Reply by Author to F.R. Goldschmied

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THE Cebeci-Smith boundary-layer method is a numerical method which is found to give results which are satisfactory for most engineering problems. There is nothing special about it. The numerical scheme is an accurate and economical one, and the algebraic turbulence model has been correlated with available experimental data to produce reasonable results. This method is not expected to handle both the separation location and the resulting pressure distribution at separation, since such a capability will require an iteration between an inviscid and a viscous flow. What I mentioned in my paper was our observation of calculating the separation point by this method for a *given pressure distribution*. I am sure Mr. Goldschmied will agree with me, that flow separation changes the pressure distribution. So, to what extent the given inviscid pressure distribution for a flow with separation can be used is questionable. On the other hand, in using an inviscid pressure distribution I feel that one can locate the separation point for both laminar and turbulent flows by using zero-c_f criterion. Furthermore, I find it ridiculous to accept that the $c_f=0$ criterion for separation depends on chordwise location only. Actually, neither the chordwise location nor the pressure drop should be used directly to predict separation. Separation is the result of boundary-layer development and occurs when the wall shear goes to zero. Chordwise location and pressure drop at separation are results and not prerequisites for predicting separation.

Mr. Goldschmied claims that he and his associates have developed a method for predicting pressure distributions with massive separation. However, upon close examination, I find their method to be another self-predicting empirical correlation. Because the mystic Goldschmied separation criterion fails to predict the true separation pressure level, he adds a correction which also changes the separation location. So far, so good, but then he proceeds to test his method on cases which formed the basis for his empirical correlation! No independent test cases were presented. What else can one expect but good agreement? This is a good example of a circular argument.

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Comments on the Notion of "Loft Ceiling"

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Nomenclature

a = speed of sound
C = highest usable lift coefficient ($C_{L_{\max}}$)
E = specific energy $E=h+V^2/2g$
g = acceleration of gravity
h = altitude
h_L = loft-ceiling altitude
M = Mach number
p = static pressure
q = dynamic pressure defined in Eq. (2)
V = aircraft velocity
W/S = wing loading
γ = ratio of specific heats
λ_1 = density scale height in exponential atmosphere
λ_2 = pressure scale height in exponential atmosphere
ρ = air density
σ = density ratio

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IN the series of stimulating papers on differential turning games[1-3] H.J. Kelly has introduced the notion of "loft-ceiling" as one of the parameters defining the capture sets of the game. The loft-ceiling h_L is defined as the highest altitude permitting vertical equilibrium in level flight. Neglecting the secondary thrust effect, the vertical equilibrium is expressed by

$$qSC \geq W \tag{1}$$

where q, the dynamic pressure, can be given by two equivalent expressions

$$q=\tfrac{1}{2}\rho V^2=(\gamma/2)pM^2 \tag{2}$$

The loft-ceiling altitude is defined either by the corresponding density $\rho(h_L)$

$$\rho(h_L)=2(W/S)/V^2C \tag{3a}$$

or by the static pressure $p(h_L)$ at that altitude

$$p(h_L)=2(W/S)/\gamma M^2C \tag{3b}$$

The second expression is more adequate for cases where the maximum lift coefficient is Mach number dependent.

In French textbooks a similar notion called "plafond de sustentation" (or "lift-ceiling") is frequently used. It has been pointed out[4] that for aircraft of subsonic design the value of $M^2C(M)$ is bounded, indicating the existence of an absolute limit. For supersonic aircraft $M^2C(M)$ may have a maximum at transonic speed, but even though it recovers for higher Mach numbers its value is monotonically increasing. For such aircraft there is no absolute ceiling for vertical equilibrium; the ceiling is merely state dependent.

In the differential turning games[1-3] a reduced order aircraft model is used in which the relevant state variable is the specific energy $E=h+V^2/2g$. The "loft-ceiling" has to be interpreted therefore as the limiting altitude for vertical equilibrium, depending on the aircraft's specific energy $h_L(E)\geq h$.

Substituting in Eq. (1) the specific energy into the dynamic pressure q leads to a direct explicit relationship between the specific energy and the loft-ceiling altitude

$$E=h_L+\frac{gC}{W/S}\,\frac{1}{\rho(h_L)}=E(h_L) \tag{4}$$

This relationship is defined by the intersection of the line of a given specific energy with the line of C which designates, for a given configuration, the boundary of 1g engine-off stalling speed. The slope of the curve $E(h_L)$ is given by

$$\frac{dE}{dh_L}=1-\frac{W/S}{gC}\,\frac{1}{\rho}\left(\frac{1}{\rho}\,\frac{d\rho}{dh_L}+\frac{1}{C}\,\frac{dC}{dh_L}\right) \tag{5}$$