EXPERIMENTS IN FREE SHEAR FLOWS –

STATUS AND NEEDS FOR THE FUTURE

By Committee To Recommend Critical Experiments:
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CLASSES OF PROBLEMS

Experiments on free turbulent shear flows have two primary objectives:

(1) Accumulation of experience with simple classical flows as a basis for evaluating constants in closure models of predictive theories

(2) Applied work on flows of technological significance

Unfortunately, requirements for objectives 1 and 2 tend not to coincide in the case of free shear flows. This condition can be seen by considering the typical case of a low-speed axisymmetric jet of air flowing into still air. The flow field is usually considered to contain three zones: (a) the near field, (b) the transition (or intermediate) field, and (c) the far field.

In the near field, a gaggle of initial conditions arises from the presence of flow-control devices and upstream solid surfaces. In real flows, as contrasted to ideal models, these initial conditions often involve one or more sets of back-to-back boundary layers trailing from solid surfaces. In the transition region, the potential core has disappeared, but the mean velocity has not yet reached a state of self-similarity. In the far field, by definition, self-similarity is characteristic of the mean-velocity field and at least the second-order correlations of the velocity perturbations.

Simple scaling laws have been established only for the far field, and the properties of this zone tend to be the best established and the most used for construction of predictive theories. The vast bulk of applications, on the other hand, depend on flow characteristics in the near field. Typical cases include jet ejectors, wake signatures, base-pressure control, combustors, flow over steps and cut-outs, jet noise, jet interactions, etc.

Since the variety of applications of technological significance is so great, and since increased fundamental understanding will aid in all of them, this report is concerned primarily with classical flows to augment understanding and for model building.

THE ROLE OF CLASSICAL EXPERIMENTS

Test data for five well-known types of classical, free turbulent shear flow are included in this conference:

(1) Mixing layer (half jet)

(2) Round jet

(3) Plane jet

(4) Round wake

(5) Plane wake

Other potentially classical flows of lesser importance include

Radial jet

Zero-momentum wake

Round plume*

Plane plume*

Each of these classical flows, by definition, has the property of self-similarity. A key assumption, which is noncontroversial in the limit of large Reynolds number, is that effects of molecular transport can be neglected. What remains then is usually a simple problem in dimensional analysis with very little physical content. Given a rather vague notion of what constitutes a boundary-layer approximation for turbulent flow, an elementary argument leads in each case to simple and well-known similarity laws of a power-law type.

An instructive conclusion from these analyses, at least for purposes of planning experiments, is that as the flow proceeds downstream, the Reynolds number based on local length scale and mean-velocity scale can increase (mixing layer, plane jet), remain constant (round jet, radial jet, plane wake), or decrease (round wake).

A second conclusion is that in the absence of a pressure gradient, the plane mixing layer, in particular, has self-similar properties (corresponding to linear growth) regardless of velocity ratio and presumably also regardless of density ratio, Mach number, or whatever. This plane flow therefore provides an opportunity to study a variety of special effects, singly or in combination, under relatively clean conditions. In view of present confusion about the effect of density ratio and to a lesser degree about the effect of initial conditions, much more might well be done with this particular flow configuration. Substitution of an axisymmetric geometry for the plane geometry is a little hazardous, as long

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*Plume here refers to a free convective flow and not to a jet engine or rocket flow.

as effects of lateral curvature are no better understood than they are at present. However, studies of the axisymmetric configuration should continue because of the technical importance, particularly of the near fields in this geometry. Flow in the far field of a jet (plane or axisymmetric) is especially unsuitable as a model for investigating effects of density variations on turbulent mixing, because most of the jet fluid (eventually, all of it) is characteristic of the ambient fluid rather than the fluid used as the original momentum source. The flow is therefore a relaxation or transition problem and is best approached from this point of view.

The committee believes that other classical or near-classical experiments, if possible with known similarity properties, need to be sought and experimentally documented. Examples include (1) curved plane jets, such as are encountered in jet flaps and thrust augmenters; (2) cross- or counter-flowing streams; and (3) vortex flows, flows with large coriolis forces, or other flows which contain regions having large mean rate of strain but small turbulence production. No recommendation is being made of a single class of experiments, but rather careful consideration of what experiments are worth extensive documentation to aid model building for turbulent flows.

The description in the section "Classes of Problems" of the near, intermediate, and far fields also provides the basis for a classification of levels of difficulty, and hence levels of confidence, regarding the agreement between predictive output of a given theory and available data. At the lowest level, any theory which does not reproduce the well-established limiting similarity forms for shear and spreading rate as x approaches ∞ should be rejected, unless overriding practical considerations force its use.

