Governing Equations, Experiments, and the Experimentalist

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■ It is argued that the most important test of the understanding of any experiment is whether or not the results are consistent with the equations and boundary conditions believed to govern the flow. If they are not, then either the measurements are incorrect, the equations or boundary conditions are wrong, or the experiment performed was not the one believed to have been done. It is suggested that many apparently contradictory experimental results are, in fact, the result of the latter and have not been recognized as such because of a failure to apply the governing equations to the data The arguments are supported by examples from turbulent shear flow experiments.

Keywords: *governing equations, experiments, experimentalist, turbulent shear flow, plumes, jets, grid turbulence, screens, coflow*

INTRODUCTION

One of the principal difficulties in using the results of experimental research to test theoretical and numerical models is that often the experiments were not what they were believed to have been. While the measurements themselves may have been of the highest quality, the experimenter (and theoretician) may have failed to notice the sensitivity of the results to the externally imposed conditions of the experiment. On the other hand, he may have observed the relation of his results to the experimental design and then satisfied himself by reproducing the results of others, often by duplicating their conditions. From my own experience, and from that acquired by teaching numerous courses on flow measurement to already established experimenters, it is my opinion that most measurements, including many which appear anomalous, have been performed carefully and competently. Sometimes problems arise, however, because the experimenter may not have been entirely clear about which experiment he carried out.

In large part, I believe that the failure of experimenters to recognize which experiment they have performed comes about because of their failure to use their most powerful experimental tool—the *equations of motion*. Every flow we measure is the realization of a solution of the governing equations for the boundary and initial conditions we have imposed. The most fundamental test of our understanding of an experiment is whether or not we can write an appropriately reduced set of equations to describe it—for example, for boundary layers, the boundary layer equations; for wakes, the wake equations. If our understanding is correct and our measurements are of acceptable accuracy, the measured profiles should be consistent with the governing equations. Yet in spite of the obvious nature of this statement, there are few examples (particularly in the turbulence literature) of where experimenters have made the effort to verify that their measurements satisfy the dynamical equations believed to govern their experiment.

In the following paragraphs, I shall attempt to illustrate the perils the experimenter risks by ignoring the equations of motion. I shall do so by drawing on some of the work my students, coworkers, and I have carried out over the past decade or so in turbulent flows. The objective here is not to present the results themselves, most of which can be found elsewhere, or to emphasize how clever we have been. Rather, the objective is to illustrate how much more clever we might have been (not to mention better stewards of the research funds we were given) if we had kept the dynamical equations clearly in mind from the outset. In particular, more definitive and useful experiments could have been carried out before the researchers and sponsors were exhausted. Finally, lest the theoreticians feel too smug about this public airing of the shortcomings of experimenters, I shall conclude by suggesting that they too should be more sensitive to what the experiments are telling them.

THE AXISYMMETRIC BUOYANT PLUME

In the summer of 1974, together with my coworkers R. Alpert and F. Tamanini at the Factory Mutual Research Corporation, I carried out a series of measurements in an axisymmetric buoyant plume facility (see George et al [1]). These measurements differed rather sharply from the earlier measurements of Rouse et al [2]. The differences were believed to have been largely due to the more sophisticated instrumentation used in our experiment. Nonetheless, there was still considerable debate in the turbulence modeling community over which profiles were to be preferred (see Chen and Rodi [3]).

To provide some further insight into the differences between the two experiments, and to extend the measurements to all of the moments of interest in the turbulence kinetic energy balance, I initiated an experimental program at the University at Buffalo using the same plume source as for the Factory Mutual experiments, but in a somewhat more sophisticated setup. In this experiment (described in detail by Beuther [4]), a ma-

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jor amount of effort was expended to ensure that the source conditions could be accurately monitored. An important characteristic of the plume is that the rate at which buoyancy is added at the source must, for a neutral environment, be equal to the buoyancy integral at each height. Thus, whether or not the buoyancy integral remains equal to the source value is a crucial test of the validity of the experiment. Hence, the absence of source information had been a serious shortcoming of the previous experiments.

The appropriate form of the buoyancy integral for air can be obtained by integrating the temperature equation across the flow and is given by

$$F = 2\pi \int_0^\infty g\beta[U\,\Delta T + \overline{ut}]r\,dr \qquad (1)$$

where F is the buoyancy integral, r is the radial coordinate measured from the plume axis, g is the acceleration of gravity, β is the coefficient of thermal expansion, U and u are the mean and fluctuating vertical (streamwise) velocities, ΔT is the temperature difference from the ambient temperature (assumed constant), and t is the fluctuating temperature. For a neutral environment, $F = F_0$, where F_0 is the rate at which buoyancy is added at the source. Without the source conditions, F_0 could not be determined and could only be inferred from the profile measurements. The accurate monitoring of source conditions in plume flows is, however, not exactly a trivial task, because of the high temperatures and low velocities present, and the absence of information about them in the early experiments did not always represent a lack of effort.

In order to have some perspective for our reasoning in evaluating our experiment, it is necessary to have some idea of the experiment itself. The source provided heated air at about 300°C above the ambient temperature at a velocity of about 0.5 m/s and was approximately 6 cm in diameter. The experiment was conducted inside a screened enclosure about 2 m square by 7 m high that was open at the top and bottom. Figure 1 shows a sketch of the experimental setup. Measurements were taken only in the bottom 3 m of the facility where the ambient temperature variation was only about 1°C, whereas the top of the facility became stably stratified. We believed the slight ambient temperature variation in the measurement region to be negligible since the centerline temperatures between 1 m and 3 m varied from 50 to about 15°C above the ambient value (nominally 300 K). Both resistance wires (for temperature) and hot wires were sampled simultaneously, and care was taken to calibrate the hot wires over the entire range of the temperature and velocity variation, as in the earlier experiment. A major difficulty in making the measurements was the very long time required for measurement to achieve statistically stable averages because of the long time scales of the flow, about 12 h being required for traverses at several heights.

That we had problems became apparent almost immediately, as illustrated by the mean centerline velocity and buoyancy (temperature) values in Table 1 which summarizes these and other quantities of interest for this and several earlier and subsequent experiments. These first data (denoted as Beuther et al [5]) differed, but only slightly, from those measured earlier by George et al [1] using similar techniques. Just prior to submission of the paper (against a deadline), we had recognized a difficulty with the fluctuating temperature measurements, which were substantially lower than the earlier results.

