OPEN FORUM

A. Roshko: I'd like to tell you about some experiments on turbulent free shear layers in pressure gradients that we've been able to do. I know that Stan Birch said yesterday that effects of pressure gradient are not included in this conference, but maybe it will give us something to think about for the next conference. Actually I probably ought to be talking about the older results (with no pressure gradient) since many of you probably haven't seen them or did not hear Garry Brown's talk[1] in London last fall. Furthermore, I realize that there is considerable skepticism about them, particularly about the large vortex structures which we see (fig. 1). We were a little startled ourselves when we saw them in our first pictures but are now convinced that they are quite real and are basically two dimensional, with a scale that increases on the average with x. One thing I'd like to mention is that in the recent measurements of Spencer and Jones,[2] they find a definite spectral peak in their turbulent shear layers (in homogeneous flow). Using the average vortex spacing at any point from our pictures (actually movies) and assuming that they are convected with average speed $\frac{1}{2}(U_1 + U_2)$, we calculate a dimensionless frequency $n_0 x/U_1 = 1.7$ as compared with 2.1 from the Spencer and Jones data at $U_2/U_1 = 0.3$. (Our case is for $U_2/U_1 = 0.38$ and $\rho_2/\rho_1 = 7$.) Thus, we feel that the spectral peak measured by Spencer and Jones corresponds to the passage of the vortex structures we see in our pictures.

Another thing that has worried people about the experiments is the effect of the channel walls. These are used to set the pressure gradient and, for the case we are discussing right now, they were set for uniform pressure along the flow. Now we also wondered about the effect of the fairly close proximity of the walls, and so we made some measurements on turbulent shear layers in homogeneous flow. The results agreed fairly well with those of other investigators on homogeneous flows.

Now, for the case of a mixing layer in pressure gradient, here's the setup (fig. 2). ρ_1 and ρ_2 do not vary with x. Similarly U_1 and U_2 are constants in the case of zero pressure gradient. But if the pressure gradient is not zero then U_1 and U_2 are functions of x. Bernoulli's equation shows that if we try to maintain U_2/U_1 the

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[1]Brown, Garry; and Roshko, Anatol: The Effect of Density Difference on the Turbulent Mixing Layer. Turbulent Shear Flows, AGARD-CP-93, Jan. 1972, pp. 23-1 – 23-12.

[2]Spencer, Bruce Walton: Statistical Investigation of Turbulent Velocity and Pressure Fields in a Two-Stream Mixing Layer. Ph. D. Thesis, Univ. of Illinois, 1970.

Spencer, B. W.; and Jones, B. G.: Statistical Investigation of Pressure and Velocity Fields in the Turbulent Two-Stream Mixing Layer. AIAA Paper No. 71-613, June 1971.

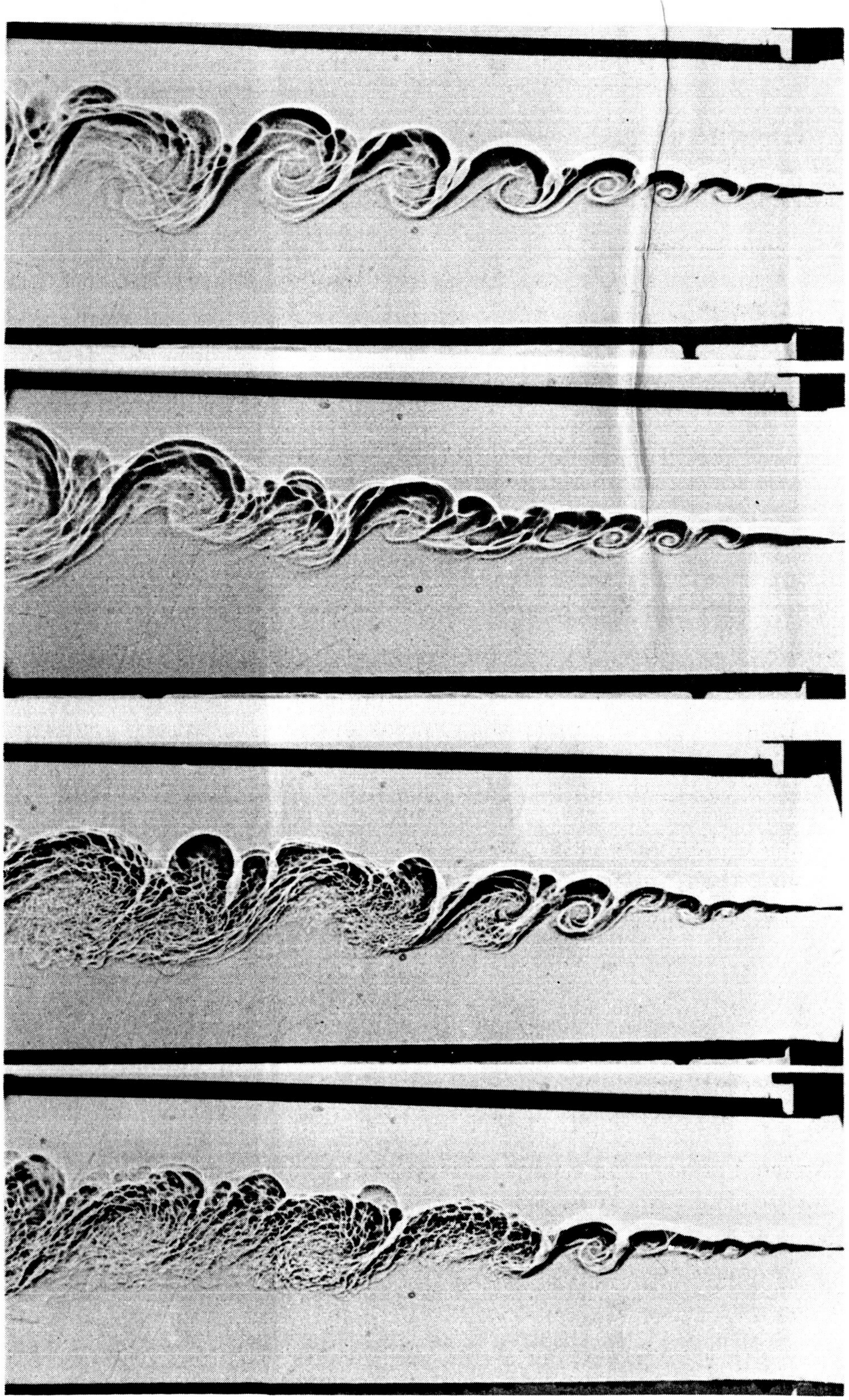
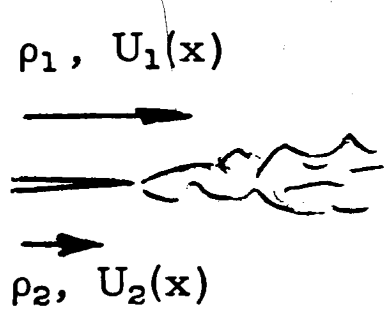


Figure 1

Effect of dU_1/dx



Similarity in y/x if

(a) $\rho_1 U_1^2 = \rho_2 U_2^2$

(b) $\frac{x}{U_1}\frac{dU_1}{dx} = \frac{x}{U_2}\frac{dU_2}{dx} \equiv \alpha = \text{const.}$

Experiment for $\alpha = -0.18$

Figure 2

same at all x, then, in general, the pressure will not develop the same way on both sides of the layer. But, for one particular case, namely $\rho_2 U_2^2 = \rho_1 U_1^2$, the pressure $p(x)$ will be the same on both sides. For that one particular case, you might hope to get an equilibrium or similarity shear layer even in a préssure gradient.

Well, if you play with this idea, you find that, if the parameter $\alpha = \frac{x}{U_1}\frac{dU_1}{dx} = \frac{x}{U_2}\frac{dU_2}{dx}$, the Falkner Skan parameter, is a constant, then you would expect to have an equilibrium shear layer. It will still spread linearly but not necessarily at the same rate as for $\alpha = 0$ where y/x is still the similarity coordinate.

To set up these flows in our apparatus we had to diverge the walls (for adverse pressure gradients) and put slots in them to allow outflow helped by some resistance added at the channel exit. One of our graduate students, M. Rebollo, did the experiments. It took some adjusting and playing around but we think we produced an equilibrium flow and I'd like to show you those results.

