# Discussion

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DISCUSSION ON THE 1st WORKING SESSION

Chairman : Prof. Ch. MASSONNET

Ch. MASSONNET :

*Now we are coming to the discussion of the 1st Working Session. I would propose you to discuss the papers in the order they have been presented. First relating to the paper of Messrs. Chen, Tall and Tebedge.*

L. FINZI :

*Speaking about heavy welded columns, I think that the size of the weld is a matter of great importance, referring to residual stresses. Please can you tell us about the minimum size that you need for the weld to avoid local failure during the tests ?*

W. CHEN :

*Well, as you have seen on this heavy shape, which has a 3 1/2" thickness, we used only 1/2" weld and, in no case, there was any kind of buckling. So, for heavy columns, you can make the weld as big as you wish and buckling will not be a problem and as small as 1/2" will not be also too small to hold these plates together.*

T.V. GALAMBOS :

*I would like to ask Dr. Tebedge to tell me where the inflection points in the column during testing were, in relationship to those two pieces added to make the columns longer.*

N. TEBEDGE :

*The inflection points during the testing were found to be in the order of 0.5 of the column length, which were within the junctions of the supplemented segments. The inflection points about the major as well as the minor axes, according to measured values, are given in the paper presented.*

Ch. MASSONNET :

*Coming to the same problem, I would like to have more information on what flat end condition is . Is it the steel plate from the testing machine ?*

N. TEBEDGE :

*Yes, there were two steel plates at both ends. At the lower end it was supported by what may be regarded as a really rigid support; thus, there were no rotations observed throughout the test. However, at the upper cross head of the machine it has been observed to rotate about the minor as well as the major axis. These values have been measured and are given in the paper.*

Ch. MASSONNET :

*Considering that you have beaten the world's record for buckling in this case, would it be possible to improve the end conditions ? I understand that it is very difficult to install knife edges or any kind of movable edges but it is, to a certain extent, a pity that the end conditions, especially at the upper edge, were not better defined.*

N. TEBEDGE :

*I agree completely with your point. Originally it was intended to test the column as pinned-end column, but for this particular heavy shape it was found that it would be quite expensive to prepare an end-fixture. Thus the only alternative was to use a fixed-end condition. Unfortunately, at the upper cross-head some end-rotations were measured and we had no way of restraining it. Therefore, for heavy columns, unless one is ready to prepare pinned-end fixtures, one may be forced to use fixed-end conditions.*

Ch. MASSONNET :

*I wonder whether it would be possible to compare these tests with simulation obtained through the computer, with the Batterman and Johnston procedure.*

N. TEBEDGE :

*I would like you to know that we also have made a prediction at the theoretical strength obtained through the computer in order to make a comparison to the theoretical results of the particular column. The program has additional features other than the one you had mentioned : it can handle also biaxial bending problems, and the variations of residual stresses and material properties throughout the section can be accounted. This program has been particularly suitable since the column failed in biaxial bending.*

Ch. MASSONNET :

*Are there any other questions on this first paper ? As it does not seem so, I shall pass to the second paper by Dr. Young, England. Dr. Young, if I have understood you correctly, you said that you obtained the same patterns of residual stresses either from the NCS (not cold straightened) or the CS conditions. Now, because we had been interested in Liege in the effect of cold straightening, I cannot understand that. Could you comment this a little more please ?*

B.W. YOUNG :

*This is an interesting point because it touches on the remarks that Dr. Alpsten made. The particular section I showed there was a 16-inch beam, universal beam section, and the same piece, as I explained, was used. Part of it was cut off for the NCS series and the rest of it was passed through the gag press. It wasn't passed through the rotorizer. Now the point I want to make is that when the gag press is used, it is possible that certain sections of the beam do not receive any plastic working and it just so happened that I took a piece of the gage length which didn't receive any plastic working. Now if the section had been rotorized, it is almost certain that there would have been considerable* *redistribution of residual stress. Yet on these bigger sections, it is possible to find a length which hasn't been redistributed in this way. It seems to me that the safe thing to do is to apply one's analysis to the undisturbed residual stress pattern. Dr. Alpsten's results, of course, were on sections which were deliberately rotorized and received quite a lot of redistribution. I also found on some similar series of small sections, namely the 8WF31 size, which had been rotorized, that there was quite a considerable redistribution of stress.*

L.S. BEEDLE :

*I think then on that point we would have to be careful about concluding that there was no influence of cold-straightening. I agree that if the cold-straightening in the gag is at remote points, then you could get a column strength that was the same as the column as delivered. But it is possible that the gag be operated to fairly continuously cold straighten the column ; the first work that, I believe, was done on this, which was done by Huber, was actually a process that involved gag straightening, and the column, just as Dr. Alpsten predicted, was considerably stronger that its as-cooled counterpart. I would like to ask another question. In connection with the cooling patterns that were measured, you showed tension in the flange tips. Did plus then mean compression ? I just thought that you had a pattern that was the reverse of what one expect both experimentally and theoretically.*

B.W. YOUNG :

*No, it is just my peculiarity that when I am dealing with compression I choose to take that as being positive. The diagram is then the opposite in sign to that more commonly adopted. In reply to the first point, I am not saying that residual stresses are not redistributed. As I pointed out, in this particular case it is possible to pick a section which has not been treated in this way and it seems to me that if a section can get through the mill and be delivered in this particular condition, at least one is on the safe side if one makes the assumption that residual stresses have not been redistributed.*

Ch. MASSONNET :

*I could perhaps comment briefly on the last point you said because one of my assistants made a theoretical investigation about the changing of the pattern of residual stresses due to cold straightening and then we simulated on the computer such bars with the new pattern of residual stresses and we found that in any case, that buckling load was higher than that for non cold straightened. This is in line with what you said.*

T.V. GALAMBOS :

*I don't want to dispute what you said but on certain shapes, for example solid round shapes, exactly the opposite can be true. The work done by Nita about 10 years ago at Lehigh indicated that if you have cold straightening residual stresses introduced into solid round bars, you can get unfavorable residual stress distribution also. So you have to be careful about what shape you use.*

Ch. MASSONNET :

*I apologize Prof. Galambos, what I said was just for double T profiles and not for other shapes.*

L. TALL :

*I would like to suggest to Dr. Young that British rolling practice is actually not different from that anywhere else in the world and indeed the residual stresses you measured are essentially identical to those measured on* *the same shape anywhere in the world. I am a little unhappy that you label one of your residual distributions "of the U.S.A. shape," that's the straight line distribution. It is true that 15 or 20 years ago we used that in some of our early work but we aren't assuming straight line distributions any more. I wish you would remove that U.S.A. label of something quite ancient. I would like to reiterate again what others have said, that cold straightening certainly has a definite influence on changing the residual stress pattern.*

B.W. YOUNG :

*I apologize for the ancient assumptions made for that residual stress pattern but the ubiquitous 8WF31 always has been given this peculiar tensile distribution in the web which I thought was very interesting because it seemed to indicate that there might be some differences in the cooling conditions. I take the point that the actual rolling process is likely to be the same. It is possible however, that temperatures may be higher in some cases, in which case the residual stresses may be formed more on the cooling bed than during the actual rolling process. The possible shrouding of the web would then induce tensile residual stresses in the web as opposed to compressive stresses in sections which were non-shrouded in the web.*

Ch. MASSONNET :

*Are there any other questions on this paper ? If not we shall pass on to Dr. Alpsten's paper and on this I would just make myself a short remark, mainly that the profile investigated by Dr. Alpsten HEA200 is the same as the DIE20, the old denomination, and this was precisely the profile that we investigated in Liege for buckling test 17 years ago so that it would be interesting to compare the patterns of residual stresses obtained by Dr. Alpsten on patterns published in a paper that I shall give you the reference.*

T. BARTA :

*I am referring to Mr. Alpsten's paper mainly because one of his diagrams is just the best occasion to put the question but it refers also to all the proceeding papers. He shows in one of his diagrams for a double T profile the variation of various material constants like yield stresses, elastic limits, and so on. So I assume it is a now generally recognized point that these quantities are non homogeneous over the cross section and their non-homogenity is included in the analysis. What I missed unfortunately in this diagram and in all the other papers presented is the non-homogenity of the modulus of elasticity. There is some experimental evidence of this non-homogenity. In England a paper by Stremowitch, and some other people, who unfortunately have made measurements in too few points, indicate a similar variation of the modulus of elasticity not to the same extent but in a probably similar shape. So I would be greatful if somebody could tell me if he has made measurements of this kind and where they are available, and if people have included them in their calculations because this would mean a consistant kind of calculation. The other point is about the boundary conditions in general. I have certain reservations between correlating pin ended columns buckling tests and stub column tests which are tested under completely different boundary conditions. From this point of view I think the experiments reported by Tebedge are interesting because at least the boundary conditions for long and stub were identical.*

T.V. GALAMBOS :

*I would like to comment on the first point that Prof. Barta made. There are some tests that were made on rolled shapes which were rotorized to determine the strain hardening on that modulus, because this is an important parameter in plate local buckling and we discovered this indirectly in frame tests where local buckling occurred prematurely. Subsequently an investigation was made and a paper* *was written on this and I can refer you to this. The material properties do change and this is one effect of rotorizing which I think needs some further investigation.*

L.W. LU :

*We have made studies on the effect of rotorizing on high strength steel beams and also there were similar tests carried out at the University of Alberta by Dr. Adams. Actually the beneficial effect derived from rotorizing process depend very much on how crooked the member was before it went through the process, and in many beams actually the beneficial effect was very limited. As a matter of fact, I don't have any figures here. I do recall that there was small improvement of the residual stress distribution but of very small importance, really. In fact, I look at some of the figures that Dr. Alpsten showed : his predictions show that there was only small improvement and our tests on beam members did only show there was a small change of the residual stresses.*

Ch. MASSONNET :

*Any other questions ? No, then we pass through Mr. Brozzetti's paper and here I have just a short comment myself, namely that the buckling curves that you have presented are from what can be called either the tangent modulus approach of the Osgood-Ramberg approach, neglecting the end effect as well as the effect of the geometrical imperfection. Now we have seen through the work of Committee 8 that the difference between this approach and the results obtained by simulating on the computer the behavior of the bar with all imperfections that have been mentioned maybe somewhat significant, could you comment on these, please ?*

J. BROZZETTI :

*In this talk, in fact, we considered only the effect of residual stresses distribution. We didn't take into account any deformations or geometrical imperfections effect. So in such case if the residual stresses distribution is symmetrical we can use a tangent modulus load theory. But if we have to take into account geometrical imperfections we have to use another model. The tangent modulus load theory does not apply any more.*

Ch. MASSONNET :

*Any other questions ? We shall pass to Prof. Lee's paper on tapered members. Could I ask him whether he has also the same approach as tangent modulus load ?*

G.C. LEE :

*Yes, it is the simplest possible way to get an elastic buckling load. But the problem we had was really how to take care of the tapering. We were using the finite element procedures. The elements are prismatic but change in depth to take care the change of stiffness from element to element. The element stiffness is obtained by following a procedure suggested by Professor Birnstiel of New York University, which is a typical tangent modular concept.*

T. BARTA :

*I appreciate the quality of the work of Prof. Lee, but I have my serious doubts of the way this should be presented into the codes because I think they are fundamentally misleading. Prof. Lee suggests to change the effective length. Now the effective length basically means a change in the points of inflection* *of the deformed column. If we have a double pinned column with a variable cross section or a tapered cross section, this length will not change. What will change will be the general shape of the curve not its points of inflection, and what should be modified should be the cross section so one should use probably a kind of modified cross section. If you come to a more complicated case, not a double pinned column but a column that is fixed at both ends, there would be an interaction so there would be a slight change in the position of the points of inflection and a change in the amplitude of the inflection. Still to put it into acceptable form to the designer which would not distort his understanding of the phenomena, I think what one should do is to affect the real cross-section -meaning the largest or smallest cross-section, whatever one takes as a reference- with a correction factor and not to apply the correction factor to the length.*