At a second level, a predictive theory should provide some information about the transition region where the conditions are essentially now initial conditions for calculation of the far field.

At a third (and possibly fourth) level, predictive theories might be able to cope with the evolution of the near field and to specify the total distance required for evolution of the downstream regions.

In each case, as in boundary-layer flows, predictions for integral quantities, local mean-field quantities, and correlations of fluctuating quantities form a second hierarchy of successively more difficult checks on the power and accuracy of predictions.

Most technological applications entail added complications, such as curved layers, recirculating flows, chemical reactions, species diffusion, pressure gradients, and gross unsteadiness. Although such experiments are essential, it is difficult, if not impossible, to obtain reliable experimental data for purposes of model building from these more complex flows. It may be possible to use them to check the output of theoretical models, but it must be recognized that in most cases, substantial differences in behavior may occur between the more complex cases and the simpler classical cases.

CRITICAL EXPERIMENTS

Five classes of experiments appear to the committee to be particularly significant for the near future.

(1) Additional experiments clarifying the effect of density variation owing to use of different gases, with and without the additional effect of density variation owing to high Mach number or other effects. Whenever possible, a density ratio of unity should be run as a base line for variable-density data. One configuration which may be valuable for study of both subsonic and supersonic plane mixing layers is flow over a two-dimensional downstream-facing step. The bottom wall downstream of the step should be porous, uniform injection of an arbitrary gas being in a direction normal to the main flow. The natural entrainment rate is presumably matched when pressure disturbances in the free stream are minimized.

(2) Experiments clarifying the role and importance of various parameters which determine the behavior of the near field as well as the conditions under which any of these parameters can be neglected.

(3) Experiments determining the cumulative effect of initial conditions in terms of the distance to fully established flow, experiments relating these results to those of the second class. Note that similarity seems to be reached significantly earlier for mean-velocity profiles than for second-order correlations of velocity fluctuations. The distance to similarity for higher order correlations is probably still greater.

(4) There exist few documented cases of coflowing turbulent layers, that is, cases where two layers of distinctly different initial turbulence structure flow side by side at the same mean speed. Data on such flows should increase our understanding of free turbulent shear flows and aid in model building.

(5) Experiments using contemporary experimental techniques (computer-assisted instrumentation, conditional sampling, and averaging) should be carried out to study structure in free turbulent shear flows in order to complement and support contemporary work on boundary layers. The emphasis among predictors at this conference has been on turbulent field methods based on conventional long-time averaging. However, there is a growing conviction among many experimenters that long-time averaging (Reynolds stresses, higher correlations, spectra) may not be the most productive way to describe turbulent flows. An alternative approach is implicit in recent work on large-eddy (or wave) structure in turbulent boundary layers by Kovasznay, Offen, and others (refs. 1 to 3). If a large eddy can eventually be described experimentally with sufficient credibility for a given flow, then averaging over a moving pattern of such eddies can serve the same purpose as conventional averaging while avoiding some of the present disadvantages (such as trying to cope with the phenomenon of intermittency). With few exceptions, research

in free turbulent flows has so far not used conditional averaging techniques or attempted to exploit the contemporary point of view.

DESIRABLE MEASUREMENTS FOR VARIOUS CASES

The committee believes that improved and extended data are needed both for construction of models and for tests of specific applications of these models. Many of the existing data fail to report significant experimental parameters and are also deficient in reporting cross-checks and in estimating uncertainties. In only a handful of cases have the effects of systematic variation of initial conditions been reported. Parameters estimated from complex flows have often been mixed indiscriminately with parameters estimated from nearly classical flows in model building.

The committee believes that rapid advancement of predictive capability requires more complete reporting of experimental data and also requires arrangements for storage of complete original data in suitable archives (not merely on small published figures).

Because of existing difficulties (see paper no. 2 by Birch and Eggers) in establishing a value for the spreading parameter σ for the plane mixing layer, the committee suggests that the dependence of σ on χ be reported. Different workers have employed different definitions for σ; the specific definition employed should always be explained. In the plane mixing layer and other flows with similarity, there is usually some confidence in the exponents for the similarity laws. Therefore, more can be learned about the effect of initial conditions on apparent origin, about rate of approach to similarity, and about effects of scatter in difficult measurements far downstream, by abandoning the usual log-log scales in favor of plotting dependent variables to the appropriate limiting power against a linear scale for the independent variable x.

