NASA-CR-201062

Development and Testing of Airfoils for High-Altitude Aircraft

Final Report

for the Period
September 1, 1994 – May 31, 1996

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June 1996

1 Completed Work

Essentially all the work proposed in the proposal has been completed or adequately addressed. Additional work on heat-exchanger aerodynamics not in the original proposal was also conceived and executed with the agreement of the Technical Monitor.

A summary of the specific tasks accomplished is given below. The corresponding detailed results have been transmitted to Dryden personnel via e-mail as soon as they became available. All e-mail transmissions have been saved, and the key ones are given in the Appendices, along with relevant plots, figures, and papers which were generated.

1.1 Airfoil design

The Apex-16 airfoil was designed specifically for the APEX test vehicle. It is intended to be representative of airfoils required for lighweight aircraft operating at extreme altitudes, which is the primary research objective for the APEX program. In the course of designing this airfoil, Fairly extensive studies were made over the Mach number and Reynolds number ranges of interest. Also considered were the airfoil thickness, pitching moment, and off-design behavior. Limited use was made of the optimization driver LINDOP [1] to fine-tune the final design and resolve the numerous conflicting requirements. Appendix B shows the Apex-16 airfoil geometry, coordinates, and computed performance polars.

1.2 Study of airfoil constraints on pullout maneuver

The maximum ceiling parameter $M^2 C_L$ value achievable by the Apex-16 airfoil (or any airfoil for that matter) was found to be a strong constraint on the pullout maneuver. It was concluded that if data is to be acquired in level flight, then some sort of lift augmentation would be required, since any airfoil has little or no excess-lift capability at its ceiling condition (by definition). A workable alternative approach which was identified is to use wing lift to achieve pullout, but then acquire data in windup turns to achieve the target C_L.

1.3 Selection of tail airfoils

Several candidate airfoils for the tail were examined. The primary goal was predictable behavior at low Reynolds numbers, and good tolerance to flap deflections. The NACA 1410 and 2410 airfoils (inverted) were identified as good candidates.

1.4 Examination of wing twist

A number of issues related to wing twist were examined. These included: simplicity, obtaining a uniform local c_ℓ across the test section, obtaining a high overall $C_{L\max}$

for the pullout, avoiding tip stall. Both washin and washout options were examined computationally. In the end it was decided that a simple flat wing was a reasonable compromise between all the requirements, and allowed the use of one mold for both wing halves — a significant cost saving.

1.5 Test section instrumentation and layout

It was decided that the instrumentation for the test section would consist of surface pressure taps, wake rakes, surface-mounted microphones, and skin-friction gauges. Using multiple wake rakes was deemed desirable to verify spanwise uniformity, but was found to be too demanding of the limited data bandwidth available. Using a single wake rake mounted on a mechanical traverse was rejected due to weight and complexity. It was decided to use a single fixed wake rake for accurate measurement of the wake momentum defect, and multiple integrating rakes to verify spanwise uniformity. Several test section layouts were designed, with varying degrees of compactness. One of these was selected by Dryden personnel for use on APEX.

The wake rake was designed specifically to capture the wakes anticipated at test altitudes. A design guide for the integrating rakes was also prepared. The layouts and design guide are attached as Appendices B and C.

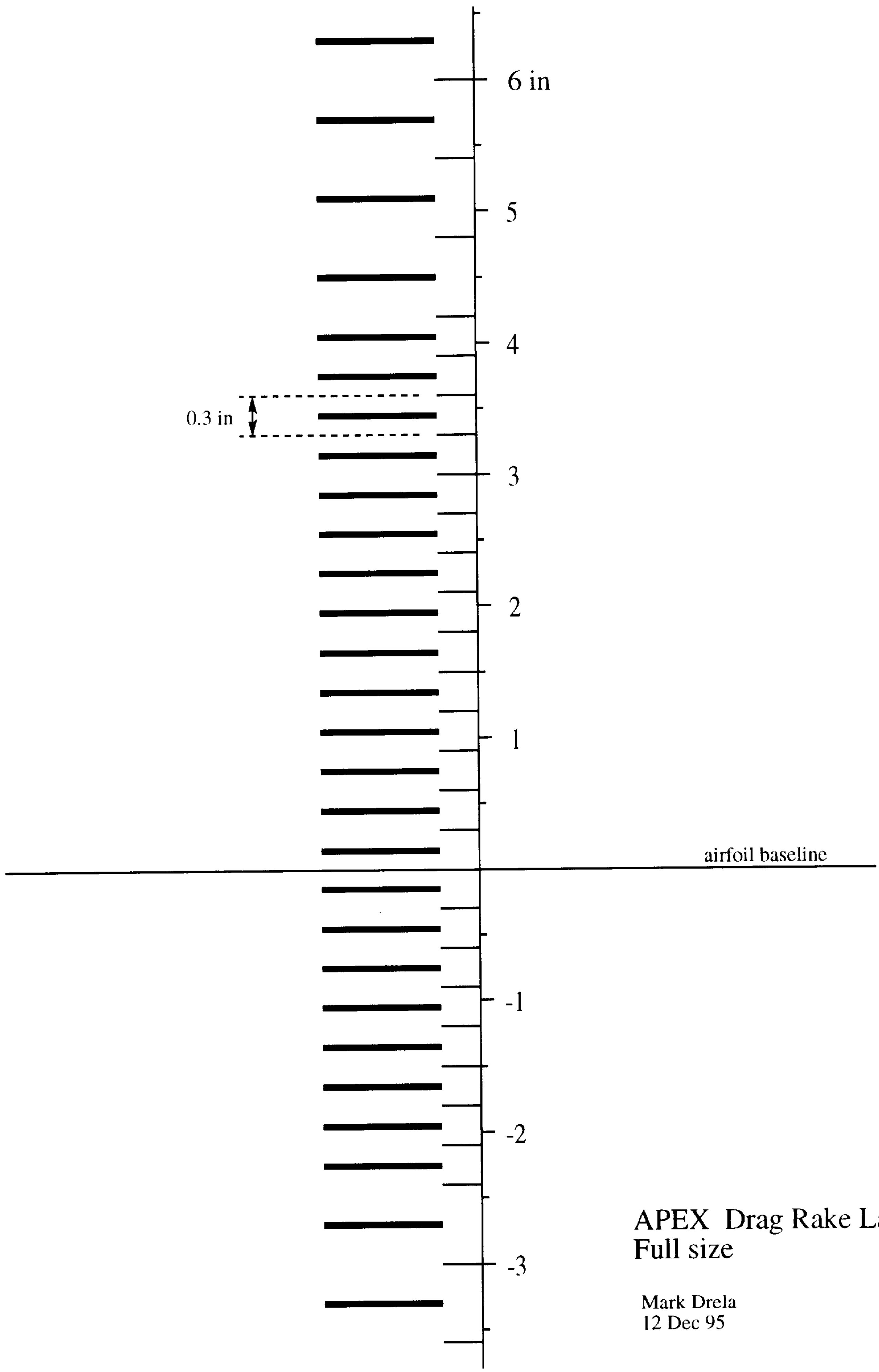
1.6 Integrated airfoil/heat-exchanger tests

A modest wind tunnel test was performed for an integrated airfoil/heat-exchanger configuration, which is currently on Aurora's *Theseus* aircraft. Although not directly related to the APEX tests, the aerodynamics of heat exchangers has been identified as a crucial aspect of designing high-altitude aircraft, and hence is relevant to the ERAST program. The computational studies and tunnel tests have proven the validity of integrating the heat exchanger with the wing airfoil to achieve surprisingly low drag levels at low Reynolds numbers. The results have already appeared in the Jan-Feb '96 issue of the *Journal of Aircraft* [2]. This paper is attached as Appendix D.

References

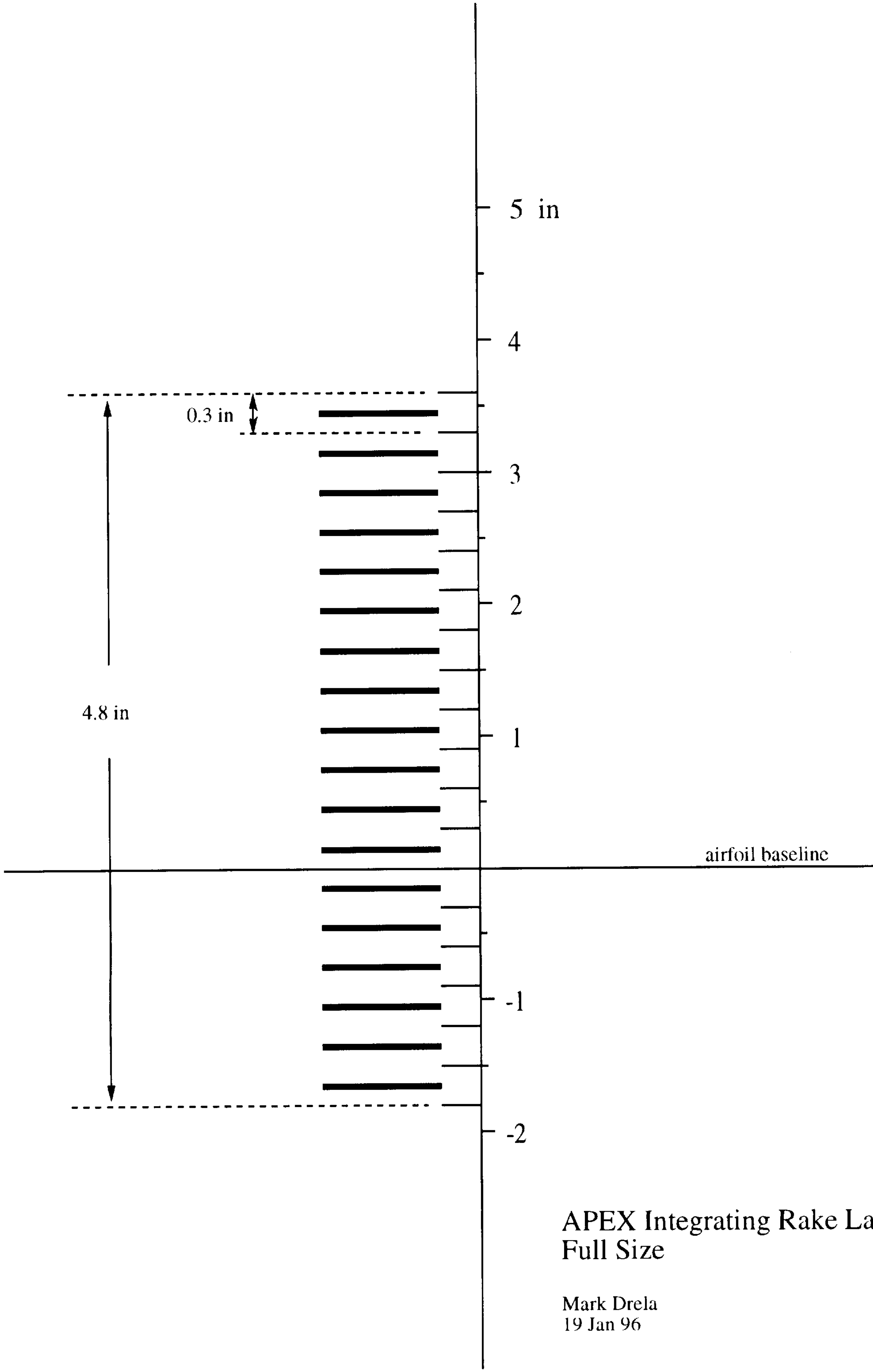
[1] M. Drela. Design and optimization method for multi-element airfoils. AIAA Paper 93-0969, Feb 1993.

[2] M. Drela. Aerodynamics of heat exchangers for high-altitude aircraft. *Journal of Aircraft*, 33(2), Mar-Apr 1996.



