Discussion of 110-M48/From the September-October 2013 *ACI Materials Journal*, p. 529

Detection of Internal Defects in Concrete Members Using Global Vibration Characteristics. Paper by H. Sezer Atamturktur, Christopher R. Gilligan, and Kelly A. Salyards

Discussion by Yun Zhou and Wei-Jian Yi

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The vibration response-based nondestructive defect detection technique has drawn great attention in the last few decades. In general, damage identification consists of three steps with the complexity extent of the identification problem: 1) detecting the occurrence of damage; 2) localizing the damage zones; and 3) estimating the extent of damage. Several damage identification techniques have been proposed for predicting damage location and severity (Doebling et al. 1996). The discussers greatly appreciate the authors' innovative and comprehensive work in identifying the internal defects of concrete members based on the proposed technique. The location and severity of size-controlled rock pocket and honeycomb defects can be identified via comparing the finite element (FE) model prediction with experimental measurements. To the discussers' knowledge, quite a few scholars did a similar dynamic test on reinforced concrete (RC) beams such as Ren and De Roeck (2002) and Unger et al. (2006), in which the possibility and reliability of using the vibration response-based defect detection technique and dynamic structural identification method were trying to be revealed. Due to the dynamic test usually generating the global vibration response of the whole structure, the detection of the presence, location, and severity of a defect highly relies on further analytical analysis. The discussers wish to comment on the authors' interesting procedure to detect the defect on the RC beam using mode shape curvature as well as Bayesian statistic inference technique. An effort seems to have been made to address the technique for the quality-control tool for fabricated concrete members with simple and repetitive geometries in the factory. However, the discussers would like to have the authors' clarification for the following problems.

1. In the section "Scaled Concrete Beams with Controlled Defects," the rock-pocket defects were reproduced by placing an aggregate cluster surrounded by an impermeable net within the wood formwork before the concrete was placed. The discussers are interested in the rationality of using this method because the density of the rock is obviously higher than the concrete, while in Table 1 it was shown that for Beam 2 and Beam 3 the mass reduction ratio β are 0.98 and 0.91, respectively. The discussers regard the local mass reduction ratio β should be larger than 1 considering the rock pocket status in Fig. 3 (right). How did the authors evaluate the stiffness reduction ratio α and mass reduction ratio β based on the four man-made defects as shown in Table 1?

2. In the section "Experimental Program," a multiple-reference impact test was conducted to determine the acceleration responses in the vertical direction under vertical impact excitations. Measurements at 57 uniformly distributed points were collected; however, in Fig. 7, only nine accelerometers were demonstrated and used. It is not clear whether the authors independently impacted nine points each time for the substructure test before integrating the corresponding frequency response function (FRF) of the substructure into the full FRF matrix. Regardless, a 2-second decay time is a little bit too short, while 0.5 Hz frequency resolution is too coarse for the beam suspended by bungee cords. In addition, in the stabilization diagram in Fig. 9, there was obviously a possible mode at round 800 Hz but was not listed in Table 2. To the contrary, there was a mode at 235 Hz in Table 2, but it was not displayed in the stabilization diagram in the same figure. Furthermore, the beams were suspended by bungee cords at each end attached to a steel frame, and the tested rigid body mode frequency was 5 Hz and the first natural frequency of the beam was 235 Hz, yielding approximately a 1:50 ratio, based on which the authors judge the free-free boundary conditions are successfully realized. The discussers are interested in what the "rigid mode" looks like. In Table 2 and Fig. 9, the "rigid mode" is also not listed or demonstrated.

3. In the section "Numerical Methodology," 162 computer runs are executed to investigate the changes in the vibration response of the beam for various scenarios of location and severity of the defects. A full factorial design of numerical experiments was conducted to generate the first five natural frequencies, mode shapes, and modal shape curvatures. The torsional sixth mode should be more sensitive to the induced defects on the beam, but the authors mentioned the torsional modes have more variability between repeated experiments. In addition, the defect scenarios are designed and produced at 1/4 location of the beam, but in Fig. 11, it seems that the changes of the fifth mode shape curvature have no certain relationship with the damage aside the damage location; it can hardly be distinguished from the curves at the location.