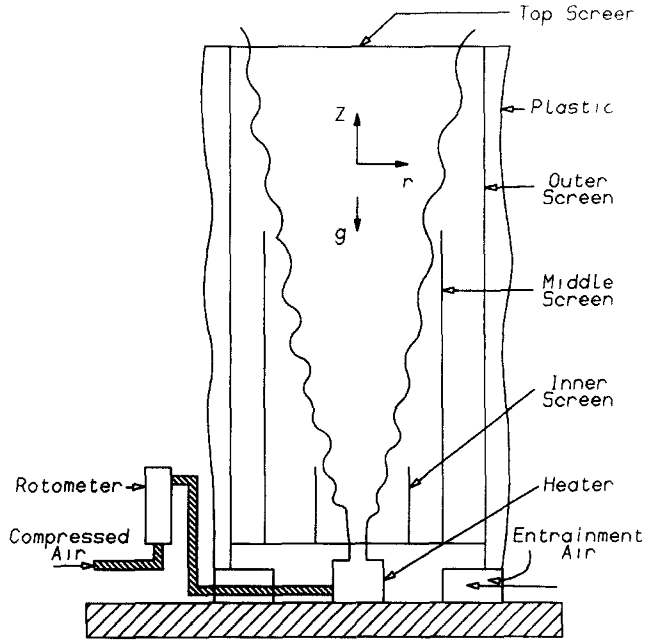

Figure 1. Sketch of plume facility [4-7]

This difference was believed at the time to be due to a problem in the data acquisition, and this was confirmed (we thought) by quickly remeasuring only the temperatures and compensating the other measurements involving temperature appropriately. It was only after publication that we checked the buoyancy integral against the value at the source and, to our horror, realized that nearly 20% of the rate at which buoyancy was added at the source could not be accounted for. This together with the temperature problem made it clear to us that we had (we thought) some serious measurement errors and that the entire experiment should be reconsidered. (Wouldn't it be nice if there were a mechanism for recalling our mistakes from the literature, in this case Ref. 5?)

After a considerable rethinking of the experiment, the measurements were repeated. This time the root-mean-square temperature fluctuations were even lower than before ($t'/\Delta T \approx 30\%$), and the buoyancy integrals ranged from about 50% to 70% of the source value, decreasing with height. It was this latter fact which provided the clue as to what should have been obvious from the beginning. We had thought the 1°C variation in ambient temperature to be negligible, and it certainly was compared to the other temperature variations of interest. The important consideration was, of course, not what

Table 1. Summary of Plume Centerline Data

Ref.	$U_c x^{1/3}/F_0^{1/3}$	$g\beta\Delta T_c \frac{x^{5/3}}{F_0^{2/3}}$	u'/U_c	$t'/\Delta T_c$	$\overline{ut}/u't'$
2	4.5	13.5	—	—	—
1	3.4	9 1	0.25	0.38	0.67
5	3.6	9 5	0.27	0.36(?)	0.55(?)
4	3.8	10.4	0.26	0.30	0.59
6	3.5	9.6	0.25	0.38	0.64
7	3 2	9.4	0 31	0.38	0.6

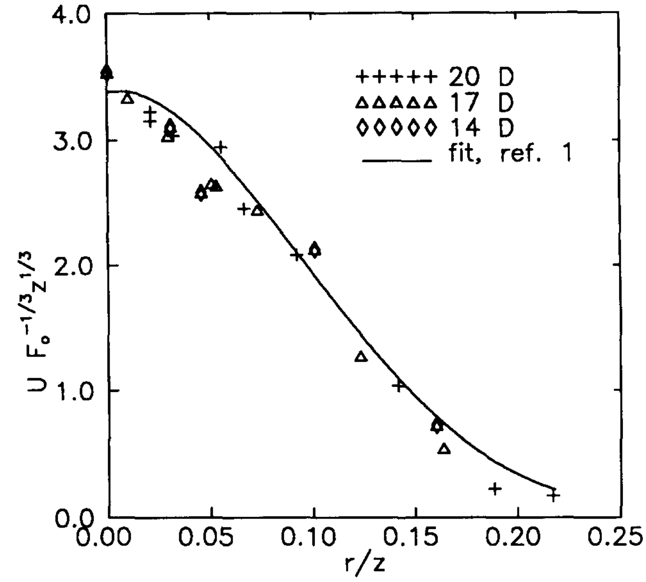

Figure 2. Plume mean velocity profile for neutral environment [7].

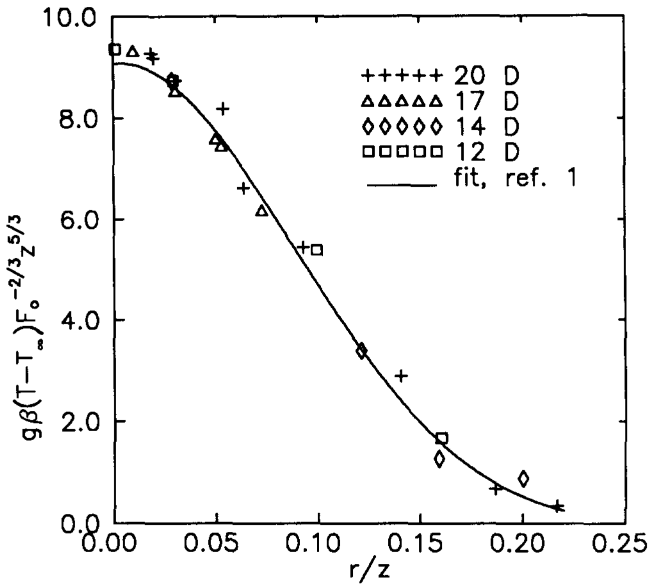

Figure 3. Plume mean temperature difference profile [7].

we thought, but what the flow experienced as measured by the dynamical equations.

The integrated buoyancy equation with a height-dependent ambient temperature can be derived from the temperature equation in the following form (Beuther and George [8]):

$$\frac{dF}{dx} = \frac{d}{dx}\left\{2\pi \int_0^\infty g\beta[U\,\Delta T + \overline{ut}]r\,dr\right\}$$
$$= \left(-g\beta \frac{dT_\infty}{dx}\right) 2\pi \int_0^\infty U r\,dr \qquad (2)$$

where F is the local buoyancy integral, x is the vertical coordinate, and T_∞ is the ambient temperature. For stably stratified flow, the ambient temperature increases with height, and the velocity integral is always positive. Thus the flow loses buoyancy by entraining ever lighter fluid.

