Reply by the Authors to J. G. Leishman

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Introduction

MANY benefits have been obtained from Professor Leishman's comments on our work.[1] However, we feel that the comments represent some misunderstanding of the paper. The first purpose of our work was to propose a leading-edge suction model for helicopters in forward flight. For a fixed wing of infinite aspect ratio the leading edge pressure is represented by

$$\Delta p = 2\rho_\infty V_\infty u \tag{1}$$

This model can be extended to finite span wings, i.e., three dimensions. For a helicopter, the relationship between u and Δp cannot be derived directly from Eq. (9) in Ref. 1. One then must question if the leading-edge suction model is valid for helicopters as an extension of two-dimensional theory to three dimensions. Eqs. (10-20) in Ref. 1 show that this extension is possible. The second purpose was to show the difference between the induced drag per unit length predicted by the two models, i.e., the induced angle model and the leading-edge suction model. As mentioned in our paper, this difference must exist for helicopter rotor blades. It is emphasized that either lifting-line theory or lifting-surface theory could be used in both models. Our purpose was to compare the two theories and not to calculate the induced power exactly. The latter needs further correlation.

In Ref. 1 we stated that "lifting-line theory is not adequate in describing helicopters in forward flight." Leishman considers this to be rather "misleading and largely untrue." His reason is that the lifting-line theory, even the classical momentum theory, will do a reasonable job in predicting the induced power. But the sense of the words "describing helicopters" can include several meanings, such as lift distribution, drag distribution, blade vibration, noise, ambient flowfield, etc. The rotor performance is only one item; it is not the unique item of these areas. There are many codes that use the lifting-line theory to predict the lifting distribution, including the very complicated free wake analysis. But we cannot be satisfied with their results, especially at higher harmonic loading (see Ref. 2, for example). In fact, it is Professor Leishman himself who pointed out many of the deficiencies of lifting-line theory in his comments. We consider that if Professor Leishman agreed on the aforemention sense of the phrase "not adequate in describing helicopters," his comment about this problem would not be presented.

It appears that Professor Leishman does not believe Table 1 in Ref. 1, because of the lower values of f. His reason is that lifting-line theory can predict induced drag reasonably. However, this is a misunderstanding. Both factors, yW/U_1 and $(y\alpha\text{-}S)$, come from the same lifting-line theory. The only modification is the two-dimensional Theodorsen function. Since in Ref. 3, which was used for Table 1 in Ref. 1, it was assumed that the number of blades is infinite, then the theory used is a steady theory. There are three factors for the case when $f \neq 1.0$. The first is the inherent difference between the two models, which we wanted to show by using Table 1 in our paper. For example, if the unsteady modifications make the slope of the lift curve decrease by 3%, or the angle of attack increase by 3%, then f will decrease by 6% (with $S/y\alpha = 0.5$). The second is the lower than usual slope of the lift curve, 5.73, used in Ref. 3 as usual. The third perhaps is the problem in the theory used to calculate the lift distribution.

Our purpose for presenting Table 1 in Ref. 1 was to show the first factor, but indeed f showed the combined effect of the three factors already mentioned. When we use the leading-edge suction model to predict induced power, practically, we must choose the theoretical value 2π in using lifting-line theory or lifting-surface theory for the lift distribution and angle of attack, or an alternative method. In this sense, we thank Professor Leishman for his comments. It is emphasized that since the values of f are far from 1.0, then perhaps this means there may be some problems in the lifting-line theory used. It certainly does not mean that the induced power is too low. However, we do not consider that the two-dimensional Theodorsen function and Table 1 in Ref. 1 were used and presented arbitrarily.

We now present Table 1a which shows the difference between the two models. It is the rotor of a CH-34, at $\mu = 0.0873$, $\psi = 90$ deg (y, W, and α are calculated). The calculation is a free wake analysis, using lifting-surface theory, completely unsteady, and similar to that used in Ref. 4. Although the lift distribution does not show good agreement with experiment, the calculated result still can be used. Since the values for f are not far from 1.0, they may reveal the difference between the two models because the slope of the lift curve is lower than 2π. In fact, the comparison between the two models (i.e., the values of f) is so simple that those who doubt the results can do the calculation themselves with whatever data is at hand.

