at the right-hand side of Eq. (2c) have the same form as those on the left-hand side and can therefore be evaluated as safely and accurately as in Eq. (1b). Incidentally, these integrals (doublet potentials) lead to simpler expressions than those on the left-hand side of Eq. (1b) (See the explicit formulas in Ref. 2.) Moreover, in any low-order implementation, the integrals on the right-hand side of Eq. (2c) have already been computed to yield the influence matrix and can simply be called from computer memory. We conclude that the modified integral equation (2c) is preferable to Eq. (1b). This is also true in the case of lifting bodies, since the additional integral over the wake is the same in both Eqs. (1b) and (2c). [See Ref. 1 for the details. There is a additive term $4\pi\phi_{\infty j}$ missing on the right-hand side of Eq. (5).]

Finally, we note that the doublet-only formulation of Eq. (2a)—regardless of the particular integral equation chosen—is less general than the formulation of Eq. (1a) based on the full Green's formula. The former approach is possible only if ϕ_∞ exists and is given at least on S. For the latter approach, only V_∞ on S is required. This is convenient in applications in which ϕ_∞ does not exist or is difficult to obtain explicitly.[4] Of course, it is not hard to incorporate both Eqs. (1b) and (2c) in the same code and to use Eq. (2c) whenever ϕ_∞ is available.

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Reply by Author to G. Gy. Groh

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GROH'S comments on the low-order panel method formulation are very useful from a mathematical viewpoint, but it should be emphasized that the methods discussed in the paper are just two of a large number of possible internal potential flows. For example, in the earlier work an internal flow parallel to the wing chord plane, i.e., $\phi_i = -x$ was also considered and showed a slight improvement over method 2 in the trailing-edge region. The main function of a "good" internal flow would appear to be to minimize the perturbation required of the doublet solution to satisfy the boundary condition. However, the formulation of such an internal flow may not always be convenient. As Groh points out, method 1 described in the paper has a simpler formulation than method 2; in fact, it probably has *the* simplest formulation of any method providing the lifting solution. To emphasize this point, method 1 uses just one term from the three-component velocity influence coefficient in the original *nonlifting* Douglas-Neumann code. However, it has three drawbacks:

1) The doublet value being the external *total* potential can lead to increased numerical error in the solution.

2) Obtaining velocities by numerical differentiation of the *total* potential is prone to error (in method 2 only the gradient of the *perturbation* potential is obtained numerically).

3) Nonzero normal velocities cannot be treated on the closed boundaries—source singularities are required to cancel the jump in normal velocity across the boundary.

As Groh points out, drawbacks 1 and 2 can be removed. A general way of achieving this is to separate the doublet distribution into two (or more) parts: an applied part, for which the velocity is known, and a small unknown part, which is to be solved. Again, there are a number of possible combinations; the obvious choice is to use ϕ_∞ as the applied distribution and to solve for the perturbation potential. This leads to Groh's Eq. (2c). The solved part of the doublet distribution (i.e., the perturbation potential) is then numerically the same as for method 2. The gradient of the *perturbation* potential is then evaluated numerically and added to the known local tangential component of V_∞.

Drawback 3 is the main reason for preferring the method 2 formulation for the general case. The power of the panel method lies in representing complete aircraft configurations, including modeling of the inlet flow, jet efflux, boundary-layer displacement effect, unsteady motions, and perturbation solutions. These all lead to the need to include the source term for the general case. As Groh observed, the two forms can be mixed in a given problem; in fact, in some complex cases, the ideal setup would be to have a number of internal flows for application in different parts of the problem to minimize the magnitude of the local doublet solution.

Finally, Groh has pointed out the missing $4\pi\phi_{\infty J}$ in Eq. (5) of the paper; we should also note that the last term in Eq. (3) should be $4\pi\phi_{\infty P}$ rather than $\Phi_{\infty P}$.

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Comment on "Effects of Atmospheric Turbulence on a Quadrotor Heavy-Lift Airship"

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THE subject paper[1] reports a study of the response of a particular LTA to atmospheric turbulence, utilizing a model and method of analysis developed in Refs. and 2 and 3. Unfortunately, there is a theoretical error in the formulation of forces caused by fluid acceleration that leads to an overestimation of the response (loads and motions). There is another flaw in the analysis—a bad assumption—that works in the opposite direction. These two points are elaborated below.