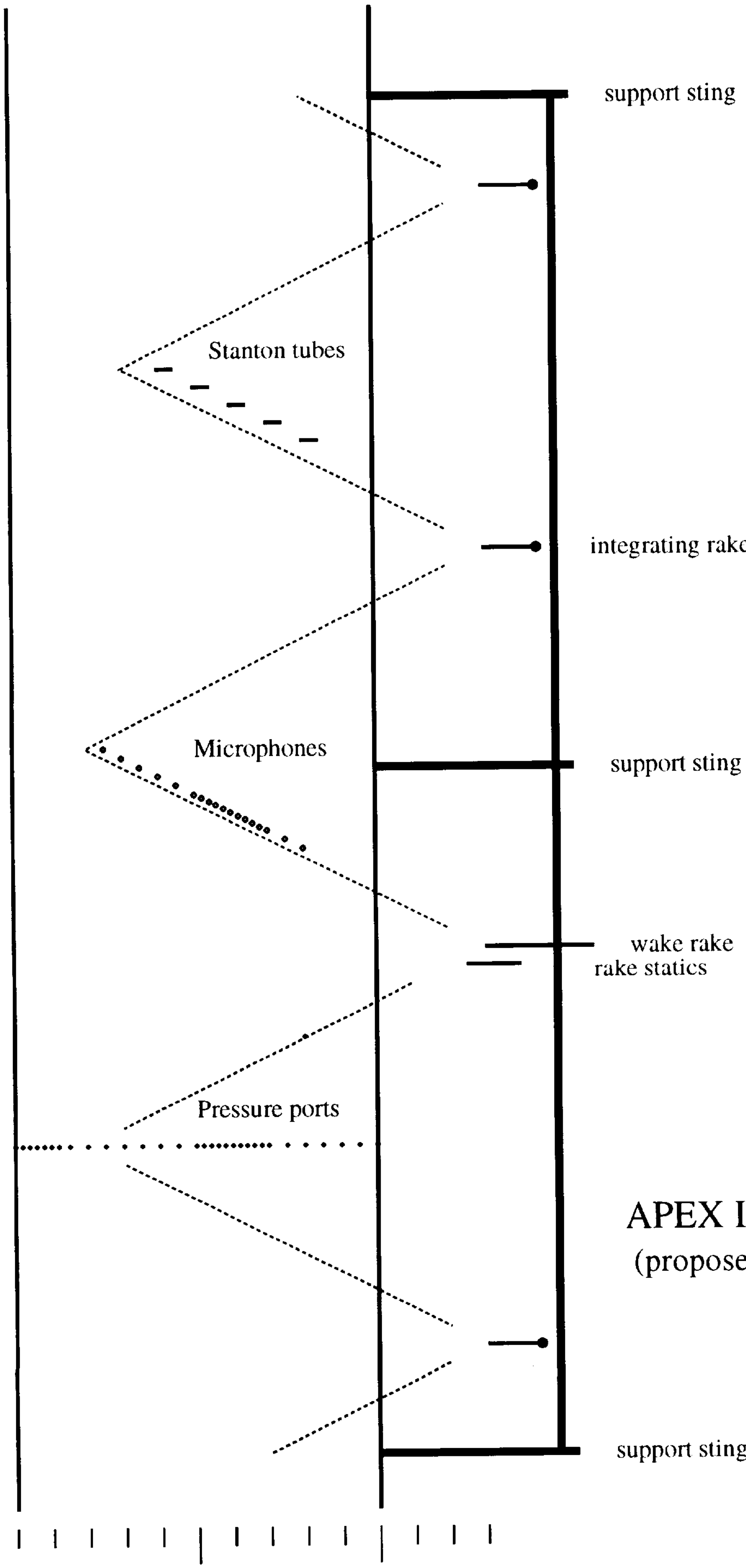
APEX Drag Rake Layout
Full size

Mark Drela
12 Dec 95



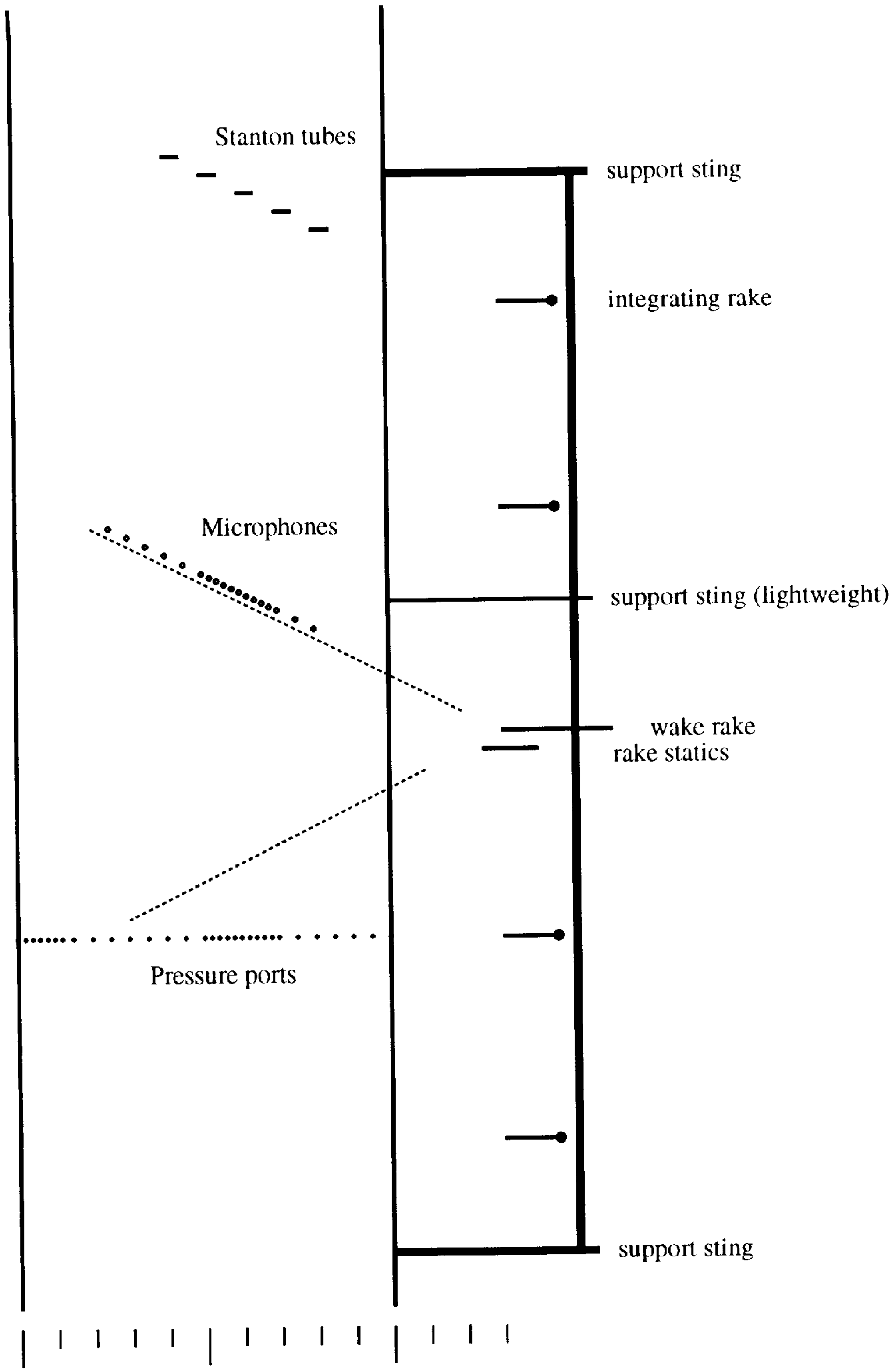
APEX Integrating Rake Layout
Full Size

Mark Drela
19 Jan 96



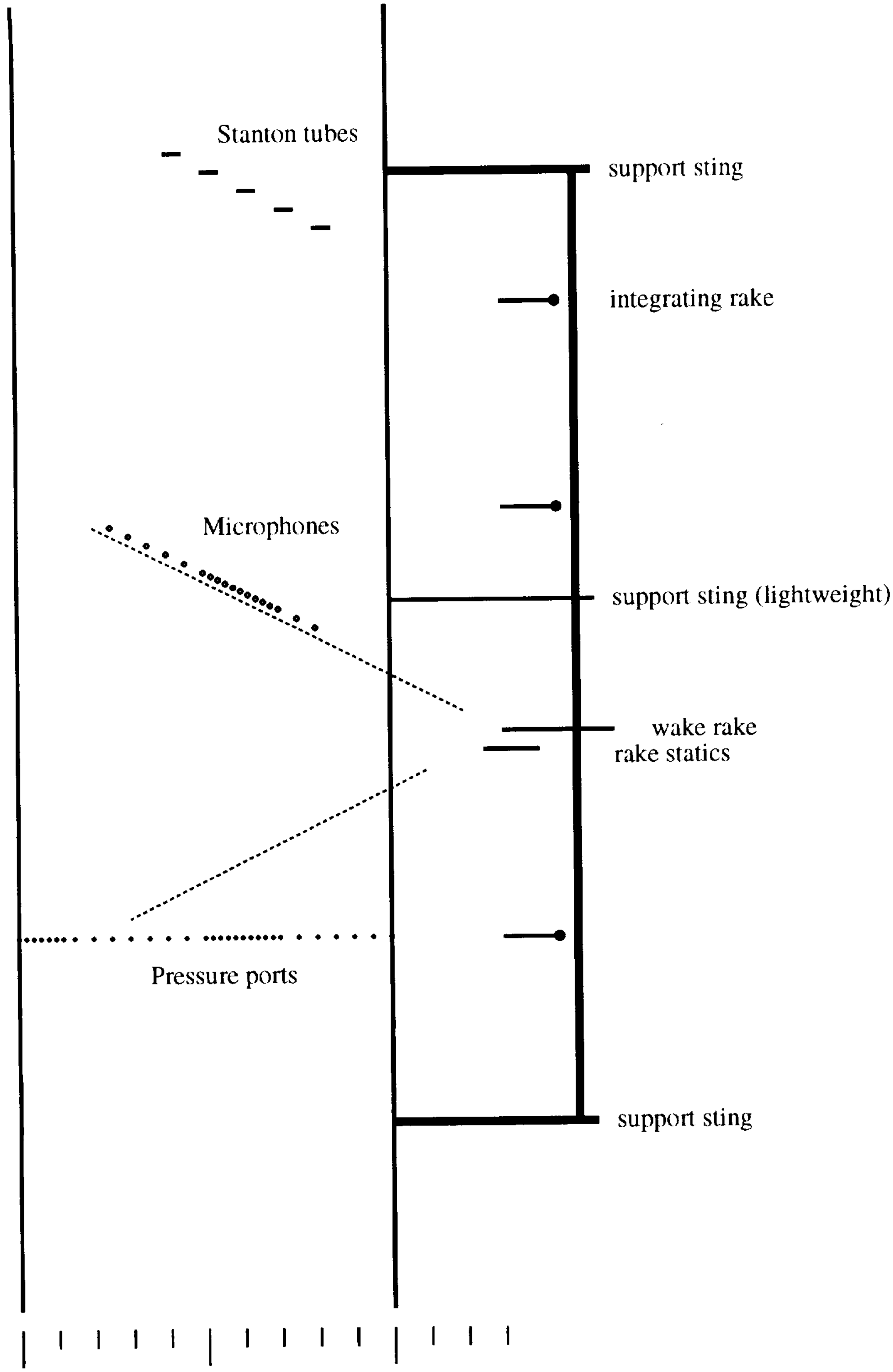
APEX Instrumentation Layout
(proposed)

Mark Drela
7 Feb 96



APEX Instrumentation Layout II
(proposed)

Mark Drela
9 Feb 96



APEX Instrumentation Layout III
(proposed)

Mark Drela
9 Feb 96

Appendix C: Integrating Rake Design Document

Integrating Rake Design

Mark Drela, MIT Aero & Astro

10 Dec 95

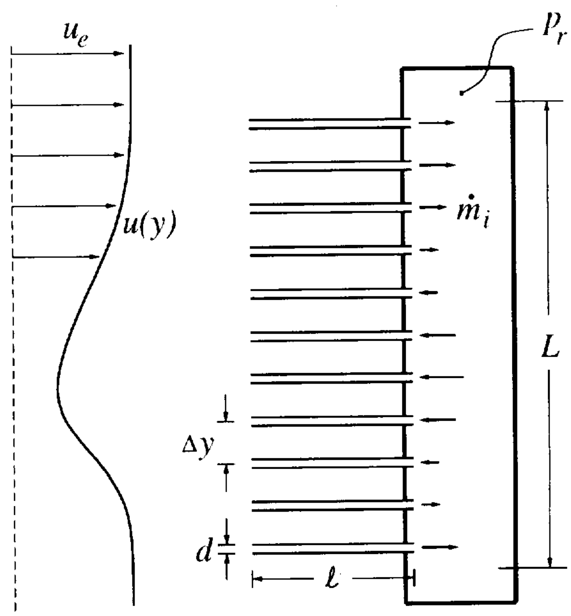


Figure 1: Integrating rake layout and dimensions

1 Basic Relations

An integrating rake consists of an array of pitot tubes all feeding into a common reservoir, as shown in Figure 1. Assuming that the static pressure across the entire wake is constant and equal to the edge value p_e, the wake velocity profile $u(y)$ will produce a y-dependent total pressure seen by the tubes:

$$p_o(y) = \tfrac{1}{2}\rho u^2(y) + p_e \tag{1}$$
$$= \tfrac{1}{2}\rho u^2(y) + p_{o_\infty} - \tfrac{1}{2}\rho u_e^2 \tag{2}$$

The non-uniform $p_o(y)$ will produce a mass flow $\dot{m}_i$ in the i'th tube. Assuming fully-developed Poiseuille flow in each tube, this mass flow is proportional to the driving pressure difference $p_o - p_r$, where p_r is the reservoir pressure to be measured.

$$\dot{m}_i = \frac{\pi}{128} \frac{d^4}{\nu \ell} (p_{o_i} - p_r) \tag{3}$$

At steady-state, all the tube mass flows must add up to zero, and if all the tubes have the same diameter d and length ℓ, the pressure differences must add up to zero as well.

$$\sum_{i=1}^{N} \dot{m}_i = 0 \tag{4}$$

$$\sum_{i=1}^{N} (p_{o_i} - p_r) \;=\; 0 \tag{5}$$

Substituting for p_{o_i} in terms of the local velocity u_i, and multiplying by the presumed-constant tube spacing Δy, we get

$$\sum_{i=1}^{N} \left(\tfrac{1}{2}\rho u_i^2 \;-\; \tfrac{1}{2}\rho u_e^2 \;+\; p_{o_\infty} \;-\; p_r \right) \Delta y \;=\; 0 \tag{6}$$

$$\sum_{i=1}^{N} \left(\rho u_e^2 \;-\; \rho u_i^2 \right) \Delta y \;=\; 2\,(p_{o_\infty} \;-\; p_r)\; L \tag{7}$$

where

$$L \;=\; \sum_{i=1}^{N} \Delta y \;=\; N\,\Delta y \tag{8}$$

is simply the height of the N-tube rake as shown in Figure 1.

The lefthand side summation in equation (7) is seen to be a midpoint-rule integration for the momentum+mass defect

$$\rho u_e^2(\theta + \delta^*) \;\equiv\; \int (\rho u_e^2 \;-\; \rho u^2(y))\, dy \tag{9}$$

$$\simeq\; \sum_i (\rho u_e^2 \;-\; \rho u_i^2)\, \Delta y_i \tag{10}$$

so that with the constant tube spacing Δy, the final result is

$$\rho u_e^2(\theta + \delta^*) \;\simeq\; 2\,(p_{o_\infty} \;-\; p_r)\, L \tag{11}$$

$$\text{or} \qquad \theta + \delta^* \;\simeq\; \frac{p_{o_\infty} - p_r}{p_{o_\infty} - p_e}\, L \tag{12}$$

For the most accurate measurement of $\theta + \delta^*$, it is clearly best to reference the reservoir and local-static pressure transducers to the freestream total pressure p_{o_∞}, so that $p_{o_\infty} - p_r$ and $p_{o_\infty} - p_e$ are direct measurements with no bias uncertainties.

2 Near-wake Corrections

Note that the integrating rake does not give the momentum thickness in isolation. If the measurements are to be used to support or validate drag calculations, this causes few problems, since the measured sum $\theta + \delta^*$ can simply be compared with the corresponding sum from the calculation. If the goal is absolute drag measurement, then it will be necessary to estimate the shape parameter $H = \delta^*/\theta$ to deduce the isolated θ. This can be done fairly accurately if only the minimum velocity in the wake is known.

$$U_{\min} \;=\; \min\left(\frac{u}{u_e}\right) \tag{13}$$

Measuring this will typically require a separate total pressure tube placed sufficiently close to the wake centerline.

Assuming the minimum velocity is known, a wake velocity profile is then quite closely approximated by the Coles cosine profile

$$\frac{u(y)}{u_e} = U_{\min} + (1 - U_{\min})\,\frac{1}{2}\left[1 - \cos\left(\pi\frac{y}{\delta}\right)\right] \quad ; \quad -\delta \leq y \leq \delta \tag{14}$$

where δ is the wake half-thickness. From the definitions of δ^* and θ, we have

$$\delta^* = \int_{-\delta}^{\delta}\left(1 - \frac{u}{u_e}\right) dy = (1 - U_{\min})\,\delta \tag{15}$$

$$\theta = \int_{-\delta}^{\delta}\left(\frac{u}{u_e} - \frac{u^2}{u_e^2}\right) dy = (1 - U_{\min})\,\delta - \frac{3}{4}(1 - U_{\min})^2\,\delta \tag{16}$$

$$H = \frac{1}{1 - \frac{3}{4}(1 - U_{\min})} \tag{17}$$

and the isolated momentum thickness then follows immediately from the measured $\theta + \delta^*$.

$$\theta = \frac{\theta + \delta^*}{H + 1} \tag{18}$$

The above results also apply to moderately assymetric wakes (such as the one sketched in Figure 1) since each wake half above and below the velocity minimum is still accurately represented by the Coles profile.

Both $U_{\min}$ and H quickly approach unity downstream, making their measurement or estimation less and less critical. Ideally, the rake should be moved downstream until it just captures the entire wake, but in practice the wake thickness and location are somewhat uncertain, and some conservatism will be required.

However the shape parameter is estimated, a correction will still need to be applied to the resulting momentum thickness if u_e/u_∞ at the rake is not unity, as is usually the case. The true profile drag/span is simply the ultimate momentum defect

$$D' = \rho u_\infty^2 \theta_\infty \tag{19}$$

where θ_∞ is the momentum thickness very far downstream. The evolution of the momentum thickness in the wake is given by the von Karman integral momentum equation

$$\frac{1}{\theta}\frac{d\theta}{dx} = -(H + 2)\frac{1}{u_e}\frac{du_e}{dx} \tag{20}$$

which can be integrated from the rake position to far downstream if a unique function $H(u_e)$ is assumed. The particular empirical assumption

$$\frac{\ln(u_\infty/u_e)}{H - 1} = \left(\frac{\ln(u_\infty/u_e)}{H - 1}\right)_{\text{rake}} = \text{constant} \tag{21}$$

results in the well-known Squire-Young formula.

$$\theta_\infty = \theta_{\text{rake}}\left(\frac{(u_e)_{\text{rake}}}{u_\infty}\right)^{(H_{\text{rake}}+5)/2} \tag{22}$$

3 Design Requirements

In light of the assumptions made in the derivation of relation (12), the following requirements must be met for the result to be valid.

1. The flow in the tubes is fully-developed Poiseuille flow.
2. The flow in each tube has negligible dynamic pressure.
3. The static pressure at the tip of each tube is the local total pressure $p_o(y)$.
4. The pressure in the reservoir is uniform.
5. The tubes all have the same length ℓ, diameter d, and spacing Δy.

Requirement 1 is equivalent to stating that the profile-development entrance length for each tube must be very small compared to the tube's length. This gives the requirement

$$\ell \gg \frac{\dot{m}_i}{4\pi\mu} \tag{23}$$

$$\text{or} \quad \ell \gg \frac{d}{16}\frac{\rho u_\infty d}{\mu} \tag{24}$$

which makes the worst-case assumption that $p_{o_\infty} - p_r \simeq \rho u_\infty^2/2$, corresponding to the entire rake being immersed in a massive separated wake. In any case, it should be possible to meet the Poiseuille-flow requirement without too much difficulty by using sufficiently fine tubes. The only drawbacks of excessively-fine tubes are fragility, and possibly an excessive settling time.

Requirements 2 and 3 are essentially equivalent, since negligible dynamic pressure in the tube automatically implies that the static pressure at the tip is nearly the full total pressure. The requirement is

$$u_{\text{mean}} = \frac{\dot{m}_i}{\rho\,\pi(d/2)^2} \ll u_\infty \tag{25}$$

$$\text{or} \quad \frac{1}{64}\frac{d}{\ell}\frac{u_\infty d}{\nu} \ll 1 \tag{26}$$

which turns out to be essentially the same as Requirement 1.

Requirement 4 means that the dynamic pressures in the reservoir must be negligible compared to the pressure drops across the tubes. With the dynamic pressures in the tubes already being negligible, this additional requirement is met by making the cross-sectional area of the reservoir larger than all the tube areas put together. With D being the reservoir diameter, the requirement is

$$D^2 \gg N\,d^2 \tag{27}$$

which can be very easily met in practice. On the other hand, an excessively-large reservoir is undesirable, since it will increase the settling time.

A design parameter which still must be selected is the appropriate overall height L. Clearly, this needs to be sufficiently large to capture the entire wake. However, making it much larger than the wake width is undesirable, since increasing L will proportionately reduce the magnitude of the pressure signal $p_{o_\infty} - p_r$, giving reduced accuracy. It is perfectly acceptable to build an oversize rake, and cap any tubes which fall well above or below the wake in order to improve accuracy.

The remaining design parameter is the total number of tubes N. This only influences the accuracy of the midpoint summation approximation to the transverse profile integral. Twenty tubes should be sufficient for typical featureless wake profiles. If the wake thickness is expected to be significantly narrower than the rake height L, more tubes may be appropriate.

4 Compressibility Effects

If quantitative results are to be obtained from the integrating rake, corrections to the above relations may be necessary if the freestream Mach number is appreciable. The exact relation for the pitot total pressure is given by any of the standard isentropic formulas, e.g.

$$p_o(y) \;=\; p_e \left(1 + \tfrac{\gamma-1}{2} M^2(y)\right)^{\frac{\gamma}{\gamma-1}} \;=\; p_e \left(1 - \frac{u^2(y)}{2h_o(y)}\right)^{-\frac{\gamma}{\gamma-1}} \tag{28}$$

For adiabatic flows at near-unity Prandtl numbers, the stagnation enthalpy can be assumed to be nearly constant across the wake and equal to its freestream value.

$$h_o(y) \simeq h_{o_\infty} \;=\; \frac{1}{\gamma - 1} \frac{u_\infty^2}{M_\infty^2} \left(1 + \tfrac{\gamma-1}{2} M_\infty^2\right) \tag{29}$$

Combining this with the second isentropic formula above gives

$$p_o(y) \;=\; p_{o_\infty} \frac{p_e}{p_\infty} \left[1 + \tfrac{\gamma-1}{2} M_\infty^2 \left(1 - \frac{u^2(y)}{u_\infty^2}\right)\right]^{-\frac{\gamma}{\gamma-1}} \tag{30}$$

which is the relation which would be used with a conventional rake to determine the velocity profile from the measured total-pressure profile and the local static pressure p_e.

Just outside the wake, where $u(y) = u_e$, the total pressure is the same as the freestream total pressure, which implies

$$\frac{p_e}{p_\infty} \;=\; \left[1 + \tfrac{\gamma-1}{2} M_\infty^2 \left(1 - \frac{u_e^2}{u_\infty^2}\right)\right]^{\frac{\gamma}{\gamma-1}} \tag{31}$$

This gives an alternative form for the total pressure profile

$$p_o(y) \;=\; p_{o_\infty} \left[\frac{1 + \tfrac{\gamma-1}{2} M_\infty^2 \left(1 - \frac{u_e^2}{u_\infty^2}\right)}{1 + \tfrac{\gamma-1}{2} M_\infty^2 \left(1 - \frac{u^2(y)}{u_\infty^2}\right)}\right]^{\frac{\gamma}{\gamma-1}} \tag{32}$$

which then replaces the equivalent incompressible form (2). A second-order Taylor series for equation (32) about $M_\infty^2 = 0$ conveniently recovers the incompressible form, but with a first-order Mach number correction.

$$p_o(y) = p_{o_\infty} - \tfrac{1}{2}\rho_\infty \left(u_e^2 - u^2(y)\right) \frac{p_{o_\infty}}{p_\infty} \left[1 - M_\infty^2 \left(\frac{\gamma-1}{2} \frac{u_\infty^2 - u^2(y)}{u_\infty^2} + \frac{u_e^2 - u^2(y)}{4\, u_\infty^2}\right)\right] + \mathcal{O}(M_\infty^6) \quad (33)$$

$$p_o(y) \simeq p_{o_\infty} - \tfrac{1}{2}\rho_\infty \left(u_e^2 - u^2(y)\right) \frac{p_{o_\infty}}{p_\infty} \left[1 + \tfrac{\gamma-1}{2} M_\infty^2 \left(\frac{u_e^2}{u_\infty^2} - 1\right)\right] \quad (34)$$

Equation (34) truncates the Taylor series, and also drops terms which are $\mathcal{O}\left[(u_e^2 - u^2)^2\right]$, which is justifiable for a typical small-defect wake profile.