G.C. LEE :

*It is true that if you use the concept of the prismatic member the effective length is defined as the distance between points of inflection and in that your comment is true. What we are doing there is not precisely that. What we do is to figure out the buckling strength of the tapered member with the various end rotational and translational restraints applied at the ends. And equating that buckling load to a different prismatic member of a different length. That's the concept we are using. It may be misleading but I submit that it is the only reasonable way to include these end restraints into the design of these tapered columns.*

Ch. MASSONNET :

*Any other questions on the same subject ? It doesn't seem so. So we pass to Prof. Nylander's paper. I have some comments myself on this paper. Well I hope that Dr. Alpsten will answer them. First observation : Prof. Nylander's paper gives only the results but not the theories, so that it is somewhat difficult to form an opinion about the significance of this paper. Second remark : the results and the theory should be compared with the works of Klöppel in Germany, Skaloud in Czechoslovakia and some work we did in a research group in Liege called the SERCOM. Now, what I don't understand really is the very low values of the reduced buckling stresses obtained in Dr. Nylander's diagram. For very low slenderness ratios b/t, tending to 0, they are as low as 0.7, and even 0.5 in certain diagrams. Now, I am prepared to accept that due to residual stresses, second order effects and very thin walls you cannot reach the yield point, but falling as low as half of the yield point seems to be terrible. I would like to have a comment on this if possible.*

G. ALPSTEN :

*Well, as I said initially in my presentation, I am not in a position to discuss Prof. Nylander's paper ; however, to your first question relating to the theory, I think he presented a little about his theory at the Amsterdam conference and a more detailed discussion will be presented in a paper in the near future. The other questions I will bring to Prof. Nylander and he will communicate with you.*

J.B. DWIGHT :

*We have also in Britain been very interested in plate buckling and we have developed a theoretical method for predicting plate strength in compression. We have also tested many box columns welded and stress relieved. We have tested many individual plates and just taking the sum of all this work and considering it we have now produced curves like this, to describe the strength of plates in the same way as we have our multiple curves for describing columns. And these curves are based on different initial crookedness in the plate and as you move down across the band of curves the residual compressive stresses is increasing. These* *are based on quite a lot of work. I am now in opposition, I hesitate to say to Prof. Massonnet because if you plot our corresponding curves on Prof. Nylander's Figure 3, the two dotted ones that you see there are our British curves or Cambridge curves with no residual stress so that should be compared with the top curve of Prof. Nylander and the 0.3 would compare with his lowest one. So it seems that our findings lie a good deal below his.*

Ch. MASSONNET :

*Excuse me, but you say your curves are below his curves. On the other hand, I saw from your papers that you go up to 1 for small values of b/t. So that I don't understand.*

J.B. DWIGHT :

*Along this axis is the b/t ratio and up this axis is the stress divided by the yield stress. In his figure 3 he shows the strengths reaching the yield at lower b/t.*

Ch. MASSONNET :

*Excuse me but if I have understood correctly Prof. Nylander's paper, we have seen on the screen several curves going to the alternate axis with figures very much lower than the yield point and it is this particular point that interests me.*

J.B. DWIGHT :

*In some of the figures I too do not understand that.*

Ch. MASSONNET :

*Perhaps we should refer to Prof. Nylander himself.*

G. ALPSTEN :

*Well I think the curves you are referring to are those where Prof. Nylander has investigated also the effect of column buckling so the fact that the curves don't go up to the point 1 here is the effect of the column buckling not the plate buckling.*

Ch. MASSONNET :

*Another question ? If not we shall pass to the last paper, that one presented today by Prof. Dwight. I shall make a short comment myself about Prof. Dwight's paper. It is congratulating him for his simple and nice formula governing the residual tension force in the weld. Now it should be compared with the other approaches. It should be compared with the theory Dr. Alpsten has developed and presented in Amsterdam at the IABSE congress.*

F. NISHINO :

*I may have missed some of your points Prof. Dwight, but I had an impression that you were talking on the ultimate strength of the plate rather than on the application for columns. For the application on columns, the most important is the stiffness of the plate for compression. Plate may lose significantly the stiffness when the load approaches to the buckling load, i.e., the load analysed by the linearized theory, which may in turn lead to the overall failure of the column. Whereas, if you really solve for the ultimate strength of the plate itself, the stiffness for compression at the maximum point is almost close to zero, and the column may have already failed. Therefore I am wondering whether* *it is really necessary to analyse for the ultimate strength of plates, if you are implying the application to the column strength, i.e., if you are dealing the local buckling strength of the component plates of a column.*

J.B. DWIGHT :

*I am not sure if I follow you Prof. Nishino. I would like to say that I regarded my paper as just a service to the people who are trying to analyse columns. It is an attempt to produce slide rule formula that you can get some idea of the residual stresses. But I am only too well aware of the great scatter that exists. Residual stress is not an exact science.*

G. SCHULZ :

*I would like to comment to Prof. Dwight's remarks that he wanted to provide help to those who develop column design curves. Well, the residual stress pattern which is predicted with this formula can be very unfavourable for some buckling cases as Prof. Dwight already mentioned. One of the reasons is that this approach does not consider the residual stresses which are in the plate prior to the welding process. For instance, for welded box sections, this results in a flat and very wide zone of compressive residual stresses, which is much wider than the actual measurements indicate. Since for welded box sections the column strength does not depend so very much on the actual magnitude of the compressive residual stresses but on the width of the compressive zone, Prof. Dwight's assumptions can lead to a much too pessimistic prediction of the column strength, in particular in the range of low slenderness ratios. As you probably will see in the next session, the British column curves for this buckling case are very low in the range of small slenderness ratios. Thank you.*

B.W. YOUNG :

*I would just like to comment on the H constant that Mr. Dwight used in his formula. The figure given was 0.13. It is possible with very thin plates to get a much smaller value of this constant because of the heating effect of the weld itself on the surrounding plate. What one relies on for the production of the tension zone is that there is a large mass of plate surrounding the weld which is relatively cool. This acts as a restraint rather like the rigid ends on the bar that Mr. Dwight showed in his diagram. Now 36 or 37 years ago Boulton and Lance Martin in England made some tests on welding residual stresses and these were carried out on very narrow plates. Two three-inch wide plates were welded together along their length. In these experiments the value of H obtained was 0.03 as opposed to the value 0.13 which can be used for larger plates. This shows how careful one must be in applying this formula to narrow plates. Of course one is on the safe side if the larger figure is taken. There is another effect on the distribution of compressive stress in the plate. If the plate is wide the compressive residual stresses tend to be uniform. As the plate narrows, not only does the width of the tension block reduce (because H gets smaller) but there is also a tendency for the compressive stresses to be inclined across the remainder of the plate. This redistribution has an effect on the buckling strength of welded I sections for example.*

L.S. BEEDLE :

*I just wondered if Dr. Alpsten had any comment on the intimation that rotarizing would not significantly increase the strength of a column ; you did not make any comment after that statement. I would guess from the residual stress measurements you showed which seemed to wipe out the cooling pattern that one would expect a significant increase in column strength.*

G. ALPSTEN :

*Well, in addition to those residual stress diagrams I showed on the slides,* *we measured, I believe, some 15 sections which have been rotarized or gag straightened and in every single one we have measured this kind of favorable distribution. So we believe you can rely upon this effect. As shown in my slide on column tests we made, I believe, 11 column tests and again all tests showed an improvement which I talked about. So I really think we can rely upon this effect at least for rotarized members. When I say rotarized members I mean members which have been rotarized in a suitable manner ; of course, you can rotarize differently and get no improvement at all.*

B.G. JOHNSTON :

*May I make a general comment, not applied to any specific paper, which concerns a matter of definition of particular interest in the preparation of the Column Research Council Guide. In the first two editions we used the term tangent modulus behavior to apply both in its traditional way to a material such as aluminium alloy with an essentially homogeneous distribution of non-linear stress-stain properties throughout the member and then also to the analogous behavior of a steel column with symmetrical residual stress patterns. In the third edition we are going to differentiate these two behaviors and restrict the use of the term "tangent modulus" to the traditional situation and simply say "critical load" with regard to the behavior of a structural steel shape with a doubly symmetric pattern of residual stress. Also we are going to restrict the use of the term critical stress to the bifurcation load at initial departure of an idealized column from straight equilibrium and let buckling be a more general term.*

W. HANSELL :

*I would like to ask one question and make one comment about the first paper. The question concerns the author's reference to something unique about the residual stress pattern that makes a biaxial column analysis a necessity. I wish the author could try to clarify what is it that is unique about the residual stresses that requires a biaxial column analysis. The comment has to do with the stress strain properties shown for the section. Primarily for the interior coupons, those taken near mid thickness, stress strain properties are distinctly non-linear ; there is no elastic-plastic behavior. It would seem, given the many other refinements we are using in theoretical column analysis, that recognition of this non-linear stress strain behavior would be a necessity for an accurate prediction of column strength. If for example we were working with a straight column, one with no initial imperfections, and we are not told the material but we are shown the stress strain diagram, I believe that many people familiar with column analysis would consider something other than elastic-plastic properties as appropriate for the column analysis and I would appreciate the author's comments on this.*

N. TEBEDGE :

*For the first question on the effect of residual stresses on the behavior of heavy columns, we found that the pattern of residual stresses do influence the column behavior. In our computer program we used different patterns of residual stress distribution on the same section to determine effects of residual stresses. For instance, for a section with no residual stress distribution the behavior of the column was seen to be entirely different from the case when the actual residual stress pattern is used. In this particular case buckling would occur about its minor axis instead of the major axis. If, on the other hand, the residual stress distribution of its rolled shape counterpart is used, again the behavior was found to be influenced. However, in spite of the different patterns the residual stresses were seen to give more or less the same ultimate strength even though the behavior would be different. In this particular column which was flame-cut there will be tensile stresses at the edges and the process of yielding of the cross section property would change differently for each load increment and this may be why residual stresses will have a major influence on* *the behavior of a heavy column. Concerning the second comment, of course it would be very interesting to use the actual stress strain distribution for each grid element instead of the idealized elastic-plastic relationship as we have assumed in our computer program but I wonder how much influence it will have. This would be an interesting study to perform in the future, but a major conclusion in this paper is that residual stresses can significantly influence the behavior and one has to use a general approach, such as biaxial bending, for such heavy columns.*

W.CHEN

*I would like to add a few comments to Dr. Hansell's first question. I think the need of a biaxial analysis is to explain theoretically the overall load deflection behavior of an axially loaded heavy column test. As far as the maximum load carrying capacity of such a column is concerned, the in-plane analysis and the biaxial analysis give no significant difference. In the biaxial analysis we consider the initial geometrical imperfections in two directions, residual stresses and variation of yield point over the section and when all these are considered in the analysis, you can see from the paper that we can predict the experimental load deflection curve very well. On the other hand, if we use in-plane analysis, one immediate question we have is that what is the effective length for an axially loaded heavy column. Since the equivalent length for the strong axis bending of the column is different from that of weak axis bending, so you can see from the test that we have two equivalent lengths, one with respect to strong axis bending and the other with respect to weak axis bending. Neither the strong axis bending analysis nor the weak axis bending analysis can satisfactorily explain the behavior of the test results. Failure of the heavy column was observed in biaxial bending with excessive bending about the strong axis.*

Ch. MASSONNET

*Thank you Prof. Chen, any other comment please ? It does not seem so. Therefore, before closing this first working session, I wish to thank all the reporters who have contributed to its success as well as all those who have contributed to the discussion.*

