DISCUSSION OF PAPERS PRESENTED AT THE NEW ZEALAND NATIONAL CONFERENCE ON EARTHQUAKE ENGINEERING HELD AT WELLINGTON ON 18th, 19th and 20th MAY 1971 AND PUBLISHED IN THE BULLETIN

STRONG MOTION DATA CENTRE: BUREAU OF MINERAL RESOURCES, CANBERRA - D. Denham and G. R. Small Vol. 4, No. 1. March 1971.

R. SHEPHERD (Christchurch)

Would Dr. Denham outline the procedure he uses to digitize the strong motion records. Is the technique a practical proposition for handling many records? Dr. Denham has referred to "difficulties" in setting up the MO2 instruments. I have heard similar comments from California. Would he elaborate on his experiences. How can the instrument be improved?

THE AUTHORS REPLY that the accelerograms are digitized at 0.02 second intervals on a scaling table. Digitized ordinates are obtained from a shaft position encoding disc which is fitted to the Y drive of the scaler. The output from the scaler is coupled to a paper tape punch. This system can handle only constant interval digitization but is fairly rapid and a typical output rate would be one number per second from the paper tape punch. Allowing for the time involved in preparing the records this is approximately equivalent to completely digitizing a 50 second accelerogram in one day. The two main difficulties we have experienced with the MO2 are as follows :

1. Loss of baselines. Without exception all the instruments installed in New Guinea lost their base trace due to the blue filter covering the base trace mirror becoming opaque. This fault which appears to be due to the high temperatures and humidities experienced in New Guinea needs immediate rectification.

2. Accelerometer block failures. We have had about a 20% failure of our MO2s due to block damage occurring during transit from New Zealand. This is higher than one would normally expect but the construction of the accelerometers is such that they cannot withstand large impacts unless the silicone damping oil is inserted.

The loss of the base line is a major drawback as this reduces the accuracy of the analysis of the accelerograms.

We are not impressed with the new timing trace introduced on the new instruments. This will effectively more than double the time spent in digitizing the record because it requires the accurate positioning of two cross wires instead of one ordinate wire.

J. P. HOLLINGS (Wellington)

If a line is drawn to just envelope the most northern epicentres on figure 1 would you agree that a large earthquake, say magnitude 8, is possible now as far north as this line.

The authors' state there is always a possibility that a magnitude 8 earthquake could occur anywhere in the New Guinea region. However, in some areas the probability of such a large event is very remote. The best way to evaluate the chance of a very large earthquake taking place is to determine the a and b terms, for the area of interest, in the magnitude/frequency of occurrence equation.

$$\text{Log } N = a - b M$$

However, although this will give some answer to the problem the errors in the large M range could be considerable due to the paucity of data in that region and the extrapolation necessary to calculate the return periods for a magnitude 8 event.

STRONG-MOTION EARTHQUAKE RECORDING IN NEW ZEALAND - R. I. Skinner, W. R. Stephenson and R. T. Hefford. Vol. 4, No. 1, March 1971.

G. H. F. McKENZIE (Wellington)

I would like to hear opinions from other engineers on the number of instruments required to record the behaviour of a building adequately. The usual practice is to put 3 instruments in a building, one at the base, one at the top and one at an intermediate height. I am concerned as to whether this is sufficient for separating out the magnitudes and shapes of all the significant higher modes of vibration. For example, if the intermediate instrument is placed at the appropriate height for the maximum displacement of second mode, it will be a level of small displacement for the 3rd mode shape. Again, if a Fourier analysis procedure is used to separate a number of different frequencies from the record of movement of only two points, one is left with a fairly insensitive procedure. In important buildings, more than one intermediate height appears to require instrumentation.

THE AUTHORS REPLY that three strong-motion accelerographs is the minimum number required for effective earthquake recording. Very large buildings and those with certain unusual features would be much more effectively recorded by more instruments. In five important buildings the Engineering Seismology Section operates networks of from 4 to 6 accelerographs. In view of the need to cover a wide range of building types, ground conditions, and geographical locations, some sacrifice in effectiveness of recording may be required in order to cover a sufficient number of buildings.

C. M. STRACHAN (Wellington)

With regard to Fig. 3(a), I am interested in the hole showing 400 feet of clay in the area of the Vogel Building. Having been concerned with the borehole to this depth in this location, the soil type is a mixture of silts, clay and gravels up to about 1" diameter. I believe similar soils exist throughout the area of marine sediments in Wellington. Therefore, the other holes which show sand and gravel I believe would be similar to that at the Vogel Building. Has any thought been given to setting up a series of stations in the East Coast area, such as Napier or Gisborne where earthquake frequency is high. Napier would be ideal with high buildings on Hospital Hill and marine sediments which I believe could be subject to liquefaction in the low areas of the town. Gisborne also has a substantial depth of marine sediments and it appeared that a substantial build up in acceleration occurred in the Gisborne earthquake of 1965 due to soft marine sediments.

THE AUTHORS REPLY that the soil data is certainly over-simplified. However, we have given the predominant materials, those which would be expected to dominate the dynamic behaviour of the soil. We have accelerographs on alluvial ground at Napier (3-component) and Gisborne (peak-reading). We are anxious to increase the number of accelerographs on rock sites. Napier and Gisborne would be suitable areas for two of these rock site instruments. However, areas of major construction must be given priority.

SEISMIC MICROZONING IN NEW ZEALAND - W. R. Stephenson. Vol. 4, No. 1. March 1971.

R. J. P. GARDEN (Dunedin)

To what extent is there a microzoning problem in those "hard rock" areas, where we have, as in parts of this country, arrays of rock layers resembling a rainbow cake - or can one assume a substantially homogeneous rock base, in comparison with the sediments.

THE AUTHOR STATES that, to date, the most frequent observations of microzone effects have been of ground damage, relative displacement, and inertia attack increases on unconsolidated sediments, and of ground damage associated with steep slopes. However, there can be little doubt that "layer cake" types of geology will modify waves to a smaller extent. Variations from rock to rock will cloud the issues, but unconsolidated sediments and steep ground will dominate considerations for some time. Sediment effects are so gross that despite variations in the hard rock we can assume a homogeneous rock base.

G. L. EVANS (Christchurch)

This paper is a clear postulation of the possibilities of inertia attack study. It raises more questions that it answers, but it sets out the problem areas clearly. With regard to microzoning I consider it is better to be able to predict bedrock to surface motion regardless of building on top and then any type of structure can be analysed with what could be the right sort of ground motion. Have any records become available from which wavelength can be found or inferred - this could give a clue to out of phase motions of long structures, such as bridges. I suspect that this was one of the influences causing damage to bridges in the Inangahua earthquake. Para. 11 and Fig. 2 is I think too simplified to be really true - there have been lots of reports (mainly by non technical people) of "sightings" of earthquakes as travelling waves. These have even been seen from the air!

THE AUTHOR STATES that recent measurements in strong earthquakes have shown significant accelerations at 7 hertz. Shear wave velocities in sediments may fall to 30 metres per second. We can thus expect wavelengths as low as 4 metres in soil. However, because of the high frequency, the associated displacements will be small. In addition, in a situation where waves are refracted from rock to soil, the effective surface wavelength will be that in the bedrock. But there is little doubt that boundary conditions of a soil mass can give rise to large relative displacements. Perhaps this is where the 4 metres becomes significant. Across an actual interface the relative displacement can be a nearly discontinuous function.

With regard to the last point; in a purely refractive situation with horizontal stratification, p waves, but not s, can give vertical surface displacements. As soon as boundaries are included, even these generalisations fail. Paragraph 11 and Fig. 2 were intended to relate to the s phase, and in conditions where vertical changes in boundaries are not rapid. In such conditions vertical ground accelerations are small.

C. M. STRACHAN (Wellington)

At the 4th World Conference on Earthquake Engineering in Santiago, Chile, towards the end of the Conference one contributor said "I know microtremors has become a dirty word at this Conference ...". We should bear this in mind and I believe observations of earthquake damage is of far more practical use than working on theoretical microtremors. It is a little unfortunate that Mr. Stephenson has dealt more fully with variation of inertia attack because it could give more emphasis than his first two types of failure, ground failure, and relative displacement attack. I believe that latter two to be more important that the former. Even if there is a build up in acceleration due to soft sediments, good design incorporates allowance for high accelerations. Good design cannot cope with liquefaction or slope failure.

THE AUTHOR STATED that Mr. Strachan's feelings about the three microzone mechanisms seem to concur with his own. Inertia attack causes larger accelerations, which can be designed against. Relative displacement and ground damage mechanisms generate enormous forces which would be difficult to design against even if we could predict them. Right now geologists can identify places susceptible to ground damage and relative displacement attacks, and town planners could heed their warnings. Inertia attack is at a different stage. Once inertia attack variations can be predicted design can proceed on the usual rational basis. Design loads can be increased for poor, and decreased for good soils. In brief, the emphasis on inertia attack reflects the fact that understanding it will lead quickly to rationally modified design criteria. By contrast, studies of ground damage effects should lead to town planning criteria.

EFFECT OF SOIL PROPERTIES ON EARTHQUAKE RESPONSE

- I. M. Parton and R. W. Melville Smith.
Vol. 4, No. 1. March 1971.

P. SWAN (Wellington)

You referred to a computer programme for analysing dynamic soil properties. What range of parameters can be handled and what form does the analysis take?

THE AUTHORS STATE that the computer program developed at the University of Auckland for analysing the dynamic response of soils is based on the "shear deformation" theory, in which soil layers are considered to be represented by series of lumped masses interconnected by shear springs that resist lateral deformations. Parameters required for this program are the dynamic shear modulus and the equivalent viscous damping factor for each soil layer. Both these parameters are dependent on the strain amplitude, which may vary between 0.005% and 5% during analysis.

G. L. EVANS (Christchurch)

The implications in the smaller values found for G_d at small strains as compared with Seed's results, are note-worthy as this implies that the determination of G_d at small strains could be more readily extrapolated. Is the straight line relationship in fig. 11 likely to be general? If so, this implies that two values found for G_d and strain could identify a line.

If the simple formula $T = \frac{4H}{Vs}$ is used it gives a layer period in 90 ft of 1.2 and in 60 ft of 0.8 secs. and this compares well with the C_2 earthquake Fig. 19.

THE AUTHORS STATE that the straight line relationships in Fig. 11 are only an approximation of the relevant curves in Fig. 9 and were drawn in this manner merely to aid computer analysis. However, test results indicate that below shear strains of about 0.1%, log G_d versus long strain relationships may become linear, and so two values found in this region would identify a line.

If the formula $T = \frac{4H}{Vs}$ is used a specific value of Vs must be assumed to apply throughout the deposit regardless of strains developed. The computer analyses show that the shear wave velocities vary considerably throughout the deposit during earthquake excitation.

C. M. STRACHAN (Wellington)

Could the authors explain why they think their results on the dynamic properties of soils vary from those of Seed and Idriss.

THE AUTHORS STATE that Seed and Idriss' dynamic soil relationships were obtained by averaging the results of tests performed on many types of soils by many investigators. These relationships are only intended for use as approximate guides to dynamic soil properties and thus can be expected to differ from specific test results as was the case here.

R. SHEPHERD (Christchurch)

On the first slide shown reference is made to "Ductility Factor" of 4. As I suspect that this is actually a base shear reduction factor rather than a ductility factor as usually defined (i.e. yield distortion ÷ elastic distortion). Would the authors clarify what is meant by ductility in the context of their paper.

THE AUTHORS STATE that the reduction factor mentioned by Mr. Shepherd is in fact a base shear reduction factor which is termed ductility factor by the N.Z. Code and the associated commentary.

H. C. HITCHCOCK (Wellington)

The authors give their results in the form of velocity response spectra because there is some evidence of better corelation between velocity response and damage incurred. Would the authors agree that this applies only to structures which can accept damage beyond yield point progressively without any well defined failure point? and that for structures or equipment which are brittle in nature the acceleration response is most meaningful? Would they agree that it is difficult to grasp the significance of the velocity response spectrum? Would the authors care to look at Fig. 16 where the approximate acceleration response has been plotted (by multiplying velocity response by $\frac{2\pi}{T}$) and consider whether the two sets of curves convey the same impression? and Fig. 14 where "Tamaki Bridge WN No. 3" surface acceleration is plotted with El Centro 40 NS. Would the authors please comment on the very high accelerations apparently associated with the white noise input at short periods also, the appearance of the acceleration peak at .2 sec. in the surface response and the input response, in contrast with Shepherd and Travers finding that the peakin El Centro at .2 sec. seemed to be a property of the top soil layer.