Figure 3 shows, for comparison, a profile of dynamic pressure for the case $\rho_2 U_2^2 = \rho_1 U_1^2$ and with $\alpha = 0$. Here similarity is shown by the fact that the points all fall on one curve when plotted against $\eta = y/(x - x_0)$. Values of x were 2 to 3 inches and x_0 was about -0.20 inch. This is a stronger test of similarity than one can get from velocity profiles or density profiles, since here we have points of maximum and minimum that all have to be the same for every value of x. Keep in mind the horizontal scale; it is a measure of the spreading angle; you see that the layer extends over a width of about 0.2 in y/x. Also noted is the location of the dividing streamline η_0.

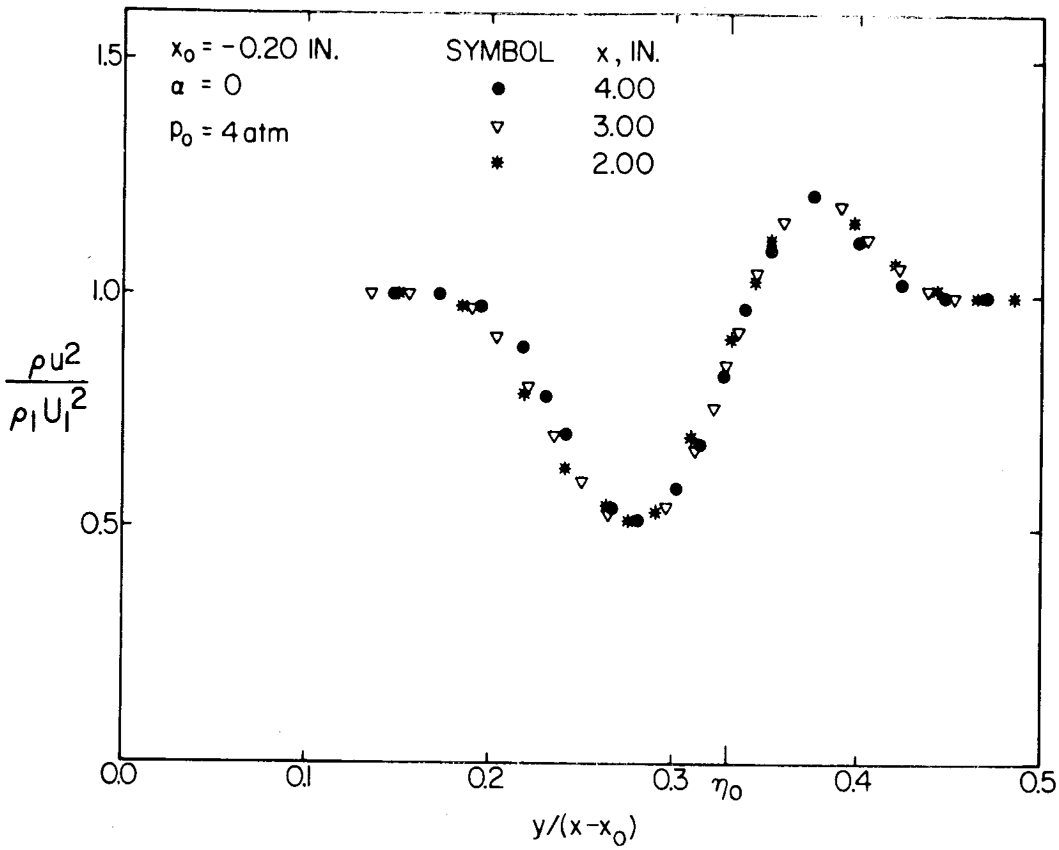


Figure 3

The next figure (fig. 4) shows the corresponding result for an adverse pressure gradient with $\alpha = -0.18$. First of all, you see that there's a tremendous effect on the spreading angle – it's just about doubled (the scale is the same as before). There's also a larger dip at the minimum. If you had a more adverse pressure gradient, you'd reach zero velocity at the minimum and would be tending to flow reversal in this part of the shear layer.

One of the better tests for existence of similarity or equilibrium is that the turbulence structure shows similarity, and we've recently begun to make measurements of this. We are able to measure the fluctuating density, or concentration, in the flow, using the probe developed by Brown and Rebollo.[3] The root mean square of the concentration fluctuation does tend to fall on one similarity curve, indicating equilibrium. Shown in figure 5 is the case for $\alpha = -0.18$ compared with that for $\alpha = 0$. Again you see the large change in the width of the layer.

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[3]Brown, G. L.; and Rebollo, M. R.: A Small, Fast-Response Probe To Measure Composition of a Binary Gas Mixture. AIAA J., vol. 10, no. 5, May 1972, pp. 649-652.