Our measurements showed clearly that the ambient temperature gradient we had assumed to be negligible was not negligible as far as the plume was concerned. In fact, our earlier measurements had probably never been in error; we simply had not been measuring a flow in a neutral environment as we had believed. As for our rapidly remeasured temperature profiles mentioned above, the facility simply had not had time to stratify before the measurements were made. Thus both sets of measurements were correct, but the flows were different. Stratification of the ambient was likely to have been present in some of the earlier measurements of others as well, and is probably the root of most of the discrepancies among different plume data.

By being particularly sensitive to the manner in which the flow responds to its environment *as measured by the dynamical equations*, it has been possible to make measurements which are representative of buoyant plumes in a neutral environment. Figures 2 and 3 show the profiles of the mean velocity and buoyancy measured by Shabbir [7] in an experiment for which both buoyancy and momentum conservation could be confirmed. Also shown in the figures are the earlier profiles measured by George et al. [1]. The agreement of the former with the latter must be regarded as "fortunate coincidence" in view of what transpired between the times of the two sets of measurements. An unexpected benefit of our efforts to measure the plume in a neutral environment is the insight gained into the effects of stratification on plumes. A word of caution about the measurements is in order, however, because of the problems confirmed subsequently in using stationary hot-wire probes for jet flows that are quite similar (see Figure 5 and discussion below).

THE AXISYMMETRIC JET

One of the most frustrating problems faced by the turbulence modelers over the past decade and a half has been their inability to calculate the axisymmetric jet in an infinite environment without choosing a set of model constants for just this flow. Almost every paper on modeling which attempts to calculate a variety of turbulent flows presents a separate set of "universal" constants which apply only to the axisymmetric jet. The problem has been that the models tend to predict a spreading rate which is too large when compared to most of the measurements. The measurements, on the other hand, have been remarkably consistent in obtaining jet half-widths of about $\delta_{1/2} \approx 0.85x$, where x is measured from an appropriate virtual origin, usually several diameters downstream from the source.

Some years ago, C. B. Baker, D. B. Taulbee, and I were interested in computing the evolution of a vertical heated jet into a buoyant plume. Since momentum is continuously produced in a plume by buoyancy, and since the evolution from jet to plume occurs because the momentum produced by buoyancy begins to overwhelm that added at the source, it was of interest for us to monitor how the momentum at any cross section of the flow changed with distance downstream. Accordingly, the program was written to ensure that the momentum equation integrated across the flow was satisfied to a high degree of accuracy and the actual value of momentum was read out at each step.

Since the jet itself was not of interest (being presumably well understood), the calculation was initiated by using the jet profile measured by several well-known experimenters, their measurements representing a reasonable consensus of most of the others. Much to our dismay, the calculation (even after considerable effort to correct the program) could never proceed beyond the first step, because the momentum integral could never be satisfied to the required accuracy. It was only after all else failed that it occurred to us to check whether or not the starting profile we had chosen was consistent with conservation of momentum.

The unique feature of the jet flow is that the integral of the streamwise momentum equation across the flow at any streamwise location must be equal to the rate at which momentum is added at the source. The appropriate form of the momentum equation is derived by using the cross-stream momentum equation to eliminate the pressure from the streamwise equation. The momentum integral for the axisymmetric jet to second order can then be obtained as (Capp [9])

$$\rho M_0 = \int_0^\infty \left(U^2 + \overline{u^2} - \frac{\overline{v^2} + \overline{w^2}}{2} \right) r\,dr \tag{3}$$

where ρM_0 is the rate at which momentum is added at the source, U and u are the mean and fluctuating streamwise velocities, v and w are the fluctuating cross-stream components, and r is the radial coordinate. It was straightforward to show that neither the profiles chosen nor any of the others available could satisfy the momentum integral equation to within better than 60–80%, discrepancies much larger than the apparent measurement error could explain (Baker [10], Seif [11]). Clearly there was a problem here, and naturally we assumed it to be in the measurements.

Our first conjecture was that the problem with the measurements lay in the use of the hot-wire technique, because the jet is a flow where the *local* turbulence intensity is a minimum of 25–30% at the centerline and increases rapidly with radius. We were, however, troubled by the fact that the measured profiles appeared to be too narrow to conserve momentum, which was the opposite of what would have been expected from the known sources of hot-wire errors which act to increase the measured values (especially cross-flow and rectification errors). Nonetheless, we were convinced that the problems lay in the measuring technique, and initiated our own axisymmetric jet investigation using burst-mode LDA techniques. (The details of this investigation have been reported by Capp [9] and more recently by Capp et al [12].) As expected, the velocity profile at $x/D = 70$ was significantly wider than previously reported ($\delta_{1/2} = 0.095x$). Entirely unexpected was the fact that the centerline mean velocity decayed less rapidly with distance than in the earlier measurements. (This was a surprise because the hot-wire measurements would have been expected to be the most accurate near the center of the flow where the local turbulence intensity is the lowest.) From a self-preservation analysis of the averaged equations of motion the asymptotic centerline velocity can be shown to be of the form

$$U_c = BM_0^{1/2}/x \tag{4}$$

For our measurements, $B = 5.95$, while for those of Wygnanski and Fiedler [13], for example, $B = 5.0$. This rather substantial difference could not be attributed to the usual errors on the hot wires. Nonetheless, the new LDA measurements did satisfy the momentum integral to within a few percent.

The normalized profile measurements at $x/D = 100$ differed only slightly from those at $x/D = 70$; and while the momentum integral was not satisfied as well as at the upstream location, it still integrated to within 5–6% of the expected value. We were therefore not prepared for the results we obtained as we moved the measuring station past $x/D = 100$. As shown in Figure 4, the centerline velocity began to drop more rapidly than expected. Also the profile narrowed, and the momentum integral fell increasingly short of the rate of momentum addition at the source. The differential equations of motion appeared to be well satisfied by the measured values. Clearly something new was happening in the flow that was inconsistent with the dynamical equations and constraints for a jet in an infinite environment.

