modus operandi of some standards committees, possibly including the ASCE 7 committee on wind loads, which does not always require that supporting data and sufficiently detailed methodological information be made available for the committee to be able to scrutinize a proposed provision and give it more than cursory endorsement. The engineers who approved the map had no access to such data and information and could not know that P&S took the liberties they did in their analyses. Such liberties included commingling data that do not belong together, using data taken at the same station to help to define the wind climate at two or three distinct superstations covering vast territorial expanses, using in the same dataset data taken over different types of topography, and eliminating from the analysis high wind speeds that didn’t match their postulated extreme speeds. Just because, according to P&E, Dorman (1982) and Holmes et al. (1990) wrote that eliminating tail data in an extreme wind dataset is advisable does not mean that it is necessarily appropriate. As noted earlier, the authors are in full agreement with P&E that a much better map can be created. We hope that such a map will be based on much more careful work than the work that went into P&S’s map, that before being presented to the ASCE Standard committee it will be carefully documented so that members of the committee can readily understand, scrutinize, and verify the assumptions and procedures used for the map’s development, that all the available data will be used, that statistical approaches will be consistent with good extreme value statistics practice, and, last but not least, that the cooperation of NOAA or other meteorologists and of qualified statisticians will be sought where needed.

We firmly maintain our position that, largely owing to improper use or failure to use available information, P&S’s map does not reflect correctly the country’s differentiated extreme wind climate, and that, contrary to P&E’s claim, given the large role played by extreme wind speeds in the design of many types of engineering structures, unnecessarily misestimating extreme wind speeds is a disservice to the structural engineering community and the public at large. We trust that this open debate on Peterka and Shahid’s work will have been beneficial to the engineering community, and wish to thank again P&E for their comments and the opportunity to participate in such a debate.

References