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Recent Advancements Toward the Understanding of Turbulent Boundary Layers

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Over the past decade almost every aspect of our traditional beliefs about wall-bounded flows has been challenged. Some beliefs have been abandoned and others modified as new theories and new measurements have abounded. No longer are direct measurements summarily dismissed because they contradict a long-standing theory, and even those experimentalists who still believe in the log law only apply it outside y^+ of several hundred. In this meeting I have been invited to look back at some of the early contributions in this revolution. I shall review briefly a few of the more controversial ideas, and highlight some of the reasons that led us in a different direction. In addition to pointing out some successes, I will discuss some failures and open questions. I will even offer a few new ideas, and of course renew some old challenges.

Nomenclature

A = universal constant in CG97
A_* = constant of proportionality
B_i = additive constant in log law, inner variables
B_o = additive constant in log law, outer variables
B_* = constant of proportionality
C = additive constant in log friction law when written as function of $\ln R_\theta$
C_i = coefficient in inner version of power law overlap solution, function of δ^+
$C_{i\infty}$ = limiting value of C_i as $\delta^+ \to \infty$
C_o = coefficient in outer version of power law overlap solution, function of δ^+
$C_{o\infty}$ = limiting value of C_o as $\delta^+ \to \infty$
C_1 = additive constant in log friction law when written as function of $\ln \delta^+$, $B_i - B_o$
D = stretch parameter to make theory independent of choice of outer length scale
$D_{\log}$ = stretch parameter for log law
D_∞ = defined to be $C_{i\infty}/C_{o\infty}$
f_i = profile function describing velocity deficit in inner part of boundary layer
f_o = profile function describing velocity deficit in outer part of boundary layer
$f_{o\infty}$ = limit of f_o as $\delta^+ \to \infty$ that depends only on $\bar{y}$
H = δ_*/θ, shape factor
I = constant of proportionality
II = constant of proportionality
g_o = part of f_o that depends only on δ^+ and upstream conditions
k = kinetic energy of turbulence, $\mathrm{m^2/s^2}$
L = energy length scale, roughly the integral scale, m
q^2 = $u^2 + v^2 + w^2$, $\mathrm{m^2/s^2}$
R_{so} = scale function for Reynolds shear stress in outer layer, $\mathrm{m^2/s^2}$
R_θ = Reynolds number based on momentum thickness defined by $U_\infty\theta/\nu$
r_o = profile function describing Reynolds shear stress in outer layer
$r_{o\infty}$ = limit of r_o as $\delta^+ \to \infty$ that depends only on $\bar{y}$
U, u = mean and fluctuating streamwise (or x) velocities, m/s
U_e = experimental guess of U_∞, m/s
U_{so} = either U_∞ or u_*, depending on which form of the velocity deficit is being used, m/s
U_∞ = mean velocity at great distance from wall, m/s
U^+ = mean velocity normalized by u_*, U/u_*
u_* = friction velocity defined from wall shear stress as $\sqrt{\tau_w/\rho}$, m/s
V, v = mean and fluctuating velocities normal to wall, m/s
x = streamwise coordinate, m
y = coordinate normal to wall, m
y^+ = dimensionless inner variable defined by yu_*/ν
$\bar{y}$ = dimensionless outer variable defined by y/δ
α = universal dimensionless constant in GC97
γ = exponent in power law theory, function of δ^+
γ_∞ = limit of γ as $\delta^+ \to \infty$
δ = boundary layer thickness, usually taken as δ_{99}, m
δ_{95} = distance from the wall at which $U/U_\infty = 0.95$, m
δ_{99} = distance from the wall at which $U/U_\infty = 0.99$, m
δ_* = displacement thickness defined from $\int_0^\infty (1 - U/U_\infty)\,dy$, m
δ^+ = ratio of outer to inner length scales defined by $\delta u_*/\nu$
δ^+_{ref} = reference value of δ^+
ε = rate of dissipation of turbulence energy per unit mass, $\mathrm{m^2/s^3}$
θ = momentum thickness defined from $\int_0^\infty (U/U_\infty)(1 - U/U_\infty)\,dy$
ν = kinematic viscosity, $\mathrm{m^2/s}$
ξ = dummy integration variable for $\bar{y}$
ρ = density, $\mathrm{kg/m^3}$
τ_w = wall shear stress, $\mathrm{kg \cdot m/s^2}$
$*$ = indicates dependence on upstream conditions

I. Introduction

A little more than decade ago, the basic characteristics of turbulent boundary layers on flat surfaces were widely believed to be well understood. The logarithmic velocity profile was believed to be universal for all wall-bounded flows (except possibly natural convection). So confident was the turbulence community in its beliefs that virtually no one even bothered to measure the skin friction, and it became common practice to simply fit the log velocity profile to a few points near the wall, usually for values of y^+ between 30 and 100, and then infer the skin friction using the "universal" and known log constants [1–3] (the so-called Clauser method [4]). It

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seemed to bother only a few [5–7] that real shear stress measurements (both momentum integral and direct) differed consistently and repeatedly from these inferred results [8,9]. Instead of causing a reexamination of the theory, it became common wisdom that there was something wrong with the experimental techniques. The careful drag and mean velocity measurements laboriously performed in the 30s and 40s [10,11] were discarded as being in error [12], along with any new attempts [7]. Even direct measurements of the boundary layer thickness (δ_{99}, δ_{95}, etc.) came to be viewed as unreliable, and these were discarded in favor of length scales "defined" from the universal log equations [12].[†] So pervasive are these ideas that it is still common to find the Clauser method used, even when direct measurements of shear stress are available, presumably because it makes the "log layer" velocity profiles appear to be universal.

Even today most turbulence models are still more or less are based on these old ideas, but in the experimental and theoretical world much is now changing. It has been a decade since the papers by George and Castillo [5,6,13], Gad-el-Hak and Bandyopadhyay [14], and Barenblatt [15] reopened the debate about the behavior of turbulent wall-bounded flows. The most attention-getting aspect of this debate has been about the validity of the log law or the power law alternative. Even those who still hold the classical views have been left in the uncomfortable position that their universal constants appear to vary from one experiment to the next, especially as the Reynolds numbers have increased. The new ideas have not been without their problems either. Some seem to work well and be definitive, but other consequences of the same assumptions have been less successful. To the casually interested onlooker and devoted researcher alike the entire field appears to be in chaos.

One purpose of this paper is to try to change the debate from empiricism and curve-fitting back to mechanics, from scaling laws to governing equations, from religion to physics and physical principles. To find truth, one must begin by asking (not ignoring) the fundamental questions. A recent text [16] after reviewing the alternative ideas concluded the classical theory was preferable "because it was *universal*." However desirable universality may be, it is no substitute for *reason, deduction, and careful experiment*. Moreover, universality is useless if nature itself is not universal. The goal of our research must be to bring the laws of nature, our deductions, and our experiments into coincidence. If universality and our other "beliefs" about wall-bounded flows fall by the wayside, then so they must.

There have been only a few theoretical contributions over the past ten years. Some perturbed and extended old ideas, but most began with the presuppositions of the old without reconsidering the fundamental questions of their validity (e.g., whether there is an outer scaling, and if so what it is). Others broke new ground using new techniques (e.g., Lie group theory [17]), but left open many questions. Most troubling has been total silence on whether departures from streamwise homogeneity matter or not, or what are the implications of Kolmogorov theory, both of which were the bases of at least the George/Castillo challenge.

There have also been a number of recent experiments. These fall into several groups. Some fell into the same traps as earlier work: assuming results to be true, then confirming that they were using circular logic (e.g., using the Clauser shear-stress method to confirm the log friction law [3]). Others truly broke new ground using direct measurements of shear stress and pushing to higher Reynolds numbers [18,19]. Unfortunately most of these higher Reynolds number experiments were unable to obtain velocity profile measurements close enough to the wall (without substantial and questionable error corrections) to be definitive. Even accepting these measurements, however, there are also problems (at least at this writing) about whether some of the new results [19] satisfy the momentum integral equation, thus raising the most serious questions about whether the measurements were correct or precisely which flow was measured at all: clearly fundamental to any argument. Moreover, there have been almost no measurements where sufficient data have been provided to actually substitute into the averaged equations (e.g., RANS, kinetic energy, etc.) and see if the terms all add up. In the high Reynolds number experiments [18,19], not even the Reynolds stress has been provided (curiously, even when measured), meaning that even the most elementary momentum balance is impossible. So we are largely left with detailed measurements of flows for which we cannot say with certainty what flow was measured, nor whether the flows we have measured satisfy the RANS equations we believe to govern them.

Largely lost in the curve fitting and conflicting data of the past decade have been a number of more fundamental issues which underlie the disagreements. Also lost has been recognition that the fluid dynamics of the 20th century has changed dramatically in the 21st. The search for scaling laws, so valuable to engineers even a decade ago, is largely irrelevant today. Modern computers and user-friendly software have made CFD the standard industrial approach to flow problems. But current computational models reflect the physics as we understood it more than a half-century ago. What CFD, and especially large eddy simulation (LES), needs now is research that leads to a better understanding of the "physics" of the boundary layer so that closure models can be improved to reflect it. No matter whether we pay our allegiance to the traditional ideas or the new ones, the continuing difficulties with computations of complex wall-bounded flows (or even simple flows with pressure gradients) suggest strongly that we have missed something important.

The objectives of this invited paper are limited. I have been asked to review some of the iconoclastic ideas that were introduced by George and Castillo [6] (hereafter referred to as GC97). I will also discuss briefly how my own thinking has evolved since then, largely due to input from colleague T. Gunnar Johansson, my continuing interaction with my former students (especially Luciano Castillo [20] and Martin Wosnik [21] and their students, especially Jungwa Seo [22] and Xia Wang [23]). And finally I will suggest some questions for future research. The primary focus will be on the zero pressure gradient boundary layer, because it appears to be the most problematic. To keep the big picture in view, few details of the analysis will be provided, because these have been published elsewhere.

II. Velocity Deficit for the Outer Part of the Boundary Layer

There is no disagreement (to the best of my knowledge) about the proper scaling for the part of the turbulent boundary layer closest to the wall, the viscous sublayer. All agree for various reasons that the form proposed by Prandtl [24] is correct, i.e.,

$$\frac{U}{u_*} = f_i(y^+, \delta^+) \tag{1}$$

The reason for including the argument δ^+ will be addressed, but it is crucial for theoretical analysis. Note that there is considerable disagreement over exactly what region of the flow is governed by this, especially if the argument δ^+ is neglected or if only its limit as $\delta^+ \to \infty$ is considered, say $f_{i\infty}(y^+)$. An additional functional argument could be added to account for the possible effect of upstream conditions, but none has ever been observed for this so-called viscous sublayer.