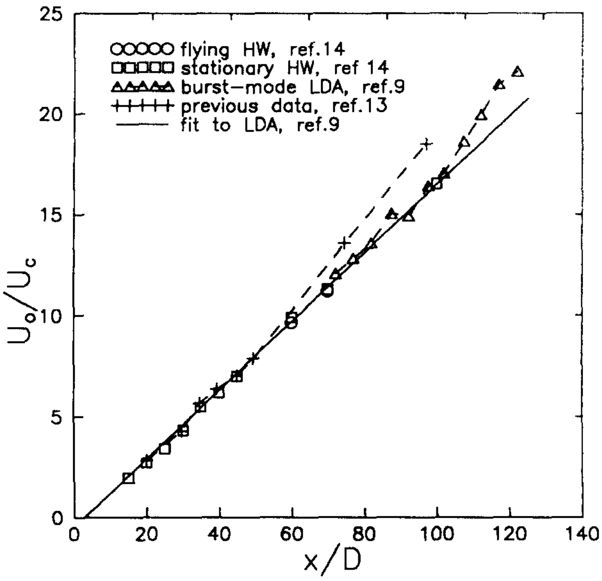

Figure 4. Jet centerline decay [9].

The solution to our dilemma (and to understanding the earlier measurements) lay in recognizing the fact that our experiment (and all of the others as well) was being carried out, not in an infinite environment, but in a box (or room) of finite dimensions. It mattered not whether we *thought* the walls were sufficiently far away to have no influence on the flow. What mattered was whether or not the walls *were* far enough away that the flow was governed by equations appropriate to an infinite environment. The failure of the momentum integral to fully account for the momentum added at the source at every cross section of the flow was a clear indication that the walls were too close.

The physical reason for the failure of our experiment (and the earlier ones as well) is quite easy to see—in hindsight. The jet can spread only by entraining mass from the nonvortical fluid outside of it. In an infinite jet this entrained mass is provided from infinity, but in a laboratory jet it must be fed by a reverse mean flow outside of the jet itself. Since at any cross section of the closed room the net mass flow must be zero, the reverse flow must increase in magnitude in the streamwise direction because of the increasing mass flow of the jet itself. While this would not appear to have major consequences for the velocity measurements if the room were large, it can have very important consequences for the dynamics of the jet itself. The reason is that the negative momentum being carried backwards makes the same contribution to the momentum integral as positive momentum being carried forwards. This can be easily seen by splitting the momentum integral into two parts: an integral over the jetlike part of the flow, and one over the return flow:

$$M_0 = 2\pi \int_0^{R_{\text{jet}}} \left(U^2 + \overline{u^2} - \frac{\overline{v^2} + \overline{w^2}}{2} \right) r\,dr + \iint_{\text{return}} U^2\,dA \tag{5}$$

where R_{jet} is chosen to enclose the jetlike part of the flow.

It is clear that the negative return flow "steals" momentum from the primary jet flow. Since it is only the constancy of the momentum integral for the jet alone which distinguishes the flow as a jet in an infinite environment, the flow naturally no longer behaves as such a jet when the return flow contribution to the integral becomes significant. By rather simple arguments it is possible to show that for the jet momentum integral at $x/D = 100$ to retain 95% of the rate at which momentum is added at the source, the area of the room must be more than 10^5 times that of the exit area of the jet (see Ref. 9)! This is substantially greater than for most experiments, especially when the effects of screen enclosures are also considered (see below).

Figure 5, taken from Hussein [14], shows the mean velocity profiles measured in a facility which is large enough to allow the jet to behave like a jet in an infinite environment for several hundred diameters. The profiles were measured with burst-mode LDA and both fixed and flying hot-wire techniques. The important point is not that the moving hot-wire probe and burst-mode LDA results are the same (since both could be wrong), but rather that they satisfy the complete governing equations for an axisymmetric jet in an infinite environment. Most of the other moments also differ significantly from those previously reported. It perhaps comes as no surprise in view of what we now know that this new profile can be calculated by a turbulence Reynolds stress model—without changing the model parameters from the planar case (see Taulbee et al [15]).

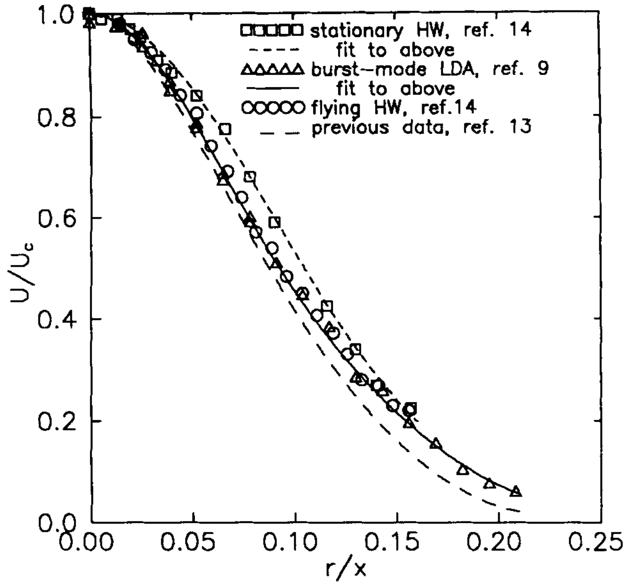

Figure 5. Jet mean velocity profile [14].

THE EFFECTS OF SCREENS AND COFLOWING STREAMS

A common practice in experiments involving turbulent shear flows is to attempt to isolate the experiment from environmental effects by means of a screened enclosure. This has been particularly the case where the flows have low or zero mean velocity at their outer edges. The idea is to allow the relatively weak entrainment flow necessary to sustain the growth of the shear region, while at the same time to dampen disturbances which are sometimes unavoidably present in the laboratory. The physical basis for this use of screens is that the pressure drop across them is a function of the flow rate, so the higher velocity disturbances are more strongly damped. This is, of course, the same reason screens are employed in the settling chambers of wind tunnels to smooth disturbances in the flow.

Another common practice is to impose, on the shear flow of interest, a weak coflowing stream. This greatly facilitates measurement in the regions of the flow where the mean velocity is very low, since most velocity-measuring techniques function poorly without a mean flow. The implicit assumption in this practice is that the limiting shear flow at zero coflow is smoothly approached as the coflowing stream velocity is reduced.

