REPORT OF CONFERENCE EVALUATION COMMITTEE

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INTRODUCTION

For the past 2 days we have been exposed to the concentrated comparison of screened data with the results of months and months of effort on the part of our best predictors. Apparently some honing of the methods took place even before the conference as the predictors generally confronted a broader range of flows than those which conditioned the original choices of their empirical constants or functions. Thus, part of the objectives of the conference were accomplished before its start.

To the working engineer the proliferation of the methods appearing helter-skelter in the journals has presented a confusing picture. We believe that the codification of these methods and their exposure to the same broad set of data should go a long way in clarifying the limits of validity and the areas of usefulness of the different classes of methods.

As the reader can judge, the presentations were uneven and the results do not lend themselves to easy evaluation. It would indeed be presumptuous and unscientific to render any definitive judgments on the performance of the 13 heterogeneous predictive methods in the conference; we could do more damage than good. This situation is, in part, brought about by the less satisfactory state of experimental information for free shear layers than for attached boundary layers.

Rather, we shall report on our strongest impressions of the issues pertinent to the modeling of turbulent flows primarily for applied objectives, as conveyed by comparison figures[1] (pp. 699 to 737), and by the lively discussions during the conference sessions (which many participants continued late into the night). An engineer interested in free turbulent shear flows will find these figures a gold mine of information. In fact, the experimental data with the many theoretical predictions provide him with a zeroth-order tool for his own quick engineering estimates. He is also referred to the correlation of Stanley F. Birch and James M. Eggers (paper no. 2) for the spreading rate of simple mixing layers which is useful on the same level.

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[1]Only a small part of these comparisons was available to the Conference Evaluation Committee. Thus, we could not pinpoint several of the methods which leave a lot to be desired.

Before we proceed to the predictive methods themselves, we need to consider the potential uses of these methods and relate them to the distinguishing characteristics of the methods. Advances in technology usually involve the development of more efficient systems out of existing concepts. Examples include reductions in system size and weight through optimization of cooling requirements and reaction volumes in (1) power generating equipment, (2) propulsion and vehicle systems, (3) varieties of chemical process plants, and (4) specialty hardware such as in gas dynamics and chemical laser systems. In many of these systems public concern over the questions of thermal, chemical, and noise pollution must also be considered in present and future designs. Achieving high performance, as well as controlling pollutant emissions (where appropriate), requires the development of highly accurate predictive techniques. Such systems usually contain turbulent free shear layers and mixing zones (including jets and wakes). The understanding and description of these turbulent shear flows should enable us to predict the structure of the flow field and determine relationships among design parameters. Due to the intractable nature of turbulent flows, approximate mathematical models must be formulated to describe these turbulent processes.

Many problems require definition of the mean flow only. Quantities of interest include the spreading rate, penetration, and degree of nonuniformity in the mean-flow properties downstream of the origin of the mixing zone. The aerodynamic influence of a shear layer on its environment and the effectiveness of momentum exchange and fuel distribution in a combustor are typical examples of problems requiring primarily a definition of mean-flow properties. For these problems a simple representation may be employed for the turbulent exchanges. However, these models must account accurately for the effects of velocity, temperature and density differences, and pressure gradients.

Another class of problems requires more detailed descriptions and computations of the turbulent structure. These problems include (1) the propagation of disturbances through turbulent layers, (2) the generation of noise, (3) dynamic loads on aircraft, and (4) flows involving kinetic processes where the reactants are not intimately mixed and rates of reaction cannot be defined in terms of mean-flow properties only. For example, predictions of ignition and pollutant emission are crucially sensitive to trace amounts of free radicals and these, in turn, depend upon the local turbulent spectrum. It is therefore apparent that the complexity required in turbulence modeling depends on the problem. That is, the selection of a method should depend upon a careful determination of the information required to resolve the particular needs of the user. It would be unreasonable to employ a highly complex method where only gross quantities are needed. Conversely, low-order techniques cannot be expected to provide answers to problems requiring detailed information on turbulent structure.