In Fig. 12, light gray lines represent an ensemble of mode shape curvature simulations with internal defects of varying levels of severity at various locations along the beam, and dark lines represent the experimentally obtained mode shape curvature for mode five for each excitation location. There should be nine dark lines in Fig. 12, while there are obviously fewer lines shown in the Beam 2 figure than in those of Beams 3 to 5. The ensemble of simulated mode-shaped curvature envelopes the experimentally obtained mode shape curvature, so the authors conclude one kind of damage pattern in the FE model will best match the mode shape curvature collected from the experiment. The conclusion was obviously too rough.

4. In the section "Localization of defects," the Bayesian statistic deduction technique was used for damage localization, but in the paper it was roughly introduced that "[t]hrough successive and systematic comparisons of measurements with model simulations, the posterior distributions of parameter x are obtained for all four beams with defects (Beams 2 through 5)." The details of how to obtain the posterior distribution are not introduced. Furthermore, the mean value of the posterior distribution used

for the damage location is questionable, and in general the maximum likelihood estimation was the first recommended value to use. In Fig. 13, it was obvious that the mean value of the posterior distribution can localize the correct location in the potential damage region for Beams 2 and 3, while for Beams 4 and 5, the localization line is biased to the boundary of the region. Will the identification technique not be sensitive to the honeycomb type damage? The authors did not explain about this.

5. In the section "Determining stiffness reduction ratio," to infer the stiffness reduction ratio α, the prior knowledge gained in the previous section regarding the location of the defect indicates the fifth segment. The reduction in the mass due to the defect remained an unknown and was treated as a random variable with uniform probability between 0.7 and 1.0. The stiffness reduction ratio α was deduced and concluded in Table 3. It seems that the inferred values for the four damaged beams remain almost the same as 0.88 and 0.89. Does the fifth mode shape curvature have a lower differentiation in stiffness identification? Additionally, the resolution ratio of the posterior distribution bar chart in Fig. 14 is not unified.

6. In the section "Determining mass reduction ratio," the localization of the internal defect and the severity of the defect for reducing the stiffness, which were respectively identified in two steps, were taken as the prior information, then the mass reduction values were inferred in the third step. *A priori* information given in the previous section regarding the likely values for mass reduction for Beams 4 and 5 was used. The discussers are curious about which value the authors used—the inferred value or the correct value? Due to the identification errors accumulated in the former two steps, the actual damage mass would differ from the posterior distribution estimation value, which can be verified by identification errors in Fig. 15. The mass deduction for Beams 2 and 3 have also not been addressed. The discussers guess the model of the beam has not been carefully calibrated at the initial stage and results in the identification errors.

7. In this study, the Bayesian inference technique was employed. The defect detection problem is evaluated in several stages of increasing levels of information regarding the defect, where the first objective is to localize the defect. Upon localization, the purpose then becomes one of determining the reduction in stiffness and mass, respectively (in each stage only one parameter was being identified). The shortcoming of this strategy was inevitably amplifying the error step by step. In an actual damage identification process, the locations as well as the mass and stiffness reductions are unknown parameters; the errors in the first step will lead to the following accumulated errors. Actual multi-variable Bayesian parameter identification can be conducted once in a scenario to potentially reduce the error accumulation, or the least-square method can directly be used to look for the optimized three parameters via comparing the 162 runs of defect simulation with the measured results.

In general, the vibration response-based nondestructive test generates the global response of the structure and will not be sensitive to the local damage. The proposed damage identification method in this paper can hardly be used in the actual structure because of the idealization of the boundary condition as well as the lower defect identification ability even for one damage location; but it is suitable for fabricated concrete members with simple and repetitive geometries in the precast concrete industry, as mentioned by the authors. The general nondestructive defect testing for the precast member was to use the ultrasonic inspection technique, which is more accurate and quick. Clarification of the questions would certainly help in better use of the proposed method for analytical and practical test problems.

