Discussion on the 5th working session

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DISCUSSION ON THE 5th WORKING SESSION

Chairman : Dr. L.S. BEEDLE

L. S. BEEDLE :

We start with the first paper, which was the paper by Mr. Sfintesco. I would ask a question : if a new series of shapes were introduced, or if the steel industry developed a new steel that had a significantly different yield point or a new process that would change the residual stress characteristics, what then would be the approach ?

D. SFINTESCO :

This is a rather difficult question. This new type of shape should be a little better defined in order to give you a more precise answer. I just told in my presentation that, as far as another type of section can be considered to belong to the same population, statistically speaking, it is quite easy to determine a relatively small number of tests which could be accepted as an addition to the first basic investigation. I am afraid I am not prepared to define now where the limit is, or where the subject will be so far from the basic investigation, that it would require a completely new definition. I wonder if Mr. Strating would have any comment on that ?

J. STRATING :

In relation to the experimental curves, right ? Well it is not necessary for the industry to develop a new shape because, as I have shown, only a limited number of sections were tested in the experimental program and we did some additional tests on HEM 340 sections in Lehigh just to see what happens when the sections get bigger. Well, I overlooked one of the concluding remarks in my presentation, in fact part of the reason why the Monte-Carlo method was explored was to develop means to include new shapes and new steels on a consistent statistical basis. I am not proposing at this colloquium that this is the only way we have to do it in the future, it was just an exploration of the possibilities of this kind of approach. It seems to me possible, by minor changes in the probability density functions of the variables involved, eventually followed or completed by relatively inexpensive measurements on sections instead of going into real buckling tests that we can generate buckling curves on a probabilistic basis for various shapes. Some more comments will be made later about the two papers that actually treat the same subject, Bjorhovde's paper and my paper, and we will come back to that when we are discussing those two presentations.

Ch. MASSONNET :

I think that your question, Mr. Chairman, is a very interesting and fundamental one and that there are here at least two types of people : those who consider that European work as been a statistical work supported by a simulation on computer, and those who think that we have made a simulation on computer supported by experimental work. Well, I shall not dispute about the two categories. I should personally lean towards the second approach, but your question brings me to tell you that we have precisely this problem in Belgium, because some of our steel plants are developing now new types of steel and I would answer as follows. We have simulated the behavior of this new columns on the computer by taking as much information as we could regarding the stress-strain diagram, and eventually measuring residual stresses and if we can -and we have done that- control the results of the simulation by a rather small number of tests, more precisely by two families of 8 columns each for the two critical slenderness ratios of 90 and 50, we would consider that decent enough for introducing this new buckling curve into the Specifications.

D. SFINTESCO :

I think there is no matter of playing with words and saying one approach is supported by the other one, but, in my opinion, from the beginning when we started with this experimental approach with statistical interpretation we have made it as a support for a theoretical analysis so I fully agree with you and I am also on the second side. We never have thought that everything could be solved in this way but the experimental research on this basis should give two points. The first was to ensure that theoretical investigation will fit with the test results, and the second was the aspect of the factor of safety which as you know, in most of the curves was established in a more or less arbitrary manner and with variability along the curves. This was the main purpose to support the theoretical investigations and to attain a consistent degree of safety.

L.S. BEEDLE :

Let's go on to the next paper, Mr. Tebedge's paper. I would ask a question since the title has "heavy" how do you define heavy ?

N. TEBEDGE :

Actually we are not defining explicitly the term "heavy" as far as this program is concerned ; there was no need for it. We simply followed what has already been defined, and as far as the latest proposal on this definition is concerned, it is primarily based on the thickness criterion. Columns with flanges more than 30 mm or 1 1/8" are considered "heavy" along with the width to depth ratio according to European Convention practice. For example if the depth to width ratio is less than 1.2, it is considered light. Thus, two factors determine the choice of the appropriate column curve ; namely the thickness and the width to depth ratio. As far as this study is concerned, it has been shown that this is not really a sufficient criterion to determine whether a column is heavy or not.

G. SCHULZ :

I would like to comment on Tebedge's remarks on European definitions of heavy and light sections. The ratio height/width of 1.2 which he mentioned, does not define heavy or light sections at all. It defines two groups of rolled I sections. The group with a ratio smaller than 1.2 has more unfavourable residual stresses than the group with the ratio larger than 1.2. This refers strictly to the profiles listed in Euronorm with a flange thickness up to 40 mm.

