INSTABILITY OF AN ELASTICALLY SUPPORTED BEAM UNDER A TRAVELLING INERTIA LOAD

By D. E. Newland*

It is shown that an unstable bending wave may be excited in an elastically supported beam by a travelling inertia load. Since the occurrence of this dynamic instability reduces the axial buckling load of the beam, the result is relevant to present studies of the temperature buckling of continuous welded railway track.

INTRODUCTION

A RECENT research note by Binnie (1)† showed how unstable waves may be generated in a flexible membrane by a stream of air. The mechanism of instability involves inertia forces from the moving air stream setting up travelling waves in the stationary membrane. It occurred to the author that the same mechanism would be effective when the inertia load of a moving train travels over laterally flexible railway track.

Continuous welded track may buckle sideways if the axial compressive load in the rails due to thermal expansion is sufficiently high. A lateral kink then appears in an otherwise straight line or smooth curve. Buckling is prevented by ensuring that the ballast provides adequate lateral support. Although there has been a great deal of research into railway track behaviour, some of which has been published (2) (3), experimental evidence (4) indicates that the calculated safety margins may apparently be reduced under adverse conditions. Since, to the author's knowledge, published theories have not considered the lateral dynamic instability introduced by a moving train, it is of interest to calculate the magnitude of this effect, particularly as it has been noticed that buckling sometimes develops under a moving train. In applying the basic theory of this note to the railway problem, only lateral bending waves of the track are considered; vertical oscillations are not included.

Theory of bending waves

The track is assumed to be represented by a straight beam of mass per unit length m and bending stiffness EI, supported on an elastic foundation of lateral spring stiffness γ per unit length. An axial compression load P acts as a result of temperature stresses in the rails. An infinitely long train of mass per unit length M and zero stiffness is assumed to be travelling along the track at speed U. If the lateral deflection of the track from its centre-line is y at axial co-ordinate x (Fig. 1), and the lateral reaction force between track and train is $F\,dx$, the equation of motion of the track is (5)

$$EI\frac{\partial^4 y}{\partial x^4}+P\frac{\partial^2 y}{\partial x^2}+m\frac{\partial^2 y}{\partial t^2}+\gamma y+F = 0 \quad . \quad (1)$$

The reaction force $F\,dx$ may be obtained by determining the lateral acceleration of length dx of the moving train, which, if the train has velocity U in the $+x$ direction, is

$$\left(\frac{\partial}{\partial t}+U\frac{\partial}{\partial x}\right)^2 y$$

Therefore

$$F = M\left(\frac{\partial^2 y}{\partial t^2}+2U\frac{\partial^2 y}{\partial x\,\partial t}+U^2\frac{\partial^2 y}{\partial x^2}\right) \quad . \quad (2)$$

which, on substitution into equation (1) gives the full equation of motion

$$EI\frac{\partial^4 y}{\partial x^4}+(P+MU^2)\frac{\partial^2 y}{\partial x^2}+2MU\frac{\partial^2 y}{\partial x\,\partial t}+(m+M)\frac{\partial^2 y}{\partial t^2}+\gamma y = 0 \quad (3)$$

One solution of equation (3) is a travelling wave described by

$$y = \exp\{i(kx-\omega t)\} \quad . \quad . \quad . \quad (4)$$

Substituting equation (4) into (3), it can be seen that k and ω satisfy the dispersion relation

$$EIk^4-(P+MU^2)k^2+2MUk\omega-(m+M)\omega^2+\gamma = 0 \quad (5)$$

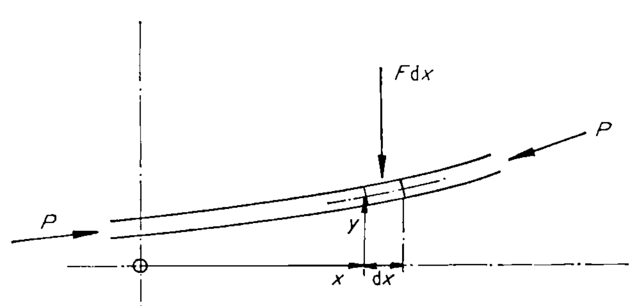

Fig. 1. *Plan view of the lateral deflection of an axially loaded beam*

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Conditions for instability

Following Binnie (1), if an instability exists the parameter ω must be complex so that

