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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, y ou 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.