DISCUSSION ON THE 2nd WORKING SESSION

Chairman : Prof. T. V. GALAMBOS

T. V. GALAMBOS :

*We are coming to Dr. Witteveen's paper. Any questions or comments ?*

G. C. LEE :

*Mr. Chairman, I wonder if I can request the author to extend briefly his presentation of this very interesting research so that he may cover the following two questions*

1. *The bending case : I wonder whether there are substantial differences in creep characteristics between tension and compression and if so how that is taken care of.*

2. *The second question has to do with one of his conclusions regarding the speed of heating :*

*In the uniformally heated case I can understand using a small scale experimental scheme where the effect is negligibly small. However if we want to go to a larger section and particularly the non-uniform heating cases, the specific question is how to scale the heat equation so that a small model test can be interpreted in the actual case. I am interested in the case where non-uniformity may exist in the cross section particularly for heavy shapes as well as the case when non-uniformity exists longitudinaly along the column. It seems to me that the convection term in the heat equation, non-conservative in nature, is difficult to handle in the scaling.*

J. WITTEVEEN :

*I am very glad for these comments because this gives me the opportunity to tell a little bit more than is possible in only ten minutes.*

*Your first question was whether there is any significant difference in creep properties in bending and compression. First I must say that we did not investigate the creep phenomenon itself. We just investigated the influence of the heating rate of steelmembers on its critical temperatures. Because at temperatures of more than about 300°C steel appears to creep, the heating rate possibly may be of importance. So, instead of determining the creep properties itselves, we chose a more pragmatic approach. This is done by investigation of the influence of different heating rates on the critical temperature of steelmembers. As far as bending is concerned we performed small scale tests on beams.*

*These beams were heated with different heating rates in the range of 5°C/min till 50°C/min. Also the load-level was varied, while the midspan deflection was measured during each test.*

*The critical temperature was defined as the temperature at which a maximum deflection of 1/40 th of the span was reached. We did not find for those beams any significant influence of the rate of heating on the critical temperature. The same appeared to hold true for other deflection criteria. For columns, as I told you just before, we did not find any significant influence of the heating rate as well. So, as far as we are interested in the effect of the difference of creep properties on the critical temperature of steelmembers under the different applied heat conditions, the same conclusions can be given for bending as for compression.*

*Concerning the second question, dealing with scale-problems at non-uniform heating I should like to say the following :*

*Generally spoken, in steel structures under real fire conditions the heat distribution in the cross-section as well as in longitudinal direction will be non-uniform. This will obviously result in varying mechanical properties and thermal elongations and/or stresses. It must be expected that the mechanical response of a steelmember will be influenced by these effects. Indicating scale laws to simulate these effects are thought to be extremely difficult and not practical.*

*To solve problems like this, in our opinion first the basis problem (i.e. data, the column with uniform temperature distribution) has to be solved. With the experimental theoretical solutions can be checked. Knowing the theoretical solution for this case, it will be possible to give theoretical solution in case of non-uniform temperature distributions.*

*Of course such calculations are deterministic. To get a more detailed insight in non-uniform heating of structures use can be made of more sophisticated calculations on a basis of probability concepts.*

<u>B.G. JOHNSTON</u> :

*Dr. Beedle has asked me to extend further my suggestion to differentiate between the traditional tangent modulus theory and the extension to steel columns with residual stress. In the traditional tangent modulus theory (as originally enunciated by Engesser) one can go directly from a non-linear stress-strain curve of a material in compression to the column strength curve. The relationship is independent of cross section. Shanley showed that the tangent modulus load represents a true bifurcation of equilibrium, and he pointed the way to the later computer evaluation of maximum column strength slightly greater than the tangent modulus load. Shanley made possible the extension of tangent modulus theory to the analogous critical load theory of a steel column with a bi-linear stress-strain curve and a bi-symmetric pattern of residual stress. But the fact remains that there is no direct relationship between the stress-strain curve of structural steel and the column strength curve in the traditional pre-Shanley or Engesser tangent modulus theory because the shape of cross-section is also involved.*

*Now while I am on that topic I would like to put down for the record the fact that the original concept of the residual stress effect in steel columns was developed intuitively at Lehigh University in the mid 1940's as an outgrowth of tests on box girders by I. Madsen which had demonstrated the fact that residual stress does indeed lower the buckling strength of both columns and plates.*

*Lehigh University proposed that this effect be researched under the direction of a committee of Column Research Council on materials which was then under the chairmanship of Dr. William Osgood. After accepting this assignment Dr. Osgood became personally quite interested in this topic and he prepared independantly and off the record (without submitting his findings back to the committee) his theoretical paper on the topic. He assumes incidentally a parabolic distribution of residual stress as I remember it.*

T. BARTA :

*I would like to add some comments to the problem which you have raised now. I think the most precise answer to this question is due to Euler. Euler has basically two definitions of his critical load : the one which is usually referred to as the Euler load, and another one, which he gave in an earlier paper where he used what he called the "moment of stiffness". Now this concept contains special cases -all the definitions of tangent moduli and so on- and is basically more general because it refers to the proper stiffness of a member and Euler is even precise in stating how to get this moment because he proposes, and apparently never did it, to do some flexural tests on specimens and to find from them what this moment is. Now this is probably a more realistic approach to the problem. Later on the concept of stress was invented, which is absolutely purely a mathematical concept. Nobody could ever measure a stress, but we can measure our strains, and strains contain a lot in a point and these are also mathematical abstractions. We are using complicated procedures of integrating over the cross section and so on but the probably best definition still is the stiffness of member tested for bending and then an approximation to it would be the various definition given here for the tangent and other moduli.*

L.S. BEEDLE :

*First a comment on your historical review there Bruce, I can't let such a discussion go by without us recognizing the work of Dr. Ch. Yang. He was the one who carried out the first theoretical explanations of the buckling strength of centrally loaded columns with residual stresses. This was at the same time that Dr. Osgood was carrying out his work. It was Dr. Yang who recognized that the buckling strength of a member with residual stress could be represented by the tangent modulus concepts taking into account that portion of the cross section that remains elastic.*

*Back to the point of "definitions". There is not time to discuss it now, but it is something that I really think we should think about. You spoke, Bruce of three definitions : the "critical load", "buckling" and "tangent modulus" load. Is not it the latter that you were speaking of in terms of a tangent modulus load that would include a homogeneous characteristic of the stress-strain reliationship ? just wonder if that is not turning too far back into history. The aluminium industry, as I understand it, and in explaining the light gauge thin walled welded and cold-formed members those two industries in the U.S. are very much based on the shall we call it post-1947 tangent modulus statement. This is something I think we should consider.*

J. STRATING :

*I just want to make a historical remark also because I want to give credit to those who deserve it. I want to point out that it was a Dutchman who already before Euler proposed that the buckling load of the column was a function of a constant over* $L^2$*. He determined this relation apparently from actual tests on masonry structures. These tests showed that a constant was involved and that* $L^2$ *was in it too. He also concluded that the dimensions of the column play a role in this constant. This Dutchman was Van Musschenbroek ; he did this in*

*Leiden, a well known and famous University, in 1729.*

B. G. JOHNSTON :

*With reference to Dr. Beedle's comment, the aluminium industry does use the tangent modulus load as a basis for column strength but in this application there is no need to differentiate between the pre-Shanley and post-Shanley concepts. In a general way the concepts are essentially the same for steel and aluminium. But in structural steel we cannot go directly from a small compressive test specimen to the column strength curve as in the case of aluminium or stainless steel.*

T.V. GALAMBOS :

*I think the historical section will be closed. I personally prefer the even more ancient way described by Vitruvius where the shape and size of a column should be formulated on the basis of the shape of the legs of a young lady.*

## DISCUSSION ON THE 3rd WORKING SESSION

Chairman : Dr. F. NISHINO

F. NISHINO :

*The discussion is now open for the first paper of the second half of this third session : the paper of Prof. Steinhardt.*

J. STRATING :

*I would like to ask Prof. Steinhardt about his buckling curve for the aluminium members. I want to know if there was a computer program derived which determines the maximum strength of the aluminium columns because the slides you showed us proved very definitely that flange buckling occurs as well as web buckling and this probably induces the failure of the specimen. I want to know whether you included this in your model because we are very interested in this particular problem of interaction between plate buckling of either web or flanges and buckling of the overall column.*

O. STEINHARDT :

*This computer program was developed for the whole section only, not for the local buckling of the flanges. But we have firstly reached the buckling-point of the flanges, and therefore the limit load indicated by a non-linear P-$\delta$-diagram, is a little lower than supposed before.*

W. HANSELL :

*In view of the reportedly small residual stresses in aluminium sections I am curious to know why the tests reported by Dr. Steinhardt were on annealed aluminium shapes.*

O. STEINHARDT :

*We used only not welded profiles but we have annealed one half of the test-pieces, and the other half not. The differences of the test results have been very low.*

F. NISHINO :

*If there is no other questions, we go into the paper of Prof. Massonnet.*

J. STRATING :

*I would like to ask Prof. Massonnet about his statistical exploitation of the test results. I am familiar with the Student-Fisher distribution ; if you have a sample as small as 3 or 4 specimens it is possible to use this distribution, assuming that the variable is normally distributed, to compute confidence intervals of the mean. If you have a sample of 3 or 4 specimens and if you compute the mean value and the standard deviation of the four specimens then the standard deviation of the mean is equal to the overall standard deviation divided by the square root of the number of specimens ; so, for a sample as small as 4, the standard deviation of the mean itself is half the standard deviation of the sample. And then you look at a Student's t-table and you enter it at a number of degrees of freedom equal to (n-1). So, for example, if the number of degrees of freedom is 3, you will find a value of about 3 or 3.2 for t, which corresponds to a probability of 0.025 if that is the probability that you accepted ; in that case the confidence interval of the mean is equal to the mean value plus or minus 1.5 or 1.6 times the overall standard deviation. So it's hard for me to understand that if you adopt the value of 2.6 or 2.7, as you have done in your paper, you would get confidence limits between which 97.5 per cent of all your test results will fall. Because for a band around the mean of 1.6 times the standard deviation, I only know, with a 97,5 % probability, that the mean of my sample falls into this band and not all the test results. That is just a comment I want to make. You are probably talking about confidence limits of the mean and not of the whole population.*

Ch. MASSONNET :

*You know, I am not as clever in statistical theory as you are yourself ; I am quite willing to admit that. However, we have proceeded as follows : first, we have looked at the statistical variation in the material, namely in the Ramberg-Osgood formula and we have obtained, as I told you, three Ramberg-Osgood approaches. We have derived by simulation on computer only two buckling curves : the first one related to the mean values of the material, and the second related to the lower limit at 97.5 % confidence interval. What I have said is that nearly all the experimental points fall above these lower buckling curves, but that, however, the lower end of the statistical bracket calculated by the formula that I have indicated, taking the value of K enlarged to take into account the smallness of the sample, falls sometimes below the theoretical curve.*

J. STRATING :

*Of course I understand your procedure, I only want to make clear that it is not a consistant statistical approach. You are familiar with the fact that this may be a hobby of mine, I have presented this more often. You are also familiar with my point of view that if you approximate, by computer simulation, experimental lower bound curves you are not carrying out statistical simulation. If you simulate the lower bound curve by adopting a set of imperfections in your column or variations in your parameters, you are never sure whether you have an unique statistical solution that you can transform to other sections and you have to be very careful if you do this ; that is what I wanted to point out.*

Ch. MASSONNET :

*Well, I agree with you that our procedure of deriving the buckling curve from the lower material curve is not completely catholic in the statistical sense but, in waiting for something better that you will probably be able to produce yourself, we have produced this, which may be open to certain criticism of course.*

