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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. Many 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, particularly George and Castillo.[1,2] 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.

This paper is dedicated to the memory of Rolf I. Karlsson[a]

Nomenclature

A	Universal constant in power law theory, value = 2.9 in GC97
A_*	Constant of proportionality
B_*	Constant of proportionality
C	Additive constant in log friction law when written as function of $\ln\delta^+$
C_1	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 $C_{i\infty}$ as $\delta^+ \to \infty$
C_o	Coefficient in outer version of power law overlap solution, function of δ^+
$C_{o\infty}$	Limiting value of $C_{o\infty}$ as $\delta^+ \to \infty$
D_∞	Defined to be $C_{i\infty}/C_{o\infty}$
f_o	Function describing velocity deficit in inner part of boundary layer
$f_{o\infty}$	Part of f_o that depends only on $\overline{y}$
f_i	Function describing velocity deficit in outer part of boundary layer
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, m^2/s^2
q^2	$u^2 + v^2 + w^2$
R_{so}	Scale function for Reynolds shear stress in outer layer
R_θ	Reynolds number based on momentum thickness defined by $U_\infty\theta/\nu$

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[a]Rolf Karlsson was a giant of industrial fluid mechanics. He contributed to the solution of many problems for his principal emplorer, Vattenfall Utveckling (a large Swedish energy company). He also played an important role in improving the quality of flow analysis throughout Europe by his active involvement in the ERCOFTAC and other committees. His commitment to fundamental research and the careful experiments he and his co-workers carried out not only contributed much to the field, but set an example for industrial fluid dynamicists everywhere. As an Adjunct Professor of Experimental Fluid Dynamics at Chalmers, he was a valued colleague and resource for students and faculty alike. He died in December 2004, a month after participating in the mini-symposium on turbulent boundary layers at Chalmers. We honor him for his courage and contributions thoughout his life, but especially those near the end.

r_o	Function describing Reynolds shear stress in outer layer
U, u	Mean and fluctuating streamwise (or x) velocities, m/s
U_∞	Mean velocity at great distance from wall, m/s
U_e	Experimental guess of U_∞
U_{so}	Either U_∞ or u_*, depending on which form of the velocity deficit is being used
u_*	Friction velocity defined from wall shear stress as $\sqrt{\tau_w/\rho}$
V, v	Mean and fluctuating radial velocities, m/s
V_c	Contraction volume as function of distance from exit
W, w	Mean and fluctuating velocity components in z direction
x	Streamwise coordinate, m
y	Coordinate normal to wall, m
y^+	Dimensionless inner variable defined by yu_*/ν
$\overline{y}$	Dimensionless outer variable defined by y/δ
α	Universal dimensionless constant, value = 0.46 in GC97
δ	Boundary layer thickness, usually taken as, δ_{99}
δ_{95}	Distance from the wall at which $U/U_\infty = 0.95$
δ_{99}	Distance from the wall at which $U/U_\infty = 0.99$
δ_*	Displacement thickness defined from $\int_0^\infty (1 - U/U_\infty)dy$
δ^+	Ratio of outer to inner length scales defined by $\delta u_*/\nu$
γ	Exponent in power law theory, function of δ^+
γ_∞	Limit of γ as $\delta^+ \to \infty$
ε	Rate of dissipation of turbulence energy per unit mass, m^2/s^3
ν	Kinematic viscosity , m^2/s
ρ	Density, kg/m^3
τ_w	Wall shear stress, $kg - m/s^2$
θ	Momentum thickness defined from $\int_0^\infty (U/U_\infty)(1 - U/U_\infty)dy$
ξ	Dummy integration variable $\overline{y}$

I. Introduction

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 was simply inferred from fitting the log profile to a few points near the wall, usually for values of y^+ between 30 and 100. In fact the 'log'-based ideas were so well-accepted that it seemed to bother only a few that real shear stress measurements (both momentum integral and direct) differed consistently and repeatably from these inferred results. Instead of causing a re-examination of the theory, it became common wisdom that there was something wrong with these techniques. The careful drag and mean velocity measurements laboriously performed in the 30's and 40's were discarded as being in error. 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 those 'defined' from the 'universal' log equations.[3] Dissent was squelched as journals (through their reviewers) practiced St. Augustine's view of science: *"God has reserved hell for the curious."*

Even today, many turbulence models are still more or less are based on these old ideas. But in the experimental and theoretical world all has now changed. It has been a decade since the papers by George and Castillo,[1,2,4] Gad-el-Hak and Bandyopadhyay,[5] and Barenblatt[6] re-opened 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 be time-dependent, and vary from one experiment to the next. But 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 are less successful. To the casually interested on-looker and devoted researcher alike, the entire field appears to be in chaos. A recent text after reviewing the alternative ideas concluded the classical theory was preferable 'since it was universal'. By the same argument we should all embrace ignorance, but we do not because we are scientists who believe in reason, deduction and data. So it must be with our 'beliefs' about wall-bounded flows: reason, deduction and data must be brought into coincidence. The purpose of this paper is not to change any minds, but rather it is to change the nature of the debate from empiricism to mechanics, from scaling laws to governing equations, from curve-fits to physics and physical principles.

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 is it?) Others broke new ground using new techniques (e.g., Lie group theory[7]), but left open many questions. Most troubling to me 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. I will comment more on these points later.

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[8]). Others truly broke new ground using direct measurements of shear stress and pushing to higher Reynolds numbers.[9,10] 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 satisfy the momentum integral equation, thus raising the most serious question about precisely which flow was measured at all – clearly fundamental to any argument. Moreover, there have been almost no measurements where sufficient data has been provided to actually substitute into the averaged equations (e.g. RANS, kinetic energy, etc.) and see if the terms all add up. So we are largely left with detailed measurements of flows for which we can not say with certainty what flow was measured, nor whether the equations we believe to govern them really do. I will not comment on this further here, as I have expressed myself previously, v. George.[11] Since many of the principals in this debate are present at this symposium, I will leave it to them to review the past decade of experimental work and to present and defend their own work. I am certain the discussion following their presentations will be lively (and probably following mine as well).

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. What CFD needs 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. Current computational models reflect the physics as we understood it more than a half-century ago. 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[2] (hereafter referred to 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,[12] Martin Wosnik[13] and their students, especially Terri Seo[14] and Xia Wang[15]). And finally I will suggest some questions for future research. The primary focus will be on the zero pressure gradient boundary layer, since that appears to be the most problematical. In order to keep the big picture in view, few details of the analysis will be provided, since these have been published elsewhere.

II. The 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[16] is correct; i.e.,

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

The reason for the δ^+ will be addressed below, but it is crucial for theoretical analyses. 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 to the best of my knowledge none has ever been noted 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:

$$\frac{U-U_\infty}{u_*} = f_o(\overline{y}, \delta^+, *) \qquad (vonKarman) \qquad (2)$$

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

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

where $\overline{y} \equiv y/(D\delta)$. All are written as deficits from the free stream velocity in order 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[b]. δ_{99}, the point at which the mean is velocity is 99% of the free stream 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 experiments very difficult.