Various permutations of laminar and turbulent boundary layers can occur on the solid surfaces which are involved in the generation of a free shear flow. Documentation of these boundary layers just before separation should be an inherent part of near-field studies and should also be recorded whenever persistence of effects of initial conditions into the intermediate and far field is of concern.

For the case of laminar initial boundary layers, two kinds of instabilities have been observed; sinuous oscillations of the entire layer (ref. 4) and vortex roll-up of the individual layers (ref. 5). Sinuous instability is also reported by Brown and Roshko (paper no. 18 of this compilation) for the turbulent case. The presence of such instabilities should be suspected and reported if found.

In confined jets the committee urges that static pressure, including at least wall pressure, be measured and reported. In closed regions of separation (with recirculation), both curvature of the dividing mean streamlines and lateral constraints on the flow (for example, end conditions and wall proximities) should be reported. There may be a tendency for cellular three-dimensionality to develop, often with unsteady elements; this possibility should be carefully examined.

The round jet involves relaxation of one classical flow in the near field toward another classical flow in the far field. Our belief is that such flows should be studied extensively for the sake of the relaxation property, with particular attention to the rate of approach to the final equilibrium state. From the predictor's point of view, such relaxing flows involve a change in local length scale. Examples of relaxing flow studies are the Chevray and Kovasznay experiment (ref. 6) where a symmetric pair of turbulent boundary layers relaxes toward a plane wake; the Knystautas experiment (ref. 7) where a row of round jets relaxes toward a plane jet; and the productive study by Prabhu and Narasimha (ref. 8) of a plane wake relaxing after being perturbed. The relaxing of a jet or wake with significant density variation toward a constant-density flow has previously been mentioned. Note that initially rectangular or elliptical wakes or jets are known to interchange their major and minor axis of symmetry (at least once) during their approach to equilibrium. In all cases, it should be expected that the relaxation process may be extremely slow.

More work directly on instrument development is needed, for example,

(a) for means to measure static pressure fluctuations in a moving fluid

(b) for use in high temperature fluids, and

(c) for more accurate sampling techniques for measurement of species concentration

EXPERIMENTAL PRECAUTIONS AND PITFALLS

Considerable caution is required regarding three-dimensionality in nominally plane flows. End-wall boundary-layer effects which modify the entrainment process, and other obstacles which block or otherwise displace the flow, can give seriously distorted results. Note specifically that comparison of $\bar{u}(y)$ profiles at various lateral stations is not a sufficient guarantee of two-dimensionality. Streamwise invariance of momentum flux is a much better check. Specific calculations and experiments are needed to ascertain when three-dimensional effects are negligible.

In axisymmetric (or three-dimensional nonround) flows, traverses should be made in more than one direction both as a cross-check and because transformation of major to minor dimension by action of turbulent shear stresses can occur.

A key property in the growth of any free turbulent shear layer is entrainment of nonturbulent flow from the surroundings. Jets, in particular, have an equivalent sink effect (ref. 9), and this effect needs to be studied and understood by the experimenter.

Because of entrainment, the surrounding fluid is not really at rest, and the presence of walls (as in flow over a step or a cavity) can seriously affect the value of the spreading parameter. It follows that any aspect of an apparatus which can affect entrainment (for example, closed versus open test sections, walls close to separated flows, downstream obstacles, etc.) can introduce scatter in any correlation of results. Even for a flow as simple as the round jet out of a wall, the presence and size of the wall may modify the pressure field and the spreading rate. Although no single prescription can be given, it is recommended that specific consideration of the entrainment process be incorporated in planning and designing any realization of a classical experiment in the sense used herein.

In supersonic cases, caution is necessary to avoid (or at least document) the effects of lip shocks and other pressure perturbations. Observations of static pressure for supersonic regimes are also essential for clear interpretation.

Direct calibration of hot wires for compressible flows (Mach number greater than 0.3) is essential in order to separate effects of velocity from those of temperature and density. (See, for example, ref. 10.)

REFERENCES

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3. Gupta, A. K.; Laufer, J.; and Kaplan, R. E.: Spatial Structure in the Viscous Sublayer. J. Fluid Mech., vol. 50, pt. 3, 1971, pp. 493-512.
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8. Narasimha, R.; and Prabhu, A.: Equilibrium and Relaxation in Turbulent Wakes. J. Fluid Mech., vol. 54, pt. 1, July 1972, pp. 1-17.
9. Taylor, Geoffrey: Flow Induced by Jets. J. Aero/Space Sci., vol. 25, no. 7, July 1958, pp. 464-465.
10. Morkovin, Mark V.: Turbulence in Small Air Jets at Exit Velocities Up to 705 Feet Per Second. J. Appl. Mech., vol. 25, no. 2, June 1958, pp. 314-315.