THE AUTHORS STATE that empirical studies have shown that, generally, the maximum particle velocity is a better indication of damage potential to structures rather than maximum displacement or acceleration, and for this reason the computed surface accelerations have been reduced to velocity response spectra. Furthermore the response spectrum technique has been used as a tool for transforming the acceleration record and isolating the required information rather than as an aid to studying the effect of the surface motion on different types of structures. In analysing the results the authors were concerned mainly with detecting changes in the input motion due to the selective filtering action of the soil layers. Whether the velocity or acceleration spectrum is used, approximately the same nett results will be obtained. Some authors have presented such results in terms of 'amplification spectra' but due to the oscillatory nature of the component spectra the accurate determination of such a plot may be difficult. Shepherd and Travers detected a resonance-type peak in the El Centro surface motion response spectrum, whereas the peak referred to in the discussion has approximately the same spectral value for both bedrock and surface motion, i.e. it is not a resonance-type peak. The high accelerations produced by the Gaussian sequences at low period values are presumed to be due to t fact that the ordinates are not smoothed are highly oscillatory in nature.

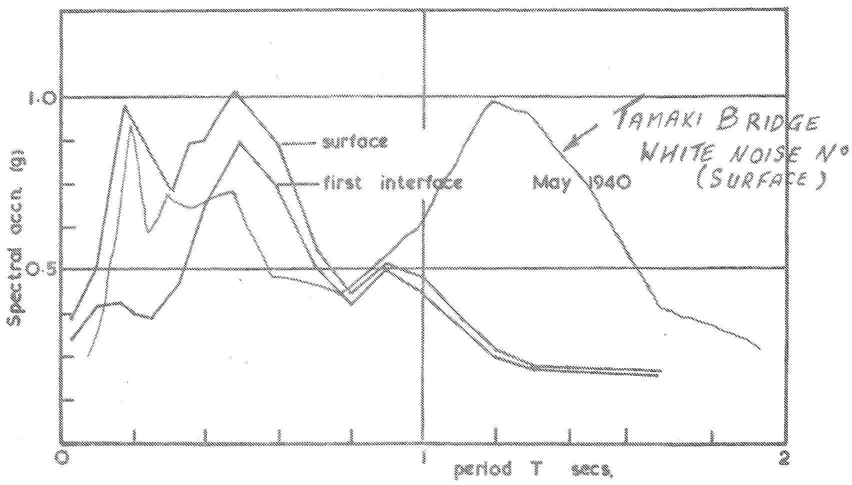


FIG 14 El Centro N.-S. Response Spectra
after Shepherd & Travers (1968)

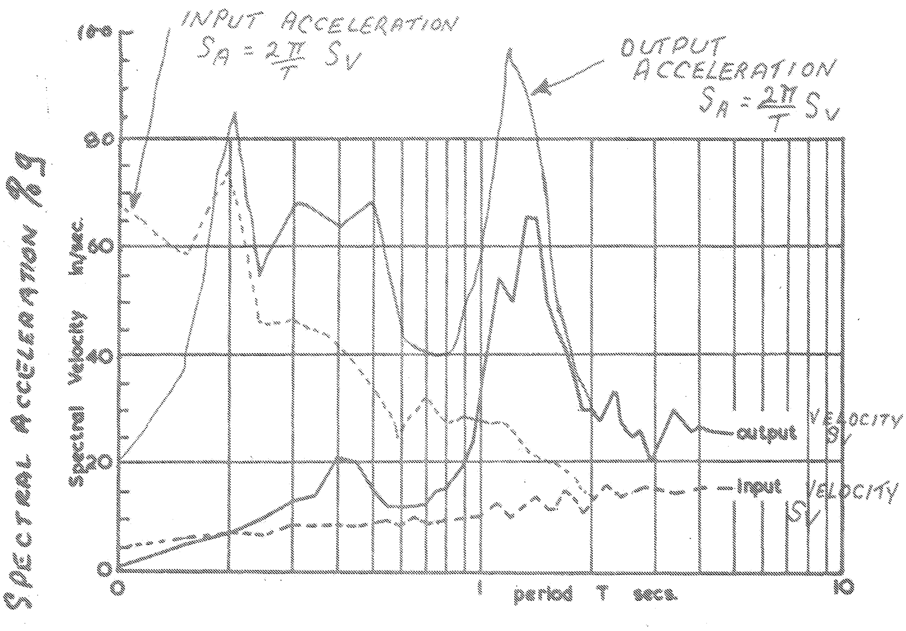


FIG. 17 Response Spectrum of Surface Motion
White Noise No. 4 input 5% damping

EFFECT OF SOIL PROPERTIES ON EARTHQUAKE RESPONSE Cont.

L. G. CORMACK (Wellington)

The paper discusses testing on soil samples conducted as part of the design studies for the new Pakuranga Bridge, and the way in which the dynamic shear moduli obtained were used in an "equivalent linear elastic" direct integration analysis of the soil deposit to determine soil displacements and modified surface accelerations at intervals throughout the earthquake. Whereas the paper is largely academic in content it may be of interest to mention briefly the part the test results played in the structural concept chosen. A rigorous analysis of such a combined soil-pile-superstructure system would ideally involve an approach of the type discussed by Penzien in references (1) and (2). Initial studies showed however that it was possible to simplify the problem by neglecting interaction between the soil and the piles. This assumption is conservative but not excessively so (reference 2). The results of the authors' analysis were therefore used to provide the following three significant items of design information.

1. The maximum deformations to which the piles would be subjected.
2. The total expected movement of the soil layers.
3. The modified acceleration response spectrum at the surface of the deposit.

From these results, the foundations, abutments and superstructure were designed accordingly. It was fully recognized throughout that the El Centro N-S earthquake was not particularly suitable as an input base rock signal, but at the time of the analysis no other earthquake was available. The purpose of the dynamic testing was principally to determine the scale of the effect of a large earthquake on the soil layers and the bridge so that a sound engineering concept could be chosen. It is felt that the El Centro earthquake was adequate for this purpose.

References

(1) J. PENZIEN: "Soil Pile Interaction". Earthquake Engineering, Prentice Hall 1970.

(2) PARMELEE, PENZIEN, SCHEFFEY, SEED AND THIERS: "Seismic Effects on Structures supported on Piles extending through deep sensitive clays". Institute of Engineering Research. University of California (Berkely), August 1964.

THE AUTHORS STATE that they find Mr. Cormack's comments on the design procedure most interesting and hope that a fuller account of this will be published in the near future. At the time of the authors' analysis a full range of input motions (as listed in the text) were available, but these and the resulting surface motions seemed of little interest to the designers.

A FIELD TEST FOR DYNAMIC SOIL PROPERTIES - G. L. Evans. Vol. 4, No. 2, April 1971.

W. R. STEPHENSON (Lower Hutt)

These types of measurement are important, and much more should be done, but under circumstances where predictions relating to strong motion records may be made. Because of the infrequent occurence of strong earthquakes, there is a great temptation to the profession to fill in gaps in facts by performing incomplete experiments, or by guessing. In the present case, a complete experiment would, after determining shear velocities down to bedrock, measure a strong earthquake on bedrock and soil.

THE AUTHOR STATES there is no doubt that prediction relating to strong motion is the ultimate aim. Any dynamic predictions based on the small scale tests outlined in this paper should if at all possible be checked out with strong motion records. This means that some small scale dynamic predictions should be made in areas where strong motion records have already been obtained on bedrock and overlying soil, (if any exist) or a suitable site should be found where strong motion recorders can be established and the small scale dynamic tests done as well. In addition to this sort of comparison and check on prediction accuracy of the "total system", the field dynamic test does provide a method of finding the right order of magnitude of the shear modulus and this factor can be used in a variety of mathematical processes including finite element analysis of subsurface material, soil structure interaction and dynamic soil pressure problems.

P. W. TAYLOR (Auckland)

1. Has the author considered the possibility that, in sandy soils, elastic moduli may be dependent on overburden pressure, and hence may increase with depth? With a linear increase in modulus, curved (approximately circular) wave paths are possible.

2. Concerning the relationship between Mr. Evans' paper, and that presented by Messrs. Melville-Smith and Parton, who found the shear modulus to be strain dependent, the fact that definite values of shear and compression wave velocities can be measured suggests that the amplitudes of strain in field velocity measurements are in the region (say below 0.01%) in which the relationship is flat. That is, at these amplitudes, the modulus is independent of strain. It is in the region 0.1 to 1% where strong amplitude-dependence is found, and these amplitudes are found only in fairly large earthquakes. Would the author please comment on this observation.

THE AUTHOR STATES :-

1. Regarding the possible change in elastic moduli with pressure increase, it is important that the test method be further developed to carry out tests down bore holes of relatively small size so that variations in the velocities and densities can be found in depth. There are practical difficulties with some materials in getting good density samples. One of the objectives in digging the large holes in the series of tests outlined in the paper was to find what the variation with depth might be. In this particular case there

are increases in density and quite considerable changes in shear modulus values, but it appears that these are primarily related to the stratification of the loess. The possibility of curved wave paths has not so far been considered.

2. For small strains and including some range of values up to, at present, an unknown limit the shear modulus does appear to be independent of the strain (i.e. purely elastic behaviour). This is shown as Mr. Taylor says by the relatively constant velocities found, not only from these small impact tests but also from larger explosion type tests used in seismological exploration. However, there does seem to be a small variation in some velocity values but the real cause of this has not been found yet. Before practical use can be made of the shear modulus results from velocity tests some relationship must be found between this relatively "constant" value of the shear modulus and the strain dependant values. Velocity tests rapidly provide an upper limit value for the shear modulus and the laboratory dynamic test gives the lower value range for larger strains. The straight line relationship on the logarithmic plot in Melville-Smith and Parton's paper gives an indication of the sort of relationship between strain and shear modulus values that might be put to practical use in extrapolating values of shear modulus from the velocity tests.

THE ECONOMICS OF EARTHQUAKE ENGINEERING - J. P. Hollings. Vol. 4, No. 2. April 1971.

C. T. J. BUBB (Melbourne, Australia)

1. Can the author derive a maximum or optimum size for his hypothetical insurance fund?
2. Have any computer based block by block damage studies been done for New Zealand cities?

THE AUTHOR STATES that in considering the maximum size of the hypothetical fund we may ignore the value of existing construction and deal only with new works, since the paper is concerned only with new work. If \$500 million is assumed as New Zealand's yearly investment, with an overall loss ratio for New Zealand of 5%, the fund would require a yearly contribution of 5% x 1½% x \$500m. (para 4.0), i.e. \$375,000. In 50 years, assuming no major disasters, this would grow at 6% compound interest to \$109 million. As is indicated by the dotted line B.F.G.H. etc., in Figure 1 the growth of the fund would then become excessive and the contributing rate could be reduced without risk. In short, one cannot talk of a maximum fund size but only of a maximum contributing rate. This was estimated (para 4.0) at 1.5% x (loss ratio) in the paper using the conservative crude methods described therein. The computer studies suggested in the second question would presumably give a factual estimate of loss ratio for existing construction in future earthquakes and could then be used to estimate the contributions needed to maintain a fund to protect existing construction. No such studies have been attempted in New Zealand so far to my knowledge.

O. A. GLOGAU (Wellington)

One of the merits of Mr. Hollings interesting paper is that it shows the need for more factual information on some of the data used. Take the example of a High Rise Building. (7.2). I believe that Freeman's typical pre 1932 R.C. Building has far greater reserve energy capacity than a modern multi-storey building not designed or detailed for earthquake resistance and that the collapse ratio in a major earthquake for that group could be very high, depending on the response characteristics of that group, and the parameters of the earthquake. To show how sensitive some of the results are, an increase in the collapse ratio from 1% to 10% changes the author's figures from 4½% to 60% (interpolated), i.e. the sum to be invested in the disaster fund. The sum that can be spent on damage protection is also very sensitive (for that example) to inaccuracies in the assumptions. I therefore believe that the author is far too pessimistic in what could be spent on engineering protection - I am glad to see notwithstanding that in his own practice the author provides some of the best engineering protection in this country and at moderate cost. A further point - too often money not spent on engineering protection does not find its way into a disaster fund but - notwithstanding the need for aesthetical considerations - into excessively elaborate and hazardous building claddings.