My coworkers and I became acutely aware of the hazards associated with both of these approaches in our attempts to carry out the buoyant plume experiments described above. After several generations of students managed to produce quite different plumes with what appeared to be only slight changes to the facility, we began to suspect that our screens were, in fact, not far enough away. Our suspicions were confirmed by the measurements of the mean temperature profiles for several different screen configurations as shown in Figure 6, taken from Shabbir [7]. Clearly there was a need for more than just a guess on our part as to whether or not the screens were far enough away. Once again we had been guilty of ignoring the equations of motion in the design and conduct of our experiments. While we had not used coflowing streams in our experiments, the differences between our results and those of others who had caused us to suspect a similar problem.

Since the screens and coflowing streams affect the entrainment flow, it makes sense to ask what dynamical role the entrainment flow plays in a turbulent shear flow. (Note that its kinematical role is obvious—it increases the mass of the turbulent flow.) It is easiest to address this question by considering the two-dimensional example of the planar jet. The governing equations at high Reynolds number can be written as

$$U\frac{\partial U}{\partial x} + V\frac{\partial U}{\partial y} = -\frac{1}{\rho}\frac{\partial P}{\partial x} + \frac{\partial}{\partial x}(-\overline{u^2}) + \frac{\partial}{\partial y}(-\overline{uv}) \tag{6}$$

$$U\frac{\partial V}{\partial x} + V\frac{\partial V}{\partial y} = -\frac{1}{\rho}\frac{\partial P}{\partial y} + \frac{\partial}{\partial x}(-\overline{uv}) + \frac{\partial}{\partial y}(-\overline{v^2}) \tag{7}$$

$$\frac{\partial U}{\partial x} + \frac{\partial V}{\partial y} = 0 \tag{8}$$

where x is the streamwise coordinate, y is the cross-stream coordinate, U and V are the mean streamwise and cross-stream velocity components respectively, u and v are the corresponding fluctuating velocity components, and P is the mean pressure. One of the characteristics of shear flows in general and jets in particular is that they tend to vary more

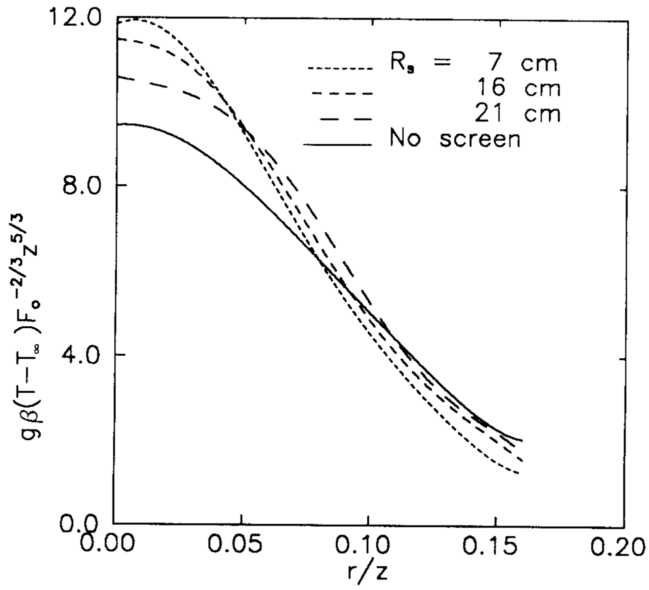

Figure 6. Effect of screen configuration on plume mean temperature difference profile [7]

slowly in the streamwise direction than across the flow, so that $\partial/\partial x \ll \partial/\partial y$. If the external flow is also at rest, the equations can be reduced to

$$U\frac{\partial U}{\partial x} + V\frac{\partial U}{\partial y} = -\frac{1}{\rho}\frac{\partial P}{\partial x} + \frac{\partial}{\partial y}(-\overline{uv}) \qquad (9)$$

$$0 = -\frac{1}{\rho}\frac{\partial P}{\partial y} + \frac{\partial}{\partial y}(-\overline{v^2}) \qquad (10)$$

The y-momentum equation, Eq. (10), is of primary interest here because it makes it clear that the cross-stream pressure gradient which drives the entrainment flow is almost entirely due to the gradient of the cross-stream turbulence intensity. This equation can be integrated across the flow from $y = \infty$ (where the pressure is known and there is no turbulence) to any location y to obtain

$$P_\infty - P = \rho\overline{v^2} \qquad (11)$$

Since $\rho\overline{v^2}$ is positive and is maximum near the center, the pressure drops with decreasing radius. It is this radial pressure gradient which is responsible for the mean entrainment flow. Since $\overline{v^2}$ is a second-order quantity, so are the pressure drop and the mean velocity field it produces. Thus, consistent with our hypotheses about the jet, its spreading rate is also a second-order quantity. It should be clear that anything which interferes with this rather delicate pressure variation will directly affect the spreading rate of the jet.

To see now the effects of screens and coflowing streams, it is necessary to return to the full cross-stream momentum equation, which can be modified by adding to it the continuity equation, Eq. (8), multiplied by V. Rearranging the terms yields

$$\frac{1}{\rho}\frac{\partial P}{\partial y} = \frac{\partial}{\partial y}(\overline{v^2} + V^2) + \frac{\partial}{\partial x}(\overline{uv} + UV) \qquad (12)$$

This can also be integrated from infinity to y to yield

$$\frac{P_\infty - P}{\rho} = [(\overline{v^2} + V^2) - V_\infty^2] + \frac{d}{dx}\int_y^\infty (\overline{uv} + UV)\,dy' \qquad (13)$$

From continuity it follows that $V \approx U\delta/L$, while from the necessity of retaining the Reynolds stress in the averaged equations,

$$\overline{v^2} \approx \overline{uv} \approx U^2\delta/L$$

where

$$\delta \approx \left(\frac{\partial}{\partial y}\right)^{-1} \quad \text{and} \quad L \sim \left(\frac{\partial}{\partial x}\right)^{-1}$$

Then both the $V^2 - V_\infty^2$ term and the integral term are of order $U^2(\delta/L)^2$, which is an order of magnitude less than $\overline{v^2}$ and consistent with Eq. (11). (Note that $\delta/L \approx 0.1$ typically.) It is clear, however, that even if these terms are small, they still have a second-order influence on the entrainment flow. Thus if either term is modified by the boundary conditions by an amount equal to its value in a jet in an infinite environment, then the pressure gradient driving the entrainment flow and the spreading rate of the jet will change by the same fraction. In view of the variety of external conditions imposed by different experimenters, it is not surprising that many papers have been written trying to reconcile variations of less than 10% in the spreading rates of free shear flows.