An additional effect of compressibility will be to produce a non-uniform kinematic viscosity profile across the wake. This will affect the tube mass flows $\dot{m}_i$, which are still given by relation (3), but ν must now be replaced by the local kinematic stagnation viscosity $\nu_o(y)$. Assuming that the dynamic viscosity varies as $\mu \sim T^b \sim h^b$ results in

$$\frac{\nu_e}{\nu_o} = \frac{\mu_e\, \rho_o}{\mu_o\, \rho_e} = \left(\frac{h_e}{h_o}\right)^b \frac{p_o\, h_e}{p_e\, h_o} = \left[\frac{1 + \frac{\gamma-1}{2} M_\infty^2 \left(1 - \frac{u_e^2}{u_\infty^2}\right)}{1 + \frac{\gamma-1}{2} M_\infty^2}\right]^{b+1} \left[\frac{1 + \frac{\gamma-1}{2} M_\infty^2 \left(1 - \frac{u^2(y)}{u_\infty^2}\right)}{1 + \frac{\gamma-1}{2} M_\infty^2}\right]^{-\frac{\gamma}{\gamma-1}} \quad (35)$$

which is suitably approximated by its Taylor series.

$$\frac{\nu_e}{\nu_o(y)} = 1 + \tfrac{\gamma-1}{2} M_\infty^2 \left[-(b+1)\frac{u_e^2}{u_\infty^2} + \frac{\gamma}{\gamma-1} \frac{u^2(y)}{u_\infty^2}\right] + \mathcal{O}(M_\infty^4) \quad (36)$$

$$\simeq 1 + \tfrac{\gamma-1}{2} M_\infty^2 \left[\left(\tfrac{1}{\gamma-1} - b\right) \frac{u_e^2}{u_\infty^2} - \frac{\gamma}{\gamma-1} \frac{u_e^2 - u^2(y)}{u_\infty^2}\right] \quad (37)$$

The viscosity exponent b can be estimated from the accurate Sutherland's viscosity law

$$\mu(T) = \mu_\infty \left(\frac{T}{T_\infty}\right)^{3/2} \frac{T_\infty + T_S}{T + T_S} \quad ; \quad T_S = 110^\circ K \quad (38)$$

by matching its logarithmic derivative at the freestream condition.

$$b = \left.\frac{T}{\mu} \frac{d\mu}{dT}\right|_\infty = \frac{3}{2} - \frac{1}{1 + T_S/T_\infty} \quad (39)$$

The rake tube mass flow relation (3) now becomes

$$\dot{m}_i = \frac{\pi}{128} \frac{d^4}{\nu_e \ell} \frac{\nu_e}{\nu_{oi}} (p_{o_i} - p_r) \quad (40)$$

and the zero net mass flow condition (5) is

$$\sum_{i=1}^{N} \frac{\nu_e}{\nu_{oi}} (p_{o_i} - p_r) = 0 \quad (41)$$

$$\text{or} \quad \sum_{i=1}^{N} \frac{\nu_e}{\nu_{oi}} (p_{o_\infty} - p_{o_i})\, \Delta y = (p_{o_\infty} - p_r) \sum_{i=1}^{N} \frac{\nu_e}{\nu_{oi}} \Delta y \quad (42)$$

The summation on the righthand side produces

$$\sum_{i=1}^{N} \frac{\nu_e}{\nu_{oi}} \Delta y \;\simeq\; L \;+\; \tfrac{\gamma-1}{2} M_\infty^2 \frac{u_e^2}{u_\infty^2} \left[\left(\tfrac{1}{\gamma-1} - b \right) L \;-\; \tfrac{\gamma}{\gamma-1} \left(\theta_k + \delta_k^* \right) \right] \tag{43}$$

where θ_k and δ_k^* are the kinematic thicknesses defined with the density profile omitted.

$$\theta_k + \delta_k^* \;=\; \int \left(1 - \frac{u^2}{u_e^2} \right) dy \tag{44}$$

Substituting the total pressure approximation (34) into equation (42) produces

$$\sum_{i=1}^{N} \frac{\nu_e}{\nu_{oi}} \rho_\infty \left(u_e^2 - u_i^2 \right) \frac{p_{o_\infty}}{p_\infty} \left[1 + \tfrac{\gamma-1}{2} M_\infty^2 \left(\frac{u_e^2}{u_\infty^2} - 1 \right) \right] \Delta y \;=\; 2 \left(p_{o_\infty} - p_r \right) \sum_{i=1}^{N} \frac{\nu_e}{\nu_{oi}} \Delta y \tag{45}$$

which then reduces to the compressible equivalent of equation (12)

$$\begin{aligned} &(\theta_k + \delta_k^*) \left\{ 1 + \tfrac{\gamma-1}{2} M_\infty^2 \left[\frac{u_e^2}{u_\infty^2} \left(\tfrac{\gamma}{\gamma-1} - b \right) - 1 \right] \right\} \\ &\quad = \; 2 \frac{p_{o_\infty} - p_r}{p_{o_\infty}} \frac{u_\infty^2}{u_e^2} \frac{p_\infty}{\rho_\infty u_\infty^2} \left\{ L \;+\; \tfrac{\gamma-1}{2} M_\infty^2 \frac{u_e^2}{u_\infty^2} \left[\left(\tfrac{1}{\gamma-1} - b \right) L \;-\; \tfrac{\gamma}{\gamma-1} \left(\theta_k + \delta_k^* \right) \right] \right\} \end{aligned} \tag{46}$$

where the higher powers of M_∞^2 and $(u_e^2 - u^2)^2$ have been neglected as before. An explicit if somewhat imposing expression for the kinematic momentum+displacement thickness then follows.

$$\theta_k + \delta_k^* \;=\; 2 \frac{p_{o_\infty} - p_r}{p_{o_\infty}} \frac{u_\infty^2}{u_e^2} \frac{p_\infty}{\rho_\infty u_\infty^2} L \frac{1 \;+\; \tfrac{\gamma-1}{2} M_\infty^2 \frac{u_e^2}{u_\infty^2} \left(\tfrac{1}{\gamma-1} - b \right)}{1 \;+\; \tfrac{\gamma-1}{2} M_\infty^2 \left[\frac{u_e^2}{u_\infty^2} \left(\tfrac{\gamma}{\gamma-1} - b \right) \;-\; 1 \right] \;+\; \frac{p_{o_\infty} - p_r}{p_{o_\infty}}} \tag{47}$$

Again, the natural reference pressure for the reservoir transducer is clearly p_{o_∞}. The ratio of static/dynamic pressure in the equation above can of course be replaced by the Mach number

$$\frac{p_\infty}{\rho_\infty u_\infty^2} \;=\; \frac{1}{\gamma M_\infty^2} \tag{48}$$

which is itself obtained from the total/static pressure ratio. The local edge/freestream velocity ratio follows from equation (31)

$$\frac{u_e^2}{u_\infty^2} \;=\; 1 \;-\; \frac{2}{(\gamma-1) M_\infty^2} \left[\left(\frac{p_e}{p_\infty} \right)^{\frac{\gamma-1}{\gamma}} \;-\; 1 \right] \tag{49}$$

Appendix D: Journal of Aircraft Paper

Appendix A: Record of technical exchange via email

Return-Path: drela
To: James Murray <murray@rigel.dfrf.nasa.gov>
Cc: drela
Subject: Re: Apex Program experiment definition
In-Reply-To: Your message of Tue, 01 Feb 94 09:07:41 -0800.
<9402011707.AA00999@rigel.dfrf.nasa.gov>
Date: Tue, 01 Feb 94 16:00:04 EST
From: drela

Jim,

If the balloon can go as high as you want, then the low wing loading and high Mach are not necessarily incompatible -- you just start higher. This will also give a lower Reynolds number. Also, having a higher wing loading will not likely increase or decrease the chances of a successful pullout. Again, all that is affected is the altitude at which you pull out. The Mach number right after pullout will be largely unaffected. The main effect of wing loading will be on Reynolds number after pullout.

As far as the minimum Mach and maximum Reynolds numbers and CL!, needed for validation... this depends on the airfoil among other things. Also, you can't separate the target M, Re, and CL this way. The target is really a surface in M-Re-CL space. Do you have a target airfoil and M-Re-CL baseline to start from? If so, I can tell you which way to go.

You ask about the CL range to test. The reality is that you don't have much say about the CL near the airfoil's ceiling conditions. You have to slow down right up to stall, otherwise you get Mach buffet. Either way, the drag goes way up.

I cannot tell whether the "glove" you are referring to is just an instrumented section of the wing, or a different airfoil section slid over the wing. If you have a "different airfoil" glove, you want to match the glove's circulation (chord x CL) with the neighboring wing. If not, then you will shed vortices from the glove ends and possibly get significant three-dimensionality. Frankly, I wouldn't trust data where the circulation was mismatched by more than 20%, say. Note that if the glove has a bigger chord, then its CL should be LOWER than that of the wing. You mentioned the possibility of using a locally high CL on the glove and moderate CL on the wing. If the glove has a bigger chord, this is a bad idea.

Ideally, the glove has the same chord as the wing, in which case you will match the circulation for any alpha if the zero-lift lines of the wing and glove airfoils line up also.

- Mark

==

Return-Path: drela
To: Robert Geenen <geenen@cs1.dfrf.nasa.gov>
Cc: drela, diesel
Subject: Re: RAK Wind Tunnel Data
In-Reply-To: Your message of Fri, 30 Sep 94 14:53:49 -0700.
<9409302153.AA07595@cs1.dfrf.nasa.gov>
Date: Fri, 30 Sep 94 21:38:00 EDT
From: drela

Bob,

We got your data e-mailing - thanks. I think I'll need the documentation being sent by snail-mail to figure out what it all means.

Re: Airfoil work.
The approach we're taking right now is we're looking at a number of various airfoil design concepts. You never really start "from scratch", since airfoil design is always iterative at some level. I've always had good success in "tweaking" an existing airfoil to a slightly different Mach and or Reynolds number, but it may be that the Kennelly airfoil might be targeted too far from the Re-Mach combinations we're talking about. I'll have to sift though the data to be sure, though.

Re: NS solvers.
The problem with all the present NS solvers I'm aware of is that they do not treat the separation bubbles in an adequate manner. Some sort of amplification-based transition criterion is a must, since TS waves dominate separation bubble behavior, and separation bubbles dominate low-Re airfoil behavior. I don't see fundamental barriers to a proper treatment of separation bubbles in an NS solver, it's just that no one has done it as far as I know. If done right, NS predictions could be quite useful, I think.

The stuff that Dini and Maughmer do addresses the TS transition mechanism reasonably well, but as far as I know, they rely strictly on panel methods and do not deal with transonic flows. Maybe that has changed recently.

- Mark

==

Return-Path: drela
To: Robert Geenen <geenen@cs1.dfrf.nasa.gov>
Cc: murray@wilbur.dfrf.nasa.gov, diesel, drela
Subject: Re: RAK Wind Tunnel Data
In-Reply-To: Your message of Thu, 06 Oct 94 13:58:29 -0700.
<9410062058.AA19467@cs1.dfrf.nasa.gov>

Date: Sun, 09 Oct 94 01:09:40 EDT
From: drela

Bob,

Re: ISES for thick airfoils.
All else being equal, the thickness of the airfoil is not an issue for ISES MSES accuracy. I've done calculations of 50% thick strut fairing sections with reasonable results.

Re: advantages of thicker airfoils.
I've looked closely at thickness/weight tradeoffs for Langford's new long-range Theseus airplane. For a minimum gross takeoff weight, it wanted a very thick (over 20%) airfoil, which would allow a greater span, which allowed less takeoff fuel for a given range requirement. The optimum was extremely flat, though, and the aircraft corresponding to a 15% thick section was just a bit heavier, but also smaller and more practical in that sense.

I expect the tradeoff to be somewhat different for a short range, very high altitude mission like that of Perseus A. The fuel weight doesn't come into it as much, so the advantages of large span are different in nature -- lower max power rather than lower cruise efficiency. I'm not sure yet what effect this has on the "optimum" airfoil. In general, smaller airplanes want thinner airfoils, since the structural constraints are less severe, and the Reynolds number problems are more severe.

- Mark

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Return-Path: diesel
Date: Wed, 19 Oct 1994 10:02:36 -0400
From: diesel (Thomas Washington)
To: drela, geenen@csl.dfrf.nasa.gov

Bob,

The main concern is that if the airfoil is designed to have enough margin to survive the pull-out maneuver the rest of the flight will probably be well within the "safe" operating range. Say you perform a 2g pullout at Cl = 1.2, M = 0.6, when you level off the Cl will have dropped to 0.6 for the same mach number. I dont think these conditions would present valuable data.

I would think that it would be more interesting to begin the flight near the upper performance limit predicted by MSES and probe around in this region. I think that an objective should be to gather as much data at or above this predicted performance limit while the aircraft is near 90-100k ft. As the altitude drops and the atmospheric conditions become normal the data gathered there will cover the "safe" operating range.

In order to do this some sort of lift-augmentation system is necessary to lower the wing loading required during the pull-out.

There is another option which is dependent on the data aquistion system. If some sort of high-speed data aquisition system is employed, data can be collected during the pull-out. The airfoil could be designed for the pull-out maneuver and the data collection runs could consist of a series of dives and recoveries. These runs would consist of data collection at or near Clmax for various mach and Reynolds numbers as the altidude decreases.

This option is based upon the assumtion that a data aquisition system can take "snapshots" of quasi-steady state conditions during the pull-out. I have no idea of what types of systems are available to perform this task.

A Cl of 1.0 at M = 0.65 is a good design maximum for for pull-out. Any higher than that (in terms of M^2.Cl) and it becomes harder to ensure a safe recovery.

TW

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Return-Path: drela
To: Robert Geenen <geenen@csl.dfrf.nasa.gov>
Cc: murray@ra.mit.edu, Alex_Sim@qmgate.dfrf.nasa.gov, drela, diesel
Subject: Re: APEX and Related Questions
In-Reply-To: Your message of Wed, 23 Nov 94 13:10:03 -0800.
<9411232110.AA15265@csl.dfrf.nasa.gov>
Date: Tue, 29 Nov 94 00:13:43 EST
From: drela

Bob,

Here are a few thoughts on the points of your last email.

1) Re: Tail airfoil. I would go with something like a NACA 0010, 1410, or 2410. These work well down to Re = 100000 with no special treatment. A minor redesign can make them effective down to Re = 50000. You easily can count on -0.5 < CL +0.5 from the 0010, with this CL range shifted up or down with the cambered 1410 or 2410.

I don't like to rely on trips or vortex generators, since they are a pain in the neck, and involve some risk in sizing, spacing, and positioning. If they work at low altitudes, it doesn't mean that they will work up high, so there is no way to test them.

2) If you size the tail so that you don't need to exceed CL = + -0.5 on the tail, then separation

is not something to worry about. Incidentally, sailplane tails rarely exceed CL = +/-0.2 in normal operation, but they can go to maybe +/-0.8 in some violent pitch maneuvers. A simple approach would be to just make the tail volume maybe 25% bigger than some typical small sailplane.

For the ailerons, I would go with strongly assymetric deflection, maybe +1/-3, or even +1/-5. With the airfoil barely hanging on in the pullout, any significant downward aileron deflection blows off the boundary layer and nixes any lift addition. Upward deflection is always effective, and produces yaw moment in the right direction.

It might be very productive to build an RC glider model of the APEX to figure out suitable control throws, CG limits, trim settings, adverse yaw, etc. There should be some local RC flyer, inside or outside of NASA, who would drool at building something like this. To be conservative, I would size it to give Re maybe 20% smaller than the real APEX at altitude. Even with no Mach effects, you'll learn quite a bit from it, I think.

3) W/S = 4 psf , 30km gives

		1g	1.5g	2g
M*sqrt(CL)	=	0.48	0.59	0.69
Re*sqrt(CL)	=	200K	245K	280K

Transonic effects start to set in at 1.5g, M*sqrt(CL) ~ 0.6. The airfoil will die above M*sqrt(CL) ~ 0.67 or so, so this is the target range. At the same time, I would like to see Re*sqrt(CL) < 300K if possible. Clearly, for the 3.1 ft chord, W/S = 4.0 is desirable. Higher W/S will tend to increase Re rapidly, not just because of the higher flight speed, but because you will finish the pullout at a lower altitude. Have you done the simulation runs to ascertain the effect of W/S on minimum Re? Maybe going to higher aspect ratio may be a way to get lower Re with a reasonable wing loading. Higher AR will also mean smaller energy loss in the pullout and any high-g turns which may be necessary to drive up M*sqrt(CL) during data acquisition. On the other hand, a higher AR adds weight. I haven't done the numbers to see if the higher AR buys you much overall.

Incidentally, Tom's latest airfoil looks extremely attractive. It's quite different from the Kenelly section, which does not appear to be at all suitable for these Reynolds numbers.

4) The Perseus airfoil was designed for well-subsonic operation (M = 0.35) at 25 km. Since then, the installed power and wing loading have more than doubled! Now, the

wing airfoil will start to run into Mach trouble at around 26.5 km, which is actually a reasonable margin from 25 km. It can't go much above M*sqrt(CL) = 0.60, since it is way overcambered for high-Mach operation. Still, I don't expect this to be a limiting factor for Perseus A. It looks like the things that are more likely to limit Perseus are the prop, available excess power, and/or LOX capacity -- all directly caused by the weight growth.

5) I have a student building a model of the front-mounted radiator airfoil. We hope to be in the tunnel early next year.

Mark

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Return-Path: diesel
Date: Fri, 9 Dec 1994 17:03:32 -0500
From: diesel (Thomas Washington)
To: drela, geenen@csl.dfrf.nasa.gov

What I have looks to be a good baseline airfoil for this mission. It can be used "as is" or with small modifications (camber, thickness...) to adapt to the evolving aircraft.

The section is 13% thick, with about 4% camber. It was designed for the pull-out recovery, (Cl = 1.2-1.3, M = 0.5-0.6 or so... .

The "incompressible" Clmax is around 1.35, This limit is lowered significantly with increasing Mach number to around M*sqrt(Cl) = 0.63, where the boundary layer is blown off the top by the shocks. Its pretty safe to say the upper limit is still M*sqrt(Cl) = .65.

The lower end is up to debate. Presently, the leading edge separates on the lower side at around Cl = 0.5. This can be adjusted depending on aircraft needs.

Unless there are any other requests, this is the airfoil I will be using to collect the data. Going beyond the point-of-no-return should be no problem. Again, just let me know where you need data, and in what form and I can get things running.

TW

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Return-Path: drela
To: Robert Geenen <geenen@csl.dfrf.nasa.gov>
Cc: diesel, drela
Subject: Re: APEX Airfoil
In-Reply-To: Your message of Thu, 15 Dec 94 14:15:58 -0800.
<9412152215.AA17470@csl.dfrf.nasa.gov>
Date: Wed, 21 Dec 94 00:34:26 EST
From: drela

Bob,

Tom left for the holidays just after you sent your last message.

I put the APEX 14 airfoil which Tom sent through a few optimization cycles, making the APEX 16, which is a slight improvement all around. I'd like to make this the "official" current airfoil. Overall, it is quite well-behaved up to M=0.65 (or M*sqrt(CL) = 0.65). It rapidly falls apart after that.

I cranked out almost all the M,Re combinations you requested. I'll send a uuencoded, tar file of the directory which contains all of these polar files. Unpack as before:

```
% uudecode mail_file      it's NOT necessary to remove email lines
% uncompress apex16.tar
% tar xvf apex16.tar
```

The file naming is Ma.Re (.65.1000 is a M=0.65, Re=1000K polar). You can plot any combination of polars using the IRIS executable "pplot" which is in the directory. Just run it -- it's menu-driven. If you are not on an IRIS, I can send the source and plot library for pplot.

I've also included some random *.pos PostScript files. Some of these you can regenerate as needed with pplot. The coordinates are in the blade.apex16 file.

You'll notice that I ran M=0.75 polars instead of the M=0.80 you requested. At M=0.80, the airfoil generally refuses to converge, indicating large-scale unsteadiness (Mach buffet). Looking at the CL-CD polar trend, it also looks like the polar would collapse to near-nothing at M=0.80. For the CW13, it seems this would happen at a somewhat higher Mach, like 0.85. In case you want to compare these results with the tunnel data you sent, keep in mind that those tests had significant 3D relief from the rather low aspect ratio, which reduces the effective Mach number. The higher Mach also causes the effective aspect ratio itself to be reduced, according to the Prandtl-Glauert rule. At M=0.8, the effective AR is 0.6 times the actual AR. The 3D relief effect will not be very significant on the high-AR Apex aircraft, so the 2D polars are much more representative.

You also asked for post-stall polars. Again, I can't get steady solutions once the drag starts to skyrocket. If you need data for higher alphas, I would just extend the last CL to whatever alpha you need. This should fall within the uncertainty and/or unsteadiness band.

- Mark

==

Return-Path: drela
To: Robert Geenen <geenen@os1.dfrf.nasa.gov>
Cc: diesel, drela
Subject: Re: APEX Airfoil
In-Reply-To: Your message of Wed, 21 Dec 94 10:20:06 -0800.

<9412211800.AA06446@os1.dfrf.nasa.gov>
Date: Wed, 21 Dec 94 13:50:55 EST
From: drela

Bob,

I'll send the pplot source code with a makefile. Unpack it as usual and make pplot:

```
% uudecode mail_file
% uncompress pplot.tar
% tar xvf pplot.tar
% cd Pplot
% make pplot
```

The "undercut" on the front lower surface of APEX 16 is just to add loading there -- it's sort of like bottom aft camber but at the front. It increases CLmax and reduces Cm (!). Despite these attractive features, you don't often see this on airfoils since it kills the low-CL end of the polar, but for a HALE application the low-CL end is irrelevant.