CLASSIFICATION OF METHODS OF INCORPORATING TURBULENCE EFFECTS

As indicated by the wide variety of methods presented at this conference, there are a number of approaches to the prediction of turbulent shear flows. It seems worthwhile to make a general classification of these approaches in order that minor differences between two methods should not obscure their commonality and that we may see where the various contributions to the conference fit into a broad methodological structure. This classification will pertain to the means by which the physics of turbulence is incorporated into the describing equations, not to the means by which the resulting equations are solved.[2] To emphasize the distinction, the equations developed for a sophisticated closure scheme could conceivably be solved by an integral method, although to do so would not be reasonable.

The basic problem in the description of turbulent flows (as usually treated in terms of the time average of the dynamical variables and their correlations) is that an open-ended hierarchy of equations results from the systematic treatment of the original, time-dependent conservation equations. This is the so-called closure problem. To illustrate, for constant-density flows the momentum equation corresponding to direction x_i involves the mean velocity components $\bar{u}_i$ (i = 1, 2, 3), the mean pressure $\bar{p}$, and the six correlations $\overline{u_i'u_k'}$ (i,k = 1, 2, 3). In many flows of applied interest, boundary-layer approximations and other simplifications apply so that these six reduce to one, namely, $\overline{u_1'u_2'}$.

Until recent years this problem was overcome, in most engineering problems, by introducing a closure model at the first level in hierarchy, e.g., by formally introducing an eddy viscosity ϵ, so that $-\overline{u_1'u_2'} = \epsilon\left(\partial u_1/\partial x_2\right)$ and by then relating ϵ to specified flow properties and to the dependent variables. Today this approach provides only one class of methods for turbulent free mixing flows. The variations on the eddy-viscosity approach involve introduction of mixing length concepts, turbulent Prandtl and Schmidt numbers, and sophisticated correlations for ϵ involving a variety of effects – for example, variable density. In view of the minimal content of the physics of turbulence in this class of methods, it is somewhat surprising that in many problems of applied interest entirely adequate answers are provided. This can be seen for several of the contributions to this conference, especially where empirically determined constants are grafted on the correct dimensional constraints of the procedure. Thus, if the problem relates to the spreading rates and diffusion – either of a coaxial jet in a moving stream or of the wake of a reentry body – the customary and generally adequate approach for its solution would involve utilization of some appropriate model for ϵ, preferably one which has been verified by comparison with experiment in a closely related flow. This situation concerning

[2] The reader would benefit greatly at this stage by referring to reference 1 and to sections 4 and 5 of reference 2. Longer discussions can be found in references 3 and 4.

the utility of eddy-viscosity methods is likely to continue; that is, such methods will be entirely adequate for many problems. In this conference the contributions of David H. Rudy and Dennis M. Bushnell (paper no. 4); L. S. Cohen (paper no. 5); H. H. Korst, W. L. Chow, R. F. Hurt, R. A. White, and A. L. Addy (paper no. 6); Joseph A. Schetz (paper no. 8); J. H. Morgenthaler and S. W. Zelazny (paper no. 9); and V. Zakkay, R. Sinha, and S. Nomura (paper no. 10) fall within this first classification.

As mentioned previously, there do arise in engineering applications problems which involve more of the physics of turbulence than is contained in eddy-viscosity approaches. In addition, there are cases in which the eddy-viscosity approaches are inadequate for the prediction of mean properties. For example, flows which involve abrupt changes in the character of the main stream, with the so-called nonequilibrium effects and relaxation to a new dynamical state, are not well predicted by methods employing eddy-viscosity concepts because of their local nature.[3] Because of this situation and because of the advent of high-speed computers which make feasible the numerical treatment of complex systems of partial differential equations, new approaches have been developed in recent years which incorporate more of the physics of turbulence into the describing equations. These approaches possess greater variety and flexibility. At this conference the contribution of B. E. Launder, A. Morse, W. Rodi, and D. B. Spalding (paper no. 11) provides an excellent review of several of these new methods; an earlier paper by W. Rodi and D. B. Spalding (ref. 5) gives a somewhat broader perspective.