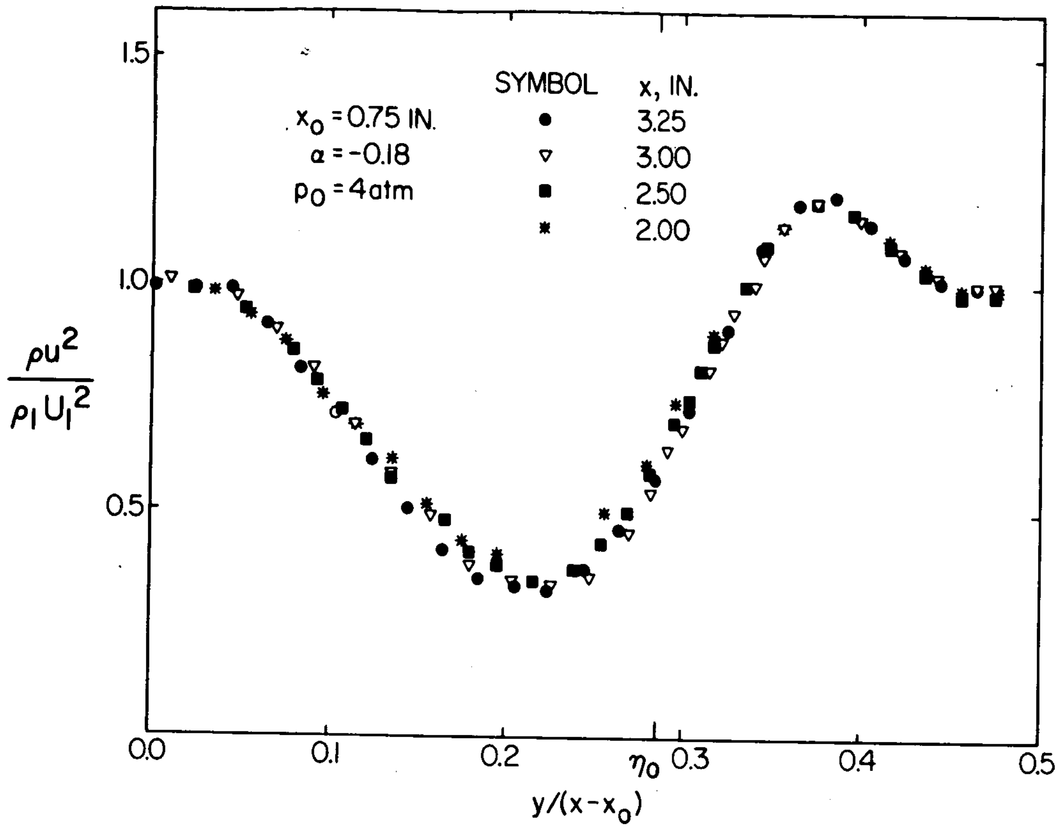

Figure 4

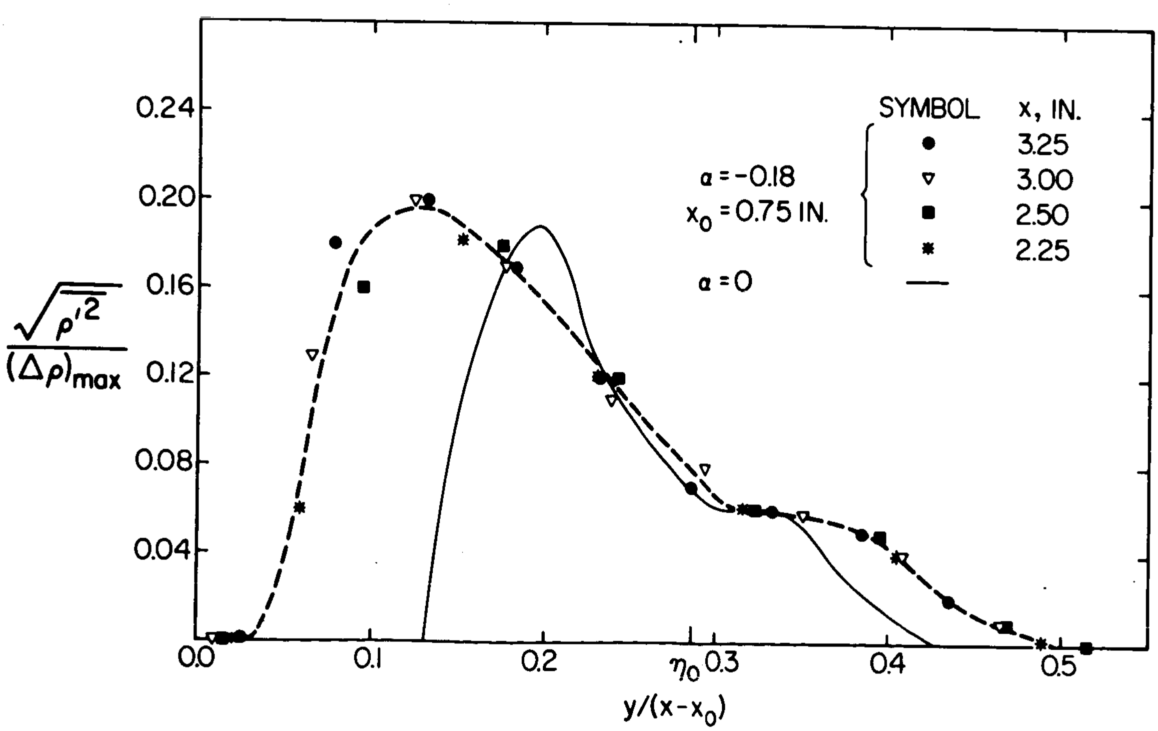

Figure 5

From the measurements of the mean density profile and the mean velocity profile, you can compute the shearing stress distribution; in figure 6 is a comparison of this for $\alpha = 0$ and $\alpha = -0.18$. There is quite a difference in the maximum shear stress.

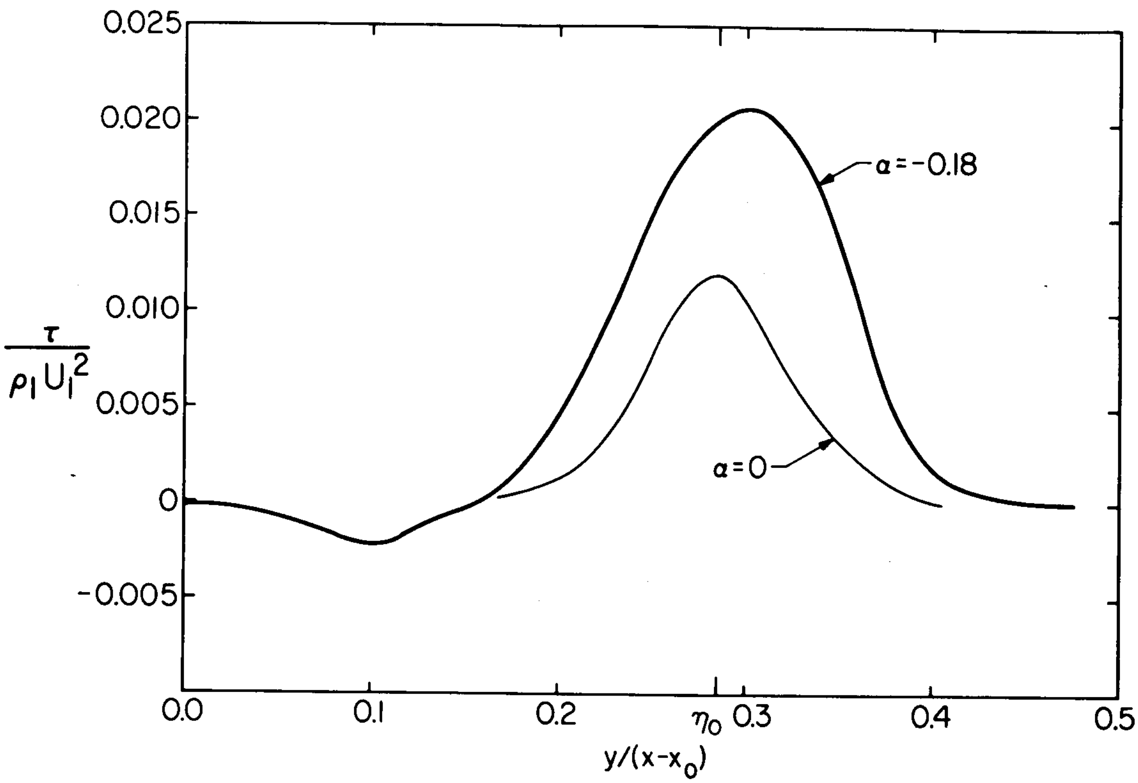

Figure 6

The next figure (fig. 7) shows the same comparison for the turbulent mass diffusion $\overline{\rho' v'}$. There is not a great difference in the maximum for the two cases.

The last figure (fig. 8 on p. 636) is a summary of some of the parameters that can be computed from the mean profiles. For example, the maximum shear stresses and mass diffusions are shown. If you use the measured and computed profiles to infer eddy viscosities and diffusivities you find the results shown here. (The asterisk indicates values on the dividing streamline.) The eddy viscosity, normalized with x, is much larger for the case $\alpha = -0.18$. Even if normalized with δ (which itself is larger in the adverse gradient), it still is about 50 percent larger than for $\alpha = 0$. The eddy diffusivity normalized with x is not much changed. But, most interesting, the turbulent Schmidt numbers here are nothing like what we've been hearing about today or at any other time that I know about. They are down at around 0.2 and 0.3 rather than about 0.8 to 1.

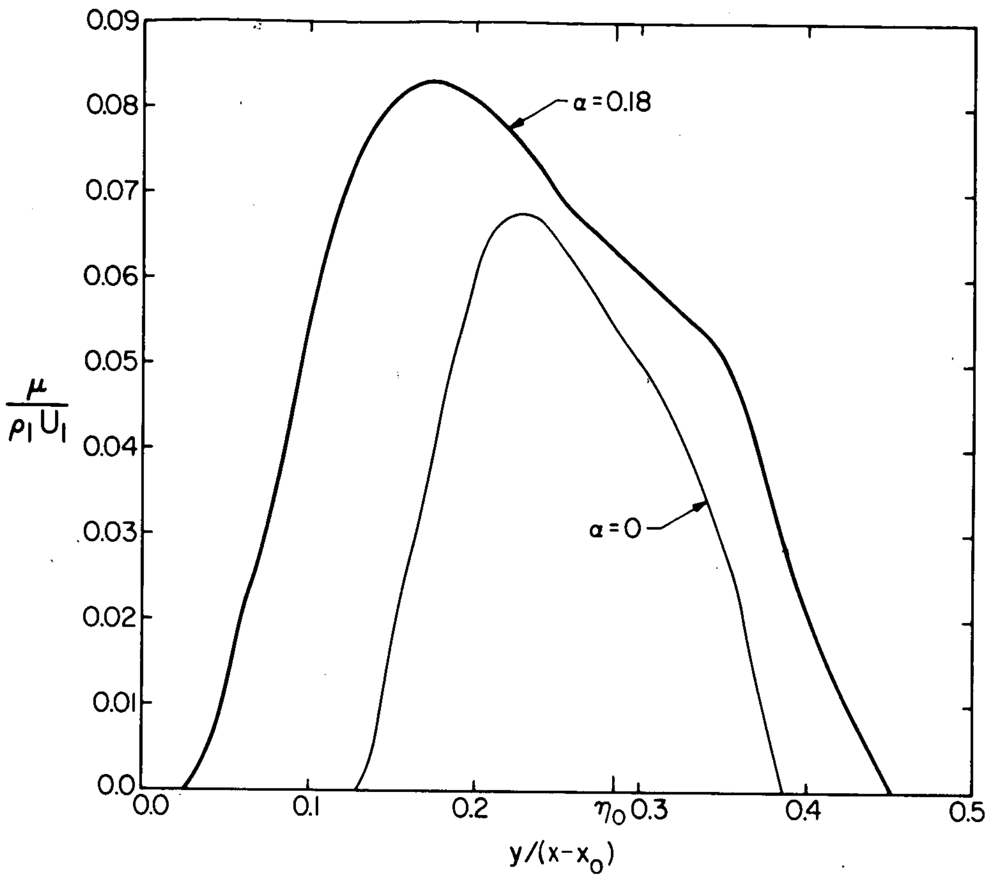

Figure 7

In closing, I'd like to say that Dr. Garry Brown was very much involved in this work until he left for Australia a year ago, Mr. Manuel Rebollo made most of the measurements you have seen today, and the research is sponsored by the Office of Naval Research.