DISCUSSION

R. B. Edelman: I will address this question to the panel in general: Have you given any consideration to the type of instrumentation that should be used for the types of quantities that one might be interested in, in turbulent flow; for example, mean flow properties, the turbulence quantities, and then finally concentration measurements. And perhaps I should generalize this to include reacting flows as well, which is an area that is of ultimate interest to many of us.

D. E. Coles: Well, I will think out loud a little bit, I would like to be excused from talking about reacting flows. I do not have any experience with those and perhaps I do not want any. I think the most promising instrument these days is the laser Doppler anemometer and I think very rapid developments will occur. We even have hopes of being able to measure vorticity with one of those things, if we can figure out how to do it. It is not so much a matter of instrumentation as it is a matter of attitude toward these flows, I think. One of the points that Kline made and that I would like to underline is that there is a significant body of experimenters, mostly working in the boundary-layer field, who are not going in the same direction as everybody else. They are aiming at substituting some other kind of formulation for these turbulent problems than the classical one which began with Osborne Reynolds about 100 years ago. Laufer made the point that you clearly throw away all phase information in the problem as soon as you draw bars over things, and experience has shown that clever kinds of conditional sampling and conditional averaging can reveal highly unexpected properties of these turbulent flows. I think the first of these was intermittency, but there are many others; the bursting phenomenon, the sublayer structure in boundary layers, and large-scale structure in shear layers. All these things suggest that what is going to happen or what may happen is that the eddy chasers will succeed in catching an eddy and assigning some properties to it, a shape, and an intrinsic velocity field. The kind of averaging we will come to, maybe 10 years from now, will be an average over a moving pattern of these eddies treated as operators. I like the word operators; others do not agree with me. This is virtually a complete rejection of the traditional schemes of thinking in terms of correlations, auto- or cross-correlations, Reynolds stresses, spectral operators, etc.; and I would like to see more work done in the free shear flow business along these lines. I think Wygnanski has been the only man who has tried to apply these ideas in the sense that the boundary-layer people are doing, although I think the Brown and Roshko experiments[1] will certainly have to be continued with this point of view. You have to explain those pictures. You have to know about those pictures and if you know about those pictures you should be nervous about Reynolds stress.

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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.

S. J. Kline: I think there is a general dearth of experimental work in relation to the level of theoretical work. Some of the big hangs-ups are in the data and there is not enough data taking and even less effort on instrumentation. There is even no really good textbook on the subject of how measurements are made in moving fluid fields. There are very few schools that give courses in this subject. I can advertise a little bit here, we are one of the very few that do. I think that more effort on both of those problems is needed and that there is really an enormous amount of work to be done.

H. H. Korst: Considering myself now as a spokesman for those who contributed to this conference as predictors, I wish to direct attention to the lack of concise and needed information concerning initial conditions in some of the selected test cases. This, in particular, refers to the specification of the microstructure for the starting profile.

If a well-defined attached boundary layer precedes the mixing process, one may assume that the work of Maise and McDonald[2] provides some guidance; yet, there seems to be evidence that such a structure does not carry over smoothly into the developing free shear layer but may actually be subjected to a rapid and rather catastrophic breakdown as a consequence of large-scale instabilities as has been shown so dramatically in the work of Brown and Roshko.[3]

So I would like to ask the panel to suggest what instrumentation and observations may be needed to cope with, and extract information on, boundary-layer breakdown which may have a large influence on the structure and initial development of near wakes.

S. J. Kline: Yes, I wanted to say one think about that and I wanted to show some pictures that I brought with me. Mr. Oseberg,[4] in my laboratory a couple of years ago did study a plane jet in water using a kind of visualization technique which is well-known to you. He studied three different velocity ratios and three different sets of initial conditions. He did find instabilities for the laminar separating layer very much like those in the pictures that Professor Roshko showed. When one has a turbulent boundary layer leaving the surface, one still gets quite a lot of instability but it is not as clean and as pronounced. I had hoped to show those pictures but the projectionist tells me that the motion-picture copy that I brought with me has no holes in it – so we will not see those, but they do appear in Oseberg's thesis.[4] As far as instrumentation goes, I think there are lots of ways of measuring that – a simple visual technique which Roshko has already shown you will get beginning measurements. Cross-correlation measurements, which

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[2] Maise, George; and McDonald, Henry: Mixing Length and Kinematic Eddy Viscosity in a Compressible Boundary Layer. AIAA J., vol. 6, no. 1, Jan. 1968, pp. 73-80.