$$\omega = \alpha + i\beta \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Substituting equation (6) into (5) and equating real and imaginary parts gives

$$\alpha = \frac{MUk}{M+m} \quad . \quad . \quad . \quad . \quad (7)$$

$$\beta^2 = \frac{\left(P+\frac{mM}{m+M}U^2\right)k^2 - EIk^4 - \gamma}{(m+M)} \quad . \quad (8)$$

The system will be unstable if β has a positive real value, which will be the case if the numerator of (8) is positive, i.e. if

$$\left(P+\frac{mM}{m+M}U^2\right) > EIk^2 + \frac{\gamma}{k^2} \quad . \quad . \quad (9)$$

The right-hand side of inequality (9) depends on wave number k, and will have its minimum value when, by differentiation,

$$k = \left(\frac{\gamma}{EI}\right)^{1/4} \quad . \quad . \quad . \quad . \quad (10)$$

Hence, the limiting condition for instability is

$$\left(P+\frac{mM}{m+M}U^2\right) = 2\sqrt{EI\gamma} \quad . \quad . \quad (11)$$

When the velocity U is zero, this condition is merely that the axial load reaches its static buckling value (5)

$$P = 2\sqrt{EI\gamma} \quad . \quad . \quad . \quad . \quad (12)$$

When the velocity is not zero, the inertia loading of the moving train interacts with the forces in the track to reduce the axial load at which buckling will occur. The presence of a moving train may therefore reduce the lateral stability of railway track, and allow buckling to occur at a lower axial load than would otherwise be the case.

Numerical example

Tests on straight welded railway track (4) have shown that axial loads up to about 170 ton ($1{\cdot}7\times10^6$ N) can be resisted without buckling. This corresponds to a temperature rise of about 94 degF (52 degC) above the stress-free condition.

The typical mass of track per unit length (including rails, sleepers and fastenings) is about 170 lb/ft (250 kg/m); for the train it could be about 1500 lb/ft (2200 kg/m). Therefore at 100 mile/h (160 km/h)

$$\left(\frac{mM}{m+M}\right)U^2 \simeq 45 \text{ ton } (4{\cdot}5\times10^5 \text{ N})$$

The maximum allowable temperature rise may thus apparently be reduced by about

$$\left(\frac{45}{170}\right)(94) = 25 \text{ degF } (14 \text{ degC})$$

on account of the unstable effect of a passing train.

This conclusion is, of course, dependent on the far-reaching assumptions described above. Nevertheless it appears to merit further investigation.

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COMMUNICATION

The following Communication relates to the paper 'Communication on the invalid use of an isentropic expansion index for wet steam', by D. J. RYLEY, published in 1970, vol. 12, No. 3, p. 215.

Professor G. F. C. Rogers, B.Sc., C.Eng., F.I.Mech.E., and **Mr Y. R. Mayhew,** B.Sc.(Eng.), C.Eng., F.I.Mech.E. —In our view the most important point that Dr Ryley raises in his Communication is contained in the 'Recommended procedure', where he says that when steam *initially in wet equilibrium* expands the vapour phase *will* supersaturate. Only at some point later will nucleation and approximately equilibrium flow occur. This is certainly not generally known: nor does it conclusively follow from his own earlier paper [his reference (**21**)] from which it would seem that *may* is a more appropriate word than *will*. If indeed it is believed that initially wet steam is supersaturating upstream of a nozzle throat how should the designer proceed? Dr Ryley's Communication would have been more helpful if he had explained the procedure rather than refer to (**21**) and (**22**) which are by no means clear on this point.

There are a number of other points in Dr Ryley's Communication which call for comment.