In a European sense the column tested probably was not a heavy column. What we would define a heavy column would have a flange thickness higher than that of the test specimen.

N. TEBEDGE :

Then, what is the thickness for defining heavy ?

G. SCHULZ :

Well, the sections listed in Euronorm end up with a flange thickness of 40 mm. It was agreed to define as heavy shapes those which have a flange thickness greater than 40 mm.

I also would like to comment on your remarks on the discrepency between test results and the column curve at $\lambda = 50$. I would like to question whether the statistical evaluation of the column tests was done correctly and I would like to direct this question to Dr. Carpena. As you will remember, the tests were made at two slenderness ratios, $\lambda = 90$ and $\lambda = 50$. A total of 8 specimens at each slenderness ratio were tested. At $\lambda = 90$, where the influence of residual stresses and out-of-straightness reaches its maximum, the test results were close together, and mean value minus or plus $2\times$ standard deviation covered a very small part. Test results and column curve at ($\lambda = 90$) were in good agreement. Just opposite at the slenderness ratio $\lambda = 50$. At this slenderness ratio the six lower test results were very close together and two results were very high and quite a distance of the main bulk. This distribution is very different from that at $\lambda = 90$ but the same law was applied, mean value minus $2\times$ standard deviation, assuming that the distribution is still a Gaussian distribution. The two very high test results caused quite a standard deviation. As a result, the experimental buckling load, calculated as mean value minus $2\times$ standard deviation undercuts the theoretical curve quite a bit. And my question to Dr. Carpena is if for a distribution like that at $\lambda = 50$, the assumption of a Gaussian distribution is still valid, it obviously does not interpret the test results correctly.

A. CARPENA :

As far as I remember, the statistical test, adopted in order to check if the distributions of the experimental buckling stresses were or not normal, confirmed that they were normal. This conclusion is true for the distribution of the buckling stresses at the slenderness ratio of 50 and also for $\lambda = 95$; and it is true too if we take away the buckling stresses of the Italian columns.

In these conditions it seemed to us quite correct to accept the safety criterion of ECCS, i.e. the mean value of the experimental buckling stresses less two standard deviations as the ultimate stress to adopt for the design of columns.

Why did we include or not the results of the Italian beams ? Because their yield point was around 21 kg/mm^2 (against a range of 23 - 25 for the other European columns) which is less than the minimum of 22 kg/mm^2 required by EURONORM.

D. SFINTESCO :

I would like to add that, for the selection of the specimens for this experimental investigation, we have of course taken the samples with the usual variations in every respect, dimensional, crookedness, and material characteristics.

But we have put some limits which were intended to be the conditions under which they would have been rejected for use in structures. For instance when the dimensional variation or the value of the yield point were beyond the tolerances of the standard the members had to be rejected. And I think in this particular case at least one should have been rejected or not be included in this interpretation. That is the reason why it is out of line.

N. TEBEDGE :

Actually what we presented there was what we simply observed. We are not to blame or be congratulated for closer agreement so what you see there is just what has been observed.

O. STEINHARDT :

There may be a difinition given by the quotient between circumferential length and area of a cross section in relation to the rolling, welding, and cooling process and also the yield question may be touched here.

N. TEBEDGE :

All relevent cross sectional dimensions are given in Fig. 2.

L. S. BEEDLE :

I think we had better move to the next paper, the paper of Bjorhovde and Tall. I would ask a question, to start the discussion, just to clarify the final conclusion. If I heard you correctly out-of-straightness was a more important parameter than residual stress. Now does this apply to the whole family of column curves of all cross sectional shapes ? Thinking in terms of the significant variation in column strength, as between one that is welded with UM plates and one that is welded out of flame-cut plates, it sounds rather strange that out-of-straightness is more significant than these variations.

R. BJORHOVDE :

Dr. Beelde's question is well taken. As I mentioned in my presentation, the shape of the overall residual stress distribution is of course of the utmost importance for the column strength, and that is why welded built-up columns with universal mill plates have so much lower strength than, for instance, flame-cut ones. This is one of the examples which is illustrated in the study. The random variation of the residual stress that I mentioned is indicative of the ± variations of the residual stresses measured in many samples of exactly the same shape.

L. S. BEEDLE :

Were your calculations based on tangent modulus or on maximum strength and why do you use one or the other ?

