1997

Discussion on the paper by A. Khalak & C.H.K., Williamson Dynamics of a Hydroelastic Cylinder with Very low Mass and Damping

Khalak, A.

Journal of Fluids and Structures (1997) 11 , 549 – 552

Journal of Fluids and Structures (1997) **11,** 549–552

DISCUSSION

ON THE PAPER BY
A. KHALAK & C. H. K. WILLIAMSON

"DYNAMICS OF A HYDROELASTIC CYLINDER WITH VERY LOW MASS AND DAMPING",

JFS 1996, **10,** 455–472

T. SARPKAYA

In a special issue of this journal (Vol. 10, 1986), *in memoriam* of the late O. M. Griffin, Khalak & Williamson (referred to hereafter as KW) discussed the "Dynamics of a hydroelastic cylinder with very low mass and damping" (pp. 455–472). The invited article is not correct in its interpretation of the history of the subject. Two of the citations made to this writer (TS) read: "Sarpkaya (1978) had argued, on the basis of numerical experiments with a wake oscillator model, that the response probably did not go inversely proportional to S_G for values *larger* than unity," (p. 456), and then again "Sarpkaya (1979) argues that the added mass term should not be arbitrarily separated from the fluid force." This writer regrets to note that both comments of KW are out of context and do not correctly reflect either the past (1978, 1979) or the present views of TS. This writer will, therefore, explain briefly what he has stated (not argued) in 1978 and in 1979. As to the analysis of KW, their conclusions speak for themselves: "The results of this logic are in direct opposition to the experimental results which show a greater amplitude for lower m*, in the case of lower branch resonance."

Regarding their first comment, this writer wishes to state categorically that he has never used either what was then known as the "wake oscillator model" or its various versions. Such a model was proposed by Hartlen & Currie (1970) and subsequently explored in great detail by the late O. M. Griffin in his numerous papers on wake oscillators, until he too finally discarded it. The low esteem this writer had of the oscillator models was clearly stated in Sarpkaya (1979). If the authors would peruse TS's paper, carefully this time, they will note that what this writer has done was to present the exact equation of motion (without any assumptions regarding the mass or added mass) and equate it to the measured fluid force which, in turn, was decomposed into two parts, as in Morison's equation. Subsequently, the experimentally-determined force coefficients were used to represent the total force and to predict the dynamic response with great accuracy. The wake oscillator model of Hartlen & Currie (1970) and its numerous versions have failed to do the same during the past 26 years. However, this writer's results and the method of analysis have been successfully used by many others [e.g., Iwan & Botelho (1985), Iwan & Jones (1987), Pantazopoulos (1994), Patrikalakis & Chryssostomidis (1985), Staubli (1983)] and the accuracy of the TS data has been vindicated by comparison with data obtained by Moe & Wu (1990) in

0889–9746/97/050549 + 04 $25.00/fl970090

Trondheim and by Gopalkrishnan (1992) at MIT, during the past 25 years, as discussed in detail in Sarpkaya (1995).

It is on the basis of his experimental and numerical work, not on the basis of some arguments based on numerical experiments, that Sarpkaya (1978, 1979) has derived the relative amplitude expression given by

$$A/D = 0{\cdot}32/[0{\cdot}06 + S_G^2]^{1/2}, \tag{1}$$

which replaced an earlier, purely empirical equation, obtained by Griffin *et al.* (1975) through the use of a least-squares fit to the then existing data. Equation (1) yields a limiting amplitude of $A/D = 1{\cdot}3$ at $S_G = 0$ and still represents the existing data rather well.

Let us now consider Khalak & Williamson's second citation to our work: "Sarpkaya (1979) argues that the added mass term should not be arbitrarily separated from the fluid force. However, his argument addresses the practice of some researchers who model the flow to define an *empirical* added mass term which represents all of the acceleration-dependent forces. Instead we separate the inviscid fluid forces from the viscous forces." Obviously, KW's comment was in defense of their unsuccessful attempt to divide the transverse force into an inviscid added mass (in their case, the displaced mass of the cylinder) times the transverse acceleration and a viscous force dependent on the square of the ambient velocity. At one point in their highly simplistic analysis, they even talk of "constants which must be *measured empirically.*" Their conclusions, which will not be discussed here in detail for the sake of brevity, show the futility of their lengthy arguments. In fact, simple reflection shows that their proposal cannot possibly succeed as it stands, but this is not the time nor the place to discuss it further.

The third point we wish to touch upon briefly is KW's discussion of the effect of m* for constant damping. They have stated, *without any references,* that "A basic question one may pose is whether, for low mass-damping the effects of m* and ζ are described by their effects on the mass-damping, m*ζ." This is a very old question. This writer first raised it on 6 March 1976 at a meeting at CalTech, with Professors A. Roshko, W. D. Iwan, and D. Coles, among others. Subsequently, TS has discussed it in great detail in Sarpkaya (1978) and at numerous meetings, including BOSS-92. Evidently KW, having seen Sarpkaya (1978), apparently overlooked the detailed discussion of m*ζ.