The scaling for the outer part of the boundary layer is quite another story. It is in fact the velocity deficit profile for the outer 90% or so that lies at the heart of the debate over the zero-pressure-gradient turbulent boundary layer, so I will discuss it first. Three alternative velocity deficit profiles for the outer boundary layer (and corresponding functional relationships) have been proposed. They are

$$\frac{U - U_\infty}{u_*} = f_o(\bar{y}, \delta^+, *), \qquad \text{(von Kármán)} \tag{2}$$

[†]Note that Coles [12] states exactly what choices he made and why. This is science as it should be, because even when the assumptions and conclusions turn out to be wrong, the reasons can be identified. Unfortunately many who followed Coles have not been so careful, and in fact often seem unaware of his choices, especially about the boundary layer thickness.

$$\frac{U - U_\infty}{U_\infty} = f_o(\bar{y}, \delta^+, *), \qquad \text{(George/Castillo)} \qquad (3)$$

$$\frac{U - U_\infty}{U_\infty} = \frac{\delta_*}{\delta} f_o(\bar{y}, \delta^+, *), \qquad \text{(Zagarola/Smits)} \qquad (4)$$

where $\bar{y} \equiv y/(D\delta)$. All are written as deficits from the freestream velocity to avoid having to account (functionally at least) for the contribution to U from things that happen nearer the wall in the viscous layer. All scale the coordinate normal to the wall, y, by the local boundary layer thickness, δ, which can be defined from any convenient reference point in the outer layer, as long as a factor of D is included in the definition so the flow can decide how it depends on the particular length scale chosen.‡ The point at which the mean is velocity is 99% of the freestream, δ_{99}, is a convenient choice. Other choices are possible (e.g., δ_{90}, δ_{95}), but of course this will change the value of D in any theory. In fact, the field has suffered from a mixture of choices which makes comparison among theories and experiments difficult, because their ratios tend to be Reynolds number dependent, at least at the relatively low Reynolds numbers of most experiments.

In addition, all the deficit profile functions in their most primitive form necessarily retain a dependence on the local Reynolds number, δ^+, which is really a ratio of the outer length scale, δ, to the length scale for the inner boundary layer (or viscous sublayer), ν/u_*. The functional dependence on this ratio of length scales should vanish in the limit as $\delta^+ \to \infty$.§ And as $\delta^+ \to \infty$, the velocity profiles produced by the velocity deficit profile functions should become true outer velocity profiles, all collapsing to the same curve in the limit (at least for fixed upstream conditions). For finite Reynolds numbers, however, the velocity deficit profile function actually represents the entire velocity profile, in which case δ^+ is a parameter which distinguishes the various profiles as they diverge from one another near the wall.

Finally, the argument "$*$" has been included in the functions just to indicate that there may be other things whose influence on the shape of the profile cannot be ruled out a priori, the leading candidates among them being boundary-layer transition trips and other upstream conditions, freestream turbulence, etc. The very presence of this extra argument means we must be very careful about comparing different experiments and DNS simulations with each other. It will be interesting if they give the same results, but we have no reason to expect they will, at least without making additional assumptions. Because of this we should not attempt to establish Reynolds number dependence the way it has often been done, namely by simply measuring at a single position along the surface, then increasing the tunnel speed. We must instead increase δ^+ (or equivalently R_θ which is approximately three δ^+ over the range of most experiments) by measuring farther downstream for *fixed external conditions*. And obviously the inverse procedure must be followed to establish dependence (or independence) of upstream conditions. Only a few have been very careful about this [25,26].

The most familiar deficit (found in all texts) is the proposal on empirical grounds of von Kármán [27] given by Eq. (2). (Note that von Kármán did not include the argument $*$, because he expected independence of upstream conditions.) The same empirical scaling lies at the root of almost all theoretical analysis since Millikan [28] and Clauser [4]. Most experimenters have ignored the importance of the functional dependence on δ^+, but its necessity has been used by theoreticians (e.g., Panton [29] and Afzal and Yajnik [30], among others) who apply singular perturbation theory by expanding f_o either in inverse powers of δ^+ or u_*/U_∞ (which is uniquely linked to δ^+ by the friction law.)

‡This factor, D, was omitted in the GC97 paper, but was included in later versions. Note the factor must be included in any theory, but the precise value of D will depend on the particular theory. D gets absorbed into the additive constant if logarithms result.

§No properly scaled function can blow up in the limit as one of its arguments becomes infinite, because if it does we have scaled it incorrectly.

The second choice of Eq. (3) is probably as old as the study of boundary layers, but was largely abandoned after the 1950s when evidence seemed to favor the Millikan/Clauser approach. It was resurrected by my coworkers and me, not because it collapsed the profiles better, but because it converged to a Reynolds number invariant solution to the RANS equations for the outer boundary layer.

The third choice was an empirical proposal by Zagarola and Smits [31], who noted that it collapsed the velocity profiles (from most experiments at least) remarkably well. Interestingly, the Zagarola/Smits scaling reduces in the limit of infinite local Reynolds number to either of the first two choices, depending on whose theory you believe.

Before proceeding further, I want to make it clear what is at stake here. If Eq. (2) is valid, then the overlap region between the inner and outer boundary layer is logarithmic. This is a simple consequence of the fact that the inner and outer velocity scales are the same, nothing else. If Eq. (3) is correct, then the overlap region is governed by a power law. Again, this is a simple consequence of the fact that the inner and outer velocity scales are different, nothing else. Thus almost the entire debate of the past decade or so was (and still is) really about the velocity deficit. We practice in the field of mechanics, which is all about writing equations that relate forces to motion. Therefore we should expect (even demand) that the governing equations play some role in deciding which approach (or scaling), if any, is correct. Then if the data do not agree, either our data are wrong, our equations are incorrect, our deductions from them flawed, or all of the aforementioned. Real physical understanding (of the type that leads to models) only comes when the equations and data agree. To this point they do not seem to agree, at least for turbulent boundary layers, and especially the zero-pressure-gradient boundary layer.

Amazingly, because we are talking about fluid mechanics here, the Navier–Stokes equations (or their Reynolds-averaged versions) have played little to no role, even in many theoretical papers about boundary layers. But among those who actually bother try to use them, the heart of our disagreements lies in the equations that govern roughly the outer 90% of the boundary layers (at least above R_θ of a thousand or so). For the outer part of the zero-pressure-gradient boundary layer at infinite Reynolds number these reduce to just continuity and the x-momentum equation given by

$$U\frac{\partial U}{\partial x} + V\frac{\partial U}{\partial y} = \frac{\partial}{\partial y}[-\langle uv\rangle] + \frac{\partial}{\partial x}[\langle v^2\rangle - \langle u^2\rangle] \qquad (5)$$

The last term is the streamwise gradient of the normal stress difference, part of which comes from integrating the y-momentum equation across the flow and using it to substitute for the pressure gradient. Because these terms are of second order compared to the others, they are usually neglected in theoretical analyses, GC97 being the exception. The viscous terms are exactly zero in the infinite Reynolds number limit, and it is their neglect that precludes the applicability of this equation to $y^+ < 30$ or so. And of course it is the presence of these viscous terms at finite Reynolds number (and in the turbulence Reynolds stress equations as well) that is accounted for by the parameter δ^+ in all of the aforementioned deficit functions. Note that the mean convection terms (left-hand side) are negligible only in the inner 10% of the boundary layer. This means that in low Reynolds number boundary layers ($\delta^+ < 300$) there is really no part of the boundary layer that is independent of viscous and mean convection effects, i.e., no inertial sublayer. Moreover, values of $\delta^+ > 3000$ are necessary for there to exist a significant region in which both viscosity and mean convection are absent in the single point momentum equation, the so-called inertial sublayer.

Our objective now is to examine whether any of our candidate deficits represent a similarity solution to Eq. (5). If any do, then they represent a proper scaling law because the solution, $f_o(y/\delta, \delta^+, *)$, will reach an asymptotic limit, dependent only on y/δ, and no longer evolving with increasing δ^+. *If our candidate scaling law does not represent a similarity solution, it is at most a "local" scaling law which must change with increasing δ^+, because the governing* *equations say it must*. This was the single most important point of the GC97 paper.

Note that there is no debate about the outer scaling velocity for fully developed pipe and channel flows; it is the friction velocity, u_*. The reason is that these flows are homogeneous in the streamwise direction, so the left-hand side of the momentum equation vanishes identically. The remaining balance between pressure gradient and viscous stress on a control volume dictates that Eq. (2) is the appropriate form [32]. By contrast the outer boundary layer has two velocity scales, both imposed by the boundary conditions: U_∞ (from the outer boundary condition on the velocity), and u_* (from the inner boundary condition imposed by the constant stress layer on the Reynolds shear stress). If we rule out as unthinkable the possibility that $u_*/U_\infty \to \text{const} \neq 0$, then the ratio, u_*/U_∞, must be Reynolds number dependent, so both Eqs. (2) and (3) cannot be equivalent in the limit as $\delta^+ \to \infty$.

Each of the velocity deficit forms can be substituted into Eq. (5) to see if they represent appropriate similarity solutions. We drop for simplicity the normal stress term, but it is possible to consider simultaneously all of the Reynolds stress component equations [6,20,21,33]. If we let U_{so} represent either u_* or U_∞ when it multiplies the profile function, f_o, and we define $\bar{y} = y/\delta$, the result (after some manipulation) is

$$\left[\left(\frac{U_\infty}{U_{so}}\right)\frac{\delta}{U_{so}}\frac{dU_{so}}{dx}\right]f_o + \left[\frac{\delta}{U_{so}}\frac{dU_{so}}{dx}\right]f_o^2 - \left[\frac{U_\infty}{U_{so}}\frac{d\delta}{dx}\right]\bar{y}f_o' - \left\{\frac{d\delta}{dx} + \left[\frac{\delta}{U_{so}}\frac{dU_{so}}{dx}\right]\right\}f_o'\int_0^{\bar{y}} f_o(\xi)\,d\xi = \left[\frac{R_{so}}{U_{so}^2}\right]r_o' \tag{6}$$

where the Reynolds shear stress has been substituted for using the similarity form, $\langle -uv\rangle = R_{so}(x)r_o(\bar{y},\delta^+)$, and the $'$ denotes differentiation with respect to $\bar{y}$. Note that the continuity equation has been integrated to substitute for V, a fourth -order approximation in u_*/U_∞.

All the principals mentioned agree on the ultimate goal: to find a similarity solution to the outer equation. And they agree that there is no similarity solution possible at finite Reynolds number (i.e., including the viscous term). But it is at this point the analyses diverge. *Thus it is how they diverge and whether it matters* that should be the focus of the debate, *not whose curves fit the best over the limited range of Reynolds numbers we can measure*. (As will be shown, everyone's theory can be made to fit almost everything!)

Clauser [4], using the von Kármán deficit of Eq. (2) for which $U_{so} = u_*$, discovered that he did not quite have a similarity result in which all of the parameters fell out, leaving an equation invariant to Reynolds number. In fact, he was left with the problem of terms involving du_*/dx and u_*/U_∞. He chose (in the absence of obvious alternatives) to neglect them, and at first glance this appears quite reasonable. The ratio, u_*/U_∞ is quite small (typically a few percent or less). Because these neglected terms occur to first order in his equations, one can conclude that the velocity deficit represented by Eq. (2) is a first-order similarity solution to the outer equations.

But is a first-order solution reasonable, or even viable? GC97 argued it is not. They argued that it is the momentum integral equation which governs the overall boundary layer and it is second order in u_*/U_∞, i.e.,

$$\frac{d\theta}{dx} = \left(\frac{u_*}{U_\infty}\right)^2 \tag{7}$$

It is commonly (and erroneously) believed that because the main contribution to θ comes from near the wall, then the main contribution to $d\theta/dx$ must also come from near the wall. In fact, as my colleague T. Gunnar Johansson has recently made very clear in a seminar at Chalmers, the opposite is true (see also Johansson and Karlsson [34]). Because the near-wall region grows very much more slowly than the outer part of the boundary layer, almost all of the contribution to $d\theta/dx$ comes from distances far from the wall. Incidentally, this also explains the difficulty of many experimenters in obtaining an experimental momentum balance: in addition to the obvious problem of not measuring profiles at different distances downstream (to allow an estimate of $d\theta/dx$ at all), they concentrated their measurements close to the wall and simply did not have enough resolution in the outer part of the boundary layer where the main differences in the integral occur (e.g., Österlund [18]). It also supports the GC97 conclusion that the outer equations must be analyzed to at least second order in u_*/U_∞, because it is the changes in the outer flow that make the biggest contribution to $d\theta/dx$, and therefore dictate the evolution of the inner.

