Readers' Forum

Brief discussions of previous investigations in the aerospace sciences and technical comments on papers published in the AIAA Journal *are presented in this special department. Entries must be restricted to a maximum of 1000 words, or the equivalent of one* Journal *page including formulas and figures. A discussion will be published as quickly as possible after receipt of the manuscript. Neither the AIAA nor its editors are responsible for the opinions expressed by the correspondents. Authors will be invited to reply promptly.*

Comment on "A New Solution Method for Lifting Surfaces in Subsonic Flow"

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THIS commentator objects to the characterization by Ueda and Dowell[1] of the principal approximation of the Doublet-Lattice Method (DLM)[2] as inconsistent while the "new solution" contains actual inconsistencies. The principal approximation was clearly stated in the original paper (Ref. 2, p. 281): "In order to converge to Hedman's vortex-lattice results for steady flow and to improve the approximation of Eq. (7), the authors have found it necessary to subtract the steady part ($\omega = 0$) from $\mathfrak{K}$ before applying the preceding formulas, and then to add the effect of a horseshoe vortex." This made the DLM consistent with the Vortex-Lattice Method (VLM) of Hedman[3] (and also of Rubbert,[4] Dulmovits,[5] and Belotserkovskii[6]). It is Ueda and Dowell who are inconsistent in the limit of zero frequency with the accepted formulation of the VLM[7] which is characterized by *swept bound vortices* on a *swept* planform. The characterization of their method is also inconsistent: their Doublet Point Method (DPM) is not a point method at all, but is a *rectangular* DLM in which the doublet line is *not* swept. The tradeoff that has been made is between the rectangular geometry that permits exact spanwise integration of the kernel and the swept geometry of the bound leg in the DLM with its necessary approximations.

A comparison of the DPM and DLM on swept planforms should have been the subject of the Numerical Results section of Ref. 1, but, instead the comparisons are primarily with the pressure loading function (kernel-function) methods of Laschka[8] and Rowe.[9] These comparisons hardly demonstrate any advantage of the DPM over the DLM! A meaningful comparison with the effects of variable sweep could have been made with the circular wing for which an exact solution exists.[10] The circular wing was a standard problem in the AGARD SMP study of isolated surfaces and the DLM results are included in the summary report of Woodcock.[11] Even when a comparison of the two methods could have been made with the experimental data of Försching et al.,[12] the opportunity was missed: Petkas' version of the DLM[13] was correlated with Försching's control surface data in Figs. 2 and 3 of Ref. 14 (which is Ref. 10 of Ueda and Dowell!).

Another inconsistency in Ref. 1 is the use of an undefined dimensonal reduced frequency, presumably the angular frequency divided by the freestream velocity. The accuracy of results for a reduced frequency $k = 1$ and 5 chordwise elements is questionable inasmuch as a sufficient number of lifting elements per wave length may not have been used.

Reference 1 also does not discuss the nonplanar problem. It is not likely that the nonplanar kernels[15,16] can be integrated in closed form even with the rectangular simplifications, so this leaves us with one more "new solution" to the *isolated* lifting surface problem. The DLM was motivated by the T-tail flutter problem[17] (the T-tail was an example in the original paper[2]), and the DLM now provides a solution for subsonic wing-body interference.[18] The DPM does not appear to offer comparable growth potential.

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