R. BJORHOVDE :

The computations were based on the maximum strength of the column, and the initial out-of-straightness therefore was included as an important factor. The maximum strength approach was chosen because the initial out-of-straightness is always present in real columns, and a fraction of realism thereby was added to the method of solution.

L. S. BEEDLE :

Any other questions on this paper ? We will then go on to the paper of Mr. Strating. I have a question here : you went to the trouble to measure the end eccentricities and the out-of-straightness if I understood the slide correctly. Why did you stop there and why did you not measure the residual stresses and their possible variations or the yield points and their possible variations ? Why did you stop at making only some of the measurements in order to complete the calculations ?

J. STRATING :

The answer is that I did not make those measurements. The measurements were all part of the European Convention's buckling program and they had been started somewhere in the 60's and all this information was just available and it seems that, until now, I am the only one who did something with these measurements. For each column that was tested a data sheet was prepared, there were strict regulations drawn up by Committee 8.1 of the European Convention on how the tests were to be carried out and what measurements were to be taken. So each column was measured, the dimensions were measured at five points along the length, like Mr. Tebedge has already mentioned for the tests that were carried out at Lehigh, the yield stresses were determined, I showed the histrograms of the yield stress according to the three methods : stub column, strips taken from the flanges and the webs and according to the Euronorm. The initial out-of-straightness was measured for each specimen and only a limited number of residual stress measurements were done. I did not go into that in my presentation because the time was lacking. It was very difficult to obtain actual values for the residual stress distribution. I was able to find about 10 stub column tests that were carried out at Liege, I suppose by Prof. Massonnet or one of his co-workers, for which the complete load-deformation diagram of the stub columns were recorded. Generally, the stub columns were only tested in compression to determine the average yield stress but Prof. Massonnet did some measurements on the deformations also. What I did was, that is of course a very crude method, to find the stress where the deformation starts to increase non-linearly and use this to get an estimate of the maximum residual stress present in the IPE 160 columns. I also derived the coefficient of variation of those values. I looked also at what other people have done on residual stresses and I quote those in my paper. I came up with an assumption about the residual stresses that was based partly on the grouping done by Dr. Schulz in Graz, in his dissertation, and partly on the measured results I have from the tests of Prof. Massonnet, I adopted the value of .2 times the yield stress. I am well aware that the residual stress is not a function of the yield stress but just for convenience this value was adopted. I assume a residual stress distribution very much like the one that was adopted by Bjorhovde, that is a parabolic distribution in the flanges and a constant distribution in the web. This is convenient because I only considered weak-axis bending, which is the manner the specimens which I tried to simulate in my program were tested. I hope that answers your question. So there was a lot of information available but some important information was lacking. One interesting thing came up when I drew up those histrograms when considering the initial out-of-straightness of the columns. The initial out-of-straightness was also measured for each column on both flanges, each country and each laboratory used another method for that measurement. The results of these measurements prove that we never actually looked and dissiminated the information because the histrogram shows that initial out-of-straightnesses are present larger than L/1000, they were present in columns that were tested. Well of course you can say that when we get beyond the tolereances that are given in our regulations we should reject the specimens, that is one point of view. On the other hand I'm not so sure that this will always be done in practice. I wonder whether, if columns get to a shop and are being welded onto, the tolerances will be kept, therefore I think it is not a bad thing to have this effect included. I was very fortunate, some days ago, to read the complete thesis of Dr. Bjorhovde and discovered that we found *practically the same kind of distribution for the initial out-of-straightness. He adopted an extreme value probability density function and I adopted a normal one but that's more for convenience because I had to carry out my simulation by generating random numbers and combining those, I was a bit pressed by time so I chose a normal distribution function which is very easy to generate on the computer because there are generally standard procedure available. I understood from Dr. Bjorhovde that he was very glad that I had found the same distribution as he had. We both found a peak and steep fall off at L/1000 so that is about the shape of the distribution, I also found more or less the same mean values and standard deviation as he had. Now I want to make a comment on both papers because they seem to treat the same subject. If you will have time later on to look a little more closely at the paper I presented and the paper that Dr. Bjorhovde presented you will find that in my paper I discussed three different approaches to find the lower bound curve in buckling. The second approach employs the function that describes the carrying capacity of the column as a function of numerous variables, by a Taylor expansion you can carry out a linearization, just like Carpena showed for the yield stress at slenderness ratio $\lambda=0$, Dr. Bjorhovde adopted this approach. That is one method to obtain a probability density function, I adopted another approach. I had an interesting discussion with Dr. Bjorhovde yesterday at the cocktail party, I hope he still remembers it. I suggested that what we should do in the near future is to calibrate our maximum load computer programs. We can adopt one particular section with the same dimensions and the same imperfections and the same mechanical properties and see if we come up with the same maximum loads. This will show whether the computer programs are comparable because we both use maximum strength theories but he has his simplifications, I have my simplifications so we will see what that adds up to. Then the next stage would be, and that is what I am very interested in, to adopt again a particular section for example an IPE 160 or any other section, adopt a set of values for the dimensions of the section and for the probability density functions which correspond to the various parameters like initial out-of-straightness, residual stresses, the shape of the residual stress distribution, yield stresses etc. They do not have to be realistic values, they can be hypothetical just as long as we both use the same assumptions. Then I will generate a Monte-Carlo curve and he will generate his column curve spectrum and we will see whether they compare. We can compare directly both methods because his paper and my paper are treating exactly the same subject. Dr. Bjorhovde's approach is statistical but it is based on some dubious assumptions. My approach is not, it is in a statistical sense much easier because I do not have to do any difficult statistical computations. I just generate numbers, find the histrogram and fit a curve to it, and then I have the shape of the distribution function. But I am very interested whether his method and my method come up with the same answer because I have some reasons to believe that he may be using less computer time than I and the computer time involved may be a restricting factor in Monte-Carlo simulation. Just for information, I can tell you that generating the buckling curve I have shown costs about 90 minutes computer time. So even if there are some slight differences between the two methods it may lead to accepting Dr. Bjorhovde's kind of approach. I have already suggested in my paper that it may be worthwhile to investigate the method of the linearization but we just have to make sure that we do not get too significant errors. I had another 10 minutes presentation Mr. Chairman. Thank you very much.*