P. A. Libby: Does that mean that the concentration profiles and the velocity profiles are not related according to the Crocco relation?

A. Roshko: Yes, I think so. I think that when you have this tremendous density difference which is continually maintained, the physics is rather different from where you have a small concentration that is passively floating around. I think all the ideas about Schmidt numbers near 1 come from cases where the contamination is rather small – that is, that there is not a large density difference or concentration difference.

I. E. Alber: I'll just ask you the question that I asked you before in private for the whole audience to comment on. Do you expect that you would get the same structure in the shear layer if you had an initial turbulent boundary layer ahead of the separation point, compared with the case which you ran where you had laminar initial boundary layer?

	$U_1/U_2 = \sqrt{7}$	$\rho_1/\rho_2 = 1/7$
$\frac{x}{U_1}\frac{dU_1}{dx} = \alpha$	0	- 0.18
u^*/U_1	.70	.59
ρ^*/ρ_1	1.78	1.70
$(\rho u^2/\rho_1 U_1^2)_{min}$	0.52	0.32
$(\rho u^2/\rho_1 U_1^2)_{max.}$	1.22	1.20
$\sqrt{\overline{\rho'^2_m}}/(\rho_2 - \rho_1)$	0.19	0.20
$\tau_m/\rho_1 U_1^2$	0.012	0.021
$\mu_m/\rho_1 U_1$	0.068	0.082
$\frac{\nu_{t*}}{(U_1 - U_2)x}$	0.0016	0.0040
$\frac{\mathcal{D}_{t*}}{(U_1 - U_2)x}$	0.010	0.012
Sc_t	0.16	0.33

Figure 8

A. Roshko: Well, I really don't know the answer, but I don't think so. Again, my feelings about this relate to some of my ideas that were kicked around here in regard to the stability – that this is really an instability phenomenon. For example, I think that instability in the supersonic layer would be rather different from the instability in the subsonic layer. I think, in fact, it accounts for the difference between the σ in the supersonic and the subsonic cases. I think that supersonic layers are stiffer in some sense than the subsonic ones.

I. E. Alber: But the instability mode would be different depending on the shape of the initial profile. Then you would have a much fuller profile in the turbulent case than the laminar case. You may expect a different response.

A. Roshko: It's not the initial profile that matters, it's the average profile at any point in the developed layer, which has a universal shape. In other words, the instability from any point in the shear layer is determined by the profile at that time. So I don't think that, if I understand you correctly, the initial profiles of the separation points should matter if there is any validity at all to these ideas about mean flow similarity.

V. W. Goldschmidt: There are three things I want to refer to. These are measurements taken related to the problems of (a) upstream effects, (b) similarity, and (c) stability. Figure 1 (from unpublished data) relates to upstream effects. It shows the widening rates of plane free jets (the inverse of σ) on the ordinate. Along the abscissa is shown the turbulence intensity at the mouth of the different jets. These values may be in slight absolute but not relative error. As you see there is an increase in widening rate with upstream turbulence intensity. Shown, just for reference, is where $\sigma = 9$ and 11 would be located.

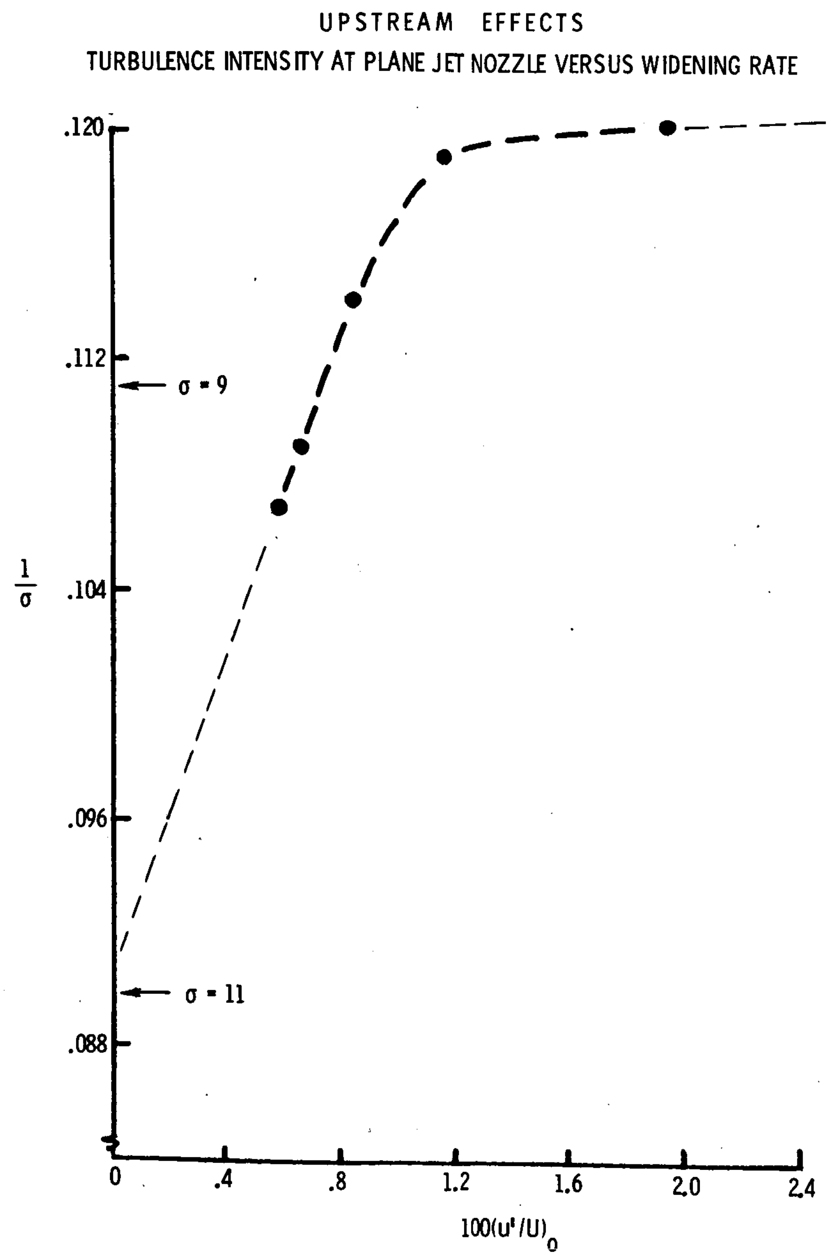

Figure 1

Figure 2 considers the macroscale downstream of a circular jet. The macroscale is on the ordinate, a dimensionless radial coordinate on the abscissa. The three curves are for three different x/d stations. Never do we get them to scale with anything. Although the velocity profiles look similar, the turbulence intensity looks similar, and the Reynolds stresses seem to reach similarity, the macroscales do not. These results were published in the Trans. ASME, Ser. D: J. Basic Eng.[4] Similar results (still unpublished) were

SIMILARITY

RADIAL DISTRIBUTION OF LONGITUDINAL MACROSCALE FOR CIRCULAR JET

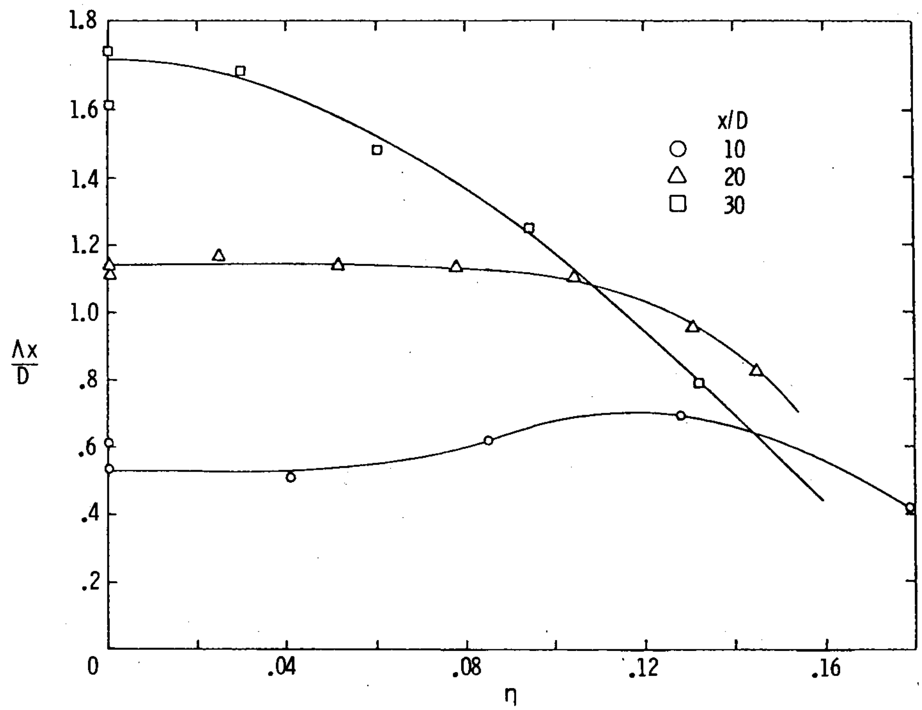

Figure 2

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[4]Goldschmidt, V. W.; and Chuang, S. C.: Energy Spectrum and Turbulent Scales in a Circular Water Jet. Trans. ASME, Ser. D: J. Basic Eng., vol. 94, no. 1, Mar. 1972, pp. 22-26.