These new methods may be characterized according to whether they introduce the turbulent kinetic energy alone or other velocity correlations as well, and according to whether the one or more length scales which arise in the analysis are specified algebraically in terms of computed quantities such as the thickness of the mixing layer, or are defined by partial differential equations. For clarity, consider one of the early types of these new methods. (See ref. 6 and paper no. 16 by Thomas Morel, T. Paul Torda, and Peter Bradshaw.) It has been observed that in a wide variety of flows the mean turbulent shear is related algebraically to the turbulent kinetic energy; that is, $\overline{u'v'} = a\overline{q^2}$, where a is a semiempirical function. Thus, one adds to the usual set of mean equations, the equation for the conservation of $\overline{q^2}$, an equation which is on the second level of the hierarchy of describing equations. However, this equation involves certain correlations, for example, the triple correlation $\overline{u_2'u_i'u_i'}$. These must be described in terms of the principal dependent variables by what is usually called "structure" or "modeling"; the modeling process introduces at least one length scale. In applications of this early method, only one length scale appears and is taken to be an algebraic function of the thickness of the shear layer.

[3]The method presented by David H. Rudy and Dennis M. Bushnell (paper no. 4) attempts by appropriate phenomenology to extend the mixing length concept to some of these problems.

A closely related method is termed the "Prandtl energy method" wherein the eddy viscosity is expressed in terms of $\overline{q^2}$ so that

$$-\overline{u_1'u_2'} \propto q\Lambda \frac{d\bar{u}_1}{dx_2}$$

where Λ is a suitable length scale. Again an equation for Λ must be established.

Further developments involve use of the conservation equations for velocity correlations other than those associated collectively with $\overline{q^2}$. Each of these equations are entries in the second level of the hierarchy of describing equations and involve modeling to close the set of equations. Indeed, a separate equation for the mean shear $\overline{u_1'u_2'}$ can be included. It will be recognized that these methods provide, as part of the solution procedure, predictions for some of the physical aspects of turbulence. For example, in the method outlined above, the turbulent kinetic energy is found in the course of the solution and this may be of interest by itself. Furthermore, the mean shear depends on the turbulent kinetic energy which involves convection, diffusion, and dissipation and therefore involves an upstream history. Accordingly, the shortcomings listed previously regarding nonequilibrium effects may be more readily overcome.

In applications of these methods involving well-defined length scales, the assumption of relatively simple relations for these lengths is justified. However, in problems involving more than one scale (e.g., when two shear layers with different characteristic dimensions interact or when a turbulent boundary layer expands around a downstream facing step), these simple relations are not convincing and more complicated means of describing the length scale (e.g., by partial differential equations) are probably needed. There are at least three means for deriving appropriate length-scale equations; each generally involves a high degree of modeling with attendant possibilities for error.

The many feasible combinations of correlations and length scales make possible a wide variety of methods and have led to a proliferation of publications on these methods in recent years – a mixed blessing. These new methods are under rapid development; some will become obsolete through further refinement. Their utilization should be undertaken only if the nature of the flow and of the desired information warrants dissatisfaction with the less sophisticated methods. It should be emphasized that the incorporation of the elusive effect of variable density into these methods has only been tentative and that much improvement is in order in this regard.

The new methods require specifications of initial data – for example, the distribution of turbulent kinetic energy or other correlations at some initial station. Thus exploitation of these more powerful methods for more accurate predictions, in fact, requires more detailed a priori knowledge of the initial features of the flows than is generally available.[4] Scattered evidence suggests that downstream developments of free shear flows are more sensitive to such details of initial turbulent structure than are developments in wall boundary layers, both experimentally and numerically. Adequate assessment of the new methods involving turbulence characteristics will naturally place additional demands on the experimentalists. As additional correlation terms which are explicitly modeled in the theories become accessible to direct, accurate measurements, the credibility of the methods could increase convincingly beyond that based on present comparisons of gross quantities and mean profiles. In this connection reference is made to the report of the Committee To Recommend Critical Experiments, and to the open forum discussion disclosing scepticism as to the relevance of the Reynolds' type of averaging to the important physical mechanisms governing the flows.

