provides model proportional differential and integral feedback gains

$$K_{PD_m} = \begin{bmatrix} -3.025 & -0.466 & 0.634 & -5.475 \\ -0.426 & 3.19 & -2.102 & -0.791 \end{bmatrix} K_{I_m} = \begin{bmatrix} -3.143 & 0.347 \\ -3.347 & -3.143 \end{bmatrix}$$

Next, IFB is incorporated into the aircraft dynamics. Various values for Q and R in the PI, Eq. (9), were investigated. The values

$$Q = \text{diam}[1, 1, 1, 1.\ 1]; \quad R = \text{diam}[10, 10]$$

were selected on the basis of low proportional differential and integral feedback gain values and such that the eigenvalues of the closed-loop airplane are similar to those of the closed-loop model. The gain values are

$$\left[\begin{array}{c|c} K_{PD_p} & K_{I_p} \\ \hline K_1 & K_2 \end{array}\right] = \left[\begin{array}{cccc|cc} -4.063 & -1.638 & 2.97 & -6.249 & -3.218 & 0.7598 \\ -1.989 & 4.331 & -4.298 & -1.791 & -0.796 & -3.176 \\ \hline 1.07 & 0.309 & -0.5032 & 1.403 & 0.36 & -0.313 \\ 0.451 & -0.848 & 1.94 & 0.658 & 0.0846 & 1.207 \end{array}\right]$$

Typical time history results are shown in Fig. 2. These results illustrate the decoupling and the zero state error properties and the good transient behavior for this formulation of the problem.

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Technical Comments

Comment on "A Finite-Element Method for Calculating Aerodynamic Coefficients of a Subsonic Airplane"

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WE wish to take issue with Hua[1] who contends that "In the steady aerodynamic forces area, Woodward,[2] Belotserkovskii,[3] Hedman,[4] and Kâlmân et al.[5] have made their prominent contributions. However, most of their works are only concerning the longitudinal forces and moment. A general consideration about all six aerodynamic coefficients of an airplane has not been found in the literature." A general consideration of all six degrees of freedom *is* to be found in the literature if one recognizes that Refs. 2–5 have been concerned primarily with *distributions* of aerodynamic loads; the integration of these distributions to obtain six-degree-of-freedom aerodynamic coefficients is a trivial matter. Furthermore, Refs. 2–5 have been more fundamentally concerned with *aerodynamic influence coefficients* that are *independent* of any specific motion, so that calculation of load distributions arising from longitudinal, lateral, or directional motions is again trivial.¶

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¶The rotary cross-derivatives are, of course, exceptions to this, and so are other derivatives depending on second-order effects.[6-8]

We might also note that Hua's interest in performance is satisfied by a steady flow solution, but that his interest in flying qualities requires, in addition, an oscillatory solution.[9-11] However, that is another matter; it is our purpose here only to comment on Ref. 1, insofar as it is incorrect or inaccurate.

In the matter of wing-body interference, Refs. 2, 5, and 12 apply two different techniques to account for interference per se: Refs. 2 and 5 use lifting surface elements over the fuselage surface in the vicinity of the wing-fuselage juncture; Ref. 12 (with results also shown in Ref. 5) utilizes a wing image system, as well as an axially segmented line doublet within the fuselage, to simulate the interference. Both techniques approximate the fuselage as a cylinder. The approaches of Refs. 5 and 12 have been combined and extended in Ref. 11 to include unsteady wing-body effects. A slender body idealization of the fuselage is made to account for varying diameter, while the image system within a cylindrical interference surface is maintained to offset the effects of the lifting surface vortex (and doublet in the oscillatory case) system. The use of the image system for wing-body interference was first used by Multhopp[13] in 1941. In Ref. 1, however, we find no consideration of images, but find the assumption (presumption ?) that a source and doublet on the body axis will simulate fuselage interference and that the vortices on lifting panels give negligible contribution to the total force on a cone. Had Hua made a calculation of the spanwise loading on a midwing-fuselage combination, he would have found that his wing lift would have vanished at the side of the fuselage because of the lack of images. Also because the lifting surface induced lift is ignored, his fuselage lift will be considerably less than the lift that should carry across the fuselage. This defect is not so apparent in the example Hua has chosen, where the fuselage is tangent to the low wing at the centerline.