Based on this line of reasoning, GC97 argued that neglecting terms of order u_*/U_∞ is tantamount to assuming the outer boundary layer does not grow, or is in effect like a channel. Therefore it is not surprising to me that people who believe in the classical analysis have a problem finding universal values for the constants in the so-called Coles wake function, which accounts for part of the variation of the mean velocity in the outer part of the boundary layer. (Gad-el-Hak and Bandyopadhyay [14] document this problem nicely.) The same folk also believe the overlap layers of boundary layers, channel, and pipe flows to be the same, often without realizing they have "assumed" them to be the same. But even if they are wrong, they are almost right, because the neglected terms really are small, and the boundary layer (especially the inner part) is almost parallel. But "almost" is a local idea (or an engineering approximation, if you will), and at best an uncertain proposition on which to begin a deductive theory. Recognizing this, a number of authors have tried to develop higher-order theories using perturbation expansions in powers of $1/\delta^+$ or u_*/U_∞ (e.g., Panton [29], Afzal and Yajnik [30], and others), all beginning with classical deficit law and ending with it as the asymptotic limiting solution.

So what about the alternative given by Eq. (3)? It most certainly does *not* provide a better collapse of the data for the zero-pressure-gradient boundary layer (at least according to the recent data), although curiously enough it may for pressure-gradient boundary layers [20,23,35,36]. If this is the case then why is this form of the outer velocity deficit even of interest, having previously been discarded by both von Kármán and Clauser, among others? The reason is that it is a similarity solution (perhaps the only solution) to Eq. (6), and it remains a solution even if the pressure gradient is included. Moreover, the solution can be extended to the component Reynolds stress equations [6], and can be shown to be valid to at least *third* order in u_*/U_∞. This means that Eq. (3) (or something proportional to it) is the only form the outer deficit can have and still be independent of Reynolds number in the limit (as $\delta^+ \to \infty$). Or said another way, data scaled in any other way will not collapse in the limit as the Reynolds number becomes infinite, unless of course the boundary layer equations are wrong or we have used them incorrectly. Obviously these questions are what we should be debating if the data appear to not agree.

Because the asymptotic scaling laws are different, the two velocity deficit laws produce very different asymptotic behavior of the momentum thickness, displacement thickness, and shape factor. The actual functional dependencies can be obtained by using empirical fits to the profiles and by integrating the friction laws in the manner first proposed by Clauser [4]. Of most interest though is the asymptotic behavior as $\delta^+ \to \infty$. The velocity deficit scaled with u_* produces the following asymptotically (Clauser [4]):

$$\delta_* = A_*\delta\frac{u_*}{U_\infty} \tag{8}$$

$$\theta = A_*\delta\frac{u_*}{U_\infty}\left[1 - B_*\frac{u_*}{U_\infty}\right] \to A_*\delta\frac{u_*}{U_\infty} \tag{9}$$

$$H \to 1 \tag{10}$$

where A_* and B_* are integrals involving *only* the limiting outer velocity profile, f_o. These can be compared to the GC97 results for the outer profile scaled with U_∞ which reduce in the limit to

$$\delta_* = I\delta \tag{11}$$

$$\theta = II\delta \tag{12}$$

$$H \to \text{const} > 1 \tag{13}$$

where I and II similarly involve integrals of only the outer velocity profile, f_o. (Note that it is not a valid test of either theory to integrate the actual velocity profile instead of the limiting form of it, because then both sets of equations are satisfied by default.)

Which best describes the data? GC97 found excellent agreement with the data to that point in time, but truth-to-tell the earlier theory can be made to fit pretty well also. The new data from IIT (unpublished, but presented in various forums recently [19,37]) suggest that the classical theory works best, but as will be shown, these data (in their current form at least) do not satisfy the momentum integral equation of Eq. (7). So at this point the results are inconclusive (at least for the zero-pressure-gradient boundary layer). The range of Reynolds numbers is small enough that almost all theories work pretty well, the curse of all turbulence research it seems. Moreover the data are very, very far from any kind of asymptotic behavior. So like with the form of the velocity deficit itself, we are left only with intellectual arguments to guide us. GC97 (and subsequent papers) argued that unity was not a physically realistic limit for the shape factor because it implies no boundary layer profile at all, hardly a reasonable limit if one increases Reynolds number by increasing distance along a surface. They further argued that because the momentum and displacement thickness clearly grow downstream and do not go to zero, the fact that δ was blowing up relative to them (because u_*/U_∞ is asymptotic to zero) meant that the entire approximation was invalid. To the best of my knowledge this criticism of the classical analysis has not been rebutted.

We have one deficit proposal left, the Zagarola/Smits (ZS) proposal of Eq. (4). Interestingly, the ZS-scaling can produce either of the other two deficits, depending on which is correct. If $\delta_*/\delta \to u_*/U_\infty$ as the Clauser/Millikan theory suggests, then the ZS-scaling is asymptotic to Eq. (2). If, on the other hand, $\delta_*/\delta \to$ const as GC97 argue, then the ZS-scaling is asymptotic to Eq. (3). So it would seem that everyone should be happy because no matter who is right, the ZS-scaling will make them think they are. Also, because differences in upstream conditions tend to appear in the ratios of δ/δ_*, δ/θ, etc., the ZS-scaling seems to provide a way to remove these. Note that when investigators fix the distance downstream and vary only the tunnel speed, this is precisely what varying the upstream conditions means, because the Reynolds numbers of all trips and other upstream influences change.

Castillo and coworkers [35,36,38,39] have shown the ZS-scaling to collapse mean velocity data for boundary layers with pressure gradient, different upstream conditions, and even roughness. To add further weight to the argument for the ZS-scaling, Wosnik and George [40] showed that it collapsed all the GC97 theoretical velocity profiles over orders of magnitude greater range in δ^+ than all experiments. Recognizing that the success of the ZS-scaling could not just be coincidence, Wosnik and George were able to show that if the velocity deficit function, $f_o(\bar{y}, \delta^+, *)$ could be separated into the product of two functions, one containing the y-dependence, the other independent arguments, then the ZS-scaling resulted. This is easy to show, and we can learn something I did not realize before I began to prepare this paper. Let $f_o(\bar{y}, \delta^+, *) = g_o(\delta^+, *) f_{o\infty}(\bar{y})$. Substituting into the definition of the displacement thickness, and ignoring the small contribution from the wall layer yields

$$U_\infty \delta_* \equiv \int_0^\infty [U_\infty - U]\,\mathrm{d}y \tag{14}$$

$$\approx U_\infty \delta \int_0^\infty g_o(\delta^+, *) f_{o\infty}(\bar{y})\,\mathrm{d}\bar{y} \tag{15}$$

$$= g_o(\delta^+, *) U_\infty \delta \int_0^\infty f_{o\infty}(\bar{y})\,\mathrm{d}\bar{y} \tag{16}$$

Because the integral can only have a single universal value (by our hypothesis that $f_{o\infty}$ has no other arguments than $\bar{y}$), it follows immediately that

$$g_o(\delta^+, *) \propto \frac{\delta_*}{\delta} \tag{17}$$

which is exactly the factor needed to produce Eq. (4) from Eq. (3). Now to see something new: because g_o does not depend on $\bar{y}$, the product of functions, $g_o(\delta^+, *) f_{o\infty}(\bar{y})$, can be substituted for $f_o(\bar{y}, \delta^+, *)$ in Eq. (6) without changing the overall conclusions (except for some terms involving derivatives with respect to δ^+ which are asymptotically zero). Because some terms depend only on f_o whereas others depend on products of f_o with itself, its integral, and its derivative, *the presence of g_o does change the coefficients in the mean momentum equation*. Because the equation is now different for different g_o (corresponding in turn to different upstream conditions), it can produce different solutions for the mean velocity and Reynolds stress profiles. These might not collapse from one experiment to another or even over a range of Reynolds numbers, at least if scaled only by U_∞. But all these mean velocity profiles should collapse together using the ZS-scaling, because it is the equilibrium similarity solution. This is exactly as observed, at least by Zagarola/Smits, Castillo, Wosnik, and others.

III. Friction Laws

The inner velocity profile function of Eq. (1) can be combined with either of the velocity deficit laws to achieve a corresponding friction law. Classical singular perturbation theory is of limited value for this, because it begins by setting out a set of inner and outer equations which must first be solved, then the solutions of each are stretched to cover an overlap region. The last step involves a composite solution obtained by summing (or multiplying) the two solutions together, then subtracting (or dividing) by the common part. Unfortunately for turbulence, one must first have a turbulence closure model before the inner and outer equations can be solved, and the model in essence presumes the answer. Millikan [28] skirted the problem nicely by recognizing that he did not really need to know the solutions to the inner and outer equations to find the common part, only how they scaled. The same procedure was used by Clauser, but much more elegant versions have been derived since. All more or less follow the methodology detailed by Tennekes and Lumley [41] which matches derivatives of the velocity in the limit as the ratio of scales, δ^+, becomes infinite (c.f., any of the numerous recent texts on turbulence.) The exception to this was the approach invented in GC97 which they termed *near-asymptotics*. Their problem with the usual approach (and my problem still) is that I do not know of any singular perturbation problem I can solve analytically for which the classical turbulence matching methodology gives me the correct answer. Apparently the classical methodology only works for problems which we cannot solve analytically, a rather discomforting situation to say the least.

The GC97 near-asymptotics approach was *not* invented to obtain a power law solution; that can be obtained the usual way by matching derivatives in the limit of infinite Reynolds number. It was invented for two reasons: First because, based on our early curve fits [5], we were concerned about the apparent dependence of the exponent, γ, on δ^+. And second we were concerned about the possibility that the power exponent might go to zero, which the classical approach could not handle. So we invented an alternative procedure which solves for the overlap solution at finite Reynolds number (hence the term near-asymptotics). As an aside, we applied the same methodology to two other problems, the turbulence energy spectrum in homogeneous turbulence, and to pipe and channel flows. In both cases, the results were quite successful. For the former, near-asymptotics produced finite Reynolds number deviations from the $k^{-5/3}$-law (in fact, $k^{-5/3+\mu(Re)}$, Gamard and George [42]) that were in wonderful agreement with the almost simultaneous experiments of Mydlarski and Warhaft [43]. And for the latter, the logarithmic velocity and friction laws showed near perfect agreement with all the friction and velocity data for the superpipe and DNS channel simulations over a range of R^+ from 198 to 60,000 (Wosnik et al. [32]). Moreover, the same approach yielded quite satisfactory results for the plane walljet as well (George et al. [33]). I am somewhat disappointed that so far no mathematician to my knowledge has pursued the implications of near-asymptotics, because the methodology seems considerably more general than the usual approach and appears to include the familiar Poincaré expansions as a special case.

So what are the results of these two competing scaling laws and approaches for the zero-pressure-gradient turbulent boundary layer? Matching Eqs. (1) and (2) using the standard approach yields a logarithmic friction velocity profiles in the overlap region and a friction law given by

$$\frac{U - U_\infty}{u_*} = \frac{1}{\kappa}\ln(\bar{y} + \bar{b}) + B_o \tag{18}$$

$$\frac{U}{u_*} = \frac{1}{\kappa}\ln(y^+ + b^+) + B_i \tag{19}$$

and

$$\frac{U_\infty}{u_*} = \frac{1}{\kappa}\ln\delta^+ + [B_i - B_o] \tag{20}$$

where B_i and B_o are the additive parameters for the logarithmic overlap profiles in inner and outer variables, respectively. Note that in the classical theory these are assumed constants, but as noted by Wosnik et al. [32], they may also be very weak functions of the local Reynolds number and only asymptotically constant. The offset in y, either b^+ or $\bar{b} = b^+/\delta^+$, is a necessary consequence of the need to be invariant to choice of origin [17]. For the log law in boundary layers it is usually taken to be zero, but the value of $b^+ = -7$ seems to work better for channel and pipe flows [32]. Note that near-asymptotics can be applied to this matching as well. The results will differ only in that there will be an additive term which varies inversely with $\ln D\delta^+$, and vanishes as $\delta^+ \to \infty$, in exactly the same manner as for the pipe [32].