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), $\nu/u*$. The functional dependence on this ratio of length scales should vanish in the limit as $\delta^+ \to \infty$, and as it does 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. 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 function just to indicate that there may be other things whose influence on the shape of the profile can not be ruled out *a priori*, the leading candidates among them being upstream conditions, free stream turbulence, etc. The very presence of this extra argument means we must be very careful about comparing different experiments 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 few positions 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. Few have been very careful about this.

The most familiar deficit (found in all texts) is the classical proposal of von Karman[17] given by eqn 2. It was originally proposed by Stanton and Pannell[18] for pipe and channel flows, and simply appropriated by von Karman for boundary layers since it appeared to collapse the velocity profile data better than the second alternative, eqn 3. Most experimenters ignore the importance of the functional dependence on δ^+, and accept the classical choice on the same empirical grounds. And that empirical choice lies at the root of almost all theoretical analysis since Millikan[19] and Clauser,[20] the contributions of me and my co-workers being the exceptions. The second choice is probably as old as the study of boundary layers, but was largely abandoned after the 1950's when evidence seemed to favor the Millikan/Clauser approach. It was resurrected by me and my co-workers, *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

[b]This factor was omitted in the GC97 paper, but 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.

proposal by Zagarola and Smits,[21] 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. One might surmise then that almost everyone would be happy with the ZS alternative, but surprisingly, some advocates of the classical scaling report difficulty getting their data to collapse using it.

Before proceeding further, I want to make it clear what is at stake here. If eqn 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 eqn 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. Obviously, data notwithstanding, since we practice in the field of mechanics (which is all about writing equations that relate forces to motion), we should expect the governing equations to play some role in deciding which, if any, is correct. Then if the data do not agree, either our data is 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 the turbulent boundary layer, and especially for zero pressure gradient boundary layer.

Amazingly (since we are talking about fluid mechanics here) the Navier-Stokes equations (or their Reynolds averaged versions) played little to no role in many theoretical papers about boundary layers. Exceptions include the efforts of Clauser,[20] some of the recent work of Perry and his students, and that of me and my students, some of which will be considered herein. 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 boundary layer at infinite Reynolds number these reduce to just continuity and the x-momentum equation given by:

$$U\frac{\partial U}{\partial x} + V\frac{\partial U}{\partial y} = \frac{\partial}{\partial y}\left[-\langle uv\rangle\right] + \frac{\partial}{\partial x}\left[\langle v^2\rangle - \langle u^2\rangle\right] \tag{5}$$

The last term is the streamwise gradient of the normal stress differences, part of which comes from integrating the y-momentum equation across the flow and using it to substitute for the pressure. Since these terms are of lower order than 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 absence 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 deficit functions above. Note that the mean convection terms (left hand side) are negligible only in the inner 10% of the boundary layer, which means that in low Reynolds number boundary layers there really is no region independent of viscous and mean convection effects.

The objective now is to examine whether any of our candidate deficits represent a similarity solution to eqn 5. If any do, then they represent a proper scaling law since 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^+, since the governing equations say it must. This is the single most important point of this paper, and for that matter any I have been involved with on this subject. Note that there is no debate about fully developed pipe and channel flows. Since they are homogeneous in the streamwise direction, the left hand side vanishes identically and the balance of pressure gradient and viscous stress on a control volume dictate that eqn 2 is the appropriate form (c.f. Wosnik et al.[22]).

Each of the velocity deficit forms can be substituted into eqn. 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.[2,12,13,23] If U_{so} represents either u_* or U_∞ when it multiplies the profile function and we define $\overline{y} = y/\delta$, the result (after some manipulation) is:

$$\begin{aligned}\left[\left(\frac{U_\infty}{U_{so}}\right)\frac{\delta}{U_{so}}\frac{dU_{so}}{dx}\right]f_o \quad &+ \quad \left[\frac{\delta}{U_{so}}\frac{dU_{so}}{dx}\right]f_o^2 - \left[\frac{U_\infty}{U_{so}}\frac{d\delta}{dx}\right]\overline{y}f_o{}' \\ &- \quad \left\{\frac{d\delta}{dx} + \left[\frac{\delta}{U_{so}}\frac{dU_{so}}{dx}\right]\right\} f_o{}'\int_0^{\overline{y}} f_o(\xi)d\xi = \left[\frac{R_{so}}{U_{so}^2}\right] r_o{}'.\end{aligned} \tag{6}$$

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

All the principals mentioned above 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 crux of the debate, not whose curves fit the best over the limited range of Reynolds numbers we can measure. (As will be shown below, everyone's can be made to fit everything.)

Clauser using the von Karman deficit of eqn 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 blush this appears quite reasonable. The ratio, u_*/U_∞ is quite small (typically a few percent or less). Since these neglected terms occur to first order in his equations, one can conclude that the velocity deficit represented by eqn 2 is a first order similarity solution to the outer equations.

But is a first order solution reasonable, or even viable? GC97 argue it is not. They argue 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 since 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.[24] Since 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: 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. It also supports the GC97 conclusion that the outer equations must be analyzed to at least second order in u_*/U_∞, since it is the changes in the outer flow that make the biggest contribution to $d\theta/dx$, and therefore dictate 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 with 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[5] document this problem nicely.) And 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, since 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) which often does not extrapolate well over a large range.

So what about the alternative of eqn 3. It most certainly does *not* provide a better collapse of the data for the zero pressure gradient boundary layer, although curiously enough it may for pressure gradient boundary layers.[12,15,25,26] 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 Karman and Clauser, among others? The reason is that it is a solution (perhaps the only solution) to eqn 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, and can be shown to be valid to at least *third* order in u_*/U_∞. This means that eqn 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. 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 Navier-Stokes equations are wrong or we have used them incorrectly. Obviously this is what the debate should be about if the data don't appear to 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 from empirical fits to the profiles and by integrating the friction laws in the manner first proposed by Clauser.[20] Of most interest though is the asymptotic behavior as $\delta^+ \to \infty$. The velocity deficit scaled with u_* produces the following asymptotically (v. Clauser[20]):

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

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

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

where A_* and B_* are integrals involving *only* the limiting outer velocity profile, f_o. These can be compared to the CG97 results for the outer profile scaled with U_∞ which reduce in the limit to:

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

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

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

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

Which best describes the data? CG97 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. So 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 guide us. CG97 (and subsequent papers) argued that unity was not a physically realistic limit for the shape factor, since it implies no boundary layer profile at all. They further argued that since the momentum and displacement thickness clearly grow downstream and do not go to zero, the fact that δ was blowing up relative to them (since 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, but it has been recognized implicitly. In particular, a number of authors have tried to develop higher order theories using perturbation expansions in powers of u_*/U_∞ (e.g., Fendell, Panton, Afzal and others). These all begin with classical deficit law, and thus would seem to have the same problem in the limit: namely, they are not similarity solutions to eqn 5.

