Reply by Author to B. T. Fang (Regarding his Comment on "A New Method of Solution of the Eigenvalue Problem for Gyroscopic Systems")

Leonard Meirovitch*
Virginia Polytechnic Institute and State University, Blacksburg, Va.

THE matrix product AB of Fang can be identified as the matrix $I^{-1/2}$ of Meirovitch. Then, the matrix P of Fang becomes

$$P = -B^T A^T G A B = -I^{-1/2} G I^{-1/2}$$

because I is symmetric. The eigenvalue problem of Fang, Eq. (3) of the Comment, uses the matrix

$$P^2 = (-I^{-1/2} G I^{-1/2})^2 = I^{-1/2} G I^{-1} G I^{-1/2}$$

and its eigenvalues are λ^2. On the other hand, the eigenvalue problem of Meirovitch, Eqs. (20) and (22) of the Paper, uses the matrix

$$K' = I^{-1/2} K I^{-1/2} = I^{-1/2} G^T I^{-1} G I^{-1/2}$$

and its eigenvalues are ω^2. In Fang's notation, it is clear that

$$K' = P^T P = -P^2$$

because G is skew symmetric. Hence, no material difference exists, except that K' is positive definite if I is positive definite, which in nicer than saying that P^2 is negative definite if I is positive definite. This latter statement, which was not included by Fang in his Comment, is necessary, because only then the added statements concerning the eigenvectors hold true.

One of the powerful aspects of the Paper is the derivation of the real symmetric eigenvalue problem, Eqs. (14) and (17), which permits the solution of the eigenvalue problem for gryoscopic systems by a large variety of existing computer programs. The transformation from Eqs. (14) and (17) to Eqs. (20) and (22) is standard (see, for example, Eq. (4.134) of Ref. 10). Since the book appeared in 1967 and this transformation was known before that date, one may safely assume that there was a computer subroutine available for it. The identification of such a subroutine can be of interest to some readers.

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*Professor, Department of Engineering Science and Mechanics. Associate Fellow AIAA.

Comment on "Unsteady Separation Phenomena in a Two-Dimensional Cavity"

Demetri P. Telionis*
Virginia Polytechnic Institute & State University, Blacksburg, Va.

A LOT has been published recently on unsteady separation as desribed in a few very recent review articles.[1-3] Yet it appears that many investigators have misunderstood some of the basic ideas that have been recently advocated by a school of thought to which I belong. O'Brien's paper[4] on "Unsteady Separation Phenomena in a Two-Dimensional Cavity" is a typical example.

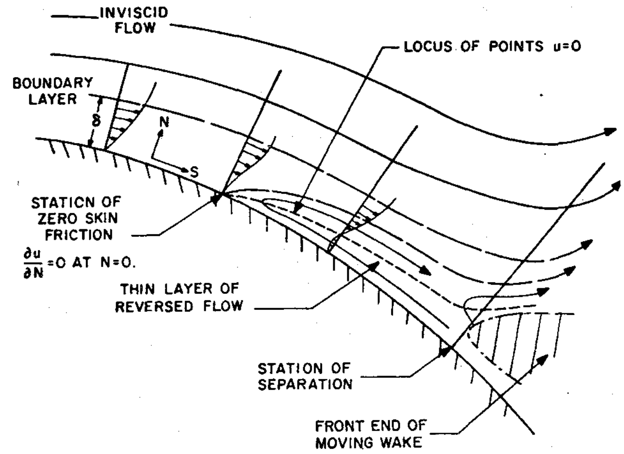

Fig. 1 Schematic sketch of the streamline configuration in the neighborhood of unsteady separation. The dash-dot line denotes the boundary of the wake and does not coincide with any streamline.

I believe that Ref. 4 represents a fine piece of work, and there is nothing wrong either with respect to the mathematical model or with the physical conclusions the author draws for this particular problem. I only disagree with the way she compares qualitatively her results with our work. The physical problem that we have been working on is totally different. We are concerned with external flows of very large Reynolds numbers Re and the well known phenomenon that solutions of the Navier-Stokes equations for $1/\mathrm{Re} \rightarrow \mathrm{O}$ and $1/\mathrm{Re} = \mathrm{O}$ are totally different. We define separation as the point where the flow leaves the solid boundaries and turns into the freestream thus generating a wake. A careful study of the steady boundary layer, which in the limit of $\mathrm{Re} \rightarrow \infty$ has infinitely small thickness,revealed that at separation the wall shear vanishes[1]. This is not true though for unsteady flow[1] as depicted schematically in Fig. 1 which I borrowed from Ref. 1. The study of unsteady separation is of paramount importance in aerodynamics since it governs phenomena like unsteady airfoil stall, rotating stall of axial flow compressors, flutter, and others.

It may appear that our differences with O'Brien are differences of terminology. She is certainly free to use the term separation for points where streamlines emanate from the wall. I would rather call these points, points of detachment, and I feel that physically they are no different than a point of a rear stagnation. O'Brien though has qualitatively compared in her conclusions her results with our work. The comparison is not very successful. Her phenomena are totally viscous and in her case I believe that the solution for $1/\mathrm{Re} \rightarrow \mathrm{O}$ and $1/\mathrm{Re} = \mathrm{O}$ are the same. The catalytic effect of viscosity $\mu \rightarrow \mathrm{O}$ is absent. Points of dettachment in unsteady flow of the kind O'Brien has studied are points like the one marked by $\partial u / \partial N = \mathrm{O}$ in Fig. 1 and do not seem to have any major engineering significance.

O'Brien criticizes the use of the boundary-layer equations, and yet I do not know of any other realistic method of calculating external flows for practical engineering problems, especially if the boundary layer is turbulent. Her full momentum equations do not show any singular behavior at "her" point of "separation", a fact which is well known for years[1]. O'Brien then proceeds to claim that her method permits a clearer view of the physical process. I agree that her results are very interesting, but the phenomenon she is trying to illuminate bears no resemblance to unsteady separation.

A minor point perhaps could be added here. With regard to the Moore Rott Sears model of "midstream separation" a lot of analytical and experimental work has been published[1]. A

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*Associate Professor, Department of Engineering Science and Mechanics. Associate Member AIAA.