Theme Report on Topic 11
DYNAMIC BEHAVIOR OF STRUCTURAL ELEMENTS

by

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Having innocently accepted the responsibility of preparing a theme report on the basis of experiences of technical sessions past which did not include more than ten papers through which to weave a skein, the writer was seriously shaken to be confronted with a total of 53 contributions with 37 of them in the long-paper classification. As would be expected, no matter how conscientious the initial screening process, the subject matters of the papers range far and wide, and they are not all interrelated. To do justice to all papers and to the theme is well beyond a few weeks' work. At the time of this writing, having overcome the depression caused by the Napoleonic instinct of conquering all and having made his peace with the Procrustean alternative, the writer will proceed to take the only option open to him, given the time and his circumstances. Rather than doing justice to all, he will do injustice to a few.

It is difficult to pass without comment on the wealth of the material contributed for this session. It has been said that the organizers had to look far and wide to find sufficient papers for the First World Conference on Earthquake Engineering held in San Francisco in 1956. Twenty years later, we find that a single session of the conference has sufficient material to fill a book or two. It is not only the number of the papers that impresses the writer but also their quality. Many of the 37 long papers deserve much more space. One must also consider that this is true for many of the sixteen "short" papers, their location in that category having been a result of the selection lottery. It would be of great benefit to researchers using the proceedings of this conference if all authors listed in the closure publications where their work has been or will be documented in sufficient detail.

The primary intent of this report is to suggest to the authors several specific topics for discussion or clarification. These topics are related to specific papers. But there are a few general ideas which must be discussed initially.

Columns and Shear Columns

Several investigators have contributions on the behavior of reinforced concrete "columns" with well supported conclusions about their behavior under cyclic loading into the nonlinear range of response. However it could be misleading to project these conclusions to understand the behavior of buildings with the only generic identifier being "column" because some of the conclusions, while quite correct for short stubby columns, may not apply to columns of "ordinary" proportions. The image problem is quite similar

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to that which has existed for walls. The "shear walls" to which papers by Kobori - Inoue-Kawano and Yamada-Kawamura-Katagihara-Moritaka refer are indeed shear walls with the full implications of the definition. On the other hand, the "shear walls" to which the paper by Takayanagi and Schnobrich refers are in quite a different classification because their response is dominated primarily, in that particular case almost entirely, by flexure. We have come to a similar situation with respect to "column."

Notwithstanding the fact that in the past the writer was quite in favor of dropping the qualifier "shear" from "shear wall", having read the papers contributed to the session and their conclusions, he is in favor of adding a similar qualifier to the classification "column." Even though geometry by itself does not provide an infallible criterion, it would seem to this writer that all elements with M/Vd ratios less than two should be classified as "shear columns." (M = maximum moment, V = corresponding shear, d = effective depth.) He is not delighted with this definition but it does appear to be an acceptable compromise when weighed against the alternatives of coining a new word or perpetuating the self-negating definition of "deep beam" through the choice of "deep column."

Thus, "wall" and "column" would apply to elements of which response history would or could be governed by flexure. "Shear wall" and "shear column" would refer to elements of which response is dominated by shear.

Ductility, Hysteresis, and Damping

If discussions during and after this session lead to a reasonably well defined policy statement about the use of these three interrelated terms, the conference will have served the profession well.

What is of particular interest to the writer is that whenever these terms are mentioned, there lurks in the background, if not explicitly mentioned, a question of "goodness." The professional reader is by now conditioned to interpret the experimental results by asking questions such as "was the ductility over four?", "was the hysteresis loop stable?", as if these were universally applicable criteria of goodness. Various trends in the opinions expressed in the contributions to this session underline the brittleness of applying such rigid criteria at large. Some investigators have judiciously refrained from making even implied conclusions about the levels of desirability of the classes of response they have observed. We note that, in studying the response of braced steel frames, decay in the load limits of the hysteretic loop with increase in displacement does not scandalize the observers. Those working with shear columns do not press the panic button when they identify practically inevitable bond failures.

Clearly, one may not make absolute judgments about the goodness of the response independently of the function of the element. There may be few arguments against the desirability of a stable hysteresis loop in the case of an only column but a decaying hysteresis loop may not be intolerable for connecting beams of walls.

The writer would like to ask the contributors to focus their experience on the following questions with the hope that a generally applicable set of criteria may be distilled from the many perspectives manifest in the papers.

Does ductility, however defined, provide a complete basis for interpreting or "predicting" earthquake response?

What are the goodness criteria for hysteretic response?

Is it useful to convert the measured properties of the hysteresis-loop into a measure of damping? What is the preferred conversion rate? Would a simple ratio such as that of the area divided by the average slope suffice?