The effect of a coflowing stream is to cause the integral of Eq. (13) to become divergent unless the channel is of finite width (as it always must be). If U_0 is the coflowing velocity and H is the channel half-width, the streamwise derivative of the extra contribution to the integral term is of order $U_0UH(\delta/L)$. This must be much less than the value of the integral term without the coflow for the jet to develop as a jet in an environment at rest. Thus the necessary constraint on the coflow is

$$U_0UH(\delta/L^2) < U^2(\delta/L)^2$$

or

$$U_0H < U\delta \qquad (14)$$

The product U_0H can be recognized as a measure of the volume flow in the coflowing stream, while $U\delta$ is the volume flow in the jet. The inequality can be satisfied only if the total volume flow in the coflow is less than that in the jet, a very low value indeed! (It is interesting to note that this inequality is satisfied if the jet is entirely fed by backflow as discussed earlier, since the net volume flow back equals that forward in the jet.) Note that the considerations here are quite distinct from those dealt with earlier regarding the streamwise momentum. For the case of coflow, streamwise momentum is added to the jet by the entrainment flow, so the momentum integral for the jet part of the flow is continuously increasing. In view of both these considerations, measurements taken in coflowing streams should be viewed as being just that, and are probably seldom representative of flows which develop in an environment at rest.

The effect of the presence of screens on the entrainment flow can also be considered from Eq. (13). In effect, placing a screen in the entrainment flow creates a jump in the pressure field due to the pressure drop across it. If the screen is modeled by its pressure coefficient C_p, Eq. (13) can be modified to obtain

$$\frac{P_\infty - P}{\rho} = \left[\frac{C_p}{2}\right] V_s^2 + [(\overline{v^2} + V^2) - V_\infty^2] + \frac{d}{dx}\int_y^\infty (\overline{uv} + UV)\,dy' \qquad (15)$$

where V_s is the velocity at the screen.

For the screen to have a negligible effect on the development of the flow, the first term on the right-hand side of Eq. (15) must be at least *two* orders of magnitude less than $\overline{v^2}$. Thus,

$$\frac{1}{2}C_p V_s^2 \ll \overline{v^2} \qquad (16)$$

For a planar jet, the ratio of the rms value of v to the centerline velocity is about 20%, which is only about a factor of 2 higher than the asymptotic value of the entrainment velocity ($V_s/U_c \approx 0.1$). Thus the choice of screen, and in particular its pressure coefficient at the Reynolds number corresponding to the entrainment velocity through it, is critical in determining whether or not the flow develops as a jet in a porous box (or even as a jet in an ordinary box) or as a jet in an infinite environment.

Similar considerations apply to all other shear flows. The two-dimensional flows present the greatest difficulties, because the entrainment velocities tend to go to constant values outside the shear region. The axisymmetric flows, on the other hand, have the nice feature that their entrainment velocities roll off as the inverse of the distance from the axis. Thus it is always possible to find a location for the screen far enough away that the effect on the flow is negligible. Because of the high entrainment rates of jets and plumes, however, this location can be very far away indeed, and screens may be better avoided altogether.

THE DECAY OF GRID TURBULENCE

The last example I shall consider here comes from some recent work that Y. O. Han and I carried out, the initial purpose of which was to carry out measurements of turbulence through a strong contraction. It had been noticed some years before by Shabbir [16] that the existing measurements of these flows did not satisfy the turbulence kinetic energy equations unless the dissipation rate (which had not been measured) was negative, a physical impossibility. Therefore, an important objective of our experimental program was to ensure that our measurements did conserve energy with the correct sign of the dissipation. To be certain of this, several of the fluctuating velocity derivatives were to be measured directly so as to provide an independent measure of the dissipation.

It would appear from the statement of the problem that we were finally beginning to learn from our previous experiences and were considering the equations of motion from the outset. And so we were—at least as far as the measurements through the contraction were concerned. We were much more casual, however, about the measurements behind the grid in the duct preceding the contraction. These measurements were of interest only to determine the state of the turbulence entering the contraction and to ensure that our grid was like everyone else's. It was not!

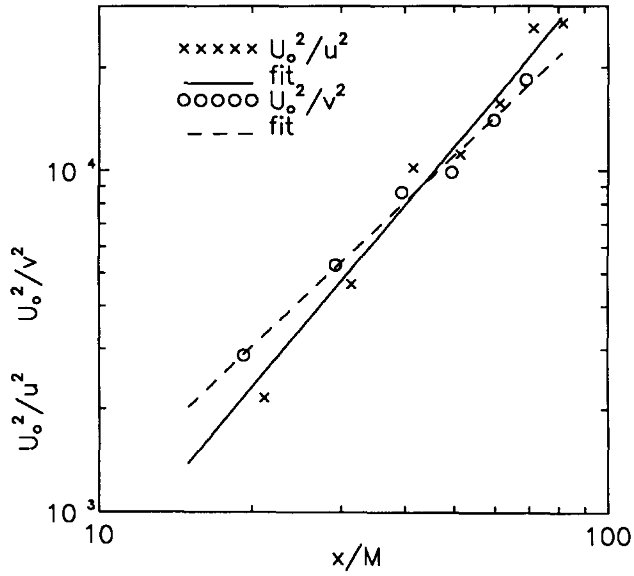

Figure 7. Decay of $\overline{u^2}$ and $\overline{v^2}$ behind grid [17].

Figure 7, from Han [17], shows the traditional plots for turbulence behind a grid of $U_0^2/\overline{u^2}$ and $U_0^2/\overline{v^2}$ versus x/M, where U_0 is the mean duct velocity (assumed constant), $\overline{u^2}$ and $\overline{v^2}$ are the mean square streamwise and radial fluctuating velocities, x is the distance to the grid, and M is the grid mesh size. The unusual feature of the measurements which caught our attention is the relatively large difference in the power law exponents describing the decay of the two components of the turbulence, -1.79 versus -1.46. In most of the measurements previously reported of turbulence behind a grid, the different components decayed with power laws that were almost equal (see Comte-Bellot and Corrsin [18]). (In fact, the slight differences observed in the rates of decay have been the basis for theories on the "return to isotropy" and on the role of the pressure–strain rate terms in distributing the energy among components.)