In closing, I regret that I was unable to accept Professor Dalton's invitation to contribute to the Griffin Memorial issue of JFS, notwithstanding the comments of the Editor in his Epilogue.

References

Griffin, O. M., Skop, R. A. & Ramberg, S. E. 1975 The resonant, vortex-excited vibrations of structures and cable systems. Paper OTC-2319, *Offshore Technology Conference,* Houston, TX.

Hartlen, R. T. & Currie, I. G. 1970 Lift-oscillator model of vortex induced vibration. *ASCE Journal of the Engineering Mechanics Division* **96,** EM5, 577–591.

Iwan, W. D. & Botelho, D. L. R. 1985 Vortex induced oscillation of structures in water. *ASCE Journal of Waterways, Port, Coastal and Ocean Division* **111,** WW2, 289–303.

Iwan, W. D. & Jones, N. P. 1987 On the vortex-induced oscillation of long structural elements. *ASME Journal of Energy Resources Technology* **109,** 161–167.

Pantazopoulos, M. S. 1994 Vortex-induced vibration parameters: critical review. *Proceedings of the Offshore Mechanics and Arctic Engineering Conference,* **1,** 199–254.

Patrikalakis, N. M. & Chryssostomidis, C. 1985 Vortex-induced response of a flexible cylinder in a constant current. *ASME Journal of Energy Resources Technology* **107,** 244–249.

Sarpkaya, T. 1978 Fluid forces on oscillating cylinders. *ASCE Journal of Waterways, Port, Coastal and Ocean Division* **104,** WW4, 275–290.

Sarpkaya, T. 1979 Vortex-induced oscillations–a selective review. *Journal of Applied Mechanics,* **46,** 241–258.

Sarpkaya, T. 1995 Hydrodynamic damping, flow-induced oscillations, and biharmonic response. *ASME Journal of Offshore Mechanics and Arctic Engineering* **117,** 232–238.

Staubli, T. 1983 Calculation of the vibration of an elastically mounted cylinder using experimental data from forced oscillation. *ASME Journal of Fluids Engineering* **105,** 225–229.

T. Sarpkaya
Mechanical Engineering
700 Dyer Road, Rm. 339
Naval Postgraduate School
Monterey, CA 93943, U.S.A.

AUTHORS' RESPONSE

We are pleased indeed to receive the thoughtful comments of T. Sarpkaya regarding our paper in the *Journal of Fluids and Structures* (**10,** 455–472, 1996). He has valid points to make. There can be no doubt that Sarpkaya's well-known prolific work and ideas in the area of research on vortex-induced vibrations merit more care and attention in our Introduction and elsewhere in the paper than was given. Our referencing was insufficient and incorrect, for which we must apologise.

We are indeed clearly incorrect to use the terminology "wake oscillator model" applied to Sarpkaya's 1978 paper. We should also refer to the basic question of the relevance of the combined mass-damping parameter to the maximum response amplitude, which was raised originally by Sarpkaya (1978). Reference to his work in the second paragraph of p. 456 should be given as follows:

> "Sarpkaya (1978) has considered the use of this parameter S_g carefully. He has predicted the response of elastically-mounted cylinders using an equation of motion in which he inputs fluid force data extracted from an extensive set of forced vibration experiments. This original technique has been utilized successfully by a number of other researchers since that time (Iwan & Botelho 1985; Patrikalakis & Chryssostomidis 1985; Staubli 1983; and others). From analysis of his results, with this novel approach, Sarpkaya (1978) deduces that *the response amplitude is not affected simply by the parameter S_g alone, but rather by the individual effects of mass and structural damping* (although S_g alone is reasonably good at collapsing the data for S_g above 1·0). In fact, referring to the use of the combined mass-damping parameter, Sarpkaya (1995) later states that "the justification or the possible inadvisability of such a procedure has neither been seriously questioned nor systematically investigated", and he deduces that the dynamic response is governed by the *independent* values of m* and damping. Although we consider this question in the present work, an overall conclusion concerning the usefulness/relevance of the combined mass-damping over a large range of mass and damping clearly cannot be addressed on the basis of the present results."

On p. 468, we should perhaps make clear our intentions with the discussion of the linear theory, as follows:

> "Our intention behind this section, which adapts the traditional linear theory for vortex-induced vibration to the case of low mass ratio, m*, is not to propose this as a model for the obviously nonlinear response behaviour at arbitrary reduced velocity. Rather, the straightforward theory below is made clear, so that we may qualitatively discuss the added mass effects on the amplitude response, without appeal to empirical coefficients at this point."

Finally, we should replace the words "Sarpkaya argues", found in two places in our paper, by the more apt words:

> "Sarpkaya shows".

A. Khalak and C. H. K. Williamson
Mechanical and Aerospace Engineering
Cornell University
Ithaca, NY 14853*, U.S.A.*