of course, be reduced by the attenuating action of the dissipators.)

M. J. N. PRIESTLEY AND M. J. STOCKWELL

The authors thank Mr. McKenzie for his contribution, but cannot subscribe to his conclusions. We believe that it is his arguments rather than our analyses which are misleading, for the following reasons.

Mr. McKenzie states that the isolation system chosen was incorrectly sized, and that either a more flexible bearing system, or a stronger damper, should have been provided.

In fact, the bearings were designed on the basis of reference A, to the recommended maximum compression stress of 5.5 MPa under HN loading[2]. Considerably higher compression stresses were allowed under HO loading[2]. From commercially available bearings, a shear stiffness per bearing in the range 640 - 4000 kN/m was possible, with the realistic range being 640 - 1500 kN/m. The value adopted, at 910 kN/m, is close to the most flexible bearing available. Adopting the most flexible commerically available bearing would have resulted in a 15% increase in the natural period, which cannot be expected to cause any fundamental change in characteristics.

The alternative suggested by Mr. McKenzie, of strengthening the dissipator (to give it a yield strength of 12.5% of the structural weight) is clearly impractical. With the 5% dampers as analysed, peak pier moments were higher than required for a ductile pier design. Utilising a stronger damper would have had two undesirable effects - stiffening the elastic system and decreasing the elastic natural period, which Mr. McKenzie acknowledges to be undesirable, and further increasing the peak seismic response accelerations of the superstructure. The consequence would have been to require expensive increases in foundation design, and increases in pier longitudinal reinforcement. The concept of needing to provide additional protection against seismic forces as a result of incorporating a base-isolation system appears to us to be philosophically unacceptable.

Mr. McKenzie states that reproportioning the b aring/damper stiffness to a natural period of 2.0 sec. will result in satisfactory performance under 1.5 x Bucharest (and 1.5 x El Centro), whereas large inelastic displacements will result from the monolithic design. Dealing with this last point first, we are at a loss to comprehend how these large inelastic displacements occur. Table 1 in the paper indicates that very moderate curvature ductility factors of about 4.2 and 3.0 can be expected from the monolithic design under 1.5 x El Centro and 1.5 x Bucharest respectively. Mr. McKenzie's comments on the response cf the isolated 2.0 sec. period design are suspect as they are based on the post-yield stiffness. The response spectra approach is based on the elastic stiffness, which will result in a much lower period than 2.0 sec. because of the damper elastic stiffness.

We regret that the Bucharest 2% damping response spectrum shown in Fig. 9 was a crude approximation based on an early analysis of the accelerogram. Since writing the paper the more accurate corrected spectra have become available from the U.S. Geological Survey at Menlo Park[(B)]. The response spectra from this source for 0-20% damping are shown in Fig. A for the Bucharest 1977 S - N component used in our analyses. It is significant that the improved performance claimed by Mr. McKenzie for a 2.0 sec. period structure is not apparent in Fig. A. Even for 10% equivalent viscous damping (which is rather more than could be expected for the damped system), peak response at 2.0 sec. is expected to be 0.45 g, using the approach adopted by Mr. McKenzie ignoring damper stiffness. The structure ductility would then be expected to be in the vicinity of 2.0. However, since approximately 90% of the structure yield displacement is provided by bearing displacement, while all the inelastic displacement will now be provided by pier plastic displacements, the curvature ductility corresponding to the structure ductility of 2.0 will be very high (about 20 in this case, depending on the estimated plastic hinge length). This behaviour is illustrated in Fig. B. Clearly under 1.5 x Bucharest, the curvature ductilities will be much greater (approximately doubled).

Finally, it is of interest to examine the likely superstructure displacements for a 2.0 sec. period design. From the U.S.G.S. source[(B)], at 10% damping the peak superstructure displacement under 1.0 x Bucharest 1977 S - N is estimated to be 450 mm. This is vastly in excess of the capabilities of existing bearings, yet Mr. McKenzie wishes us to believe that the system would respond elastically in satisfactory fashionunder 1.5 x Bucharest.

We attempted to indicate in our paper that base-isolation is not a panacea for seismic ills. Seismic response may well be improved by base-isolation, but equally, performance may be adversely affected if the earthquake characteristics include high energy in the long-period range.

REFERENCES

A. "Provisional Rules for Rubber Bearings in Highway Bridges". M.O.T. Memorandum 802, London.
B. "Analyses of Bucharest 3/4/77 Earthquake Accelerogram". Unpublished report. USGS Seismic Engineering Branch, Menlo Park.

"A CONSIDERATION OF P-DELTA EFFECTS IN DUCTILE REINFORCED CONCRETE FRAMES" - T. Paulay.

Bulletin of N.Z. National Society for Earthquake Engineering, Vol. 11, No. 3 September, 1979.

