1 A heavier vortex core moving downward.
2 A heavier vortex core moving upward.
3 A lighter vortex core moving downward.
4 A lighter vortex core moving upward.

In Cases 1 and 4, the vortex ring receives acceleration due to buoyant and gravitational forces. However, the vortex ring in Case 1, by having denser fluid in the core than the ambient fluid, is more unstable with respect to centrifugal instability mechanism than that of Case 4 which has lighter fluid in the core than the ambient fluid. Similarly, although in Cases 2 and 3 the vortex ring receives deceleration due to buoyant force, the vortex in Case 2, again by having denser fluid inside the vortex, is much more unstable with respect to centrifugal instability mechanism than that of Case 3. Therefore, the present investigation which is Case 1 should differ from Turner's results which is Case 4 not to mention the difference in different density ratio used. In our Fig. 4, the distance of laminar flow is the distance that a vortex with denser fluid inside can maintain the flow pattern at stage 2 as shown in Fig. 2 or the shape like photo 1. Therefore, a vortex ring, although unstable, may remain essentially a ring shape and travel a considerably longer distance than the laminar distance. It should be noted that in Turner's Fig. 12 the final height, which is the total distance, is not the distance of laminar flow as defined in our paper. Disregarding the effect of centrifugal instability, Turner's results show that in the case of a density difference of 20 percent (compared with our 50 percent) the final height may reach a distance only about 20 times that of the diameter. In addition, Turner experimented with a fixed input condition and varied the buoyance, while in our experiment we fixed the buoyance and varied the input condition. We do agree with Professor Chen that the breakdown at stage 2 of our Fig. 2 may also occur for neutral density vortex range. The centrifugal instability can be shown, for example, from Rayleigh theorem, to manifest outside the vortex core in the region where the azimuthal velocity, relative to the core center, decreases with the radius. This instability is known to occur in neutrally buoyant fluid such as investigated by Krutzsch in reference [3] of our paper. However, this instability should be stronger for the case of heavier vortex core than that of a neutral one and should be weaker for the case of buoyant vortex ring.

Concerning the initial exit condition, we agree with Professor Mattingly in that the ring stability should depend on the initial condition under which the ring is generated. We slowly fed the smoke, which is cooled to room temperature through a coil, into the vortex chamber radially while the orifice remains open. When the smoke is about to fill the whole chamber we observed that the smoke drifted out along the edge of orifice in a manner like a teapot effect. If the feeding is too fast a strong drift downward along the center of orifice is observed. However when the valve controlling the smoke flow is shut off there is a duration of about 5 sec in which the smoke is almost stopped from drifting downward even though the orifice is not closed. In this 5-sec duration the experiment began by turning off the d-c current to the magnet that controls the piston. We found that this process is quite satisfactory as it creates the least disturbance with our experiment. We know that when a vortex is generated in the observation chamber there is another vortex drawn into the smoke chamber. Therefore, several seconds will elapse before the smoke will finally drift out of the chamber again. However, the vortex in the observation chamber is too far away to be disturbed by the drift. Our data show that the range of impulse time varies from 0.04 to 0.4 sec. To obtain the actual time from our Fig. 1 we have $t(\text{sec}) = D^2 (\nu N_{st} N_{re})^{-1} = 170(N_{st} N_{re})^{-1}$.

Thus to get the longest impulse time is to choose the smallest Reynolds and Strouhal numbers. For $N_{re} = 1200$ and $N_{st} = 0.28$, we get $t = 0.5$ sec. We certainly agree that a 2.6-sec impulse time for a 10-in-dia piston impulsing a 2-in-dia orifice will produce a jet flow.

We would like to thank Dr. Viets for offering further observations on the formation of subring and the "upside down mushrooms" structures. This shows further that the subring formation is dominated by the gravitational force.

In conclusion, we feel that we covered the flow pattern for all ranges of Reynolds and Strouhal numbers that are capable of generating a circular vortex ring under the present arrangement of the experiment. Of course this does not cover the case of immiscible combination of fluids or the case of nonstationary ambient fluid.

Governing Equations for Vibrating Constrained-Layer Damping Sandwich Plates and Beams[1]

D. J. MEAD.[2] The authors are to be commended for the attempt to put a new slant on the theory of sandwich plates and to produce a simplified governing equation of motion. If equation (37) is valid, it will lead to considerable simplification in computing the plate dynamic properties.