THE AUTHOR STATES that he agrees in principle with Mr. Glogau's remarks (mainly that modern buildings not designed for earthquake could have a higher collapse ratio than Freeman's examples) but not with the actual figure he suggests of a 10% collapse ratio. The 1% of the paper might be low, but 10% is certainly high. In support of this, in the '67 Caracas earthquake, four modern buildings collapsed out of a total of more than 1000 over 10 storeys high and more than 6000 over 4 storeys high. Thus the collapse ratio is only a fraction of 1% for this example where very low lateral force factors were used.

R. I. SKINNER (Lower Hutt)

The general approach to assess economic and human loss is very promising. A valuable feature of the approach is that it highlights the areas of greatest uncertainty in design information. A particular point is that earthquake conditions which cause collapse of modern buildings often bear little relationship to conditions which destroyed earlier forms of construction. Recent earthquakes in Manila and Caracas cities killed some 700 people by the collapse of large, recently constructed buildings. However, there were no casualties among the many people housed in the old buildings. Hence observations by Freeman bear little relationship to the casualties to be expected from modern buildings.

THE AUTHOR STATES that Mr. Skinner is making the same point as Mr. Glogau, and while I agree that Freeman's figures can be used only with judgement and great discretion, I think it is a mistake to discard his work altogether. The reasons for this opinion are that his work was based on a rational evaluation of actual damage to structures (it is admitted modern multistorey structures are different from those in San Francisco in 1906) and we have absolutely no substitute for Freeman's studies. We can only make spot checks of loss ratios from recent earthquakes as checks on Freeman - frequently these turn out to be surprisingly close to Freeman's figures, as in the examples quoted in answer to Mr. Glogau. Until the detailed studies suggested in the contributions of Messrs. Bubb and Kenna are carried out to provide realistic estimates of that vital parameter "loss ratio", we have to guess with such assistance from Freeman and others as may be available.

L. F. KENNA (Wellington)

Congratulations to Mr. Hollings for directing attention to the costs of earthquake resistant design and construction. The amount of capital expenditure being incurred to protect against very infrequent earthquake hazards is receiving increasing attention: For example Pravda* July 28, 1970 reports that "in some areas it would be better to improve the current system of state earthquake insurance". A knowledge of the additional costs of aseismic provisions for various types of construction and for specified levels of protection would assist future deliberation over the choice of a prudent balance between structural protection and insurance protection for various types of occupancy. Can Mr. Hollings provide actual cost data of such additional aseismic provisions for past work, and also compile and provide, to the Society of BRA-NZ, such costs for future work?

* Associated text of interest reads:

"Economist Questions Anti-Seismic Construction

GOSSTROY's new anti-seismic construction norms were the object of criticism by Soviet scientists recently. Academician L KANTOROVICH of Akademgorodok and five members of the Institute of Earth Physics complained in Pravda July 28 that the norms are based not on the frequency of earthquakes in a given region but on a map giving only zones of severity according to the Richter scale. The criticism points out that the frequency of earthquakes of an intensity of 8 in a given area occur only every 275 to 3500 years. According to the article, a hundred million roubles have already been invested in anti-seismic construction. The Institute of Earth Physics over the last two years has developed a computerized system of determings the frequency and economic loss due to earthquakes based on past records. This system would seem to provide a sounder basis for determining where to use the more expensive construction. In some areas, however, it would be better to improve the current system of state earthquake insurance, the authors suggest."

THE AUTHOR STATES that a partial answer to Mr. Kenna's question will be found in Beca and Tork's study (Reference 1 of the paper). This work brought up to date to suit current design standards would seem to be as much as Mr. Kenna can hope for, since in all design offices there is always too little time to stop, re-examine the design for the effects of varying levels of seismic resistance, and then to prepare and maintain records for comparible purposes. This is not to say that Mr. Kenna's suggestion is not possible and it would certainly be of value provided that funds could be found to meet the very considerable cost.

B. W. BUCHANAN (Wanganui)

The author has performed a valuable service for engineers in demonstrating that sums expended on aseismic design may, in many cases, be more than can be economically justified. Some assumptions, mostly of a very conservative nature, have been made in order to arrive at figures for discussion. However, there appear to be some areas where a more critical examination would lead to significant adjustments to the calculations presented.

1. In the example of para 2.0 it is shown that earthquake insurance can be obtained for 6 or 7% of the cost of a structure. While this insurance will indeed give complete financial protection it cannot be offset against normal earthquake engineering design as it is implicit in the argument that the owner has a normal design in order not to be liable for lives lost. If the owner is to obtain financial protection by insurance alone (i.e. if he is to be permitted to "opt out" of code requirements) he must provide against claims for lives lost. Hence it would appear that his premiums will rise above the figures quoted.

2. In para 4.0 the author postulates a "disaster earthquake shaking every structure in New Zealand on average once every 50 years. If this earthquake has an intensity at its epicentre of say MM9, it is generally accepted that in the same period there will also be about 10 earthquakes of intensity MM8. Thus some earthquake engineering, perhaps 50% of "disaster" earthquake requirements must be included in all projects if the owner is not to be faced with damage not every 50, but every 5 years. That this does not happen to even the worst brick buildings is evidence that the author's "disaster earthquake does not recur with the regularity stated, and is therefore an upper bound. However, reassessment of some of the examples given, with provision for damage by smaller earthquakes at closer intervals, would be appreciated.

3. In the third and fourth paragraphs of para 4.0, it is shown that $1.00 p.a. compounded gives $290 after 50 years, but that $18.4 p.a. is required to repay a borrowing of $290 over 50 years. However, for a strict comparison it would seem that the "borrower" could expect to have the benefit of no commitment whatever for the first 50 years, i.e. until the first earthquake occurs. Should not line AA'C of figure 1 of the paper be replaced by a straight line, AC?

   Does the author agree that if money is worth more to the owner than the 6% interest available he would be advised to borrow when necessary? (In practice few owners would be able to do without insurance as without it they would be unlikely to secure mortgage finance).

4. In para 7.6 the author points out that full earthquake engineering of city water supply systems is essential. Would he agree that replacement of non-seismic fire stations is a matter of equal or greater importance, on the same grounds?

THE AUTHOR STATES :-

1. Yes, in that example it is agreed the owner must have code compliance in order to avoid liability for lives lost. So far the private investor we are talking about whether he should spend additional sums over and above those needed for bylaw compliance, or whether he should insure. (This is mentioned about halfway down para 2.) With this proviso the premiums quoted are appropriate.

2. A disaster earthquake every 50 years was taken for conservatively and with simplicity assessing the risk. A more refined analysis would certainly consider smaller, more closely spaced checking. The concepts remain the same.

3. Yes, to be strictly comparable A'C should be replaced by AC but the high cost of borrowing still means the disaster fund concept is of much lower cost when averaged over a period.

   If a fund is the lowest cost way of disposing of the risk, then investment in a fund is the correct action, irrespective of earning rates on capital.

4. Certainly there should be a programme for replacement or strengthening of dangerous fire stations.

   Mr. Buchanan has pointed out correctly that in 4.0 fourth paragraph 5th line Alt. (ii) should be Alt. (i), and in the 10th line Alt. (i) should be Alt. (ii).

AUTHOR'S SUMMARY:

Though the many approximations made in the paper have been challenged in the discussion, it appears the basic concepts have not. Thus one might conclude that with the assistance of further research (mainly into appropriate loss ratio values) the methods set out in the paper could be used to help form a better balance between levels of code loading and insurance investment.

At the request of the author (Mr. J. P. Hollings) Dr. R. R. Allan has contributed the following note on the effects of inflation on the calculations set out in the paper.

EFFECT OF INFLATION ON A DISASTER FUND

A. Intuitive Evaluation

Inflation is the progressive devaluation of money through the reduction of its purchasing power due to rising prices. Because of inflation, the purchase of an item may require a greater number of dollars today then the same item purchased two years ago. This is not to infer that, today, the item costs more in real terms, since the manhours of work which earn the sum required for purchase may be no different from the manhours required two years ago. Thus a numerical change in dollars required for purchase may not represent an alteration in economic or financial 'value'. Money only establishes equivalences between different items (for example, a pair of shoes and board and lodgings for a week), and real changes in value occur only when these equivalences are altered.

The rate at which inflation occurs is different for different people. Increasing the price of meat has no effect on the vegetarian, and imposition of additional sales tax on luxury items has more impact on the wealthy. To the private individual, to the industrialist, to the investor; the rate of inflation will depend on his source of money and how it is utilised. Various indices are prepared to reflect changes in costs, the most widely known being the Consumers' Price Index.

How, then, should inflation be taken into account in an economic study? F.P.S. Lu, in his book Economic Decision Making for Engineers and Managers, states that :

"If all items of expenditure and income in an economy study are inflating at the same rate, inflation is irrelevant. The reason is that, before the alternatives can be compared, all the cash flows have to be converted to real costs, which must be based on purchasing power of money at a particular time, say, the present, and which are therefore independent of inflation". (p.141)

Inflation cannot be ignored if all items of expenditure and income are not subject to inflation. For example, an investment in rental property showing a nett return of 8 per cent on capital, is more profitable than investment in mortgages at 8 percent. The true value of the property in effect, remains static, whereas the amount lent on mortgage diminishes in value due to inflation. Thus, if inflation is assumed to be 3 percent annually, the effective rate of return on mortgages is $(1.08/1.03 - 1) \times 100$ percent, or approximately $8 - 3 = 5$ percent.

Provided adjustments are made to the rates of return from investments which are not self-compensating, inflation-wise, inflation need not be a consideration in a preliminary formulation of the disaster fund. A detailed analysis of such a fund should take into account any differences in rates of inflation but, in the first instance, we may assume that the rate of inflation is fixed for all sectors as a first approximation.

The foregoing comments are quantified in the following mathematical formulation of the effects of inflation.

B. Mathematical Evaluation

Define:

X : value at beginning of year 1, that is, the construction cost, which is also the replacement value at that time.

d : annual rate of depreciation in insured value; $d = 0$ if the insured value is the replacement value. Insured value at beginning of nth year is therefore $(1 - d)^{n-1}X$.

i : annual rate of inflation.

c : annual rate of contribution; e.g. cP contributed at start year 1.

r : annual rate of return on investments by fund, including capital gain, if any.

Let
$D = 1 - d$
$I = 1 + i$
$R = 1 + r$

We may set out the insured value, contribution, and current value of:

Beginning Year	Insured Value	Contribution	Current value of cumulative contributions
1	X	cX	cX
2	IDX	$cIDX$	$cXR + cIDX$
3	I^2D^2X	cI^2D^2X	$cXR^2 + cIDXR + cI^2D^2X$
n	$I^{n-1}D^{n-1}X$	$cI^{n-1}D^{n-1}X$	$cX(R^{n-1} + IDR^{n-2} + \ldots + I^{n-1}D^{n-1})$ $= cX\dfrac{R^n - I^nD^n}{R - ID}$

Suppose $R = \overline{R}I$

Then at the beginning of year n:

current value of cumulative contributions

$$= cX\,\frac{\overline{R}^nI^n - D^nI^n}{\overline{R}I - DI}$$

$$= I^{n-1}\;cX\,\frac{\overline{R}^n - D^n}{\overline{R} - D} \qquad \ldots\ldots (1)$$

whereas

insured value

$$= I^{n-1}D^{n-1}X \qquad \ldots\ldots (2)$$

If, in real terms, inflation had an effect on financing the fund, the time which elapses before the fund equals the insured value will vary with varying rates of inflation. But, equating (1) and (2) the factor I^{n-1} cancels out, and I has no bearing on the relationship between current value of contributions and the current insured value. That is, inflation has no real effect, apart from that which it may have on $\overline{R}$.