J. STRATING :

*Just one more remark. I don't disagree with this kind of approach, but I want to have it made clear that it is not really a statistical approach. When you start doing this kind of thing you either start from a truly statistical statistical approach or you make it clear to everyone that you are not carrying out a statistical approach. I don't mean you in particular, this is a general remark, Prof. Massonnet, it is not a personal question.*

Ch. MASSONNET :

*I just want to make clear that we have made a semi-statistical approach in the same sense as the semi-probabilistic theory of safety.*

F.M. MAZZOLANI :

*Just a question to Prof. Massonnet. Your simulation curves are based upon stub column tests, which take indirectly into account residual stresses and elastic limit distribution. In this case, the simulation curves are similar to the prediction of the tangent modulus theory based on the Ramberg-Osgood law. If we neglect the shape factor starting from tangent modulus theory, it may be shown that the behavior of buckling curves depends upon the hardening factor n, but also upon the ratio between elastic modulus and elastic limits. This fact makes the interpretation of these curves easy and also allows the classification of the wide range of alloys from the point of view of buckling. What do you think about this ?*

Ch. MASSONNET :

*Well, I don't agree with you that our approach is identical with the so called tangent modulus approach for two reasons. I think -and I have said that this morning already- that the basic difference between simulation on computer and tangent modulus approach is that simulation on computer takes into account first the Shanley effect, secondly the geometrical imperfections that you neglect in your tangent modulus approach and thirdly the effect of the shape of the cross section. Now, regarding these various effects, the effect of the yield point is almost eliminated by the non-dimensional character of the buckling curves. We also found that the shape of the cross section does not have any definite effect. But, please, recall that we have only investigated two shapes, namely the I section and the tube. And it remains now the effect of the ratio as you mentioned of the offset yield point divided by Young's modulus, but we think that this effect is rather small because the effect of the first parameter ($\sigma_{0.2}$ is eliminated by the non-dimensional approach and because Young's modulus varies only in very small limits : for all aluminium alloys, it varies between 6 500 kg per square millimeter and 7 000 so that it is nearly a constant and you cannot see any influence of this parameter. It is explained in detail in the report that the lower buckling curves simulated on the computer have been obtained by using a low value of Young's modulus, namely 6 500 kg/mm$^2$.*

J.B. DWIGHT :

*I think it is a wonderful achievement that Prof. Massonnet and his team has now produced, just two common curves to cover all those aluminium alloys in the world. I think this is real progress but I am just questioning whether the curves might be slightly wrong for design purposes. It is a matter of principle that I shall try to put over. You really got three factors to consider. If you are in steel or if you are in aluminium you have got the initial crookedness, you have got residual stresses and you have got the curved knee on the stress-strain curve.*

*In the case of steel those first two are a factor but it has got a sharp knee except in the middle of these very thick section we are told. Now in the case of aluminium you have got ($\delta_0$ ), you have got very small residual stresses we have been shown. It is the first time I have ever seen any aluminium residual stresses. This was very interesting. But you have certainly got a knee, and it is this knee that I do not think has necessarily been covered right. I used to sell aluminium for seven years, (and when one sold it you knew what E was) but in the British code of practice and in other countries too, they have a thing, called offset in America ; we talk about a 0.2 % proof stress which is specified there. So if I call that ($\sigma_{0.2}$ ) you specify a minimum value for your 0.2 % proof stress in aluminium and you cannot tie down a supplier of the wonder metal to make his aluminium to have his stress strain curve like this or like that. As long as it reaches the minimum specified figure for the 0.2 % proof stress, it is allowed to go outside the factory gate. This lower value here is a bad one from point of view of strut design. It has got a more rounded knee and it will have a lower strut curve that this good one up here. An this is a matter of the value you take for the n or whatever it is in the Ramberg-Osgood formula and I think it is very difficult to decide just on a few samples whether you have in fact taken the worst value for this constant n in the formula. Of course one important thing is that tension and compression stress-strain curves will be different the compression will certainly have the more adverse shape. So I am just suggesting that this is an aspect that needs some study. I would like to show two pictures on the viewer how we tried to do it a few years ago in Britain but I do not think we did it all that well. These are meant to represent some stress-strain curves. One is just for common aluminium alloys and what we attempted to do was to draw the curve so that it went through the 0.2 % proof stress. Then, when it passes the guaranteed ultimate value it gave us some way of controlling the knee. What you cannot do is to say that the sharpness of the knee is determined by the ratio of the 0.1 % proof to the 0.2 % proof. It is very critically affected by that and no one will quote you a ratio, since manufactures do not want to know about the shape of the stress-strain curve. So what we did was we used the ultimate as a kind of guide and took a rather pessimistic value for this end and then we just applied a straight forward tangent modulus Shanley-Engesser approach. We assumed the strut was straight we ignored $\delta_0$ and we ended up with things that were so near to straight lines that we took straight lines in the end. That's how it stands in our British code at the moment for aluminium, and the Canadians do a similar thing. But there are those who say that this is unsafe and that we ought to be rounding the corner because we did not take the ($\delta_0$ ) into account. But on the other hand it could be argued statistically that you won't get the worst ($\delta_0$ ) at the same time you get the most rounded knee. So I shall leave you with those thoughts.*

Ch. MASSONNET :

*Well, Prof. Dwight, I am not sure to be able to answer all your criticisms but I agree with you that we should have taken care of the fact that the compression and tension stress-strain diagrams are not the same and that this we did not do. Actually, we based all our computer simulation calculations on stub column tests, that mean on compression tests. For this criticism you are right. Secondly, the only thing I can tell you, I have forgotten to say earlier in my presentation, is that the Ramberg-Osgood approach was excellent ; I mean by this that the difference between all our results and the suitable adjusted Ramberg-Osgood curve was less than 1 % in all of our results, so that actually the Ramberg-Osgood formula fits very well with all our tests. What I could add is also that, given an alloy with a certain definite chemical composition, it is represented by a certain value of n, that I call in spite of something better the strain hardening coefficient. You know that the various alloys have very different values of n that depend on the steepness of strain hardening and the sharpness of the knee, so that giving ($\sigma_{0.2}$) and n would represent fairly well all aluminium alloys in my opinion.*

*The effect of Young's modulus is much smaller. I do not think that I have answered all your criticisms but some ot them perhaps.*

J.B. DWIGHT :

*I would just like to say that I do not think it is chemical composition only it is a matter of how much they stretch it. You might have two alloys with identical composition but one might give much more stretching in the extrusion plant to straighten it because of much more curvature in the knee. I think this knee could be much more curved on some specimens than what you get in your laboratory.*

Ch. MASSONNET :

*Well, I suppose that some answer to the last remark of Prof. Dwight may be derived by what I shall say now. As you have seen, Mr. Sfintesco and Mr. Djalaly have treated their results on a purely statistical basis, but it is interesting also to compare their results with theory, I mean with simulation calculation on computer and we have compared the French experimental results with all computer simulations curves. This will be to a certain extent an answer to Prof. Dwight. We have assumed that the French alloys have mechanical properties identical to those of the Italian, Swedish, Belgian alloys having the same chemical composition. And now we have compared statistically the French results with our computer curves and unfortunately I have not any slides but I have here a big diagram in which you see 4 computer curves and 4 families of French results and those of you who are interested could consult these. This paper will be published very soon in the IABSE publications. We have obtained a very good agreement between the French results and our simulation curves for these 4 different families of alloys, which seems to prove that there is some truth in this type of work. Thank you.*

T. BARTA :

*I would like to ask a question about Djalaly-Sfintesco tests. I was extremely interested that there are tests done for* $\lambda = 10$ *. I have not seen any buckling tests done at such a low range of slenderness. My question is : have these tests been done under the same boundary conditions as the other tests and is this real buckling ? I mean, buckling as a column or some kind of straight buckling.*

D. SFINTESCO :

*I will answer for Djalaly. In fact they have the same boundary conditions, this answers your first question. Now for the second question : the tests were performed in 1966 at that time Djalaly was not there, but it seems, from the report, that no local buckling occured prior to the overall buckling.*

*For the first question raised by Prof. Massonnet, Djalaly said that he has compared those theoretical curves with the curves that he has given on the statistical basis. Djalaly seems to say that some differences appear between the experimental results and your proposal, which is based on the theoretical approach.*

F. NISHINO :

*Thank you very much for these interesting discussions.*

DISCUSSION ON THE 4th WORKING SESSION

Chairman : Prof. O. STEINHARDT

O. STEINHARDT :

*The discussion is open to speak about the paper of Kato.*

Ch. MASSONNET :

*We are following with much interest the efforts of our Italian collegues toward the development of a new theory of imperfect latticed and batten struts. In the meantime, we were confronted in Belgium with the drafting of our new specifications and, in particular with the problem of harmonizing the rules for latticed and batten struts with the rules of ECCS for regular struts and it could interest the audience to know the technique we have used for obtaining this harmonization. We started from Timoshenko's well known theory for latticed struts which is exposed in his book of elastic stability. He obtained in this book the formula :*

$$P_{cr} = \frac{P_{Euler}}{1+\frac{P_{Euler}}{GA_{reduced}}}$$

*where A reduced is the reduced area of the bar involved in the shear stiffness.*

*We have*
$$P_{Euler} = \frac{\Pi^2 EI}{l^2} = A\frac{\Pi^2 E}{\lambda^2}$$

*But, to take into account the imperfections, we replace* $P_{Euler}$ *by*

$$P_{ECCS} = A\frac{\Pi^2 E^x}{\lambda^2}$$

*where* $E^x$*, called buckling modulus, is a fictitious modulus derived from the European curve a, b, or c, which is applicable.*

*If you have no lattice and if you have no batten your theory must boil down to the theory for regular struts. By the use of this fictitious modulus, we have obtained this harmony I was speaking about. I suppose this should be of certain interest for some nations which are willing to introduce the curve of the European Convention in their Specifications.*

T. V. GALAMBOS :

*I would like to ask Dr. Finzi a question. In your tests of double angles you note that you use a nominal yield stress of 36 Ksi. Did you normalize your test results to the actual yield point of the material that was used ?*

L. FINZI :

*No, we refered to the nominal yield point when comparing the experimental results, as our philosophy was : well, let's take the grade of steel we are going to use really in the steel structures in our countries and let's verify if there is compliance between the experimental results and the suggestions of the regulations.*

T.V. GALAMBOS :

*So you may have a difference because of that.*

L. FINZI :

*Yes, we have the data and they are a bit higher, I would say. If we have a steel with a guarantee 36Ksi yield point we always have something more. In our case we had about 40 or 41.*

J. LINDNER :

*I want to know what was the influence of the various types of bolts ? I have understood that when you have spoken about.*

L. FINZI :

*Well, the time at our disposal was not so long, so you could see the slide for only a few seconds, but in the slide the different points are marked with different letters. We used 10 K bolts of the friction type and 8 G which are high strength bolts and 5 D which is a normal type of bolt in Europe, and we tightened them to have a friction effect or not. For some specimens we also used hot galvanized bars and bolts as this last type of built-up members is very common for hot galvanized trusses or power transmission towers. In this way you see that, as you increase the friction effect, in a similar way the efficiency of the connection is increased. If you do not tighten the bolt you are going down. The lower points on the slide were for untightened connections.*

W. F. CHEN :

*I would like to make one comment on Dr. Nishino's paper. In that paper, as described by Dr. Nishino, they follow Horne's theory. In that theory the column moment curves are constructed and then the maximum moment is obtained from those column moment curves. As we know, moment is related to curvature by the moment-curvature relation ; so I think the column moment curves actually can be converted to column curvature curves easily. Then column curvature curves really are column deflection curves, so I think this theory is similar to the column deflection method and is nothing special.*

F. NISHINO :

*Essentially the method is similar or almost the same with the so-called column deflection curve method or the like. The thing that I wish to emphasize is that Horne's criterion is incorporated in the CDC method. The criterion with this combination is powerful to compute the stability limit.*