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The theoretical error relates to the "buoyancy" force that acts on an LTA associated with acceleration of the air mass that surrounds the vehicle. Acceleration of the air implies a pressure gradient through the fundamental equation of motion

$$\nabla p = -\rho \frac{DW}{Dt} \tag{1}$$

where W is the air velocity vector relative to an Earth-fixed frame of reference. The force acting on the body is ∇p times the volume of the body (when ∇p is uniform). The convective derivative in Eq. (1) can be expanded to read

$$\frac{DW}{Dt} = \left(\frac{\partial}{\partial t} + W \bullet \nabla\right) W \tag{2}$$

of which one representative scalar component is, for example,

$$\frac{DW_X}{Dt} = \frac{\partial W_X}{\partial t} + W_X \frac{\partial W_X}{\partial x} + W_y \frac{\partial W_x}{\partial y} + W_z \frac{\partial W_x}{\partial z} \tag{3}$$

If the wind consists of a steady uniform $\bar{W}$ parallel to 0x, and fluctuating components (u_g, v_g, w_g), Eq. (3) becomes

$$\frac{DW_x}{Dt} = \frac{Du_g}{Dt} = \frac{\partial u_g}{\partial t} + (\bar{W} + u_g)\frac{\partial u_g}{\partial x} + v_g \frac{\partial u_g}{\partial y} + w_g \frac{\partial u_g}{\partial z} \tag{4}$$

The error in Ref. 1 can be traced to a misuse of the above result in Eq. (5-4) of Ref. 3, which reads (using my notation)

$$\frac{Du_g}{Dt} = \frac{\partial u_g}{\partial t} + (u - \bar{W} - u_g)\frac{\partial u_g}{\partial x} + (v - v_g)\frac{\partial u_g}{\partial y} \tag{5}$$

where (u, v) are components of the LTA velocity relative to the Earth-fixed frame. The authors have treated Eq. (2.9) of Ref. 4, which is cited as justification of Eq. (5), as an absolute rate of change (of fluid velocity), when it is actually a "perceived rate of change"—something quite different. The absence of the z term in Eq. (5) is inconsequential, it is the replacement of the *air* velocity $(\bar{W}+u_g)$ by the *relative* velocity $(u-\bar{W}-u_g)$ that does the damage. For it leads to the absurd results that the buoyancy force (pressure gradient force) *depends on the speed of the body* (u, v) *and increases linearly with it,* and is present even when it should be zero. When the acceleration is zero, solving for $\partial u_g/\partial t$ from Eq. (4) with $Du_g/Dt = 0$ and substituting in Eq. (5) gives (omitting the last term):

$$\frac{Du_g}{Dt} = (u - 2\bar{W} - 2u_g)\frac{\partial u_g}{\partial x} + (v - 2v_g)\frac{\partial u_g}{\partial y} \neq 0$$

This demonstrates that Eq. (5) is wrong. The dependence of this force on speed can no more be true for $\partial p/\partial x$ than for the vertical buoyancy associated with gravity $(\partial p/\partial z = \rho g)$. The forces associated with ∇p can in fact be expected to be very small, quite the opposite of what the authors assert in several places in Refs. 1-3. This is because at even modest airspeeds, for example, in upwind flight at any ground speed ≥ 0, the assumption of "frozen turbulence" is quite good. Frozen turbulence implies that fluid motion changes are imperceptible in the time scale of interest for the flight problem, hence, so must the associated buoyancy forces.

The formulation of virtual mass effects in the subject work is correct in principle, however, since there really is a virtual mass and, hence, a fluid momentum proportional to the *relative* motion of body and fluid. The rate of change of this relative velocity (which is not the same as a fluid acceleration) therefore generates a rate of change of fluid momentum, and, hence, an aerodynamic force (see a related problem in Ref. 5, p. 235). However, the formulation used by the authors is exact only when the fluid motion extends uniformly to infinity. It is not exact for the random motions associated with turbulence, nor for the sinusoidal variations in the individual waves of shearing motion that combine to represent turbulence. Therefore, the implicit assumption in Refs. 1-3 is good for spectral components of very long wavelengths, but becomes increasingly worse as the wavelength becomes shorter. It could be expected to be quite bad at wavelengths less than the characteristic dimension of the vehicle.

