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discussion

NOTE: The following three discussions are being reprinted from the December, 1968, issue of the JOURNAL OF BASIC ENGINEERING. The Authors' Closure was not included with the discussions; we apologize for the omission and the inconvenience to the authors.

On Real Fluid Flow Over Yawed Circular Cylinders[1]

C. DALTON.[2] Chiu and Lienhard have used the Blasius-series approach toward computing the laminar boundary layer around the yawed cylinder. The calculation of the crosswise boundary-layer velocity is exactly the same as if the flow were completely two-dimensional. The approach was discussed by Sears [8] and was found to be representative of this type of flow. The method used to compute the crosswise velocity is well known to give an erroneous velocity profile past θ equal to approximately 70 deg from the leading edge of the cylinder. The computed angle of separation was found to be 108.8 deg which is consistent with the results in the discussion of the Blasius-method by Schlichting [10].

The spanwise velocity field was computed from the Blasius-type crossflow and normal velocity components. The spanwise velocity was not found to separate as far as 120 deg from the leading edge.

Since the crosswise flow was determined to separate before the spanwise flow, the authors conclude that the laminar boundary-layer separation is controlled by the crosswise flow. This statement is probably correct, but the conclusion cannot be drawn on the basis of the calculations performed by the authors.

The actual boundary layer is known to separate at approximately 80 deg from the leading edge for an unyawed cylinder. The large difference between the actual and computed crossflow invalidates any use of the computed crossflow toward the determination of any other boundary-layer property. Since the spanwise flow was determined through the use of this inaccurate representation of the crosswise flow, it is felt that the spanwise flow is at least as inaccurate as the crosswise flow in the region between θ equal to 70 deg and θ equal to 108.8 deg.

The inaccuracy involved in the determination of the crosswise and spanwise flows severely limits the use of these velocity components as a basis for drawing any conclusions. Therefore, based on their calculations, it is felt that the authors do not have a basis for stating that the crosswise flow controls separation although this is probably a correct interpretation, as indicated by the experimental results.

[1] By W. S. Chiu and J. H. Lienhard, published in the JOURNAL OF BASIC ENGINEERING, TRANS. ASME, Series D, Vol. 89, No. 4, pp. 851–857.

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A. ROSHKO.[3] The authors state that it is necessary to "learn whether or not the crosswise boundary-layer component is the same as in unyawed flow." It seems to me that one can immediately say it is, on the basis of the general independence principle for crossflow over an infinite yawed cylinder; hence one can immediately state that the "separation point" will be at 108.8 deg, as in the corresponding two-dimensional calculations, and it is not necessary to prove this all over again. On the other hand, all this is beside the point, since a classical boundary-layer calculation is not relevant to the problem of separation from a circular cylinder, which is at this time still very much a research problem. To illustrate this point another way, consider vortex shedding from a cylinder of, say, triangular cross-section, where the separation points are known, a priori; namely, at the edges, for both two-dimensional and yawed cases. But this hardly is a proof that the cosine law will hold for the shedding frequency.

In short, the authors appear to have assumed the result rather than proved it. Their assumption is spelled out in the last sentence on p. 4; it may very well be a valid one, but the discussion preceding it is irrelevant to it.

L. TREFETHEN.[4] Would the authors be willing to comment on whether the vortices are parallel to the yawed cylinder, or normal to the flow, or perhaps at an in-between angle? Also, if the cylinder is free to move, would its vibration affect the angle?

Authors' Closure

We are grateful to Professors Dalton, Roshko, and Trefethen for their interest in our paper. The paper was originally undertaken in an attempt to clarify the various cosine law statements that are frequently made about vortex shedding. We view the chief contribution of the paper as lying in this clarification, and in our description of wake behavior. Professor Trefethen's short questions relating to features of wake behavior will be deferred for a moment, however.