L.S. BEEDLE :

*Referring to the question that Prof. Galambos asked Prof. Finzi about the normalization, I think I would simply put in a plea that, when tests are presented, they should be normalized. It depends, of course, on what the question is. If the question is how good is the theory, then I think it is essential to normalize the data. On the other hand if the question is how does the column test compare with what one would use on the basis of what the contract says, then that is another question. But it seems to me the important one is how does the test compare with what would theoretically be predicted and if we take into account differences of tightening fasteners then I would say it is important to take into account the difference in the basic yield point of the material.*

L. FINZI :

*I would like to underline this on the first series of tests : the main object of these tests was to prove the adequacy of the European curve c for simple struts and also to put in evidence how important is the type of connection both for intermediate connections and especially for end connections. If we wish to compare experiments with theory this is a completely different problem. In fact in this case we should go through a probabilistic approach and it is not analysing the results obtained on 3 or 4 or 5 specimens that we can verify a theory. We hope to be able to do it in the future. This first set of tests was out of the above point of view.*

O. STEINHARDT :

*Any questions or remarks on the next reports ?*

L.S. BEEDLE :

*I will raise a point, since no one is asking a question yet, that is perhaps a reminder, on this word imperfection. My little dictionary here says, and this is what it would mean to an American : "imperfection" is a deficiency. Now if we refer to imperfections as out-of-straightness it is a rather philosophical question, I guess. Are we going to refer to variations in yield point as deficiencies ? Mr. Carpena just suggested that we should use the fact that the yield point is higher than what actually might be delivered and I am not sure that it's right to call that an imperfection. Residual stresses are present and there is nothing we can do about it. To call a steel member imperfect because it has internal stresses does not seem the right word. I am not sure what the correct word is, perhaps variation which means change, change from the ideal. Variations might be a better word.*

D. SFINTESCO :

*Just a slight remark to this problem of terminology. I guess this word "imperfection" comes from the fact that the first kind of imperfections which has been observed was the out-of-straightness. So the member was imperfectly straight. And with further study some other parameters were put into the same category. I think this is the origin of the word "imperfection".*

M. MARINCEK :

*I just wanted to explain that perhaps this is a continuation of the case when we have perfect elastic, perfect plastic diagram and then we think we are not perfect if we have non-homogeneity in this diagram and if we have residual stresses.*

T. BARTA :

*I think I would agree to a great extent with Mr. Marincek's definition. I think the imperfection is a difference between the real word and the imperfection of our capacity of formulating it or the difference between the idealizations we are forced to make and the real things.*

T.V. GALAMBOS :

*I must add something also on imperfections : just a word of caution. We are dealing here with steel structures in the inter-phase between two technologies. One is our own structural technology and the other one is the metallurgical technology which manufactures the material. And to a man who sells the steel, the word imperfection means something entirely different, namely a metallurgical flaw, a crack or something of that kind. So I would urge that we should choose a better word.*

D. SFINTESCO :

*Well, I think everybody will agree with Dr. Barta when he says that a member in compression with slenderness ratio 0 is not a column. But who can say from which slenderness ratio a member in compression becomes a column ? We know there is an imperfection in our capacity to express a point. Now in some column curves quite often this gap which we cannot very perfectly, exactly define is expressed by the kind of straight line which brings into the column curves a sharp knee. I think this is also a kind of imperfection, or imperfect expression of what happens in reality, because we all know that in a phenomenon there is always a law of continuity unless something happens at the precise point or moment. So as soon as we have a sharp knee in a curve this is an expression of a kind of deficiency in expressing what really happens. I think personally we should be more prudent in expressing the column curves for this very first part of the slenderness ratios perhaps by putting something in dashed line, because as soon as we have a theory this theory has to cover the whole field and we also need a connection between the members in compression and what happens with other members for instance in tension in order to get a consistent degree of safety. This is the reason why some theories have developed curves which do not have such a knee. But anyhow the limit from which a member in compression becomes a column can only be conventional or arbitrary.*

O. STEINHARDT :

*There are many inferences and many parameters but the main question is to find out the most important ones and only several ones, not too much, only three or four of such things.*

T. BARTA :

*I would like to reply to Prof. Steinhardt and Mr. Sfintesco. I think this is just one of the important parameters to find : where is the limit of buckling ? So far, columns have been tested up to* $\bar{\lambda} = 0.3$ *it was flexural buckling of the American tests. It would be very interesting to have tests performed in see where this limit is because when we come to a kind of transition to other elements our point of view changes, as I have defined it in the first part of my paper. The question is not then to see if we have sudden failure by bending and so on. Then we would have crashing if it is concrete or in the case of steel we would have local plate buckling or something else. So the problem is different and therefore I think the straight line is meaningless as such, it is just this is not buckling, that's something different.*

L.S. BEEDLE :

*Well, just on this point of flexural buckling, at Lehigh tests have been done and I am sure Prof. Tall would probably remember how low the slenderness ratio went, but it was practically zero. The buckling then is controlled by the strain hardening modulus, not by the modulus of elasticity and the agreement between the tests and the theory is in fact very good. So, while it is an academic question and the strengths are way above the yield stress level, these is such a thing as flexural buckling at very low slenderness ratios.*

O. STEINHARDT :

*Ladies and Gentlemen, I thank you very much for your interest in the discussion and I thank all the reporters of this conference.*

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.*



Bg. 30 AK 23

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 $\sigma$ $\lambda$ curve should be stopped at the yielding stress for those values of $\lambda$ 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 $3 \times 10^{-3}$. 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.*

DISCUSSION ON THE 6th WORKING SESSION

Chairman : Dr. D. SFINTESCO

D. SFINTESCO :

*Dr. Milek will tell us what has been decided at the last CRC meeting on the matter of column strength, so he brings very fresh news from CRC.*

W. MILEK :

*My comments constitute a report on the position of CRC in the U.S. on the maximum strength concept at multiple column curves. There have been several meetings and considerable informal discussion and I must say that there have been honest differences of opinion which are not yet fully resolved.*

*We all here have had an opportunity to hear presentations on recent developments during the last two days and in my opinion it is evident that a great deal of progress is being made toward refined knowledge of the strength of pin-ended, axially loaded columns. I also think that it is apparent that the subject has not been exhausted. There are still questions that need further study. Particularly, further study is needed on columns in real structures as contrasted to the pin-ended columns.*

*The consensus of task group 1 of CRC, based upon their own discussions which is consistent with the discussions that have taken place here, is that CRC in the U.S. take the position that there is much new knowledge about the strength of columns which need to be reported to the professionals. But in view of the several important factors which have not been studied, it is too early to make recommendations for revision of our design procedures. Knowledge that now exists will be presented in the guide as suggestions needing further review.*

*Now what are the principal things that we now know ? It has been amply demonstrated that given adequate information -that is relative to the mechanical properties, the magnitude and distribution of residual stresses which includes the method of manufacture, the geometry of the cross section, the out-of-straightness-maximum strength gives a highly accurate estimate of the true pin-ended column. It is also known that each of the above factors has a significant effect on the strength of the columns, therefore procedures which do not include all parameters involve errors in the estimate of strength. Scatter band for columns in general is quite broad, as shown by several of the slides today.*

*One method for improving the accuracy and design procedures would be to use multiple column curves in which geometrical factors and method of manufacture are taken into account in an approximate way by these several column curves. Prof.*

*Johnston has just proposed an alternate procedure as have the two earlier speakers. This may provide an alternate, perhaps more practical approach. Third, it is know that the tangent modulus concept provides good estimate of strength for stainless steel and aluminium structural members therefore probably should not be discarded.*

*What are some of the factors that merit more studying ? Basically the principal questions involve the fact that more information needs to be developed relative to actual columns in real frames as contrasted to the pin-ended columns which are certainly excellent for a laboratory tool but are very rarely encountered in real structures. Some of these items are :*

1) *the work completed includes only bi-symetric cross sections. Design recommendations would be silent on asymetric sections or would require some sort of arbitrary assignment to a particular column curve. Such asymetric cross sections are important, for example single angle struts or T chords of trusses. At the present time we really have no good recommendations under the new concept.*

2) *The bulk of the residual stress measurements have been made on relatively light sections. Limited work has been done on very heavy sections thus the statistical analysis for the third curve, the lowest curve is based on a rather population. Also there is a significant gap in material thickness between the sections that have been studied and constitute the background for column curve 2 and the thicker sections that are the background for column curve 3. I believe the thickness range that jumps from 1 1/2" thick to about 3" thick is important. More information is needed over the full range of thicknesses in use. We need to answer the question if there is a threshold of thickness or, more logically, a transition between the light and heavy sections.*

3) *More information is probably needed on the effects of straightening procedures. Is it justified to design straightened sections at a higher stress than the non-straightened sections ?*

4) *Some work has been done on the effect of through thickness variation and residual stresses and it has tentatively concluded that it does not have a profound effect. Therefore as a simplification, conservative assumptions of uniform residual stresses equal to the surface value have been used in the work to date. Possibly more study is needed especially on the very thick cross sections.*

5) *The work completed is limited to pin-ended columns, which are structural elements which are useful in the laboratory as a starting point for design of compression members. However, such members do not occur in real engineering structures. There is always some end restraint which is important and it is not rational to overly refine the knowledge of pin-ended laboratory columns without parallel study of the effects of other parameters in real structures.*

6) *The maximum strength concept incorporates out-of-straightness in the length or as a length times some factor. Bjorhovde reports that the single most important variable was this out-of-straightness factor. Use of the single out-of-straightness factor L/1000 works fine in pin-ended laboratory columns. On the other hand incorporation in a design formula for real columns in structures is probably not logical for three reasons. First, in beam columns the effect of out-of-straightness diminishes rapidly as the moment increases by reason of the interaction formula. Second, it is common practice in design to modify actual lengths of restrained columns to an equivalent pin-ended column by use of the equivalent length approach. This involves that by multiplying the length by a k factor, by including out-of-straightness in a pin-ended column strength formula, the out-of-straightness would also be increa-*

*sed. This just does not seem logical. Also, in high strength steels the out-of-straightness factors are more important. These questions do need study. Perhaps out-of-straightness should be handled as some sort of a separate corrective factor.*

7) *It has been demonstrated that interaction formulae give good estimates of strength in beam columns. If the design rules for simple columns which in effect are an ingredient in the interaction formulae are changed, the accuracy of the results of the interaction formulae would obviously also be affected.*

8) *Elastic-plastic stress/strain properties used in the designs and studies today and possibly the influence the knee in the stress/strain curve on column strength followed by strain hardening for thick-steel sections have not been evaluated, but may be important in some structures.*

*As a result of the above considerations, -what we do know and what appears to require more study- the CRC in the U.S. has decided that the three column curves as they have been suggested here will be included in the guide. They will be presented as best fit for the particular sections used in the research. The ECCS curves will also be included and the mathematical representations of these curves will be included, but column curve selection tables will not be included. In other words the intent is to present a as complete information as possible on the present state of knowledge but recommendations for implementation in design will not be included.*

D. SFINTESCO :

*Thank you for this most comprehensive report, which shows us that on many points we still need more knowledge and that we are far from having solved every problem, even in this limited field of the pin-ended column. Now may I call for comments on the first paper, of Marek and Skaloud ?*

Ch. MASSONNET :

*Thank you for giving me permission to make some remarks about all the papers before leaving because I have to take the train back to Belgium. I was struck by the phenomenon that many of us seem to like to obtain analytical representations of the buckling curves. This is of course natural for many reasons, one of these is that it is much easier to enter an analytical curve into a computer for design work for instance. Now, many proposals have been made, among others the Perry-Robertson and a nice model by Prof. Vogel, and previously also the Dutheil approach. All of these approaches are very good ; now, to decide between them, you could proceed as follows : (and this work has been done in Liege some years ago but not on a full basis) you use the least square method of curve fitting and you study which of these analytical representations gives the minimum of the sum of squares. Now a paper along this line has been written and presented to ECCS Committee 8 by one of my collaborators, Mr. S. Baar, and he has found that, of all the algebraic formulea representing the European column curves (a,b,c), the Dutheil approach had the best mathematics. Now, if any of you are interested in this paper, it should be easy to have some copies and to send them to you.*