If you just use an untapered flat wing, you will get very benign stall characteristics with no special treatment. Just keep in mind that the average CLmax will decrease, since there will be some local CL decrease towards the tip. How much depends on the effective aspect ratio (corrected for Mach number with Prandtl-Glauert). A simple lifting-line solution should give you the local CL pretty accurately.

The roughness waviness is much less significant than on a sailplane -- that's one nice thing about low Reynolds numbers. If you are using sailplane-level technology, like female molds, etc., then waviness is a non-issue.

- Mark

==

Return-Path: drela
To: Robert Geenen <geenen@os1.dfrf.nasa.gov>
Cc: drela, diesel, Alex_Sim@qmgate.mit.edu, bowers@wilbur.mit.edu,
 Elise_Gravance@qmgate.mit.edu
Subject: Re: APEX 16 - Cm
In-Reply-To: Your message of Mon, 09 Jan 95 12:16:37 -0800.
 <9501092016.AA04965@os1.dfrf.nasa.gov>
Date: Mon, 09 Jan 95 16:30:19 EST
From: drela

Bob,

The positive dCm/dalpha is typical for low-Re airfoils, and the transonic flow makes it worse. There's no good way to alleviate this that I'm aware of. Yes, the effect is much smaller at higher Re. You won't see these effects with the Eppler-Somers code, since they do not account for the viscous displacement effect on the pressure distribution -- at least that's my understanding. Viscous displacement is the mechanism responsible for the Cm behavior. MSES does duplicate the Cm behavior seen in Abbott and Doenhoff, for example.

You actually identified a very good point:

Experimentally determining Cm behavior in the high-M low-Re regime is something that perhaps should be one of the goals of APEX. Cm depends on Cp(x) details, which may not be captured well enough in the calculations. Certainly, there is no reason to expect that the Cm behavior will mimic conventional high-M high-Re transonic airfoils, since the shock structures are vastly different. Clearly, this is something you want to measure. However, this will require Cp(x) measurements at a fairly large number of points -- much like pressure-tapped wind tunnel airfoil model.

- Mark

==

Return-Path: drela
To: Robert Geenen <geenen@cs1.dfrf.nasa.gov>
Cc: drela, diesel, bowers@wilbur.dfrf.nasa.gov
Subject: Re: APEX - Update Questions
In-Reply-To: Your message of Wed, 01 Feb 95 10:42:47 -0800.
<9502011842.AA25661@cs1.dfrf.nasa.gov>
Date: Wed, 01 Feb 95 15:26:20 EST
From: drela

Bob,

We haven't had much rain OR snow here. Both Tom and I are cross-country skiers, and we're pissed! Last winter (84" total snowfall) was like paradise. But I digress....

\>1) What's the best way for us to get a copy of MSES? I have an old
\> version of ISES right now, so it's probably time to upgrade.

I can easily send it in one big tar file. If you have Dec, IRIS, HP-9000, or RS-6000 workstations, I expect it to run right out of the box. Suns are almost as easy. Who should I send it to?

Incidentally, the qmgate addresses in the Cc list always bounce for some reason:

Alex_Sim@qmgate.dfrf.nasa.gov
Elise_Gravance@qmgate.dfrf.nasa.gov

\>2 Is it worth the effort to model our control surfaces using MSES? Have
\> you encountered unique problems with deflected control surfaces for
\> the low Reynolds number, high Mach case?

Yes, I expect there will be a very large nonlinearity for positive (aileron down) deflections. You want quite large aileron deflection assymetry, up/down = 3/1 or more. Near CLmax, the down-aileron adds mostly drag (and hence adverse yaw), so there's no point in deflecting it down more than a few degrees. The up-aileron is much more effective.

\>3 Where is the separation point with respect to the transition point?
\> Can we get plots of separation location? I know that the idea is
\> to have transition occur as soon after separation as possible to
\> increase the chances of reattachment. Is that what happens on the
\> APEX 16 airfoil?

At high lift at low Re, there are typically two separation locations:
- laminar separation at the start of the bubble
- turbulent separation near the trailing edge

I don't print these out since they don't tell you very much. A much more relevant quantity is the kinematic shape parameter Hk distribution. The peak value in the separation bubble indicates the magnitude of bubble losses, and the peak value at the trailing edge indicates how close you are to separation, or how "bad" the separation is. The MSES plotter MPLOT gives a plot of Hk vs x.

\>4 Back before Christmas I asked about surface roughness/waviness
\> criteria. Your reply made it sound like low Reynolds number airfoils
\> are not sensitive to surface waviness or roughness. That's seems
\> to go against what I know about transition triggering mechanisms.
\> Could you elaborate. (Part of the reason for this discussion is that
\> we are already talking to wing designers and builders and they are
\> asking what quality they will need to ensure. I've been telling
\> them that typical sailplane construction techniques will be adequate,
\> but I need more detailed info on this topic.)

I didn't mean to say that roughness/waviness is not important for APEX --- just that it's significantly LESS important than on a sailplane. In fact, at the lowest Re = 200K, say, a little bit of strategically-placed roughness or waviness can help, but the appropriate location changes with CL, which is one of the reasons I don't like to rely on BL trips.

I understand that the builders want a definite number for the maximum deviation, but the deviation by itself is meaningless aerodynamically. You also need the wavelength of the deviation. For example, adding 1/10" normal to the surface everywhere will have practically no effect. On the other hand, adding a 1/10" high, 1/2" long bump on the upper surface at 5% chord would be a disaster. The location itself is just as important. That same 1/10" high, 1/2" long bump placed on the bottom surface at 95% chord would be totally innocuous.

There are two approaches you can take here:

a) Expend enormous efforts at quantifying the effects of innacuracy

magnitude, wavelength, and location, and then tell the builders
just what accuracy is needed at each surface location.

b) Use overkill.

I vote for approach b), using the sailplane-level of quality as
an appropriate level of overkill. If there is a significant
cost-quality tradeoff for the molds, I would tell the builders
to sink more money into the front 60% of the upper surface and
around the leading edge. The rest of the airfoil is much less
important.

>5 I was wondering how sensitive MSES is to smoothness of coordinates?
> Could the effect of surface waviness on airfoil performance be
> determined by adding waviness to the coordinates given to MSES?

MSES can predict the effects of long-wavelenth deviations, but not for
for short ones. Very short waves are in effect roughness. I don't
know what the lower limit for the "trusted" wavelength is, so I wouldn't
trust any such study. Part of the problem is that for efficiency reasons,
MSES uses the e^n "envelope method", which is a somewhat simplified form
of the full-blown e^n transition prediction methodology. The envelope
method assumes that the BL evolves in a reasonably gradual manner.
The strong periodic whacks it would get from intentional surface
waves might put its accuracy into question. I've always worried
about this, so a check, I set up MPLOT to show the transition
location you WOULD have gotten from the full-blown e^n method.

>6 The last question I had was about the design philosophy of the APEX16
> airfoil. With no airfoil design experience I was wondering what
> trade-off if any was made in order to ensure the wide, flat drag
> bucket? Is there any possibility that drag could be sacrificed to
> get better Cm characteristics? I think this is a long shot, but it
> never hurts to ask.

Tom might want to add to this, but as I see it, the key problem was
controlling the separation bubbles over the operating range of Mach
numbers. We could have done better at the higher Mach by having
a flatter "rooftop" Cp there, but this causes an LE spike which
kills the upper surface BL at lower Mach. A compromise resulted
the airfoil being "OK" at all Mach numbers, but not the best that
you could do at any specific Mach number. This is typical of any
compromise design, of course. I used the optimizer at the end
just to fine-tune this compromise.

As a separate issue, the reliance on bottom aft loading had to be
reduced, since at low Re the bottom BL couldn't negotiate the high
bottom aft pressure that the Kennelly section has, for example.
Instead, we relied more on front loading, which doesn't have the
low-Re BL separation problem at all. You can't get as much lift
out of the front loading as you can get out of aft loading, but
on the other hand, the front loading permits a nearly all-laminar
bottom surface, which is good news for CD.

- Mark

===

Return-Path: drela
Received: from localhost by henry; (5.65 1.1.8.2/26Apr95-0903PM)
id AA25393; Sat, 22 Apr 1995 20:28:13 -0400
Message-Id: <9504230028.AA25393@henry>
To: Robert Geenen <geenen@osl.dfrf.nasa.gov>
Cc: drela, diesel, bowers@wilbur.dfrf.nasa.gov
Subject: Heat-Exchangers
In-Reply-To: Your message of "Wed, 01 Feb 95 15:26:20 EST."
Date: Sat, 22 Apr 95 20:28:13 -0400
From: drela
X-Mts: smtp

Bob,

I'll send you a more-or-less final version of the
Heat-Exchanger paper, in uuencoded, compressed form.

The bottom line from the wind-tunnel test you guys
paid for is that the front-mounted HX installation
looks quite promising. The data is kind of ragged,
since the wind tunnel model was not very accurate
(undergraduate + foam + fiberglass .NE. no-machining).
Also, the instrumentation was rather low grade,
and tunnel time was too short to permit a thorough
checkout of spanwise uniformity, flow misalignment, etc.
I'll also send a plot of the design geometry and
what was taken off the actual model to give you an
idea of the deviation in geometry. It looks awful,
but the model was fairly uniform spanwise. I ran the
calculations with the model geometry, so the results
are valid for comparison purposes. Incidentally, the
test model geometry is computed to have a significantly
lower CLmax -- by about 10%.

The encouraging news is that the measured drag
of the airfoil HX combo is astoundingly low given
the very large radiator area it's carrying, even
at the rather low Reynolds number.

One important result from this test is that a low
velocity ratio of V1/Vinf = 0.10 is possible at
25 km altitude, which means that getting 0.05 or less
at 10 km (tropopause) should be quite doable. Getting
down to V1/Vinf = 0.05 at 10 km is absolutely essential
if you want to fly significantly above 25 km. Otherwise,
the radiator drag will consume most of your engine power
at ceiling (see Figure 3 in the paper).

This test might suggest some "real" followup tests.
One is to see how small a V1/Vinf can you get versus
Reynolds number. We couldn't get above Re = 450K in our
wimpy tunnel without giving the motor a hernia.
It would be particularly useful to see how small
you can make it at Re = 2.5M (10 km), since this is
a key sizing parameter. Also, having an accurate model
and good instrumentation couldn't hurt either.

\- Mark

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Return-Path: drela
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0803PM)
id AA02597; Thu, 11 May 1995 15:39:25 -0400
Message-Id: <9505111939.AA02597@henry>
To: Robert Geenen <geenen@osl.dfrc.nasa.gov>
Cc: diesel@henry.mit.edu, drela
Subject: Re: APEX Design
In-Reply-To: Your message of "Tue, 09 May 95 10:36:47 PDT."
<9505091736.AA24907@osl.dfrc.nasa.gov>
Date: Thu, 11 May 95 15:39:25 -0400
From: drela
X-Mts: smtp

Bob,

Re: Wing twist.
There are several issues here:

1 Keeping the wing simple and untwisted, so that the mold works for both wings.

2 Getting the full CLmax out of the wing. The rectangular planform will naturally stall at the root first, so you can't get the full CLmax of the airfoil, which lengthens the pullout. This favors washin twist, since this will make the CL more uniform across the span.

3 Getting a uniform CL across the span for 2-D section flow and more reliable measurements. This also favors washin.

4 Reducing induced drag. This favors slight washout.

5 Avoiding tip stall and keeping ailerons effective. Thw untwisted wing shouldn't have any problems here, since the tip CL will already be lower.

Frankly, I don't see any reason to washout the wing, since tip stall is not a problem for a flat wing, and washout will buy you a Cdi reduction of -0.0015 at most. This is in the noise. The 10 degree linear washout you mentioned is HUGE. At M = 0.6, with the root right at stall, I get the following CL variation across the span from my Vortex-Lattice code:

2y/b	CL	-10 linear washout
0.0	1.20	
0.4	0.91	
0.6	0.49	
0.9	0.33	
1.0	0.0	

Avg CL = 0.78 e = 0.953

Assuming CLmax = 1.2, this means you are throwing away 1 - 0.78/1.2 = 35% of the airfoil's maximum lift capability! There is also the problem of the CL changing rapidly across the test section, which will produce significant 3-D effects on the potential flow and possibly on the boundary layers.

A simple flat wing has the following CL variation at M = 0.6:

2y/b	CL	flat wing
0.0	1.20	
0.4	1.16	
0.8	0.99	
0.9	0.78	
1.0	0.0	

Avg CL = 1.06 e = 0.967

This costs you only 1 - 1.06/1.2 = 12% of CLmax, and the CL variation at the test section is more uniform but still not ideal. Note that this has *less* CDi than the 10 deg washout wing, which is way over-twisted.

I would favor a small washin for reasons 2 and 3 above. The following washin distribution looks very good from a strictly aero viewpoint:

y(ft)	alpha(deg)
0	0.0
10	0.3
15	1.6
18	2.8
20	4.0

It gives the following CL variation:

2y/b	CL	+4 gradual washin
0.0	1.20	
0.4	1.20	
0.7	1.17	
0.8	1.11	
0.9	0.95	
1.0	0.0	

Avg CL = 1.12 e = 0.906

This gives the most lift, but more important is the nearly-flat CL at midspan. CDi is bigger, but the penalty is tiny compared to the total drag. Also, CL is lower at the tips despite the washin. Note also that the washin wants to be sharply nonlinear. A linear washin gives the following:

2y/b	CL	+4 linear washin
0.0	1.11	

0.4	1.19
0.7	1.17
0.8	1.09
0.9	0.92
1.0	0.0

Avg CL = 1.10   e = 0.899

This has a CL peak at midpsan rather than at the root,
which may not be too bad.

My feeling is to go for the washin, preferably the gradual one, with maybe a bit less than the full 4 degrees to keep the stability & control folks from gasping. The flat wing is OK if you don't want to make a left and right wing mold, but it's not ideal from the aero measurement viewpoint. Washout serves no purpose that I can see.

If you go for left and right molds, you might as well taper the planform a bit, from 70% span outward, say. With modest taper, zero twist looks appropriate, so the whole washout thing may be a non-issue. Also, taper would also greatly alleviate aeroelastic twisting, which may or may not be a problem.

* * *

Re: Aileron effectiveness.
I think I mentioned this before. I would use strongly assymetric throws, like +1/-5 or even 0/-5. Tom is looking at the effects of aileron deflection to quantify this better.

* * *

Re: Tail airfoil.
I don't think that the APEX 16 is appropriate for the tail. It is very intolerant of downward flap deflection (up elevator), which is a very bad trait for a tail airfoil. Tom can check this out, but I'm certain that a NACA xx10 would be much better, especially with a little tuning for the low Reynolds numbers.

Looking at the APEX PDR document, it struck me how small the tail aspect ratio is. Someone obviously wanted to keep the tail Re high, but that seems a bit extreme. I would think that a somewhat higher tail aspect ratio would have less tail area (and hence less weight) for the same dCm/dalpha. Re=200K or even 150K doesn't scare me a bit if the CL doesn't exceed 0.5, say. Do you know what the tail CL limits are?

* * *

Re: Reporting.
I think that our steady e-mail banter is in the spirit

of "reporting", and is a lot more time-efficient. I'm saving it all and can put it in a handy bound volume for the bureaucrats at both ends if they want it. The tunnel data in the heat-exchanger paper is certainly a progress report of sorts.

I certainly plan to put together a final document of some sort. I wasn't aware that the progress reports go to CASI. I thought that it was just the final report. Frankly, I think that the progress reports would just fill up their file space, since it would all get repeated in the final report anyway. I can certainly organize and send them the interim stuff if they raise a stink, but otherwise it seems like a waste of everyone's time.

\- Mark

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Return-Path: drela
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0303PM)
id AA05299; Sun, 14 May 1995 22:24:00 -0400
Message-Id: <9505150224.AA05299@henry>
To: Robert Geenen <geenen@cs1.dfrc.nasa.gov>
Cc: drela, diesel
Subject: Re: APEX Design
In-Reply-To: Your message of "Fri, 12 May 95 10:57:04 EDT."
<9505121457.AA14115@cs1.dfrc.nasa.gov>
Date: Sun, 14 May 95 22:24:00 -0400
From: drela
X-Mts: smtp

Bob,

A NACA 0012 or 0010 is very hard to beat as a tail airfoil at Re=300K-500K or so. It has all the right qualities -- high CLmax, good tolerance of flap deflection, etc. For lower Re, it can be tweaked a bit to work well down to Re=100K, mainly by shifting the max-thickness point forward more and more as Re is decreased.

For the APEX tail, I was thinking of starting with the NACA 0010 and adding some camber, since I assume your tail CL requirements are not symmetrical. Hence, I looked at the NACA 1410 and 2410 at M=0.6, Re=150K. It turns out that the transonic effects at the ends of the polar have roughly the same effect on Cp(x) as moving the max-thickness point forward at low Mach, so that these airfoils look well-matched to the lower Re at the higher Mach. In fact, the transonic suction side Cp(x) for the 2410 is quite similar to the APEX 16, but looks to be tailored for somewhat lower Re. If you look at the polars, the 1410 and 2410 are doing fine at Re=150K, while the APEX 16 starts to seriously poop out at 200K. Clearly, they are better than the APEX 16 for the tail.

Looking at the polars of the 1410 and 2410, I think you want something midway, i.e. a NACA -1.5 410. This should give you a usable CL range of -0.4 .. 0.6, which hopefully meets the requirements. Note that the APEX 16 won't go much below CL=0, which is another reason not to use it for the tail. I'll make some check-out runs on the NACA sections with the elevator deflected. I'll send the zero-deflection polars immediately.

The only nuisance with the NACA sections is the irregularity in CL alpha around alpha=0, which is due to a large separation bubble sloshing around near the trailing edge and upsetting the Kutta condition. On a 3-D surface this wouldn't be nearly as much of a problem, since the bubble is not likely to sit at the TE all across the span at any given alpha. So in a spanwise-averaged sense, the CL alpha disturbance will be much weaker. Still, it may be a good idea to put a row of turbulator bumps just ahead of the hinge line to kill the bubble and make the elevator behavior more linear and predictable. My aversion to turbulators doesn't apply here, since you're not RELYING on them -- they're just cheap insurance. Also, if they trip the BL too early, it's of no consequence, unlike on the wing airfoil. The turbulator bumps want to be quite large -- at least 1/4" high.

- Mark

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Return-Path: drela
Received: from localhost by henry; (5.65 1.1.8.2/06Apr95-0903PM)
id AA22928; Wed, 7 Jun 1995 17:13:53 -0400
Message-Id: <9506072113.AA22928@henry>
To: Robert Geenen <geenen@cs1.dfrc.nasa.gov>
Cc: diesel@henry.mit.edu, drela@henry.mit.edu
Subject: Re: APEX Design
In-Reply-To: Your message of "Wed, 07 Jun 95 12:47:06 PDT."
<9506071947.AA05622@cs1.dfrc.nasa.gov>
Date: Wed, 07 Jun 95 17:13:52 -0400
From: drela
X-Mts: smtp

Bob,

The NACA 2412 for the tails is OK with me. A 2410 would go to somewhat lower Re, but that's probably not too much of an issue if you use turbulator bumps ahead of the hinge line, as I discussed earlier.

I'm not so sure about the wing fences. I don't think you want them ahead of mid-chord, since the extra-slow fluid in the fence-wing corner will separate before the wing airfoil and possibly muck up the wing's BL at the test station. Very minimal fences between x/c = 0.6 .. 1.0 should be OK to prevent any separated fluid from the wing root migrating out across the span. My first guess is to start the fence at x/c = 0.6, extend it aft tangent

to the airfoil surface, ending up at about y/c = 0.07 at the trailing edge, and extending back to x/c = 1.05 or 1.10 . The bottom fence needs to be only about 1/2 the height of the top fence at most.

Yes, hatches should go on the bottom of the wing, and the top of the tail. I don't see any problems with bellcranks sticking out on either side, as long as they are not immediately next to the test section.