Both Professors Dalton and Roshko have challenged our introductory statements in verification of the idea that the cross flow will determine the separation line. We investigated two pages in this exercise because the independence principle says only that the *boundary layers* can be calculated independently. It gives no á priori information relating to separation. Had the spanwise flow separated first, the point would not have been that the independence principle had failed, but rather that the boundary layer equations indicate separation at some point that varies with yaw angle. The literature does not provide any real

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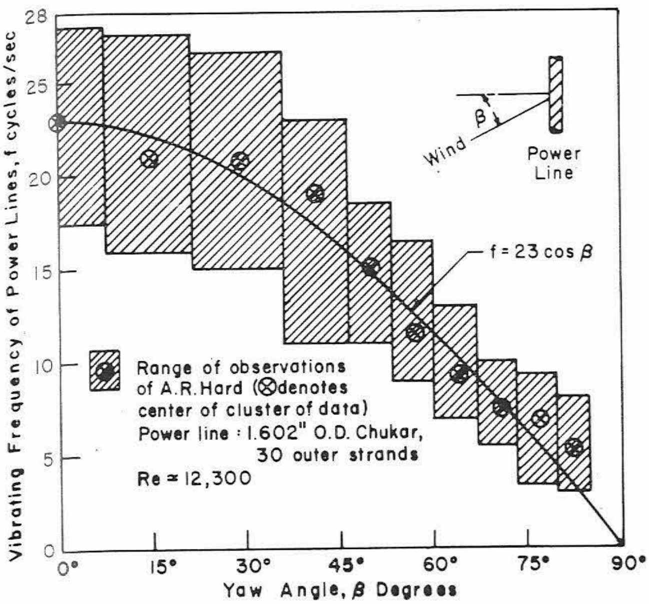

Fig. 1 Field data illustrating approximate conformity of vortex shedding from transmission linear to a cosine dependence upon yaw angle

answer to the question as to whether a more general separation principle exists. Thus, we do not feel that the independence of the complete crosswise and spanwise flows was sufficiently well established to have justified us in adopting it.

Professors Roshko and Dalton note that the true location of the separation will in fact be established by the pressure in the near wake. Indeed, Professor Roshko is responsible for very important explanations of this behavior.[5,6] The imprecision of the Blasius series and the sinusoidal free stream velocity distribution at large θ was a fact that we acknowledged at the end of our *Introduction* section. However, we used these approximations in hopes of gaining at least a qualitative indication as to whether or not the crosswise component really separated before the spanwise component did. A comparison of our Figs. 3 and 5 in the original paper shows that separation in the crosswise mode develops far more rapidly than in the spanwise mode. In the absence of a complete independence principle—one that applies to more than the boundary layer equations—this is helpful evidence in its favor.

Thus, the real question is that of saying whether or not our qualitative verification has enough accuracy to be convincing. We feel that it has, because the failure of spanwise flow to separate is so pronounced. We certainly did not assume the thing that we set out to show, as Professor Roshko submits, nor did we (by the way) "prove the cosine law." We actually showed that the conventional cosine law is an overstatement of our equation (15), and that it applies in some instances.

Professor Trefethen's conundrum about the orientation of the vortex lines was as unexpected as it was intriguing. Vortex lines inclined at an angle other than the yaw angle, β, would indicate that there is a spanwise shift in *phase* of the vortex shedding process.

Plenty of available data testify to the three-dimensionality of the wakes of *unyawed* cylinders. Phillips[7] found spanwise correlation of wakes up to 30 diameters in the range $40 \leq Re \leq 80$. In the range $80 \leq Re \leq 100$—about the range of the Tritton[8] transition in the wake instability—he found that wakes would go out of phase over this spacing if the flow were disturbed. In the range $100 \leq Re \leq 160$ he found good spanwise correlation only for spacings less than 20 diameters. Subsequently Bloor's[9] hot-wire observations of cylinder wakes for $200 \leq Re \leq 10^4$ showed even stronger spanwise irregularities in the turbulent-wake regime. Both she and Phillips made the important observation that this is to be expected since turbulence is inherently 3-dimensional. The experiments of Macovsky[10] and Humphreys[11] also show strong spanwise irregularities between $Re = 10^4$ and transition. Humphreys found that near transition these irregularities took the form of short spanwise cells (1.4 to 1.7 diameters in length) of oscillating phase. Very recently, Gerrard[12] presented an exceedingly helpful experimental study of wake three-dimensionality. He observed *straight vortex lines, inclined at about 14° to an unyawed cylinder* at $Re = 83$ (just below the Tritton transition). He was unable to explain this phase shift, and he noted that the angle shifted randomly from positive to negative. At higher Re's he gave good documentation of the evolving cellular structure observed by earlier workers.