[3] 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.

[4] Oseberg, Öyvind K.; and Kline, S. J.: The Near Field of a Plane Jet With Several Initial Conditions. Rep. MD-28 (NSF Grant GK-10034 and Contracts AF 49(638)-1278 and AF-F44620-69-C-0010), Stanford Univ., May 1971.

Vic Goldschmidt has already suggested, should certainly be very adequate to measure that kind of phenomena at least in the beginning. I would suggest that one wants to document whether the layer at separation at the back of the solid surface is laminar or turbulent because this does make a qualitative difference.

P. A. Libby: I was rather disappointed that our distinguished panel did not use the leverage of their prestige when they called for very careful and detailed experiments to point out to those in the audience who have money to disburse in this direction that they must be patient when they disburse that money. They must expect to fund that sort of work over years, not a 6-month affair. I assume that our panel agrees on that and is very embarrassed by its omission.

V. W. Goldschmidt: I cannot answer Paul's point – I would like to speak about the eddy chasers a little bit. I think Val Kibens[5] is looking at the wake of a flat plate right now and I think he is also heating it. René Chevray[6] at Stonybrook is looking at a circular jet; Fiedler[7] in Berlin is also looking at a heated circular jet and our group at Purdue University is also looking at a plane heated jet. However, I would like to direct a question to the panel. You made reference to a need for documenting information that is available and I think this is a crucial point. We cannot pass the buck to NASA. I think we can start accusing ourselves in the shortcomings of previous work. For instance, to be more direct, the selection committee for experiments forgot to include Gunnar Heskestad's[8] work which I think is the best around on plane, circular, and radial jets. But coming to the point on hand, how can we be more cautious and how would you suggest creating these archives of data that you refer to.

S. Corrsin: I am not volunteering but I would like to remind everybody that there is still no generally accepted way of measuring static-pressure fluctuations in a turbulent flow. It would be nice if somebody would go to all the trouble of developing appropriate instrumentation. I guess there are probably half a dozen places in the world where people are allegedly working on such instruments and I think that no two of them agree that anybody else is doing it right. So, if someone could set aside his interest in detailed hydrodynamics for awhile and develop such a device it would be very useful. In terms of the

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[5] Oswald, L. J.; and Kibens, V.: Measurements in the Wake of a Disk. Paper EA1, 1970 Annual Meeting of Division of Fluid Dynamics, Amer. Phys. Soc. (Charlottesville, Va.), Nov. 1970.
Oswald, Lawrence James: Turbulent Flow in the Wake of a Disk. Ph. D. Diss., Univ. of Michigan, 1971.

[6] Dr. René Chevray: Department of Mechanics, State University of New York at Stonybrook, Stonybrook, Long Island, New York 11790.

[7] H. Fiedler: Hermann Scöttinger Institute, Flür Strömungstechnik, Technische Universität, Berlin, Germany.

[8] Heskestad, Gunnar: Two Turbulent Shear Flows: I. A Plane Jet. II. A Radial Jet. Ph. D. Diss., Johns Hopkins Univ., 1963.

applicability of gradient transport models, one of the things that I pointed out a long time ago is that they have greater success in free shear flows than in wall shear flows. This is coincident with the fact that the principal axes of the mean strain rate tensor and the stress tensor happen to coincide for reasons which are unknown in free shear flows and they definitely do not coincide in wall flows. If we move one order higher in the moment hierarchy and use the Reynolds shear stress equations and the energy equations, then we move to the question of whether the gradient transport model might be appropriate for the transport of shear stress or energy. Therefore, it would be worthwhile finding out whether the principal axes of those things happen to coincide with the corresponding transport quantities and this information could be found very easily with existing instrumentation. We just have not done it for some reason which I do not know.

B. G. Jones: Professor Corrsin has commented on the current status of static-pressure information and I wish to add a further comment concerning some experimental observations which we have made at the University of Illinois. Our initial results were reported in Spencer's thesis[9] with some root-mean-square static-pressure measurements in a plane mixing layer included in the AIAA paper.[10] We did not stop our studies at that point although we realized these early measurements were severely contaminated because of velocity fluctuation sensitivity. We cannot say exactly how severe, but it was substantial and was caused primarily from the sensor tip configuration and its orientation. We have continued the studies with the same basic sensor in terms of its internal structure, using bleed type anemometer sensing, but we have modified the tip configuration to make it less sensitive to velocity contamination. Examining this new configuration (which resembles a pitot static tube) far downstream in the self-preserving region of a circular jet, we have been able to estimate the contamination to the pressure signal caused by the transverse and axial velocity field components. The axial component effect is reduced to approximately 2 percent, whereas the transverse component effect is less than 10 percent. With these levels of contamination we are able to make some estimates of corrections to be applied to both root-mean-square pressure levels and velocity-pressure results.