Is "axial hysteresis" unimportant to structural design?

Stress-Strain

The sole contribution on unit force-displacement relationships for any material was provided by Shimazu and Hirai. Their paper considers the fundamental problem of the influence of lateral reinforcement on force-displacement relationship of concrete subjected to axial as well as eccentric loading. Because the paper refers to much data not readily available to the English speaking audience, lack of any comparison with results of experiments reported in English is understandable. Shimazu and Hirai start with the relationship

$$f_1 = f''_c + K f_2 \qquad (1)$$

where f_1 is the axial unit stress, f_2 the lateral unit stress, f''_c the unconfined strength of the concrete and K is an experimental coefficient which has been found by others to be dependent on the type of concrete. The authors assume K = 4.1 as observed by Richart, et al (1) and conclude from their tests under axial loading that (a) the strain capacity of the concrete is improved at a faster rate than the strength capacity by transverse reinforcement and (b) the efficiency of rectilinear loops is approximately half as much as that of circular hoops.

It would be a significant service to the profession if Shimazu and Hirai compared their conclusions quantitatively with those in and referred to in Reference 2.

The authors' Eq. 4 deserve explication. Is it implied that with all other variables constant, a reduction in the ratio of bar diameter to core diameter would increase the efficiency of the transverse reinforcement?

Figure 3 of the paper indicates an increase in the initial modulus of the confined concrete with increase in amount of transverse reinforcement. Is this incidental to an overall pragmatic statement of the shape of the "stress block" or has it been measured in the tests?

Shear Columns

The paper by Higashi, Ohkubo, and Ohtsuka bring to the session valuable information from the extensive work being carried out under the stewardship of the Japanese Building Research Institute. Data presented in the paper show that the envelope to the force-displacement responses for a given shear column is not independent of the loading history. Number and manner of the loading cycles affect that portion of the envelope beyond the maximum load. A significant feature of the test specimens, and therefore of the scope of applicability of the conclusions, is the geometry of the columns which have M/Vd ratios of two or one.

The implications of the work reported here are clear. Given the evidence, it is not justifiable to make a dynamic response analysis based on an envelope or spinal curve which is stated independently of loading history. It is the prerogative of the authors to define the scope of this conclusion. If the number of loading cycles has a perceptible influence on the negative slope of the envelope or spiral curve, would the experience of the authors justify any conclusions about the influence of repeated cycles of biaxial cycling on this part of the force-displacement curve?

The contribution by Minami and Wakabayashi puts us into a range of less stubby columns. Half of the specimens reported had an M/Vd ratio of slightly more than two. And this paper suffers also from the frustrations of the preceding one: a wealth of data and conclusions compressed into too little space.

The authors have used a small but very simple test specimen representing a column in a frame. Space has evidently not permitted them to discuss how their results compare with similar column specimens tested under less sophisticated boundary conditions. Would the differences have been in column response if they had used the BRI test frame, with which Dr. Wakabayashi is quite familiar, described in the article by Higashi et al. How much did the joints, the area common to the column and the beam, participate in the overall measured phenomena?

It is stated that all column failures were influenced by shear, the ultimate failure culminating as a result of bond splitting. It would be very helpful for the authors to interpret their results in terms of commonly used index values such as $v/\sqrt{f'_c}$ so that their experience may be conveniently related to other investigations.

Flexural Hysteresis of Columns and Beams

Despite the reference to shear wall in the title, the series of four full-scale tests by Anicic and Zomolo should be considered in the same group with the data referring to columns and beams because the results refer specifically to the hysteretic response of flexural elements. The test specimens were developed to investigate the behavior of lintels or connecting beams of wall systems. It is evident that the authors have developed these specimens after much planning and preliminary experimentation in order to obtain data directly applicable to the prototype without having to test the entire building. It would be useful for them to discuss their criteria. Did they consider the possibility of axial forces in the lintels of the prototype structure? Have the measured initial stiffness of these beams been compared with calculated values? Is the component of the measured deflection attributable to reinforcement slip at the wall-lintel interface of perceptible magnitude? To what specific use do they intend to put the inferred damping factors that they have calculated? Are they satisfied that the fitted hysteresis loops can be projected to apply to other elements?

The tests of "bridge piers" described by Priestley, Park, Davey and Munro provide an interesting contrast to the tests of the shear columns. We are now dealing with slender columns having M/Vd ratios on the order of 3.5 or more. Another interesting difference of this paper in relation to the papers by Higashi et al and Wakabayashi et al is that the paper contains indirectly a criterion of "goodness", which leads us to infer that "ductility factors in excess of five" and a "stable loop" are good. It would be desireable for the authors to specify their criteria of goodness and consider how they would apply their criteria to the results of the shear-column tests. The limited space available for their presentation must have frustrated a complete description of the dynamic test. The closure will provide the space required to describe why they chose the experimentally measured initial stiffness for the force-displacement relationship in their dynamic analysis and why they picked a damping factor of 7%. Even though the specimens that they have used are not exactly models and are full-scale elements in their own right, it would be useful for the authors to discuss how they propose to project their observations with respect to detail failures to the actual prototypes.