We have one deficit proposal left, the Zagarola/Smits proposal of eqn 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 eqn 2. If, on the other hand, $\delta_*/\delta \to$ *const* as GC97 argue, then the ZS scaling is asymptotic to eqn 3. So it would seem that everyone should be happy since no matter who is right, the ZS scaling will make them think they are. Curiously this has not been the case for all the advocates of the classical view, and this represents a powerful argument that their claimed successful scaling with u_* cannot be accepted as confirmation of the classical theory. The results *must* be the same if the classical theory is correct. By contrast, Castillo and co-workers have shown the ZS scaling to work in a variety of contexts to collapse mean velocity data, including boundary layers with pressure gradient, different upstream conditions, and even roughness (e.g.[14,15,25,26]). To add further weight to the argument for the ZS-scaling, Wosnik and George[27] showed that it collapsed all the George/Castillo composite theoretical velocity profiles over orders of magnitude greater range in δ^+ than all experiments. Recognizing that this could not just be coincidence, Wosnik and George[27] also were able to show that if the velocity deficit function, $f_o(\overline{y}, \delta^+, *)$ could be separated into the product of two functions, one containing the y-dependence, the other the other independent arguments, then the ZS scaling resulted.

This is easy to show, and we can learn something I didn't realize before. Let $f_o(\overline{y}, \delta^+, *) = g_o(\delta^+, *) f_{o\infty}(\overline{y})$. Substituting into the definition of the displacement thickness, and ignoring the small contribution from the wall layer yields:

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

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

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

Since the integral can only have a single universal value (by our hypothesis), it follows immediately that:

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

exactly the factor needed to produce eqn 4 from eqn 3. Now to see something new: note that since g_o does not depend on $\overline{y}$, the product of functions, $g_o(\delta^+, *)f_{o\infty}(\overline{y})$ can be substituted for $f_o(\overline{y}, \delta^+, *)$ in eqn 6 without changing the overall conclusions (except for some terms involving derivatives with respect to δ^+ which are asymptotically zero). Since some terms depend only on f_o while 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 equation. This means that different values of g_o can produce different 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, since it is an equilibrium similarity solution. This is exactly as observed, at least by Zagarola/Smits, Castillo, Wosnik and others.

III. The friction laws

The inner velocity profile function of eqn 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 model before the inner and outer equations can be solved, and the model in essence presumes the answer. Millikan[19] 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[28] 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 don't know of any singular perturbation problem I can solve analytically for which the classical turbulence matching methodology gives me the correct answer. Apparently this methodology only works for problems which we can't 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,[1] 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 flow. In both cases the results were quite spectacular. 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[29]) that were in wonderful agreement with the almost simultaneous experiments of Mydlarski and Wahrhaft.[30] 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.[22]). Moreover the same approach yielded quite satisfactory results for the plane walljet as well (George et al.[23])). I am somewhat disappointed that so far no mathematician to my knowledge has pursued the implications of Near-Asymptotics, since 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 eqns 1 and 2 using the standard approach yields a logarithmic friction velocity profiles in the overlap region and a friction law given by:

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

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

and

$$\frac{U_\infty}{u_*} = \frac{1}{\kappa}\ln\delta^+ + C \tag{20}$$

where $C = B_i - B_o$, 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.,[31] they may also be very weak functions of the local Reynolds number and only asymptotically constant. The offset in y, either b^+ or $\overline{b} = b^+/\delta^+$, is a necessary consequence of the need to be invariant to choice of origin.[7] 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 .[31]

Note that 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 C using B_i and B_o as the log theory demands. In particular it is B_o that is always missing, which seems quite suspicious actually since 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.[22]). (A first attempt at this using some of the recent data of Österund[9] is included in Table 1 below.) 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 since these 'universal' log constants have also been highly 'time'-dependent, and have changed as the Reynolds number of experiments has increased. (Incidentally, this is exactly what GC97 predicted would happen.) 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[31]).

The GC97 approach applied to boundary layers yields power law solutions for both the velocity in the overlap region and for the friction law. 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, one with constant parameters (which did not seem to represent the data), and a second with a dependence on local Reynolds number. Thus 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 friction law can be written in either of two ways. First, by leaving the parameters as functions of δ^+:

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

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

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 $\overline{a} = a^+/\delta^+$. GC97 suggest that $a^+ \approx -16$, and that seems also to be consistent with the new data discussed below. Near-asymptotics provides these functions through the constraint equation which links them together (see CG97 or Wosnik[32] for a slightly improved version). In particular these must satisfy:

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

and all must be asymptotically non-zero and constant for similarity to be possible and to satisfy Kolmogorov's hypothesis for the dissipation to be finite at infinite Reynolds number (i.e., $\varepsilon \propto k^{3/2}/L$). The leading term of the solution expanded in powers of $1/\ln D\delta^+$ yields after substitution:

$$\frac{U_\infty}{u_*} = \frac{C_{i\infty}}{C_{o\infty}}(D\delta^+)^{\gamma_\infty}\exp\left[-A/(\ln[(D\delta^+)]^\alpha\right] \tag{25}$$

where $C_{i\infty}$ and $C_{i\infty}$ are the infinite Reynolds number limits of the velocity profile parameters in the overlap region, as is the exponent, γ_∞. (Note that Wosnik[13] developed a higher order solution which resolved some 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/Walker and Purtell 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 $C_i = 57.5$ might be better. (The differences were due to the evolution and emergence of better near wall velocity data for the plane walljet 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 because we felt it had all been contaminated by the so-called Clauser method results. Therefore the friction law of eqn 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, Arne Johanson and their students 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 in Fig. 1 were obtained well after the publication of GC97 (and by experimenters not particularly favorably inclined toward it). The agreement is spectacular, to say the least, especially since no curve fitting to the friction data was involved, nor was any previous friction data included in the prediction. And the agreement is spectacular over the entire range of Reynolds numbers. This was not a surprise to us, since both Wosnik[13] and Castillo[12] previously showed agreement with the earlier data of Österlund[9] at lower values of R_θ. It certainly vindicates the judgement of GC97 in choosing to discard the shear stress data existing at the time, 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 closes the loop. Like the pipe and channel flow analysis of Wosnik et al. ,[31] this means the GC97 theory is also internally consistent.