We are now in the process of applying these results to our aerodynamic noise generation program, which was mentioned briefly in our session this morning. We are examining the two-point spatial pressure correlations in the initial mixing and in the developing regions of the circular jet. We expect to continue this work in the plane mixing layer as a means of examining in more detail the initial shear layer in circular and

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

[10] 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.

plane jets. This will also enable a reexamination of the original study of the energy budget across the plane mixing layer and allow improved estimates, particularly with respect to pressure-velocity transport terms.

H. H. Korst: Coming back to the problem of preserving information: We have here a reservoir of people and minds. Having assembled, processed or produced stockpiles of data, is it safe to assume that NASA will, for the time between now and the publication of the Proceedings, act as a guardian of this information?

Furthermore, are there any plans whether this information will be kept beyond that time at a selected central location, preferably at NASA Langley Research Center? Shall we leave this question open or shall we try to exert some gentle pressure on potentially willing individuals or organizations?

D. M. Bushnell: We will publish as Volume II the data that we sent out to the predictors. This is fairly complete data, at least as far as the mean flow is concerned. The problem is not stockpiling the reports that are available, the problem is getting the details on the experiments, that is, the details that Professor Kline has called for and which have often not been included. When one gets an AIAA paper, there is no guarantee that you will get the detail you need to check out the more sophisticated numerical programs. The theses we see coming out of Georgia Tech now that are 3 and 4 inches thick may well have the detail that is necessary. But I do not know where you would store these things and how you would insure that the details are put in.

H. H. Korst: Mr. Bushnell, will you collect and make available such information, if it should be sent to you, and can we write to you at Langley for it?

D. M. Bushnell: I very regretfully decline your invitation – we pay COSMIC[11] to stockpile computer programs and there is a possibility through NASA Headquarters that they could be prevailed upon to fund other sources of storage for this type of information (the experimental data). I think this is the only hope if you want it done on a Government basis.

H. H. Korst: Is there a hope that some organization will take the responsibility at least for the time being? This information is now hoarded and collected by you. Can some responsibility be established, say through a committee, to keep this body of information available?

D. M. Bushnell: Stan Birch really is the one who has collected this stuff. Stan started working in the area about $1\frac{1}{2}$ to 2 years ago and he has an immense pile of information in this area. He was the source for most of the initial data for this conference. The other people I have are regular NASA employees and we are being pushed projectwise just like

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[11] Computer Software Management and Information Center, Barrow Hall, University of Georgia, Athens, Ga. 30601.

everybody else is in the Government, and after Stan goes (he is an NRC associate with us), we just do not have anyone who can spend their time cataloging this stuff and storing it.

S. J. Kline: Could I make one comment on this? I quite agree with Mr. Bushnell that our problem is not simply to get the stuff sent to the Ann Arbor microfilm library[12] or something like that. One of the reasons that we were able to do some of the things we did do in the 1968 Conference[13] was that Don Coles had been an archivist on boundary layers for quite a long time. It does take somebody who has some knowledge about which questions to ask and what to compare with what in order to get the right kind of information. It is not just a function of sending it in and putting it on disks. It does need somebody (we appear to have no volunteers just at the moment), but we thought that – I am not volunteering – this freeway should be in somebody else's backyard. That is the problem. If someone would do that it would help a great deal.

M. V. Morkovin: I think we are putting too much emphasis on past data. I believe that the panel actually decided that an awful lot of that data was not that good. I think the emphasis is on new data (new data with better instruments) that is just going to make the other stuff obsolete. Now we do not have any institution short of ASME, Division of Fluid Mechanics, to do something like that. I think that what we can do is to assure that in the future we have sufficiently viable data that can be preserved. I think our emphasis is on the future; I think we have spent too much time on the past.

S. J. Kline: I am sorry if I did not make myself clear. There was no intention to talk about restockpiling past data, but the intention was to have someone look at the data as it came in. We have some questions about data taken 20 years ago which we cannot answer because the people do not remember. If someone could collect the future data, look at it critically and ask those questions, you would get better documented data and it would be available. I am talking about looking toward the future, not looking at the past.