We acknowledge Leishman's point that in describing helicopters the leading-edge sweep angle should be included in the leading-edge suction. When the blade is at azimuths not at 90 or 270 deg, the sweep angle exists due to the $\mu\cos\psi$ term. Usually, in existing lifting-line or lifting-surface theory applications for predicting the lift distribution, or in the lifting-line/blade element analysis, the term $\mu\cos\psi$ is omitted. We do not have an available method to include $\mu\cos\psi$. It is unreasonable to use the lift in which the $\mu\cos\psi$ is not included, or the leading-edge suction in which the $\mu\cos\psi$ is included for Table 1 in Ref. 1. Our purposes were to demonstrate that the leading-edge suction model could be used for helicopters and to show the difference between the two models; a complicated planform (e.g., tip sweep) was beyond the scope of Ref. 1. Coincidentally, in Ref. 5, for incompressible unsteady flow, we obtained theoretically

$$S = \pi\rho_\infty\left(\frac{A(y)\sqrt{2C}}{2\rho_\infty V_\infty}\right)^2 \cdot \left(\frac{e^{2i\omega t}}{\cos\theta}\right) \tag{2}$$

[see Eq. (18) of Ref. 5, where θ is the sweep angle], which is the same as

$$Cs = \pi/8\sqrt{\beta^2 + \tan^2\Lambda}\ \lim_{\zeta \to 0}\ [\zeta\Delta Cp^2(\zeta)] \tag{3}$$

and was pointed out by Leishman when $\beta = 1$ [see Leishman's Eq. (1)]. But we are not sure that this equation applies for $\Lambda \neq 0.0$ and $\beta \neq 1.0$. Perhaps more evidence is needed for Leishman's Eq. (1).

Table 1a Value of f for varying normalized radial (r/R) positions

r/R	0.25	0.4	0.55	0.75	0.85	0.9	0.95
f	0.989	0.938	0.908	0.958	0.959	1.368	1.071

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Professor Leishman's assertion that "in the helicopter rotor blade the induced pressure distribution on the wing is no longer given in the form of Eq. (2) of Ref. 1" is very true. But this assertion doesn't alter our use of Eq. (2) in Ref. 1. As mentioned, the leading edge suction model can use lifting-line theory in which Eqs. (1-6) in Ref. 1 are accurate. In this case, any special numerical extrapolation need not be employed.

We thank Professor Leishman for reminding us that "the induced drag is certainly affected by unsteady effects," but his assertion that our statements were to the contrary is not true. We have not concluded that the induced drag is not affected by unsteady effects. The purpose of developing the leading-edge suction model is to include unsteady effects completely. We would like to know how to use the equivalent apparent mass contributions to the unsteady induced drag. Professor Leishman has also reminded us of the viscosity effects and his formula

$$Cd = 2\pi/\beta\ (1\text{-}\epsilon)\ \alpha^2$$

But again, this was beyond the scope of Ref. 1.

References

[1]Li, C. E., Poling, D. R., and Wu, D., "Induced Drag Based on Leading Edge Suction for a Helicopter in Forward Flight," *AIAA Journal*, Vol. 28, No. 2, 1990, pp. 201-204.

[2]Egolf, T. A., and Landgrebe, A. J., "Helicopter Rotor Wake Geometry and Its Influence on Forward Flight," NASA CR 3726, Dec. 1983.

[3]Wang, S., and Xu, Z., "A Simplified Method for Predicting Rotor Blade Airloads," *Acta Aeronautica et Aeronautic Sinica*, Vol. 3, No. 2, 1983, pp. 1-17.

[4]Sadler, S. G., "Development and Application of a Method for Predicting Rotor Free Wake Positions and Resulting Rotor Blade Air Loads," NASA CR 1911, Dec. 1971.

[5]Li, C. E., "On The Unsteady Leading Edge Suction of a Swept-back Wing," *Acta Aerodynamica Sinica,* Vol. 7, No. 1, 1989, pp. 1-11.

