Discussion

Elastic compressive-flexural-torsional buckling in structural members

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The article of Raven, Blaauwendraad and Vamberský in HERON 53, No. 1 discusses stability checking of torsional buckling in structural members loaded by compressive forces and bending moments. Unfortunately, many important issues are not considered and need to be commented on.
This discussion uses the same notation, literature references and equations as in the article. For new equations and references lower case characters are used. Due to space limitations, eight of the most important comments are discussed below. Further details can be found in reference [a].

1. The ultimate state applies; therefore, the bending strength according to the applied linear bending stress diagram has to be adapted to the occurring nonlinear elastic-plastic behaviour by profile factors in accordance with the failure criterion. Also the matching apparent E-modulus has to be determined from this ultimate state.

2. From the 6 equilibrium equations, the forces are eliminated so that 3 moment differential equations of the 4th order remain. The applied twice integrated 2nd order equations with zero integration constants are not generally applicable.

3. The serviceability criterion cannot be applied for stability (see [a]), also not in the ultimate limit state. Therefore, the "alarming function" of n has no meaning.

4. The failure criterion [12] is a straight line approach of the failure curve that – with exception of the Timber Code, which only applies for simple cases – has to be approximated by 2 straight lines, giving the right failure criterion for every situation. The applied straight line cut off of the Code, called unity check by the authors is not a superposition of stresses, as given in Fig. 13, and is not, at the same time, a totally different

sum of weighted stresses (Eq.(40)). Fig. 13 is wrong because it does not show the ultimate elastic-plastic stress state that determines the expression of the failure criterion.

5. Superposition is not allowed for stability, also not in the elastic stage. Thus, Eqs.(39a/b) are wrong. The analysis should be based on the first expanded Fourier term of the total maximal load. This means that in the article only partial loading is regarded without the right general treatment by this first expanded term of the total determining load.

6. The series and parallel linking of n-values is not valid, even not in the most simple Eulerian loading case discussed here where all eccentricities and M_z are zero because

$$\frac{1}{n} = \frac{F}{F_{ez}} + \left(\frac{M_y}{M_k}\right)^2 = \frac{F}{F_{ez}} + \left(\frac{M_y}{M_{k0}}\right)^2 \frac{1}{(1-F/F_{ey})(1-F/F_T)} \, . \tag{34}$$

This is not a simple linking of n-values because F_{ey} and F_T are not negligible (e.g. for thin walled profiles with nearly equal stiffness in both main directions).

7. The interaction equation between failure by buckling and by lateral buckling is the critical stability equation for all materials. This equation is lacking in the article.

8. In the remainder of this discussion it will be shown that the chosen splitting of variables is not allowed. The splitting

$$v = v_0 + v_1 + v_2 \tag{9}$$

of the variable v into an initial value v_0 ; a first order displacement by the external load v_1 and a second order displacement v_2 , is impossible because the components: v_1 , v_2 follow the second order equation, Eq.(a), and v_1 cannot satisfy the first order equation, Eq.(b), as well. The splitting of variables is incorrectly based on the loading case Eq.(17) because for $M_{x1} = M_{z1} = 0$, only a trivial solution of Eq.(b) is possible which is $v_1 = \varphi_1 = 0$. Non-trivial solutions follow from possible loading cases of [12], given in [14] as Eq.(4.07). Then, following the article, splitting of: $w = w_1 + w_2$; $v = v_1 + v_2$; $\varphi = \varphi_1 + \varphi_2$ (when initial values are left out for shortness of illustration) leads to

$$EI_y(w_1+w_2)''+Fw-M_{x1}v'-M_{z1}\varphi+M_{y1}=0\ ,$$
$$EI_z(v_1+v_2)''+Fv+M_{x1}w'+M_{y1}\varphi+M_{z1}=0\ , \tag{a}$$
$$-GI_t(\varphi_1+\varphi_2)'-M_{z1}w'+M_{y1}v'+M_{x1}+M_{x2}=0\ .$$

The displacements with index 1 should also follow the linearized equations

$$EI_y(w_1)''+M_{y1}=0\ ,$$
$$EI_z(v_1)''+M_{z1}=0\ , \tag{b}$$
$$-GI_t(\varphi_1)'+M_{x1}=0\ .$$

As mentioned this is impossible because w_1 , v_1 and φ_1 cannot satisfy Eq.(a) and Eq.(b) at the same time. Substitution of Eq.(b) into Eq.(a) for a solution, as done in the article, gives Eq.(4.08) of [14] and Eq.(18a) (for small values of M_{x1} and M_{z1})

$$EI_y(w_2)''+F(w_1+w_2)-M_{x1}(v_1+v_2)'-M_{z1}(\varphi_1+\varphi_2)=0$$
$$EI_z(v_2)''+F(v_1+v_2)+M_{x1}(w_1+w_2)'+M_{y1}(\varphi_1+\varphi_2)=0 \tag{c}$$
$$-GI_t(\varphi_2)'-M_{z1}(w_1+w_2)'+M_{y1}(v_1+v_2)'+M_{x2}=0$$