D. SFINTESCO :

*Thank you Prof. Massonnet and if I may add something to your particular comment, I shall certainly not be against what you just said about this approach but I should like to complete in some way what has been reported by Dr. Dwight before on the remark of Sir John Baker saying that the Europeans did a good job because it fits with the Perry-Robertson formula. I should say they probably have done even a better work than Sir John Baker thinks, because this approach seems to fit with several formulae and it is obvious that in some areas of the world, in some countries, people, for various reasons, would like not to change the theories to which they are accustomed as far as they can fit with the practical results and I think this is by no means bad.*

*Now, are there any comments on the second paper by Bjorhovde and Tall ?*

J. BROZZETTI :

*I would like to ask some questions to Dr. Bjorhovde and Dr. Schulz. We saw two proposals for multiple column curves but if we study particularly these proposals we can see some differences between the two. For example in the Lehigh proposal one curve stands for annealed sections. Instead in Dr. Schulz and Prof. Beer's work you have two curves, depending upon the bending axis. I would like also to make another comment about the lowest curve presented by Dr. Bjorhovde. I think people have a slight tendence to put too much residual stresses in those very heavy shapes. As far as I remember we made some measurements at Lehigh about the 14 WF730 and we never got so much residual stresses in this heavy shape. I don't understand why you have a so low curve. Dr. Schulz said that in fact the magnitude and the distribution of residual stresses change with the amount of welding. I am not quite sure about that, because we have several experiments also at Lehigh and our conclusion was that the speed of welding did not have a very significant effect on the residual stresses due to welding. I would like to have some answer about these questions.*

R. BJORHOVDE :

*With regard to Mr. Brozzetti's question about the assignment of major and minor axis bending of annealed shapes to the same column curve, the answer is simply that the differences in major and minor axis strength were too small to warrant placing the two cases in different categories. This applies to the development of the American multiple column curves, which was done somewhat differently than in the European study. As you can see, curves a and b of the European proposal are much closer together than our curves 1 and 2, and henceforth much smaller differences in strength would cause a change in the classification. Although the number of annealed shapes that was included in our study is not statistically large, I believe that the assignment of annealed shapes to the uppermost of the three curves is substantiated by their classification in the European proposal. Mr. Brozzetti's comment about the residual stresses in the W14 × 730 shape is not quite relevant, since the shape he measured was found to have been cold-straightened. Finally, I would like to agree with Mr. Brozzetti in his statement to Dr. Schulz that the magnitude and distribution of the residual stresses do not change with the amount of welding. Our studies at Lehigh University over a number of years have proved that conclusively.*

G. SCHULZ :

*Dr. Bjorhovde already indicated as one of the reasons the different distance between the European and the American curves. But there is another reason for the discrepancies in placing sections in appropriate column design curves. It seems that for the American and the European version of the column curves, there is quite a difference in the philosophy that led to their establishment. The American curves are based on L/1000 as the European curves, but the selection of the three curves was done quite differently. For the selection of each of the American curves, a wide band of column curves was plotted and the mean value out of this band was selected. The European curves were established according to the philosophy on which the evaluation of the tests were based. They do not correspond to the mean value, but to the mean value minus 2 × the standard deviation.*

L.S. BEEDLE :

*Mr. Brozzetti commented about the residual stresses he measured in a 14 WF 730. Of course that shape was cold straightened which would reduce the residual stresses below what Dr. Alpsten would have predicted. That in fact is one of the reasons why further studies are needed on the effect of cold straightening, as Dr. Milek has emphasised.*

J. BROZZETTI :

*Yes I agree with Dr. Milek's work but as far as I remember we had several discussions with Alpsten and I did not agree too much with him about the theoretical predictions he gave in one of his reports. His predictions were quite unfavorable, and as far as I remember on some diagrams you have almost one tenth of the shape which was already yielded ; but we never find these results when we measure the residual stresses in some of those heavy shapes.*

L.S. BEEDLE :

*Because what you measure is the residual stress in a cold bent shape.*

J. BROZZETTI :

*Yes I agree but in fact it was partly straightened. I do not know if you remember Dr. Beedle but it was only the flange which was partly affected. It was not the entire shape I guess.*

R. BJORHOVDE :

*The W 14 × 730 shape which has a flange thickness of 5 inches was cold straightened, or there was at least very strong evidence to that effect. Concerning the residual stresses that may occur in non-straightened members of that size, there are indications from the measurements of heavy plates that these correlate well with the magnitude of the residual stresses that one may expect to find in heavy unstraightened members. These results compare favorably with the studies that were made by Dr. Alpsten. For example, at the edge of a 6 inches thick universal mill plate of material with 36 ksi yield stress one will find a compressive residual stress of approximately 28 to 30 ksi. This stress is considerably higher than that measured in a W 14 × 730 shape, where the maximum compressive residual stress was about 18 to 20 ksi.*

J. BROZZETTI :

*Yes but, I think you agree with me, we never find in those rolled plates any residual stresses reaching the yield point ; in fact we find up to 2/3 of the yield point, that's all. I would like to ask another question. I don't understand why you have some differences between the curve b as proposed for example in the general Lehigh approach and the experimental curve of ECCS. In fact both apply to the same case and I do not see why you have a difference between them. If the theory does not agree with the experimental curve we should retain the experimental curve and not the theoretical one, this is my point.*

G. SCHULZ :

*Well, If I follow you, you are referring to the experimental curve of ECCS and curve b of ECCS. Well, there is no difference between those two curves.*

D. SFINTESCO :

*May I mention just the story of this thing. We had quite long discusssions at the European Convention about this particular point because in a previous presentation of the Graz results the curve b was not so near the experimental curve. We discussed for some long time about that and as a consequence some adjustments were made in the program in order to put the curve in accordance with the experimental results. I agree with Dr. Schulz's position for the present situation.*

G. SCHULZ :

*There was originally, I guess this was 1966 or earlier, a first suggestion with four column curves. Curve 2 of this proposal did not agree in the low slenderness range with the experimental curve. There was a deviation of two or three percent. When we had to reduce the number of curves to three, the present curve b was introduced, which agrees quite well with the experimental curve.*

D. SFINTESCO :

*As a matter of fact in the first version of this curve b which we have discussed at the European Convention there were two differences : in the range of the lower slenderness ratios the computed curve was lower and in the higher range it was higher than the experimental points. It has been adjusted in the meantime and we have now a fairly good agreement between the two curves. So maybe Brozzetti is referring to the older version of these curves.*

G. SCHULZ :

*Mr. Chairman, may I answer to the question Prof. Dwight was asking Prof. Massonnet with regard to the sections of high strength steel ? We did not recognize the reduced influence of the residual stresses due to the higher yield point, therefore, the suggested curves are slightly conservative for rolled sections of high strength steel. The slide shows that the gain in column strength which was omitted is very small. In particular, for the sections of high strength steel presently used in Europe, the gain is too small to make the jump into the next higher column design curve.*

D. SFINTESCO :

*Dr. Schulz, we have seen that in your program, you have used what I would call an idealized pattern of residual stresses with symmetrical distribution. However, in all residual stress measurements made either by Prof. Massonnet or at Lehigh, in fact everywhere, we always see a non-symmetric distribution of the residual stresses. Now one can think that the symmetry of distribution of these residual stresses may affect the buckling process itself. Did you make any trial to see how it works with a non-symmetrical distribution ? I think it should have been possible with your program.*

G. SCHULZ :

*No, it is not possible because we consider only the bending in the direction of one axis. As long as we do not consider biaxial bending we probably won't be able to get a significant gain in column strength due to an unsymmetrical residual stress pattern.*

D. SFINTESCO :

*Well of course any mathematical model cannot reproduce all irregularities which exist in reality, but I think this may not be a minor effect for the phenomenon itself. I wonder if it should not be investigated.*

G. SCHULZ :

*It could be done. But actually, if the conditions are extremely unsymmetrical, we should not anymore apply the theory of pure flexural buckling.*

R. BJORHOVDE :

*Mr. Chairman, a comment to your question on the effects of an unsymmetrical residual stress distribution. A study was made by Gernot Beer when he was studying at Lehigh University, and he found that unsymmetry in the residual stress distribution was quite insignificant. In this particular study Mr. Beer investigated the strength of box sections.*

D. SFINTESCO :

*Well, the effect may be different for I sections.*

G.C. LEE :

*I think the question of unsymmetrical residual stress can best be lumped into the geometrical imperfection parameters. It is difficult to see how a perfectly straight or nearly straight member can have unsymmetrical residual stresses.*

T. BARTA :

*I think the question about the asymmetric distribution of residual stresses might be important if one thinks of flexural torsional buckling but this has not been considered by the team in Graz and this will obviously play in some stage of higher slenderness. We have made a couple of years ago an attempt to generalize imperfections by taking fine imperfections for flexural or torsional buckling. This can be done by the Perry-Robertson or by any other variant and by adjusting to test results and this probably would agree better with assymmetric distribution of residual stresses.*

D. SFINTESCO :

*As a matter of fact in the experimental series of tests of the European Convention even for members which were on knife edges we have measured the torsion at mid height and there was always some torsional effect. We have records of these measurements.*

*May I now call on comments on Dr. Dwight's paper.*

M. MARINCEK :

*I think that the dimensionless buckling curves have an advantage.*

O. STEINHARDT :

*I have a remark to the paper of Prof. Vogel and also to the last note of Prof. Marincek. The effective-length-method, as a reasonable approach for the determination of critical buckling loads of single story frames, on the one hand is too safe in some cases, that is the statement of Dr. Vogel. In my opinion it is not sure that on the other hand for clamped single storey frames with drift influences by outside columns, this definition could be used. In special cases we have found that (without some additional explanations for the plastic configuration !) the safety margin for the whole system may be very much less, than supposed when using only the new German guiding principles. Otherwise I refer here to my paper given the last day about the same problem for Aluminium construction.*

U. VOGEL :

*Prof. Steinhardt I am not able to give an answer because I have not studied the frame with fixed basis on the column foot. So it could be that you are right but I cannot comment on this. I would like to take the opportunity to give an answer to the comment of Prof. Massonnet. It was not my intention to establish* *another mathematical formula for the European buckling curves. I used this method only as a tool to get an idea of the order of magnitude of a representative imperfection which I could use then to treat a frame or a framed column.*

D. SFINTESCO :

*Thank you, any other comment on the same paper ? Then any remarks on Prof. Johnston's paper ?*

G. SCHULZ :

*May I comment not on Prof. Johnston's paper but on the remarks of Mr. Milek mentioned the initial out-of-straightness as the most important parameter for column behavior and for the determination of the maximum strength. Well, this statement probably has to be modified. The out-of straightness is another important factor, but its influence is definitely smaller than that of the residual stresses, and depends very much on the magnitude of the residual stresses which are present in the section. For sections with high residual stresses the influences of a variation of the out-of-straightness is comparatively small.*

R. BJORHOVDE :

*I would like to comment on what Dr. Schulz said. As far as the out-of-straightness is concerned, I think Mr. Milek was referring to the part of my study which was dealing with the probabilistic nature of the maximum strength. I think I again should point out that when the column strength variables are treated as random variables, and henceforth producing a random variation of the maximum strength of the column, that is when the initial out-of-straightness attains its greatest importance. It by far supercedes the importance of the residual stresses when their random or variations about the mean are being included in the analysis. Again, we are here talking about the random variations that occur in a number of samples of identical shapes. Thus, Dr. Schulz and I are covering entirely different matters. I also think that Dr. Johnston might want to add a few words relative to the importance of the initial out-of-straightness and the residual stresses.*