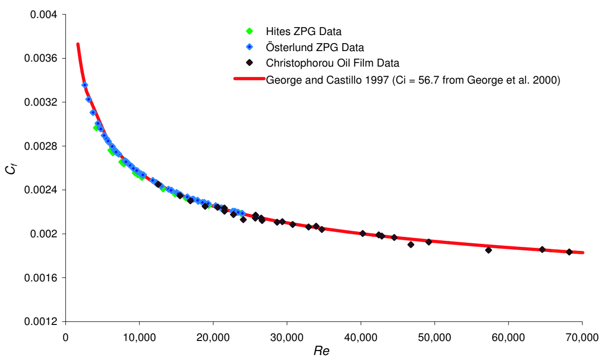


Figure 1. c_f versus R_θ from oil film data compared to GC97 theory (from Nagib et al.[10]).

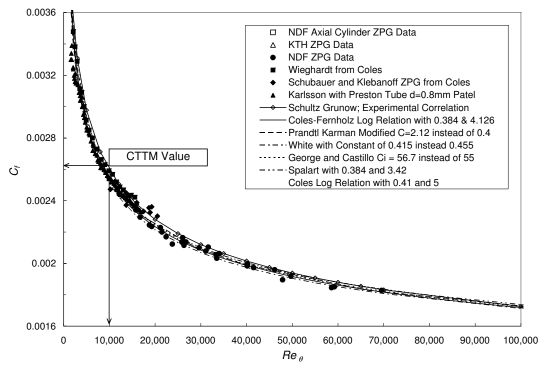


Figure 2. c_f versus R_θ showing various curves optimized to fit data. (H. Nagib[33])

So is this the end of the story? Interestingly not. Fig. 2 was provided to me by Hasan Nagib after he participated in a mini-symposium at Chalmers on wall turbulence. In it he shows a plot of a number of empirical and theoretical curves on top of the same data and curve of Fig. 1. Two aspects are of special interest. First it includes data of my recently deceased and dearly beloved colleague, Rolf Karlsson, from his Ph.D. dissertation[34] in 1980. This data was never accepted for publication, in spite of repeated attempts, because it did not agree with the conventional wisdom. Rolf's data points virtually overlay the more recent data. Happily Rolf saw this plot just before his death. We of the turbulence community, and especially those who were reviewers, should collectively be ashamed of having been so hostile to the results of his careful experiments. Are we a science, or religion? Is it our goal to train only disciples who blindly follow our lead and stifle all serious challenges to our beliefs (e.g., by abusing the anonymous review process). Or do we seek to raise new prophets who challenge the past, find the holes in our reasoning, and bring forth a better future, thereby justifying society's faith and financial investment in us?

The second aspect of the plot is that almost all the curves look pretty much like the data. Particularly surprising to me, at least, is the almost perfect agreement between the fitted plot of the log curve (labelled

Coles/Fernholz) with both the data and the GC97 theory. The log curve was given by:

$$\frac{1}{c_f} = \frac{1}{\kappa} \ln R_\theta + C_1 \tag{26}$$

Note that this is *not* the same as eqn 20 since it depends on R_θ instead of δ^+, but it can be related (at least asymptotically) using eqn 9 (or for that matter eqn 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 CG97 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 CG97 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.[10] claimed in their Perryfest presentation.

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 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 have different limiting behavior?

IV. The velocity profiles

One might imagine from the consensus above about 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[35] 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 is 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 seven meter long test section. This is an extensive set of data with 70 profiles measured at four downstream positions and 10 different free stream 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-90's, and although they have frequently been cited to argue for the log law (and against all other alternatives), the data has never been made publicly available. The same is true for the more recent experiments of Nagib and Christophorou as well, and in fact the plots shown in the previous section were taken from a pdf file of a presentation by Nagib.

The failure of the IIT group to make its data available considerably handicaps any serious discussion. Naturally it makes any evaluation or rebuttal of any claims impossible. On the other hand, since they claim agreement with the Österlund experiments for the most part, this paper will concentrate on that data and that of Smith. The absence of the IIT data is particularly unfortunate, since 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 short-coming is a tragedy (at least from a theoretical perspective), since the experiment was an otherwise heroic effort.

Figure 3 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 ratio u_*/U_e was 0.0356 for both of the Österlund and 0.354 for the Smith experiment, so the differences

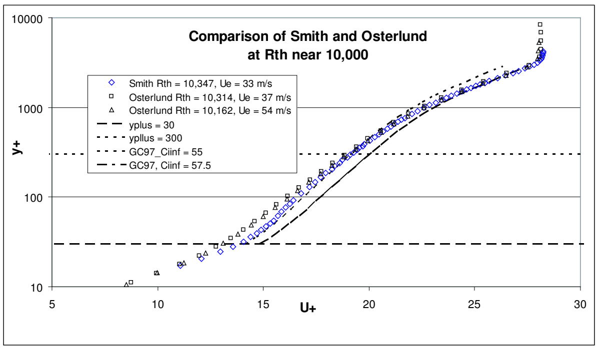


Figure 3. Three velocity profiles at $R_\theta = 10,000$ from Österlund[9] and Smith[35] along with GC97 profiles.

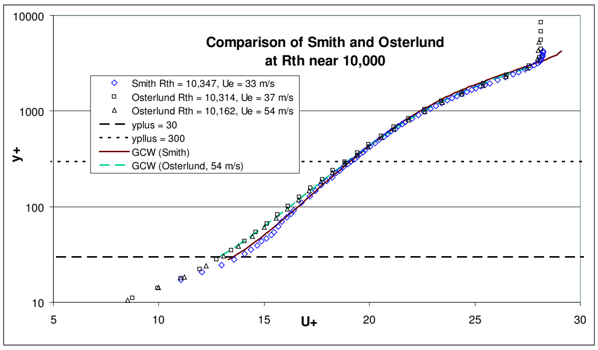


Figure 4. Three velocity profiles at $R_\theta = 10,000$ from Österlund[9] and Smith[35] along with fits of eqn 27.

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, since 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 can not 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. The new measurements from IIT (if and when they are made available) will probably not resolve this, since they can hardly be argued to be "independent". Moreover both sets of data were taken with hot-wires, which are notorious for their behavior near surfaces. The WALLTURB experiment currently underway in Europe under the supervision of Michel Stanislas using modern optical techniques in the 20m boundary layer facility at Lille has perhaps the best chance of resolving the discrepancies.