Presumably this kind of messy three-dimensionality will also exist behind yawed cylinders, and any question as to the inclination of vortex lines will have meaning only at low Reynolds numbers. The arguments that we have developed in our study would indicate that the crosswise flow component is independent of yaw angle. Therefore, at low Reynolds numbers we would expect to find the same kind of random inclination of vortex lines toward the cylinder as Gerrard found. At higher Reynolds numbers we would expect the same spanwise irregularities in the crosswise component that would normally occur in an unyawed flow. Vortex lines normal to the flow, or inclined at an angle β to the cylinder, would constitute a special case that we would not ordinarily expect under any conditions.

The effect of vibration of an unyawed cylinder was discussed by Lienhard and Liu,[13] among others. They measured the fairly wide range in which the vortex frequency locked onto the cylinder frequency. They also showed that if the vortex frequency didn't lock onto the fundamental cylinder frequency, it would probably lock onto some multiple or submultiple of it, instead. Thus, the wake of the vibrating cylinder is likely to be "organized," with respect to phase variation, by vibration. We would be even less inclined in this case to look for vortex lines that are inclined toward the cylinder.

Fig. 1 summarizes data that bear on this question. These are frequency response observations by Hard[14] for the *in situ* vibration of a stranded power transmission line. The scatter in the frequency of the cylinder probably occurs because the cylinder does not always move at the vortex frequency. Nevertheless these data cluster almost precisely upon the cosine law. Accordingly, vibration does not alter the vortex shedding seriously enough to change the effect of yaw angle.

Detailed experiments are needed to provide reliable answers

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[5] Roshko, A., "A New Hodograph for Free-Streamline Theory," NACA TN 3168, (1954).

[6] Roshko, A., "On the Wake and Drag of Bluff Bodies," *Journal of Aeronautical Science*, Vol. 22, No. 2, 1955, p. 801.

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[10] Macovsky, M. S., "Vortex Induced Vibration Studies," David Taylor Model Basin Report 1190, 1958.

[11] J. S. Humphreys, "On a Circular Cylinder in a Steady Wind at Transition Reynolds Numbers," *Journal of Fluid Mechanics*, Vol. 9, Part 4, 1960, p. 603.

[12] Gerrard, J. H., "The Structure of the Wake of a Circular Cylinder," *Journal of Fluid Mechanics*, Vol. 25, Part 1, 1966, p. 143.

[13] Lienhard, J. H., and Liu, L. W., "Locked-in Vortex Shedding Behind Oscillating Circular Cylinders, With Application to Transmission Lines," ASME Paper No. 67—FE-24.

[14] Hard, A. R., "Observation of Aeolian Dynamic Strain on McNary Ross Powerline Near Paterson, Washington," Jan., 1957 Washington State University, Div. of Ind. Res. Dept. to Bonneville Power Administration.

to either of Professor Trefethen's questions. Although we are fairly confident of what they would reveal, such experiments could well produce surprises and probably should be made.

On the Shock Wave Velocity and Impact Pressure in High-Speed Liquid-Solid Impact[1]

B. C. SYAMALA RAO[2] and N. S. LAKSHMANA RAO[2]

We wish to congratulate the author for his useful contribution on a subject of interest to several investigators. The paper very elegantly shows that for high-speed liquid-solid impact, the propagation velocity (C) of the shock wave is different from the acoustic velocity (C_0) in the undisturbed liquid.