Interpretation of $\overline{R}$:

The return from an investment is often reduced to its 'net present worth' by making allowance for inflation, and $\overline{R}$ is really the effective return on the investment which is derived after such an adjustment.

Although profit is profit regardless of whether it is due to cash earnings or capital gain, description of $\overline{R}$ is made clearer by taking the cases with and without capital gain separately.

(a) With capital gain:

Suppose sum S is invested and is showing a cash rate of return of g annually. Then the value of the investment after one year is:

$$RS = S + iS + gS$$

$$= IS + gS$$

$$\frac{R}{I} = 1 + \frac{g}{I}$$

or $\overline{r} = \dfrac{g}{1 + i}$

Hence the apparent cash rate of return, r, must be discounted to allow for inflation, i, e.g., apparent rate of return of 10% under 5% inflationary trend is effectively:

$$\frac{10}{1.05}\,\% = 9\tfrac{1}{2}\%$$

when capital appreciation is present.

(b) Without capital gain:

Similarly,

$$RS = S + gS$$

$$= IS + (g-i)S$$

whence

$$\overline{r} = \frac{g - i}{1 + i}$$

Hence the apparent cash rate of return must again be discounted, after a deduction has been made to allow for depreciation in value of the principal amount invested. e.g., apparent rate of return of 10% under 5% inflationary trend is effectively:

$$\frac{10 - 5}{1.05}\,\% = 4\text{-}3/4\%$$

when capital appreciation is not present.

Conclusion:

Provided the rate of return of the investment is derived correctly, inflation will have no effect in the situation analysed above. However, the following points should be noted:

(i) If the rate, i, of inflation is not the same for the value of the risk insured and the value of the investments made, then there will be an effect due to inflation.

(ii) The alternative situations of insuring for 'replacement value' or 'residual value' are both covered by the above analysis. However, the annual rate, c, of contribution will differ in each case. (In a real situation, the fund would need to accommodate both, but for the sake of argument they may be considered as two separate funds.)

SEISMIC DESIGN OF HIGH RISE BUILDING - G. H. F. McKenzie. Vol. 4, No. 2. April 1971.

K. W. WILLIAMSON (Auckland)

It is sometimes suggested that earthquake forces need be considered as acting in 2 independent directions only, i.e. along either of the 2 main axes of the structure. At a typical interior column of a framed building with 2 way beams, the beam strength for lateral loading at 45° to the main axes would be greater than the strength of a single beam, and the column strength would normally be less. These changes in relative strength can be considerable and could easily lead to yielding of the column before yielding of the beam even though the column has an adequate reserve of strength for lateral load applied along a main axis of the building. It seems that considerably greater column strength would be required to cope with this loading case. Would the author consider 45° loading to be a design loading case? and has he any comments about the relative merits of circular versus rectangular arrangements of bars?

THE AUTHOR REPLIES that the possibility of 45° loading, which has been pointed out by Mr. Williamson, is an important point which tends to be overlooked by designers. However, if the response periods of the structure along the 2 main axes are appreciably different, I think that the probability of a maximum response occuring along a 45 degree line is low. If the response of the 3 dimensional structure can be considered as the resultant of the response of two 2 dimensional systems at right angles, the two systems would have to reach their maximum response at the same time during the duration of the earthquake, and the 2 systems of different frequencies would have to coincide in phase at the instant when each reached the maximum positive value of the cycle, and to achieve this coincidence in the cycle of maximum amplitude for each system. This requires a compounding of coincidences which appears unlikely if the 2 systems have appreciably different response periods. If the 2 systems have similar response periods, then I think that the mutual column weakening effect of biaxial bending would tend to pull the responses of the 2 systems at right angles into phase with one another, in which case the probability of a maximum response occurring along a 45 degree line is moderately large. In such circumstances I think that 45° loading would have to be a design loading case. This would require an increase in the margin of column strength over beam strength, and would cause design difficulties. Hence I think that it is worth-while to take measures to ensure that the structural systems along the 2 main axes have appreciably different response periods. This can be done in many ways - for example by adopting different beam spans in different directions, or by making the beams in one direction main beams, which support secondary beams running in the other directions, with the main beams having greater depth and consequently greater stiffness.

In reply to Mr. Williamson's question on the relative merits of circular versus rectangular arrangements of bars in column sections, I think that a rectangular arrangement of bars makes more effective use of a rectangular column section even for biaxial bending if ultimate strength design is employed. However much thought has to be given to detailing to obtain adequate confinement in a rectangular section.

O. A. GLOGAU (Wellington)

I would like to add the following comment with regard to the question of a 45° earthquake attack on a column in a framed building. In a short paper in the Bulletin of the Society of Earthquake Engineers I gave this as one of the reasons why we had to confine columns even though we set out to achieve beam hinging. Having provided but not allowed for simultaneous yielding of framing beams in two directions plus a certain ductility for the column matters may not be quite as bad as they appear to be because I understand from Professor Rosenblueth that for many earthquakes there appears to be a reasonable spread of large pulses in all directions. If in future this pattern can be further confirmed then we are not faced with having to absorb the entire earthquake energy by a column hinge mechanism but only instantaneously with a reversion to beam hinging mechanisms when the direction of earthquake attack is closer to one of the principal axes of the building for subsequent pulses.

THE AUTHOR STATES that the inelastic response spectra which have been published for components of earthquakes in a particular direction take into account the spread of pulses in all directions and indicate that appreciable ductility factors are still required along each main axis. However for response at 45° to the main axis, as I have pointed out in reply to Mr. Williamson, if the response is the resultant of 2 perpendicular responses of different frequencies it is likely to be much smaller than the vector resultant of the maximums of each of the 2 perpendicular responses. The spread of pulses in all directions would make the likelihood of the 2 systems simultaneously reaching their maximums even lower. On the other hand, if the response periods along the two main axes are very similar, and the effect already mentioned tends to pull them into phase, then appreciable ductility factors along a 45° line could be required.

B. J. KEMP (Wellington)

The author suggests that beam hinges should develop before column hinges, but this implies the possibility of precasting hinge development, which in turn depends on a knowledge of steel strengths. Steel specifications tend to set lower limits for yield and U.T.S. and manufacturers sometimes boast that they exceed these limits generously. Would the author suggest that steel yields and U.T.S. be specified more closely, including upper limits? Perhaps the example of the precise specifying of prestressing steel sets an example for reinforcement manufacturers.

THE AUTHOR STATES that the extent to which actual yield strength of reinforcing steel exceeds specified yield strength has caused the author and his colleagues much concern in design work, and much investigation work has been carried out. In other advanced earthquake engineering areas, such as California, the same problems exist, with even greater margins of excessive yield strength. There is evidence that the cost of closer control of the steel properties may be high and that it may be more economic to accept present material uncertainties and to take appropriate design measures to compensate for possible margins.

T. PAULAY (Christchurch)

For a complex structure the proposed method of computing ductilities may amount to very consierable work. Would it not be feasible and perhaps more efficient to approach this from the study of deflections (i.e. drift) of the structure. The maximum elastic deflection and the subsequent postelastic displacement could be determined at the approximate level of the centre of mass. When a computer is used it should not be too difficult to do this for a multistorey structure which is being modified in its behaviour in the process of developing plastic hinges. While tracing the development of an overall ductility factor of 4 a suitable programme could trace the ductility demand for each of the affected members of the structure.

THE AUTHOR STATES that the study of deflections is the very way in which he envisages the strain energy method of calculating overall frame ductility being applied. You will note that at the end of section 6 the text states that "the individual load-deflection graphs can represent individual members or single storeys or other sub assemblages of members." If separate graphs are used for individual members, there may be a considerable amount of work involved, but there are cases where one graph can be used for a deflection of a complete storey. Figs. 8, 9 and 10 give a typical example of a structure where a separate graph for the deflection of each storey gives all the information required, with comparatively little work. In general I prefer this approach to Mr. Paulay's suggestion of determining the maximum elastic deflection and the subsequent post-elastic displacement at the approximate level of the centre of mass, because I feel that the latter approach could hide important information about the behaviour of the frame. The approach in figs. 8, 9 and 10 gives the designer more information about the effect of the behaviour of each storey on the overall equivalent ductility.

PROFESSOR R. PARK (Christchurch)

Mr. McKenzie is to be complimented on his thought provoking paper. The overall ductility factor for a frame is found from the individual load-deflection graphs for parts of the frame assuming an elasto-plastic response. However it is important to realize that after the first loading into the yield range reinforced concrete flexural members do not behave in an elasto-plastic fashion - the loop has a smaller area due to the softening of the stiffness caused by the Bauschinger effect of the steel and the opening and closing cracks in the compression zone. While Clough has shown that for single degree of freedom systems with periods of greater than 0.6 seconds there is not a great deal of difference in the response of elasto-plastic and stiffness degrading system, it may be misleading to place too much emphasis on the ratio of elastic and inelastic areas under an idealized load-deflection curve. Mr. McKenzie comments on the available ϕ_u/ϕ_y values for members designed to the SEAOC code. The SEAOC code limits on beam steel area ($p \leqslant 0.46 \frac{f'_c p'}{f_y p}$) ensures a ϕ_u/ϕ_y ratio of at least 16 and 11 for $p'/p = 0.25$ and 0.5 respectively, assuming f_y = 40 ksi, f'_c = 4 ksi and a maximum concrete strain of 0.004 (see Blume, Newmark and Corning, Figure 5.14). It is important to realize that this is the section ductility available up to the start of structural damage by concrete crushing. Higher ϕ_u/ϕ_y values are possible after crushing if some drop in moment capacity is acceptable. The curves for ϕ_u/ϕ_y of confined tied columns (Blume, Newmark and Corning, Figures 5.16 and 5.17) are based on a maximum concrete strain of 0.01. Again higher maximum concrete strains can be sustained by confined concrete although this may be at the expense of reduced load capacity. More evidence is becoming available[1] to illustrate the behaviour of members at high concrete strains. The calculations for ultimate curvature will be conservative if the full extent of the moment-curvature curve is not considered. Mr. McKenzie expresses concern about the lack of testing on large reinforced concrete sections. The main difficulty involved for reinforced concrete columns is the size of loading required for large specimens. For instance, a 30 in. square column with f'_c = 5000 psi would require an axial load of approximately 4,000 kips to crush the concrete alone. At present the University of Canterbury is negotiating to obtain a large capacity compression testing machine and it is to be hoped that in the near future the University will be in a position to conduct tests on large column specimens. However, available test evidence indicates that the effect of the scale on the behaviour of concrete confined by ties is not so significant and that available test information for smaller scale specimens (say half or quarter scale) may be used to deduce the behaviour of large columns.

Ref. 1. D. C. Kent and R. Park : "Flexural Members With Confined Concrete", Journal of Structural Division, ASCE, July 1971.

THE AUTHOR STATES that firstly he agrees with Professor Park that the difference between the form of the idealized elasto-plastic load deflection curve and the actual curve for repeated reversals beyond yield point of reinforced concrete flexural members may significantly alter the values of the overall ductility factors calculated under some conditions. The relevant sections of my paper should be reviewed to examine the effect of more realistic forms of the load-deflection curve. Professor Park has also drawn attention to the fact that higher ϕ_u/ϕ_y values are possible after the start of concrete crushing, if some drop in moment capacity or load is acceptable. The values recommended in my paper are those which appeared from the designer's point of view to be reasonably safe to rely on, in view of a shortage of information about the behaviour of members at high strains. It may be reasonable to increase these values to some extent as more information becomes available. However, it should be remembered that the graphs in figure 3. were based on the assumption of yield strength being maintained for the full extent of the deflection into the yield range. If the yield strength drops as maximum strain is approached, due to concrete crushing, greater ductility factors will be required for the same original yield strength. I can agree with Professor Park on the difficulty of testing large column specimens, due to the size of loading required. I think that if careful thought is given to detailing of smaller scale members, to avoid any scale effects on stress patterns or confinement conditions, and if careful thought is given to interpretation of the results, taking into consideration possible scale effects, that valuable information may be obtained from tests on smaller scale specimens. Recent test information published indicates that scale effects may not be as serious as previously thought on rectangular confined members, due to a pattern of outward spalling under ultimate deflection conditions which relies only on confinement at the corners.