On the subject of drag, Hua notes that "the induced drag prediction might not be improved without modifying the method by including leading edge suction." Among the works "only concerning the longitudinal forces," we have contributed a Note[14] on the spanwise distribution of induced drag on lifting surfaces. It was based on a straightforward application of the Kutta-Joukowski Law, without any consideration of leading edge suction. Some controversy arose[7] over this application of the Kutta-Joukowski Law in general, and to estimate the lateral-directional derivative $C_{n(p)}$ in particular, but the controversy served to clarify the various applications. What the prospects are for calculating induced drag on wing-body combinations remains to be seen, and further investigations are in order. Certainly, Hua has more to do to improve his drag calculation than simply account for leading edge suction.

The analysis of sideslip is our last area of criticism. The use of dual coordinate systems in linearized theory, i.e., a body-axis system for the elements and control points, and a wind-axis system for the element trailing vortices, not only introduces needless complications in the longitudinal case but also substantial errors in the directional case.** The most complete study of the validity of the Lattice Method is James' investigation[15] of the two-dimensional steady flow case which demonstrated the proper chordwise locations of the bound vortex and the downwash collocation point on each lifting element but obviously had nothing to say about the spanwise location of the collocation point in three-dimensional applications. From numerical experimentation, it has become apparent that the optimum spanwise location of the collocation point is at the centerline of the lifting element which, in the longitudinal case, is halfway between the trailing vortices. In Hua's coordinate systems, the collocation points are not centered between the trailing vortices, and if more vortices had been used, it is likely that at least one trailing vortex would have crossed a collocation point at some sideslip angle (for zero angle of attack) and an infinite downwash influence function would have resulted. This explains the large discrepancy in underestimating the dihedral effect $C_{l(\beta)}$ shown in Hua's Fig. 7. We can also ask how many problems are created by a sideslipping wing as its trailing vortex system intersects the fuselage, but that question may also be asked of our own development.[11] However, there are still some items for further investigation in estimating $C_{l(\beta)}$ for a wing alone. The prediction of dihedral effect for geometric or flexibly induced dihedral can be made by considering sideslip simply as a source of downwash as in Ref. 16 and the Vortex Lattice Method without modification can be used to provide the aerodynamic influence coefficients required in Ref. 16. However, the planform contribution to $C_{l(\beta)}$ from a swept wing requires some modification in procedure but none in principle. This consists of aligning the elements in the lattice system and, thus, also the control points, with the direction of sideslip. For positive sideslip β and sweepback Λ, the right wing will then have less sweep, i.e., $\Lambda - \beta$, and the left wing will have more sweep, i.e., $\Lambda + \beta$, and the wing tip regions will appear triangular. Numerical evaluation of this approach to the planform contribution to $C_{l(\beta)}$ is a topic for further study but the wind axes of Ref. 1 cannot lead to accurate estimates.

To conclude, we will certainly not agree, at least at subsonic speeds, that "the existing methods for predicting aerodynamic coefficients are not very satisfactory," but we hope that this Comment will assist the interested reader in distinguishing between the real and the imagined problems that remain.

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**Figure 1 of Ref. 1 appears to have the lifting element drawn incorrectly; i.e., the side edges are not parallel to the x-axis.

Reply by Author to W. P. Rodden, J. P. Giesing, T. P. Kalman and J. C. Rowan

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THE preceding Comment by W. P. Rodden et al. shows that several points in my original paper should be further elaborated. In the early stage of preliminary design, air-

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