While estimating the impact pressure on an elastic target, the particle velocity change in the liquid (V_1) was related to the impact velocity (V_0) and the particle velocity (V_2) in the target by the author through the equation

$$V_1 = (V_0 - V_2) \tag{18}$$

This equation is true in the case of high-speed solid-solid impact only as shown by Engel.[3] In the case of liquid-solid impact, there will be a radial outflow of the liquid at the impact interface. Hence, equation (18) becomes

$$V_1 = \alpha(V_0 - V_2) \tag{19}$$

where α is a nondimensional coefficient less than unity. Engel[3] also gives an equation to estimate as

$$\alpha = \frac{0.41}{1 + (0.59Z_0/Z_2)} \tag{20}$$

Using equation (19), the impact pressure expressed by the author in equation (7) becomes

$$P = \frac{\alpha\rho_0 C V_0}{1 + (\alpha\rho_0 C/\rho_2 C_2)} \tag{21}$$

Since the values of α as obtained from equation (20) are always less than 0.41, the impact pressures estimated taking $\alpha = 1.0$ will be considerably large. Would the author kindly explain why he chose a constant value of $\alpha = 1.0$ in his analysis?

If α is included in the analysis, the several equations derived by the author get modified as discussed below:

The main quadratic equation (9), used for deriving the value of (V_1/V_0) becomes

$$\frac{Z_0 k M_0}{Z_2}\left(\frac{V_1}{V_0}\right)^2 + \left(\frac{Z_2 + \alpha Z_0}{\alpha Z_2}\right)\left(\frac{V_1}{V_0}\right) - 1 = 0 \tag{22}$$

The first approximate solution for (V_1/V_0) obtained from equation (22) is

$$\frac{V_1}{V_0} \cong \frac{\alpha}{1 + (\alpha Z_0/Z_2)} \tag{23}$$

which is valid when

$$\frac{\alpha k M_0}{2 + (\alpha Z_0/Z_2) + (Z_2/\alpha Z_0)} \ll 1 \tag{24}$$

The second approximate solution of equation (22) applicable for higher values of M_0, is

$$\frac{V_1}{V_0} \cong \frac{(Z_2/\alpha Z_0) + \alpha k M_0}{1 + (Z_2/\alpha Z_0) + 2\alpha k M_0} \tag{25}$$

which is valid when

$$\frac{\alpha k M_0(1 + \alpha k M_0)}{[1 + (Z_2/\alpha Z_0) + 2\alpha k M_0]^2} \ll 1 \tag{26}$$

Author's Closure

The author wishes to thank the discussers for their interest. He disagrees, however, with discussers' contention that a coefficient α should be incorporated in the expressions for liquid/solid impact. The reason for this is two-fold:

Firstly, the analysis presented in the paper is explicitly restricted to the one-dimensional case, i.e., of two infinite plane surfaces coming into contact. This was clearly stated in both the Introduction and the Summary of the paper, because the author well realizes that two or three-dimensional impact, between nonplane surfaces, is a much more complicated affair. Lateral flow or particle motion is therefore ruled out here, and Engel's equations (19) and (20), which relate to the impact of a liquid sphere onto a solid surface, do not apply to the present case.

Secondly, the author doubts that Engel's analyses (see footnote in discussion) [12][4] are applicable, even to the impact of a liquid drop onto a solid surface, during that brief but crucial initial stage when compressibility phenomena govern the liquid response to the impact and maximum impact pressures are developed.

No rigorous analysis of this type of impact, taking compressibility into account, has yet come to the author's attention. The author, however, favors an argument expounded in detail by Bowden and Field [13]. According to this, an essential feature of the impact process, between a curved liquid surface and a plane solid surface, is an initial stage during which the response of the liquid is entirely compressible, and *no* lateral out-flow (such as would introduce the α of equation (19)) can occur. That is so because the perimeter of the impact interface moves tangentially outward at a speed which initially *exceeds* the velocity of the shock waves generated by the impact, see Fig. 3. The resulting shock front is, therefore, attached to the solid surface: and the compressed liquid, being bounded entirely by the solid surface on one side and by the shock front separating it from undisturbed liquid on the other, cannot flow. It is only when shock waves can overtake the interface perimeter, and reach a "free" surface, i.e., when the shock front becomse detached, that lateral flow is able to begin.

