tinuity condition was replaced by an additional control point located on the body surface near the juncture. Calculated surface pressure coefficient distributions were quite smooth and agreed well with experimental results at zero[3] and nonzero[4] angle of attack, as seen in Fig. 1 taken from Ref. 4. Comparisons are made with experimental results of Ref. 7. Solutions obtained using the piecewise linear source singularity for this class of axisymmetric bodies oscillated wildly when the source strength was forced to remain continuous across a jump in body curvature. Also, the source strength distributions obtained for the accurate flowfield representation (no continuity requirement at the juncture) displayed a discontinuous jump at the juncture from positive (source) within the nose to negative (sink) within the cylinder. This jump in strength was apparently necessary to turn the flow to be tangential to the cylindrical portion of the body just downstream of the juncture. It is expected that elimination of these continuity requirements for the linear axial source method used for the second example in Ref. 1 should lead to significant improvements in accuracy. However, it must be noted that these comments support the basic conclusion of D'Sa and Dalton, that axial-singularity methods are less reliable than a surface-singularity formulation, since accurate axial-singularity results appear to require relatively higher numerical precision and, at times, insight into the appropriate choice of element size distribution.

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Reply by Authors to J. M. Kuhlman

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WE have reservations about several issues raised by J. M. Kuhlman in his Comment on our paper.[1] First, Kuhlman is reminded that the "exact" representation of an ellipsoid of revolution by linear variation of sources between the foci is valid only under a certain "simplifying condition."[2] Even though the ellipsoid of revolution may represent an "exact solution" when the foci represent the element endpoints, this situation does not apply to our calculations because the foci did *not* represent the element endpoints. We used empirically determined distances of 2 and 98% of the body length to begin and end the element distributions. Not using the foci as element endpoints generates a numerical solution, which can represent a test case for comparison to the exact solution. In addition, it is probably true that Kuhlman's CDC calculations contributed significantly to the small errors that he reports.

The authors are puzzled by what Kuhlman means when he suggests the superposition of his line-doublet singularity distribution with a surface-singularity method to represent a body at angle of attack. When a surface-singularity method is used, it is not necessary to augment it to represent an angle-of-attack flow.

Second, Kuhlman suggests representing complex body shapes by imposing specific conditions that are far from simple on the distribution of the axial sources. Even though Kuhlman used the abutting singularity elements at the nose-cylinder or tail-cylinder junctures, there is no mathematical basis for choosing any particular adjacent elements to effect this condition. The question then becomes a matter of where the discontinuity in source strength is allowed to occur in the source distribution. This condition seems to be based purely on computational experience, especially since the axial-singularity solution is not unique.

The authors are also puzzled by Kuhlman's comment concerning a curvature discontinuity in our second example. We do not see this body as having a discontinuous curvature.

In conclusion, we understand that Kuhlman is attempting to improve the accuracy of the axial-singularity method by placing restrictions on how the axial singularities are located and distributed. Kuhlman suggests improved numerical precision, and we certainly agree. Kuhlman is reminded that simplicity of formulation and application is the strength of the axial-singularity method. With the modifications suggested by Kuhlman, the method loses some of its simplicity in formulation and becomes more of an art in application. The second test case of D'Sa and Dalton, the "complex" body shape, was the true application we sought to make. The more complex the body, the more innovative one needs to become to implement the axial-singularity method. In essence, the more complex the body, the more the method loses its simplicity. This tends to lead the user back to the surface-singularity method, which we observed to be the superior method.

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Comment on "A Ring-Vortex Downburst Model for Flight Simulation"

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IN Ref. 1, Ivan has improved on the numerical calculation method given in Ref. 2 for computing the velocity at an arbitrary point in a simulated downburst. The essence of his method is to use the closed-form solution for the stream function of a ring vortex, which is exactly equivalent to that of the

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Table 1 Strengths and radii of a four-vortex set

Vortex no.	Relative radius	Relative circulation
1	1	100.0
2	3	84.7
3	5	56.7
4	7	19.9

uniform doublet sheet of Ref. 2. He approximates the elliptic functions in it by a convenient algebraic expression, and numerically differentiates it, instead of using the closed-form solution for the velocity components directly, as was done in Ref. 2. This results in a time-saving that could be important in real-time simulations, and represents a significant contribution to the simulation of downbursts.

Ivan has made two other changes to the model of Ref. 2, however, that are not, in the writer's view, improvements:

1) He uses a single ring vortex instead of a variable-strength sheet of singularities.

2) The ring vortex has a finite core of uniform vorticity (solid-body rotation).