I'll be at Boeing from June 19 until August 11. I'll be checking my MIT email whenever possible, but I won't have easy direct access to my machine. If there is anything you want me to calculate before I leave, let me know ASAP. Tom is preparing to fly jets for the USAF, so he won't be around at MIT for the summer either. I'll be back at MIT from September on.

Mark

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Return-Path: drela
Received: from localhost by henry; (5.65 1.1.8.2/06Apr95-0903PM)
id AA19059; Tue, 29 Aug 1995 15:19:09 -0400
Message-Id: <9508291919.AA19059@henry>
To: Robert Geenen <geenen@cs1.dfrc.nasa.gov>
Cc: drela@henry.mit.edu
Date: Tue, 29 Aug 95 15:19:08 -0400
From: drela
X-Mts: smtp

Bob,

This is to let you know that I'm back at MIT. Is there anything you need me to do right now? I've scanned the APEX Engineering Meeting Summaries to date, so I kind of know what's going on.

One thing that I see that needs serious attention is the wake rake setup. I think it is vitally important that measurements be done at various spanwise locations, if only at a few selected operating points. This is to make sure that there is no significant spanwise variation. Ideally, you want to measure the Cd as a spanwise average across a significant portion of the wing -- maybe with a number of rakes, or a translating rake on a leadscrew.

I've seen rapid wake Cd variations as much as +-20% across the span of a supposedly 2-D model setup, so that the averaging can be very important. Note that even such strong spanwise variation doesn't imply that the 2D-ness of the flow is strongly compromised. There is very little resistance to the slow BL fluid from migrating slightly along the span, and even slight variations in spanwise migration add up to a lot at some distance downstream.

The total spanwise-averaged Cd is unaffected by this migration, but you can get very misleading results if you measure at only one spanwise point.

Since our last exchange, I've reconsidered the fences, and now I think that the fences may actually _aggravate_ the spanwise nonuniformity in Cd rather than help. If there is a general tendency for the BL to creep outboard everywhere, say, it will "pile up" against the outer fence and have a higher measured Cd there. Likewise, it will thin out at the inner fence. If you leave off the fences, you will avoid this piling up and thinning out, and quite likely get a more uniform measured Cd. Is there any flight experience with gloves on wing fences? Have they been demonstrated to improve the data? If not, I would leave them off.

- Mark

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```
Return-Path: drela
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0903PM)
id AA19831; Tue, 29 Aug 1995 21:42:41 -0400
Message-Id: <9508300142.AA19831@henry>
To: Robert Geenen <geenen@cs1.dfrc.nasa.gov>
Cc: drela@henry.MIT.EDU, bowers@rigel.dfrc.nasa.gov
Subject: Fences, Rakes, etc.
In-Reply-To: Your message of "Tue, 29 Aug 95 15:30:27 PDT."
             <9508292230.AA17765@cs1.dfrc.nasa.gov>
Date: Tue, 29 Aug 95 21:42:41 -0400
From: drela
X-Mts: smtp
```

Bob,

Instead of the sheet metal fences to hold the rake support, you could just go with thickwall tubes mounted to the bottom of the wing with brackets. The tubes can carry the pressure tubing or wires, depending on where you put the transducers or scanivalves.

The +/- 3 inches of rake travel you mention seems too small to me. I would go for at least +- 1 foot, so that you cover at least a chord width. I see two ways to go here:

1 Use 1 rake with a traverse mechanism as you suggested.
2 Use several fixed rakes.

Option 1 obviously requires less data channels, but option 2 has almost everything else going for it:

* Mechanical simplicity and reliability.
This is obviously important with the small number of APEX flights envisioned. Also, it's hard for me to see how the pressure tubing could allow for the rake motion without having a flexed tubing bundle flapping in the breeze and shaking everything.

* Shorter development and construction time.
I've built a number of wake rakes. They are trivial compared to a motor traverse mechanism. Also, once the milling machine is set up or programmed for drilling the rake body, building several rakes doesn't take much additional time. Since APEX is behind schedule I gather, the faster you can build the rake system the better.

* Simultaneous spanwise measurements.
This means that you don't have to slew the rake, wait for the pressures to settle, and then make another spanwise measurement. Each operating condition can therefore be shorter, so that you'll be able to do more operating points per flight. It's hard to argue against having data from more operating points from any given flight!

Obviously, I'm in favor of the multiple rakes. I'm also willing to do a rough sizing and mechanical design for the system as you suggested. I would need to know the specs on the pressure transducer/scanivalve system you will be using, number of channels allotted, etc. I would also need the structural X-section of the wing, so that I can figure out how to mount the whole thing.

When is the CDR ?

- Mark

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Return-Path: drela@henry.MIT.EDU
Received: from PACIFIC-CARRIER-ANNEX.MIT.EDU by henry; (5.65/1.1.8.2/06Apr95-090
id AA17241; Wed, 18 Oct 1995 17:40:50 -0400
Received: from HENRY.MIT.EDU by MIT.EDU with SMTP
id AA21798; Wed, 18 Oct 95 17:40:46 EDT
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0903PM)
id AA17211; Wed, 18 Oct 1995 17:40:43 -0400
Message-Id: <9510182140.AA17211@henry>
To: Al Bowers <bowers@wilbur.dfrc.nasa.gov>
Cc: drela@MIT.EDU
Subject: Re: APEX wake rakes...
In-Reply-To: Your message of "Wed, 18 Oct 95 11:10:34 PDT."
             <9510181810.AA08292@wilbur.dfrc.nasa.gov>
Date: Wed, 18 Oct 95 17:40:43 -0400
From: drela@henry.MIT.EDU
X-Mts: smtp
```

Al,

I agree with Dan Somers... I don't think that there's any danger of Goertler vortices forming. However, any spanwise variations are more likely to be caused by the slight 3D-ness of the wing, the pressure ports, rake mounts, etc. The chance of spanwise variation is not large, but it can't be ruled out based on prior high-Re glove experience. The presence of large separation bubbles makes this section MUCH more susceptible to slight spanwise-flow inducing perturbations.

In any case, I have an idea: I propose we use one fixed dense rake to measure the wake profile, and add several fixed integrating rakes on either side to check for non-uniformity. As you may know, an integrating rake measures the sum (delta* + theta). It has some uncertainty depending on what you assume for the flow in the pressure tubes and reservoir, but this uncertainty should be the same for all the integrating rakes, so for _comparative_ measurements they should be very good. It is essential that all the integrating rakes are essentially identical, but that shouldn't be a problem. Of course, the big advantage is that an integrating rake requires only one channel.

If this sounds OK to you, I can lay out both the real rake and the integrating rake geometry. What is your current estimate of the number of ports available for all the rakes? This affects how much sweat I have to expend to position the rake tubes properly. Also, have you people ever worked with integrating rakes? If not, I can give a few pointers.

- Mark

==

Return-Path: drela
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0903PM)
id AA20959; Fri, 20 Oct 1995 16:00:36 -0400
Message-Id: <9510202000.AA20959@henry>
To: Al Bowers <bowers@wilbur.dfrc.nasa.gov>
Cc: drela@henry.MIT.EDU
Subject: Re: APEX wake rakes...
In-Reply-To: Your message of "Fri, 20 Oct 95 09:55:59 PDT."
<9510201655.AA01476@wilbur.dfrc.nasa.gov>
Date: Fri, 20 Oct 95 16:00:35 -0400
From: drela
X-Mts: smtp

OK, let's go for the integrating rakes, then. I don't have any detailed references on them, but I've analyzed them and it's not too hard to see what is required for them to work as intended:

1) Some of the tubes have flow going in, and some have it going out (obviously). For the whole rake to work as expected, the pressure drop across each tube must be proportional to the mass flow through the tube. This means that each tube must have fully-developed laminar (Poiseuille) flow over most of its length, with the flow velocity << V_infinity. It is also necessary that the end of each tube sees the full local stagnation pressure. Both of these requirements demand that the tubes be sufficiently long and thin.

2) The length of each tube must be inversely proportional to the local tube spacing. Of course, the simplest thing is to use evenly-spaced equal-length tubes.

3) The reservoir into which the tubes dump into must be sufficiently large to have negligible velocities along its length.

If these conditions hold, then the pressure in the rake reservoir is

```
                                 2
p_rake = p_static + 0.5 rho Ue  [ 1 - (delta* + theta)/L ]
```

where 0.5 rho Ue^2 is the local dynamic pressure just outside the wake, and L is the height of the rake.

I would reference all rake Scanivalve pressures to freestream p_total. With a regular or integrating rake wake the thing you're after is deviations from p_total, so that this is the logical reference. For example, measuring p_rake - p_static gives you 1 + (delta** + theta)/L , while measuring p_rake - p_total gives you (delta** + theta)/L directly. The latter quantity can be quite small relative to 1, so you want to measure it directly if at all possible.

The wing surface pressures may be best referenced to freestream static, although here it doesn't matter so much, since the pressure deviations are larger. Using p_total is always "safer", I think, since it is much easier to measure reliably.

I propose the following setup.

One "master" p_total for referencing all Scanivalves. This can tap off the air-data probe, although you may want a separate probe to decouple the instrumentation from the aircraft flight-control systems.

One p_static referenced to master p_total from a trailing probe or whatever to get pitot pressure. This is then slightly corrected for Mach to get true dynamic pressure 0.5 rho V^2.

Another p_static referenced to vacuum for pressure-altitude and Mach number measurement. This is a normal altimeter I assume?

The dense rake should have its own local static probes in addition to the usual total tubes (all referenced to the master p_total). Ideally, you want one static port above and below the wake.

The integrating rakes have only one pressure value
referenced to master p_total.

On-board thermister placed in a bleeding total probe
to get T_total and hence density and viscosity.
This is necessary only for Reynolds number and
true airspeed.

The 26 dense rake ports are barely enough, but should
be OK with intelligent tube spacing. If the scheme
above is OK, I'll lay out the rake geometry ASAP.

- Mark

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Return-Path: drela
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0903PM)
id AA22374; Mon, 4 Dec 1995 00:39:36 -0500
Message-Id: <9512040539.AA22374@henry>
To: Al Bowers <bowers@wilbur.dfrc.nasa.gov>
Cc: drela@henry.mit.edu
Subject: Re: DFRC mtg?
In-Reply-To: Your message of "Tue, 26 Nov 95 14:23:44 PST."
<9511262223.AA02933@wilbur.dfrc.nasa.gov>
Date: Mon, 04 Dec 95 00:39:36 -0500
From: drela
X-Mts: smtp

Al,

I've got a reasonable estimate for the rake location
and size:

$x/c = 1.30$ (TE is at $x/c = 1.0$)

$y/c = -0.05 \ldots 0.10$ (dense ports)
$y/c = -0.10 \ldots 0.20$ (sparse ports)

The actual wake thickness at that x/c location might
be anywhere from delta(y/c) = 0.05 to 0.10, but it
moves up and down with alpha. The "dense" y/c range
above will catch it over all reasonable operating points,
and that's where we want a relatively dense set of
rake ports (that's what I mean by dense). The sparse
port set above and below the dense range is mainly to
make sure that all the wake is covered. In the
higher-Mach operating points, there is also a weak shock
wake which goes up to about $y/c = 0.30$, but this has
a very small contribution to the total momentum defect.
The delta(Cpo) in this shock wake is less than 0.004,
which is probably too small to measure reliably anyway.

I would make the integrating rakes cover the range

$y/c = -0.075 \ldots 0.125$

These must have uniform port tube spacing. I think
20 tubes would be more than enough.

- Mark

==

Return-Path: drela
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0903PM)
id AA09363; Sun, 10 Dec 1995 22:16:47 -0500
Message-Id: <9512110316.AA09363@henry>
To: Al Bowers <bowers@wilbur.dfrc.nasa.gov>
Cc: drela@henry.MIT.EDU
Subject: Re: DFRC mtg?
In-Reply-To: Your message of "Fri, 08 Dec 95 13:26:23 PST."
<9512082126.AA19905@wilbur.dfrc.nasa.gov>
Date: Sun, 10 Dec 95 22:16:46 -0500
From: drela
X-Mts: smtp

Al,

The only drawback to having the rake too far forward
is that there will be a significant static pressure
variation across the wake top to bottom. This makes
backing out the data kind of tricky. You definitely
want two static ports on the rake -- one just above the
top tube and one just below the bottom tube. The static
pressure is assumed to vary linearly in-between, but this
gets rather dubious close to the trailing edge.

Still, the $x/c = 1.3$ location is fairly conservative.
You can move it forward to $x/c = 1.2$ if it makes things
easier. The rake y/c dimensions and location should
be about the same at that location.

Yesterday we had all sorts of rain coming down all day,
so I wrote up

Mark's Compleat Guide to Integrating Rakes

Somehow it ballooned to 7 pages after I got really going.
The compressibility-correction stuff got really ugly.
Anyway, I'll send it separately in PostScript form.
I would only seriously look at Sections 1 and 3.
Sections 2 and 4 are only important if you need
absolute rather than relative measurements, which is
not the case hare. The bottom line that we're interested
in is equation (12). Equations (24) and (27) are the
relevant sizing criteria you should consider.

- Mark

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Return-Path: drela
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0903PM)
id AA12669; Mon, 11 Dec 1995 17:14:32 -0500
Message-Id: <9512112214.AA12669@henry>
To: Al Bowers <bowers@wilbur.dfrc.nasa.gov>
Cc: drela@henry.MIT.EDU
Subject: Re: argh!
In-Reply-To: Your message of "Mon, 11 Dec 95 11:01:01 PST."
<9512111901.AA23385@wilbur.dfrc.nasa.gov>
Date: Mon, 11 Dec 95 17:14:32 -0500
From: drela
X-Mts: smtp

Al,

I laid out a reasonable rake geometry for 26 tubes,
for x/c = 1.2 or 1.3. I'll send a full-size PostScript
drawing of it separately (my brain-damaged email handler
can't insert files). You'll notice that I divided up the
y-interval into port intervals of varying widths, and then
placed a tube at the center of each interval. This allows
the simplest-possible midpoint-rule integration with each
port simply weighed by its interval width:

```
                 N
 Integral = Sum  ( ... p ...)  delta(y)
                i=1       i             i
```

Besides the normal-pressure gradients I discussed previously,
another argument against placing the rake too close to the
trailing edge is that the wake centerline velocity is quite
close to zero there, at least for this airfoil. This means
that the total pressure signature there will be very small,
and you won't be able to measure the local momentum
defect very accurately. For example, at one particular
operating point of interest (Re=200K, M=0.65, CL=1.1),
the wake centerline velocity at x/c = 1.1 is u/ue = 0.1 .
The rake tube at that location will see a total pressure of

Po = Pe + 0.01 (0.5 rho Ue^2)

which is darn close to Po = Pe. In other words, it's
very hard to measure the difference between u/ue = 0.1
and u/ue = 0.0 , but this difference is quite significant
to the integrated momentum thickness and drag. You want
to measure the wake after its minimum velocities have
reached reasonable levels -- at least u/ue = 0.2 or more.

Remember that the wake thickness on this airfoil is several
times larger than on typical high-Re airfoils. This means that
you have to go back proportionately farther back to allow the

wake to relax reasonably well. If you normally place rakes at
x/c = 1.05, then this airfoil would call for x/c = 1.15 or more.

Putting the integrating rakes at the trailing edge would be OK
in theory, but again you have the problem of negligibly-small
velocities giving poor accuracy. The integrating rake does
"see" such low-velocity regions quite well, but this contributes
into delta* much more than theta. So the sum theta + delta*
is accurate, but you can't be sure that theta itself is captured
well enough, since this may be masked by delta*. And theta is
what counts, ultimately. Also, it would be nice to have the
integrating rakes at the same x/c as the drag rake, so they can
be compared. Is it a problem to mount the integrating rakes
on stings cantilevered from the trailing edge? It seems this
shouldn't be that hard to do.

- Mark

==

Return-Path: drela@henry.MIT.EDU
Received: from PACIFIC-CARRIER-ANNEX.MIT.EDU by henry; (5.65/1.1.8.2/06Apr95-0903PM)
id AA01399; Fri, 19 Jan 1996 22:57:39 -0500
Received: from HENRY.MIT.EDU by MIT.EDU with SMTP
id AA10581; Fri, 19 Jan 96 22:57:27 EST
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0903PM)
id AA02005; Fri, 19 Jan 1996 22:56:47 -0500
Message-Id: <9601200356.AA02005@henry>
To: Jeff_Bauer@qmgate.dfrc.nasa.gov
Cc: bowers@wilbur.dfrc.nasa.gov, drela@MIT.EDU
Subject: Integrating rake
Date: Fri, 19 Jan 96 22:56:46 -0500
From: drela@henry.MIT.EDU
X-Mts: smtp

Jeff,

The January 8 APEX Engineering Summary sez:

"Jeff Bauer: Mark Drela will be defining the rake..."

I just want to let you know that I've already sent
a full-size layout of the wake rake to Al Bowers.
I also sent Al a writup on sizing of the integrating
rake tubes, etc.

Since I don't see Al listed in the Summary, I assume
that he wasn't at the meeting, so I'm letting you know
directly. If what I sent isn't enough, lemme know.

- Mark

==

Return-Path: drela
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0903PM)

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id AA31671; Sun, 4 Feb 1996 12:53:54 -0500
Message-Id: <9602041753.AA31671@henry>
To: Al Bowers <bowers@wilbur.dfrc.nasa.gov>
Cc: drela@henry.mit.edu
Subject: Re: APEX PDR...
In-Reply-To: Your message of "Thu, 01 Feb 96 12:51:06 PST."
             <9602012051.AA12315@wilbur.dfrc.nasa.gov>
Date: Sun, 04 Feb 96 12:53:53 -0500
From: drela
X-Mts: smtp
```

Al,

Re: Your questions...

>1 they have located a GPS antenna near the spar on the upper surface
>at about mid-span (on both wings).

UGH!!!

This is VERY serious. I would be very weary of putting _anything_ on the front upper surface of the airfoil. At low Re, near CLmax, it takes a very small perturbation to really muck up the flow. I have firsthand experience in this...

On our Monarch HPA, we had a strut protruding from the top of the 3-foot chord wing at 25%. The strut was airfoil shaped, and was a mere 6" chord and 1" thick. Tufts showed that it caused a separation wedge which was 2 feet wide at the trailing edge! I expect APEX to be even more prone to this because the transonic flow on top makes all blockage disturbances propagate sideways along the span with little attenuation. If the antennas stay where they are, they will seriously taint any data you get from the instrument station. Moving them to the tips would be a good solution.

>2) the builder is making up the wing skins with a lower surface that
>wraps forward over the leading edge, and then has a slip joint to the
>upper surface.

I would prefer the seam to be on the lower surface, but I don't think that this is that big a deal. At significant Mach numbers, The upper flow forward of 5% is still strongly favorable, even at high CL. If it will hold things up or cost significantly more money, I wouldn't move the seam.

>3 we talked sometime ago about deleteing the wing fences as a way to
>hang the rakes and their associated support structure. I wanted to
>clarify that, indeed, we do not want any fences. how about lower
>surface fences only? Most of our experiment is on the upper surface,
>and that would still give adequate structure to hang the hardware off of.

Yes, I now think that fences are too risky. Since there will be some spanwise CL variation due to the untwisted rectangular wing, I expect that there will be some spanwise flow near the trailing edge and maybe in the bubble. A constant spanwise flow in itself has little effect on the 2-D characteristics (infinite swept theory) -- what matters is the spanwise _gradient_ of the spanwise flow, which a fence would aggravate. In other words, if there is any spanwise flow, you want it to just let it occur uniformly across the test section without trying to block it with fences. If you do block it, it will just pile up against the fence and artificially thicken the BL there. At the "upstream" fence the BL will thin out, which is equally bad.

I would go with the bottom-mounted fences. These don't have to be thin, floppy sheet metal, by the way. A thin version of a jetliner flap-track fairing would be OK. Even an open tube would work fine, and would certainly be lighter than anything else. You could mount a permanent half-circle cradle on the wing bottom. The tube could just bolt down on this, and thus give a simple means to relocate the rake's fore-and-aft position if this proves necessary.

>From all the computational and experimental evidence
I've seen, putting such junk on the aft bottom of an airfoil has little measurable effect anywhere else.

\- Mark

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```
Return-Path: drela@henry.MIT.EDU
Received: from PACIFIC-CARRIER-ANNEX.MIT.EDU by henry; (5.65 1.1.8.2 06Apr95-090
id AA01826; Tue, 6 Feb 1996 16:10:43 -0500
Received: from HENRY.MIT.EDU by MIT.EDU with SMTP
id AA16393; Tue, 6 Feb 96 16:10:19 EST
Received: from localhost by henry; (5.65 1.1.8.2/06Apr95-0903PM
id AA00850; Tue, 6 Feb 1996 16:10:28 -0500
Message-Id: <9602062110.AA00850@henry>
To: Al Bowers <bowers@wilbur.dfrc.nasa.gov>
Cc: drela@MIT.EDU
Subject: Stanton tubes, mics, etc.
In-Reply-To: Your message of "Mon, 05 Feb 96 10:28:51 PST."
             <9602051828.AA17587@wilbur.dfrc.nasa.gov>
Date: Tue, 06 Feb 96 16:10:28 -0500
From: drela@henry.MIT.EDU
X-Mts: smtp
```

Al,

If I had a choice, I'd be more inclined to go for the mics rather than the Stanton tubes. The problem with a Stanton tube is that to extract Cf from it requires the assumption that you have a standard turbulent BL with a normal-looking log layer. At our Reynolds numbers, however, the log layer is not very distinct, especially since the BL is stressed so hard. Also, knowing the Cf precisely wouldn't help all that much in figuring out the behavior of the BL in the pressure-recovery region. The BL dynamics there are dominated by pressure gradients and shear stresses in the outer part of the BL. The wall shear just doesn't have much to say. I'd say having 5 Stanton tubes would be more than adequate to give a broad-brush indication of what's happening near the wall. As a first cut, I would put them at

40, 50, 60, 70, 80, 90%.