At this conference in addition to the contributions of B. E. Launder et al. (paper no. 11), those of Paul A. Libby (paper no. 12), P. T. Harsha (paper no. 13), P. M. Heck and M. A. Smith (paper no. 14), P. J. Ortwerth (paper no. 15), Thomas Morel et al. (paper no. 16), and C. E. Peters and W. J. Phares (paper no. 17) involve these newer techniques.

IMPRESSIONS

When the reader embarks upon his own study of the results in the comparison figures (pp. 699 to 737), he would do well to refer constantly to the classification sheets (pp. 739 to 761) for the concise description of the methods as well as for an appreciation of the costs.

– Substantial progress has been made in sharpening the predictive capability for both mean and turbulent quantities in several basic flow configurations. Yet no single technique appears sufficiently general to warrant classification above all others.

– The cross-checked and related group of methods reported by the team at the Imperial College of Science and Technology represents a broad approach which promises a systematic development toward a practical degree of generality.

– The one integral technique presented is adequate for mean properties but while more flexible than other integral methods, it cannot provide the details of the flow offered by finite difference methods.

– The selection of a method by a user should be based upon the requirements of the problem and the cost of computations.

– Information concerning computational costs can be determined from the data given on the program descriptions in the classification sheets (pp. 739 to 761).

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[4]See also comments on the "near field" in the report of the Committee To Recommend Critical Experiments.

– Many methods have been oversold. It is most important to specify limitations of regions of applicability of the methods; this has rarely been done in the past.

– One wonders what the agreements would be if the answers were not known.

– Methods in the same formal category may be quite different in their effectiveness. It apparently depends, in part, on who is using the procedure and on the details of the modeling or fitting of empirical constants. When an author's method is employed by a competitor for comparison purposes, the author invariably complains that his method was not used to the best advantage by hands unfamiliar with the fine points.

SPECIFIC PROBLEMS

Variable Density

In many important turbulent flows, the density varies in space and time due to variations (a) in temperature or (b) in composition, or both. To describe these flows the various predictive methods for turbulent flows with constant density must be extended or reformulated to take into account satisfactorily the effects of variable mean and fluctuating density for both cases (a) and (b). This is one of the central problems in applied turbulence research. However, the physics of such flows must be better understood before their analytic description can make substantive progress. The report of the Committee To Recommend Critical Experiments has emphasized the role of high-quality experimentation in developing this understanding.

The equations describing the mean flow field and the various correlations, in cases involving variable density, can be written in a variety of ways depending on how the decomposition into mean and fluctuating quantities is performed. For free shear flows of interest here, the consideration of mass-averaged quantities after Favre (ref. 7) leads to equations which are more compact. But if the quantities which are desired either for comparisons with experiment or for purely predictive purposes involve conventional time averages, then some estimates of the correlations involving density must be made a posteriori. On the other hand, if all dynamical quantities are decomposed in the straightforward manner into a mean-plus fluctuation, the resulting equations are cluttered and various a priori approximations must be considered.[5] No matter which approach is used for describing equations and/or method of solution, the means for incorporating the effects of variable density are generally unclear.[6]

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[5]In the conference, J. Laufer cautioned that in some cases the experimenter may be measuring a mass-averaged quantity without being aware of it.

[6]As a revealing exercise the reader is urged to decide on a consistent mean equation of state for a perfect gas by perturbing the instantaneous forms $p = R\rho T$ and $RT = p/\rho$ (where p is pressure, R is gas constant, T is temperature, and ρ is density).

One should keep in mind that in many flows of applied interest large variations of density do not persist over extended downstream regions. For example, in the case of an axisymmetric jet of hydrogen injected coaxially into an airstream, the mean concentration of hydrogen on the axis decreases rapidly, so that within several orifice diameters on the order of 10, the mean-density difference from the axis to the external flow becomes relatively small. Such insensitive flows do not provide crucial tests of predictive methods, although for the practicing engineer agreement between prediction and experiment may be all he requires.