(1) In the synopsis he states that 'The use of the Zeuner equation $\gamma = 1{\cdot}035 + x_1/10$, based upon equilibrium expansion, is therefore invalid and should be discontinued'. He seems to contradict this in the conclusions where he says that 'It may, however, be stated that after nucleation has occurred in a nozzle, the remainder of the expansion thereafter is nearly in thermal equilibrium', thus implying that some recommendation for the isentropic index in a wet equilibrium expansion is indeed needed after all.

(2) When a fluid whose state is fixed by two independent properties, e.g. $v = f(p, s)$, expands isentropically, inevitably there will exist a relation $v = f(p)$. Sometimes the relation takes the form pv^n = constant precisely and sometimes it will take this form approximately. But we strongly deprecate the use of the symbol γ for n, because according to B.S. 1991 and common usage γ is defined as c_p/c_v. The isentropic index is

$$n = \left[\frac{\partial(\ln p)}{\partial(\ln v)}\right]_s = -\frac{v}{p}\left(\frac{\partial p}{\partial v}\right)_s = -\frac{\partial v}{\partial p}\left(\frac{\partial p}{\partial v}\right)_T \frac{c_p}{c_v}$$

and it is equal to c_p/c_v only for a limited number of fluids, e.g. for perfect gases and for a gas obeying the Callendar equation

$$u - B = \frac{1}{n-1}\, p(v - C)$$

(3) Dr Ryley states that 'Proceeding from equation (1) and ideal gas theory it is quickly shown that the critical pressure ratio, r_c, for expansion in a nozzle is $r_c = (2/\gamma + 1)^{\gamma/(\gamma-1)}$'. In fact, as shown in his reference (**17**), pp. 398–99, it is only necessary to assume equation (1). Ideal gas assumptions are irrelevant except in so far that an ideal gas (i.e. one obeying Boyle's Law, Joule's Law *and* having constant specific heats) happens to follow equation (1) precisely in an isentropic change.

(4) Dr Ryley says that 'Most modern steam tables now avoid reference to a wet steam expansion index, though it survives in one case (**19**)'. In fact the most modern tables, viz. 'Properties of Water and Steam in SI Units' by E. Schmidt (Springer, 1969), give extensive data for this very purpose.

(5) In the 'Recommended procedure' with steam initially dry, Dr Ryley advises us to assume that the 'Expansion is accurately described by the law pV^γ = constant . . .. The value will normally vary during an expansion and a suitable mean value should be chosen'. But the second sentence contradicts the first, because if the index varies the polytropic law cannot *accurately* describe the expansion.

Without wishing to go out of our way to defend his reference (**17**) on the problem raised, it is really difficult to see where we and many others have sinned. Two issues must be distinguished: (1) when does equilibrium expansion occur precisely or at least approximately, and (2) when can an isentropic process, whether in stable or metastable equilibrium, be adequately described by a polytropic law with an index n.

In chapter 9 of reference (**17**) on non-flow processes we discuss in principle the stable-equilibrium isentropic expansion which may, for example, quite reasonably occur in a cylinder. In this context we stress the precise polytropic expansion of a perfect gas with constant specific heats, and the approximate nature of the polytropic index in superheated or wet expansion, together with an example of how the mean value of n can be calculated in a particular isentropic expansion. The relation there discussed would be equally applicable in steady-flow processes where equilibrium expansion was possible, or where superheated behaviour can be extrapolated into the meta-stable supersaturated region. Thus the perfectly valid conclusions for equilibrium expansion (non-flow) are referred forward to chapter 18 on one-dimensional flow.

In chapter 18 simple isentropic flow relations are derived which are valid for any fluid when a polytropic index can be assumed to be constant throughout an entire expansion. According to Dr Ryley, and we concur, this is of interest where an expansion is entirely (*a*) in the superheat region, (*b*) in the superheated and supersaturated region, (*c*) in the wet equilibrium region. It is merely incidental whether the very approximate values of n quoted are used, or a mean value calculated from the proper end states, or an appropriate value taken from his references (**18**) or (**20**) or from Schmidt's tables mentioned earlier.