The good thing about the mics is that they specifically give the data which is of interest: TS wave development in the laminar BL and the front part of the bubble, and also the location of transition. It is these things which have the greatest uncertainty in the codes.
I would put the mics in the following % chord locations:

25 30 35 40 45 50 52 54 56 58 60 62 64 66 68 70 75 80

We can fine-tune this if you have more/less ports available.

The dense mic region should pinpoint transition fairly well. This should be visible on the Cp(x) curve as well. Speaking of which, I hope you'll have a reasonable Cp port density over 50-70%. That's where most of the action will take place.

Finally, all the Stanton tubes, mics, and Cp ports should not be placed upstream of the rake(s). Do you have enough room for everything?

- Mark

===

Return-Path: drela
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0903PM)
id AA06415; Fri, 9 Feb 1996 00:17:55 -0500
Message-Id: <9602090517.AA06415@henry>
To: Al Bowers <bowers@wilbur.dfrc.nasa.gov>
Cc: drela@henry.MIT.EDU, drela
Subject: Re: one last time...
In-Reply-To: Your message of "Thu, 08 Feb 96 13:11:14 PST."
<9602082111.AA25430@wilbur.dfrc.nasa.gov>
Date: Fri, 09 Feb 96 00:17:55 -0500
From: drela
X-Mts: smtp

Al,

The instrumentation specs look mostly OK.
Here are some additions and possible mods:

The drag rake will need two static ports, roughly at the top & bottom of the "fine" region. These should also be offset sideways by 1" or so to avoid interference from the total tubes. The streamwise location of the actual port holes on the static probes should be at the same x/c as the tips of the total tubes.
The average of these two static ports will give the local velocity at the wake rake, and the difference will give the transverse pressure gradient and hence the flow curvature.

It seems that the 30 deg wedge exclusion zones may cause a shortage of real estate to place everything. If that's a problem, you may want to move the rakes forward to x/c = 1.0 . I wouldn't go any farther forward, though.

I'll send a PostScript sketch of the layout I have in mind. The Stanton tubes are the lowest priority, and they are also the most intrusive, so I would place these farthest out.

Note that I'm not counting the pressure ports ahead of 25% chord in the 30 deg rake exclusion zone. This should be OK, since the BL is stable ahead of that location, and the ports will not precipitate a disturbance wedge. This allows a tighter setup.

You have a Cp port at 100% chord on the upper surface? If it makes things easier, this port can face straight back rather than upward. Knowing the TE base pressure would be useful.

Some observations:

Overall, it seems that hanging all the stuff behind the wing will be difficult. You have to hammer into the instrumentation guys the importance of keeping everything light, otherwise there may be a flutter risk. Of course, you can always dump lead into the leading edge to compensate, but that is a last resort.

It seems the crossbar for mounting the rakes will be difficult to make light, stiff, AND with a small frontal area. Ideally, it would be a streamlined carbon fiber tube. We've built many such tubes for our hydrofoil boat. The procedure is to take a thinwall aluminum tube (0.015" wall or less), flatten it into a suitable ellipse, wrap it with prepreg carbon, wrap it with heat shrink tape, and cook it. When done, you pour in pool acid to dissolve the aluminum mandrel.
This is much easier than it sounds, and the result is an outrageously strong, stiff, and light streamlined crossbar. I think the Aerovironment people could crank one out in no time. Alternatively, you can just use

a flattened aluminum tube, but this will be about 3 x heavier for the same stiffness. I think you can buy "streamline tubing" from Aircraft Spruce or whatever, but I don't know if they have the appropriate sizes. It may also be crud aluminum rather than some good hard alloy like 2024.

One potential problem that a metal crossbar will have is the thermal-expansion mismatch with the wing. In contrast, a carbon crossbar would be matched very well.

I would most likely go with aluminum tubes for the mounting stings. They don't have to be all that small -- up to 1.25" diameter is probably OK. This way you can use a thin wall and save weight.

Enough rambling for now.

Tell Tony that I already surfed his Web page.

- Mark

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```
Return-Path: drela
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id AA06705; Fri, 9 Feb 1996 10:50:00 -0500
Message-Id: <9602091550.AA06705@henry>
To: Al Bowers <bowers@wilbur.dfrc.nasa.gov>
Cc: drela@henry.MIT.EDU
Subject: Re: one last time...
In-Reply-To: Your message of "Thu, 08 Feb 96 13:11:14 PST."
             <9602082111.AA25430@wilbur.dfrc.nasa.gov>
Date: Fri, 09 Feb 96 10:50:00 -0500
From: drela
X-Mts: smtp
```

Al,

I've made two alternative instrumentation layouts which are more compact than Layout I. I think you should remove the requirement of not having anything in front of the integrating rakes. In fact, it would be beneficial to see if there actually is any disturbance from the pressure taps or whatever. An integrating rake is a cheap way to do this.

I like Layout II the most. Note that it has 4 I-rakes instead of 3. If this is a major problem, then Layout III is passable. It would be nice to have that extra I-rake outside of the pressure-tap spanwise station, though.

- Mark

==

```
Return-Path: drela
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0903PM)
id AA09099; Tue, 5 Mar 1996 10:04:06 -0500
Message-Id: <9603051504.AA09099@henry>
To: Al Bowers <bowers@wilbur.dfrc.nasa.gov>
Cc: drela@henry.mit.edu, jeff_bauer@qmgate.wilbur.dfrc.nasa.gov
Subject: Cf
In-Reply-To: Your message of "Fri, 01 Mar 96 14:49:30 PST."
             <9603012249.AA26546@wilbur.dfrc.nasa.gov>
Date: Tue, 05 Mar 96 10:04:05 -0500
From: drela
X-Mts: smtp
```

>I ran some numbers for max and min diameter of the
>Preston tubes. The max diameter is about 0.0015c
>and the min diameter is about 0.08c.

Well, shewt.

This leads me to reconsider the whole reason for measuring Cf in the first place -- it is one of the terms in the von Karman momentum equation that makes the BL grow:

```
 d(ln theta)       Cf                   2   d(ln Ue)
 -----------  =   --   -   (H + 2 - Me )   --------
     dx            2                          dx
```

It turns out that on the back 50% of the upper surface, the Cf term is fairly minor compared to the d(ln Ue)/dx (pressure gradient) term. At x/c = 0.75, the equation balance is roughly