C. E. Peters: It seems to me that all of us have more than one kind of document that we can disseminate. Papers are not the place to present the details of experiments. Most of us have access to some reporting technique, for example, a technical report of some sort, and if it has not been brought up before I strongly suggest that you summarize your results in papers but go to the trouble of writing reports, like Jim Eggers'[14] report or

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[12] University Microfilms, Inc., Ann Arbor, Michigan.

[13] Kline, S. J.; Morkovin, M. V.; Sovran, G.; and Cockrell, D. J., eds.: Computation of Turbulent Boundary Layers – 1968 AFOSR-IFP-Stanford Conference. Vol. I – Methods, Predictions, Evaluation and Flow Structure. Stanford Univ., c.1969.

[14] Eggers, James M.: Velocity Profiles and Eddy Viscosity Distributions Downstream of a Mach 2.22 Nozzle Exhausting to Quiescent Air. NASA TN D-3601, 1966.

Don Chriss'[15] report from our laboratory, in that kind of detail so that you do not need a central repository for data. It is then recoverable, particularly for those people who have different interests in the experiment than the original experimenter. I picked these two examples because they have relatively good tabulations of the mean flow results, which is one thing that is overlooked so often.

S. Corrsin: I want to make a comment about Dr. Peters' suggestion that published papers could be essentially summaries and that internal reports of some kind could be the repositories of all knowledge. One of the greatest tragedies in public science and engineering in the world has been the growth of the shadow literature of reports which are not generally referenced and which are generally inaccesible to anyone who does not have a Government contract. There is nothing more distressing to a graduate student who does not have a contract than to see a bibliography in someplace like the AIAA Journal where two-thirds of the things referred to are inaccessible to him. I think this is exactly the wrong way to go.

C. duP. Donaldson: Maybe the thing that really should be done, since it is going to be very difficult to get somebody to do this archival job, would be for the people here at NASA to prepare a report which listed what they considered to be the minimum standards for a decent job of reporting a turbulence experiment.

D. M. Bushnell: I think that Professor Kline is much more able to do that especially in regard to the committee activity.

S. J. Kline: We have not dealt with the details of whether one should measure particular kinds of correlations because that is so dependent upon which model you are looking at and also because we are aware of the difficulties that Professor Corrsin just mentioned. There is no point in recommending that somebody measure static pressure correlated with something else if you cannot measure static pressure. I will agree that we should say something about the need to be able to measure static pressure. But I would like to emphasize again what I said in the beginning of this talk that we would like to hear all the ideas from this audience on exactly the point that Dr. Donaldson mentioned. We did include remarks about the need for a variety of flows to be documented, that is, flows where there are large mean strain but not much production as being the kind of thing people ought to think about and investigate.

D. E. Coles: I want to add some detail to some things Steve Kline said in the summary. I am not myself a practitioner of the delicate art of mobilizing armies of rate equations to calculate the development of turbulent flows. I would leave that to the people who are now doing it and others who may wish to join them. I would stop with something much

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[15] Chriss, D. E.; and Paulk, R. A.: An Experimental Investigation of Subsonic Coaxial Free Turbulent Mixing. AEDC-TR-71-236; AFOSR-TR-72-0237, U.S. Air Force, Feb. 1972. (Available from DDC as AD 737 098.)

simpler. But, regardless of the use that is to be made of the data, I think it would be refreshing to see a number of experiments done with the attitude that they are dealing with relaxation problems involving some kind of perhaps, linear system relaxing toward an equilibrium state. One of the flows in this conference, the Chevray and Kovasznay flow with a plane wake developing from a boundary layer, is the kind of flow I have in mind. You understand the starting condition for that flow; the boundary layer has been documented to death by now. You understand the wake of that flow. What you do not understand is what happens in between and the defect in those particular measurements is that they do not give enough detail. They do not give enough detail both in kinds of measurements and in the resolutions of the measurements. The exciting things in that relaxation process all happen in about the first two boundary-layer thicknesses behind the trailing edge.