Our measurements appeared even more strange when the anisotropy ratio $[\overline{u^2}/\overline{v^2} - 1]$ was plotted as shown in Figure 8. This ratio is always positive near a normal grid but has generally been observed to approach zero as the turbulence decays and is swept downstream. Our anisotropy ratio approached zero rather rapidly, and then passed through it to negative values. Even though we knew we had a contraction terminating our duct and knew that it caused the u component of the turbulence to be suppressed and the v component to be enhanced, it was relatively far away and was not believed to have an influence on the flow so far upstream. In particular, the contraction diameter corresponded to $18M$ and was located a distance of $120M$ from the grid, while we were considering only the measurements in the (20–80)M range behind the grid, more than two tunnel diameters from the contraction entrance.

As usual, we suspected a problem in the measurements and invested considerable effort in reevaluating them and our techniques. We were able by the use of multiprobe coherence methods to identify the presence of large-scale coherent disturbances in the flow related to the blower and facility. By confirming that these disturbances were the same whether or not the grid was present and by showing them to be uncorrelated with the turbulence, we were able to show that their

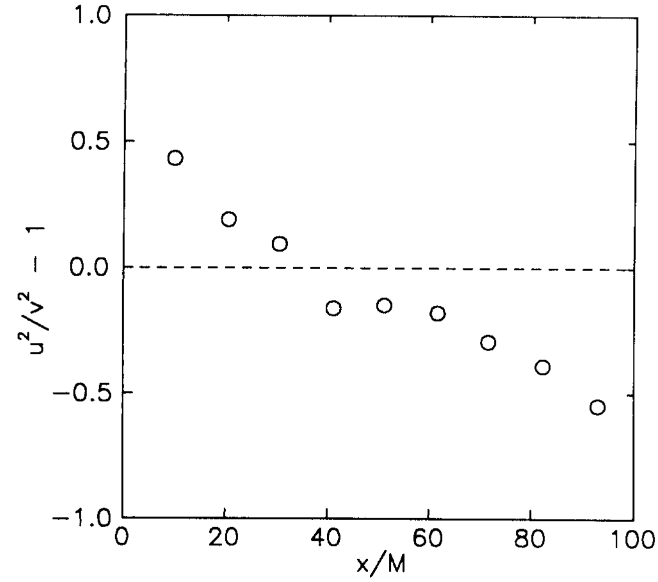

Figure 8. Variation of anisotropy with distance from grid [17].

effect on the turbulence intensities could be removed by subtracting the mean square values measured in the flow with no grid from those measured with the grid present. The effect of the corrections for the turbulence measured in the contraction was significant, and provided the resolution to our original enquiry. However, the effect on the measurements behind the grid was nearly negligible.

Only after all else failed did we examine the full governing equations for the turbulence components. For the two components of interest here these can be written as

$$U\frac{\partial}{\partial x}\left(\frac{1}{2}\overline{u^2}\right) = -\overline{u^2}\frac{\partial U}{\partial x} - \epsilon_u + (\text{third-order terms}) \quad \mathbf{(17)}$$

and

$$U\frac{\partial}{\partial x}\left(\frac{1}{2}\overline{v^2}\right) = -\overline{v^2}\frac{\partial V}{\partial r} - \epsilon_v + (\text{third-order terms}) \quad \mathbf{(18)}$$

where U and V represent the local mean velocity, ϵ_u and ϵ_v are the component dissipation rates, and the remaining terms have not been written out because they are not germane to the discussion here At the centerline of the flow, with the aid of the continuity equation for the mean flow and L'Hôpital's rule, these equations can be obtained as

$$U\frac{\partial}{\partial x}\left(\frac{1}{2}\overline{u^2}\right) = -\overline{u^2}\frac{\partial U}{\partial x} - \epsilon_u + (\text{third-order terms}) \quad \mathbf{(19)}$$

and

$$U\frac{\partial}{\partial x}\left(\frac{1}{2}\overline{v^2}\right) = +\frac{1}{2}\overline{v^2}\frac{\partial U}{\partial x} - \epsilon_v + (\text{third-order terms}) \quad \mathbf{(20)}$$

The first term on the right-hand side of each equation represents the "production" of turbulence component energy by the working of the turbulence normal stresses against the mean flow gradient. If the flow is accelerating (as in a contraction), the "production" terms act to decrease $\overline{u^2}$ while increasing $\overline{v^2}$. We had assumed, as had many before us, that these terms should be negligible in the duct, and they are—*to first order*. However, *the differences about which we were so concerned were not of first order, but of second order*! Thus even though the magnitude of the "production" terms was an order of magnitude less than the component dissipation rates, their contributions with even the very small free-stream accelerations were sufficient to alter the *apparent* decay rates of the turbulence. Once again, the flow had chosen not to ignore effects which we were perfectly willing to assume were negligible.

The consequences of the above observations on our interpretation of many of the reported experiments on grid turbulence might be quite profound. Since most experiments have been performed in facilities with contractions at the entrance and diffusers at the exit, one might expect to (and does) observe a tendency toward isotropy at the beginning and toward anisotropy at the end. These effects will be especially important to distances several tunnel diameters away from either end, a distance that encompasses the major part of most facilities. Only by directly accounting for the role of the production terms will it be possible to confirm that they are, in fact, playing no role

CONCLUDING REMARKS: EXPERIMENTERS, EXPERIMENTALISTS, AND THEORETICIANS

Lest anyone have been misled by the preceding examples, this is not primarily a paper about turbulence (although I hope the information might be useful to someone). Nor is it a paper about the misadventures and successes of the author and his coworkers. Rather, it is a paper about experiments and experimenters. The examples chosen are certainly not the only ones that could have been used, nor do I or my coworkers have a monopoly on using (and ignoring) the governing equations. They are simply the ones I know best, and they do illustrate nicely some of the more subtle challenges facing the experimenter.

To this point in the paper, I have been very careful to avoid the use of the word "experimentalist." An experimentalist performs experiments to sort theories. Sometimes he confirms them, sometimes he disproves them, and sometimes his experiments are inconclusive. When he is really very lucky, with the unique insights afforded by his experiments he might even be able to create a theory of his own. Thus the focus of the experimentalist is first and foremost on the dynamical equations. Unlike the theoretician, the experimentalist already knows the solution, for it is the flow he has realized. His objective is to find which equations and which boundary and initial conditions his solution corresponds to, and then to compare them and his results to those dealt with by the theoretician.