One class of turbulent shear flows appears crucial for the careful assessment of the effects of variable density. These are the two-dimensional mixing layers wherein the variable density arises from differences in velocity, temperature, or composition in the two mixing streams. In these flows the density differences persist indefinitely in the downstream direction. In addition, the corresponding equations have self-similar solutions so that their numerical analysis can be greatly simplified and the effects of variable density clearly exposed.

Unfortunately, two-dimensional mixing flows exhibiting unequivocal self-similarity are difficult to establish in the laboratory. The effects of initial boundary layers, of transition, and of adjacent walls in both directions (in the XY- and XZ-planes in the usual notation) appear to alter even gross but essential properties of the mixing such as the spreading parameter σ. To illustrate, we consider a special case of two-dimensional mixing, involving a high-speed airstream mixing with quiescent air under conditions such that the stagnation temperature of the moving stream equals the static temperature of the quiescent gas. This is the so-called compressible adiabatic case for which the Mach number M_1 in the high-speed flow provides the only parameter. (More generally $(1/2)(\gamma - 1)M_1^2$ should be considered the parameter (where γ is the ratio of specific tests), but since air is the only gas considered in such flows to date, we confine our attention to M_1.) The effect of M_1 on σ is at present obscure; some data indicate no effect of M_1 on σ whereas other data indicate a significant effect. Similar discrepancies exist for the low-speed mixing of dissimilar gases, e.g., helium and air. These points were brought out and emphasized at the conference in the contribution of Stanley F. Birch and James M. Eggers (paper no. 2).

It thus appears crucial to the development of accurate predictive methods for turbulent shear flows with variable density that high-quality, experimental data be obtained on a few examples of two-dimensional mixing layers with large density differences in the two streams. (For further insight and problems of implementation see the report of the Committee To Recommend Critical Experiments.) Before these are available, the various extensions to include variable density of existing predictive methods for turbulent shear flows must be considered provisional, and they cannot be used with confidence in many flows of applied interest.

Sensitivity of Prediction Methods to Parameter Values and Initial Data

We feel that many of the current methodologies cannot be fully relied upon for general application until their sensitivities to (a) selection of "empirical" coefficients and (b) specification of initial conditions has been documented. Although this type of study was not provided for at the conference, the generality and practical utility of each technique depend largely upon the implications of these sensitivities. We believe that such studies should be performed so that both the theoretical implications and the practical requirements for initial conditions can be defined.

Pressure Fluctuation Terms

Operationally, we view the predictive methods as sophisticated interpolation schemes subject to basic physical constraints, which connect functionally the past empirical data. Whenever we add another algebraic or differential equation to account for the development of another feature of turbulence, we increase the capacity of the scheme to fit better various selected characteristics of the exceedingly complex turbulent fields. It is not surprising that the extra mathematical flexibility (bought at a price which should be properly weighed by the user) leads to better predictions – within the confines of past empirical information. As the predictor works with his program, the terms in his equations tend to acquire a reality of their own which should not be confused with physical reality. Time and again this distinction is brought home with a shock as the predictor applies his machinery to regimes beyond the original empirical foundations, such as we witnessed here in connection with large density changes and higher Mach numbers.

We foresee steady progress in the refinements of the differential field methods commensurate with the need for answers to more refined technological problems, for example, for flows with reactions. The progress, however, could be more apparent than real if the modeling of the turbulence terms do not mirror physical reality accurately. The currently most suspect group of terms describes the effect of the fluctuating pressure, namely, $\overline{u \frac{\partial p}{\partial x}}$ and $\overline{v \frac{\partial p}{\partial y}}$. These terms are usually transformed $\left(\text{utilizing incompressible continuity } \frac{\partial u}{\partial x} + \frac{\partial v}{\partial x} = 0\right)$ and lumped together with "convective diffusion terms," which are distinctly different in physical nature. In compressible flow, continuity involves density derivatives and the difference in the terms is thereby underscored. Since these terms grow with M^2, their careful treatment at supersonic speeds is desirable. We commend to you the remarks of J. Laufer (pp. 687 and 688) on their possible important role in compressible turbulence. These pressure fluctuation terms may well hold the key to the understanding of the changes in free turbulence at supersonic speeds, including the apparent decrease in the spreading rate. The compressible turbulence experts conjecture that these effects would be felt in free turbulent flows at lower Mach numbers than in attached turbulent boundary layers (primarily because of the low-speed wall constraint). Thus the free-shear-layer workers have an extra task as well as an extra opportunity for research.