Bowden and Field [13] also concluded that, during this initial "compressible" stage, the impact pressure is uniform and equal to the "one-dimensional" pressure as given by equation (3). The latter conclusion appears to be somewhat oversimplified, and a more accurate picture can perhaps be deduced from Skalak and Feit's [3] results, which apply to the closely related case of a rounded or wedge-shaped solid body impacting onto a plane liquid surface. These results confirm that there is an initial stage without flow, and that the *average* impact pressure during this stage equals the one-dimensional pressure $\rho_0 CV$; but suggest that the pressure

[1] By F. J. Heymann, published in the September, 1968, issue of the JOURNAL OF BASIC ENGINEERING, TRANS. ASME, Series D, Vol. 90, pp. 400–402.

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[3] Engel, O. G., "Note on Particle Velocity in Collisions Between Liquid Drops and Solids," *Journal of Research*, National Bureau of Standards, Vol. 62A, 1960, pp. 497–498.

[4] Numbers [12–17] in brackets designate References at end of closure.

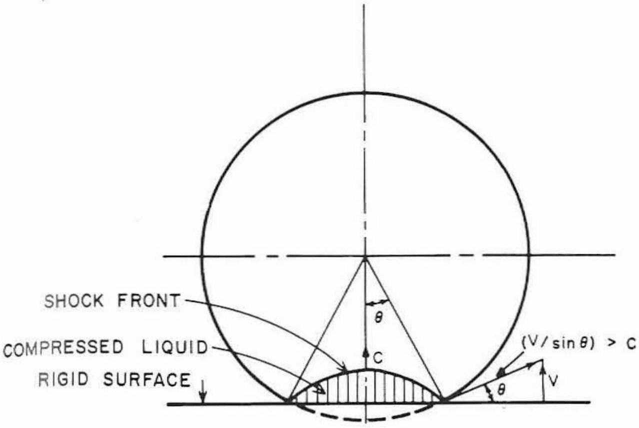

Fig. 1 Initial (compressible) stage of impact between liquid sphere and rigid surface, showing attached shock front

distribution becomes nonuniform, with the pressure at the perimeter *increasing*, and that under the center *decreasing*, as the interface angle θ grows. The significant point here is that the maximum stress experienced by the solid surface will, therefore, be *higher* than that predicted by the one-dimensional model, rather than lower as discussers have suggested. This may be one reason why liquid impingement damage and erosion has often been found to occur at unexpectedly low impact speeds.

Once lateral flow does begin, the picture changes drastically, of course, and the impact pressures do decrease. It is noteworthy that the lateral outflow velocities can be several times greater than the impact velocity, as has been observed by numerous workers [1, 2, 12]. While a rigorous treatment of this is lacking once again, Brunton [1, 14] has pointed out that the process involved is similar to that occurring in shaped charge detonations and also in explosive welding or cladding, where a high-speed jet is formed by a kind of fluid wedging action.

The latter has been analytically treated by Walsh, et al. [15], and by Harlow and Pracht [16]. They confirmed that below a critical "collapse angle" no jetting occurs. This is consistent with the previously stated assumption of no lateral outflow during the early stage of droplet impact.

It is to be hoped that in the near future someone will treat the liquid drop impact process in a rigorous manner, perhaps by means of a time-incremented numerical approach similar to the "Particle-in-Cell" method used by Harlow and Pracht [16]. The results should be most interesting from an academic viewpoint as well as useful in the context of liquid impact damage.

In a final comment (not related to the question raised by discussers), the author would like to refer to a recent contribution by Ruoff [17], which develops a more rigorous justification for a linear shock wave velocity relationship such as here proposed in equation (5). According to Ruoff, the shock velocity can be expressed as a Maclaurin expansion in the form

$$u_s = c + su_p + s'u_p^2 + \ldots \qquad (27)$$

where u_s is the shock velocity, u_p the particle velocity, and c, s, and s' are quantities which can be calculated from densities and ultrasonically measured properties of the material. Ruoff shows that for several materials s' is very nearly zero, because of the cancellation of terms which contribute to it. Equation (27) then reduces to the form of equation (5). The experimental data shown in Fig. 1 suggest, however, that s' for water is negative and not negligibly small.

The author is indebted to Dr. J. E. Field for bringing references [15] and [16] to his attention.

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14 Brunton, J. H., Personal Communication.

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