L.S. BEEDLE :

Well I would say this is at least as effective as your presentation. That was an excellent discussion. I wonder, we had better let Dr. Bjorhovde have the first response there.

R. BJORHOVDE :

I agree with Mr. Strating that a comparison of the two methods of solution would be very appropriate. As far as I can see, there are merits to both approaches. Mr. Strating's method may be easier to work with initially, since that in generating the density functions for the maximum strength one does not have to go through very complicated probabilistic mathematics. On the other hand, he will have to perform a vastly much larger number of computer runs to arrive at the same amount of data that were acquired in my study. Therefore, in the end I do believe that my approach accomplishes a good deal more. Having had to keep an eye towards developing a set of multiple column curves, one needs the probabilistic characteristics of a large number of different shapes made by the various manufacturing methods, steel grades, and so on. This is where my approach comes out better, since when a large number of what I have called column curve spectra have to be developed, one may run into excessive amounts of computer time, and the time needed to interpret the results also increases drastically when the results are available only in the form of single runs like those of Mr. Strating. I might mention that I did consider using the Monte-Carlo approach for my studies, but soon discarded it because it proved to be quite inefficient for my specific purposes. An added complication here is the fact that my computer program utilized actually measured values of the residual stresses, the geometric properties of the shapes, the yield stresses, and so on. The only factors that were assumed were the magnitude of the initial out-of-straightness and its probability density function, but these data were correlated with and substantiated by test results from other investigators. My experience is that when one is using actually measured values, convergence problems sometimes arise in the computer run. This happened especially with the heavy shapes. On the other hand, measured values form a more realistic basis than assumed ones. Another item of interest in this connection is that in order to generate a column curve spectrum for a typical shape, that is, a set of curves that illustrate the random variation of the maximum column strength throughout the full range of practical slenderness ratios, a computer time of between 10 and 40 seconds was needed on Lehigh University's CDC 6400 computer. I might add that the CDC 6400 is a fast unit, indeed. The spectrum gives the random variation of the strength of a particular column type in a given steel grade.