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AUTHORS' CLOSURE

The authors value the comments and questions posed by the discussers regarding the method for identifying internal defects using global vibration characteristics. The authors have reviewed them and respectfully respond to each of the queries individually. The responses to each discussion point are summarized as follows:

Regarding the first comment, as it can be seen in Fig. 3 (right), the rock pocket is a cluster of aggregates with a considerable amount of empty volume in between the aggregates. Although the aggregate itself may have a density greater than the concrete, when combined with the voids between the aggregates, the density is less than that of the concrete, resulting in the mass reduction ratios shown in Table 1. To determine the mass reduction ratios, the weight of the aggregate cluster is measured and divided by the volume to determine the approximate density. The stiffness reduction is calculated by reevaluating the moment of inertia of the cross section where the cross-sectional area of the defect is assumed to have zero stiffness (according to the dimensions given in Fig. 4).

Regarding the second comment, as clearly stated in the paper, a total of 57 measurement locations were used. The location and measurement direction of the sensors are shown in Fig. 6. Experiments are completed with the roving accelerometer method. A basic description and additional information about this type of hammer testing is available elsewhere (Avitabile 1998). Figure 7 illustrates just one of the series of test setups required to measure all 57 locations. The measurement parameters are determined to obtain the mode shape and curvature measurement in a most repeatable and accurate manner. The discussers' argument about the measurement duration being too short is not justifiable, as the response is fully attenuated within the measurement duration (refer to Fig. 16). The selection of 0.5 Hz frequency resolution is sufficient given the width (1600 Hz) of the frequency range of interest.

In Fig. 9, rigid body modes are not demonstrated, as they are intrinsic to the characteristics of the suspension system used and do not provide information regarding the characteristics of the beam itself. Note that the coordinate axis in Fig. 9 begins at 200 Hz. The mode at 235 Hz is identified as it appears in the frequency response measurements, as shown in Fig. 17. The mode at 800 Hz is not implemented in the analysis, as they correspond to torsional modes. Incorporation of torsional modes into the analysis could be further investigated in future studies.

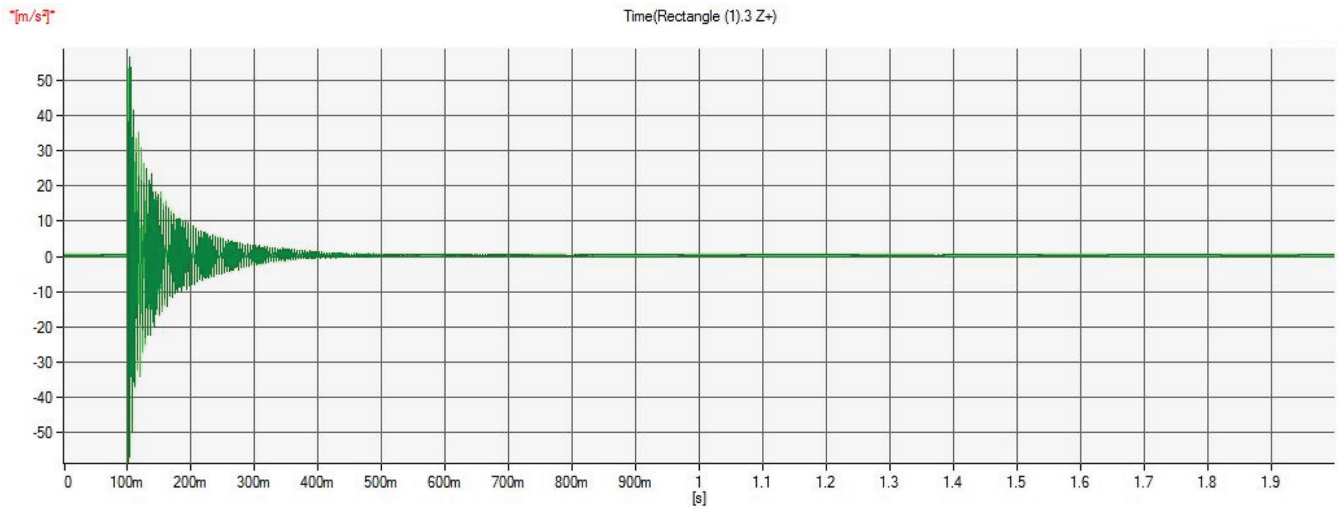

Fig. 16—Time-history measurements demonstrating attenuation of response within measurement duration.