```
      100%  =  25%  +  75%     at Re = 300K,

and   100%  =  10%  +  90%     at Re = 200K.
```

For a "normal" high-Re airfoil in cruise, it might be 100% = 70% + 30% instead. One logical conclusion is that for APEX it is more important to measure (H + 2 - Me^2) rather than Cf.

The other reason for measuring Cf is to diagnose separation. But again, I think H is a much better indicator of separation than Cf.

Let me offer a crazy idea for a fall-back if the optical gizmos don't work out:

Instead of the Cf gauges, we use minature integrating rakes.

These effectively measure H + 1, which is enough to get the whole term -- note that Ue and Me are known from Cp. Overall, I would much rather see H(x) than Cf(x). You can always closely estimate Cf from H, but not the other way around, since the Cf(H) relation is quite flat in strong adverse pressure gradients.

To put it another way: A Preston tube is a sort of a crude integrating "rake" over the lower part of the BL, and a Stanton tube goes lower still. Near separation, the lower parts of the BL are not all that dynamically important, since most of the action happens higher up - hence the rationale for the I-rakes.

A potential problem is that the I-rake reseirvoirs will cause a large separation behind them. This is clearly a risk, but I think that this can be minimized by using a minimal rake reservoir tube and flattening it. I would also place them at x/c = 0.60, 0.70, 0.80, 0.90, 1.00. I-rakes at x/c = 0.40, 0.50 would carry the greatest separation risk by far, so I would leave these off.

In any case, the I-rake in the wake will quantify any problem with separation caused by the surface I-rakes.

- Mark

===

Return-Path: drela
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0903PM)
id AA25973; Tue, 5 Mar 1996 17:39:50 -0500
Message-Id: <9603052239.AA25973@henry>
To: Al Bowers <bowers@wilbur.dfrc.nasa.gov>
Cc: drela@henry.MIT.EDU, jeff_bauer@wilbur.dfrc.nasa.gov
Subject: Re: Cf
In-Reply-To: Your message of "Tue, 05 Mar 96 10:22:43 PST."
<9603051822.AA04965@wilbur.dfrc.nasa.gov>
Date: Tue, 05 Mar 96 17:39:50 -0500
From: drela
X-Mts: smtp

Al,

"H" is the shape parameter delta*/theta, not the BL height. The integrating rake measures the sum delta* + theta, which is equivalent to

delta* + theta = (H + 1) x theta

so in a way it measures H + 1.

>But even if we have really well calibrated Preston Stanton tubes, I
>anticipate that near seperated regions, we will see very sluggish
>flows and have similar problems measuring pressures as if we had the
>drag rake too close to the TE.

Nope. Different problem. In the wake you only want theta, since that uniquely determines profile drag. The delta* part is a "contaminant" which is biggest where there is lots of sluggish flow in the BL or wake. The farther back you go, the less sluggish flow there is, and the less delta* you have to correct for.

BUT...

On the surface, it is mainly the _delta*_ that you want. The more sluggish the flow the more effective the I-rake measurement will be!

>Would you want a
>total tube at the tip to be sure that we truely did achieve free
>stream flow and the B-L is entirely embedded in the rake profile? My
>gut feel is that you would indeed like to have these things, but the
>totals might be a bit problematic. Or maybe not...

Having a separate total tube at the tip might be a good idea, but if you went to the trouble of having the extra data port, I would prefer two stacked I rakes -- one for the lower and one for the upper BL halves. Having the two readings would let you deduce what the BL is doing with more certainty. You could still measure the sum delta* + theta from the average of the two pressures, but from the difference of the pressures you could estimate the difference delta* - theta, which is also significant.

If you go with just one singe I-rake, it will just need to be taller than the largest anticipated BL thickness. I can estimate the required heights for any given condition. One hassle is that the height of the five I-rakes at x/c = 0.6, 0.7... will most likely have to be significantly different. This will preclude mass-production, but that may not be a big deal.

Do you guys have a latest estimate of the anticipated Mach/Re/CL combos you will be testing at. I can't estimate this since I don't have the latest estimates of vehicle weight and the planned test altitudes.

- Mark

===

Return-Path: drela
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0903PM)
id AA29820; Wed, 17 Apr 1996 11:44:49 -0400
Message-Id: <9604171544.AA29820@henry>
To: drela
Subject: Re: microphones...
In-Reply-To: Your message of "Thu, 11 Apr 96 10:35:56 PDT."
<9604111735.AA24699@wilbur.dfrc.nasa.gov>
Date: Wed, 17 Apr 96 11:44:49 -0400
From: drela
X-Mts: smtp

%% To: Al Bowers <bowers@wilbur.dfrc.nasa.gov>

Hi Al,

Yes, it would be nice if the microphones gave TS frequency content. The frequencies are quite large, however, mainly due to the high TAS. I'll send you a plot showing the TS amplitudes for a range of frequencies. "L" on the plot is the chord, and "w" is the radian frequency.

For the upper surface, the dominant frequencies are in the range 1500 Hz -- 6000 Hz. This is a bit brisk for sampling in time! Is there any way to have an on-board spectrum analyzer that could be sampled at a much more leisurely 1 Hz, say? I don't know if such airborne systems exist, so this may be wishful thinking.

Here's an alternative low-brow approach: Record the microphone outputs on on-board tape recorders, and play it back in the lab. I don't know how good commercial Walkman-level machines are, but 1500-6000 Hz is in the upper range of music, so they should be OK. Considering how cheap such recorders are, it's something to look at. You have two recorders per microphone for better reliability.

- Mark

==

Return-Path: drela
Received: from localhost by henry; (5.65/1.1.8.2/06Apr95-0903PM)
id AA06026; Tue, 23 Apr 1996 23:29:16 -0400
Message-Id: <9604240329.AA06026@henry>
To: Al Bowers <bowers@wilbur.dfrc.nasa.gov>
Cc: drela@henry.mit.edu
Subject: Re: Sir!
In-Reply-To: Your message of "Tue, 23 Apr 96 15:39:26 PDT."