The point of departure for Atalay and Penzien for developing an algorithm for fitting curves to existing test results highlights some of the problems encountered in not having a well defined boundary between shear columns and columns. They base their theory on the existence of an invariant envelope curve which, according to Higashi et al., might be construed as not to exist. And yet the assumption of the existence of an invariant envelope is plausible in relation to the tests described by Anicic-Zomolo, Priestley et al., and Atalay-Penzien. Expressions 7 through 12 offered by Atalay and Penzien are interpreted by the writer to be derived from a specific series of tests and therefore applicable to members having the properties of the members of that series of tests. The authors are requested to describe the domain of applicability of routines as well as specific equations that they present.

The Double Helix

Umemura, Shimazu, Tadehara, Konishi and Abe defy the seldom questioned belief that continuous transverse reinforcement is inefficient for torsion and cyclic shear because of the impracticality of a double helix. Here again the emphasis is on M/Vd ratios. Assuming that the economical use of such reinforcement is feasible, the double helix would appear to be an excellent solution for strengthening shear columns. Thirty percent increase in strength, in relation to similar columns having the same amount of transverse reinforcement in conventional hoops, is an impressive figure.

The authors' point about the nonsymmetrical hysteresis loops of specimens with conventional hoops could be elaborated on. Is the nonsymmetry a direct result of near failure in one direction affecting the response in the other direction, a rather common occurrence, in "shear-weak" flexural elements, or is it a particular feature of the reinforcement detailing? Would the authors recommend this type of reinforcement for columns as well as for shear columns? Could the authors give figures on estimates about the relative costs of using such a system?

Prestressed Concrete

The contribution by the Tbilisi group provides a change in pace not only because it concerns prestressed concrete but also because it injects a different perspective. The paper actually represents three papers in one, each with clearly defined conclusions of engineering significance.

In the first paper, the problem concerns the general dynamic characteristics of prestressed concrete beam loaded in one direction only. The specific question pertains to the influence of the level of prestress on stiffness and damping factor (as inferred from the logarithmic-decrement method). The authors find that, at any loading level, as the prestress is increased, the stiffness of the beam is higher and the damping factor is lower. These observations are consistent with the projections of the fundamental hysteresis relationships for prestressed concrete elements. The authors do not state whether they consider these observations positive or negative. The second topic in the paper refers to the relative behavior of bonded, unbonded and "grout-bonded" reinforcement. Not knowing how to interpret the authors' definition of energy absorption (is it the area under the load-deflection curve or is it the area within the loop) the writer is at a loss to comment on or even question why the "grout-bonded" system was found superior to the bonded system. The authors are also asked to comment on any experience they may have on the behavior of anchorages of unbonded beams under cyclic loading.

The third topic, very briefly reported, is quite interesting if only because of the full-scale model. The authors should comment on the consequences to the friction joint of any permanent deformations in the longitudinal beam reinforcement, especially if a gravity load is to be transferred from the beams to the column.

Trusses

The contribution by Shimazu and Fukuhara makes a convenient transition from consideration of elements to consideration of walls. This is yet another paper in this session that one would like to see in sixty rather than six pages.

If the measured initial stiffness of the brace frames were within 6% of that calculated from a linear model of the uncracked structure, the results would be acceptable without modification. Would the authors elaborate on why they recommend the model with pin joints. Is it simply a stratagem to approach the measured stiffness of the specimens or do observed behavioral characteristics of the system justify it?

Given the qualifications of the small scale environment, the author's tendency to deal with unconfined members (having an axial load less than 40% of member capacity and a nominal shear stress less than 10% of the compressive concrete strength) is of great practical interest. Would they consider the use of truss elements without lateral confinement in actual structures? It is also hoped that the authors will have the opportunity to detail the problems they have had with the joints of the test trusses.

Walls

It is instructive to reflect that the three-dimensional boxes or tubes described by Umemura, Aoyama, Ito and Hosokawa are more like flexural elements than some of the shear columns reported by Higashi et al. In that context, the detailed and painstaking study of the initial stiffness made by Umemura et al is of even greater significance. They find that the initial stiffness is better represented by the calculated "cracked-section" stiffness. Would they limit this conclusion to the particular specimens tested?