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 above). All of which 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, since 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 $d\theta/dx$ and eqn. 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 is 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 above, this is the part of the boundary layer on which $d\theta/dx$ is primarily dependent, since 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:

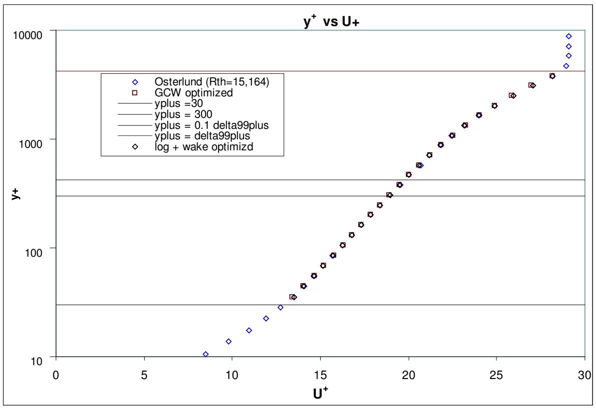
$$U^+ = C_i(y^+ + a^+)^\gamma + \left\{\frac{U_\infty}{u_*}\right\}(0.99 - C_o)\overline{y} sin(\pi\overline{y}/2) \tag{27}$$

where U_∞/u_* must be determined from eqn. 23. The second term is the 'wake' function and is a slight modification of that used by GC97 who used $(1 - C_o)\overline{y}sin(2.03\overline{y})$. The fits are shown in Figure 4 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 $C_i = 10.109$, while $\gamma = 0.129086591$, $C_o = 0.923308563$ and $C_i = 9.156959787$ 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 figure 5, which shows a comparison of power-law -plus-wake and log-law-plus-wake fits the Österlund velocity profile for $R_\theta = 14, 29$, $U_e = 54m/s$. The log-plus-wake profile uses Coles wake function and is given by:

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

For this data set $D_{log} = 1$ and $b^+ = 0$ was optimal. Both curves fit the data with an average rms error of 0.4%. Here both log-plus-wake and power-plus-wake have been fitted to the velocity profile of Österlund for $R_\theta = 15, 154$, $U_e = 27$. The plots are indistinguishable, and both both sets of "constants" (power and log) produce the measured value of u_*/U_e as well as correct integral parameters.



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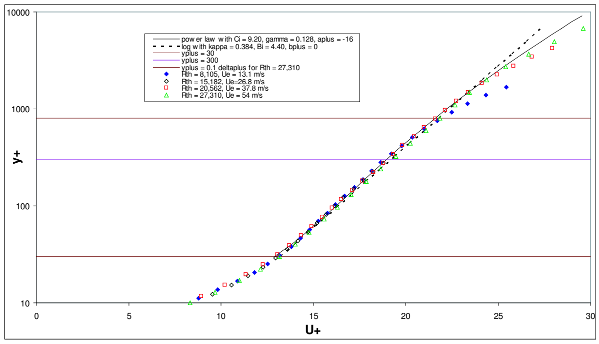

Figure 5. Fit of power and log plus wake function to data of Österlund,[9] $R_\theta = 15, 154$, $U_e = 27$ m/s.

Figure 6. Four velocity profiles from Österlund[9] at different R_θ and U_e, along with fits of eqns 19 and 21.

In spite of the above, there have been persistent and recurrent claims that somehow the Österlund data proves conclusively that the power law is not viable, and only the log law works with this data. Figure 6 plots data in inner variables at the farthest downstream position (5.5 m) for four different tunnel speeds and Reynolds numbers (R_θ = 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).

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.

The nearly indistinguishable results between log and power fits have been noticed before by Buschmann and Gad-el-Hak,[36] among others. Nonetheless, it is has been frequently argued by Nagib and co-workers[37] that the power law should be rejected because it fails the power law diagnostic test. In particular it is argued that y^+dU^+/dy^+ is constant, while $(y^+/U^+)dU^+/dy^+$ 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). Since U^+ is proportional to $(y^+ + a^+)^\gamma$, not y^+, this is not the right test! The proper test is whether $[(y^+ + a^+)/U^+]dU^+/dy^+$ is constant. In fact, the proper power law diagnostic is at least as constant as the log diagnostic, as illustrated quite convincingly in Figures 7 and 8. 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, since 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

R_θ	5486	7970	10502	12886	15182	14207	15165
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	0.12278	0.1271
C_i	9.1865	9.0136	9.1447	9.2168	9.315	9.5020	9.1899
κ	0.4091	0.3874	0.3785	0.3808	0.3895	0.3821	0.3849
B_i	4.6838	4.0608	3.7280	3.9535	4.3275	4.1883	4.1077
B_o	-2.3800	-3.3405	-3.2538	-3.1946	-3.3195	-3.0012	-3.2936
lnA/κ	-2.6985	-2.9704	-3.1101	-3.1382	-3.0804	-3.0951	-2.9998
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

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.

(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.

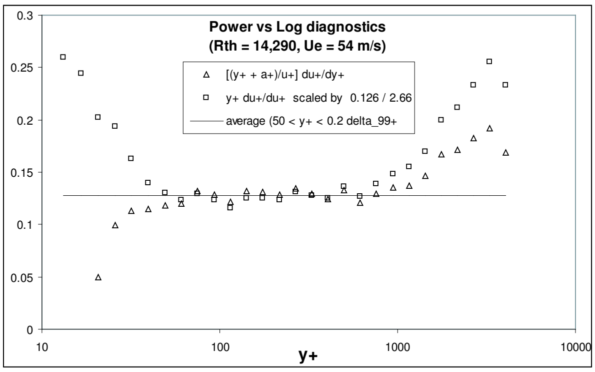

Figure 7. Power and log diagnostics for data of ,[9] $R_\theta = 14,290$, $U_e = 54$ m/s.

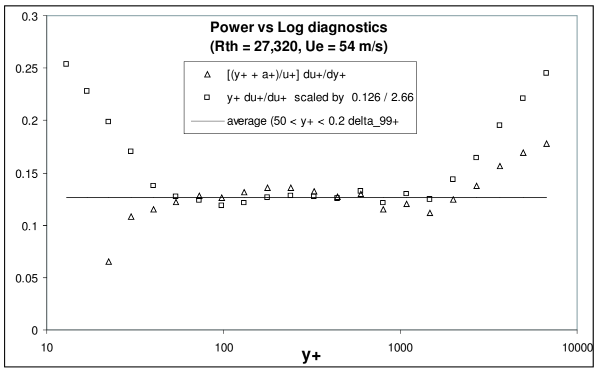

Figure 8. Power and log diagnostics for data of ,[9] $R_\theta = 27,320$, $U_e = 54$ m/s.

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 is 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. When the methodology of GC is applied to this data, neither the inner nor outer parameters vary much with Reynolds number. This is a surprise to me, since there are strong theoretical reasons to believe that the overlap region should be Reynolds number dependent. It overlaps two regions which have different Reynolds number dependencies, so it would seem this overlap region should depend on both (as first noted by Long and Chen[38]). On the other hand, all the parameters, log and power, do show a dependence on R_θ for smaller values of R_θ than those considered herein. Finally, it is clear that the Reynolds number dependence of the coefficients in the CG (or preferably the modified version by Wosnik) need to be recomputed for whatever set of data it is being used (a task that is straightforward, but beyond the scope of this paper given the large number of profiles involved).