Comment on "Calculation of Asymmetric Vortex Separation on Cones and Tangent Ogives Based on a Discrete Vortex Model"

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CHIN et al.[1] used the two-vortex model and suggested, as done earlier by Dyer et al.,[2] that the multiple solutions provide an alternative explanation of the existence of asymmetric vortex separation at zero sideslip and the measured side-force can only be predicted by second-branch solutions.

Vortex characteristics of bodies of revolution have been studied extensively through the use of several methods, including the vortex-element methods, in the framework of slender body theory. Also with the added predicaments of where are the separation lines, what is the rate of shedding of vorticity, how does the separating stream surface leave the body, what conditions should be satisfied at the separation line(s), and what is the relationship between the Kutta condition and separation from a smooth surface. Chin et al.'s technical note does not deal with these questions and confines itself to the use of the modified versions of Bryson's[3] and Dyer et al.'s[2] two-vortex models, with additional ad hoc assumptions.

Each vortex is connected to the fixed and prescribed separation points with a cut (a feeding sheet) of vanishing small vorticity. The net force on the total vortex system is rendered globally zero, separately on each side. The line vortex does not lie along a streamline, or in the cross flow. Its velocity is not equal to the local fluid velocity. The moment acting on each vortex and its feeding sheet is not zero and cannot be rendered zero without introducing additional ad hoc assumptions. Furthermore, the model ignores the effect of the secondary separations.

Bryson forced the separation line and the positions of the line vortices to be symmetric about the incidence plane. A Kutta-type condition (the tangential component of the cross-flow is zero on the body) was invoked at the separation points. In spite of its remarkable simplicity, Bryson's analysis predicted the normal force at the early stages of motion fairly accurately.[4] At later times, the force drops sharply and unrealistically. However, the most remarkable feature of Bryson's model is that it owes its limited success to the forced symmetry of the vortices.

In 1969, Davis[5] recast Bryson's model to remove the forced symmetry. The use of initial values including symmetric separation points exactly identical to those of Bryson failed to produce symmetric vortex positions. It was discovered that the matrix yielding the rates of change of the strengths and positions of the vortices is ill-conditioned and the slightest truncation error leads to abnormally large values for the vortex strengths and positions. The unexpected and surprising asymmetry of the vortices was not interpreted as an explanation of the sectional side force which has since become an important problem.[6] Rather, it was discovered that the two force-balance equations for the vortex-cut are the source of the ill-conditioned behavior of the matrix yielding the strengths and positions of the two vortices. This, in turn, is due to the fact that the moment acting on a vortex and its connecting sheet is not zero. In other words, imposed symmetry can hide computational instability. Thus, the vortex asymmetry resulting from the ill-conditioned nature of the approximate equations explains neither the existence nor the non-deterministic behavior of the side forces. This is not to say that the side force is not a consequence of vortex asymmetry, but rather to emphasize that the source of asymmetry resides upstream of the separation points, not in the unstable behavior of the approximate equations, based on numerous ad hoc assumptions and empirical parameters. In fact, a careful multivortex analysis of the impulsively-started flow by Sarpkaya and Shoaff,[7] without resorting to the vortex-cut and no-force assumption, did not yield a bifurcation to asymmetric state, at least without introducing an asymmetry in the separation points and/or in the shear layers. A similar conclusion has been reached by Almosnino[8] using a non-linear vortex-lattice method. A detailed discussion of the vortex methods is given by Sarpkaya.[9] A thorough discussion of the forebody and missile side forces is given by Hall.[6]

It is concluded that the two-vortex model is a crude approximation to a very complex problem, the use of force-free feeding-sheets connecting the prescribed separation points to the line vortices does not yield a moment-free system, the resulting equations are ill-conditioned for the vortex strengths and positions, and the vortex asymmetry resulting from the ill-conditioned nature of the governing set of equations explains neither the existence nor the nondeterministic behavior of the side forces, with or without circulation reduction and a number of ad hoc assumptions to bring the calculated and measured values into closer agreement.

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