One of the reasons I mentioned this is that we have what I consider to be a very beautiful piece of work by Prabhu and Narasimha,[16] who produced a plane wake with a body which they tinkered around with until the wake was in equilibrium somewhat sooner than it would be behind a cylinder, for example. Then this wake was run through a pressure gradient and the test was to define some measure of the departure from the equilibrium state in the new constant-pressure field and how fast this flow recovered. It turned out that it approached the asymptotic state exponentially. That is a very nice thing to know if you could figure out what the exponent is or how to calculate it. The departure was measured in terms of the difference in the amplitude of the shearing stress distribution compared with the amplitude that went with the velocity profile in the equilibrium state. Given the velocity profile, there are two stress distributions, the equilibrium one and the one you measure. These are different and that difference is the measure of the departure from the equilibrium in this approximation. I have mentioned some other flows that I think are examples of relaxation problems. One of these has to do with the wake behind a body which is not round, for example, rectangular or elliptical. There has been a little work done on this wake, mostly at Colorado State University. These wakes are a little rubbery; the axes downstream in the wake are inverted with respect to the major and minor axes of the body itself.

Jets do the same thing. It would be nice to know something about these flows. Sooner or later somebody will have an idea. I do not claim that I know what should be done with information like this; but I do claim that the more of this that there is the sooner the ideas will appear. Most of these flows (except for the mixing layer which is in a class by itself) flow with variable densities, heated wakes or jets with either density

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[16] Narasimha, R.; and Prabhu, A.: Equilibrium and Relaxation in Turbulent Wakes. J. Fluid Mech., vol. 54, pt. 1, July 1972, pp. 1-17.
Prabhu, A.; and Narasimha, R.: Turbulent Non-Equilibrium Wakes. J. Fluid Mech., vol. 54, pt. 1, July 1972, pp. 19-38.

differences due to Mach number, or density differences due to composition, are also relaxing flows. There is a significant region of the flow where, if you only looked at the velocity, you would think of it as being in equilibrium, but if you look at the density, there are significant density differences, which are wiped out eventually by entrainment. I am saying that I think that this is a simple corner of the shear flow problem that I would like to see somebody spend some time in. I mention even the experiment by Knystautas.[17] I do not know how many of you are familiar with it but I thought that this was a well-conceived experiment. It was a series of round jets at the trailing edge of an airfoil spaced a couple of diameters apart. Presently what you saw far downstream looked like a plane jet as it obviously would have to, except that I think that if you look carefully enough at it, it might not look like a plane jet – the scale might be haywire.

The reason for this statement is the experience people have had with grid turbulence. As we know the characteristic velocity in a plane wake decreases like $x^{-1/2}$, and therefore if you produce turbulence with an array of plane wakes you would expect the square of that velocity or the turbulence intensity to decrease like x^{-1}, which is about what it does. If you make a grid out of spheres we know that the characteristic velocity downstream of a sphere decays like $x^{-2/3}$. Therefore, the energy in that kind of isotropic turbulence should decay like $x^{-4/3}$ which is a different rule, although the tendency is to think that isotropic turbulence is just that no matter where you find it. Well, the experiment was done by Kistler (unpublished) some years ago at our place. I never saw the results, but my impression was that the decay rates were different.

S. Corrsin: Geneviève Comte-Bellot[18] conducted experiments with a silver dollar grid made out of aluminum at our place and the decay was the same as behind square rods and round rods.

D. E. Coles (speaking from a sketch on the blackboard): I am not sure this is original, but it is a representation of something a student named Ikawa[19] is doing at our place; he is somewhere in the depths of Guggenheim and we see him infrequently. He is interested in the mixing layer in which the external stream is supersonic. The scheme is simply to inject, I believe air, but it obviously does not have to be air, through a porous wall and to adjust the injection rate until the pressure disturbances are a minimum in the external flow. That means you have the entrainment tailored right, the entrainment on the low-speed side. All sorts of nasty things happen to you when you try to do this, like secondary

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[17] Knystautas, R.: The Turbulent Jet From a Series of Holes in Line. Aeronaut. Quart., vol. XV, pt. 1, Feb. 1964, pp. 1-28.

[18] Comte-Bellot, Geneviève; and Corrsin, Stanley: The Use of a Contraction To Improve the Isotropy of Grid-Generated Turbulence. J. Fluid Mech., vol. 25, pt. 4, Aug. 1966, pp. 657-682.

[19] Hideo Ikawa is a student of Dr. Toshi Kubota, Professor of Aeronautics at California Institute of Technology. The Ph. D. thesis of Ikawa is to be published in June 1973.

flows in the cavity and so on. They also found that they had to put a thing downstream to steer the flow out of the cavity and to minimize the pressure disturbances; otherwise, there is liable to be a separated region there and some shocks and so forth. But this is a geometry that I think might be very useful in looking at what I consider to be the deepest mystery in the subject right now – the effect of density on the mixing layer. As far as I can tell, that is a mess! Certainly, if no other experiments get done, I would like to see that subject cleared up.