The reason we used a more complex model in Ref. 2—i.e., a variable-strength doublet sheet instead of a constant one—is that we considered the latter to be inadequate for a downburst model (see Figs. 3 and 5 of Ref. 2). We concluded that the single-vortex model generates horizontal shears that are too weak, and vertical velocities that are too small for a given maximum horizontal wind. That is, the three quantities—maximum horizontal wind, maximum vertical wind, and maximum wind shear—do not have appropriate relative magnitudes. This weakness is not corrected by Ivan's use of a core. The variable-strength sheet also has the advantage of offering the user additional parameters to adjust to enable the model to be tailored to meet varying requirements.

The merit of using a finite core in the ring vortex is questionable. The fluid dynamics is spoiled by a discontinuity in velocity at the boundary of the core. This is obvious from an examination of the figure on page 238 of Lamb.[3] This figure shows that the streamlines are not circles concentric with those of the core. Consequently, some streamlines of the irrotational field exterior to the core must cross those interior to the core. If the flight path being studied does not intersect the finite core, the existence of the core is irrelevant. If it does intersect it, it must do so at either 2 or 4 points and there will be a discontinuity in the wind vector at each of these. This discontinuity is nonphysical, undesirable, and unnecessary. The reason given by Ivan for introducing the finite core is to avoid the very high velocities that can exist close to the center of a line vortex. As the velocity distributions in Ref. 2 clearly show, this is not a problem for landing and takeoff flight paths, which are necessarily close to the ground and, hence, well removed from the vortex core, which would normally be at a much higher altitude.

The advantages of Ivan's computation method and the better physical modeling of Ref. 2 can easily be combined. One needs only to use a few concentric vortices of different radii—3 or 4 would do. The relative spacings and magnitudes of such vortices can be calculated with the aid of Eq. (4b) of Ref. 2. One could then get better and more flexible wind fields, with 3 or 4 times the computation time needed in Ref. 1. By way of example, the relative strengths and radii of a four-vortex set are shown in Table 1.

Finally, a point of style and editorial policy. The paper uses computer variable names and symbolism instead of conventional mathematical symbols. (I have refused to accept these in theses.) Equations like (2) and (14) of Ref. 1 are simply ugly, and are less convenient to print and read than conventional mathematics. Computer jargon has its proper place; that place is not as a substitute for mathematical notation in journal articles.

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Reply by Author to B. Etkin

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PROFESSOR Etkin presents the interesting concept of using "a few concentric vortices of different radii" to approximate the downburst wind shear effects modeled in Etkin's Ref. 2 with a variable-strength doublet singularity sheet. That concept involves extra costs in computational complexity that may be warranted for special downburst simulations requiring the benefits of better representations of the secondary features of the wind shear flow patterns.

The following were the rationalizations for considering the single-ring vortex model to be a useful though simple model of downburst wind flow patterns:

1) The parameter of primary importance in classifying the severity of wind shears in downbursts was considered to be the maximum differential horizontal velocity across the downbursts as summarized in several papers that presented meteorological downburst data from the Joint Airport Weather Studies (JAWS) Project of 1982. During takeoffs or landings in downbursts, an airplane's major loss of aerodynamic energy height was assumed to be affected primarily by the magnitude of the change in horizontal wind encountered and only secondarily by both the gradient of horizontal wind shear and the downburst's vertical downdraft velocity within 200 ft of the ground. The latter region is of prime concern for airplanes encountering hazardous wind shears.

2) True, the majority of critical simulated takeoff and landing flight paths through downbursts are at low altitudes and are thus unlikely to intersect the finite core of solid-body rotation assumed for the ring-vortex model in Etkin's Ref. 1. However, the finite core provides a simple artificial interpolation technique to produce graceful variations in the wind disturbances encountered at higher noncritical altitudes, as might be reached following a go-around maneuver in a piloted real-time flight simulation. The pilot's perception of such a simulated finite-core wind shear is unlikely to be affected adversely by the fluid dynamic discontinuity in wind shear velocity at the boundary of the core, particularly if a typical simulation superimposes on the downburst wind components an additional set of short wavelength turbulence components that increase the work load on the pilot.

Concerning the final point on style, I hope that future increasing usage of computer workstations will lead to general acceptance of computer variable names and symbolism in journal articles concerning computer software. Computer variable names were used directly in the original development and checkout of the software for my ring-vortex downburst model and, for expediency, those same computer variable names were also used in AIAA Paper 85-1749 to avoid tedious translations to conventional mathematical symbols.

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