Mr. Andrews, who has seen a draft of Professor Paulay's paper, has suggested that his comment might be useful in promoting constructive comment of aspects of the draft concrete code if published now while comments are still being received by SANZ. Professor Paulay has agreed to this procedure but will reply to the comment, and to any other contributions, in a subsequent Bulletin.

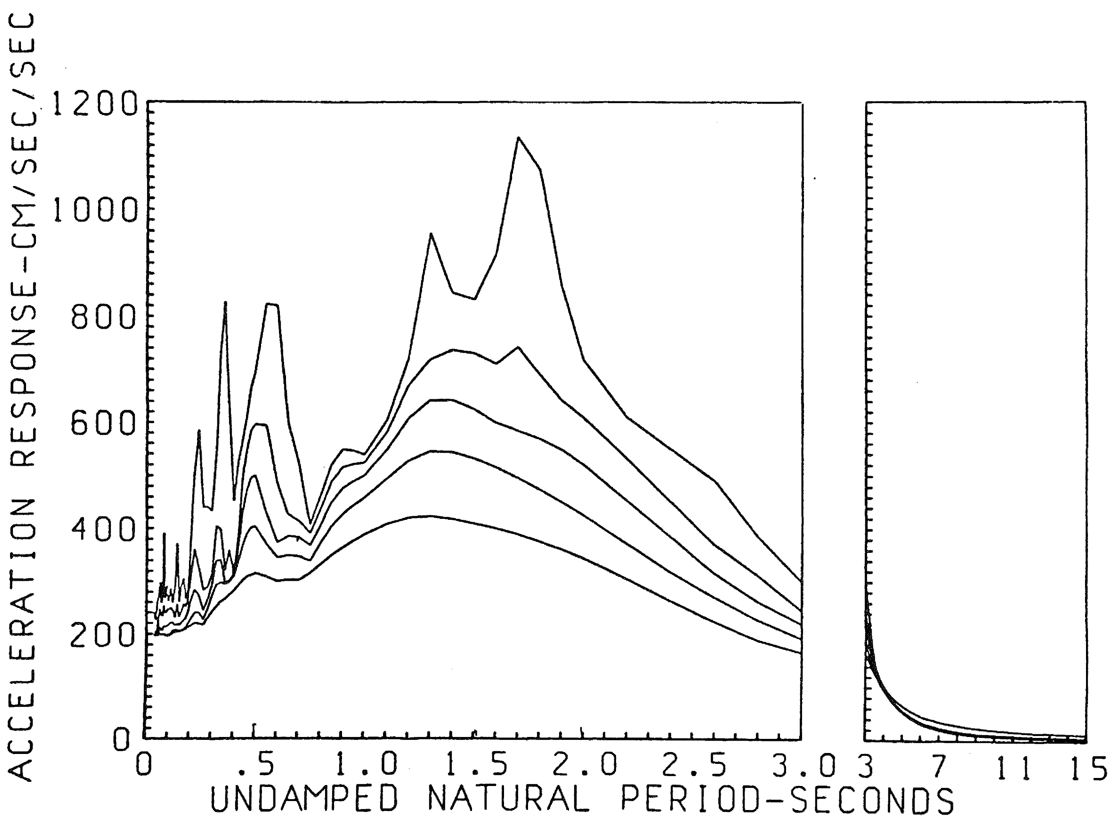

FIGURE A: ABSOLUTE ACCELERATION RESPONSE SPECTRUM BUCAREST (INCERC), 3/4/77. 2122 GMT, S-N 0.2.5.10.20 PERCENT CRITICAL DAMPING SEISMIC ENGINEERING BRANCH/USGS

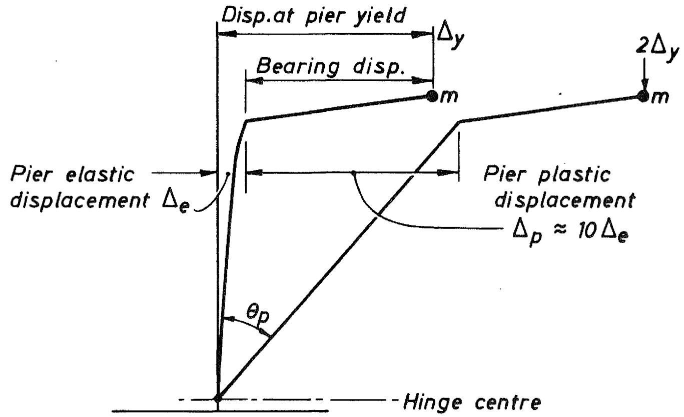

FIGURE B: PIER PLASTIC DISPLACEMENTS WITH ELASTOMERIC BEARINGS

A. L. ANDREWS*

Professor Paulay's thorough and helpful summary of recent P-delta literature is the basis of his proposal for a design procedure, one of two methods for unbraced frames which appear in the draft replacement for NZS 3101P. The other method, a copy of the moment augmentation procedure of ACI 318, deals only with elastic and creep deformation so is inappropriate and should be removed from the proposed standard.