However, this is only a partial substitution. A further elimination of index 1 deformations to solve the index 2 deformations gives for the first of Eqs.(c)

$$EI_y(w_2)''''+F(w_2)''-M_{x1}(v_2)'''-M_{z1}(\varphi_2)''=F\frac{M_{y1}}{EI_y}-M_{x1}\frac{(M_{z1})'}{EI_z}+M_{z1}\frac{(M_{x1})'}{GI_t}\ , \tag{d}$$

showing the primary moments still being in the second order equation, now in a wrong way because the first equation of Eqs.(a) should be equal to Eq.(d) when the index 1 terms of Eq.(a) approach zero, thus when the first term in the right hand side of Eq.(d) is $(M_{y1})''$. This is not the case, also not for the regarded fictive central loading without primary moments $M_{x1}=M_{z1}=0$ with $\varphi_0=\varphi_1=0$, giving $F(M_{y1}/EI_y)$ in stead of $(M_{y1})''$. Thus index 1 displacements have no meaning and have to be omitted and index 2 displacements like v_2 should be read as $v_2=v-v_0$, where v is the total displacement following as solution of Eq.(a). The second order moments, thus, do not follow from the wrong Eq.(c), that for $M_{x1} = M_{z1} = 0$ is equal to Eq.(18a).

$$\text{e.g.: } EI_y(w - w_0)'' + Fw = 0 \tag{18a}$$

with the solution $M_{y2} = nFw_0/(n-1)$, but follow from the subtraction of the solution of the linearized equation, Eq.(b), from the result of the solution of the non-linear equation Eq.(a). Thus $M_{y2} = n(M_{y1} + Fw_0)/(n-1) - M_{y1} = (M_{y1} + nFw_0)/(n-1)$, what is not noticed in the article. This also leads to a wrong definition of $n = n^*$. The definition of n^* in Eqs.(12) and (13) is an identity, replacing v_2 by v/n^* or n^* is a superfluous shortcut for $v/v_2 = (v_0 + v_1 + v_2)/v_2$. Thus, it has nothing to do with the multiplication factor n because that factor cannot be stated in advance but follows from the solution of the total displacement v of the differential equations Eq.(a). Inserting the shortcuts Eq.(12a/b), $(n^*)_y = n_y = w/w_2 = w/(w - w_0)$ and $(n^*)_z = n_z = v/v_2 = v/(v - v_0)$ with $M_{x1} = M_{z1} = 0$, in Eq.(c) = Eq.(18a) gives

$$\begin{aligned} &EI_y w''/n_y + Fw = 0\,, \\ &EI_z v''/n_z + Fv + M_y \varphi = 0\,, \\ &-GI_t \varphi' + M_y v' = 0\,. \end{aligned} \tag{e}$$

These equations cannot be solved for constant values of n_y and n_z as is done in Eq.(6.08) of [14], because e.g. $n_z = v/v_2 = v/(v - v_0)$ cannot be constant because v_0, the initial value of v, can not be proportional to v. Thus, the n^*-method - the new approach of the article and thesis - is not correct. (See [a] for the meaning of n^*). In the article the trivial solution of Eq.(b), $v_1 = \varphi_1 = 0$, is applied as a real solution and thus effective splitting of variables is applied for w only and w_1 is solved from Eq.(b) leading to a wrong failure condition Eq.(40).

$$\frac{F}{F_u} + \frac{M_{y1}}{M_{uy}} + \frac{M_{z2}}{M_{uz}} = 1 \tag{40}$$

without a multiplication of first order moment M_y. This should be

$$\frac{F}{F_u} + \frac{M_y + Fw_0}{M_{uy}(1 - F / F_{Ey})} + \frac{M_{z2}}{M_{uz}} = 1 \,. \tag{f}$$

This wrong failure criterion Eq.(40) – applied in the calculation examples of the article - is also inserted in the finite element solution of [14] what explains the same result of both identical calculations despite the error. The second term of Eq.(f) dominates in the failure criterion when the stiffness of both main directions are not far apart, leading to severe unsafe errors in design. Lack of knowledge of the failure criterion leads to other unpredictable errors as for instance in Section 6.2, the calculation example, where the failure criterion is extended to the fourth dimension by an additional bending term

$$\frac{F}{F_u} + \frac{M_y}{M_{uy}} + \frac{M_z}{M_{uz}} + \frac{M_{zfl}}{M_{uzfl}} = 1 \,, \tag{g}$$

wrongly called superposition. In Eq.(g) is $M_{zfl} = 0$ the self-equilibrium bimoment, causing zero bending moment on the section (see [4] p. 274).

The approach, starting with the same high beam equations as the Dutch Timber Code [12], [15], is restricted in the stiff direction to the solution of the first Eq.(18a), of a lateral rigidly supported beam loaded in compression with no second order effect of the bending moment, while the second order effects of compression for buckling and torsional buckling are omitted from the lateral buckling equations, Eq.(18b), (to obtain uncoupling) which therefore only apply for zero primary loading, $M_{x1} = M_{z1} = 0$ and zero eccentricities $\varphi_o = e_y = 0$. The wrong failure criterion and negligence of F_{ey} and F_T in Eq.(34), Eq.(40) and Eq.(f), show that the approach can be dangerous.