D. SFINTESCO :

*Well, gentlemen, thank you for your interesting comments. I suggest we adjourn more or less in time, as we shall have now our closing session with summaries by the chairmen of the sessions. Thank you.*

## CLOSING SESSION

Chairman : Dr. D. SFINTESCO

D. SFINTESCO :

*I wish to thank all of you for your participation in the sessions held during these two days. In this closing part of our colloquium, the session chairmen will try to summarize the very interesting presentations of reports and the lively discussions we had on some subjects, thus drawing the conclusions of our meetings.*

*The chairman of the first session, Prof. Massonnet, had to leave earlier today. He asked me to present his concluding remarks.*

D. SFINTESCO (for Ch. MASSONNET) :

*The first working session was devoted mainly to the determination of residual stresses in hot-rolled members or in members fabricated by welding. The first paper by Tebedge, Chen and Tall, entitled "Strength Behavior of Heavy Welded Columns", contains theoretical and experimental analysis of the behavior and the strength of heavy shape columns built-up from flame cut plates. Comprehensive experimental investigation was performed to determine the strength and the behavior of one particular heavy built-up shape, H23 681 ACM A36 steel. The experiments included :*

1) *measurements of yield stress through the cross section*
2) *measurements of residual stress distribution across the width and through the thickness of the component plate by the slicing method which involved corresponding longitudinal cuts*
3) *stub column tests*
4) *full-size column tests.*

*The column tests probably break the world record of buckling tests.*

*As there was a flat end condition at the low end and some measured rotation was allowed at the other end, it is difficult to compare the collapse load with the theory. However, the three-dimensional theoretical analysis involving the effect of residual stress, yield strength variation on the cross section and initial out-of-straightness in the two principal axes were performed on computer and compared with experimental data. The two main conclusions of the study are : 1) because of the particular pattern of residual stress distribution in the cross section as well as the initial out-of-straightness in the two principal planes of such heavy shape columns, biaxial bending column analysis is needed in order to predict accurately the load-deflection behavior. The strength of heavy shape columns built-up from flame cut plates is found to be higher than those of lighter welded counterparts.*

The second paper by Dr. Young, entitled "Residual Stresses in Hot-Rolled Members, attempts a thorough investigation of the residual stresses in I sections due to severe cooling after hot rolling. Dr. Young made comprehensive survey including a large number of previous measurements and containing most available results on British universal beam and column sections, in order to establish typical patterns for subsequent inelastic buckling strength calculations. He shows that these results are different from the american ones, especially for the residual stress distribution along the web, which he found parabolic instead of constant in american profiles. These differences are due, according to him, to different practices in particular in the cooling bed, and to cold straightening.

The third paper by Alpsten is entitled "Residual Stresses, Yield Strength and Column Strength of Hot-Rolled and Roller-Straightened Steel Shapes". The main purpose of the investigation was to study the improvement in column strength resulting from rotarized procedures. The rotarizing changes the residual stress distribution and may also attack the mechanical properties. The investigation was both experimental and theoretical. It included a comparison of residual stress, mechanical properties and column strength of four lots of HE 200 H shapes, all taken from the same heat. One reference lot was taken as rolled with no straightening. The three others were subjected to rotarizing treatment of increasing amplitude. These treatments were simulated theoretically and the maximum column strength was both obtained through buckling tests and by computer simulation taking into account the effects of non symmetrical residual stress, variable yield strength, initial out-of-straightness and eccentricities. The investigation showed that the maximum column strength may be increased by about 20 % due to suitable rotarizing procedure. It is suggested that this improved column strength of rotarized rolled members be considered in design by assigning the adequate column curve to this type of members.

The fourth paper by Brozzetti has for title "Effect of Welding Parameters on Simulated Built-up Column Strength". It contains two-dimensional contour maps of residual stresses in welded profiles and in the thick plates, either shear cut or flame cut, used in establishing these profiles. Residual stresses in these plates due to depositing weld seams on their central part are analysed separately. From these residual stress patterns, theoretical buckling curves for these welded I profiles are derived by the modified tangent modulus theory and conclusions are drawn regarding the effect of welding parameters. It may be observed that the theory used does not give a completely accurate picture, because it neglects simultaneously the Shanley effect and the effect of the geometrical imperfections.

The fifth paper by Prof. Lee entitled "Buckling Strength and Design Guide of Welded Linearly Tapered Column", falls somewhat outside the framework of this working session. The columns considered are H shaped sections with a linear variation of the cross sectional depth, fabricated by welding only on one side of the web. The specific content of this paper are :

1) analytical elastic buckling solution of tapered columns
2) residual stress measured in tapered column specimens welded from both shear and flame-cut plate elements
3) analytical inelastic buckling solutions of tapered columns by considering the residual stresses
4) formulation of design guides including effective length factors for centrally loaded tapered columns.

The sixth paper by Nylander has for title "Effect of Initial Stresses on Plate Buckling and Buckling of Box Columns". The plate buckling theory is based on a model for the study of the post-critical behavior consisting of a plate acting only in plate bending and a number of strips taking the membrane stress only. It was presented at the Amsterdam IABSE Congress. In the analysis of buckling of the welded box column with quadratic cross section, it is assumed that the effective *cross section consists of four angles with a flange width being equal to the effective width of the composed plates at the failure load in plate buckling. Numerical results of the calculations are given by diagrams which show that the influence of the initial stresses is of great importance. To be evaluated, these results should be compared by other more refined approaches of the same problem obtained by Klöppel, Skaloud and others.*

*The seventh and last paper by Dwight is entitled "Prediction of Residual Stresses caused by Welding. The paper shows how the longitudinal force in a weld may be estimated from a knowledge of the heat input or the size of the weld. With this information, it is relatively easy to predict a pattern of residual stress in a fabricated member. The formulae presented are appealing by their simplicity. They are not applicable to thick profiles. But, if their validity is confirmed by extensive comparison with actual residual stress measurements on welded profiles, they should constitute a convenient guide for the establishment of the residual stress pattern in new shapes of welded profiles.*

T.V. GALAMBOS :

*In my session there were two kinds of papers : one group of papers dealt with the determination of the ultimate strength of columns under a variety of conditions imposed by imperfections, residual stresses, crookedness, and so on and these papers were the ones by Young, Mazzolani and Fujita. These papers have really a great deal in common in that they all end up with curves which predict the behavior. The paper by Fujita was on H shapes and rectangular solid shapes and it differed from all of the other papers that were given here in that he introduced imperfections or initial eccentricity in the laboratory deliberately. There were specific pre-set initial deflections which were given to the columns which were tested and analysed. The method of analysis was a finite element analysis and he had subdivided his column length into a rather large number of elements. The paper by Young dealt with the study of British shapes and in contrast to all of the other analyses that have been performed in the other papers, he used the numerical differentiation technique rather than integration starting with the deformed shape of the column. In Fujita's paper a variety of effects are considered and four instead of three standard column curves are arrived at and a somewhat different column selection table was presented then as given in the standard European curves. The paper by Mazzolani dealt with the Italian shapes and their comparison with the European column curves. Residual stresses were studied as well as the initial imperfections. The paper ends up with a comparison of the predictions by the analysis vs. the European curves.*

*The other three papers concerned diverse topics. The first one that was presented was the paper by Leites and as an academician I enjoyed it a great deal. It dealt with the elastic large deflection problem but I think that its relevance to what this group is trying to do is somewhat limited. The second paper is the paper by Mateescu from Rumania and it is a very interesting paper in that it deals with the elastic lateral torsional buckling of elastically restrained columns which are loaded through the centroid rather than the shear center. The differential equations are developed, boundary conditions are given, and the solution is then described as a solution of the differential equations by numerical computer techniques. The interesting thing about this paper is that there are a number of charts given that permit the designer to analyse his problem quickly. The last paper is on an odd topic, namely fire and it was the paper by Mr. Witteveen who described the work done at his laboratory as part of the European study on the performance of columns at elevated temperatures and the major point that I got out of that paper was that it you could treat the fire problem as a time independent problem in a limited sense. This is the summary of the description of the work. While I have the podium here I would like to say a couple of things that I have observed about the work here and that I would like to plea for. I have been in the past year engaged in trying to write a probability based code for steel structures. In doing so I have had to root through millions of reports and trying to tie down test results.*

*For the sake of your successors and the younger people who are coming on, please document everything. Even though the information to you may be irrelevant and you have proved the point you want to make, somebody else may need this information vitally and it is very difficult to come by. The second point I would like to make is I would like reinforce the talk that was given by Mr. Marincek and also by Mr. Barta. Let us identify the major things and look for them. Thank you.*

F. NISHINO :

*The third session was divided essentially into two groups. The first three reports are concerned on the analysis of biaxially loaded columns whereas the three remaining reports are mainly concerned on design of columns, with introduction of the concept of probability in the last two papers. In the first paper presented by Gaylord, an analysis of beam-columns in uniaxial bending was discussed and then the same technique was extended into the biaxial bending problem. A column is integrated from the point of the maximum deflection towards both ends under a constant thrust. The stability criterion is the maximum end eccentricity which would result by changing the magnitude of the maximum deflection. It is easy but time consuming to find out the maximum eccentricity by changing the magnitude of the deflection and integrating each time for an end eccentricity. Instead an auxiliary equation is introduced by differentiating the equilibrium equation. The problem of finding the maximum eccentricity by changing the initial value of the deflection has, thus, been changed into a problem of solving two simultaneous equations which is an interesting scheme and contributed in reduction of computing time without any loss of accuracy.*

*Lindner used, in the second paper, the Ritz method with displacement function given by polynomials to study the ultimate strength of biaxially bent columns. An equilibrium position has been determined by the stationary condition of the total potential energy and then the stress distribution has been checked for any possible violation of stress-strain law. If it is violated, correction is made and the procedure is repeated until both the equilibrium and the stress-strain law are satisfied. The ultimate load is determined by observing the maximum point of load vs. deflection relations. The residual stresses and the geometric imperfections are considered in the analysis.*

*In the third paper, Vinnakota presented a summary of studies on restrained columns under biaxial bending utilizing finite difference technique. One of the points of theoretical interest is that the equilibrium equations are written with respect to an arbitrary system of axis. With this, some of the complexity which arise from the shifting of the location of the shear center and the centroid with the penetration of yielding can be avoided. Another point of interest is the springs attached at both ends. With these springs, the entire system becomes stable even when the column itself is already in an unstable equilibrium and therefore the simpliest ultimate strength analysis can be utilised ; that is, the load can be increased up to the maximum point of the load vs. deflection relationship of the column without any instability in numerical computation.*

*In the third paper Steinhardt presented design formula for columns made of aluminium alloy, which has been developed at Karlsruhe and will be employed in German specifications. The formula was derived principally based on the analysis of eccentrically loaded columns. It was pointed that the formula predicts closely not only the strength of centrally and eccentrically loaded columns which fail by excessive bending, but also for the torsional flexural buckling, and that it is more rational even compared with the existing formula in German specifications, DIN 4114. It was also pointed that in spite of the fact that there are significant differences in stress-strain relation and in residual stresses between aluminium* *alloy columns and steel columns, the formula derived for aluminium columns can also predict closely some of the column curves of ECCS.*

