with Δy given by a curve-fit of Rotta's graph of Δy^+ vs k_s^+ just mentioned,

$$\Delta y^+ = 0.9[k_s^{+\frac{1}{2}} - k_s^+ \exp(-k_s^+/6)] \quad 4.555 < k_s^+ < 2000 \quad (8)$$

But Rotta's relation for Δy^+ was derived to account only for the vertical shift of the velocity profile on the semilogarithmic plot, and no attempt was made to see if the slope, or extent, of the resulting straight-line portion agreed with experiment. Thus there is not basis for use of Eq. (8) in Eq. (7). Indeed, considerable attention has been given in the literature to the question of the appropriate origin for y in Prandtl's mixing length model for rough walls. Pimenta et al.[3] showed that, for spherical ball roughness elements of equivalent sand grain roughness of 0.031 in., the appropriate origin for the velocity profiles lay 0.006 in. below the ball tips for untranspired flow, independent of x location or freestream velocity. Reda[4] found for sand grain roughness that the virtual origin lay at approximately half the grain size below the tips. Rotta[2] has a sketch showing that he imagined his origin $y=0$ to be located at the base of the sand grains, with his reference plane for the velocity profiles Δy below this location. Clearly such a picture is not in accord with experiment. Cebeci and Chang simply do not explicitly specify where their origin $y=0$ is located relative to the roughness elements; however, Eq. (8) cannot agree with experiment since for a given k_s it gives a value of Δy which depends on u_τ and ν, whereas experiments has established $\Delta y = f(k_s)$ only.

A further point concerning Eq. (8) is that it is not consistent with the B function, Eq. (15) of Ref. 1, which it should be since both equations are based on the same data. If experimental data are to be curve-fitted, it would be more appropriate to use one curve-fit only. But then perhaps the authors did not recognize the essential equivalence of the two relations.

Next we turn to what is perhaps the most troublesome point in attempting to develop a finite-difference calcualtion method for rough walls. Since the boundary-layer equations are not valid very close to the roughness elements, there is a fundamental objection to attempting to solve the equations too close to the wall. The solution domain should be restricted to $y > y_0$. Cebeci and Chang do not explicitly address this problem but proceed by first noting that, even for smooth walls, it may be numerically convenient to apply "wall" boundary conditions at some distance y_0 from the wall, and mention use of $y_0^+ = 50$ for smooth walls. In such approach they recommend that u_0^+ be obtained from

$$u_0^+ = 2.5 \ln y_0^+ + C; \quad C = 5.2 \quad (9)$$

with v_0^+ from continuity. For rough walls they suggest that C be replaced by a curve-fit of the function derived from Nikuradse's sand grain velocity profiles,

$$C = 5.2 \qquad k_s^+ < 2.25 \quad (10a)$$

$$C = 5.2 + [8.5 - 5.2 - 2.5 \ln k_s^+] \sin[0.4258(\ln k_s^+ - 0.811)]$$

$$2.25 \le k_s^+ \le 90 \quad (10b)$$

$$C = 8.5 - 2.5 \ln k_s^+ \qquad k_s^+ > 90 \quad (10c)$$

There are a number of objections to this approach:

1) The Δy displacement model of Rotta on which the method is supposedly based has been dropped, and Nikuradse's velocity profiles are being used directly to provide boundary conditions on the momentum equations. It is surely inconsistent to ignore the Δy displacement in Eqs. (9) and (10) which are applied at $y = y_0$, and retain the Δy displacement in Eq. (7) which is applied only for $y > y_0$, i.e., when the effect of Δy is less.

2) Equation (10b) applies to sand grain roughness only; standard works, e.g., Jayatilleke,[5] show how the C function depends on roughness pattern. Since none of the experimental data in the paper are for sand grain roughness its use is somewhat difficult to justify.

3) Nowhere do Cebeci and Chang specify the location of their origin y relative to the physical rough wall. As long as only quantities such as C_f, θ, and δ^* for external flows are used to evaluate the calculation method, then, admittedly, the location of the physical wall is essentially irrelevant. However, if velocity profiles or internal flows are to be examined, then specification of the y origin is imperative.

In conclusion, it is suggested that there are many features of the calculation method proposed by Cebeci and Chang that are unacceptable, and considerable further effort is required before a satisfactory differential calculation method for rough walls can be claimed.

References

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Reply by Authors to A.F. Mills

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THE model suggested by Rotta appeared in Ref. 7 of our paper and in Ref. 2 of Mills' Comment. In that reference, Rotta briefly described his model. The description, in our opinion, however, was too brief. For this reason, when Cebeci and Smith described it again in their book,[1] they included somewhat more detail, so that it would be clear to those who are not too familiar with turbulence models and with calculation procedures associated with turbulent flows. It appears that Mills has not read that section of that book, nor has he read the recent textbook by Cebeci and Bradshaw[2] which again describes this model and comments on the value of the von Kármán constant for flows over rough surfaces.

It may be best to refer Mills to those textbooks. However, since the roots of his Comment seem to come from his misunderstanding of Rotta's model, to avoid any further difficulties, we present the following description of it.