Gradient Diffusion

In almost all closure schemes including the basic eddy-viscosity models it is assumed that the mean flux of some quantity is proportional to the gradient of an appropriate quantity, for example,

$$\overline{u_1'u_2'} \propto \frac{\partial \bar{u}_1}{\partial x_2}$$

Such assumptions are made at several stages in the analysis in the new closure schemes. Although such models have been used extensively for a long period of time and are repeatedly employed in a casual, uncritical manner, it was emphasized at the conference that gradient diffusion, in principle, applies only when the length scale associated with the large eddies is small in comparison with a length characterizing the gradient. Since this is generally not the case in shear flows of applied interest, it is clear that the use of gradient diffusion must be considered risky.

Alternatives include a bulk diffusion expression and combined bulk and gradient diffusion. (See ref. 8.) In Bradshaw's method for turbulent shear flow (ref. 6 and paper no. 16), the turbulent kinetic energy equation is closed with a bulk diffusion model. However, the general preference for gradient diffusion persists among developers of predictive methods. With improved measuring and computing capabilities, renewed attention should be given to careful assessments of alternative modeling of the various diffusion terms.

Low Reynolds Number

There is evidence that many of the research experiments – and for that matter the intended applications – show Reynolds number sensitivity. For the simple mixing layers the energy supply from the two cocurrent streams is infinite in principle, and we expect an asymptotic approach to a Reynolds-number-independent, self-similar shear layer if we only march far enough downstream. However, the jet and wake flows may be conditioned by low Reynolds number effects throughout their lifetimes. The turbulence structure then may not be "universal," and the measurement and adequate predictions of these flows may present special problems.

We believe that insufficient attention has been paid to understanding of the implications of a larger, active role of viscosity in turbulent shear flows either experimentally or theoretically and to developing modified techniques for computation of flows at lower Reynolds numbers. Some of the apparent experimental scatter may well be a reflection of the unavoidable nonuniversality.

What appears to be needed are simple rules for the experimentalist to use in order to suppress Reynolds number as a parameter. Some years ago, Corrsin provided a lower bound for the Reynolds number of a low-speed circular jet discharging into quiescent air. Bradshaw has done something similar for the two-dimensional, low-speed mixing layer (see paper no. 2). How their rules are affected by high speed and by variable density is not known.

Influence of Geometry

We agree with A. Roshko's remark (during discussion of paper no. 16) about the possibility of real difference in the turbulence structure of two-dimensional and axisymmetric turbulent flows. Although the impact may not be large on our present methods – mostly changes of coefficients – the potentially different structure of the large energetic eddies should entail consequences for the more detailed properties of the flows.

NON-BOUNDARY-LAYER ANALYSES

One of the essential features of all the prediction techniques presented at this conference is the use of the boundary-layer approximation. Evidence of non-boundary-layer effects appeared in some flows used in the conference, such as in wakelike jet flows and mixing regions involving large entrainment rates. Flow situations of this type raise doubts concerning the approximation that $\bar{v}/\bar{u} \ll 1$. In addition, there are flows in which displacement effects and longitudinal curvature alter the streamwise and normal pressure gradients, respectively.