Recently, some wind tunnel measurements have been completed at UTIAS of the spectra of normal force and pitching moment on an airship model with a fineness ratio of 4.0.[6] The results for the hull alone are compared in Fig. 1 with slender-body/strip theory. This theory assumes that each slice dx of the hull is exposed to a cross flow w_g that extends to infinity in the y and z directions. The crossflow varies sinusoidally with x but is frozen in time (hence there is zero fluid acceleration). As the body progresses through the flow it generates a time-varyng (periodic) momentum perturbation in the fluid and, hence, a periodic force reaction. No buoyancy term is included in the theory. Even so, the forces and moments measured fall substantially below the theory. The further addition of a spurious buoyance effect would render the theory even more conservative.

The second item that casts doubt on the results of Tischler and Jex[1] is the assumption of uncorrelated gust velocities at the four input points. In Ref. 2 (p. 100), the authors cite Ref. 4 in arguing that the cross correlations between turbulence components in the atmosphere can be neglected at separations over about 100 ft. This is entirely without foundation. On the contrary, Ref. 4, Fig. 13b, shows that at heights on the order of 100 m above ground, we should expect to see correlation lengths on the order of 40-150 m (130-500 ft). Thus, a large cross correlation must occur at distances on the order of 100-200 ft, especially at small wave numbers. Moreover, the numerical example of Ref. 1 uses an integral scale of 1750 ft! In that case, turbulence components will be very highly correlated at the separations used. [Equation (7) of Ref. 1, which gives the superposition of uncorrelated responses, appears to contain typographical or other errors.] A fully correlated response to four inputs could be as much as 100% larger than a fully uncorrelated response.

To conclude, there are two errors that are compensatory in sign; therefore, one cannot assess the net effect on the numerical results and the conclusions drawn. The effects of

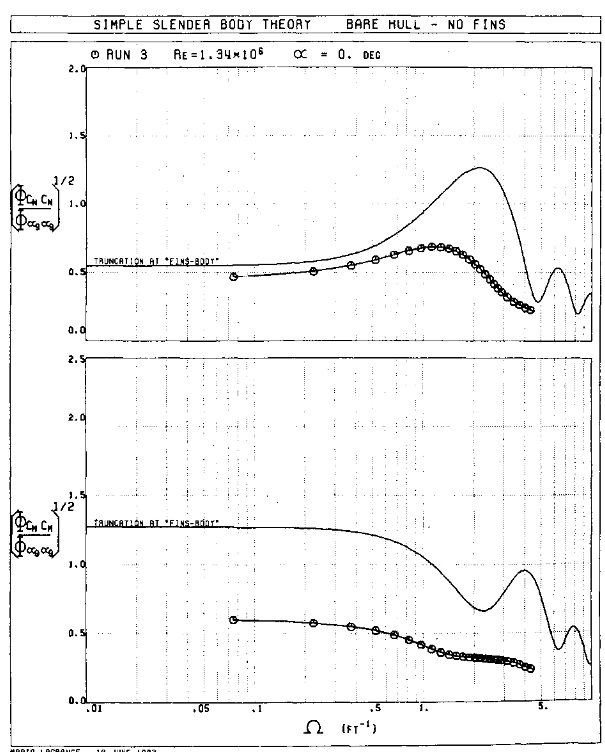

Fig. 1 Experimental results vs simulation.

turbulence on the configuration studied have to be regarded as inconclusive at this time.

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Reply by Authors to B. Etkin

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ETKIN raises two major questions concerning our analysis of the response of airships to atmospheric turbulence as presented in Refs. 1 and 2. The first question results from Etkin's misinterpretation of our equation symbology involving relative vs absolute acceleration quantities. The second question takes issue with our assumptions concerning the degree to which gust correlation affects the calculation of the motions and loads on airships.