<9604232239.AA17918@wilbur.dfrc.nasa.gov>
Date: Tue, 23 Apr 96 23:29:16 -0400
From: drela
X-Mts: smtp

Hi Al,

I looked over your instrumentation spec sheet. I asked our local turbulence-measurement guru Kenny Breuer about the required mic sensitivity, since I wouldn't even hazard a guess. He's in Sweden, but I expect an email answer any minute now...

All the tolerances look adequate except...

* Mach #. +-- 0.005 is a bit too big. 0.002 or less would be better. Since you can get Mach directly from the ratio Ptotal/Pstatic, Pstatic , the uncertainty in Mach is the same as the uncertainty in the pitot pressure Ptotal-Pstatic . This also

sets the tolerances for these pressures.

* Normal g-force. 0.1 g is too big if you want to control the CL to better than 10% during a 1-g run! 0.05 or even 0.01 would be better.

* Alpha. + - 0.3 deg seems like a lot for transonic flow. Is 0.1 deg possible?

The I-rakes layout I sent a while back should be more than adequate to capture the whole wake. Even if it misses the edge a bit, so what? We can still compare the I-rake reading with what it should see base on the real-rake measurements. This will still confirm 2D-ness (or lack thereof).

- Mark

==

Appendix B: Key figures and sketches

APEX 16

x	y
1.000000	0.002036
0.996936	0.002939
0.988795	0.005338
0.977291	0.008683
0.962745	0.012817
0.946684	0.017254
0.930138	0.021685
0.913447	0.026011
0.896699	0.030208
0.879909	0.034271
0.863091	0.038196
0.846244	0.041983
0.829370	0.045629
0.812468	0.049137
0.795546	0.052501
0.778591	0.055724
0.761610	0.058805
0.744595	0.061747
0.727557	0.064550
0.710498	0.067212
0.693423	0.069734
0.676338	0.072110
0.659238	0.074342
0.642136	0.076424
0.625022	0.078355
0.607898	0.080136
0.590773	0.081760
0.573635	0.083229
0.556493	0.084542
0.539344	0.085695
0.522184	0.086690
0.505023	0.087527
0.487855	0.088202
0.470681	0.088718
0.453510	0.089073
0.436337	0.089267
0.419174	0.089299
0.402019	0.089166
0.384874	0.088867
0.367749	0.088400
0.350641	0.087762
0.333560	0.086951
0.316510	0.085961
0.299492	0.084791
0.282523	0.083434
0.265599	0.081885
0.248739	0.080140
0.231948	0.078187
0.215238	0.076023
0.198625	0.073635
0.182124	0.071014
0.165761	0.068148
0.149557	0.065024
0.133556	0.061628
0.117799	0.057944
0.102355	0.053956
0.087310	0.049650
0.072785	0.045017
0.058990	0.040088
0.046262	0.034955
0.035096	0.029839
0.025944	0.025057
0.018879	0.020836
0.013576	0.017218
0.009594	0.014109
0.006567	0.011391
0.004250	0.008957
0.002489	0.006718
0.001204	0.004607
0.000381	0.002575
0.000024	0.000624
0.000089	-0.001269
0.000556	-0.003190
0.001466	-0.005090
0.002906	-0.006846
0.004935	-0.008327
0.007560	-0.009522
0.010851	-0.010544
0.015033	-0.011514
0.020527	-0.012515
0.028026	-0.013603
0.038406	-0.014821
0.052060	-0.016156
0.068128	-0.017542
0.085370	-0.018957
0.102840	-0.020404
0.120161	-0.021923
0.137275	-0.023559
0.154200	-0.025357
0.170985	-0.027355
0.187738	-0.029589
0.204639	-0.032060
0.221631	-0.034644
0.238067	-0.036998
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0.269935	-0.040388
0.286070	-0.041481
0.302451	-0.042255
0.318997	-0.042742
0.335659	-0.042958
0.352439	-0.042918
0.369357	-0.042645
0.386421	-0.042174
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0.895951	-0.002958
0.913086	-0.002205
0.930043	-0.001634
0.946688	-0.001280
0.962660	-0.001176
0.977023	-0.001319
0.988508	-0.001619
0.996818	-0.001946
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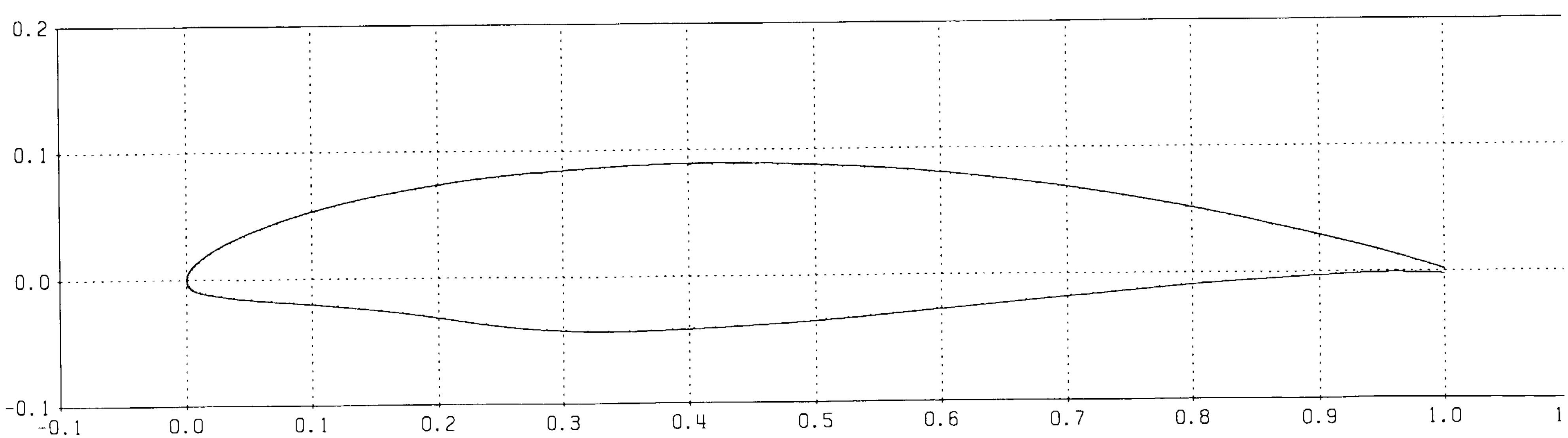
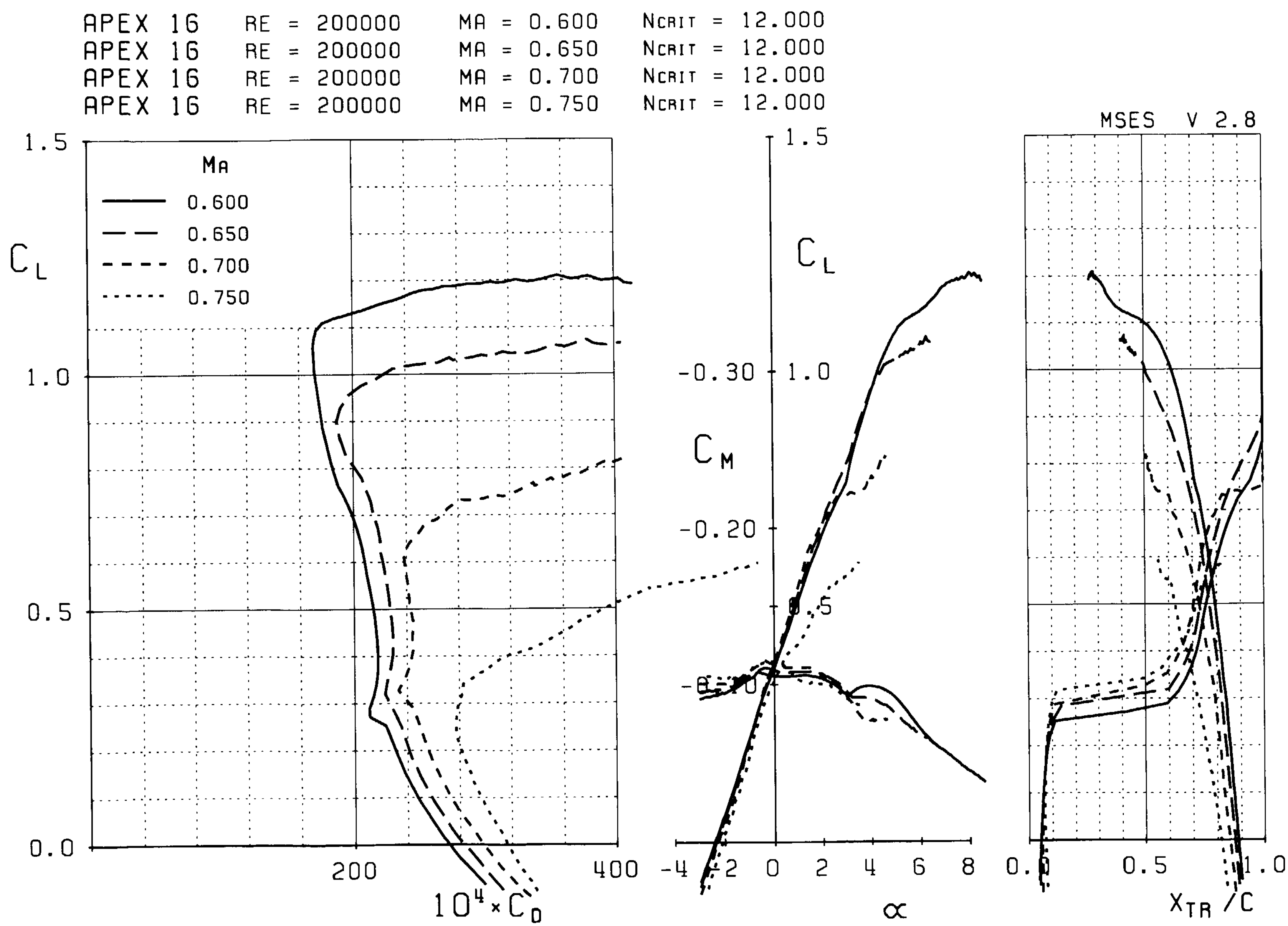
APEX-16 Airfoil

13.11 % max thickness
 2.68 % max camber





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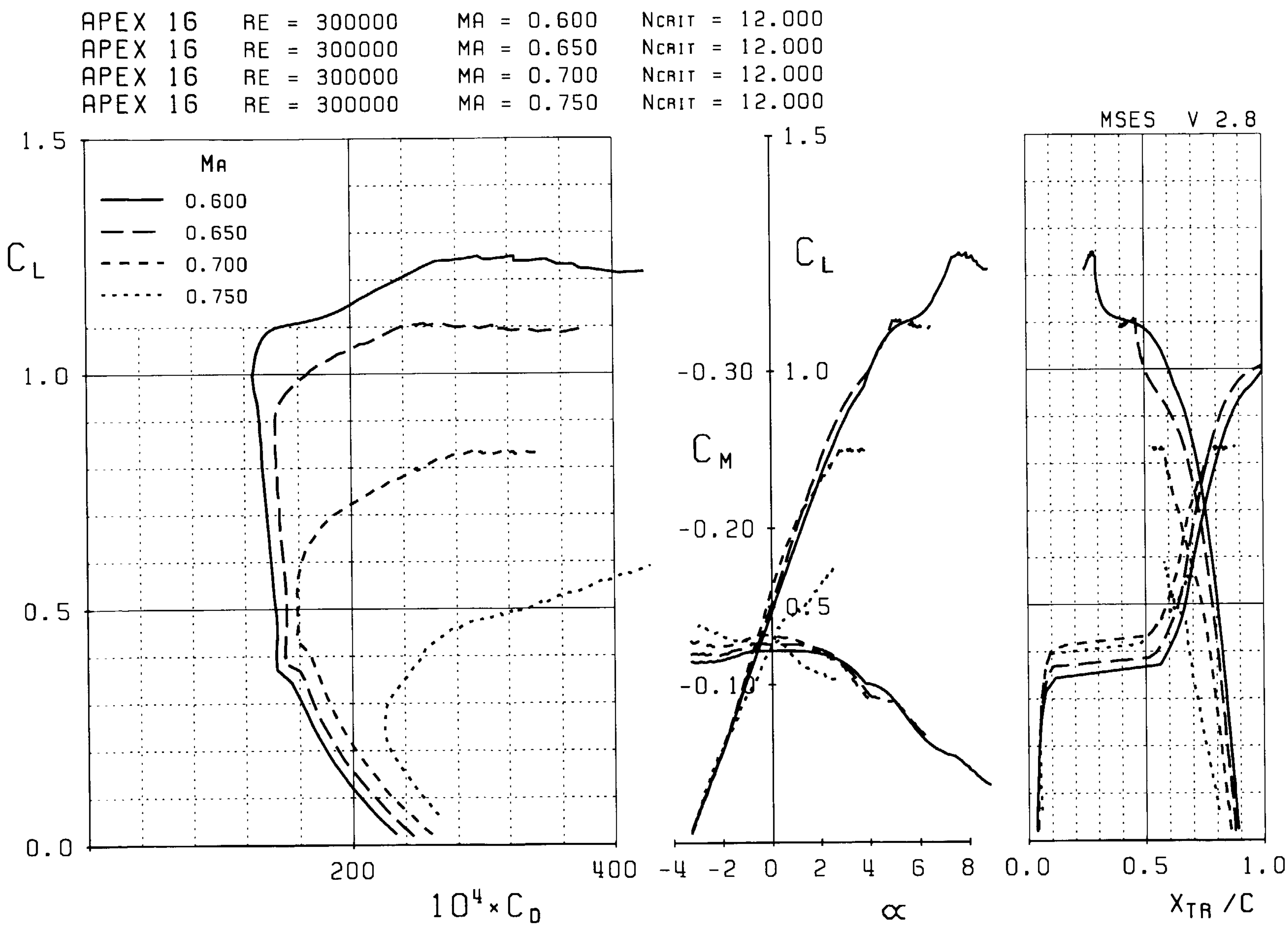
APEX 16 RE = 200000 MA = 0.750 NCRIT = 12.000
MSES V 2.8
MA
0.600
0.650
0.700
0.750
C_L
C_M
$10^4 \times C_D$
α
X_{TR}/C
1.5
1.0
0.5
0.0
200
400
-0.30
-0.20
-0.10
-4
-2
0
2
4
6
8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0.650
0.700
0.750
C_L
1.5
1.0
0.5
0.0
200
400
$10^4 \times C_D$
C_M
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-0.20
-0.10
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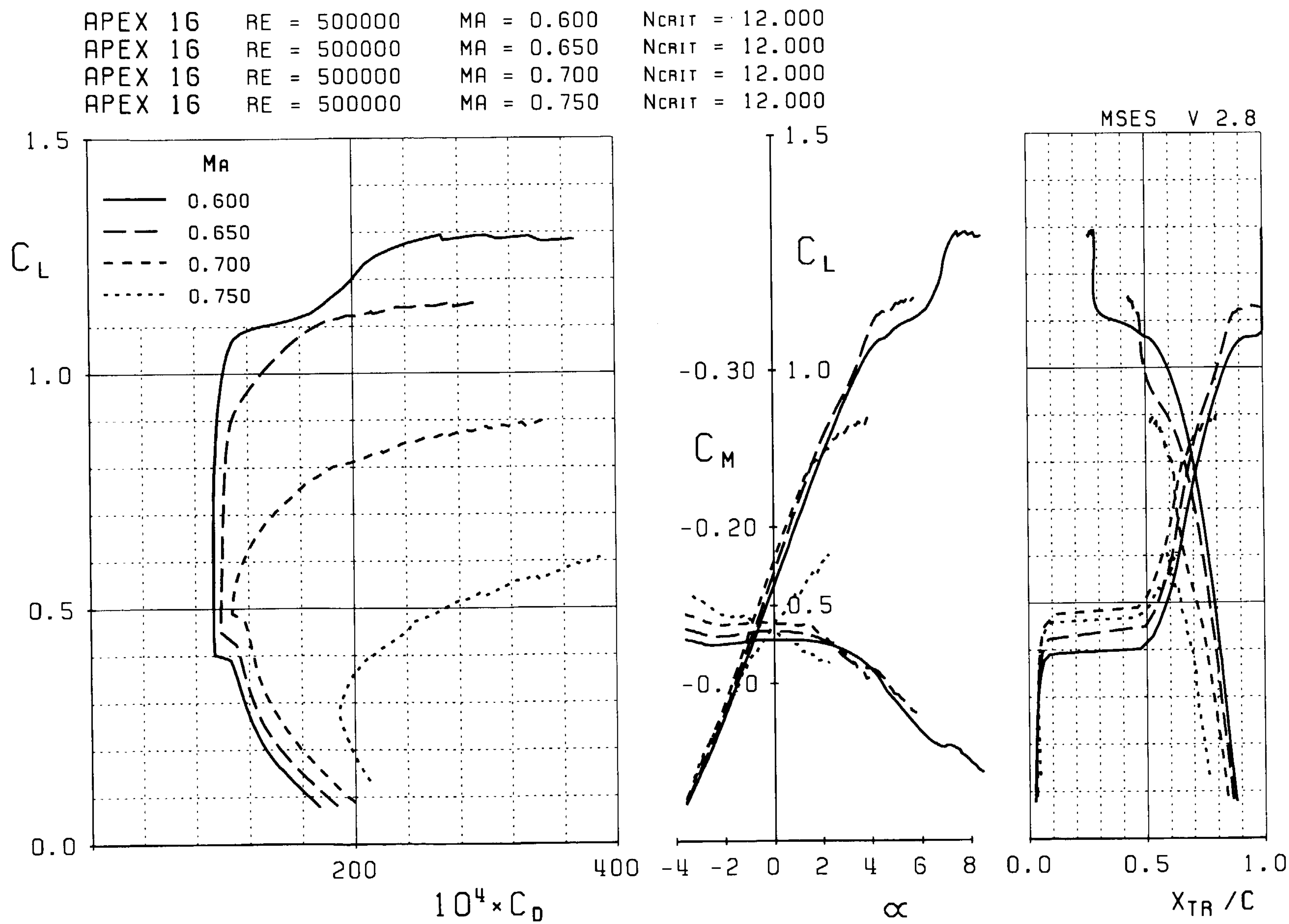
α
0.0
0.5
1.0
X_{TR}/C



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APEX 16 RE = 500000 MA = 0.650 NCRIT = 12.000
APEX 16 RE = 500000 MA = 0.700 NCRIT = 12.000
APEX 16 RE = 500000 MA = 0.750 NCRIT = 12.000
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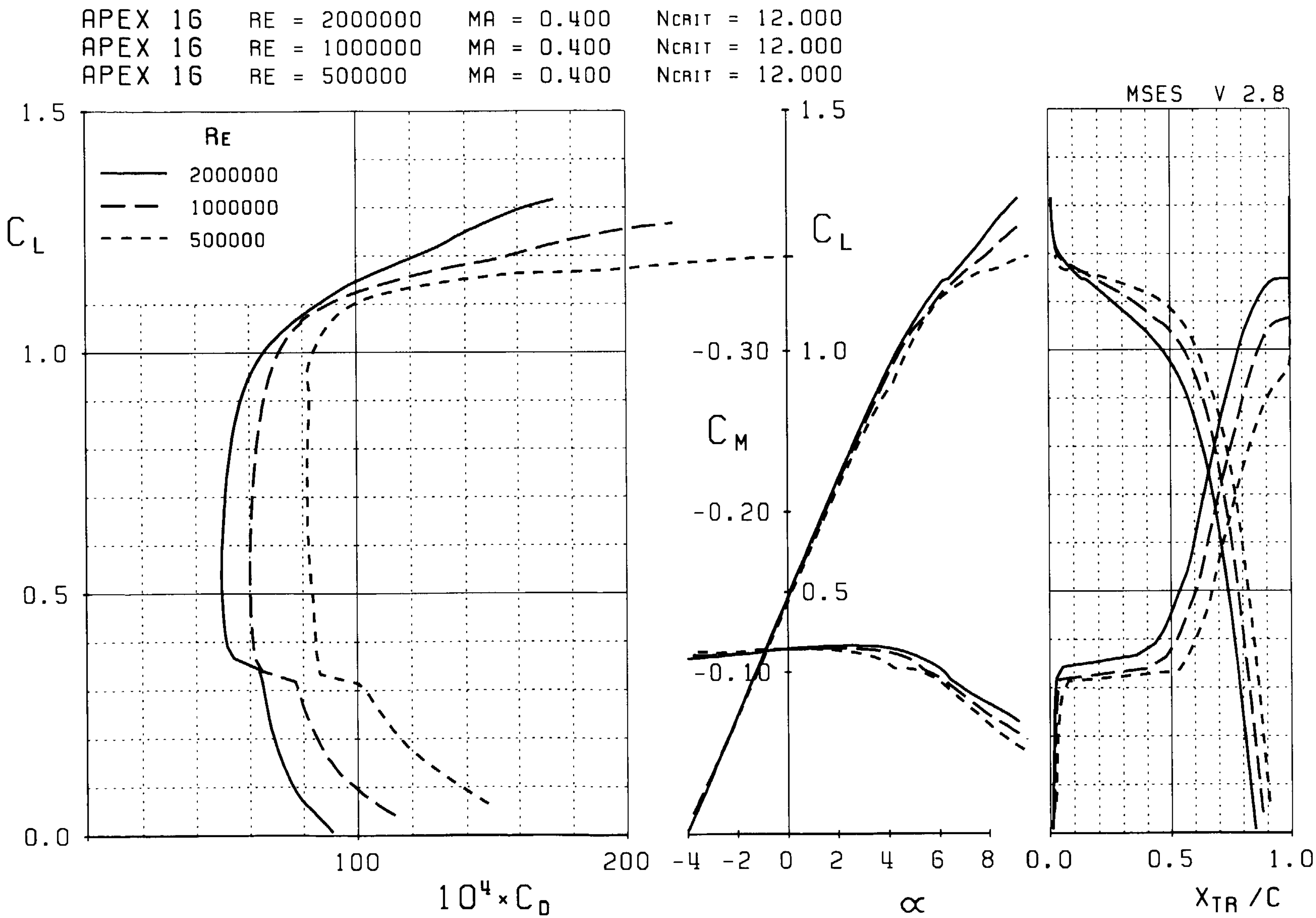
0.700
0.750
CL
CM
10^4×CD
α
XTR /C



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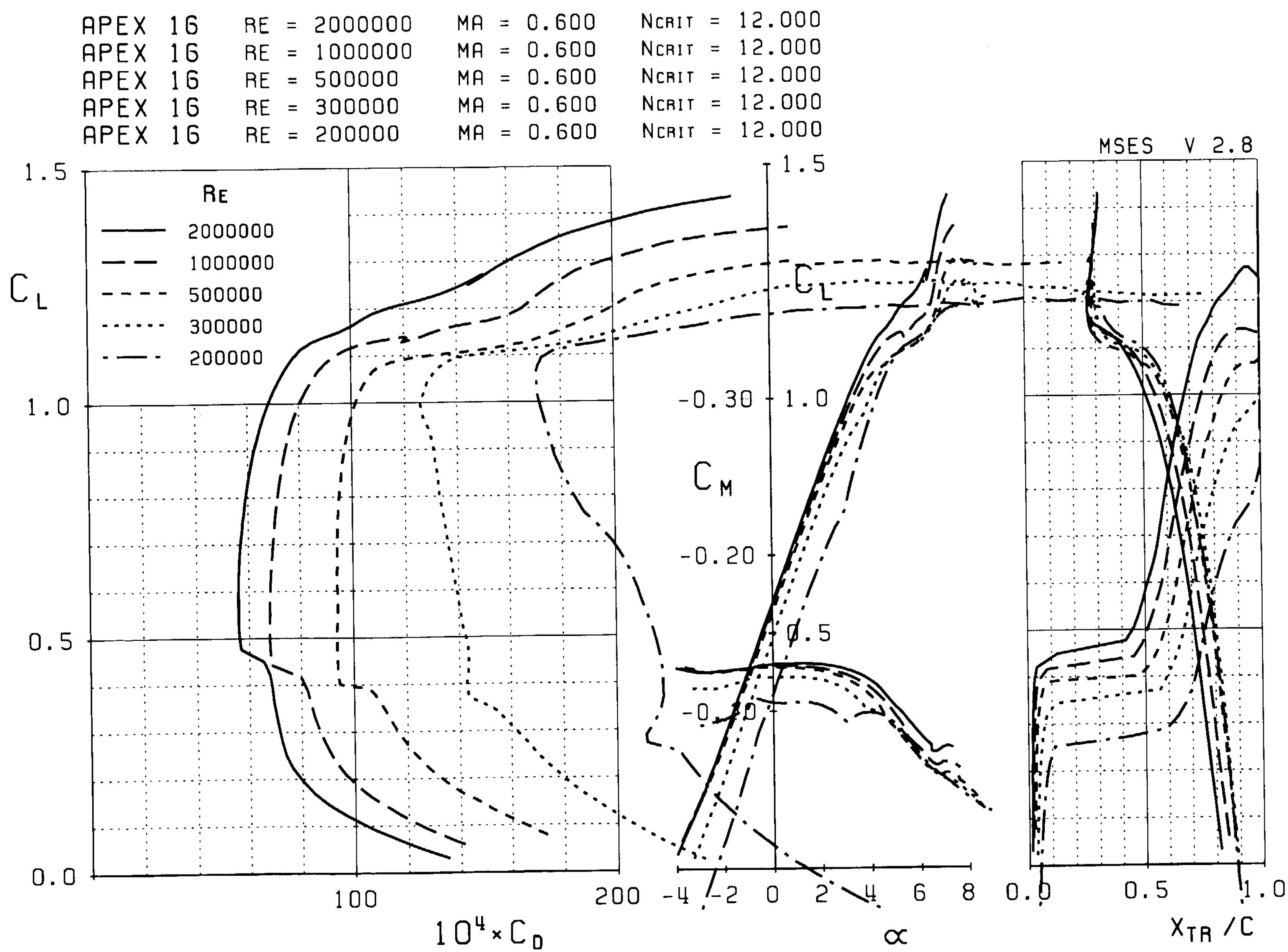
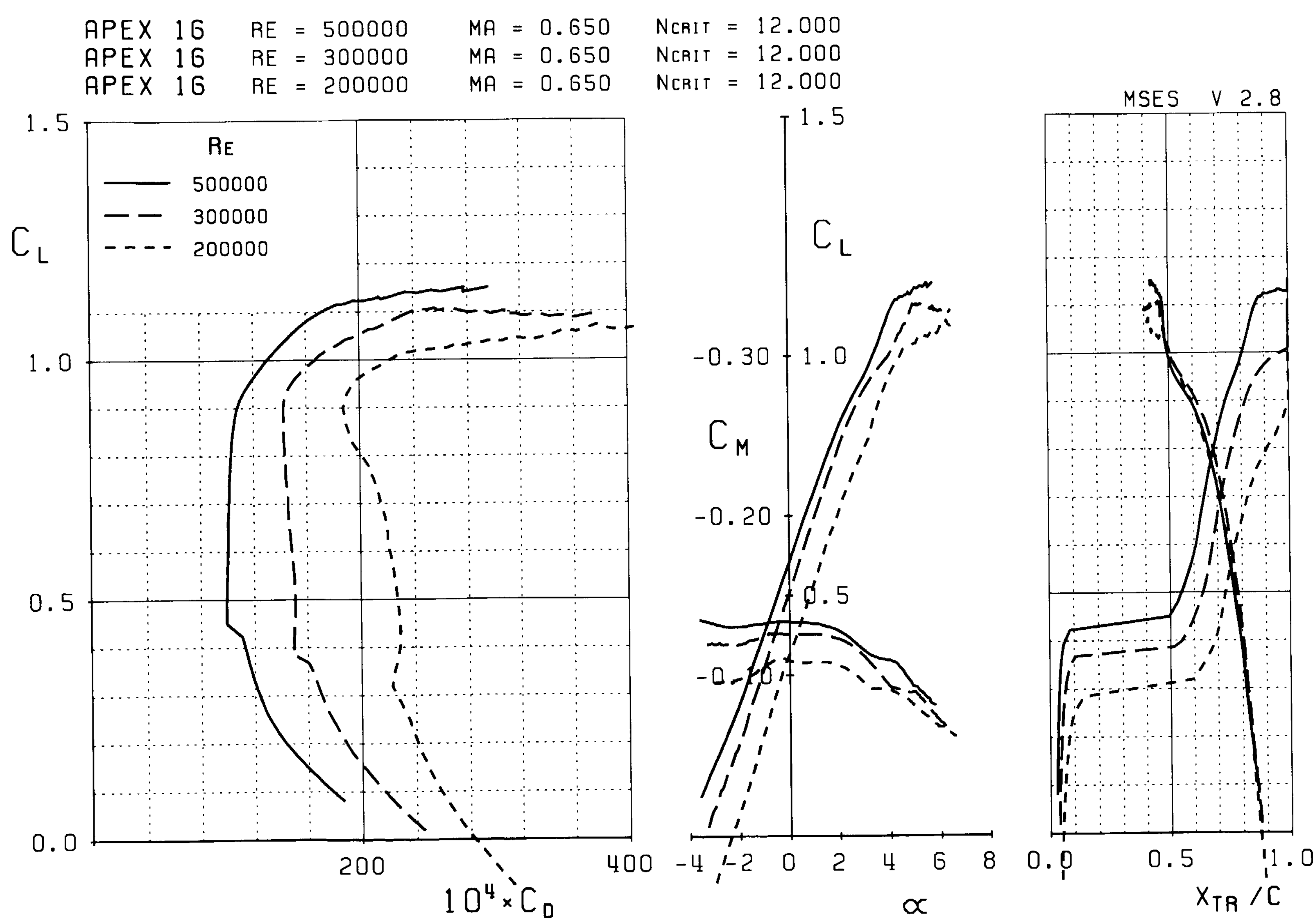
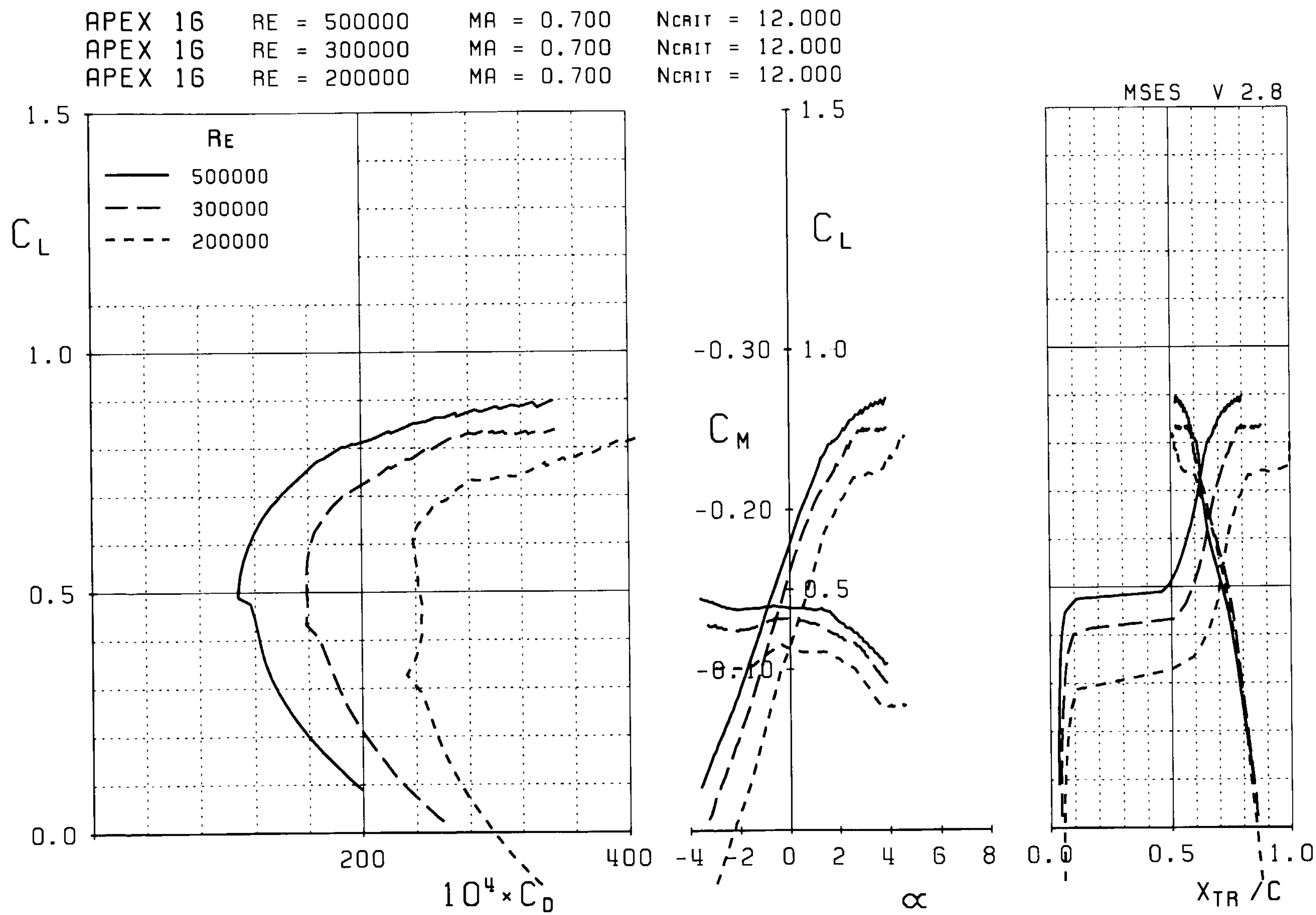
2000000
1000000
500000
C L
C M
1.5
1.0
0.5
0.0
-0.30
-0.20
-0.10
100
200
-4 -2 0 2 4 6 8
0.0 0.5 1.0
10^4 × C D
α
X TR /C



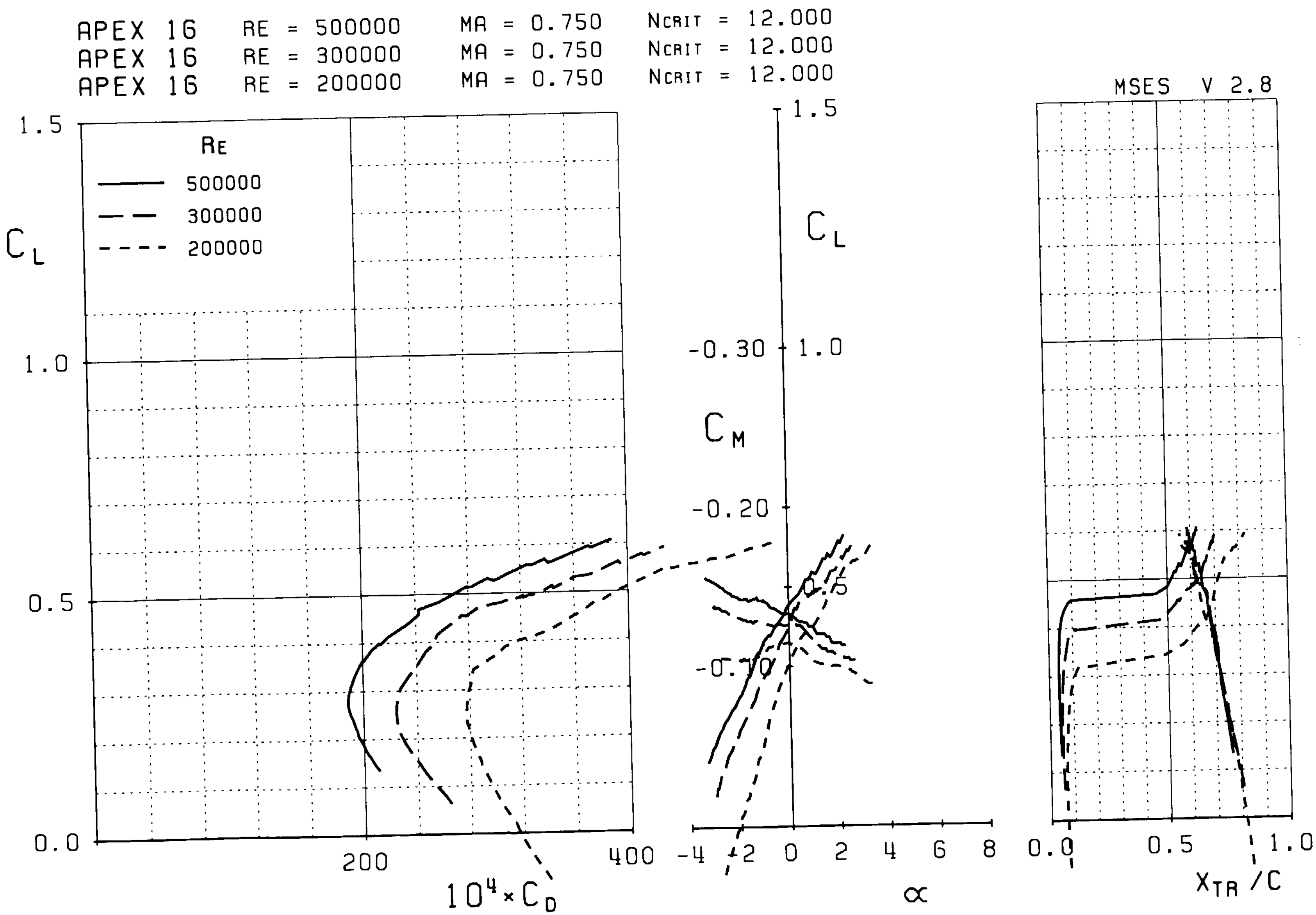
APEX 16 RE = 2000000 MA = 0.600 NCRIT = 12.000
APEX 16 RE = 1000000 MA = 0.600 NCRIT = 12.000
APEX 16 RE = 500000 MA = 0.600 NCRIT = 12.000
APEX 16 RE = 300000 MA = 0.600 NCRIT = 12.000
APEX 16 RE = 200000 MA = 0.600 NCRIT = 12.000
MSES V 2.8
RE
2000000
1000000
500000
300000
200000
CL
CM
10^4 × CD
α
XTR /C



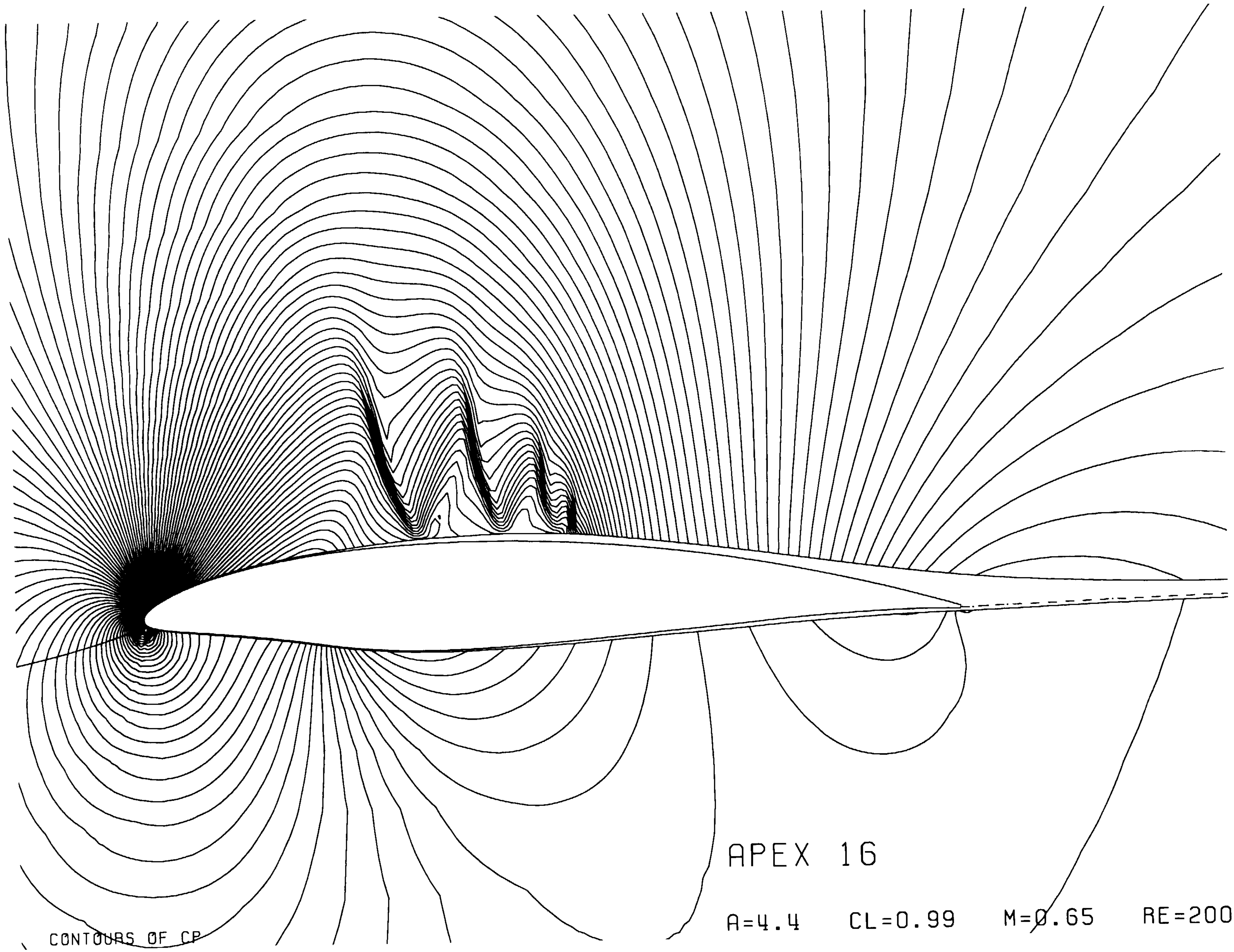
APEX 16 RE = 500000 MA = 0.650 NCRIT = 12.000
APEX 16 RE = 300000 MA = 0.650 NCRIT = 12.000
APEX 16 RE = 200000 MA = 0.650 NCRIT = 12.000
MSES V 2.8
RE
500000
300000
200000
CL
1.5
1.0
0.5
0.0
200
400
10^4×CD
CM
-0.30
-0.20
-0.10
-4 -2 0 2 4 6 8
α
0.0
0.5
1.0
XTR /C

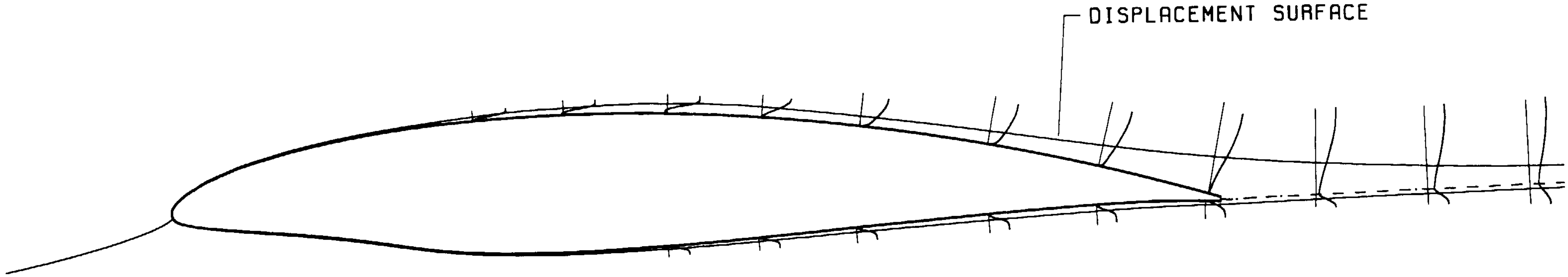


APEX 16 RE = 500000 MA = 0.700 NCRIT = 12.000
APEX 16 RE = 300000 MA = 0.700 NCRIT = 12.000
APEX 16 RE = 200000 MA = 0.700 NCRIT = 12.000
MSES V 2.8
RE
500000
300000
200000
CL
CM
1.5
1.0
0.5
0.0
-0.30
-0.20
-0.10
200
400
$10^4 \times C_D$
-4 -2 0 2 4 6 8
α
0.0 0.5 1.0
X_{TR}/C



APEX 16 RE = 500000 MA = 0.750 NCRIT = 12.000
APEX 16 RE = 300000 MA = 0.750 NCRIT = 12.000
APEX 16 RE = 200000 MA = 0.750 NCRIT = 12.000
MSES V 2.8
RE
500000
300000
200000
C_L
C_M
$10^4 \times C_D$
α
X_{TR}/C


APEX 16
A=4.4 CL=0.99 M=Ø.65 RE=200
CONTOURS OF CP



APEX 16

A=4.4 CL=0.99 M=0.65 RE=200K

BL VELOCITY PROFILES