Last year I suggested flexibility limitations for controlling P-delta so that its effects would be small enough to ignore. Simple controls for the purpose can be devised within the framework of our loadings code because our base shear spectra do not have coefficients reducing as period increases at low frequencies. My paper showed that suitable controls differed so little from those already applied for other purposes that they would be acceptable. Flexibility limitation has simplicity as a significant advantage over alternatives. If it has no faults not shared by alternatives then I believe it should be preferred.

Professor Paulay's principal objection, perhaps his sole objection, appears to be that he thinks it is too difficult to quantify drift. This, it seems to me, is equally an objection to his own proposal, as it must be to any other which seeks to account for or to limit effects generated by the interaction between gravity load and lateral deflection. I do not fully understand his reason for offering the generalisation, apparently bearing on this matter, that stiffening, to be effective, must be "radical". Presumably this is meant to suggest that stiffness is not such an important parameter that P-delta will vary approximately reciprocally with it as a first order appraisal ignoring the acceleration response increment which might be produced by a stiffness increment would indicate. A Powell and Row numerical analysis result cites is said to have shown that stiffening which might have been expected to reduce P-delta by about 60% effected a reduction of only 40%. But Professor Paulay's method features the very reciprocity that the result appears to have been quoted to discredit. Again an objection to my proposal has equal force when offered against his. Moreover, I think the single not especially anomalous result is scarcely sufficient evidence for the generalisation.

The amplification factor in Professor Paulay's proposal has first order accuracy as have some others of the same general type that have been devised. But these others are intended, in the main, for elastic structures and contemplate much smaller values of stability coefficient. It is reasonable to ignore higher order terms by substituting

$$1 + Q_r$$

for

$$1 + Q_r + Q_r^2 + Q_r^3 + \ldots\ldots \frac{1}{1 - Q_r}$$

when the relative error, which is seen to be Q_r^2, is small as it is for ranges of stability coefficient like that of McGregor and Hage, .0475 to .2. Professor Paulay's range has a lower limit of .15. When there is such a convenient summation available as there is for the amplification factor's geometric series, there is no computational advantage to be had from the approximation and no adequate excuse for using it in an inappropriate range of stability coefficients.

In establishing the stability coefficient, the paper accounts for the displacement factor set out in Table 10 of NZS 4203; but the accompanying warning that "for structures dissipating energy in a ductile flexural mode the separation requirement of this standard gives average damage protection to a class III building with 5% damping in seismic zone A at levels of motion up to one third El Centro N-S only" is unheeded. The warning is surely an acknowledgement by those who wrote the code that the provision it contains accounts very inadequately for drift and that that may or may not be tolerable for secondary damage control. A different view must be taken of unconservative drift provisions when the strength and stability of entire structures are at stake, especially as the warning suggests that a further multiplier for stability coefficient assessment might be as high as 3.

Some questions about precision of assessment will have to be resolved before the lower limit of Q_r that the paper advocates, 0.15, can be agreed upon. Compare, for example, the deliberately ignored 18% error that is involved in this with the difference between zone A and zone B earthquake simulating load, 17% of zone A load, and it becomes clear that either the limit Q_r will have to be revised downward or the basis of NZS 4203 will have to be reconsidered.

I find some difficulty in understanding why the factor 2 used to obtain storey drift was derived from the secant between ground and floor 4 in Figure 7. It seems that a factor of at least 3, obtained from detrusion of the bottom storey, ought to have been considered. I am also uneasy about the wisdom of getting so potent a factor from one study result, especially when curves 1 and 2 show that the framing of the subject building was proportioned to have flexural beam rather than the more usual shear beam characteristics. We should also not overlook the possible influence of foundation deformation, which, in the example of Figure 7, is seen from curves 1 and 2 to have been unusually small.

With no further explanations than are in the paper, it is hard to accept the author's invoking of more than the reliable strength as a means of offsetting P-delta. It seems to me that the structure cannot accommodate itself to a designer's thinking along these lines, being unable to differentiate between components of total force and moment all of which are, to some extent at least, self generated. Also, I think the equation of work represented by the triangles in Figure 6 is somewhat too facile an idea to offer much hope. As has been pointed out before (13) there are disturbing aspects to the energy dissipation characteristics of a

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P-delta affected system. For these reasons my interpretation has, in this comment, used $1 + Q_r$ rather than $1.1 + Q_r$ as the first order approximation to the magnifier, to be used with reliable rather than ideal strength in design. I think the author must give more developed support for his proposal than the paper contains to begin to make it convincing.

Finally, it seems to me that buildings at large are not of such a uniform nature that a generalisation to the effect that P-delta increments need not be considered for upper stories can possibly be justified.

ADDITIONAL REFERENCE

13. Discussion on Reference 4, Bull. N.Z.N. S.E.E., Vol. 11, No. 1, 1978.