In the case of laminar flow the method of matched asymptotic expansions provides a systematic method for accounting in part for these effects which may be associated with non-boundary-layer phenomena inasmuch as they are not included in the classical boundary-layer theory. In view of the considerable phenomenology we must introduce in order to describe turbulent thin shear layers, it appears inappropriate to use the machinery developed for laminar flow on our turbulent cases. However, some of the physical ideas which evolve from the treatment of laminar flows – for example, in accounting for the effects of large entrainment rates (suspiciously large $v^{(1)}(x,\infty)$ in the usual notation of "inner solutions") and for displacement and curvature effects – may well provide the basis for handling their counterparts in turbulent flows in a rational but nonformalistic way.

In this matter of non-boundary-layer effects, attention might well be called to a contribution of Bradshaw (ref. 9) which shows that many of the complex turbulent flows that are important in engineering are recognizable as perturbations of the classical thin shear layers. The point here is that we should carefully assess the nonclassical, non-boundary-layer effects at hand because, as Bradshaw points out, we may be able to treat these effects as perturbations to our familiar methodology rather than to plunge into an unwarranted formalism.

Behind all the activity related to flows of the boundary-layer type such as dominated this conference looms the specter of many flows which arise in practical applications and which have none of the features required for application of the boundary-layer approximation. Frequently, such flows are turbulent and are amenable only to experimental analysis. Even here our knowledge of turbulence should prove useful with respect to development of the appropriate scaling and similarity laws.

THE ROLE OF THE COMPUTER IN TURBULENCE STUDIES

One of the pregnant questions which arose in the discussion during the conference concerns the role of the high-speed computer in turbulence studies of interest to engineers and engineering scientists. There are already underway at various centers (e.g., ref. 10) studies of elementary flows – for example, flow in a cavity – in which the time-dependent Navier-Stokes equations are solved with random initial conditions. After sufficient computing time, statistics of the flow, such as the various correlations of interest, can be computed and the turbulence characteristics determined.

In this conference, J. R. Herring of the National Center for Atmospheric Research (paper no. 3) discussed several current attempts to utilize the high-speed computer in turbulence research. For a given Reynolds number, such methods attempt to reduce the computing time and increase the accuracy from that required by a brute-force attack on the Navier-Stokes equations. The rapid expansion of the power and speed of the digital computer and the steadily decreasing cost per operation raise the prospect that in the foreseeable future such techniques can be employed on problems of interest to engineers, e.g., turbulent shear flows of great complexity. It is clear from the discussion of this prospect (see the remarks of J. R. Herring, D. R. Chapman, D. B. Spalding, and S. C. Lee in the Open Forum) that there is no unanimity on this matter. It does seem certain that these new methods will continue to be developed as the capacity of digital computers grows and that certain idealized numerical experiments will be carried out with them. The results thereof may be analogous to the fundamental, high-quality experiments which presently play such a central role in the development of turbulence theories and methods. What is not clear is the impact of these developments on the engineering methods which evolve from those presented at this conference.

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DISCUSSION

R. B. Edelman: As a member of the industrial community, I would like to add some comments to help put certain aspects of the proceedings into perspective. Some of us have some very relevant practical problems to cope with, and, each day, these are becoming very much more complicated. We are the first to realize and appreciate the need for a better understanding of turbulence. Although this need is apparent, we cannot lose sight of the realities of complex models which would serve to discourage potential users from applying them, simply because the theoretical model developers have not concerned themselves with aiding in how these models should be applied. For example, in classes of problems which may require the higher order methods, the specification of initial conditions becomes more demanding. This must be the concern of both the theoretician as well as the user. If this isn't the case, then I think that we are both going to lose. Secondly, although this conference has focused on the free shear layer, and specifically the fluid mechanics aspects of the mixing process, there are numerous problems of practical relevance which even though fundamentally more complicated are in current need of solution. Specifically, I am referring to problems involving mixing inside ducts (combustion chamber problems, for example, which embody a wide variety of applications), turbojet engines, rocket engines, and furnaces. Then one must include the problems of kinetic processes, multiphase flows. There has been no mention of these aspects which are not problems that can wait until the simpler yet very instructive free-shear-layer unbounded flows are fully understood before we move on and tackle these more practical aspects. I refer to them as more practical, but they are simply problems that are in current need of a solution. What I would like to see is how these current systems of equations work now. This will help to determine the paths that should be followed to provide current stopgaps and to provide some direction for continued research. I think we should do this concurrently while we are looking at the more simple flows where it has been made quite clear, during the past 2 days, that there are many things that we don't understand. If we don't do this, and we try to work in series, instead of in parallel, I think that some valuable information and insight will just not be available on a timely basis.