One of my major complaints about current boundary-layer research is that not a single one of the advocates of the log law for boundary layers has shown me to-date, in spite of repeated prodding, that they can deduce the additive constant for the friction law $(B_i - B_o)$ using B_i and B_o from the inner and outer velocity fits as the log theory demands. In particular, it is B_o that is always missing, which seems quite suspicious actually because it is in the outer flow representation of the log law for pipes and channels where the real Reynolds number dependence appears (c.f., Wosnik et al. [32]). A first attempt at this using some of the recent data of Österund [18] is included in Table 1. In the absence of this information, there is no reason to believe the log boundary layers results are more than just curve fits, and certainly not validation of a theory. This is particularly troubling because these "universal log constants" have changed as the Reynolds number of experiments has increased. (Incidentally, this is exactly what GC97 predicted would happen.)

The GC97 approach applied to boundary layers yields power law solutions for both the velocity in the overlap region and for the friction law:

$$\frac{U}{u_*} = C_i(y^+ + a^+)^\gamma \tag{21}$$

$$\frac{U - U_\infty}{U_\infty} = C_o(\bar{y} + \bar{a})^\gamma \tag{22}$$

and

$$\frac{U_\infty}{u_*} = \frac{C_i}{C_o}\delta^{+\gamma} \tag{23}$$

where C_i, C_o and γ are functions of δ^+. Like the log law, the coordinate must be offset, this time by a^+ or $\bar{a} = a^+/\delta^+$. GC97 suggest that $a^+ \approx -16$, and that seems also to be consistent with the new data discussed next. Near-asymptotics provides these functions through the constraint equation which links them together (see GC97 or Wosnik [44] for a slightly improved version). In particular, these must satisfy

$$\ln D\delta^+ \frac{d\gamma}{d\ln D\delta^+} = \frac{d}{d\ln D\delta^+}\ln\frac{C_o}{C_i} \tag{24}$$

where all parameters must be asymptotically nonzero and constant for similarity to be possible and in order to satisfy Kolmogorov's hypothesis that the local dissipation rate is finite at infinite Reynolds number (i.e., $\varepsilon \propto k^{3/2}/L$).

The reason for the power instead of a log is quite simple: if the inner and outer scales are the same, a log results; if they are different, a power law is dictated. There are two sets of solutions to Eq. (24), one with constant parameters (which at least in 1997 did not seem to represent the data), and a second which depends on local Reynolds number. For the latter, these power law solutions are a bit more complicated than the simple log law with constant parameters (if they are indeed constant), but complexity is a small price to pay if the result is a correct representation of the physics.

The Reynolds number dependent solution to Eq. (24) can be expanded in powers of $1/\ln D\delta^+$, the leading term of which yields, after substitution into the friction law,

$$\frac{U_\infty}{u_*} = \frac{C_{i\infty}}{C_{0\infty}}(D\delta^+)^{\gamma\infty}\exp\left(\frac{-A}{(\ln D\delta^+)^\alpha}\right) \tag{25}$$

where $C_{i\infty}$ and $C_{o\infty}$ are the infinite Reynolds number limits of the velocity profile parameters in the overlap region, as is the exponent, γ_∞. (Note that Wosnik [21] developed a higher-order solution which eliminated much of the empiricism, particularly the dependence of C_o on δ^+, but that will not be considered in this paper.) The values suggested by GC97, based entirely on the data of Smith and Walker [8] and Purtell et al. [2] were $\alpha = 0.46$, $A = 2.9$, $C_{i\infty} = 55$, $C_{o\infty} = 0.897$, and $\gamma_\infty = 0.0362$, although they noted that $C_\infty = 57.5$ might be better. (The difference in $C_{i\infty}$ was due to the emergence of better near-wall velocity data for the plane walljet [33,45,46] while our work was in progress.) It is important to note that all of these values were determined independent of wall friction data, and in fact were determined only using the velocity data available at the time (and then only the part of it we believed). We did *not* include any friction data in the optimization of the constants

Table 1 Comparison of power to log fits to $50 < y^+ < 0.2\delta_{95}$ for Österlund's $U_e = 27$ m/s experiment. Last two rows show rms errors for each fit over same region.

R_θ	5,486	7,970	10,502	12,886	15,182	14,207	15,165
C_o	0.8705	0.8969	0.8997	0.9063	0.9123	0.91578	0.9135
γ	0.1246	0.1289	0.1263	0.1261	0.1249	0.1228	0.1271
C_i	9.187	9.014	9.145	9.217	9.315	9.502	9.19
κ	0.4091	0.3874	0.3785	0.3808	0.3895	0.3821	0.3849
B_i	4.684	4.061	3.728	3.954	4.328	4.188	4.108
B_o	−2.380	−3.341	−3.254	−3.195	−3.320	−3.001	−3.294
$\ln A/\kappa$	−2.699	−2.970	−3.110	−3.138	−3.080	−3.095	−3.000
power, %	$1.15e-03$	$5.09e-04$	$1.14e-03$	$2.08e-03$	$1.23e-03$	$1.22e-03$	$1.04e-03$
log, %	$9.65e-03$	$1.08e-03$	$1.77e-03$	$1.13e-03$	$1.25e-03$	$2.19e-03$	$1.77e-03$

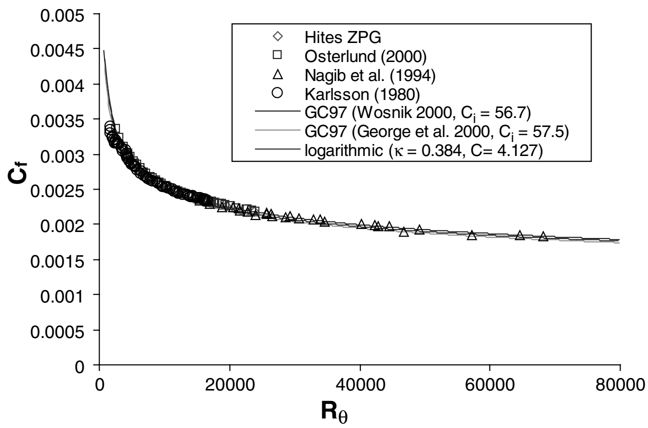

Fig. 1 c_f vs R_θ from oil film data [19] compared to Karlsson [7] and GC97 theory . The different theoretical predictions were entirely due to the value of C_i, which was chosen determined originally to be 55 [6], but subsequently modified to 56.7 [21] and 57.5 [33]. Also shown is Eq. (26) with the values suggested by Nagib et al. [19].

because we felt they had all been contaminated by the so-called Clauser method results. Therefore the friction law of Eq. (25) was definitely not a fit to the data, but truly a *prediction* in any sense of the word, a true rarity in turbulence.

So how did we do? Happily through the efforts of Hassan Nagib and Arne Johanson and their students Michael Hites, Chris Christophorou, and Jens Österlund to make oil films work in their facilities at KTH and IIT we can now say. All of the data (except Rolf Karlsson's) in Fig. 1 (replotted from one provided to me by Hassan Nagib) were obtained well after the publication of GC97. Also shown on the plot is a set of previously unpublished data from the Ph.D. dissertation in 1980 of my recently deceased and dearly beloved colleague, Rolf Karlsson. These data were never accepted for publication, in spite of repeated submissions, because they did not agree with the conventional wisdom and "accepted" values. Rolf's data points virtually overlay the more recent data. Happily Rolf saw this plot just before his death.

The agreement between our prediction and these experiments is spectacular, to say the least, especially because no curve fitting to the friction data was involved, nor were any previous friction data included in the prediction. And the agreement is over the entire range of Reynolds numbers. This was not a surprise to us, because the same predictions agreed well with the long-discarded data [12] of Schultz-Grunow [10] and Wieghardt [11], neither of which were used in choosing the parameters. Also, Wosnik [21] showed agreement of the theory with the data of Österlund [18] at lower values of R_θ. All sets taken together certainly vindicate the judgement of GC97 in choosing to ignore the shear-stress data existing at the time (because we had no idea which to believe), and focus instead on using the raw velocity data. Moreover, by predicting the values of shear stress to be determined later using only the velocity-determined constants, this in effect closed the loop and showed the theory to be internally consistent: at least for the velocity profiles available to us at the time.

IV. Some Real Surprises from Curve Fits and the Momentum Integral

So is this the end of the story? Interestingly not, and for several reasons. Firstly, as noted by Nagib et al. [19], almost all the theoretical and empirical curves can be made to look pretty much like the data by optimizing the choice of empirical constants in them. In fact, a variety of choices of the GC97 constants works just as well, and even the power law of Eq. (23) with *constant* coefficients fits to within 0.1% average absolute error. Particularly surprising to me, at least, is the almost perfect agreement between the data and the fitted plot of the logarithmic curve given by

$$\frac{1}{c_f} = \frac{1}{\kappa}\ln R_\theta + C \tag{26}$$

Note that this is *not* the same as Eq. (20) because it depends on R_θ instead of δ^+, but it can be related (at least asymptotically) using Eq. (9) [or for that matter Eq. (12). The parameters were chosen by optimization to be $\kappa = 0.38$ and $C_1 = 4.1$. To satisfy myself that these curves could really be this close, I put both the GC97 friction and the log relation side-by-side on a spreadsheet with δ^+ as the independent variable. I determined the value of R_θ by using the methodology of GC97 and confirmed these values by direct integration from the momentum integral using the friction curve. Then I compared the resulting values of c_f for a given value of R_θ (or equivalently, δ^+). For the value used in the plots of $C_{i\infty} = 56.7$, the two friction data were in the nearly constant ratio of 0.98 with a standard deviation of 0.2% over a range from $200 < R_\theta < 300,000$! If the alternative value suggested by GC97 of $C_{i\infty} = 57.5$ is used, then the ratio is 1.00 with an rms error of 0.21% over the same range. If the range of the data is increased to a million, the curves still differ by only about 1% (less if the lower value of $C_{i\infty}$ is used), thus confirming what Nagib et al. [19] claimed in their 2004 Perryfest presentation at Queens University.

To me, at least, this is nearly unbelievable! How can two analytical forms with such very different functional dependencies produce virtually identical results over such a large range? One might conclude that the two theories, log and GC97, must be the same. As noted by GC97, this appears to be possible only if $\gamma_\infty \to 0$ as $\delta^+ \to \infty$, and that appears to violate Kolmogorov's idea of finite local energy dissipation at infinite Reynolds number. This also would imply that $u_*/U_\infty \to$ const as $\delta^+ \to \infty$, which would in turn require an infinite value of κ in the log theory. So we are left at this point with a dilemma: How can both theories appear to be correct when they also at least appear to have different limiting behavior?