It is this focus on the governing equations which distinguishes the "experimentalist" from the "experimenter." Contrary to popular belief, an experimentalist must understand and be conversant with theory. While it may not be his bent to generate long expansions or invert difficult transforms, he must have an intimate understanding of the physics of the process and must not be intimidated by the mathematics. It is my belief that we terribly handicap students in their education when we excuse them from the high-level theory courses because of their experimental inclinations. In doing so, we cut them off from their ability to communicate with the theoreticians, and thereby rob them of the opportunity to place their work in its proper perspective and understand its objectives.

As the preceding sections should make clear, it is very difficult to be an experimentalist. In fact, it is probably impossible to say that one *is* an experimentalist. Each time I have felt confident that I have become one, I am confronted with a situation in which I find that I have lost my focus on the governing equations and their constraints. And as a result, I am once again humbled by the intricate physics of a phenomenon I had dismissed as trivial or well known and not worthy of proper consideration. Perhaps instead of being viewed as a vocation, the experimentalist should be viewed as more of a state of mind, a goal toward which the experimenter strives. It is my belief that our failure as experimenters to recognize and strive toward this goal has cost us dearly in terms of understanding and in terms of being useful to the engineering community.

I suggest that we experimenters and would-be experimentalists are not entirely to blame for our failures. We have not always been well served by the theoreticians among us. All too often we have recognized the presence of a new phenomenon and have been given no theories to sort. In part this comes about because the tendency of many theoreticians is to dismiss too lightly the results of experiments which suggest that their view of the world is not complete. Why, for example, in view of all the theoretical models tested against the flows discussed here was not the question of the importance of the boundary conditions brought to the forefront by those who should have understood it best? Why, in spite of the abundance of experiments that document the importance of initial conditions on turbulence, is there not a single theoretical model which does not a priori rule out such a dependence? Why, in spite of the ever-growing body of evidence that coherent structures play an important role in turbulence, has so little theoretical effort been focused on their dynamical role?

Perhaps the answers to these questions lie once again with the experimenters. We have often been more concerned with adapting the latest experimental technique, or inventing a new calibration, or expanding our data acquisition systems, than we have with making sure we knew which flow we were measuring or in clarifying our reasons for measuring it. The problem of "what we don't know" has tended to be replaced by the problem of "what we don't have."

Also, instead of applying our intellectual skills to understanding the dynamics of flows, we have tended to be satisfied with simply assigning labels to what we observed and didn't understand. For example, exactly what does it mean dynamically that there might be "bursts" or "horseshoes" or "ring-like structures" in the flow? We speak of the "vortex-pairing mechanism" and deceive ourselves by doing so, because "mechanism" implies dynamical equations. One can search much of the experimental literature hard and long without finding any equations, much less those describing a mechanism! Like the Biblical story of Adam naming the animals in the Garden of Eden, we have set out to name or label each new phenomenon. We have then confused that labeling with physical understanding to the extent that we are content to speak to each other in the common language of labels. We thereby avoid the embarrassment of admitting that even we do not understand what we are talking about. As for the theoreticians—they've long since been bored stiff with trying to make sense of our gibberish, and have gone off to more rewarding pursuits.

Certainly there exist fine examples of experimental work in which the experimenters practiced the highest forms of experimental art and were able to overturn the conventional wisdom by the quality of their effort. (My favorite recent example: the wake study of Wynanski et al [19], which documents almost beyond doubt the persistence of the effect of initial conditions.) And of course we need sophistication in our experimental techniques. And characterizing classes of flows and phenomena is often the beginning of understanding. Nevertheless, while the preceding paragraph may risk overstating the case, the fact is that we have largely been content as experimenters to settle for something less than our highest calling. We must first and foremost be "fluid mechanicists," and in doing so we will find ourselves becoming "experimentalists."

Each of us brings something different to the exciting challenges with which nature and technology present us. Only as we individually and collectively focus on the dynamical equations can we hope to achieve results consistent with our level of effort. And with that focus will also come the joy of discovering and understanding nature's never-ending variety of fluid mechanical tricks.

I would like at long last to thank Professor James F. Bell, Professor (now Emeritus) of Solid Mechanics at the Johns Hopkins University for the time spent inspiring a neophyte fluid dynamicist with the challenge of being an experimentalist. I am also grateful to my mentors, especially Professors John Lumley, Stanley Corrsin, and Blair Kinsman, for making me conscious of the need to discipline my ideas by the governing equations, and for giving me the confidence and wherewithal to do so The students and colleagues with whom I have shared the adventures described herein have already been cited, but these technical references cannot convey my gratitude for the fun we've had together.

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NOMENCLATURE

B	coefficient, Eq. (4), dimensionless
C_p	pressure coefficient for screen, dimensionless
F	buoyancy integral, Eq. (2), m^4/s^3
F_0	buoyancy integral at source, Eq. (1), m^4/s^3
g	gravitational acceleration, m/s^2
H	channel half-width, m
L	$(\partial/\partial x)^{-1}$, m
M	grid mesh size, m
M_0	source kinematic momentum, Eq. (3), m^4/s^2
P	mean pressure, Pa
P_∞	pressure at infinite radius, Pa
r	radial coordinate, m
R_{jet}	radius encompassing jet, m
T	mean temperature, K
t	fluctuating temperature, K
T_∞	ambient temperature, K
u	fluctuating streamwise velocity, m/s
U	mean streamwise velocity, m/s
U_c	centerline mean velocity, m/s
U_0	coflowing stream velocity, m/s

v fluctuating radial velocity, m/s
V mean radial velocity, m/s
V_∞ entrainment velocity at infinity, m/s
V_s entrainment velocity at screen, m/s
w fluctuating azimuthal velocity, m/s
x streamwise coordinate, m

Greek Symbols

β coefficient of thermal expansion, K^{-1}
δ $(\delta/\delta y)^{-1}$, m
$\delta_{1/2}$ radius where $U = U_c/2$, m
ΔT temperature difference $T - T_\infty$, K
ΔT_c centerline value of ΔT, K
ϵ_u dissipation of u, m^2/s^3
ϵ_v dissipation of v, m^2/s^3
ν kinematic viscosity, m^2/s
ρ density, kg/m^3

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