Discussion on the 3rd working session

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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-δ-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.

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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 (δ_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 δ_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 (δ_0) into account. But on the other hand it could be argued statistically that you won't get the worst (δ_0) at the same time you get the most rounded knee. So I shall leave you with those thoughts.

<u>Ch. MASSONNET</u> :

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.