The second interesting and most puzzling point about the agreement of the new Nagib et al. [19] friction data is that they do not satisfy the momentum integral equation. Figure 2 shows the directly measured skin friction plotted against that determined from the authors' own fit to their momentum thickness data. The discrepancy at the highest Reynolds numbers is approximately 40%! This is far more than might be considered reasonable experimental error (c.f., Johansson and Karlsson [34]), and it is even more puzzling given the care of the IIT group to insure a zero pressure gradient. Shown for comparison is the Österlund [18] data. For these data, which reach only Reynolds numbers of less than half the more recent data, the difference between momentum integral and oil film estimates is within a few percent. In fact, if the momentum integral determination from the fit to the Österlund curve is extended, this lies quite close to the IIT oil film data. It might be argued that differentiating empirical fits to the momentum thickness data introduces large errors (even though it does not seem to do so for the Österlund data). Therefore the same information has been presented another way, this time by integrating the wall shear stress (from the oil film data) and comparing it to the measured momentum thicknesses. This is

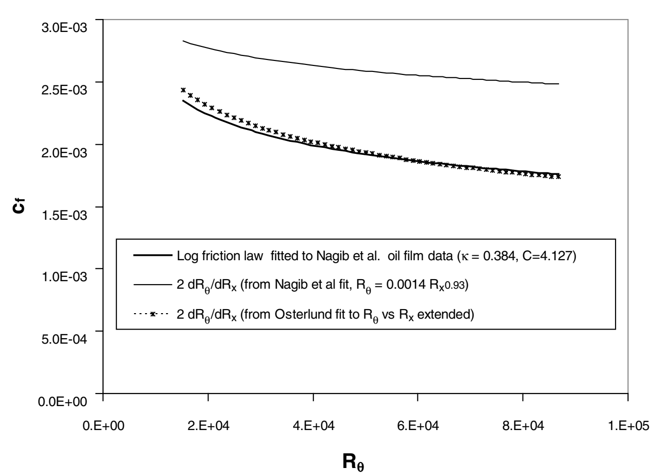

Fig. 2 Skin friction from oil film and Eq. (7) using data of Österlund [18] and Nagib et al. [19] Note the large discrepancy between the oil film and momentum integral results for the latter.

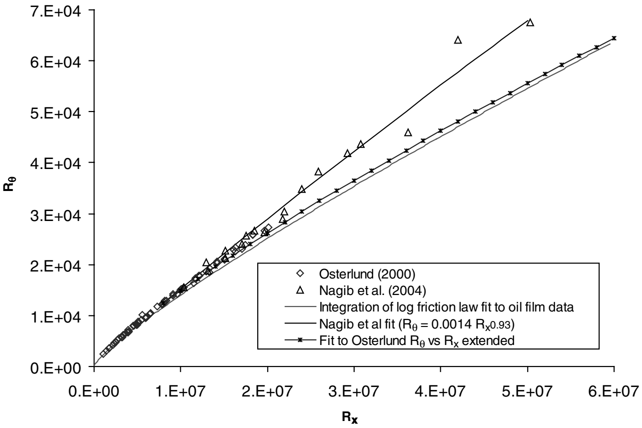

Fig. 3 Measured momentum thickness compared to that obtained by integrating Eq. (7) using oil film skin friction data for data of Österlund [18] and Nagib et al. [19] Note the large discrepancy between the oil film and momentum integral results for the latter.

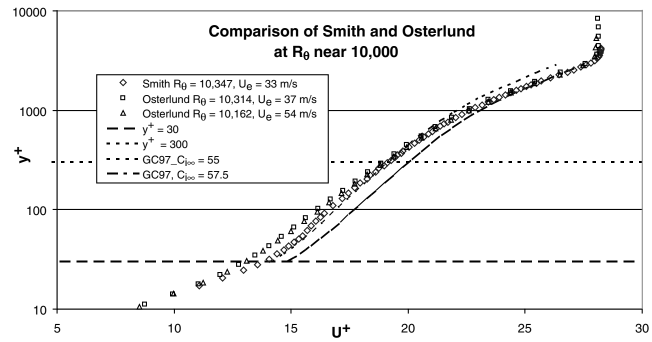

Fig. 4 Three velocity profiles at $R_\theta = 10,000$ from Österlund [18] and Smith [47] along with GC97 profiles. The horizontal lines at $y^+ = 30$ and 300 mark the lower and upper limits of the mesolayer.

presented in Fig. 3, and again the results are the same. For the IIT experiments, the momentum thickness determined from the integrated oil film shear stress data falls substantially below that actually measured. By contrast the KTH data show reasonable agreement. And again the curve fit to the Österlund experiment momentum thickness data provides better agreement with the IIT integrated friction than does IIT's own data. This is indeed a mystery, and raises serious questions about mean velocity measurements in the IIT experiments. This will be addressed further in the next section.

V. Velocity Profiles

One might imagine from the consensus about the friction coefficient that there might be a corresponding consensus about the velocity profiles. Unfortunately, this is not the case. Since the GC97 paper, three sets of experiments have been carried out, in part to try to resolve some of the issues raised by it. The first of these was carried out at Princeton by R. Smith [47] and L. Smits at fixed upstream conditions. In all, ten profiles were measured for $4,601 \leq R_\theta \leq 13,189$. (Note the data were not available when the analysis of GC97 was performed and it was under review before these results became available.) The data are on the web and can be readily accessed by anyone. The second was carried out by J. Österlund and A. Johansson (with considerable help from H. Nagib) using the MTL tunnel of KTH, which has the advantage of a 7 m long test section. This is an extensive set of data with 70 profiles measured at four downstream positions and 10 different freestream velocities, some of which were repeated. Values of R_θ ranged from 2,532 to 27,320. The mean velocity profiles have been made available to the public, but unfortunately (for reasons that are not clear), the Reynolds stress profiles have not. Finally, there were experiments carried out by H. Nagib and M. Hites at IIT in the mid-90s, and although they have frequently been cited to argue for the log law (and against all other alternatives), the data have never been made publicly available. The same is true for the more recent experiments of Nagib and Christophorou [19] as well, and in fact the plots shown in the preceding section were taken from a pdf file of a presentation by Nagib. On the other hand, the problems cited in the preceding section with the momentum integral balance of these measurements are most certainly related to problems with either the outer flow or the velocity measurements themselves, so perhaps the data would not have been so useful anyway.

Thus, of necessity, this paper will concentrate on the Österlund data and that of Smith. The uncertainties about the IIT data are particularly unfortunate, because both the KTH and Princeton experiments are lacking sufficient detail in the outer 50% of the boundary layer to determine the crucial outer length scale parameters (δ_{99} or even δ_{95}) to within even a few percent. For example in the Österlund experiment, typically only three or at most four data points were measured outside of $y/\delta > 0.5$. This shortcoming is a tragedy (at least from a theoretical perspective), because the experiment was an otherwise heroic effort.

Figure 4 illustrates the theoreticians' problem with the velocity profiles. Three experimental velocity profiles are shown at approximately the same value of $R_\theta = 10,000$, two from Österlund ($R_\theta = 10,314$, $U_e = 36.4$ m/s and $R_\theta = 10,162$, $U_e = 53.7$ m/s) and one from Smith ($R_\theta = 10,347$, $U_e = 33.1$ m/s). The profiles have been plotted in inner variables, $y^+ = yu_*/\nu$ and $U^+ = U/u_*$, because there seems to be consensus about the proper friction law. The ratio u_*/U_e was 0.0356 for both of the Österlund experiments and 0.0354 for the Smith experiment, so the differences cannot be attributed to the method of shear-stress determination. The two Österlund profiles differ only slightly from each other, but both differ substantially from the Smith profile. Similar differences exist for all the other profiles from both data sets when compared at near equal values of R_θ. Which profile, if any, is correct? The reason for the difference is not at all obvious, because both experimental groups can claim considerable expertise. In the absence of Reynolds stress measurements and a careful balance of the differential momentum equation, we simply cannot know which (if either) is correct. Both sets of data show Reynolds number dependencies inside the inertial sublayer, which is probably mostly due to the variation of the ratio of probe size to viscous length scale. Or it may be due to the differing characteristics of hot wires and flattened total head tubes (Princeton) close to the walls. The new measurements from IIT (if and when they are made available) will probably not resolve this, because they can hardly be argued to be "independent" given the close interaction of this group and KTH. It should be noted that in spite of the well-known problems of using hot wires near walls, the careful LDA measurements of my colleague T. Gunnar Johansson [34] show excellent agreement with the hot-wire profile of Österlund at $R \approx 3000$. So at this point there is some reason to believe that the hot-wire data might be superior, at least at low Reynolds numbers. The EU-sponsored multipartner WALLTURB experiment currently underway in Europe under the supervision of Michel Stanislas using modern optical techniques in the 20 m boundary layer facility at Lille has perhaps the best chance of resolving these discrepancies and uncertainties.

Also shown on the figure are the velocity profiles proposed by GC97, which are clearly unlike either set of recent measurements. When $C_{i\infty} = 55$, the agreement with the Smith profile near the wall is quite reasonable, but it differs substantially in the outer flow. By contrast, with $C_{i\infty} = 57.5$ the fit near the wall is quite unsatisfactory, but quite reasonable outside, presumably because it gets the friction law right (as noted in Sec. III). But these provided an excellent fit (see Figs. 14–17 of GC97) to Purtell et al. [2] at low Reynolds numbers and the Smith and Walker [8] measurements over the entire range of Reynolds numbers available (up to $R_\theta = 48,292$). The former used small hot wires, whereas the latter used total head tubes which were even more flattened than in the Princeton experiment. In light of this, it seems clear that the Reynolds number dependence incorporated by GC97 into the parameters was indeed due to the total head tube errors and not the flow.

All of this raises the question: how could GC97 theory could have made such a remarkable prediction of the friction data of Österlund and the IIT experiments, and yet the inertial sublayer (and mesolayer) profiles be so wrong? The answer lies in the manner in which GC97 decided what to use for friction data, because no reliable data were available. First they computed the momentum integral using the Smith/Walker data (in outer variables only), and computed the wall skin friction from $\mathrm{d}\theta/\mathrm{d}x$ and Eq. (7). Then they used that friction data to determine the scaled velocity profiles in inner variables. Finally, they determined the overall Reynolds number dependence to fit the Smith/Walker and Purtell profiles in both inner and outer variables. If the new data are to be believed (either set), the near-wall Smith/Walker data must be substantially in error (perhaps because of the very flattened total head tubes used and the consequent low Reynolds number on them). On the other hand, the Smith/Walker, Smith, and Österlund data all more or less agree on the shape of the velocity profile in the outer part of the boundary layer (say, outside of $y/\delta_{99} > 0.1$). But, as noted, this is the part of the boundary layer on which $\mathrm{d}\theta/\mathrm{d}x$ is primarily dependent, because this is where the largest changes occur. Obviously this is why our determination of the Smith/Walker shear stress was correct, even though their profiles near the wall were not. And because of the errors in the latter, the GC velocity profiles (and constants) are also in error, at least in the manner in which they split the Reynolds number dependence between the inner and outer flow.

So it is clear we must separate the question of the specific choice of constants and Reynolds number dependence from the question of whether the basic profile of GC97 has the right dependence on y. We examine this first by considering the composite velocity profile (overlap plus wake function) given by

$$U^+ = \frac{U}{u_*} C_i (y^+ + a^+)^\gamma + \left\{\frac{U_\infty}{u_*}\right\}(0.99 - C_o)\bar{y}\sin\left(\frac{\pi\bar{y}}{2}\right) \tag{27}$$

where U_∞/u_* must be determined from Eq. (23). The second term is the "wake" function and is a slight modification of that used by GC97 who used $(1 - C_o)\bar{y}\sin(2.03\bar{y})$. The fits are shown in Fig. 5 on top of both sets of data. They are within 0.3% for the Österlund data, but depart from the Smith data near the wall. For the Smith profile, $\gamma = 0.113$, $C_o = 0.890$, and $C_i = 10.1$, whereas $\gamma = 0.129$, $C_o = 0.923$, and $C_i = 9.16$ for the Österlund profiles. The value of $a^+ = -16$, the choice of GC97, proved to be near optimal for all profiles. Clearly both sets of experimental profiles can be described with only a small change in the parameters from their original values.