V. 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 $dU/dy \propto 1/y$ in the inertial sublayer, which integrates immediately to a logarithm. Now suppose that this is only *almost* correct, and that in reality $dU/dy \propto 1/y^{1+\epsilon}$. Clearly this integrates to a power law, $U \propto y^{1+\epsilon}$, even if ϵ is an infinitesimal. So what could be responsible for this dramatic shape in analytical form? And could the very different analytical form (power vs log) really make much difference in the actual profiles.

The reason why the CGW boundary layer analysis predicts a power law behavior instead of a log is the streamwise inhomogeneity. The Asymptotic Invariance Principle 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 above, 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 $U_\infty^2 d\delta/dx$, 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 since 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[39] and George and Castillo.[1]

Pipes and channel flows are quite different, however, as noted by Wosnik et al.[31] (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 while 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. Since the right hand side is positive and decreasing with x, θ is increasing, but at a decreasing rate[c]. But 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 $\gamma_\infty \ln y^+ \approx zero$; i.e.,

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

$$\frac{U}{u_*} = C_i e^{\gamma lny^+} \approx C_i(1 + \gamma lny^+ + \ldots) \tag{30}$$

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 to big (about 56 instead of 5 or 6). This was very frustrating, since the value of κ was quite reasonable.

[c]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 since θ is truly constant once the flow is fully developed.

Instead of a McClaurin series (expansion around zero), consider an expansion around an arbitrary reference value, say δ^+_{ref} (or equivalently, $\ln \delta^+_{ref}$). This is a better comparison, since all curve-fitting is centered around some value. Start with our friction law given by:

$$\frac{U_\infty}{u_*} = \frac{C_{i\infty}}{C_{o\infty}} \exp[\gamma_\infty \ln \delta^+ - A/(\ln \delta^+)^\alpha)] \tag{31}$$

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

$$\frac{U_\infty}{u_*} = \frac{U_\infty}{u_*}\Big|_{ref} + \frac{C_{i\infty}}{C_{o\infty}} \left[\gamma_\infty + \alpha A/(\ln \delta^+_{ref})^{1+\alpha}\right)] \{\exp[\gamma_\infty \ln \delta^+ - A/(\ln \delta^+)^\alpha)]\}(\ln \delta^+ - \ln \delta^+_{ref}) + \ldots \tag{32}$$

Keeping only the leading term gives exactly the log 'law' of eqn 20; i.e.,

$$\frac{U_\infty}{u_*} = \frac{1}{\kappa} \ln \delta^+ + C \tag{33}$$

The 'locally constant' coefficients are given by:

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

and

$$C \equiv \frac{U_\infty}{u_*}\Big|_{ref} - \frac{U_\infty}{u_*}\Big|_{ref} \ln \delta^+_{ref} \left[\gamma_\infty + \frac{\alpha A}{(\ln \delta^+_{ref})^{1+\alpha}}\right] \tag{35}$$

$$= [1 - \gamma_{ref} \ln \delta^+_{ref}] \frac{U_\infty}{u_*}\Big|_{ref} \tag{36}$$

The κ value computed in this manner should be exactly the same as that shown in the log friction law of U_∞/u_* versus R_θ. The C computed here, however, is not the same C_1 of the log law on the plots, since 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 eqn 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 GC 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 CG97 parameters cited above, the values of κ and C 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 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 computed is 7.2 with a standard deviation of 1.3 %. Both of these are very close to the experimental fits of the previous 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.

VI. 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,[38] since 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. (Besides, I took my first formal fluids course from Long using Lamb's text and survived, so I have earned the right to appropriate his word, and hopefully honor him by doing so.) In any case, it has now been almost a decade, but the 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 our our confusion. Figure 9 shows precisely where we thought (and still think) it is. I will deliberately avoid for now what we thought the effect on the mean velocity profile was, and focus instead on the underlying physics since that is the most important for all modelers.