Mills may be confused by the shift, Δy, employed by Rotta and by the shift of the virtual origin Δz. The latter is employed to shift the experimental data on the semilog plot of u vs y to

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obtain a straight line for the portion of the curve which ought to be in the logarithmic region and plays no role in the model used to compute turbulent flows. To obtain the skin-friction coefficient from the slope of the straight-line portion of the curve, it is customary to assume that the von Kármán constant retains its value for flows over smooth surfaces (see Ref. 2, p. 177). It is thus seen that the argument for parallel log-law portions for smooth and rough flows is an essential part of bringing the experimental data into tractable form. The shift Δy^+ employed by Rotta is simply the difference between the smooth and rough log-law portions of the curves on semilog plot as u^+ vs y^+ (see Fig. 4.29 and 4.30 of Ref. 1) and bears no relationship to the shift of virtual origin Δz.

The virtual origin $y = 0$ is a distance Δz below the crests of the roughness elements. The y in Eqs. (7) and (9) in Mills paper are indeed measured from the virtual origin. Thus the contradiction in the eddy-viscosity formulas and the wall boundary conditions "invented" by Mills does not exist. The appearance of Δy in Eq. (7) only affects the rate of approach to Eq. (9) when performing integration. However, this problem has been clearly overshadowed by the questions of where to start the solution and what the boundary conditions should be because the sublayer is reduced or nonexistent. It has been found by experience that the logarithmic boundary conditions, Eq. (9), are satisfactorily applied if $y_0^+ \geq 50$ and $u_0/u_e \geq 0.15$ are satisfied simultaneously.

Mills' objection 2 toward the end of his Comment simply states the obvious: Of course we are well aware that Eqs. (8) and (10) will have different forms for different roughness geometries (see Ref. 1, p. 129, Fig. 4.27). Unfortunately, so far only the sand-grain type roughness has been well researched and documented.

Before we conclude, we would like to offer a word of caution. If the agreement between "theory" and all the available experimental data on the subject is quite good (so good that it may be difficult to beat these predictions by other turbulence models), it is perhaps advisable to be cautious in light of this agreement before raising objections which do not bear up under the weight of the theoretical, computational or experimental evidence.

References

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Comment on "Pressure Dependence of Burning Rate of Composite Solid Propellants"

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Nomenclature

C_i = arbitrary constants
E = activation energy
k_1 = kinetic rate constant
k_0, k_2 = arbitrary constants
P = pressure
R = universal gas constant
$\dot{r}$ = regression rate
T = temperature
ΔV^* = volume of activation
T_f = flame temperature
T_s = surface temperature

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In a recent contribution, Rastogi et al.[1] derived an expression for the pressure dependence of the regression rate of solid propellants by appealing to a model of the gas phase as a flow reactor. Because their reasoning is defective, the formula finally obtained is only an empirical correlation with no insights into mechanisms.

A pivotal and incorrect assumption in the derivation is their Eq. (8), which writes a rate constant

$$k_1 = k_0 \exp(-P\Delta V^*/RT) \tag{1}$$

in which k_0 is independent of pressure and temperature. For any gas phase oxidation of a fuel, k_0 is certainly not temperature independent. A usual assumption is the Arrhenius form which converts Eq. (1) to

$$k_1 = k_2 \exp[-(1/RT)(E + P\Delta V^*)] \tag{2}$$

To now make their mathematical simplification of linearizing the exponential with only a 5% error, a necessary condition is: $(E + P\Delta V^*)/RT < 0.3$.

Commonly assumed values of E/RT range indefinitely upward from about 2. Indeed a whole field of combustion analysis rests on an assumption of asymptotically *large* activation energy.[2] Without the linearization, the form of the resulting integration would be more complicated:

$$\dot{r}^2 = C_1 P[e^{-bQ} + C_2 + C_3 Q + C_4 Q^2 + C_5 Q^3 + \ldots] \tag{3}$$

where $Q = E + P\Delta V^*$.

The text reference[3] cited by the authors does not apply to the present problem because 1) it defines

$$\left(\frac{\partial \ell n k}{\partial P}\right)_T = \frac{-\Delta V^*}{RT}$$

which applies only to isothermal pressure changes; 2) it is presented only in the context of liquid solutions; and 3) it notes that at higher pressures ΔV^* depends on pressure. There is little known about ΔV^* for gases.

Another error was made in obtaining their Eq. (6) by assuming no change in specific volume of the process, since in propellant combustion the volume of products exceeds the volume of reactants. That fact must be accounted for by making the missing molecular weight in Eq. (6) dependent on C_f through whatever stoichiometry is assumed.

As it stands, the expression for burning rate Eq. (13) cannot be correct because it also requires that the variable L be both the reactor length and the divisor in the gas side surface temperature gradient in Eq. (1), such that

$$\frac{\partial T}{\partial x} = \frac{T_f - T_s}{L}$$

The thus assumed linear temperature profile across the gas phase cannot be universally, if ever, true. What few temperature measurements have been made by various investigators[4] impeach the linear assumption. The expression $[(T_f - T_s)/L]$ estimates the gradient in the center of the zone, not at the regressing surface.

All of these objections taken together should spell a rejection of the rationale for this correlation of burning rate with pressure.