I must confess to some uneasiness, however, about the simplification that has been achieved. In the first place, it leads to only *two* boundary conditions which need to be satisfied at each end of the beam (as shown in equations (42) (48)). That this is inadequate for a clamped boundary of a sandwich beam is easily shown. Such a boundary must prevent transverse displacement $\hat{W}$, and rotation $d\hat{W}/dx$ (as expressed by equations (43) and (44)) if there is no shear deformation in the face plates. In addition, *a condition must be imposed on the in-plane motion of the face-plates.* A fully clamped boundary must also have $U = 0$, but it is conceivable that the boundary might allow U displacements, at the same time as it maintains $\hat{W} = 0$ and $d\hat{W}/dx = 0$. In this case, say, we might have to impose a zero value on the mid-plane face-plate stresses, σ_x. Thus *three* conditions in all are required at the clamped end.

The governing differential equation, being biharmonic, only requires two boundary conditions for each end. It seems, then, that in deriving this simplified equation some important constraint has in effect been imposed, and this excludes the possibility of satisfying all three of the important boundary conditions.

In the second place, the results shown in Table 1 have me worried!

1 Because the results from the Mead/Markus equation should be the same as those from DiTaranto's (the two equations stem from identical assumptions, and were derived by similar analyses).

2 Because the Mead/Markus/DiTaranto results should be virtually exact for the wave numbers considered, which imply bending wavelengths of about 60 times the thickness of the thickest face plate. This means that face-plate shear deformation and rotatory inertia are negligible—which Mead/Markus and DiTaranto assumed.

This latter fact prompts the question "Why does the new theory of Yan and Dowell yield different results from Mead/Markus and DiTaranto?" I suggest that it is due to Yan and Dowell assuming zero transverse *stress* in the layers of the sandwich, whereas Mead/Markus and DiTaranto assumed zero transverse *strain* but finite (nonzero) transverse stress. The

[1] By M.-J. Yan and E. H. Dowell, published in December, 1972, issue of the Journal of Applied Mechanics, Vol. 39, No. 4, Trans. ASME, Vol. 94, Series E, pp. 1041–1047.

[2] University of Southampton, Southampton, England.

transverse stress of Mead/Markus and DiTaranto was correctly in equilibrium with the transverse inertia forces of the plate layers and with their rates of change of transverse shear force. Through this, the inertia forces of one layer were reacted by shear forces in all three layers of the plate. Yan and Dowell's assumption seems to lead to the conclusion that the transverse inertia forces in any layer are reacted by the shear forces in that layer, and in that layer alone. It is this assumption I suggest, which leads to their simplified differential equation, their two (and not three) boundary conditions at an end, and to their different numerical results.

I shall appreciate the authors' comments, and look forward to being corrected if I am wrong.

Authors' Closure

The governing equations were originally derived for the study of constrained-layer damping mechanisms in sandwich plates and beams. The authors were very surprised to arrive at such a simple equation form, which helps greatly in studying the complicated phenomena of constrained-layer damped systems. It seems to the authors that not only the final equations are interesting, but also equations (28)-(32). These equations will reduce to the set of Timoshenko beam equations when only one-layer beam is considered. It is possible that many more interesting and surprising results for multiple-layered plates can be derived following the same method.

Mere knowledge of the governing differential equation can lead to information on the boundary conditions through a variational statement. If the system of equations (33)-(35) or equations (11)-(23) can be reduced to the biharmonic equation (37), then the boundary conditions which are compatible are only four (rather than six). Additional boundary conditions cannot be entirely independent, otherwise they will lead to either trivial solutions or no solution. If the vibration of the sandwich is of main interest, this biharmonic equation with its boundary conditions will yield enough information on the "global" behavior of the system. If "local" behavior in the sandwich is desired, the solution of $w(x)$ will lead to solutions to other functions such as U, S,... or $u_i^{(0)}$, $u_i^{(1)}$,.... This group of local solutions should not affect the global picture of vibrations. The assumptions and constraints which lead to this biharmonic equation have been clearly stated in the Introduction.

Table 1 clearly shows that the solution of Mead/Markus is *very* close to Yan/Dowell.

The authors are also interested to know "Why does DiTaranto's equation yield results different from Mead/Markus?" We do not have the answer to this question either. Also the authors could not see how "the conclusion that the transverse inertia forces in any layer are reacted by the shear forces in that layer, and in that layer alone" can be obtained from equations (11), (28), or (33). All physical variables are indirectly if not directly coupled. Finally, we note that the present theory has given results in reasonable agreement with experiment.[3]

The discusser's interest and kind remarks are greatly appreciated.

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[3] Yan, M.-J., and Dowell, E. H., "High-Damping Measurements and a Preliminary Evaluation of an Equation for Constrained-Layer Damping," *AIAA Journal*, Vol. 11, No. 3, Mar. 1973, pp. 388-390.