References

[a] T.A.C.M. van der Put, "Critics on the thesis and Heron article of Raven et al", online Dec. 2007, www.dwsf.nl/downloads

Author's reply

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Reading the criticisms, the authors feel that Dr. Van der Put (here after: critic) misunderstands or at least misinterprets our paper. Hereafter, the comments are addressed in the same order as presented.

1. Indeed, the developed analysis method only applies for elasticity. This has been purposely decided by the authors. They believe that timber and steel structures hardly can profit from plasticity considerations. Additional strength capacity practically gets lost again because of loss of stiffness.

2. Yes, we eliminate the forces and work with equations of moments only. This is what all renowned experts in stability research do for the considered type of beam-columns (i.e. Chen and Atsuta). Shortening due to normal forces and shear deformation can be readily neglected.

3. We agree that serviceability cannot be applied in the ultimate limit state. But we also know that some might like to apply this type of criteria in the serviceability state. The article just shows that displacements can be calculated, using the same method, if wanted.

4. The fourth item of criticism regards superposition. Superposition of loads can yield disastrous results. However, once the displacements have been determined for a given load combination, then the normal force and bending moments can be computed and we must superpose the stresses they cause.

5. Strictly speaking, the criticism on superposition is correct in terms of rigorous mathematics. However, for practical application the proposed superposition appears very useful. It has been verified that negligible errors are made. This has little to do with whether or not applying expansion series.

6. Critic disputes the item of parallel and serial linking. He replaces the formula of the authors

$$\frac{1}{n_z^*} = \frac{1}{n_{zM}^*} + \frac{1}{n_{zF}^*}$$

by

$$\frac{1}{n} = \frac{F}{F_{Ez}} + \left(\frac{M_y}{M_k}\right)^2 = \frac{F}{F_{Ez}} + \left(\frac{M_y}{M_{k0}}\right)^2 \frac{1}{\left(1 - F / F_{Ey}\right)\left(1 - F / F_T\right)}$$

but omits the factor $\left(1 - I_z / I_y\right)$ in the numerator, the eccentricity of loading in the denominator and all constants for the applied member types and loads.

The authors know that further refinement of the theory indeed justifies considering the additional factor. Here again, the authors have by purpose chosen for an easy, sufficiently reliable method, rather than sophisticating up to the last percentage (see again Chen and Atsuta [4]).

7. The equation for interaction between failure by buckling and by lateral buckling is lacking in the article. Yes, because the authors have well-founded that buckling and lateral buckling can be tackled independently for the considered class of problems.

8. A. Criticism is: splitting of the variables is not allowed. Unfortunately, this is a stiff misconception. The critic supports his statement by the argument that the variables must satisfy both his sets of differential equations (a) and (b), which he considers being impossible. However, this is not judged correctly. The authors first solve the 1st order problem, which automatically implies that Eq. (b) will be satisfied. Now displacement w_1 and M_{y1} are known and can be entered in the 2nd order Eq. (a), from which the displacements w_2 and φ_2 subsequently can be solved.

B. It is unclear to the authors what critic tries to prove in Eq. (c) and Eq. (d). He produces Eq. (c) by substitution of Eq. (b) in Eq. (a), which is still understood. The next step is the derivation of Eq. (d), which is unclear. Anyhow, the authors disagree very much with Eq. (d), and the conclusion based on it is definitely wrong. Authors are deeply convinced of the correctness of the decomposition of displacements in an initial, 1st order and 2nd order part. It is pertinent sound from the viewpoint of mathematics and has been checked in a number of ways, amongst which comparison with results of finite element analyses.

C. Critic has a valid point in objecting to the unity-check:

$$\frac{F}{F_u}+\frac{M_{y1}}{M_{uy}}+\frac{M_{z2}}{M_{uz}}=1$$

Indeed, in spite of the correct notation in Eq. (40) in the further development here the amplification factor for moments in the strong direction has been omitted. To do it correctly, one should not use the 1st order moment $\bar{M}_{y1}$, but the total moment:

$$\bar{M}_y=\left(\bar{M}_{y1}+F\bar{w}_0\right)/\left(1-F/F_{Ey}\right).$$

The authors let themselves rule by the experience that the effect is small in practical applications. E.g. the result in the calculation example (p. 195):

$$\frac{555+300\times 0,020}{1125\times(1-300/29265)}-\frac{555}{1125}=0,011\text{, causes an increase of only 1 \%.}$$

It does not alter the fact that critic is right.

D. Authors are surprised by the comment on the contribution of the self-equilibrating bimoment in the unity-check. Yes, the bimoment causes a zero bending moment on the section, however, pertinently non-zero stresses. For I-sections the contribution to the total stress can be significant.

Summing up, the difference in opinion between critic and authors mainly focuses towards three items: (i) Elasticity versus plasticity, (ii) Theoretical strictness or practical accuracy, (iii) Valid or invalid differential equations. Authors have given clear grounds for all choices made and have benchmarked the results in a number of ways. Hence, the approach of the article is not dangerous, on the contrary, it has produced reliable and pleasantly-applicable results for a clearly-defined class of problems.