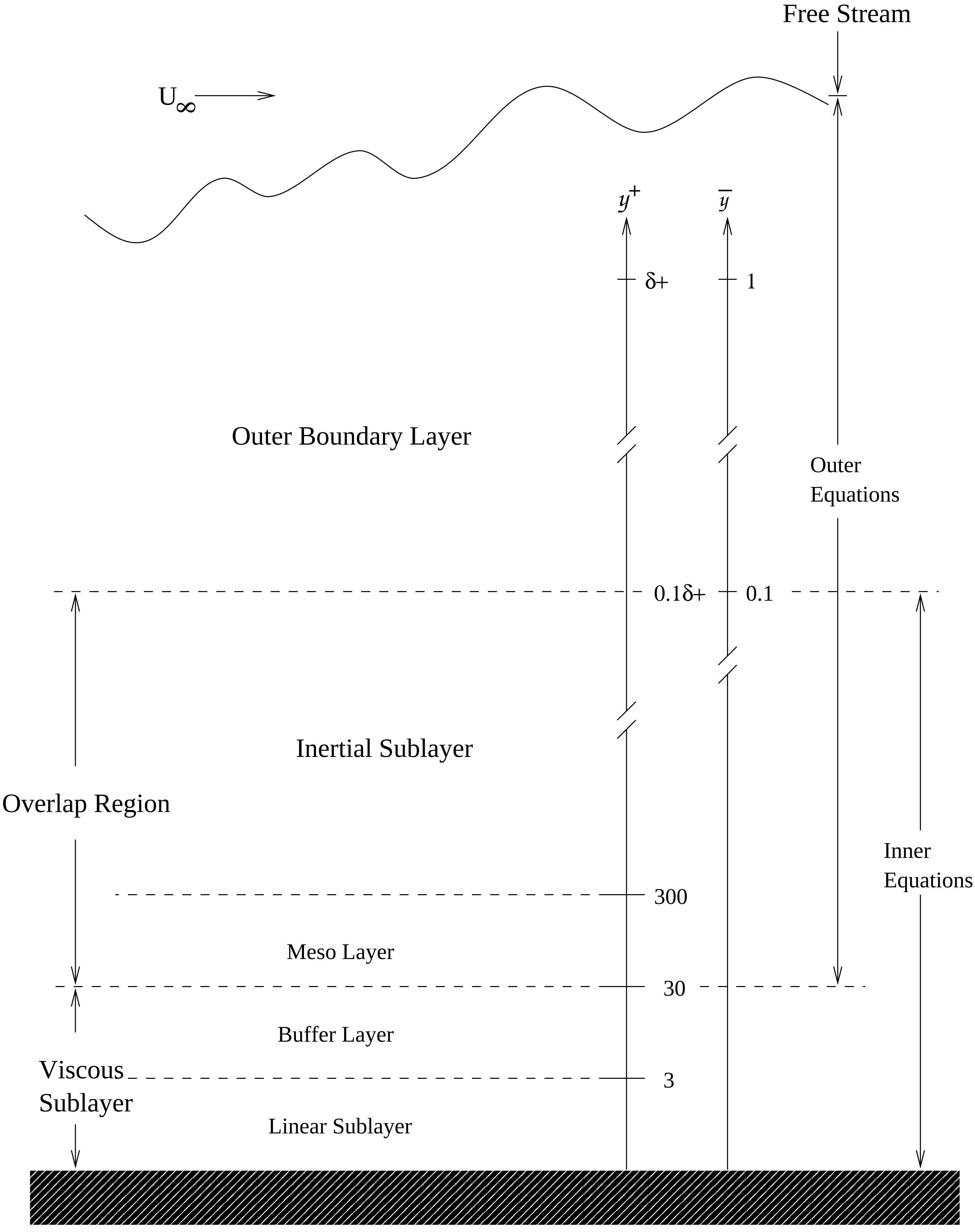

Figure 9. Sketch from GC97 showing various layers in turbulent boundary layer in inner (left) and outer variables (right). The single point 'inner RANS equations' have no mean convection terms in the constant stress region. The single point 'outer' equations have no viscosity. And the *single point* equations for the 'overlap region' have neither mean convection nor viscosity, only Reynolds shear stress. But in the meso-layer 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 $\delta^+ > 300$ is a necessary condition for there to be an inertial sublayer at all. Thus many experiments at lower Reynolds numbers have *only* a mesolayer!

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 to 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, since 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 'meso'-layer, 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 equal if the Reynolds number is high enough), but the two point equations are not at any scale of motion.

Why does all of this matter? Who really should care about this? Well first of all, experimentalists should, since they really shouldn't be fitting log or power law profiles to this region. Happily this is one of the greatest change over the past decade: now few do! When GC was first presented in the early 90's (years before publication, for the obvious reason), 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 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. This would seem to make their task easier, since they would need less, not more resolution close to the wall. To the best of my knowledge no one has exploited this to-date.

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 has the beginnings of a $k^{-5/3}$ range? Answer: Outside of y^+ of a few hundred. In GC97 we were not smart enough to think of looking at the structure function equations, but several groups now are. 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. If there were a mesolayer, this is precisely how one would expect the structure functions to behave. (Strangely these comments never seem to appear in the published versions of their papers, raising again the question of the tyranny of orthodoxy in the turbulence community.) 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.

VII. What have we learned since GC97 that we really didn't 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 flow changes from one similarity state to another. The whole idea of *equilibrium similarity* (as we have come to call our methodology since George[4]) is that the 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 the Castillo and George[25] we asked how it could be that a flow which had evolved into an equilibrium similarity state could ever get out of it, since 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 re-laminarize 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 U. 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 U. Arizona) were never definitive as to the 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 the laminar growth rate of $x^{1/2}$ 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 George[40] which laid out the theory of equilibrium similarity (although it was not called that until later[4]). In fact, I found two similarity solutions, one for very high turbulence Reynolds number, and an 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 couldn't 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. [41]), thanks in part to a very difficult experiment he carried out in the KTH windtunnel, 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 since 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 re-laminarizes 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 above 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 since 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,[25] Elsberry et al.,[42] see especially the appendix). Moreover, they stay there until something catastrophic happens, sometimes even well past separation.[26] But as noted above it was also not clear how they ever get out of this state, which of course they do when they re-laminarize, or when the entire boundary layer detaches. Only a week ago I had the opportunity to participate by phone-link in a presentation at RPI by Bayou Cal, one of Luciano

Castillo's current Ph.D. students (who also spent a year at Chalmers in our turbulence masters program). During that presentation it dawned on 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 have real analytical criteria for avoiding or encouraging separation and re-laminarization.

VIII. Summary and conclusions

So where do I think this all stands. The truth is that I really don't 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 be more apparent 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, since both presume an inner region that is unaffected by the pressure gradient.) Or perhaps the increasing interest on two-point statistical models (like LES) will change the focus from the mean profiles to quantities for which the models do show greater differences.

Regardless of outcome, the exercise and experience of the last decade has taught me a lot about human behavior, and especially that scientists are human. Some years ago I was having breakfast with another recently deceased giant of the field, Tony Perry of the U. of Melbourne. As we were laughing over our differences, a rather distraught young man approached me and asked if he could speak to me privately. When we met later that morning, he laid out for me in numerous plots his recent data, which he felt for the most part showed excellent agreement with GC97. I agreed, and was thrilled, both for him and us. Finally he got to his real problem: his graduation was being held up because his Ph.D. advisor would not accept the student's view of the data which challenged his own biases. A few months later when the same student's dissertation arrived in the mail, I opened it with considerable interest. To my astonishment GC97 was not even referenced, nor was it in the subsequent journal article. His advisor had won, if not the intellectual debate, at least the content of the dissertation. The old ideas he had learned himself as a student were safe and unchallenged. Moreover by insisting on his interpretation in all publications, he had made it more difficult for others to challenge. I've often wondered what became of the student, but I have not heard of nor seen him since. Unfortunately, although this example is particularly egregious, as I have travelled the globe talking about turbulent boundary layers since 1981, I have come to recognize that the suppression of ideas in this field has not been that uncommon (Rolf Karlsson's experience as a case in point). Happily there is evidence that this has at last begun to change, if it has not changed already.

For past few years I have preferred to take a back seat, 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 don't 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 USA 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 has reached its limits, and once again there is the beginning of a recognition that we need some new ideas. Fortunately as we researchers on fundamental turbulence have opened the curtains to let in the light so we can see the dust and cobwebs of our own thinking, we find ourselves in a perfect position to help them. 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.

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References

[1] W.K.George and Castillo, L., "Boundary Layers with Pressure Gradient: Another Look at the Equilibrium Boundary Layer," *Near-Wall Turbulent Flows*, edited by C. S. R.M.C. So and B. Launder, Elsevier, Amsterdam, 1993, pp. 901 – 910.

[2] W. K. George, W. and Castillo, L., "Zero-pressure-gradient turbulent boundary layer," *Appl. Mech. Rev.*, Vol. 50, No. 12, 1997, pp. 689 –729.

[3] Coles, D. E., "Young person's guide to the data," *Proc. AFOSR-IFP-Stanford Conf. on Compuation of Turbulent Boundary Layers*, edited by D. E. Coles and E. A. Hirst, Vol. 2, Thermo Sci. Div., Dept. Mech. Engr., Stanford U., Palo Alto, CA, pp. 1–45.

[4] George, W. K., "Some New Ideas For Similarity of Turbulent Shear Flows," *Proc. ICHMT Symposium on Turbulence, Heat and Mass Transfer, Lisbon, Portugal (1994)*, edited by Hanjalic and Pereira, Hemisphere, NY, 1995.

[5] Gad-el-Hak, M. and Bandyopadhyay, P. R., "Reynolds Number Effects in Wall-Bounded Flows," *Appl Mech Rev.*, Vol. 47, pp. 307 – 365.