DESIGN LESSONS FROM RECENT DESTRUCTIVE EARTHQUAKES - R. I. Skinner. Vol. 4, No. 2. April 1971.

O. A. GLOGAU (Wellington)

Has Mr. Skinner seen any damage to piles under buildings. These are at present rarely if ever designed for ductility but could be expected to be part of the building soil response system required to absorb inelastic energy in certain cases. Does this damage perhaps remain buried?

THE AUTHOR STATES that no direct evidence of severe pile damage has been seen. In the case of the Sheraton Hotel near Caracas the pattern of column damage suggested that piles had suffered differential settlement, or possibly some were damaged. Detailed dynamic measurements, before and after severe earthquakes, should help in checking for possible pile damage.

C. M. STRACHAN (Wellington)

I am interested in Mr. Skinner's comment that the soil in Manila could liquify under continued vibration. Do you have a record of the gradings of these soils. Also, could you give me any idea of what method has been used in Niigata to stabilize soils for new and repaired building. Has vibroflotation been used?

THE AUTHOR STATES that much of the soil in the upper stratum at Manila satisfied the conditions; below the ground-water table, low blow counts, and fine-grained sand of uniform size. Sands satisfying these three conditions underwent extensive liquefaction during the Nugata earthquake of 1964 and the Tokachi-oki earthquake of 1968. I have no information on the use of vibroflotation in these areas.

G. A. EIBY (Wellington)

Since the two earthquakes affecting Manila originated in the same azimith, what justification is there for asserting that there was a different direction of "attack"! Is it not more reasonable to suppose that buildings damaged in the first shock possessed different characteristics of response to the two events?

THE AUTHOR STATES that the direction of attack on many buildings was frequently very clearly evident from the nature of the damage. These differences in damage could not be accounted for by differences in the period ranges of the ground vibration.

D. DENHAM (Canberra, Australia)

Have you any figures relating to the duration of the shaking, the predominant frequencies present in the ground motion and the maximum acceleration experienced during the Manila and Caracas earthquakes?

THE AUTHOR STATES that the duration of violent shaking during the Caracas earthquake of 1967 was about 45 seconds and the duration of the Manila earthquake of 1968 was about 30 seconds. No strong-motion records were taken in areas of severe damage. The Manila 1968 earthquake was recorded on firm ground at Quezon City five miles NE of the severe damage area of Manila. The maximum recorded acceleration was 0.07 g. However, the maximum acceleration in the damage area of Manila was probably 0.2 g or more.

P. D. F. SWAN (Wellington)

Two buildings shown have considerable lack of symmetry with shear walls on one side only. To what extent, from your observation, did floors act as diaphragms in transmitting lateral loads to the shear walls? and what damage did the floors sustain.

THE AUTHOR STATES that the floors acted as very effective diaphragms. The floor damage observed was always associated with vertical deformation. This vertical deformation was caused by stairways, damaged columns and broken beams.

L. G. BROWN (Wellington)

Mr. Skinner's paper entitled "Design Lessons from Recent Destructive Earthquakes" features damage to buildings in the Philippines. He makes no reference, however, to the integrity of the workmanship, nor the very poor standard of materials. It is my opinion that we in New Zealand should be careful not to place too much emphasis on experiences of this type in areas where very low standards are permitted for reasons of political graft and lack of Government responsibility. Having spent some time in the area referred to I feel most strongly on this subject. It has been my observation that by world standards our building codes bear comparison with any other area. I am sure more benefit would result from encouraging more activity on the education of the technician and the building community generally.

THE AUTHOR STATES that the comments in the paper are based on observation of earthquake damage in Venezuela, Japan, Manila, New Zealand and the New Guinea territories, and on a study of many earthquake damage reports. The quality of materials and workmanship for Manila buildings varied widely, from good in many buildings to inferior in others. These factors and their effects are discussed in the detailed reports listed under references.

G. L. EVANS (Christchurch)

Plastic Reserve in Buildings (Para. 3). The observation that "normally assumed plastic reserve failed to materialize" seems to me to be profoundly important. The author gives some of the reasons for this and relates this to be ommission of panels in the first storey. Would the inclusion of shear panels in the first storey have prevented the damage? - judging by other evidence presented (Para. 4) probably not much unless due as in Para. 6. In columns where ties unwrapped, why did this occur? Were ties badly lapped or did they actually break?

THE AUTHOR STATES that had the low-strength panels extended to the first storey of the reinforced-concrete buildings, there would have been a small reduction in column damage in many cases. Ties unwrapped because the corner laps were taken through 90° instead of 135°. There were a very small number of tie breaks.

ELASTIC SOIL-STRUCTURE INTERACTION - A. J. Carr and P. J. Moss. Vol. 4, No. 2. April 1971.

R. D. SHARPE (Christchurch)

If at some stage the authors are going to include in their analysis a finite-element model of the Sir Stanley Goosman bridge, would they like to make an estimate of the width of the soil-section on which they will place the structure. If only a narrow section is taken the considerable mass of the bridge lumped at the surface of the valley may significantly alter the fundamental frequency of the whole section.

THE AUTHORS STATE that Mr. Sharpe quite rightly raises the query as to what width of the soil mass should be included with the bridge for the proposed analyses. Certainly if a too narrow strip of soil is taken the mass of the bridge could have a marked effect on the behaviour of the combined soil-structure system. This would be unrealistic considering the real distribution of mass of the bridge relative to that of the soil in the valley cross-section. However, the real problem is one of three-dimensional elasticity and hence the only complete solution would be to treat the problem as such, but this unfortunately, by reasons of computer size and computational time, is not practical. There are two basic approaches available if the problem is to be considered by using a two-dimensional analysis.

The first method is to take the analysis as outlined in the paper and using engineering judgement select a thickness of soil that would provide a realistic mass distribution between the bridge and the surrounding soil. Then in subsequent analyses, vary this thickness to measure the sensitivity of the response to the thickness choice. This approach will provide some experience on which a suitable thickness for future analyses may be based. It is intended that we shall use this approach for our initial analyses but it is obvious that this method becomes more difficult when the structure has a more significant effect on the total response. The second and more accurate method takes into account the three dimensional nature of the problem and it is hoped that it will be used for later analyses. In this approach, a normal finite element idealization is used for the valley cross-section and bridge structure, as at present, but a Ritz method of analysis is utilised in the normal direction (i.e. along the valley). Such a method of analysis is similar to the more familiar Fourier Analysis in that the variation of displacement along the valley will be assumed to belong to some finite series of function. As the problem will be symmetric about the cross-section suitable functions could be cosines, damped cosines or hyperbolic cosecants. For each member of the series a two-dimensional analysis would be carried out and the results summed to give the final solution. Though this approach would be much more expensive than the first method it would still be much less expensive and demanding on computer storage than a full three-dimensional finite element analysis.

G. L. EVANS (Christchurch)

I wish to emphasize the practical importance of this type of dynamic analysis. Although expensive in computer time it is economical for relatively large projects. In particular its application to the Taramakau Bridge has provided the bridge designers with a figure (approximately 6") on which to base the abutment design for the required amount of displacement. Current analyses including the bridge have provided figures for the differential movement between the bridge and the ground. Mr. Sharpe may care to enlarge on this.

Replying to Mr. Evans, THE AUTHORS FEEL THAT, in fact, these analyses, besides being practical need not be regarded as expensive provided that the data is known with a reasonable degree of certainty so that many sensitivity analyses are not required. Each analysis of the bridge site required approximately forty-five minutes computation time on the IBM 360/44 computer at the University of Canterbury. Some work has so far been carried out using an extremely simple finite element idealization of the bridge structure. The results give an excellent agreement with theoretically calculated natural frequencies for the bridge and as the fundamental frequency is approximately ten times that of the valley section, the results show, as expected, that the motion of the bridge is largely that of the valley, the excitation accelerations that are likely to excite the bridge itself being largely filtered out by the soil system.

M. J. N. PRIESTLEY (Lower Hutt)

Results given by the authors in Fig. 5 indicate substantial lateral tensions in the soil. Do the authors feel that the simple finite-element model used, which allows such tensions, gives a realistic representation, and could they comment on the likely changes in response behaviour resulting from a model in which the soil has restricted, or even zero tension capacity.

With respect to the question by Mr. M. J. N. Priestley, THE AUTHORS WOULD POINT OUT THAT the displacements shown in figure 5 are those of the mode shapes and as such have no specific magnitude. The authors agree that large tensions could not be sustained in a soil system but this could be accounted for by repeating the analysis, where necessary modifying the elastic properties in those regions, as outlined in the paper. This would be expected to lower the natural frequencies of free vibration and probably lead to an increase in the displacements for the earthquake response. The authors agree that the most accurate approach would be to use an analysis where properties could be allowed to vary according to the sense and magnitude of the strains existing in the soil. Such an analysis would be non-linear, superposition would no longer be valid and a modal analysis would be meaningless. For this reason our analysis was confined to linear elastic systems. The only approach to the non-linear problem would be to use a direct integration of the equations of motion and to date these have only been attempted for framed structures where the size of the problem is much smaller and the constitutive laws of the materials are much better understood. Computational time for a non-linear earthquake analysis using the finite element idealization shown would be measured in hours or days and not in minutes.

STEADY STATE VIBRATION TESTS OF A SIX-STOREY REINFORCED CONCRETE BUILDING - A. M. Reay and R. Shepherd. Vol. 4, No. 1. March 1971.

G. H. F. McKENZIE (Wellington)

The amplitude of the translational vibration of the base of the building shows that a seismic recording instrument at the base would not record the applied ground motion, but would instead give a record composed largely of building movement. This supports the writer's view that, in shear wall buildings, it is desirable to have an additional instrument in the ground, at a reasonable distance from the building, to measure the true ground motion. I realize, of course, that for city buildings, it is not always possible to site ground instruments clear of buildings.

THE AUTHORS STATE that Mr. McKenzie rightly suggests that in this particular type of soil-structure system, seismic recording instruments in the base of the building would record a significant component from the response of the structure to an applied ground motion. The authors agree that it would be desirable to have an additional instrument in the ground. Measurements made during the reported tests suggest that a separation distance between the instrument and the building equal to the width of the building will ensure that the recorded motion is not modified by the response of the structure.

R. MAYES (Auckland)

In the synopsis values of critical viscous damping are quoted. Would the authors please inform us how these were obtained.

THE AUTHORS STATE that the values of equivalent viscous damping were calculated from the amplitude response curves of the structure in each of the normal modes recorded. Where the amplitude response of a normal mode was recorded without significant interference from other normal modes the equivalent viscous damping was determined by dividing the width of the amplitude response curve at 0.707 times the peak amplitude by twice the natural frequency of the mode. (The method only being accurate for low damping levels.) Where significant modal interference was present the individual normal mode properties, including damping, were determined by an iterative technique. (10)

P. JOHNSTONE (Wellington)

In your paper it was stated that 32% of the top deflection was found to be due to ground compliance, and also that the agreement between the measured and predicted frequences was good, which could imply a contradiction. These questions mainly relate to the manner in which the frequency was predicted, and the consequences of the assumptions made.

1. How was the value of the soil stiffness assessed? Were any laboratory tests carried out? How sensitive are the predicted frequencies to changes in the assumed soil stiffness?
2. How do you cope with the changing stiffness of the soil? I am not an expert in soil mechanics but I understand that the stiffness of the soil would increase with increased loading. Also shaking of the soil itself may alter its stiffness properties.
3. What percentage of the damping can be attributed to the soil, in view of the 32% ground compliance? How would this vary with the shaking force?
4. How were the stiffnesses of the various structural elements assessed? Was the shear wall-frame interaction studied with respect to the individual floor displacements?

Could it be possible that cancelling errors occurred when predicting the frequency; these being associated with the structure and soil stiffnesses.