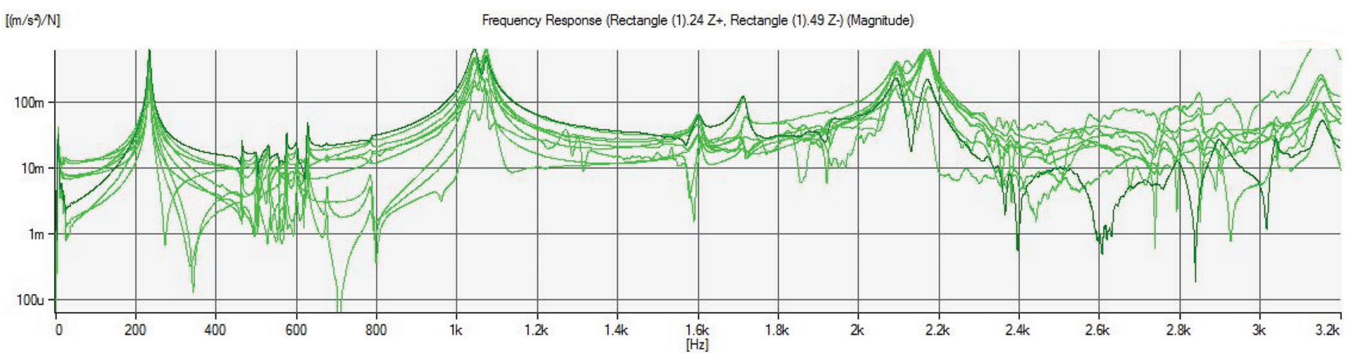

Fig. 17—Frequency response function of beam with no defects.

Regarding the third comment, the authors are not clear why the discussers claim that the torsional modes would be more sensitive to the particular defect of interest, as it has been well documented that the sensitivity of modal parameters to damage and defects varies for each mode as well as for the damage and defect type of interest (refer to Atamturktur et al. 2011, Prabhu and Atamturktur 2013, and references provided therein). In fact, it is the authors' opinion that there is no clear way of reaching this conclusion and making such claims without a thorough sensitivity analysis. The fact that torsional modes demonstrated higher levels of variability due to experiments is an aspect that is related to the testing conditions and the authors are unclear regarding the point the discussers are trying to make. The authors would like to emphasize to the discussers that Fig. 11 presents the fifth mode shape curvature for all 57 measurement locations. Note that the x-axis in Fig. 11 is not the length of the beam, but rather the number associated with the measurement location. For this reason, it is not expected that the damage location at 1/4 of the length would be immediately evident in Fig. 11.

The authors' conclusion regarding the existence of an FE model that will best match experiments is a fairly straightforward conclusion that forms the basis of model calibration. From an ensemble of model predictions, one can always find a model to best match experiments. The discussion presented in the paper about the model simulation enveloping the experiments was to confirm the range of parameter input values (defined herein as a uniform prior distribution). The discussers are invited to study excellent articles on model calibration under uncertainty available in the literature. Many of these articles are provided in the references of Atamturktur et al. (2012) and Atamturktur and Laman (2012).

Regarding the fourth comment, the scope of this article does not allow a lengthy discussion on the Bayesian calibration technique used (primarily due to length limitations). The authors provided a reference in the manuscript for Higdon et al. (2008), which provides the technical details of the implemented approach. The authors implemented Gaussian process model (GPM) emulators to expedite the Markov Chain Monte Carlo (MCMC) runs. The priors for the hyperparameters of the GPMs are implemented as recommended by Higdon et al. (2008) and the MCMC runs are executed until converged statistics are obtained (with a total of 50,000 MCMC runs). The discussers are encouraged to review the earlier publications of the first author on the use of this Bayesian calibration technique (such as Atamturktur et al. 2012), which provide more detailed discussions that should alleviate any confusion.