noted for a plane jet. The problem of some kind of periodicity or stability or something coming in is alluded to in figure 3.[5] Trying to answer the question, "Do jets flap?," two-hot-wire probes were placed on opposite sides of the center of a plane jet. The time cross correlation of these two is shown in the figure. A negative correlation for no time delay on either of the signals ($\tau = 0$) would imply or suggest flapping. The figure shows that there is flapping, and what is interesting is that there is a certain periodicity to this flapping.

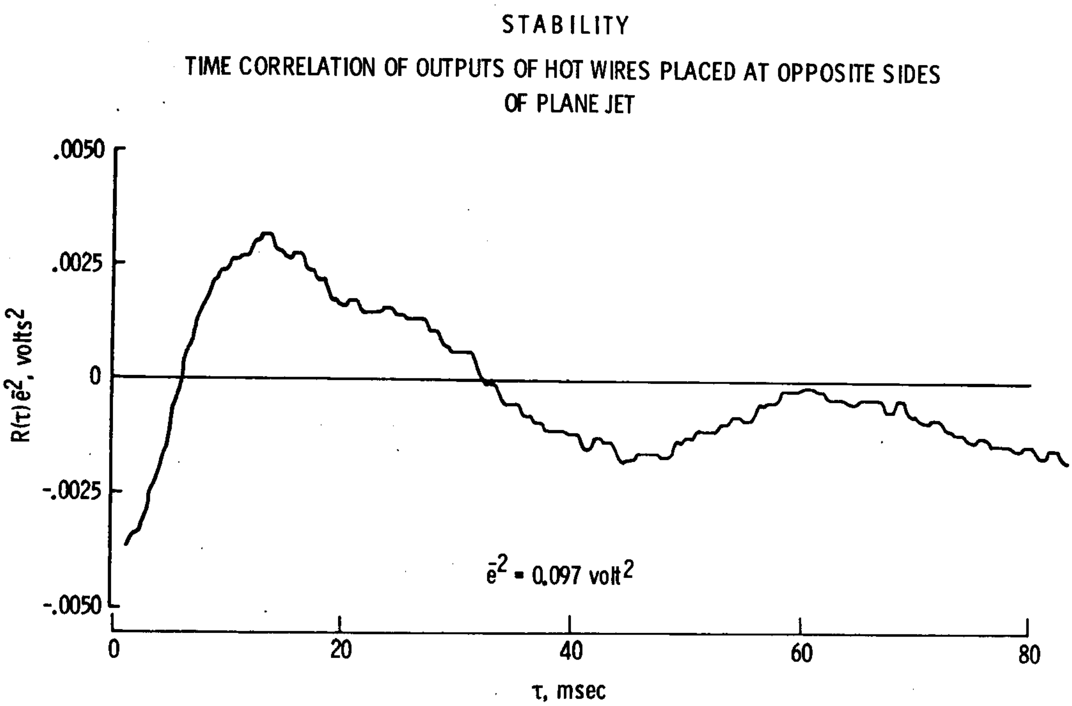

Figure 3

[5]Goldschmidt, V. W.; and Bradshaw, P.: Flapping of a Plane Jet. Phys. Fluids, vol. 16, no. 3, Mar. 1973, pp. 354-355.

V. W. Goldschmidt: I have a question for Jack Herring that we discussed after his presentation, but I think it would be worthwhile to bring it to the group as a whole. My question was whether, as you recall in the model proposed, there was a need for what you call a random force or a random wave mix that regenerates and brings energy back into the flow. The distribution of this random input was made random throughout all wave numbers. My question to him was can this input be made selective over wave numbers and, hence, try to simulate how the turbulent flow gains energy from the mean flow and how dependent would his results be on the distribution of this random input. Did I make this question correct?

J. R. Herring: Yes. It is the same question you asked yesterday. Perhaps I wasn't very clear on this point. What I meant yesterday was that you start by modeling the turbulence "force," the $(V \cdot \Delta V)$ term (the nonlinear terms in the Navier-Stokes equations), as if they were normally distributed multipoint Gaussian. In having made the assumption that the turbulence force could be so modeled, you need no information about the spectral shape because you are going to compute that in a self-consistent way after you have finished constructing the theory. Next, having made the Gaussian assumption, you immediately discover that along with such a random stirring force, in modeling turbulence, you will need an eddy viscosity because random stirring always increases the energy of systems to which it is applied. This eddy viscosity (really a generalized eddy viscosity) is determined by the spectrum of the random stirring force representing the "turbulence force" through the condition of energy conservation. In that case you have a model with no empirical constants and this is Kraichnan's direct interaction approximation.[6] Of course, the discussion here was not a derivation but rather a description of the ingredients of the theory. However, a derivation along these lines has been developed by Phythian.[7]

V. W. Goldschmidt: If I could elaborate on that, the reason for the question is that we found that when we use different distributions for this random stirring force we could generate different macroscales on the resulting flow, although the microscales remained basically unchanged.

M. V. Morkovin: Could you elaborate on your critique of this subgrid-scale closure? As I understood you, because of a mismatch of the statistical behavior on one side of the large wave numbers and the actual computed nonstatistical values on the other side of the wave-number interface, you do have propagation of errors into the larger scales. You made some very definite statements about the amount of spoiling that this error propagation would do to your original results.

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[6]Kraichnan, Robert H.: Direct-Interaction Approximation for Shear and Thermally Driven Turbulence. Phys. Fluids, vol. 7, no. 7, July 1964, pp. 1048-1062.

[7]Phythian, R.: Self-Consistent Perturbation Series for Stationary Homogeneous Turbulence. J. Phys. A: Gen. Phys., vol. 2, no. 2, Mar. 1969, pp. 181-192.

J. R. Herring: Yes, this is a very topical subject now because it limits the ultimate predictability of equations that you are using. If you make small error perturbations on the small-scale structures, these feed back and contaminate the large scales. Hence, any deterministic aspects that these large scales may originally have can be ruined. I believe that the figure quoted now is that within about three eddy circulation times a given scale will be contaminated with the error originally residing at very small scales. As to how this affects the large scales in a real problem depends upon the geometry of the flow, whether the flow has boundaries and so on. The defenders of the subgrid-scale theory argue that the method is to be used to forecast "typical" flows, and as such, the theory need not tie back to any particular initial conditions.

M. V. Morkovin: Isn't there a difference between behavior in three-dimensional turbulence and two-dimensional turbulence? Two-dimensional turbulence, as long as we are away from the small viscous scales, should have circulation preservation, vorticity preservation. And so automatically you do have a chance of cascade upstream, I mean up to the larger scale. Is this the problem that you are talking about? Wouldn't the same thing be true for three-dimensional turbulence?

J. R. Herring: Errors initially in the small scales work their way back to larger scales in both two and three dimensions. Their penetration into the large-scale regime is aided – in two dimensions – by the energy cascade to larger scales that you mentioned. However, the "error cascade" to larger scales occurs in three-dimensional turbulence, despite the fact that here the energy cascade is dominantly to smaller scales.

P. A. Libby: I think it would be very valuable for the evaluation committee if Dr. Herring could make some conjectures about the future prospects of these more deterministic methods of calculation. He has seen, in the last day or so, some of the complexities which the engineers have to deal with. I think the answer we would like to hear from him really is what he thinks in the next 10 years perhaps, when computers become even more powerful, as to whether or not all the methods that he has heard today would be simply passé, and we would all be doing, even for relatively simple calculations, these more advanced methods based on direct integration of Navier-Stokes, direct interaction, or the subscale closure and things of that sort. I think it would be very valuable for us to have his notions in that regard for the sort of flows that we have been dealing with here not boxes.

