Discussion on the 4th working session

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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.