*For the last two papers I had some difficulty to take notes during the presentation and also I have difficulty to read French. I am rather afraid that there might be some misunderstanding in the 5th paper presented by Massonnet. The results and the conditions of CIDA research conducted at the University of Liege on stability of tube-and I-shaped aluminium alloy columns were presented. A similar computer simulation technique as used in deriving the column curves of ECCS has been used in order to estimate the strength of the columns with due consideration to the statistical variation of parameters. The theoretical study has been substantiated by a series of experiments sufficient in number to make use of statistical treatment. It was found that the stress-strain diagrams of aluminium alloys studied could be represented by Ramberg-Osgood curve. The dispersion in mechanical properties is small and two stress-strain curves could be sufficient for the practical design purpose for six alloys studied. The influence of cross sectional shapes for the strength curves was found very small.*

*In the last paper, Sfintesco and Djalaly treated statistically a large number of test results on simply supported axially loaded aluminium columns, and established non-dimensional column curves for the probable collapse limit with a constant probability of failure. The dispersion was also presented as a function of slenderness ratios which would serve as the basis to define variable safety factors in order to have constant reliability. The discussion followed for the last two papers indicated that the probabilistic approach is an important aspect for better design of columns and that it will be one of the topics that need continued investigations.*

O. STEINHARDT :

*The fourth "working session" in its first group Kato, Finzi and Nishino has dealt with problems of tubular struts, with centrally compressed built-up struts and with ultimate strength of box columns. This special cross sections may be designed on the conception that -with introductions of special imperfections- they could have the same safety factor as tension bars. The behavior of the component-struts and fasteners in built-up sections further has to be investigated. The influence of welding maybe more important for struts with low eccentricity of axial loading. Hot rolled wide flange columns need another consideration than welded, these latter have a more pronounced reduction in buckling stresses. The second group of this fourth session, manifesting some profound basis problems of buckling, dealt with by the colleagues Marincek, Barta and Carpena led the discussors to the opinion that mathematics and nature, computer and brain are to distinguish and that history and practical experience give some advice what engineers way into the future has to be. That, in my opinion, is the main question, namely to find out from the numerous influences (or parameters) the most essential ones. For the world wide standardization there must be developed in the next future some uniform and approximatly exact buckling curves for the 4 or 6 buckling situations in light or heavy rolled and welded cross sections ; by good will, that could be a short-time-conclusion.*

L.S. BEEDLE :

*The fifth session dealt with the European Column Research with the heavier shapes of Europe. These were studied at Lehigh and with the probabilistic, statistical, and load factor considerations.*

*How many here have seen a centrally loaded pin-ended column in a structure ? How many have seen one ? I have seen about 50. They are in a bridge in Czechoslovakia, centrally loaded, pin-ended columns in a structure, not in a test machine. Are there others ? Probably very few.*

*Well, all of this concern about a rarity is probably for two reasons :*

1) *The fact that column design in most cases makes use of an interaction formula one end of which is the case for M = 0. Until there are some alternates, this will be with us for a while.*

2) *Another reason is because of a comment the cynic once made that we do research on those problems for which there is a hope of getting a solution !*

*Sfintesco reported that the wide variation in column formulae led initially to the ECCS program and he outlined the approach that was taken.*

*Tebedge showed the good correlation at $L/_r$ = 95 and something less than implied at $L/_r$ = 50 by the ECCS curve for these heavier shapes of this European series that were tested in the U.S.A.*

*Bjorhovde presented the probabilistic approach applied to the parameters such as e/L, residual stress, yield point stress variation etc.*

*Strating described the Monte-Carlo method to predict column buckling curves and showed them to be in reasonable agreement.*

*Cornell distinguished between the variables that could be measured and those that had to be assumed or that he believed had to be assumed.*

*Galambos presented what he calls a simplified method of column design. Whether it looks simple or not, it does contain the key elements of what is needed for the "load-factor" design method.*
*The discussion brought out a number of things that should be summarized :*

1) *The suggestion that we should "calibrate" the computer programs as developed in Europe and in the United States to make sure that when one puts in the same material one gets out the same maximum strength. Also this would apply to calibrating the influence of various parameters.*

2) *When we talk about the out-of-straightness of columns, we should use values that would correspond to what actually exists in buildings.*

3) *The need for data on variation of the influence of the thickness of the shapes, and the end restraint effect, and the out-of-straightness factor.*

4) *Many other needs were outlined later the following session by Bill Milek.*

*I sensed from the discussion that these three areas are open.*

1) *How to cope with a new column shape, the introduction of a new method of fabricating, and a new type of steel.*

2) *The use of column tests. On the one hand, they are used to confirm a theory, in which case we study the variations due to yield point, geometrical variations, residual stress, and out-of- straightness. On the other hand the column tests are a statistical basis for empirical column curves and then probabilistic approach is used considering some of these same factors as in the theoretical approach to see if the empirical curves can be justified.*

3) *How to apply the results to design. In the probabilistic approach, how will this in fact be applied to design ? How will we apply the design curves like curve d and curve 3 which are below what existing specifications would call for ?*

D. SFINTESCO :

*The sixth session was dealing with what I would call the final point of the research on column strength : the results which are directly interesting for practical design. In the first paper by Marek and Skaloud we had a description of the current CS specifications and also indications on the new ones which are being prepared now. We have seen that they apply the probabilistic approach and we could also notice that they are adopting the European curves, at least the two upper ones, but they found that the lower curve was in their opinion too conservative.*

*The next paper by Bjorhovde and Tall on the development of multiple column curves deals with the American attempt to introduce multiple column curves in order to reduce the deviation between real and design column strength. In the first part the paper gives a review of the deterministic investigations on maximum strength of different shapes. It is followed by a description of the probabilistic computation studies. It shows that in the United States both approaches are now being considered.*

*The third paper by Beer and Schulz on the basis of the European column curves which has been discussed just before, shows the results which have been now adopted by the European Convention. I shall not go into the description of this particular paper. I wish to point out that these curves which have been developed have been found satisfactory enough for being approved by the European Convention and that they are already adopted in current codes or in codes which are submitted for approval in several European countries, at least in Belgium, Italy, Norway, and probably within a short time in several other countries. This means that all these countries have accepted the concept of the multiple column curves and also of the different yield points as explained in the paper of Carpena in the session before.*

*Dr. Dwight's paper, a very comprehensive and interesting paper pleads, as the title says, for interpreting the ECCS curves by means of the Perry-Robertson formula. I must say this demonstration was very convincing and tempting by the arguments which have been given. However, I would point out to the remarks of Prof. Massonnet and myself : there may be also satisfactory interpretations with other theories and other formulae and if one looks at the experimental results it is almost always possible to adjust more than one theoretical approach. This is probably even more so if one thinks that most column curves including those of the ECCS contain amounts of empirical arbitrary and comprise decisions, so it is always possible to adapt them to various theories. I am afraid that several countries may prefer to keep the theories which are familiar to them.*

*Prof. Vogel's paper goes a step further by going from the inexisting pin-ended column to the real structural member. I think in some way his concept can be already found in some theories which have been developed but it is extremely interesting to have this suggestion which opens a practical way for the application of the basic column curves to structural members in compression.*

*The last paper, of Johnston, is analysing the step from the standard or code maker to his victim who is the designer. There is of course a significant difference between the first who may want, and probably have, to refine design rules in order to attain a good approach of the actual carrying capacity of the column and the second who may hate complexity by still being interested in material savings. The author suggests a very simple formulation in good agreement with the curves proposed on both sides of the Atlantic.*

*Finally we had a most interesting report of Mr. Milek reflecting the position of CRC and we are must indebted to him for having reported here on the position of this body. This shows at the same time an open mindedness towards findings from elsewhere and a rather prudent position on some points on which the CRC does not wish to commit itself as long as research is not enough completed.*

*As a final remark I'm saying that in general we can see from these papers a tendency to the general acceptance of the fact that multiple column curves are necessary for a close approach of the carrying capacity of columns of various shapes, sizes and fabrication procedures.*

*The problem of the resulting complexity for the designer is also a general concern. A good philosophy may be even for standards to give the means of a refined method for maximum material savings and an envelope of very simple formulae or tables which can be used depending upon the choice of the designer and of the object of the study.*

*Well, now gentlemen, this closes the summaries of the six sessions. I would like now to ask Prof. Johnston if he may say a few words as closing remarks on this symposium.*

B.G. JOHNSTON :

*As we went through these two days I was struck by the fact that various persons touched on what I would like to call 4 different worlds. First of the world of theory. Within the world of theory we can theorize on theoretical columns and come up with column formulae for either a perfect or imperfect column. Then there is the world of the testing laboratory. Now, with regard to the testing laboratory column we sometimes make the mistake of thinking of it as a real column. But it is almost as far from the world of reality as is the theoretical column. We can learn a great deal more about the testing laboratory column than we can about the real column. Thirdly there is the imaginary real world. Our ideas about the real column in the real world can be formulated on paper but they still are essentially imaginary. Finally there is the real world and the real column in it which involves all of the variables inherent in the problems we have been talking about and in addition the effect of walls, windows, load uncertainties, and many other things. I think it would be well if we tried to be very careful as to which world we are in when we are talking about columns.*

D. SFINTESCO :

*Well gentlemen, before closing this last session I think I should like to make three remarks. First of all at the end of these presentations and discussions we can say that this colloquium has been a success as expected. It may not be unappropriate to think once more that it was initiated by Prof. Beer. Its success is of course due to the large participation of many outstanding personalities from the world of research in this field. We had an impressive number of very interesting papers and of course all these papers and the discussions have to be published in the proceedings of this symposium. No decision has been made yet on the practical way to produce them but in any case we take care to have them. So in this respect I should like to announce two deadlines. The first one does concern myself, I would like to commit myself to send a letter with precise instructions as to the way in which the authors can contribute to these proceedings. I shall try to do it not later than the 15th of December. The authors will be asked to contribute to the practical preparation of the proceedings by giving once more their paper, probably in the standardized form. Thus in most cases they would have to be retyped. It will be easier for every author to retype his own paper than for somebody else to type all of them. We should like to have these new versions sent by the authors before the end of January. Of course there will be a difficult problem about the transcription of the discussions. We have no solution yet and we shall look for that.*

*Another point : we said and I think everybody is convinced that this colloquium was very useful. Therefore it is very natural to think that another one should be held after some time. This was the first time that people from several areas of the world have met together on this particular problem. As we have seen for instance from Mr. Milek's report, there are lots of gaps in our knowledge. There are some problems for the pin-ended column still to be studied, but a most important point is of course the real bar within the structure. I think the next colloquium could* *be devoted to these two aspects : completing the knowledge on the pin-ended column and, probably even more important, dealing with the real member in the structure. In my opinion it would be quite reasonable to think of such a meeting in three years from now.*

*Now my last words will be to thank the authors and the discussers for their outstanding contributions. To thank especially all those who have travelled from far to come for this special meeting. I may be allowed to express also thanks to those of my staff who helped organize and hold this meeting successfully. Now a last personal remark. I took the liberty to prepare for all foreign participants a small personal present which you will find just in going outside from here as a souvenir from this colloquium. An dnow I am pleased to pass the chair to our host Mr. Wahl.*

<u>L. WAHL</u> :

*Gentlemen, we are now at the end of this meeting and I have the task of closing a meeting which has lasted for two long days of work. I suppose that many of you are very tired already but I hope nevertheless that none of you has been overstressed. You have listened to about 40 different contributions and the various chairmen have tried to summarize the results of the presentations and discussions. Let me hope that the engineers will soon know exactly how to design a column of optimum dimension in a building. Perhaps as suggested by Mr. Sfintesco you are so happy about this meeting that you may as well stay for a while in Paris and enjoy some other of its aspects. I thank everybody for coming, I thank Mr. Sfintesco, his staff and the secretaries for the organization. Thank you.*

<u>L.S. BEEDLE</u> :

*We certainly cannot leave without expressing our appreciation to our host Mr. Wahl and especially to Mr. Sfintesco, for someone who in August, this past August, had to pick up the challenge of organizing this meeting and holding it and conducting it in such fine style really deserves our thanks.*