J. R. Herring: I tried, in the talk to limit myself to simple geometries to get some insight into whether the parameterizations make sense in terms of a more deductive theory. When it comes to doing problems with complicated geometries you have in mind in this conference and at large Reynolds numbers, a direct application of the statistical theories seems to me out of the question. At best, one can hope for solutions to problems with simple geometries, such as parallel plate shear flow or cylindrical pipe flow. It seems to me that the future value of such statistical methods may lie in their ability to shed light on simpler closures by way of either generalizing them or determining their constants. For example, I think that the proposals of Hanjalić and Launder's model[8] are in some sense suggested by the direct interaction kinds of equations, although the latter are certainly more complicated and laborious to solve. Possibly one should try to extract information from the direct interaction theory to get a theory at the level of Hanjalić and Launder by assuming shapes for the various spectra – energy spectra and so on. Some work has been done along this line already by Leslie.[9] That may be a good avenue along which to attack the problem and I have tried to give some indication of how such a calculation would go at the end of the printed version of my talk. To summarize, then, it would appear that the future rule at statistical turbulence theory in the type problems discussed here would be in improving and generalizing existing second-order, single-point moment closures.

As for the other two approaches, the subgrid-scale closure and the spectral method, these appear to have a better chance of being used directly on complicated problems discussed here. The subgrid-scale method has already been used for shear flows and thermal convection by Deardorff.[10] Of course, this method is a model of turbulence and hence is open to doubts on this account. However, as I pointed out in my talk, it is a method which can be progressively improved (at the expense of more computer time), and so hopefully one can avoid the disquieting doubts of other procedures, where errors are less easy to assess. This method could be applied, now, to the problems dealt with at this conference. No one has done so, principally I guess because of the additional programing needed to treat the complicated geometries you have. Of course, the method cannot, at present, deal with the boundary layer itself. It seems to me that such a calculation would be very valuable, because, it would predict so much flow structure, that it would be easy to tell whether the method is sensible or not. You cannot do this with the "global-averaged" procedures described here by, say, Launder and Donaldson, because of the averaging over the turbulent structure.

With reguard to the spectral technique, one must be more guarded. This method deals with all scales, so its principal contribution is at low Reynolds numbers, where there aren't many. It may serve (and is now serving) as a useful tool in assessing other

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[8]Hanjalić, K.; and Launder, B. E.: A Reynolds Stress Model of Turbulence and Its Application to Thin Shear Flows. J. Fluid Mech., vol. 52, pt. 4, Apr. 25, 1972, pp. 609-638.

[9]Leslie, D. C.: Simplification of the Direct Interaction Equations for Turbulent Shear Flow. J. Phys. A: Gen. Phys., vol. 3, no. 3, May 1970, pp. L16-L18.

[10]Deardorff, J. W.: A Three-Dimensional Numerical Investigation of the Idealized Planetary Boundary Layer. Geophys. Fluid Dyn., vol. I, no. 4, Nov. 1970, pp. 377-410.

methods, such as the subgrid-scale method. For example, for some of the two-dimensional calculations carried out at The National Center for Atmospheric Research (NCAR) by Orszag, the behavior of the large scales seem to be relatively insensitive to the detailed phases of the small scales. All that's apparently required is for the magnitude of the small scales to be approximately correct. Such calculations tend, in this case, to bolster one's confidence in the subgrid-scale method.

S. J. Kline: Something bothers me about that, and I'm not quite sure of all of what you are saying. Let me try to explain myself. If you try to do what Professor Launder and Professor Spalding were doing where they were dealing with the ratios of production and dissipation, then this implies that you are saying something about the production. I am not at all persuaded from the physical evidence that the nature of production is the same as the nature of the cascade process in the statistics of decay, which is the box problem. If that is true, then there is a fundamental gap between the box problem and the kinds of things that we are doing here. Can you comment on that?

J. R. Herring: I'm not sure that I understand the question.

S. J. Kline: What I am saying, is that those models which seem to fit, for example, the two-point space-time correlations for shear flows (the only ones that we know that work at all, and also the visual data, the data that Gupta has taken with Laufer, and so forth) suggest that what one sees during the peak periods of production, which is intermittent, is of a qualitatively different nature from the processes that lead from there to decay. Now if that is true, then it seems to me that there is a fundamental gap between doing problems of statistics in which you are dealing with the decay process and cases where there is strong shear and high turbulence production.

J. R. Herring: Well, maybe I can answer part of that. Let me say as a general comment that the theories are capable of treating any kind of flow with mean fields (production) present or not. In my talk, I stressed the decay problem because it is simplest and because we have more numerical experience with it than with problems having production terms. I would agree that the physics of turbulent production is fundamentally different from that of decay; but I think the statistical theory may be flexible enough to handle both production and "decay." The issue you raised about the intermittency is, however, a problem with statistical moment theories, and it is not clear to me that they can handle correctly highly intermittent flows.

J. Laufer: I think that is a very fundamental question that Professor Kline brought up. And maybe it might be worthwhile to argue about this a little further. The sort of modeling that we have seen today and yesterday really tried to model some average equations, primarily Reynolds equations and some higher order equations of the turbulent quantities. We have not yet tried to – certainly not in this conference – make some physical model of turbulence itself. There is a great deal of skepticism about the formulation of the turbulence problem, a lá Reynolds, in several people's mind working with turbulence. I think many of us are convinced that a great deal of information, especially phase information, is lacking in that. And the obvious consequence of that is that we cannot close the problem. So when we are talking about the production term in the Reynolds equation, to me it is really just a nomenclature – I have no physical picture of it. Certainly not until the first suggestions as to how this production might take place (made in Professor Kline's group) did I begin to get a physical picture. That was really the first notion that suggested some actual physical process that this is the way the flow might break down and produce turbulence. So the question is, how far can we go by trying to do the modeling according to these equations. Might it not be better to go back more into the physical work and the modeling? And this actually has been done – any of you who have tried to get into the details of Townsend's book[11] – he actually had a very physical picture in mind when he tried to come up with a value for turbulent Reynolds number. That I consider to be a sort of modeling – physical modeling. Clearly this is a much more difficult task, and I certainly don't have any obvious suggestions of how we can do that. But I think that it would be worthwhile for people who work in this area to think in terms of other possible modeling processes besides the ones that we have heard in the past 2 days.

A. Roshko: I'd like to pick up the idea that Paul Libby started and has been continued here – and that is, the question that has also come to my mind occasionally, one of these days will computers be able to calculate the flow directly without putting in any physical models let alone Reynolds equations. I think that there is some possibilities that this might occur. I'm thinking of some examples that I know of where you try to model a vortex shedding, for example, behind a circular cylinder by just letting vortices peel off from the cylinder into the wake and do a time calculation. This reproduces these flows at least qualitatively fairly well. Just this year in the Physics of Fluids, there was a short paper by a couple of Russians, Kadomtsev and Kostomarov,[12] in which they try to model the mixing layers that we have been talking about here so much. They simply feed vortices off the splitter plate and let it go. This is really just solving the Euler equation on a computer, leaving out the viscous terms. We do think that these free turbulent flows are viscous independent, and it is really quite remarkable that you get mean velocity profiles which look quite reasonable. My objection is that this is a two-dimensional calculation; these are all line vortices. However, for some flows like these mixing layers and vortex shedding down a cylinder, I think the large structure is to some extent two dimensional. I think there might be some progress in that direction. There has been really little work of this kind done.

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[11]Townsend, A. A.: The Structure of Turbulent Shear Flow. Cambridge Univ. Press, 1956.

[12]Kadomtsev, B. B.; and Kostomarov, D. P.: Turbulent Layer in an Ideal Two-Dimensional Fluid. Phys. Fluids, vol. 15, no. 1, Jan. 1972, pp. 1-3.

D. B. Spalding: On this particular point of whether it will be practical in the near future to compute turbulent flows by a time-dependent solution of the Navier-Stokes equations, there is a simple computation that one can do about the number of grid points that one will need. Undoubtedly, if we are to proceed in that way, we must have grid points sufficiently close together so that we can accurately describe the behavior in the smallest eddies where energy is being dissipated. Now these are very small indeed. And so we can compute just how many grid points we will need, and thus see by how many orders of magnitude our current computers are away from being able to compute a turbulent pipe flow. I think it is quite a long way.