I also would like to comment on Dr. Cornell's work, some of which may be tied directly into my studies. I think it serves his work great credit that he has considered what he has termed the error of the theory, and this has been included as a random variable in his analysis. In fact, the computations that were done with the computer program I was using were compared with a number of column test results for different rolled and welded H-shapes and box shapes, and the theoretical computations proved to be accurate to within 5 %. This means that the theory that was being used is accurate to within approximately 5 % of the experimental values. The 5 % deviation is first of all indicative of the error in the theory. It is also indicative of the error in the testing procedure, because there are some test factors that are uncontrollable. For example, in real life one does not know exactly what constitutes a pinned-end column, and as we know even a very small amount of end-restraint will lead to a higher apparent tested column strength. Such an end-restraint can, for instance, be introduced by having pinned ends that are not moving completely freely. The alignment of the column in the testing machine also is important, since even a small amount of end eccentricity will reduce the maximum strength. These are but two of a number of factors that need be considered. Dr. Cornell's considering both the error in the theory and the random variation of the column strength parameters is therefore indeed a worthwhile effort. A final comment to Mr. Strating : my method of analysis is certainly of a probabilistic nature : the use of assumptions is quite irrelevant, as long as the column strength factors are treated as random variables and incorporated as such in the analysis.

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W. HANSELL :

I would like to make several comments. First of all I believe it would be correct to describe the 5 % figure that Dr. Bjorhovde just gave as more of a mean error rather than the largest or range of errors between theory and experimental comparison. A second point, we have had some discussion an initial crookedness and values that may exceed specification tolerances and the question of whether they do or do not get into buildings. This begins the focus on the real problem, the column in the building, and I propose that the place where initial curvature should be measured is on erected columns. I would expect under some circumstances to see some significant differences between initial curvature measured as the shape comes off the straightening process and the shape as it appears in the building. In particular it is common practice in the United States to erect columns in two story tiers or more. L/1000 for a column that runs for two or three stories is a lot larger than the initial curvature of that column between floors when it is erected in the building. Lastly I would like to endorse as a very useful comparison the suggestions and comments of Bjorhovde and Strating on comparison of deterministic maximum load programs and then a statistical comparison of the maximum load confidence intervals or boundaries established from a theoretical analysis of available statistical data. I would also like to suggest that perhaps Dr. Carpena would be interested in participating with the other two institutions in such a comparison.

L.S. BEEDLE :

We had in the presentations at the Japanese Regional Conference on Tall Buildings, a little over a year ago, some of the first good figures I have seen on the actual out-of-straightness of members as they finally end up in the building and that's what counts.

J. STRATING :

I am glad to hear that Bjorhovde needs only 40 to 50 seconds computer time to compute curve spectra. So that as far as that's concerned there will be no problems in getting this comparison done because it will not cost much money on your part, it costs more money on my part but I am prepared to carry these costs. I want to make a remark about future work, right at this moment we are adapting our computer program to include end restraints and we are collecting data as to what kind of amount of end restraint you can expect in a column which is executed as a pinned ended column. We will include the end restraint also as a random variable in the pinned ended columns and see how much it increases the load carrying capacity of the column. This will be ready in not too long a time.

L.S. BEEDLE :

Now let's go to questions on Prof. Galambos'paper.

R. BJORHOVDE :

Am I right in understanding that the safety index of 4 was adopted on the basis of a committee decision ?

T.V. GALAMBOS :

At an informal committee meeting.

R. BJORHOVDE :

Now, the safety index is indicative of the probability of failure. As far as I can recall, an index value of 4 would correspond to a probability of failure of approximately 1/10,000.

T.V. GALAMBOS :

Yes, but I think this is something that has to be looked at, after you look at the types of calabration that we performed, and then some people around the table will have to decide which is which. That is not an easy thing to do.

W. HANSELL :

I would like to comment that, for the first time at our session here, we have seen an attempt to look at the column problem in a relatively complete manner in which the many sources of variation in resistance have been combined with estimates of variation in load as it occurs in buildings. It is not until we are really able to look at the complete load and resistance problem for columns in buildings that we can get a reasonable estimate of structural safety or structural reliability and I believe that is the strong point of the study that Dr. Galambos is talking about. With regard to safety index values, the project at present is in a research phase. We are certainly not now at a stage where we are ready to adopt for design pruposes any one particular safety index although our calabrations to current design seem to indicate values on the order of 4 1/2 to 3 1/2 for β. There is also some recently presented work that throws into some question the idea of using safety indexes as an approach for structural reliability. I am referring to recent work by Ditlevsen which needs considerable evaluation at this point but does suggest that numerical safety index values may or may not be valid criteria for structural safety.

O. STEINHARDT :

To speak about this load factor and resistance : are imperfections, structural and geometrical ones and so on, part of loading or part of resistance ? The load factor problem in the smaller boundaries is a problem of ponderation but you cannot divide the imperfections in the real way reducing to geometrical ones or so.