Interestingly, log profiles (also with a velocity deficit) can similarly be made to fit both profiles, as illustrated in Fig. 6, which shows a comparison of power-law-plus-wake-function and log-law-plus-wake-function fits to the Österlund velocity profile for $R_\theta = 15,154$, $U_e = 27$. The curves and data are indistinguishable. Both curves fit the data with an average rms error of 0.4%, and both both sets of "constants" (power and log) produce the measured value of u_*/U_e as well as correct integral parameters. The log-plus-wake profile uses a Coles wake function given by

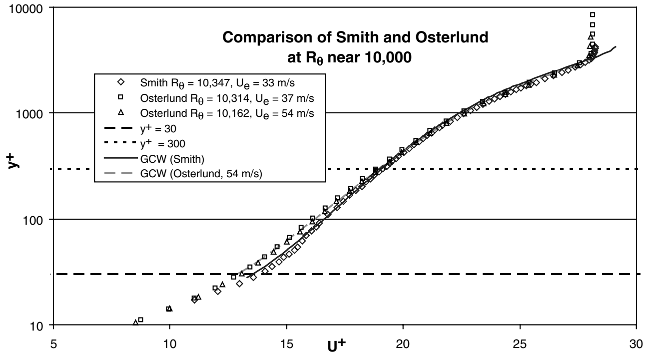

Fig. 5 Three velocity profiles at $R_\theta = 10,000$ from Österlund [18] and Smith [47] along with fits of Eq. (27). The horizontal lines at $y^+ = 30$ and 300 mark the lower and upper limits of the mesolayer.

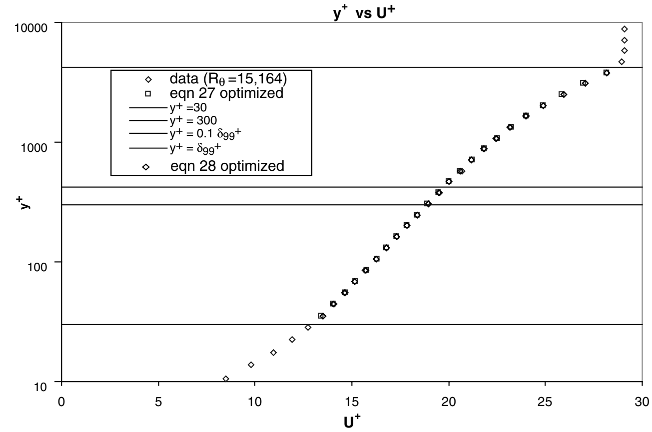

Fig. 6 Fits of power-plus-wake-function and log-plus-wake-function to data of Österlund [18], $R_\theta = 15,154$, $U_e = 27$ m/s. The horizontal lines at $y^+ = 30$ and 300 mark the lower and upper limits of the mesolayer, whereas that at $y^+ = 0.1\delta^+$ marks the approximate outer limit of the overlap region.

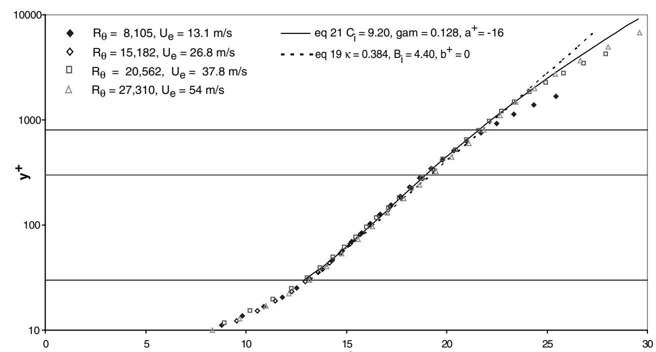

Fig. 7 Velocity profiles from Österlund [18] at different R_θ and U_e, along with fits of Eqs. (19) and (21). Data: $R_\theta = 8,104$, $U_e = 13.1$ m/s; 15,182, 26.8 m/s; 20,562, 37.8 m/s; 27,310, 54 m/s. Theory: $C_i = 9.2$, $\gamma = 0.128$, $a^+ = -16$; $\kappa = 0.384$, $B_i = 4.40$, $b^+ = 0$. The horizontal lines at $y^+ = 30$ and 300 mark the lower and upper limits of the mesolayer, whereas that at $y^+ = 0.1\delta^+$ marks the approximate outer limit of the overlap region for the $R_\theta = 27,310$ profile.

$$U^+ = \frac{1}{\kappa}\ln(y^+ + b^+) + B_i + \Pi\sin^2\left(\frac{\pi D_{\log}\bar{y}}{2}\right) \tag{28}$$

For this data set $D_{\log} = 1$ and $b^+ = 0$ were the optimal values.

VI. Overlap Region: Log vs Power

Österlund et al. [48] claim that the Österlund data prove conclusively that the power law is not viable, and that only the log law works with these data. Figure 7 plots the Österlund mean velocity data in inner variables at the farthest downstream position (5.5 m) for four different tunnel speeds and Reynolds numbers ($R_\theta = 8,105$, 15,182, 20,562, and 27,310 corresponding to 13.1, 26.8, 37.8, and 54 m/s, respectively). Also shown are best fit power and log laws with a single set of constants for both (i.e., the slight Reynolds number dependence has been ignored). The values of the offset were found by optimization to be $a^+ = -16$ for the power law (exactly what GC97 said it was), and zero for the log law (exactly the value most log advocates prefer). Clearly, in contradiction to the earlier claims, it is impossible to distinguish between the log and power law results.

Table 1 summarizes the results of a regressive fit of both the power and log laws to the overlap region ($50 < y^+ < 0.15\delta_{0.95}$) for the data at different downstream positions and $U_e = 27$ m/s. The rms errors of the fit over the range are summarized in the last two lines. It would be quite easy to make a case for either theory, and extremely difficult to rule one out.

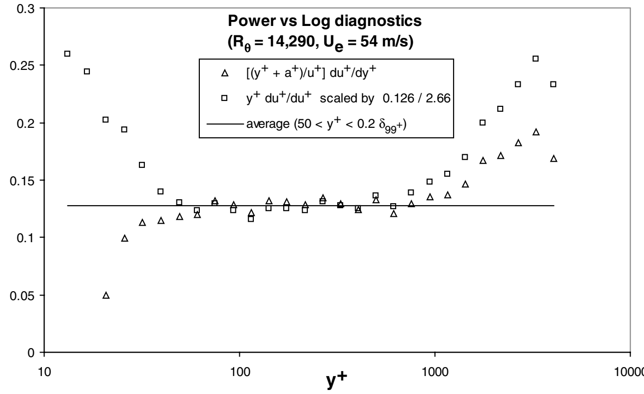

Fig. 8 Power and log diagnostics for data of Österlund [18], $R_\theta = 14,290$, $U_e = 54$ m/s. $a^+ = -16$. Horizontal line indicates constant γ or κ.

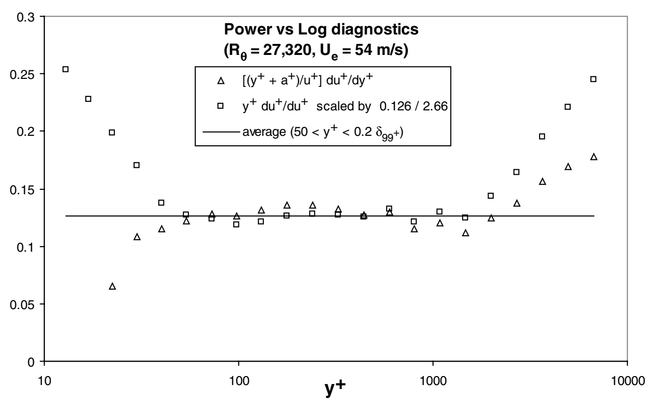

Fig. 9 Power and log diagnostics for data of Österlund [18], $R_\theta = 27,320$, $U_e = 54$ m/s. $a^+ = -16$. Horizontal line indicates constant γ or κ.

The nearly indistinguishable results between log and power fits have been noticed before by Buschmann and Gad-el-Hak [49], among others. Nonetheless, it is has been frequently argued by Hassan Nagib and others (e.g., Österlund et al. [48]) that the power law should be rejected because it fails the power law diagnostic test. In particular it is argued that $y^+ \mathrm{d}U^+/\mathrm{d}y^+$ is constant, whereas $(y^+/U^+)\mathrm{d}U^+/\mathrm{d}y^+$ is not. The problem with this argument has been pointed out repeatedly by Wosnik (e.g., APS 2000, Princeton 2002) and many times by me in private communications (all apparently to no avail). Because U^+ is proportional to $(y^+ + a^+)^\gamma$, not y^+, this is not the right test! The proper test is whether $[(y^+ + a^+)/U^+]\mathrm{d}U^+/\mathrm{d}y^+$ is constant. In fact, the proper power law diagnostic is at least as constant as the log diagnostic, as illustrated quite convincingly in Figs. 8 and 9. In both cases, $a^+ = -16$, the value suggested by GC97. The deviations from horizontal over the range $50 < y^+ < 0.15\delta_{95}$ are random and the rms error is about 4%. (Note that this is actually a more severe test for the power law, because the errors in U enter both the numerator and denominator.) These two profiles have been randomly selected from the 30 analyzed, and are representative. Interestingly, which works best (power or log) for any particular profile seems to be more dependent on tunnel speed than any other factor, a fact obscured if all are plotted together.

In summary, both log and power law theories clearly fit the data to within a few tenths of percent over the range of their expected validity ($30 < y^+ < 0.1\delta_{99}^+$). Of particular interest with the Österlund data is the near absence of Reynolds number effects across the entire mesolayer and inertial sublayers. This lack of Reynolds number dependence for this data set also characterizes the probability density functions in the overlap region [50] which show universal behavior (but curiously, not the outer part where such behavior might have been most expected). Thus these data are quite unlike the earlier data considered by GC97 where the inertial layer showed almost all the Reynolds number effects, and very much influenced their choices of functional dependencies and parameters. The Reynolds number independence of the inertial layer is somewhat surprising, because there are strong theoretical reasons to believe that the overlap region should be Reynolds number *dependent*, as first noted by Long and Chen [51]. It overlaps an outer region which is nearly independent of viscosity, and an inner region which is dominated by it. So it would seem this overlap region should depend on both. On the other hand, all the parameters for both the log and power laws do show a dependence on R_θ for smaller values of R_θ than those considered herein. Regardless, it is clear that the Reynolds number dependence of the coefficients in the CG97 (or preferably the modified version by Wosnik [21]) need to be recomputed for whatever set of data is being used, at least until some consensus is reached about which data set is correct.

VII. Near Equality of Analytical Relations

We have always suspected that the power and log laws were closely related, and perhaps indistinguishable. The reason for this is very simple. The simplest argument for the log law is that $\mathrm{d}U/\mathrm{d}y \propto 1/y$ in the inertial sublayer, which integrates immediately to a logarithm. Now suppose that this is only *almost* correct, and that in reality $\mathrm{d}U/\mathrm{d}y \propto 1/y^{1-\epsilon}$. Clearly this integrates to a power law, $U \propto y^\epsilon$, even if ϵ is an infinitesimal. But how could these very different analytical forms (power vs log) make so little difference in the actual profiles? And what could be responsible for this dramatic change in analytical form?