J. R. Herring: If I can make one comment along those lines. Some of those calculations done at NCAR on decaying turbulence suggest that it is not really so – that if you wish to calculate the large-scale structures accurately, you don't have to do a clean job of calculating the tiniest dissipation scales present. The examples I'm referring to here are two-dimensional calculations; it may be that in three dimensions there is a difference.

M. V. Morkovin: I was hoping that Dr. Spalding would share with us some of the early experience of Imperial College with three-dimensional flows. As I understand it you are computing things that have streamwise vorticity. How much of a complication, how much for instance, do you have to increase the number of grids to get something decent. Those are pioneering, rather smeared numerical experiments as yet, but they must be giving you some feeling of what the future potentials are. Isn't that right?

D. B. Spalding: I think that my answer must follow the lines of what I was saying just now. We are doing three-dimensional time-dependent computations, and we are doing them for turbulent flows. But we are doing them only by the use of turbulent models. They are still the quite simple two-equation turbulent models. We do not have higher level ones for three-dimensional flows. But we are struggling with computer storage at that level. And I think that we should have to have computers of many orders of magnitude greater capacity before we could approach the task of solving for turbulent flows by any other means than by way of turbulent models. But once that is said, there are no special problems about three-dimensional flows. If you've got a computer program that solves the three-dimensional equations and the continuity equation, and then an equation that will solve for any other scalar, like the energy or the dissipation rate or like concentration, then you just go ahead and solve. We have seen that there is still much comparison to be done between predictions and measurements for two-dimensional free turbulent boundary layers. For two-dimensional recirculating flows, there is much work to be done, and after that there is a comparison to be done with three-dimensional turbulent flows. So there is an immense amount of utilization of these techniques. And we are just at the very beginning of this.

S. J. Kline: I agree with Professor Spalding. You will recall that a number of people have made this calculation that he suggests. Howard Emmons has made it, Bill Reynolds has made it, and I think a number of other people have made it. And it is not just a small jump from present computers. I think you will recall that if you want to do a complete solution to the smallest scales as he suggested, then there is a graph in the 1968 Conference proceedings, I think Emmons[13] did that particular one, which shows that you have to run about 30 years on a computer that is slightly larger than anything that exists to get out one point for a fairly simple flow. That is pretty hard to imagine. So unless we get really a very big jump in computer power, one has to do something other than simply putting the Navier-Stokes equations into the computer and letting it run. Now the next question that immediately comes beyond that is what approximations do you make. Jack Herring suggested one kind of approximation, which may in fact be a very good one – that is, you model the small scales and try to do the larger scales. But that still is a modeling. And it still involves all these problems. I think that that really is the situation. I think it is naive to say that you are going to put all this in the computer and it is simply going to run. I wanted to revert also to the exchange between Jack Herring and me earlier and add one other remark – it may be helpful, I don't know. And that is, if you look at Lahey's model[14] of the two-point space-time correlation which does seem to fit all of them remarkably well, it is a two-part model which is the sum of the Markov noise and a traveling wave which has a stochastic jitter on the wave number and a stochastic phase coefficient. The situation is analogous in some simple sense to a simple harmonic oscillator which is underdamped or overdamped. If you work out the spectra using that model, it is essentially overdamped. But the constants are such that, if you just do long-term averaging, you don't see any peak at all. If you do the short-term stuff, there is a marked peak for certain kinds of events. If you take a long-term average, which is equivalent mathematically to Reynolds stress averaging, then this simply washes out. Experimentally, this would be the same as trying to find the critical frequency or resonant frequency of a simple harmonic oscillator by perturbing an overdamped system – you don't get anything. This is quite disturbing in terms of understanding the basic physics, as John Laufer said, from measurements based on the normal kinds of long-term averages.

D. R. Chapman: I would like to say a few words on behalf of the large computers as one who looks at their possibilities rather optimistically. The trends over the last decade or 15 years are such that computer speed is increasing by a factor of 10 approximately every

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[13]See Comments by H. W. Emmons on page 651 for his present views.

[14]Lahey, Richard Thomas, Jr.; and Kline, Stephen J.: Stochastic Wave Model Interpretation of Correlation Functions for Turbulent Shear Flows. Rep. MD-26, Stanford Univ., Mar. 1971.

3 years, and the cost to do a given number of calculations is going down by a factor of 10 about every 5 years. So if you make a computation of how long it would take you to compute a hundred million grid points, which I did, by the present numerical methods of solving the unsteady Navier-Stokes equations that Bob McCormick uses at Ames and extrapolate these past trends as they have been going on out in the future, you come to a situation, where I estimate about 1982 or 1985, that the computers will be able to do such calculations in a practical amount of time and at a practical cost. Now a hundred million grid points is quite a few. I would like to make a comment about Howard Emmons' calculation also. I think that he was unduly pessimistic. He picked a grid size and assumed that you had to maintain that grid size throughout the flow. He picked a size that was probably necessary to maintain only in the sublayer. Because of that, he computed that it would take very much longer to do on the computer the unsteady Navier-Stokes equation for pipe flow of say a Reynolds number of 10^7 than it would for 10^5. His grid scale was roughly 100 different and in three dimensions, that means a factor of a million. If you only have to use the smaller grid scales in the sublayer the amount of computation time to do a Reynolds number of 10^7 is the same as to do 10^5. In my judgment, it is going to be more nearly independent of Reynolds number. After all, the velocity profiles in a pipe when plotted in terms of the right variables (shear stress, etc.) are independent of Reynolds number, surface roughness, and so forth. So I think that Emmons' paper has been quite misleading to many people about the ultimate prospects of what the large computers will do.

S. C. Lee: I just want to make the same kind of comment on this large computer. With our experiments recently we have calculated a flow around a sphere using NCAR's CDC 7600. We start with rotational symmetry, solving the entire Navier-Stokes equation. With a Reynolds number equal to 20, it only takes 5 minutes to get the results. By the time the Reynolds number reached 130, the flow began to oscillate. The time for the CDC 7600 to calculate to a Reynolds number of 300 is more than 10 hours. So we decided that that was enough, and we stopped. If we are going to calculate as far as we are talking about, in the neighborhood of Reynolds numbers of 100 000, I really don't know how long it would take.

T. Morel: I think that there is some limitation to this. We can't expect to increase it indefinitely. We are limited by the physical side of the computers, since the speed of light is just the speed of light.

D. R. Chapman: This point has been raised a number of times before, but the computer people that I have talked to are not concerned about this limitation for quite a while yet. The size of the computers keeps going down, and the idea of putting the computers in parallel gets around this to a large degree. Whether or not the trends of the last 15 or 20 years or so keep on going, we really won't know until another 10 years have gone by.

But the trends on log paper are not linear, they are accelerating. In other words, 10 years ago, the average time it would take a computer to increase its speed by a factor of 10 was longer than 3 years – it was 4 years or so. At least I haven't heard anyone in the computer business identify a foreseeable limit on the trends.

D. B. Spalding: There is another subject that I would like to raise. I would like to refer to a remark that Dr. Corrsin made yesterday in the Discussion for Donaldson's paper (paper no. 7), which I think is a very important one. He cast some doubts on the gradient-diffusion type of approximations for transport. I think that it is very important and that we ought to think about it. It came to my mind when he spoke about the book by Bosworth[15] in which there is a connection made between this problem and that of radiated transfer. In radiated transfer, one of the important parameters is the ratio of the mean free paths of the radiation to the dimensions of the apparatus. In turbulence transport we have the problem that the mean free path of the turbulence eddies is not small compared with the dimensions of the apparatus; the mixing length is one-tenth of the width. It is for this reason that we are in an area where the gradient type of approximation isn't quite good enough. That connection with radiation theory has a useful aspect to it. There are, coming from the astrophysicists, quite simple theories for radiative transport which do better than the conduction – the gradient type of approximation. I want to say that it is possible, it has been done, to work out the corresponding transport equations in turbulence also. One can write a correlation, for example, the temperature-prime—velocity-prime correlation, for the transport of heat. Write that in terms of a random velocity and a temperature of the outward moving and a temperature of the inward moving fluid. Following the exact pattern which is used in radiation theory, we find ourselves with an equation saying that the flux is proportional not to the gradient of the average temperature but to the gradient of the sum of the temperatures of the outward going and the inward coming balls of fluid. And then only in the limit of small mean free path, or whatever the appropriate parameter is in turbulence, do we reduce to the gradient type of approximation. I think I have said enough. I think that remark of Professor Corrsin is important and that we can follow up its implications by turning to radiation theory to get a practical improvement by that means.