THE AUTHORS STATE that Dr. Johnstone suggests that there could be a contradiction between the large degree of soil-foundation interaction and the close agreement between predicted and measured mode frequences. However, the approach (5) adopted by the authors to the prediction of the normal mode frequences ensured that there was no contradiction. In answer to Dr. Johnstone's particular questions, the foundation flexibility coefficients were derived by considering the dynamic force-deflection characteristics of the base of the prototype structure vibrating in the fundamental mode. Thus the actual soil-structure interaction characteristics of the prototype (including the effects of soil mass) were used in the prediction of normal mode properties. This procedure effectively eliminates the consideration of one variable, soil flexibility, from the comparison between measured and predicted normal mode properties, enabling a realistic study of the dynamic properties of the building structure itself. With regard to the question of sensitivity of the modal frequencies to the assumed soil stiffness, the soil stiffness was not assumed, as suggested by Dr. Johnstone, the actual effective soil stiffness being used in the analysis. The sensitivity of the mode frequencies to the stiffness of a particular element of the system, such as the soil stiffness, is a function of the stiffness of the particular element and the relation between it and the stiffness of the remainder of the system. The stiffness of the soil may not necessarily increase with increased loadings, changes in the stiffness depending on the type of soil. No significant change in the soil stiffness occured for varying frequency or force levels, an expected result due to the small soil pressure changes induced by the dynamic testing in relation to the static bearing pressures from the dead weight of the building. The proportion of damping attributable to the soil was not determined, only the damping for the total system being calculated. No increase in damping was detected for an eightfold increase in excitation force in the first translational mode. The stiffness of the shear walls was determined by loading araldite models of the shear wall structures and measuring the lateral deflection. Lateral stiffness matrices were determined for the reinforced concrete frames (3) and were added to the lateral stiffness matrices for the shear walls, the appropriate soil flexibility being included. The criteria for adding the lateral stiffness matrices is compatability of deflection of the shear walls and frames at all floor levels, and thus the effects of shear wall-frame interaction are included in the theoretical analysis. Dr. Johnstone suggests that cancelling errors could occur with respect to the soil and structure stiffness. This possibility does not exist in the fundamental modes because, as stated above, the soil structure characteristics used in the analysis were determined from the response of the prototype soil-structure system.

W. R. STEPHENSON (Lower Hutt)

The authors refer to running the exciter steadily from 2 to 13 H2. What time was allowed for this run up? Did the authors also do a run down? If so, how different were the modal frequencies during run up and run down? How different would the modal frequencies have been had velocity detectors been used?

In reply to Mr. Stephensons' questions relating to the method used for the steady state dynamic testing, THE AUTHORS STATE that the exciter speed was only changed steadily during the preliminary testing of the building. Throughout all the principal tests the exciter was maintained at a constant speed until steady state vibration of the structure was obtained, and the response of the structure was then recorded for that frequency. No change in the steady state response of the structure was recorded for increasing and decreasing frequency increments. No difference in modal frequencies would occur if velocity detectors were used instead of displacement detectors, provided the effects of damping are considered when determining the normal mode frequencies from the response curves.

References

(10) Reay A. M. and Shepherd R. "The Separation of Two Combined Normal Modes". Journal of Sound and Vibration, 1971, 17 (2).

FULL-STRENGTH BUTT WELDS OF REINFORCING BARS BY THE PRESSURE-GAS PROCESS - I. C. Armstrong. Vol. 4, No. 2. April 1971.

I. D. STEWART (Wellington)

How soon will a similarly acceptable process for pressure gas welding of HY60 steel be available for general use and indeed what is the probability based on tests already carried out that such a process will eventuate?

THE AUTHOR STATES that further practical experience in use of the process in construction is now necessary. Therefore, work in the near future is likely to cover the production of a range of equipment for the bar sizes available, setting up the techniques on the first few construction jobs, and further consideration of methods of production testing. Additionally, development of an acceptable technique for welding HY60 bars to NZS 1879 will be undertaken as soon as is practicable. The problems of oxidation and resulting decarburisation of butting surfaces which have a low carbon content should be significantly reduced in the case of HY60 bars which have an increased carbon content. A few full-strength pressure-gas welds of HY60 bars have been tested without difficulty, and since chilling of the steel does not occur due to the large heat input, this grade of reinforcing bar is expected to be readily weldable by the pressure-gas process.

J. P. HOLLINGS (Wellington)

What is the closest bar spacing recommended for practicable field welding?

THE AUTHOR STATES that for field welding of vertical bars in a structure, the bar clamp must be positioned on one side of the bar requiring 7 in. clearance from the bar axis, and the torch must be oscillated through a 90-degree angle at a $5\frac{1}{2}$ inch clear radius on the other side. Scale drawings giving main dimensions of these units are provided in reference (1) of the paper as an aid to designers in checking suitable bar arrangements (e.g. by cardboard cut-outs). The above clearances would permit shear wall reinforcement in rows $7\frac{1}{2}$ in. apart with bars spaced at $4\frac{1}{2}$ in. centres in each row. Bundled bars in columns may not be suitable for the pressure-gas equipment, but welding of spaced single column bars should, in many cases, be practicable with some ingenuity and possibly re-arrangement of construction operations. A range of sizes of equipment should eventually become available, so that smaller, lighter units can be used where appropriate. Thus, smaller clearances could be used as the manufacture of equipment permits.

THE SHEAR STRENGTH OF SHEAR WALLS - T. Paulay. Vol. 3, No. 4. December 1970.

O. A. GLOGAU (Wellington)

We have had some difficulty in arranging the diagonal reinforcing recommended by the author. There often seems no way of getting even a small amount of reinforcing horizontally past the corners of openings above and below spandrels in shear walls. Is there any concern with regard to early damage to the triangular portions above and below the diagonal reinforcing - i.e. cracks starting from the corners of the openings. This may be of no structural significance but how do you tell this to the client. Do we need to write this on the beams prior to the earthquake.

THE AUTHOR STATES that the paper did not elaborate on the behaviour of diagonally reinforced deep beams which Mr. Glogau questioned. All test beams presented in Table

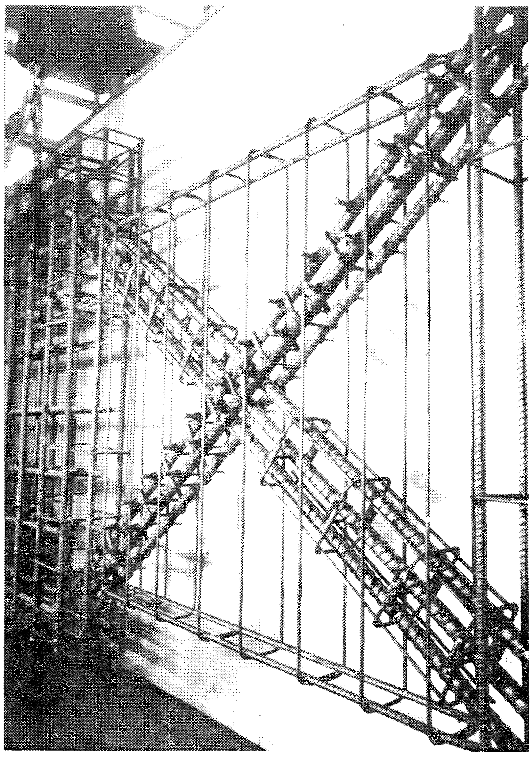


Fig. 10 Diagonal Principal Reinforcement in a Deep Spandrel Beam.

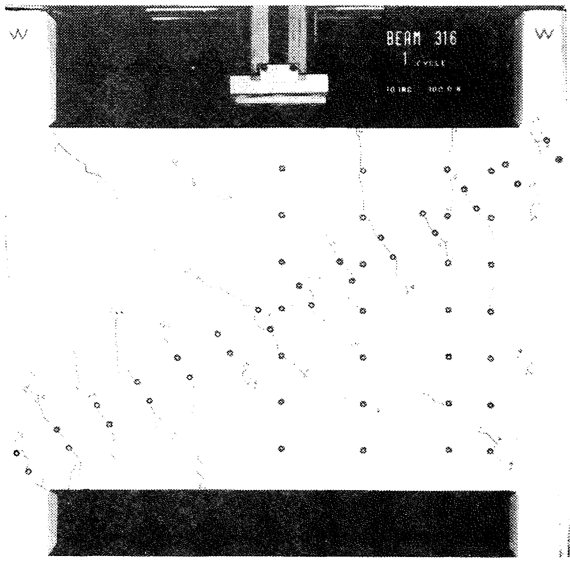


Fig. 11 Crack Pattern in Beam 316 at the end of the 1st Load Cycle (v = 600 psi).

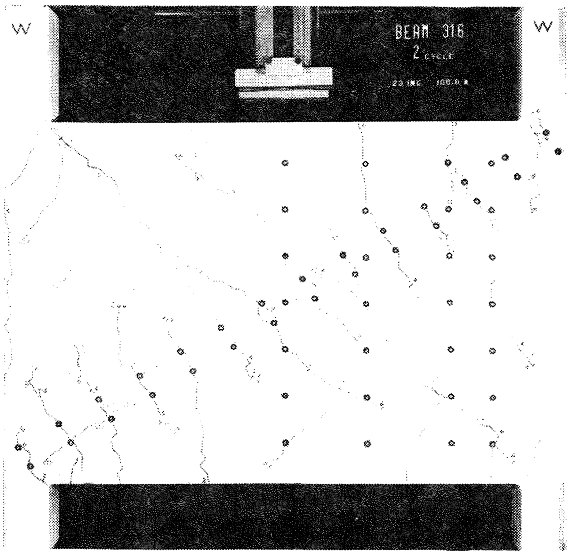


Fig. 12 Crack Pattern of Beam 316 in the 2nd Load Cycle (v = 600 psi.).

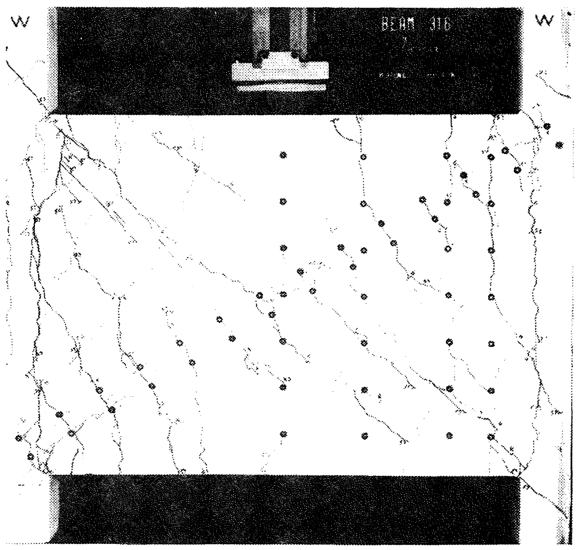


Fig. 13 Crack Pattern of Beam 316 after the 7th load Cycle (v = 660 psi.).

I are of the conventional type, containing equal top and bottom horizontal flexural reinforcement and vertical stirrups. In a preliminary test the principal reinforcement was placed diagonally. A typical reinforcing cage, to be used shortly in another test, is shown in Fig. 10. With the omission of the principal horizontal bars it is expected that no difficulty will be encountered where the diagonal steel is to be anchored in the adjacent shear walls. The horizontal bars and the stirrups were intuitively provided for crack control and basketing purposes when the concrete is breaking up after alternating high intensity cycle loading. It is felt that the diagonal bars should provide sufficient protection against excessive cracking during a moderate seismic disturbance at a point where the continuity of the horizontal bars might present difficulties. If the diagonal bars are not highly stressed at the corners of the beam there appears to be no reason to expect excessive cracking in those areas. Figures 11, 12 and 13 show the crack patterns of a test beam at various stages of its loading. The cracks which formed at the end of the first cycle, at a load corresponding with 600 psi. nominal shear stress, appeared already at 50% of this load. This load happens to coincide with the maximum shear capacity of the beam as predicted by the current ACI Code (see Fig. 7). The crack patterns at the end of the 2nd and 7th load cycle, shown in the other two figures, suggest that the formation of cracks is largely governed by the behaviour of the diagonal reinforcement and that no early damage to the triangular part above or below the diagonal reinforcement would be anticipated. The test beam illustrated failed in the 13th load cycle where, because of strain hardening, the predicted ultimate load was exceeded by 10% at a nominal shear stress of 900 psi. The attained ductility, with respect to the overall behaviour of the beam, was in excess of 15. Further details are available elsewhere (15, 16).