The reason why the CG97 boundary layer analysis predicts a power law behavior instead of a log is the streamwise inhomogeneity. Their asymptotic invariance principle (AIP) demands that any outer scaling law produce asymptotic similarity of the outer boundary layer equations. A consequence is that the outer scaling velocity must be at least asymptotic to U_∞ (e.g., U_∞ itself or $U_\infty \delta_*/\delta$ if δ_*/δ is itself asymptotically constant.) As noted, this is does not imply that profiles of even the mean velocity will collapse with this scaling velocity *at finite Reynolds numbers*, only that they converge to an asymptotic limit. Interestingly, the same AIP concluded that the turbulence normal stresses ($\langle u^2\rangle$, $\langle v^2\rangle$, $\langle w^2\rangle$) also scale asymptotically with U_∞^2, but the turbulence shear stress, $\langle uv\rangle$, scales with $U_\infty^2\,\mathrm{d}\delta/\mathrm{d}x$, which is itself asymptotically proportional to u_*^2. By contrast, the asymptotic scaling velocity for all of the inner profiles is u_*. Clearly these can all be the same only if $u_*/U_\infty \to$ constant, which (as noted by GC97) seems implausible. And because the inner and outer velocity scales are different, a power law overlap layer results, even by the primitive matching technique used in George and Knecht [52] and George and Castillo [5].

Pipes and channel flows are quite different, however, as noted by Wosnik et al. [32] (see also the appendix of GC97). If one forms a scaling velocity for the outer flow from the pressure gradient and the diameter, it always reduces to something proportional to u_*. This is because of the momentum integral constraint, which in the absence of streamwise mean accelerations simply reduces to a balance between the pressure acting on the cross section and the wall shear stress acting on the walls. Thus both the near wall and core flows are scaled asymptotically by the same velocity, u_*. And almost any matching scheme will produce a log law in the overlap region.

The different overlap solutions for boundary layer and pipe flows are thus a consequence of the fact that the former is inhomogeneous in the streamwise direction whereas the latter is homogeneous. Of course the boundary layer is *almost* homogeneous, especially in the wall layer. But, just as it requires only an infinitesimal amount to kick an integral from log to power law behavior, so apparently is the effect of inhomogeniety on the Navier–Stokes equations.

Given that the physical reason for the power law in boundary layers is its streamwise growth, should we expect the differences from the logarithmic behavior of pipes and channels to be easy to see? Certainly one might argue that the rate of change with x is so small, especially near the wall, that homogeniety in x is a reasonable approximation. The momentum integral provides more insight into our dilemma. Because the right-hand side is positive and decreasing with x, θ is increasing, but at a decreasing rate.‖ The fact that its rate of increase is second order in u_*/U_∞ does suggest strongly that there may not be much difference in the solutions.

In CG97, realizing that the values of γ were quite small (typically a tenth or less), we expanded our power law velocity profile in inner variables as a McClaurin series around $\gamma_\infty \ln y^+ \approx 0$, i.e.,

$$\frac{U}{u_*} = C_i y^{+\gamma} = C_i e^{\gamma \ln y^+} \approx C_i(1 + \gamma \ln y^+ + \ldots) \tag{29}$$

If we call the coefficient of the log term, $1/\kappa$, this gave $1/\kappa \approx \gamma_\infty C_{i\infty}$ which was about the answer we expected for κ of about 0.45 give or take a bit. But the same expansion also gave the additive constant in the approximate log law as $C_{i\infty}$ which was much too large (about 56 instead of 5 or 6). This was very frustrating, because the value of κ was quite reasonable.

Instead of a McClaurin series (expansion around zero), consider an expansion around an arbitrary reference value, say δ^+_{ref} (or equivalently, $\ln \delta^+_{\mathrm{ref}}$). This provides both a better comparison and is more consistent with what actually happens in treating experimental data, because all curve-fitting is centered around some value. Start with our friction law given by

$$\frac{U_\infty}{u_*} = \frac{C_{i\infty}}{C_{o\infty}} \exp\left[\gamma_\infty \ln \delta^+ - \frac{A}{(\ln \delta^+)^\alpha}\right] \tag{30}$$

Differentiation with respect to $\ln \delta^+$, expanding and evaluating at $\ln \delta^+_{\mathrm{ref}}$ yields

$$\frac{U_\infty}{u_*} = \left.\frac{U_\infty}{u_*}\right|_{\mathrm{ref}} + \frac{C_{i\infty}}{C_{o\infty}}\left[\gamma_\infty + \frac{\alpha A}{(\ln \delta^+_{\mathrm{ref}})^{1+\alpha}}\right]\left\{\exp\left[\gamma_\infty \ln \delta^+ - \frac{A}{(\ln \delta^+)^\alpha}\right]\right\}(\ln \delta^+ - \ln \delta^+_{\mathrm{ref}}) + \ldots \tag{31}$$

Keeping only the leading term gives exactly the log "law" of Eq. (20), i.e.,

$$\frac{U_\infty}{u_*} = \frac{1}{\kappa} \ln \delta^+ + C_1 \tag{32}$$

where $C_1 = B_i - B_o$. The "locally constant" coefficients are given by

$$\frac{1}{\kappa} \equiv \left[\gamma_\infty + \frac{\alpha A}{(\ln \delta^+_{\mathrm{ref}})^{1+\alpha}}\right]\left.\frac{U_\infty}{u_*}\right|_{\mathrm{ref}} = \gamma_{\mathrm{ref}} \left.\frac{U_\infty}{u_*}\right|_{\mathrm{ref}} \tag{33}$$

and

$$C_1 \equiv \left.\frac{U_\infty}{u_*}\right|_{\mathrm{ref}} - \left.\frac{U_\infty}{u_*}\right|_{\mathrm{ref}} \ln \delta^+_{\mathrm{ref}}\left[\gamma_\infty + \frac{\alpha A}{(\ln \delta^+_{\mathrm{ref}})^{1+\alpha}}\right] \tag{34}$$

$$= [1 - \gamma_{\mathrm{ref}} \ln \delta^+_{\mathrm{ref}}]\left.\frac{U_\infty}{u_*}\right|_{\mathrm{ref}} \tag{35}$$

The κ value computed in this manner should be exactly the same as that shown in the log friction law of U_∞/u_* vs R_θ. The C_1 computed here, however, is not the same C of the log law on the friction plots, because the latter must be computed from $\delta^+ = R_\theta(\delta/\theta)(u_*/U_\infty)$ using the functional dependence of δ_*/δ and u_*/U_∞. If the classical theory is correct, it follows from Eq. (9) that the ratio of R_θ/δ^+ should be asymptotically constant, from which it follows that B_o should be constant if B_i and C_1 are. On the other hand, if the GC97 theory is correct, then the value of B_o computed in this manner will be Reynolds number dependent. Someday we will know, but we do not as of this writing.

Using the values of the GC97 parameters, the values of κ and C_1 can be computed for any value of δ^+ chosen as the reference. Amazingly, the average value of κ computed from $1000 < \delta^+ < 100,000$ is 0.357 and the standard deviation is less than 0.7%! The value of C_1 varies a bit more around its average value of 7.32 with a standard deviation of 2%. If the range is reduced to $1000 < \delta^+ < 20,000$ which covers the range of the experimental data, the value of C_1 computed is 7.2 with a standard deviation of 1.3%. Both of these are very close to the experimental fits of the preceding section. It is easy to see why the experimenters have such a rigid belief in the log law, even from the perspective of a power law theory.

VIII. What About the Mesolayer?

The last point from the GC97 paper I would like to discuss briefly is the concept of the *mesolayer*. It is perhaps unfortunate that we chose the same word as earlier used by Long and Chen [51], because the physics of our mesolayer is much different than that proposed by them. Nonetheless they rightly recognized that there was a problem with the prevailing theory for the lower part of the inertial layer, and the word "meso" was quite appropriate for what we thought was missing as well. In any case, it has now been almost a decade, but our mesolayer idea has received very little attention. Nevertheless, I believe more strongly than even before that the mesolayer is not only present, but very important to our understanding. Moreover, the failure to recognize its existence is the source of much confusion, especially in understanding DNS and low Reynolds number

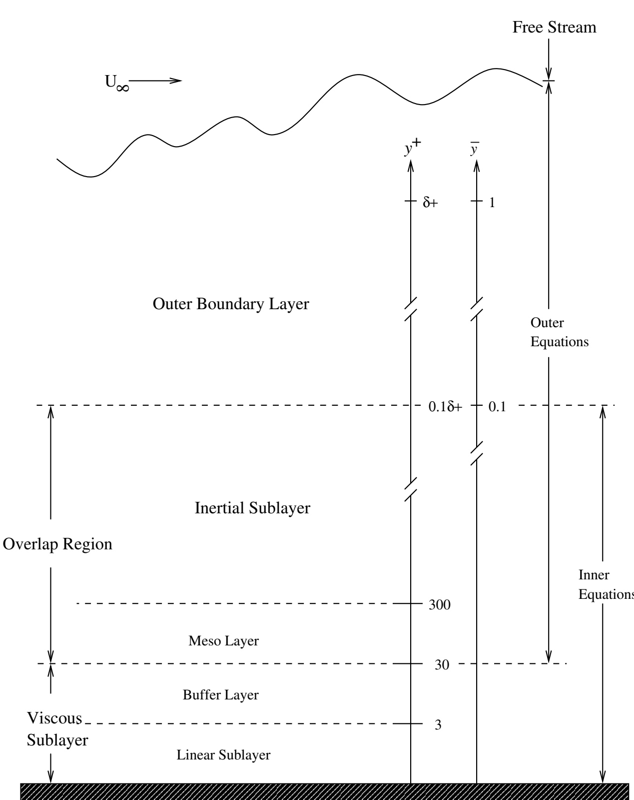

Fig. 10 Sketch from GC97 showing various layers in turbulent boundary layer in inner (left) and outer variables (right). The single-point "inner RANS equations" have no mean convection terms in the constant stress region. The single-point "outer" equations have no viscosity. And the single-point equations for the "overlap region" have neither mean convection nor viscosity, only Reynolds shear stress. But in the mesolayer part of the overlap region, the two-point equations still are influenced by viscosity at the energetic and Reynolds stress-producing scales of motion. Note that only above $y/\delta > 0.1$ are the convection terms in the mean momentum equation negligible, so $\delta^+ > 3000$ is a necessary condition for there to be an inertial sublayer at all. Thus experiments at lower Reynolds numbers have *only* a mesolayer!

‖Note that as pointed out by CG97 and contrary to common belief, θ does not approach a constant, but in fact increases almost linearly with x. Thus the whole concept of a "wake region" in boundary layers is erroneous. By contrast, it does make sense for pipe and channel flows because θ is truly constant once the flow is fully developed.

experiments. Figure 10 shows precisely where we thought (and still think) the mesolayer is located. I will deliberately avoid for now what we thought the effect on the mean velocity profile was, and focus instead on the underlying physics because that is what is the most important for all modelers.

Why do we think there is a mesolayer? The answer is simple: the two-point RANS equations tell us there must be. Although the viscous stress is negligible above $y^+ \approx 30$, the viscous terms in the two-point equations are not. In fact, they are never negligible at the smallest scales of the turbulence anywhere in the boundary layer. But if there is a sufficient scale separation between the energetic scales of the turbulence and dissipative scales, then the energetic scales are not affected by viscosity. On the other hand, if there is not a sufficient scale separation, then viscosity affects all the scales of the turbulence. We know this scale separation requires that the ratio of integral scale to Kolmogorov microscale be at least 10^4 for the turbulence to behave like high Reynolds number turbulence. Below this, the turbulence behaves very differently. This is well known to all who work with hydraulic models, and are often forced to compensate for "scale effects." If you want the energetic and Reynolds stress producing scales to behave like the real world, then you better make sure the turbulence in the experiment has a scale separation of at least 10^4; otherwise you will be doing a low Reynolds number experiment which may not reflect at all the phenomenon you are trying to model. This is also well known to jet researchers who have noticed that jets below an exit Reynolds number of 10,000–20,000 behave very differently than high Reynolds number jets. And of course it is of great concern to conscientious and knowledgeable DNS researchers as well, because they can almost never achieve the necessary scale separation to have anything that resembles a high Reynolds number flow.