The principal difficulty arises when there is an appreciable discontinuity as when nucleation occurs after the throat in a nozzle. It would certainly be helpful if Dr Ryley would clarify the procedure to be followed by a designer in this situation, if indeed anything can be done beyond including the increase in entropy due to this irreversibility in the overall isentropic efficiency of the nozzle.

AUTHOR'S REPLY

Dr D. J. Ryley—I am grateful to Professor Rogers and Mr Mayhew for the trouble they have taken to prepare so detailed a communication. I will reply to their numbered points first.

(1) It was originally observed by Binnie and Woods (**C1**) that following reversion in their nozzles the steam continued its expansion nearly in equilibrium. It was not quite in equilibrium: some small degree of supersaturation remained. Because this occurs in a nozzle one must not assume it necessarily occurs in any other flow passage. In rare cases where a slow expansion occurs and the specific surface area of liquid–vapour interface is very great, an isentropic index could be recommended.

(2) I agree. The distinction between isentropic and polytropic processes is now understood and appropriate symbols recommended. This distinction has not always been understood and if one reads the older papers and books confusion is found both in symbols and ideas.

(3) I agree. The denominator in equation (7) should, of course, read $\gamma+1$.

(4) I did, in fact, inspect a number of steam tables from different origins, but the publication by E. Schmidt was not available when my Communication was written. It was, however, anticipated in the paper by Bach (original reference (**18**)) and I believe the data referred to by Professor Rogers and Mr Mayhew are here included. This paper perpetuates precisely the outlook which I am pleading is incorrect. Consider, for example, the issue of sonic velocity, V_a, in wet steam. For *gases* $V_a = \sqrt{\gamma RT}$, but an attempt to use this to determine experimentally the value of γ for wet steam incurs the contradictions which Bach observed, on which he commented, but the significance of which he failed to understand. Taking his Fig. 3, there is seen to be a huge predicted discontinuity in the magnitude of the sonic velocity across the saturated steam line. If an experimental measurement is made of the sonic velocity within a given volume of saturated steam, a single large drop of water then introduced and the measurement retaken it is absurd to suppose that V_a will have changed by several hundred feet per second. Measurements made by Deich *et al.* (**C2**) do in fact show that the sonic velocity is not subject to any rapid change at the phase boundary curves. This was also found experimentally some years earlier by Clinch (**C3**). Sonic velocity is related to sonic frequency and liquid dispersion in addition to dryness fraction.

The inadequacy of equation (2) may be further illustrated by considering wet steam expansion (say) in a cylinder. Suppose again that the liquid mass presents a minimal interfacial area to the vapour. Rapid expansion causes the vapour fraction to expand virtually as if dry with its own 'private' expansion index which will be approximately the mean isentropic value appropriate to the conditions. The phase change across the interface proceeds much more slowly and may barely have commenced when the volume expansion is complete. The expansion index in equation (2), supposedly applicable to the mixture, has not described the process. In this process one must recognize that end states have become subservient to rate processes. It is of interest to know that the first suspicions of delayed condensation arose from observations on the reciprocating steam engine (**C4**).

(5) The issue here is semantic. The expansion obeys the law pV^{γ} = constant inasmuch that $\gamma = C_p/C_V$ has a precise meaning and the phenomenon is not complicated by phase change. Unless $d\gamma = 0$ in addition to ds (which can occur for limited expansion ratios) a constant value of γ cannot be used and one must resort to a mean value.

Supersaturation in turbines is now principally of interest to turbine designers because nucleation influences blade erosion, and it is against this background that the work leading to references (**21**) and (**22**) was conducted. My correspondents claim that these papers lack clarity. They certainly do! Nature does not see fit to simplify her laws for the convenience of steam engineers and these papers are attempts to analyse very complicated phenomena. Professor Rogers and Mr Mayhew will place us all still further in their debt if they can reduce nucleation and kinetic drop growth theory to the level of undergraduate understanding!

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