The authors regret that Eq. (5-4) of Ref. 2, which was presented in the introductory discussion of the turbulence environment (Sect. 5, Art. A), was confused with the actual equations used to calculate the apparent mass and pressure gradient forces (Sec. 8, Art. H). Our sometimes cumbersome system of subscripts and superscripts is needed to distinguish among the many vector quantities, axis systems, and components of the heavy-lift airship system modeled in this study.

Our force equation development carefully distinguishes between *relative* (apparent) *air mass/vehicle* acceleration, denoted in Ref. 2 by $\mathring{V}^a$, and the absolute air mass acceleration, denoted in Ref. 2 by $\dot{V}^{am})_{total}$. The axial component of the *relative* acceleration vector ($\mathring{V}^a$) is given in Ref. 2 [Eq. (8-9)] by:

$$\mathring{u}^a = \mathring{u}_h - \left(\frac{\partial u^{am}}{\partial t} + u^a \frac{\partial u^{am}}{\partial x} + v^a \frac{\partial u^{am}}{\partial y}\right)$$

where

$u^a = u - u^{am}$ = relative axial airspeed

$v^a = v - v^{am}$ = relative lateral airspeed

u^{am}, v^{am} = air mass (gust) velocity components

$\frac{\partial u^{am}}{\partial x}$, $\frac{\partial u^{am}}{\partial y}$ = air mass velocity gradients

$\mathring{u}_h$ = hull body axial acceleration

The quantity in parentheses is denoted by Du^{am}/Dt in Eq. (5-4) of Ref. 2, and is termed the "relative *air mass* acceleration" in that discussion.

The various quantities in the preceding equations are given in components of the hull center-of-volume axis system (hence, the h subscripts in Ref. 2). Also, the quantities u^{am}, v^{am}, $\partial u^{am}/\partial x$, $\partial u^{am}/\partial y$ are the *effective* quantities at the hull center of volume (hence, the superscripts in Ref. 2), and are obtained from spacial averaging among the four gust input points, as described in Refs. 1 and 2.

The so-called "apparent-mass-type" forces, which are due to the change in momentum of the *relative* flow, depend on $\mathring{V}^a$. For example, [from Eq. (8-195) of Ref. 2],

$$X = -\rho \forall (K_a \mathring{u}^a + K_c qw - K_b rv)$$

where $\mathring{u}^a$ is the x component of $\mathring{V}^a$, ρ the atmospheric density, $\forall$ the hull volume, q, r the body axis angular rates in pitch and yaw (excluding air mass motion), v, w the body axis linear velocities (excluding air mass motion), and K_a, K_b, K_c the so-called "apparent-mass" constants.

In contrast to the above "apparent-mass-type" forces, the "dynamic buoyancy pressure gradient," ∇P, depends on the *absolute* air mass acceleration, $(\dot{V}^{am})_{total}$ defined in Eq. (8-18) of Ref. 2. Expanding this quantity in the axial direction yields [from Eq. (8-18) of Ref. 2]

$$(\dot{u}^{am})_{total} = \dot{u}^{am} + u^{am}\frac{\partial u^{am}}{\partial x} + v^{am}\frac{\partial u^{am}}{\partial y}$$

where

$$\dot{u}^{am} = \frac{\partial u^{am}}{\partial t} = \text{acceleration of the air mass}$$

The "dynamic buoyancy" force is obtained from [see Eq. (8-243) of Ref. 2]

$$X_{db} = \rho \forall (\dot{u}^{am})_{total}$$

Thus, the buoyancy force depends on $(\dot{u}^{am})_{total}$ which involves air mass quantities *only*, and does not depend on hull motion, as asserted by Etkin.

Having established the mathematical accuracy of our analysis, we now consider the relative importance of the air mass acceleration terms ($\partial/\partial t$) for airship motions. Conventional aircraft have a very small buoyancy ratio ($\rho\forall g/mg << 1$), and only second-order unsteady aerodynamic contributions. For such aircraft, the "frozen-field" approximation which neglects the air mass acceleration terms ($\partial u^{am}/\partial t$, etc.) yields reasonable results for all but nearly convected flight.[3] However, the airship is a special case since it has a large relative buoyancy ($\rho\forall g/mg \doteq 1$), and very small drag forces at its typically low operational speeds. In fact, this is the secret of their fantastic cruise endurance. This neutral buoyancy condition and large apparent mass renders the

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