So what does this have to do with the near-wall region? Very simple. Below values of y^+ of 300–500, the viscous terms in the two-point equations affect all scales of motion. Therefore, no matter how high the Reynolds number of the external flow, the turbulence in this region always behaves like low Reynolds number turbulence. Or said another way, below y^+ of about 300, viscosity affects all scales of motion, including those which produce the Reynolds stress. Thus this region is truly a *mesolayer*, meaning it is *in-between*; or to use a slang expression, "*It is neither fish nor fowl.*" The single-point equations are inviscid (meaning the total stress and Reynolds shear stress are nearly equal if the Reynolds number is high enough), but the two-point equations are not inviscid at any scale of motion (meaning the viscous term is non-negligible at any spatial separation, no matter the Reynolds number).

Why does all of this matter? Who really should care about this? Well first of all, experimentalists should care a lot, because they really should not be fitting inertial theories to this region. Happily this is one of the greatest changes over the past decade: now few do! When GC97 was first presented in the early 90s (years before publication), it was customary to fit the log law to $y^+ = 30$ and to optimize the fit at 100. Now almost no one even tries to look below values of y^+ of at least a few hundred. In effect, without admitting it, they have conceded the existence of the mesolayer. But the group of researchers that really should pay the most attention to the existence of the mesolayer is the LES community. Their whole approach to turbulence depends on a scale separation, and they have serious problems if there is none. Below $y^+ \approx 300$ there is none, so they should stop trying to make their models work below here and find a different approach. The existence of the mesolayer and its consequences would seem to make the LESers task easier, because they need less, not more, resolution close to the wall (presuming they have correct models for it).

But you might ask: Where is the experimental evidence? It is actually well known, but again the implications are not acknowledged. Where in the boundary layer does one first see a one-dimensional velocity spectrum that resembles high Reynolds number turbulence and has at least the beginnings of a $k^{-5/3}$ range? Answer: outside of y^+ of a few hundred [53,54]. In GC97 we were not smart enough to think of looking at the structure functions nor the structure function equations, but several groups now are [55–57]. Interestingly, in their presentations at meetings they comment on the persistence of viscous effects well outside of $y^+ = 30$, and note that this seems curious given the assumption of inviscid behavior beyond this point. But this is precisely how one would expect the structure functions to behave if there were a mesolayer. It is my prediction that as the Reynolds numbers of DNS increase and we can be confident the effects of the outer flow have pulled away from the overlap region, then we will see indisputable evidence in the structure functions for the existence (and importance) of the mesolayer.

IX. What Have We Learned Since GC97 that We Did Not Realize Before?

I would have to say the most interesting thing I have learned in the past decade that relates to boundary layers is a new understanding of how turbulent flows change from one similarity state to another. The whole idea of *equilibrium similarity* (as we have come to call our methodology since George [13]) is that the flow evolves to a state wherein all the relevant terms in the governing equations come into an equilibrium with each other. At the same value of the scaled coordinate at any position downstream, all of the terms have the same relative value. In other words, they go up (or down) in constant ratio as the flow evolves downstream. In the conclusion of Castillo and George [35] we asked how it could be that a flow which had evolved into an equilibrium similarity state could ever get out of it, because there was nothing in the equations to make this happen. Our particular concern was how an equilibrium similarity boundary layer (which almost all pressure gradient boundary layers seem to be) could ever relaminarize or separate. Now I think we know. And as a consequence I think we now have a better understanding of why the zero-pressure-gradient boundary seems "to never really get there."

The clue came from the recent work of another former Ph.D student, Peter B. V. Johansson, and me on the axisymmetric wake. The problems with this particular flow were originally called to my attention by Israel Wygnanski of the University of Arizona at the 1987 APS meeting in Eugene, OR. Careful measurements by very competent experimenters (including Steve Cannon and Frank Champagne, also at the University of Arizona) were never definitive as to whether the flow evolved to a similarity state or not. The classical self-preservation analysis predicted that the wake should spread as $x^{1/3}$, but the measurements seemed to be somewhere between $x^{1/2}$ and $x^{1/3}$. (In fact, in earlier experiments few measurements of growth rate or centerline velocity were reported at all, a curious omission.) I first addressed this problem in [58] which laid out the theory of equilibrium similarity (although it was not called that until later). In fact, I found two similarity solutions, one for very high turbulence Reynolds number, and another for low turbulence Reynolds number, but still very turbulent. The first reproduced the classical growth rate dependence of $x^{1/3}$ with the mean velocity deficit dropping as $x^{-2/3}$, but with coefficients that depended on the flow history and upstream conditions. The second also depended on flow history, but evolved as $x^{1/2}$ with the mean velocity dropping as x^{-1}. I could not decide which, if either, would govern in a real flow, nor could I decide whether one could evolve into the other and if so which came first. Happily Peter and I were able to resolve this (Johansson et al. [59]), thanks in part to a very difficult experiment he carried out in the KTH wind tunnel, and in part to a very, very long-time DNS we obtained from Michel Gourlay, then at Colorado Research Associates.

What we came to realize is that the axisymmetric wake is one of the few free-shear flows where the local turbulence Reynolds number actually decreases downstream. Therefore, unlike all other shear flows, the viscous terms in the single- and two-point equations governing the flow actually become more important as the flow evolves downstream. In fact, even if the flow begins with very high turbulence Reynolds number for which all of the viscous terms in the single point RANS equations are negligible and for which the two-point equations have no influence of viscosity on the large scales (i.e., so that $\varepsilon \propto u^3/L$), eventually the viscous terms grow back into the problem because the local Reynolds number is dropping as $x^{-1/3}$.

When these neglected viscous terms become non-negligible, we no longer have a viable similarity solution, at least until the viscous terms have become large enough that other terms become negligible and the new low-turbulence Reynolds number similarity solution appears. Although the velocity deficit decays, the flow never truly relaminarizes but continues to evolve as a low Reynolds number turbulence forever with constant ratio of turbulence intensity to wake mean velocity deficit. Of course the problem with the experiments is obvious in hindsight: all of the experiments were in between the high and low Reynolds number extremes, hence the difficulty in making sense of the results.

So what does this have to do with turbulent boundary layers? For the zero-pressure-gradient boundary layer, just about everything. All of the boundary layers we see in the laboratory or simulate in our computers are "in-between," and are undergoing this evolution in reverse, from low Reynolds number to high. In particular they are in between the only two true equilibrium similarity states: the laminar similarity solution (Blasius) on the one hand, and the infinite Reynolds number solution we have discussed on the other. Thus, away from the extremes of very low or very high values of δ^+, any expectations on our part of more than a "local" collapse of data are likely doomed to failure, no matter how successful they may appear to be over a limited range. But because the variable controlling the evolution seems to be $\ln \delta^+$, that "limited range" can appear very large indeed.

Boundary layers with pressure gradient have been particularly puzzling, in part because in contrast with the zero-pressure-gradient boundary, the outer part of the most pressure gradient boundary layers seems to evolve quite quickly to an equilibrium similarity state (c.f., Castillo and George [35], Elsberry et al. [60], see especially the appendix of the latter). Moreover, they appear to stay in this equilibrium similarity state until something catastrophic happens, sometimes even well past separation [36]. But as noted it was also not clear how they ever get out of this state, which of course they do when they relaminarize, or when the entire boundary layer detaches. During a recent phone-link to a presentation at RPI by Bayoan Cal, one of Luciano Castillo's recent Ph.D. students (who also spent a year at Chalmers in our turbulence masters program), it dawned upon all of us that precisely the same thing was going on in boundary layers with favorable and adverse pressure gradients. The local Reynolds number in some situations was not continuing to grow, but getting smaller. And as a consequence the neglected viscous terms were growing back into the problem. I am sure we will see substantial progress in understanding the consequences of this in the very near future, and perhaps will even see some real analytical criteria for avoiding or encouraging separation and relaminarization.

X. Conclusions

So where do I think this all stands? The truth is that I really do not know. In spite of their differing and seemingly irreconcilable theoretical basis, the log and power law results seem virtually indistinguishable, at least for zero-pressure-gradient boundary layers. In fact if the Österlund data had been available 15 years earlier (and been substantiated by the equations of motion), we probably would have never asked the questions we did about the log theory nor had reason to develop the GC97 alternative. Perhaps the differences will become more apparent as we obtain data that have been established beyond doubt to satisfy the differential and integral momentum equations for a zero-pressure-gradient turbulent boundary layer. Or maybe we will have to consider the previously unthinkable: that perhaps u_*/U_∞ (or c_f) does not really go to zero as the Reynolds number increases without bound. But my best guess is that the answers will come as we gain better knowledge about boundary layers with pressure gradients, which seem to behave quite differently. A significant clue will be if either theory has to change its inner parameters to accommodate the pressure gradient (like κ or γ), because both presume a near-wall region and overlap layer that is unaffected by the pressure gradient. Or perhaps the increasing interest in two-point statistical models (like LES) will change the focus from the mean profiles to quantities for which the competing models do show greater differences.

For past few years I have preferred to take a backseat, while others (especially my former students) carried forward the debate about turbulent boundary layers. In part this was because I felt I was getting too close to the problem and needed some perspective. But as well there were other problems and challenges that competed for my attention. The invitation to speak at this meeting has both given me a chance to take a trip down memory lane, and forced me to rethink some of the our previous work. But it has also given me the opportunity to observe closely a research field that is now brimming with new ideas, new experiments, and best of all, new young faces. Unlike even a decade ago, there is no shortage of open debate and intense disagreements. Journals routinely publish conflicting ideas. This is science as it should be carried out. There are still some who do not get it, and who are more interested in stifling conflicting views, or in protecting the view of the world they learned as students, than in advancing science. And occasionally one still sees ridiculous reviews that advise editors to "protect the sanctity of their journals," presumably much like an ecclesiastical council or inquisition. But, for the moment at least, their influence is on the wane, a happy trend we must strive to continue.

Twenty-five years ago those who funded research in the U.S. argued that there were no new ideas in turbulence that merited consideration, and all that was left were applications. Now the world has come full circle. The promised land of CFD based on all of those well-known theories is reaching its limits, and once again there is the beginning of a recognition that we need some new ideas. Now that we as researchers on fundamental turbulence have opened the curtains to let in the light, we can not only see the dust and cobwebs in our own thinking, but we also find ourselves in a perfect position to help. Never in my 37-year-career has there ever been more interest in asking tough questions and doing difficult experiments. Let us respond by insisting the curtains remain open as we honestly debate and learn together how the world of turbulent boundary layers really works.

Acknowledgments

I would like to thank the many who have contributed to my thinking on this subject, especially Luciano Castillo, Martin Wosnik, and Dan Ewing, who worked with me on most of the ideas. Rolf Karlsson always provided both strong encouragement and insightful criticisms before his untimely death, and he is greatly missed. Bayoan Cal, Brian Brzek, and Murat Tutkun provided useful comments on the manuscript, as did several reviewers. The discussions with Hassan Nagib over the past year were also quite helpful in understanding the limitations of both the theory and the data. But no one deserves more thanks than Mohamed Gad-el-Hak, who had the courage to publish GC97 when no one else would.

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