P. JOHNSTONE (Wellington)

Studies of both model and prototype shear walls, in the elastic range, have indicated the possibility of truss action in such walls. By observing both stress trajectories and contours of principal stress difference, paths of force appeared to originate at the floor-flange junctions, pass diagonally up through the connecting beams, and join higher flange-floor junctions. Thus the shear wall can be imagined as a truss, with the flanges forming the chords, the floor slabs the ties, and the streets take the form of those concentrated paths in the web. What are the author's views on the contribution of such truss action at or near the ultimate condition?

THE AUTHOR STATES that Dr. Johnstone's findings on model and prototype shear walls seem to indicate that already in the elastic range they are approaching the type of behaviour that could be expected in a cracked reinforced concrete structure. Flanges of shear walls and floor slabs, acting as web stiffeners, would tend to attract load originating from shearing forces. In accordance with the truss analogy this behaviour would place the wall proper i.e. the web, under diagonal compression, a desirable feature. However, one should view the performance of the flange-slab-wall junction with some suspicion. Because the flange and slab forces cannot resolve themselves into a diagonal component, except at and in the immediate vicinity of the wall, the force concentration at the junction is likely to eliminate the greater part of the slab and floor contribution towards shear transfer by truss action when ultimate load intensities are being approached.

References

(15) Paulay, T.: "Coupling Beams of Reinforced Concrete Shear Walls", Journal of the Structural Division, ASCE, Vol. 97, No. ST3, March 1971, pp. 843-862.

(16) Paulay, T.: "Simulated Seismic Loading of Spandrel Beams", Journal of the Structural Division, ASCE, Vol. 97, September 1971.

INELASTIC BEHAVIOUR OF REINFORCED CONCRETE MEMBERS WITH CYCLIC LOADING - D. C. Kent and R. Park. Vol. 4, No. 1. March 1971.

G. W. BUTCHER (Wellington)

The authors' stress-strain model for confined concrete is based upon the use of conventional closed stirrups. Such a stirrup is an inefficient means of confinement compared to spirals as the authors state. A practical alternative to heavy stirruping could be the use of welded steel mesh stirrups to reinforce the core and hence delay crack propagation. Some experimental work on alternative types of confining reinforcing has been carried out by McDonald of the U.S. Corps of Engineers. Tests were carried out on prisms and simply supported beams loaded to form hinges at the centre. The effect of mesh reinforcing to the concrete core is to raise the BC region of the stress strain diagram shown in the authors Fig. 8 from $0.2f'_c$ to possibly 0.8 to $0.9f'_c$. Would the authors care to comment on the effect this would have on the moment curvature response.

THE AUTHORS STATE that welded steel mesh stirrups make very efficient confining reinforcement because the concrete is restrained laterally at a number of points across the section rather than just at the corners as is the case of rectangular hoops. Also, the spalled areas of unconfined concrete at the edges of the concrete core are not to be so deep as that for rectangular hoops (see Fig. 7) because the points of confinement would be closer. However some difficulties in the use of such mesh are; when placed across sections in columns it would make the placing of concrete difficult; it would not tie compression steel effectively and hence may allow buckling of those bars when the cover concrete spalls; unless it was anchored effectively to the main tension steel it could not act as shear reinforcement. If due to very efficient confinement the region ABC beyond the peak of the concrete stress-strain curve of Fig. 8 was practically horizontal the behaviour of the concrete in this range would be close to that of a perfectly plastic material. This would mean that the moment-curvature response of the section would show a small reduction in moment capacity when the cover concrete spalls and an increase in moment capacity when the steel commences to strain harden but otherwise an almost horizontal moment-curvature curve after yielding had commenced (assuming cracks in the compression zone were closed). Confinement is particularly important in the case of columns. Beams may be reinforced to give a ductile moment-curvature curve with only nominal traverse steel but columns with axial compression need effectively confined concrete for ductile behaviour. The presence of supplementary ties across a column section would help confinement in much the same way as a mesh but less effectively.

R. J. P. GARDEN (Dunedin)

How much does rate of loading affect the load/deflection characteristics? A period T of 0.1 sec. is reported by Reay and Shepherd for a 6 storey building. A T of 0.1 sec. is reported also by Scrivener and Williams for post-elastic cycling of reinforced masonry walls, associated with large ductility ratios (6.9, 9.6, 32 etc.). A structural system oscillating with a period of say 0.2 sec. and having in it a member showing a member-ductility factor of 6.9 will have the strain in such member change from zero to 6.9 times yield strain in a quarter of the period. Assuming S.H. Motion, this would mean that fy is reached in something like a ninth of the quarter period, i.e. it reaches yield in five milliseconds. A personal communication from G. M. Garden of the N.Z. Agricultural E. Institute (which has done a considerable program of well instrumented monotonic impact loadings of MS) has exhibited a linear stress/strain relation up to 28.2 tsi followed by a very flat second line showing an ultimate of 32.5 tsi or higher, over elongations of 20% on 63 cm. for longer. Static tests gave fyl 21 tsi, fyu 22 tsi, fult 31 tsi & 26.4% elongation on 8" gauge length.

THE AUTHORS STATE that Mr. Garden has rightly pointed out the increase in strain rate in yielding members when the stress varies between zero and yield over only a small proportion of its $\frac{1}{4}$ cycle. This is a point which has not received sufficient attention. A recent report of ACI Committee 439[A] summarizes test data which shows the effect of strain rate on concrete and steel strength. For the structural system considered by Mr. Garden in which the materials reach yield in 5 milliseconds there may be a considerable increase in steel strength (e.g. 30%). However the results reported in the ACI report indicate that the increase in strength due to rate of loading is greater for concrete than for steel and so in a flexural member yielding of the steel will occur before the concrete reaches its maximum strain. Hence a ductile response will still occur although at an enhanced load. Evidently only low strain rates were referred to by Singh et al[7] when they reported that "the effect of rate of straining is not noticeable over the usual range of test speeds". However, many structures will have a period greater than 0.2 seconds and therefore the strain rate effect will be small in many cases. At the University of Canterbury in recent years static cyclic loading has been used in a number of tests to represent earthquake motions. This has been because such loading is more convenient to apply than dynamic loading and allows ease of collection of test data. In tests on reinforced and prestressed concrete structures the use of slow reversals of load to represent the more rapid earthquake motions is thought to be conservative because of the increase in strength of the materials with strain rate. There have been a number of tests which have shown the effect of rate of strain on the response of structural elements. For example, Takeda, Sozen and Nielsen[B] have reported that force-displacement relationships obtained from static tests on reinforced concrete members yielding in flexure may be used to satisfactorily predict the dynamic response of those members to earthquake motions. Criswell[C] has reported tests on reinforced concrete slab-column junctions which were loaded to failure in 0.009 to 0.032 seconds. The failure mechanism and crack patterns found were similar to identical specimens tested statically. The strength increased 26% for specimens failing in shear and 18% for specimens failing in flexure. The failure deflection also increased by up to 50%. The strength increases were greater than that indicated by the expected increase in material strength. Oladapo[D] has reported tests on prestressed concrete beams in which moment-curvature relationships for beams loaded to failure in 0.05 seconds were compared with those for beams loaded statically. The dynamically loaded beams showed a 15% increase in ultimate moment and a 30% increase in ultimate curvature. More dynamic cyclic loading tests on reinforced concrete structures using high strain rates need to be conducted in the future, but present indications are that use of results from static tests are conservative.

References

(A) ACI Committee 439: "Effect of Steel Strength and of Reinforcement Ratio on the Mode of Failure and Strain Energy Capacity of Reinforced Concrete Beams", Journal ACI, March 1969.

(B) Takeda, T., Sozen, M. A. and Nielson, N.N., "Reinforced Concrete Response to Simulated Earthquakes", Proceedings ASCE, V.96, ST12, December 1970.

(C) Criswell, M.E., "Strength and Behaviour of Reinforced Concrete Slab-Column Connections Subjected to Static and Dynamic Loadings", U.S. Army Engineer Waterways Experiment Station, Vicksburg, Technical Report N-70-1, December 1970.

(D) Oladapo, I. O., "Dynamic Loading of Prestressed Concrete Beams", Magazine of Concrete Research, Vol. 14, No. 40, March 1962.

USE OF YIELD RATIO RESPONSE SPECTRA TO DESIGN YIELDING MEMBERS FOR IMPROVING EARTHQUAKE RESISTANCE OF BRITTLE STRUCTURE - R. M. Gilmore and H. C. Hitchcock. Vol. 4, No. 2. April 1971.

M. J. N. PRIESTLEY (Lower Hutt)

I understand the reactors are situated on soft ground or alluvium. Have calculations been made to provide comparisons with the measured natural periods of the existing reactors, particularly with reference to the contribution of soil compliance to the structural response?

THE AUTHORS REPLY that the foundations in both locations are firm. They have not been able to calculate soil flexibilities from first principles, but have allowed for the measured flexibilities of the original structures, which includes the ground flexibilities, in the design of the yielding beams.

C. T. J. BUBB (Melbourne, Australia)

What precautions have been taken against 'overstrength' of the yielding materials apart from the use of a factor of safety of 1.3? What effect would the 'overstrength' have on the estimated safe duration of shaking (given as 100 seconds in the paper)?

THE AUTHORS STATE that they have used a factor of safety of 1.3 as a precaution against 'overstrength' arising from

(i) The 'plastic' section modulus for the beam section being larger than the 'elastic' section modulus. (About 14% larger for the yielding beams as designed.)

(ii) The increase in yield stress from the 'static' test value when the loading rate is similar to that encountered in actual earthquakes. Tensile tests (described in M.O.W. Central Laboratories Report No. 342) on similar material to that of the yielding beams showed there was an increase of approximately 18% with a loading rate giving approximately 0.06 seconds to reach yield level.

Two other sources of 'overstrength' are

(i) Actual yield strength of the material exceeding the specified minimum. Yield stresses as tested were used in designing the beams.

(ii) The increase in stress with increasing strain above about 1%. The maximum strain expected in the yielding beam material in a design earthquake is less than 1%.

If the material did in fact 'overstrengthen' to the extent allowed for, i.e. 1.3 times, then the Jennings and Husid formula suggests that the structure would survive shaking at El Centro 1940 N-S intensity for about 160 seconds instead of 100, if low cycle fatigue did not cause prior failure!

A. M. REAY (Christchurch)

Would the authors please comment on the difference in the expected mode of failure between the model studied by Jennings (involving column yielding) and the structure designed by the authors which utilizes beam yielding. Would the authors also comment on the effects of column instability associated with the swaying of the structure.

THE AUTHORS STATE that they believe that there is no difference in the modes of failure of their structure and of Jennings and Husids model because the latter is implicitly a structure where the direct compressive stresses in the columns are very small compared with the bending stresses. Both structures would fail by "ratcheting" when the bending moment from eccentric column loading finally leads to unidirectional yielding in the beams of the first structure or in the columns of the second.

BEHAVIOUR OF REINFORCED MASONRY SHEAR WALLS UNDER CYCLIC LOADING - J. C. Scrivener and D. Williams. Vol. 4, No. 2. April 1971.

O. A. GLOGAU (Wellington)

The practical designer in this material is faced with two difficulties in his efforts to achieve a reasonably ductile structure.

(1) He must adhere to the aspect ratios recommended by Dr. Scrivener and Mr. Williams and in general arrange the elements in a wall in such a manner that shear failures are precluded.

(2) Because of the vital importance of a reasonable amount of diagonal tension strength and shear in his walls to ensure flexural ductility he must be absolutely certain of the construction practices on his job. We are all only too aware how easily mortar and grout bond are lost or greatly reduced on site.

The designer should also realise that to achieve a given ductility reinforced hollow masonry will sustain more damage than reinforced concrete particularly confined reinforced concrete. The selection of the correct load factor is therefore very important. This load factor must not only allow for the degrading stiffness effect on response and hence ductility as shown by the authors, but also the effects mentioned above.

THE AUTHORS STATE that they agree with all Mr. Glogau's comments. It must be emphasized that often a ductile behaviour cannot be ensured by the designer and he must resort to a working stress design method and he must keep in mind that he will have little if any overload capacity because of the lack of ductility when shear dominates. The comment on construction practices is very valid but the authors consider that mortar and grout bond may not affect the overall shear strength as much as expected since the shear is a diagonally inclined compression/tension. Mr. Glogau's point about increased damage with reinforced masonry over that expected in reinforced concrete is very pertinent and later dynamic tests (not reported in the paper) support the view expressed that this is due to lack of confinement in the masonry situation. The selection of a design load factor must take this and many other factors into account as suggested by Mr. Glogau.