M. V. Morkovin: The diffusion-gradient problem and the diffusion terms in general, those that involve pressure-velocity correlations, are indeed one of those things that may be used as a constraint on the mathematics which may not correspond to the physics. One of the things I would like to raise is the general trend that one sees in the observations, more detailed observations of turbulence, disclosing increasingly more some aspects of instability of the field. I was wondering to what extent the computable budgets of energy

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[15]Bosworth, Richard Charles Leslie: Heat Transfer Phenomena – The Flow of Heat in Physical Systems. John Wiley & Sons, Inc., [1952].

which do have exact solutions for the pressure-velocity correlations in linearized stabilities can guide us in trying to reformulate some of the questions on these terms. There are really two questions. Is it true that we really see more and more instability behavior in turbulence, and, then, whether one can learn something from stability theories for it?

C. duP. Donaldson: I agree with Mark. I think that any way you can use an exact known solution and look at the physics of it to see if you can get a handle on how you can model something better is precisely the way to go. I don't think that we have made enough use of exact solutions in making these models initially.

J. Laufer: I think that we get back to the comment I made earlier on physical modeling. In response to Professor Morkovin's question, I think that we can say yes we see now, by making more visual observations, more and more instances where some type of instability process does take place, even in regions where we think that the flow is completely turbulent. One good example is the type of instability that the Stanford group discovered and we studied further in the sublayer of a turbulent boundary layer. We are now taking almost instantaneous measurements of the profiles, velocity profiles, in the sublayer. When we compare those profiles with the well-known figure in Schlichting's book, showing the eigenvalue distribution of a Tollmien-Schlichting wave in a laminar boundary layer, the resemblance is quite striking. This is one example of getting back to the geometry that we are concerned with at this meeting in the case of the free shear layer, the mixing layer. You have seen in Professor Roshko's pictures and again here that we are dealing with a completely well-developed turbulent flow with very definite large-scale motion in that flow that seem to indicate some type of instability origin. In fact, we are looking at the same type of problem in a water flow, where we can very nicely see how these large-scale motions generate from some interface instability originating right at the $x = 0$ station.

Written Comments

P. A. Libby and C. duP. Donaldson: We wish to call attention to the need for consideration of the proper specification of initial and boundary data when the newer methods of closure of the system of equations describing turbulent shear flows are employed. We do so because we are so familiar with the initial and boundary data appropriate for the well-known eddy viscosity and/or mixing length formulations for such flows that we may casually carry over these ideas to the newer methods, some of which lead to different orders in the normal derivative than are customary and to other differences in the mathematical nature of the describing equations.

Caution seems to be especially called for in the case of the free mixing flows under consideration at this conference. This is because in these flows we usually make the assumption that the shear stress due to molecular transport is negligible in the streamwise momentum equation and, thus, that we can drop the $\bar{u}_{yy}$ term in that equation; but in some formulations this leads to a loss in our ability to specify two boundary conditions on $\bar{u}$! Contrast this with boundary-layer flows in which we must retain molecular effects near the solid boundary because of the laminar sublayer.

It should also be recognized that in some of the new, higher order closure schemes for turbulent shear flows the mean shear stress $\bar{\tau}$ is an explicit, dependent variable, which usually appears differentiated once with respect to the normal coordinate. However, again in free-mixing flows we wish to specify $\bar{\tau} = 0$ at $y \to \pm\infty$!

Since we are dealing with parabolic equations and their solutions in either a quarter- or half-plane, we recognize that there is an interrelation between initial data (at $x = 0$ or $x = x_0$) and the boundary conditions at $y \to \infty$, or $y \to \pm\infty$. It may well be that the initial data plus some benign conditions of "boundedness" are sufficient to get us around apparent difficulties, such as those indicated previously, of not having the ability to specify all the boundary conditions we desire. However, mathematicians can usually help us on these questions only for highly idealized models of our problems.

Finally, we note that in one class of free-mixing flows, i.e., two-dimensional mixing, we have always had the problem of determining the orientation of the "zero" streamline in space. The proper treatment of the boundary conditions in this case has been provided some years ago by Ting.[16] As a result we now recognize that the theoretician may legitimately formulate his boundary conditions so as to make his analytic or numerical problem "well posed," but that when he compares his predictions with experiment or when an experimentalist employs that theory, the predictions must be properly reoriented in space so as to be consistent with the environment of the experiment, i.e., so that the zero streamlines are consistent.

These remarks should be sufficient to call attention to the need for careful consideration of the appropriate and proper specification of initial and boundary data for a set of describing equations, especially a set based on the new, higher order methods. The ease with which such equations can be "put on the computer" should not be allowed to disguise our ignorance of what constitutes a "well-posed" problem. We may well learn some important aspects of the phenomenology of turbulent space flows by considering the formalism before hasty computation.

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[16] Ting, Lu: On the Mixing of Two Parallel Streams. J. Math. & Phys., vol. XXXVIII, no. 3, Oct. 1959, pp. 153-165.

Comments on the Place of the Computer in Turbulence Research and Application*

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The current lack of understanding of the basic nature of turbulence prevents the prediction of just what approach can ultimately be successful. It is not now known whether turbulent flow is an initial instability followed by a complex, nonlinear, transfer of macroscopic kinetic energy through random interaction down through a chain of ever smaller scale motions to end as molecular chaos or whether each scale of motion develops ever more intense until it in turn becomes unstable and thus passes its energy down with various wave number and phase control or whether in these processes significant information can be fed up from the microscopic "chaos" (turbulent, molecular, or both) to control some significant aspects of the macroscopic motions.

The most pessimistic view of this situation would require a quantum statistical mechanical kinetic theory. Essentially no questions of current interest require this level of description. Furthermore it will be a long time, if ever, that 10^{25} particles could be followed in detail. Finally for turbulence research now of interest, there are many reasons to reject this view.

The most important simplification is the replacement of the effect of the molecular chaos by a laminar viscosity based upon the observation that the smallest "significant" eddy is large compared with intermolecular dimensions and the supposition that no significant control of the turbulent motions can be exerted by the details of the microscopic molecular processes. A test of this supposition is now being made in the Doctoral Thesis study by Carol Russo who is measuring the details of grid turbulence in methyl alcohol. The expected agreement with air grid turbulence will confirm that the total effect of the molecular chaos is subsumed under the Reynolds number (through ν) and that no other dimensionless variables are required and, therefore, the Navier-Stokes equations are adequate for the description of turbulent flow.

The present conference is devoted to a discussion of the varying ideas, methods, and results available to describe the essentially four dimensional $(\vec{r},t)$ turbulent flows. A wide ranging variety of ideas is being tried as is necessary because of the failure of the obvious replacement of the turbulent motions by a constant "turbulent viscosity" inspired by the molecular chaos replacement.

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*In view of the current interest in the role of the computer in turbulence studies, the editors invited Professor Howard Emmons to submit his present views on the subject.

The direct numerical solution of the Navier-Stokes equations must be carried out on such a scale that the smallest eddies with significant macroscopic effect are properly described. A very fine scale is clearly needed at high Reynolds numbers in the turbulent energy creation regions near walls and internal shear layers. I believe they will also be needed in the high dissipation regions if a complete description of the turbulent flow is desired. Thus, while computation net grading may save a few orders of magnitude of points, I do not believe that direct solution of the Navier-Stokes equations will be possible for general turbulent flows for a very long time (if ever).

Again the growth in computer speed, a factor of 10 every 3 years, will no doubt continue for awhile. However there are only about four orders of magnitude available before a speed-of-light limitation and a like increase by further size reduction. Before these limits are reached, thermal fluctuations will have to be reduced by cryogenic computer elements. Finally, parallel computation can further increase computer speed by a couple of orders of magnitude. Thus I can expect something like 10^{14} computation to be over the horizon (present machines are about 10^{7}) but not 10^{20}.

Eventually a few of the simpler, lower Reynolds numbers, turbulent flows will be directly computable and these will prove invaluable for checking out the various simplifying assumptions. However, complex turbulent flows, like flow around and in the wake of a building – or even a sphere – are not in my view likely to be directly computable for a very long time – if ever.

On the other hand, I fully expect work of the kind reported at this conference to lead to approximate methods of calculation of general turbulent flows. The successful method will incorporate both a phenomenologically and mathematically justifiable procedure which provides for the computation of the growth and decay of the appropriate turbulent transport of momentum, energy, and specie. There will eventually be a hierarchy of such methods which permit the computation of any turbulent flow with any desired precision, the decision being a compromise of need and pocketbook.