Furthermore, using the mean value of the predictions from the posterior distributions in conjunction with the standard deviation is a recommended technique by developers of this particular approach. Please refer to Unal et al. (2011), for instance.

In Fig. 13, the results demonstrate that predicted response is consistently within the "vicinity" of the known location of the defect. In fact, the authors would like to discourage the discussers to consider the results presented in this work as evidence that the method will be applicable to localize rock pocket defects. Further study is necessary to confirm the applicability of this method for full-scale beams, beams with different levels of reinforcement, geometric dimensions, defect sizes, as well as the number of defects. This manuscript only presents the potential of this method and the authors wish to avoid making hasty claims about whether the approach can detect honeycomb defects versus rock pocket defects.

The inferred value of location of defect is used in further analysis while investigating the severity of defect (from Table 3). There should be no question about the fact that the presented approach would lead to a greater difference in the discussers' prediction of the mass reduction ratio from the actual value because of the cumulative nature of error. The authors could not clearly discern what the discussers were questioning in their fifth and sixth comments and, as such, cannot respond.

Regarding the seventh comment, the authors would like to emphasize the curse of dimensionality in that trying to calibrate a large number of parameters could lead to compensations between parameters and the creation of nonunique solutions. This is precisely what was observed during the study. Decoupling the identification and classification (preferably using different response features) has been implemented in numerous prior studies (Pandey et al. 1991; Shi et al. 2000).

The authors again thank the discussers for the close examination of the article and encourage continued discussion on similar topics in the future.

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Discussion of 110-M51/From the September-October 2013 *ACI Materials Journal*, p. 559

Effects of Carbonation on Chloride Penetration in Concrete. Paper by Myung Kue Lee, Sang Hwa Jung, and Byung Hwan Oh

Discussion by K. Sivakumar

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The authors have written an interesting paper and attempted to study the combined effect of carbonation and chlorides, and in particular the influence of carbonation on the presence of water and acid-soluble chlorides. The discusser would like to raise the following to get much-needed clarity.

The authors have used two exposure regimes for testing carbonation depths of specimens presumably using concrete cylinders. In one of the regimes, after 28 days of water curing, the concrete specimens were immersed in a 5% NaCl solution for 1 week and thereafter immediately subjected to accelerated carbonation at 10% CO_2. The carbonation depth values reported by the authors in Table 3 are of no significance when compared to values reported by similar studies from past research[15,16] and actual field measurements.[17] For the sake of brevity, the discusser would like to select H1FA00 concrete and compare it with accelerated carbonation studies on similar concrete elsewhere. It has been reported[15] that the accelerated carbonation depth after 20 weeks of exposure to 4% CO_2 is 17.5 mm (0.7 in.). From another work,[16] the depth recorded for exposure to 10% CO_2 after just 4 weeks is 5.5 mm (0.2 in.). For a similar concrete kept in prCEN TS12390-10 natural exposure condition for 2.6 years, the carbonation depth recorded was 2.5 mm (0.1 in.). Therefore, with the available data from past research, the discusser finds the accelerated carbonation depth of 0.1 mm (0.004 in.) for a H1FA00 specimen at 23 weeks, reported in Table 3, as rather unusual. This has led the discusser to infer that neither carbonation has occurred nor an environment for it to happen has been provided, hence the obvious low carbonation depth values in both the exposure regimes. Given this scenario, it is premature to correlate the presence or ingress of chlorides to carbonation in this paper and, furthermore, comment on the effect of cement type and fly ash.

Carbonation can effectively happen only when the specimens are subjected to a preconditioning period to get the right moisture balance—for example, a week of exposure to natural air—after removal from the 5% NaCl solution. Moreover, the choice of 10% CO_2 remains to be explained. And, as for the other exposure regime, the temperature of the 5% NaCl solution and natural air is not stated. A figure showing the experimental arrangement would have helped readers.

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