R. J. P. GARDEN (Dunedin)

How much are load/deflection relations affected by rate of loading? A period of 0.1 sec. is reported by Reay and Shepherd for a 6 storey building. A T of 0.1 sec. is given also in the present paper for Reinforced Masonry Walls, and is associated with large ductility factors (6.9, 9.6, 32 etc.). A structural system oscillating with a period of say 0.2 sec. and having in it a member showing a member ductility of 6.2 will have the strain in such member change from zero to 6.9 times yield strain in a quarter of the period. Assuming S.H.M., this would mean that f_y is reached in something like a ninth of the quarter period, i.e. it reaches yield in about five milliseconds. A personal communication from G. M. Garden of the N.Z. Agricultural Institute gave me extensive information on a program of impact loadings, well instrumented and recorded into force, extension and timerate, which have repeatedly shown that for a time interval about 0.005 secs. MS to BS 15 has under impact loading shown linear rise of stress to a yield pt at 28.2 tsi and from these a very flat second line reaching an ultimate of 32.5 tsi or higher over elongations about 20% on 63 cm lengths. Static tests gave f_{yL} - 21 tsi f_{yu} 22 tsi, f_{ult} 31 tsi and 26.4% elongation on 8" gauge length.

THE AUTHORS STATE that since the preparation of the paper several dynamic tests on load-bearing masonry panels have been conducted using the MTS equipment at N.Z. Pottery and Ceramics Research Association, Lower Hutt. Comparison with the equivalent static tests indicates a more severe stiffness degradation and load deterioration property for dynamic cyclic loading at 1 Hz even for the most flexural case. This is contrary to normally accepted opinion regarding use of cyclic static tests of structural elements as a conservative basis for application in aseismic design which is based on the knowledge of higher steel yeild loads associated with rapid strain rates. However the larger deterioration experienced in the dynamic tests is probably due to a different cause. A major factor in load deterioration of masonry walls is the dislodgement of particles from the walls at the reaction corners. In the dynamic situation the wall motion assists in this dislodgement.

M. J. N. PRIESTLEY (Lower Hutt)

1. The test walls were directly laid on a steel base rather than a reinforced concrete base beam. As the mode of flexural failure, and perhaps to a lower degree shear failure, is influenced by lateral constraint conditions in the region of maximum compression, do the authors feel that perhaps a non-representative situation may have existed in their tests, inhibiting or retarding the formation of vertical splitting. Would it be possible that by this artificial constraint structural integrity was preserved longer than might occur in practical situations.

2. Even distribution of flexural reinforcement, rather than concentration at the ends of the wall, does not for normal wall reinforcing ratios affect the ultimate flexural load, but provides a gradual curving of the load-deflection behaviour rather than an elastic-plastic response. Could the authors comment briefly on the desirability or otherwise of this behaviour in the resistance to (a) moderate earthquakes and (b) major destructive earthquakes.

THE AUTHORS STATE that they agree that artificial 'end' conditions did exist in the tests where steel bases were used. While believing that the steel bases tended to constrain the mortar at the base and inhibit vertical splitting of the bricks, in many cases the vertical splitting was initiated in the first mortar bed above the base. Accordingly it is considered that the basic mode of failure was not appreciably affected. Further the tests were deliberately restricted to the behaviour of the wall element and it was felt that a very repeatable condition for all tests was obtained with steel without the problems of differing deformation characteristics of concrete possible from test to test. Crushing or cracking of the concrete within the base could well have proved to be a dominant action quite unrepeatable. The rotational deformation of the base element, in particular, will have a considerable effect on wall behaviour. In practice, it is likely that the deformations of the base material will provide a greater ductility capability to the system. It is true that even distribution of flexural reinforcing will provide a gradual curving of the load-deflection response. In the dynamic computer analyses conducted, the authors feel that this would have little effect on the ductility requirements as once yielding has occurred the "models" are independent of the shape of the virgin curve. It will be assumed that the ultimate flexural load of two walls, one with even distribution of flexural reinforcing and the other with reinforcing concentrated at the ends, is the same. A moderate earthquake will be taken to be of such intensity as to cause yielding of the outermost bars only in both cases. Due to the curved nature of the load deflection relation, for the same energy absorption a lower load level is required and so the even distribution will be preferable as it will result in less deterioration. However in a major destructive earthquake requiring yielding of all bars, with the steel concentrated at the wall ends, the steel strain for a given total energy absorption will be less. In both cases even distribution of reinforcing will provide better crack control and ensure structural integrity.

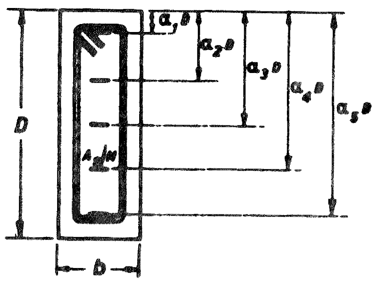
DUCTILITY OF PRE-STRESSED CONCRETE MEMBERS - R. W. G. Blakeley and R. Park. Vol. 4, No. 1. March 1971.

N. W. ALLARDICE (Auckland)

The approach to the design of seismic resistant prestressed members proposed at the last F.I.P. Conference sets two criteria for design. The first is that under moderate seismic disturbances there should be no significant loss of prestress which would reduce the serviceability of the structure. The second is that under a severe seismic disturbance the structure should survive and be repairable. It would be helpful in comparing these criteria to the experimental results given if the authors could indicate the permanent strains and loss of prestress in the tendons for the test specimens with three or more cables as indicated on Fig. 7. The points of interest would be the cracking moment, and a series of intervals covering the range of rotations up to complete loss of prestress in the various cables.

THE AUTHORS STATE that significant strains for the moment-curvature relation of a beam with five tendons have been plotted on Figure 7 in Fig. A. It should be noted that the curves are analytically derived but should give a good indication of likely experimental values. The point of cracking (M_{cr}) and the curvature when the stress in the top tendon is reduced to zero under monotonic load ($\epsilon_{s1} = 0$) have been noted. However, the latter point does not necessarily indicate that this tendon will suffer a permanent loss of prestress when the external moment is returned to zero because during both the loading and unloading the strains in this tendon remain in the elastic range. Should the elastic limit of strains be exceeded in tension for any of the tendons during the course of loading, there will be a permanent set and consequent loss of prestressing force when the external moment is reduced to zero. The point at which inelastic strains commence in the bottom tendon has been marked in Fig. A (ε_{s5} = elastic limit). Thus any curvatures greater than this will result in loss of prestress during the course of cyclic loading. This is the limit specified by the FIP Commission on Seismic Structures for "moderate" earthquakes. It is of interest to note the two cases of curvature for which particular tendons will be reduced to exactly zero stress when the external moment is reduced to zero. This represents a complete loss of prestress for that tendon due to residual inelastic strains. This situation occurs for the bottom tendon at a strain prior to unloading of $\varepsilon_{s5} = 0.0127$ and a beam curvature $\phi D = 0.0132$. The second case occurs when prior to unloading the beam curvature is $\phi D = 0.0502$ and the bottom and second to bottom tendon strains are respectively, $\varepsilon_{s5} = 0.0336$ and $\varepsilon_{s4} = 0.0236$. When the beam has been unloaded to zero moment the bottom tendon has a compressive stress and the second to bottom tendon this time has zero stress. The ultimate limit state specified by the Commission for "severe" earthquakes is reached when the maximum compressive concrete strain is 0.0035. This point has been noted on Figure A ($\varepsilon_{cc} = 0.0035$). It is apparent that this limit does not represent a point of impending collapse for properly designed members as subsequent large curvatures may be sustained. However, should it be necessary to utilise this ductility in a strong earthquake, crushing would occur in the members with consequent difficulty of repair back to a fully prestressed condition.

NOTE :-



1. SECTION GEOMETRY

No. of tendon positions, N	1	2	3	4	5
α_1			0.1	0.1	0.1
α_2		0.2		0.367	0.3
α_3	0.5		0.5		0.5
α_4		0.8		0.633	0.7
α_5			0.9	0.9	0.9
A_s/N sq.ins.	1.435	0.718	0.478	0.359	0.287

p = 0.00896 for all cases.

2. MATERIAL PROPERTIES: f'_c = 6000 psi.
f_{su} = 249,000 psi.
ε_{su} = 0.04

3. Model 3 stress block; Z_{core} = 38.2, Z_{cover} = 80.0

4. Analysis was based on $7\frac{5}{8}$" x 21" section with $1\frac{1}{2}$" cover to the stirrups.

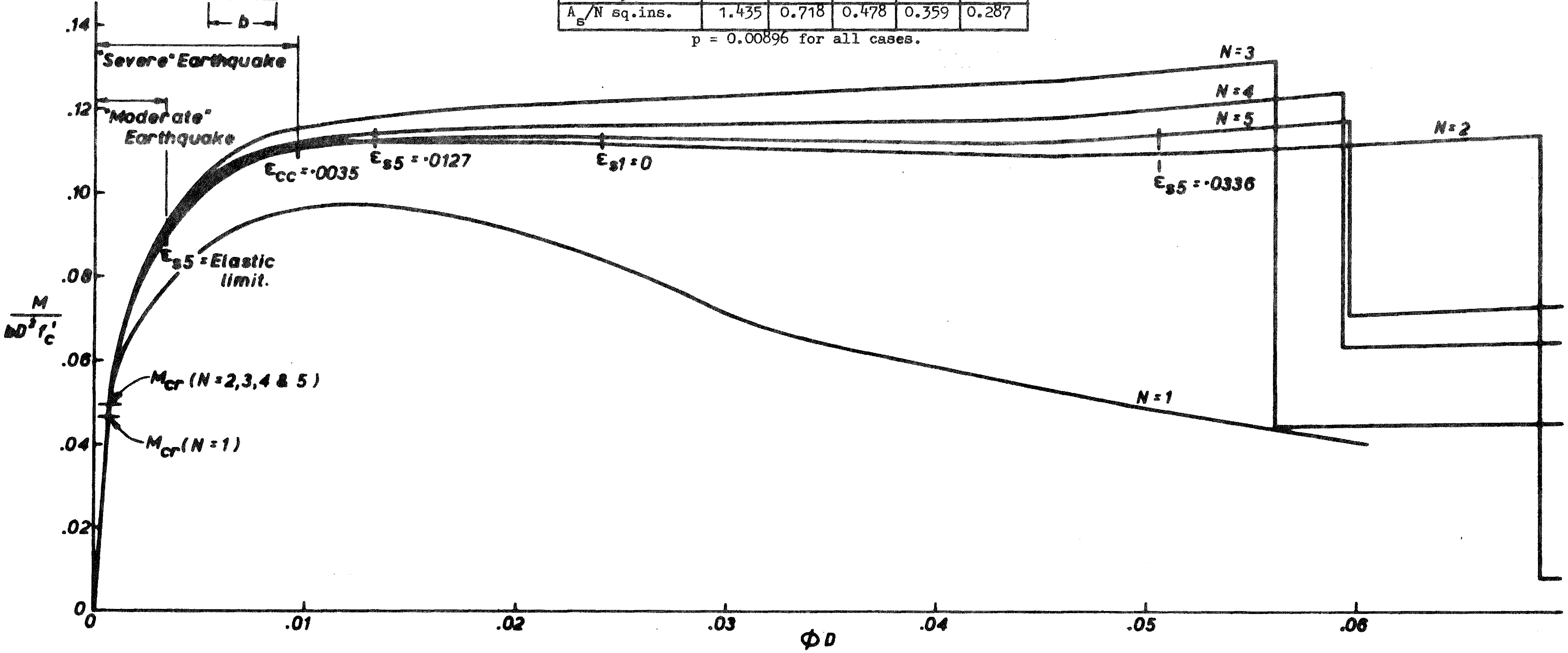


Fig. A : Stages of Development of Inelastic Strains in Prestressed Sections.