[6] Barenblatt, G. J., "Scaling laws for fully developed shear flow, Part I. Basic hypotheses and analysis," *J. Fluid Mech.*, Vol. 248, 1993, pp. 513 – 520.

[7] Oberlack, M., "Unified approach for symmetries in plane parallel shear flows," *J. Fluid Mech.*, Vol. 427, pp. 229–238.

[8] DeGraaff, D. B. and Eaton, J. K., "Reynolds-number Scaling of the Flat-plate Turbulent Boundary Layer," *J. Fluid Mech.*, Vol. 422, 2000, pp. 319–346.

[9] Österlund, J. M., *Experimental studies of zero-pressure gradient turbulent boundary-layer flow*, Ph.D. thesis, KTH, Stockholm, Sweden.

[10] Nagib, H., "The wall shear stress in zero-pressure gradient turbulent boundary layers. Do we know enough?" *Presentation at Perryfest 2004, Queens University, Kingston, ON, Canada*, 2004.

[11] George, W. K., "Governing Equations, Experiments, and the Experimentalist," *J. Exper. Thermal and Fluid Science*, Vol. 3, pp. 557–566.

[12] Castillo, L., *Similarity Analysis of Turbulent Boundary Layers*, Ph.D. thesis, State U. of NY at Buffalo.

[13] Wosnik, M., *On Wall-bounded Turbulent Shear Flows*, Ph.D. thesis, State U. of NY at Buffalo.

[14] Seo, J., *Investigation of the Upstream Conditions and Surface Roughness in Turbulent Boundary Layer*, Ph.D. thesis, RPI, Troy, NY.

[15] Wang, X., "Similarity Analysis for Turbulent Boundary Layers Subject to Pressure Gradient and Heat Transfer," .

[16] Prandtl, L., "Zur Turbulenten Strömung in Rohren und längs Platten," Tech. Rep. 4, Ergeb. Aerod. Versuch Göttingen, 1932.

[17] von Karman, T., "Mechanische Ähnlichkeit und Turbulenz," *Nachr. Ges. Wiss., Math-Phys Klasse Göttingen*, pp. 68–76.

[18] Stanton, T. E. and Pannell, J. R., "Similarity of motion in relation to the surface friction of fluids," *Phil. Trans. Roy. Soc. A*, Vol. 214, pp. 199.

[19] Millikan, C. M., "A critical discussion of turbulent flows in channels and circular tubes," Wiley, NY, pp. 386 – 392.

[20] Clauser, F. H., "The turbulent boundary layer," *Adv. Appl. Mech.*, Vol. IV.

[21] Zagarola, E. and Smits, L. J., "A new mean velocity scaling for turbulent boundary layers," .

[22] Wosnik, M., Castillo, L., and K.George, W., "A theory for turbulent pipe and channel flows," *J. Fluid Mechanics*, Vol. 421, pp. 115–145.

[23] George, W. K., Abrahamsson, H., Eriksson, J., Karlsson, R. I., Löfdahl, L., and Wosnik, M., "A similarity theory for the turbulent plane wall jet without external stream," *J. Fluid Mechanics*, Vol. 425, pp. 367–411.

[24] Johansson, T. G. and R. I. Karlsson, title = Measurements issues in high Reynolds number flows, j. . A. v. . . p. . . y. . .

[25] Castillo, L. and George, W. K., "Similarity Analysis for Turbulent Boundary Layer with Pressure Gradient: The Outer Flow, AIAA Journal," *AIAA Journal*, Vol. 39, 2001, pp. 1–41.

[26] Castillo, L., Wang, X., and George, W. K., "Separation Criterion for Turbulent Boundary Layers Via Similarity Analysis," *J. of Fluids Engr. Trans. A.S.M.E.*.

[27] Wosnik, M. and George, W. K., "Reconciling the Zagarola/Smits Scaling with the George/Castillo Theory for the Zero Pressure Gradient Turbulent Boundary Layer," *AIAA Paper No.: 2000-0912 presented at AIAA Annual Meeting, Reno, NV, Jan. 2000*.

[28] Tennekes, H. and Lumley, J. L., *A First Course in Turbulence*, MIT Press, Cambridge, MA.

[29]Gamard, S. and George, W. K., "Reynolds number dependence of energy spectra in the overlap region of isotropic turbulence," *Flow, Turbulence and Combustion*, Vol. 63, pp. 443 – 477.

[30]Mydlarski, L. and Warhaft, Z., "On the onset of high-Reynolds number grid-generated wind tunnel turbulence," *J. Fluid Mechanics*, Vol. 320, pp. 331–368.

[31]Wosnik, M., Castillo, L., and George, W. K., "A theory for turbulent pipe and channel flows," *J. Fluid Mechanics*, Vol. 421, pp. 115–145.

[32]Wosnik, M., "Improvements to the George/Castillo Boundary Layer Theory," *Bull. Am. Phys. Soc.*, Vol. 45, pp. 9.

[33]Nagib, H., "Seminar at Chalmers University, Gothenburg, Sweden, Nov." 2004.

[34]Karlsson, R. I., Ph.D. thesis, Chalmers University of Technology, Gothenburg, Sweden.

[35]Smith, R., *The effect of Reynolds number on the structure of turbulent boundary layers*, Ph.D. thesis, Princeton U.

[36]Buschmann, M. H. and el Hak, M. G., "Debate Concerning the Mean-Velocity Pro. le of a Turbulent Boundary Layer," *AIAA J.*, Vol. 41, No. 4, 2003, pp. 565 – 572.

[37]Österlund, J. M., Johansson, A. V., Nagib, H. M., and Hites, M. H., "A note on the overlap region in turbulent boundary layers," *Phys. Fluids*, Vol. 12, 2000, pp. 1.

[38]Long, R. R. and Chen, T.-C., "Experimental Evidence for the Existence of the 'Mesolayer' in Turbulent Systems," *J. Fluid Mech.*, Vol. 105, pp. 19–59.

[39]George, W. K. and Knect, P. J., "Refinement of a Power Law Theory for the Matched Layer of a Zero Pressure Gradient Boundary Layer," *Bull. Am. Phys. Soc.*, Vol. 35, 10, pp. 2294.

[40]George, W. K., *Self-Preservation of Turbulent Flows and Its Relation to Initial Conditions and Coherent Structures*.

[41]Johansson, P. B. V., George, W. K., and Gourlay, M. J., "Equilibrium similarity, effects of initial conditions and local Reynolds number on the axisymmetric wake," *Phys. Fluids*, Vol. 15, No. 3, pp. 603–617.

[42]Elsberry, K., Loeffler, J., Zhou, M. D., and Wygnanski, I., "An experimental study of a boundary layer that is maintained on the verge of separation," *J. Fluid Mech.*, Vol. 423, pp. 227–262.