E.H. GAYLORD :

I think I heard Mr. Bjorhovde say that the safety index of 4 corresponded to a probability of failure of 1 in 10 000. If the safety index of 4 was determined by calabration as it was with our present design procedures something does not seem to click here because it seems to me we would have seen many more failures than we have of structures in practice if we have been designing all these years on the basis of a probability of failure of 1 in 10 000. So where is what I am missing here that does not seem to make the probability of failure realistic if the safety index is 4 ?

R. BJORHOVDE :

I believe that the value Dr. Gaylord quoted is what I said. On the other hand, it is a purely theoretical measure, and I am very doubtful whether it really can be related to actual structural failures. It is a measure by which a family of different structures can be compared on a similar basis.

L.S. BEEDLE :

I open the discussion to any of the papers of this session.

M. MARINCEK :

It is very clear that in real life of structures we have to think probabilistically. Inspite of that I would like to put the question : "Do we still need a reasonable defined minimum guaranteed carrying capacity of the structure, for example in our case for the instability of a column ? "This minimum guaranteed value is dependant on the maximum allowable unfavourable geometrical tolerances, minimum guaranteed yield point of the chosen steel and on unfavourable but normal material imperfections with regard to residual stresses and nonhomogenity of the yielding stress. If then in the reality we have an indication for a lower instability load then is the minimum guaranteed one, this should be somehow penelized and if the value is higher, this can be sometimes positively exploited. I would kindly ask our highest specialist for the probability to give the opinion about this.

L. S. BEEDLE :

He knows who he is because he had his hand up before you described it.

J. STRATING :

Thank you Prof. Marincek. Mr. Chairman, Gentlemen, I would like to point out one thing. Every time we start talking about probabilistics in structures, we have seen this at the Tall Buildings Conferences that we had in the last two years, we have seen it all the time when we have meetings in Holland that when we are talking about probabilistics people are very eager to look at certain figures like the 10^{-4th} and 10^{-5th} that Galambos just mentioned as figures saying that one of every 10 000 structures will fall down or one of every 100 000 structures. Well fortunately this is not the case. These figures have to be included in the probabilistic approaches. The reason for adopting some kind of failure risk is to arrive at a more consistent safety in our structures. We are not saying that these are actual failure values for our structures, we just have to adopt the figure and work with it. We are all aware that we are talking about elements in structure and we know very well that if we consider a beam in a structure and compute the failure probability of this beam that the actual probability of failure is much smaller than the adopted probability of failure. You have to look at the probabilistic approach as an attempt to have consistent safety in our structures and not give too much credit to the actual failure rates that are being discussed. It is a psychological question.

D. MATEESCU :

Concerning the range of small slendernesses, as has been shown even in figure 7b of the paper by Tebedge, Chen and Tall, the instability phenomenon is not a column buckling but rather a plate buckling. Now, between these two phenomena there is a qualitative difference. Column buckling is relatively sudden and defines a critical load, whereas plate buckling does not. I consider that the σ λ curve should be stopped at the yielding stress for those values of λ which introduce column buckling for the first time. Theoretically a link with the buckling stresses of the stubs were possible, as it is done for instance in the analyses of thin-walled bars, leading to a kind of unification of these two types of structures.

N. TEBEDGE :

I will try to give a very short answer to a very long question. As far as a stub column test is concerned we test only up to 3 x 10 -3. Whereas the deformed shape shown in Fig. 8b resulted after a strain of about

100 times of the yield strain simply for a matter of interest. So that the question of plate buckling does not come into the picture at all. In short, as far as the column test is concerned the most valuable portion is shown in Fig. 8a.

L. S. BEEDLE :

Just to repeat the same point, the wrinkling of the plate does not occur until well after the plastic plateau ?

T. BARTA :

I would like to comment on the discussion between Dr. Schulz and Dr. Carpena. We did some tests very similar to the European program on small models at University College and found very similar results.

If you compare the test one should really know what the stiffness of the various machines in the various countries were and this might change the results. However, it is to be expected that in this unstable post-buckling behavior range the scatter of results will generally be larger.

T.V. GALAMBOS :

I want only to make a brief comment to what Prof. Marincek had asked for. I am not eloquent enough to describe the questions with relation to probability, and these is not enough time. But one can read the Introduction to Committee 10 in Tall Buildings Reports, a summary by Prof. Cornell I think, that it does about as much justice as I have seen anywhere and it is well worth reading.

L.S. BEEDLE :

Thank you all for participating in the presentations and the discussion.