Joints

Despite the limited number of specimens, the data provided by Blakeley, Edmonds, Megget, and Priestley are quite significant in that they are derived from full-scale tests of beam-column joints. The writer finds the authors' opening statement quite poignant. They refer to "poor joint performance reported in many previous tests." Sifting of earthquake damage observations will bring up a surprising low proportion of structures which have had primary joint damage. On that basis, it has been somewhat difficult to make a strong case for the rather drastic implications of our laboratory experience with respect to the design of joints for actual structures.

The tests reported, with properly and carefully designed joints, indicate that these joints have not become the weak link in the system. A question of practical import that immediately arises is whether a smaller amount of transverse reinforcement would have also have done the job. The authors ought to continue their investigation to establish whether lower amounts of transverse reinforcement will be acceptable. For monolithic three-dimensional structures, this question is made even more critical by the information introduced by Abad, Meinheit and Jirsa to the effect that the presence of lateral beams is a more effective way of confining the joint than transverse reinforcement. The authors of both of these papers are asked whether it would be preferable to interpret that portion of the data pertaining to the behavior of the joint on the basis of deformations of the joint rather than filtering this information through the changing flexibilities of the beams or columns.

Steel-Reinforced-Concrete (SRC)

The paper by Aoyama, Umemura, and Minamino provides a perspective on joint behavior which may be construed to be diametrically oppostie to the view that the joint should be stronger than the framing elements. Responses of the beam-column complexes tested were such that while the individual beams and columns were barely stressed beyond their limits of proportional response, the overall force-displacement response of the complex was markedly nonlinear. (See ref. 3 for similar results.) The nonlinear component of the deflection was attributed, with convincing experimental evidence, to the response of the joint or the panel zones. The authors do not consider what they have observed a handicap. Would a similar phenomenon be acceptable in a reinforced concrete system?

Masonry

The reader will be delighted to discover that six of the seven papers on masonry are arranged in pairs so that instead of three papers packed into one as in other contributions to this session, various aspects of the same topic are covered in two related papers thus making reading more convenient.

Considering that many advances in technology have sprung from two seemingly contradictory schools of thought applied to the same topic, the writer is encouraged to observe such a controversy in testing techniques related to masonry.

The two papers principally from the San Diego campus of the University of California promise some order to the chaos in masonry. There appears to be a light at the end of the tunnel for an intelligible solution of the problem of earthquake response of masonry structures based on the classic strength-of-materials progression synthesizing the behavior of complex structures from simple but universal coupon tests through the use of explicit analytical techniques.

One's hopes of order from chaos are truncated by Fig. 8 of the paper by Omote-Mayes-Clough-Chen as well as by the general approach adopted in the paper.

No effort will be made here to paraphrase or classify the two approaches for fear of distorting their messages. To provide an easy target for the two groups, the writer submits that the two methods of approach are irreconcilable. If one of the approaches is correct, the other is not. It is possible that they could both be wrong and therefore compatible but they cannot both be right and compatible, unless of course they are the same though ensconced in different verbal foliage and the writer's interpretation is inoperative.

Steel

The contribution by Mizuhata, Gyoten, and Kitamura, is concerned with the behavior of shear walls composed of a reinforced concrete diaphragm bounded by steel elements and raises some relevant questions as to the useful meaning of "ductility factor" paired with the "damage factor" or the possibility of fracture at a given number of cycles. In this light, it would be very interesting for Ozaki and Ishiyama to consider whether their simple and attractive scheme of assessing required ductility could be projected to apply to structures described by this paper.

Mitani, Makino, and Matsui report the results of ninety column tests leading to expressions quantifying deformation and energy absorbing capacity. In contrast, Suzuki and Ono point out that under practical conditions it is difficult to prevent completely the lateral deformations of members and conclude that deformation capacity of steel members under repeated loading may be beyond the reach of a generalized quantitative expression.

A similar question arises when one reads the contribution by Singh and Goel and the comprehensive papers on the behavior of braced frames by Wakabayashi, Matsui, Mitani, Nakamura, Shibata, Yoshida, and Masuda. The papers related to experimental work lead to comprehensive rules for constructing the hysteresis response of frames. Would Singh and Goel find these rules too unwieldy for convenient use? Would they suggest a compromise or a set of criteria for modifying the rules.

To conclude, the writer offers his apologies to all contributors whom he has offended by reference or lack of it. His intent was to stimulate "cross-discussion" rather than dwell on the strengths of individual papers. Consequently, many important ideas in individual papers not touched on by others could not be included in the report. His hope is that, in addition to whatever else they wish to do, all contributors will focus their oral and written remarks on some of the issues raised so that the closing discussion may reflect the will of the profession, or at least of the many contributors, on these issues.

References

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