D. M Bushnell: I have one very simple comment: I have been playing for a number of years in the sandbox of very high Mach number turbulent boundary layers. If one looks at the equations, there are some very interesting p' and ρ' terms, the type of terms that at Mach numbers less than 5 are usually dropped but now should be included. We have obtained data at very high Mach numbers where we have density changes across these boundary layers on the order of 100. When we look at these data and try to compute them, I for one have been disappointed because when we put in low Reynolds number effects, we can compute them. We have no right being able to compute them. At this conference, at long last, it looks as if the free shear layer is a flow which may exhibit some Mach number effects. Finally, we have a flow where we can start seeing if the p' and ρ' terms are important. I hope that this is the case. I hope we aren't just missing something in the experiments, such as secondary flows in the apparatus, which are tending to change the entrainment rate. Several years ago I heard Donaldson cite the example of an open jet in a hangar, and when the hangar door was opened, the entrainment rate changed. This sort of a thing bothers me. I just hope that we have a nice juicy problem here, which will tax us, rather than just something that is going to eventually degenerate into some very simple answer in terms of some inviscid effects, or some quirk in the experimental data.

J. Laufer: The question, of course, has concerned us for a number of years: In what way do compressibility effects alter a turbulent flow?

Purely from a formalistic approach, one may get a first indication for an answer if one follows Lagerstrom's arguments used on laminar flows[1] and applies them to the mean equations of motion describing turbulent flows. If the equations are put into a proper non-dimensional form, one can see right away that the Mach number appears explicitly in the equation for the mean static enthalpy as the coefficient $(\gamma - 1)M^2$ in the work term $\overline{u_i \frac{\partial p}{\partial x_i}}$ and as $\frac{M^2}{Re}$ in the dissipation term. For high Reynolds number flows of particular significance is the work term containing the pressure fluctuations. This may be seen further by considering the equations for the turbulent kinetic energy and for the mean static enthalpy:

$$\frac{\partial}{\partial x_j}\left(\overline{\frac{1}{2}\rho u_i'u_i'\tilde{u}_j}\right) = -\overline{\rho u_i'u_j'}\frac{\partial \tilde{u}_i}{\partial x_j} \boxed{-\overline{u_j'\frac{\partial p}{\partial x_i}}} + \frac{\partial}{\partial x_j}\left[\overline{u_i'\left(\tau_{ij} - \frac{1}{2}\rho u_i'u_j'\right)}\right] \boxed{-\overline{\tau_{ij}\frac{\partial u_i'}{\partial x_j}}}$$

$$\frac{\partial}{\partial x_j}\left(\bar{\rho}\tilde{h}\tilde{u}_j\right) = \tilde{u}_j\frac{\partial \bar{p}}{\partial x_j} \boxed{+\overline{u_j'\frac{\partial p}{\partial x_i}}} + \frac{\partial}{\partial x_j}\left(\bar{q}_j - \overline{\rho h' u_j'}\right) \boxed{+\overline{\tau_{ij}\frac{\partial u_i'}{\partial x_j}}} + \overline{\tau_{ij}}\frac{\partial \tilde{u}_i}{\partial x_j}$$

The terms that explicitly show an energy interchange between the mean and turbulent flow (terms that occur in both equations but with opposite signs) are shown in the dashed boxes. Now since the interaction due to viscosity usually occurs at high frequencies where the energy level is low, it is primarily the term containing the pressure that indicates the important new sources (or sinks) for the production of turbulent energy in addition to the well-known production term involving the Reynolds stresses.

[1] Lagerstrom, P. A.: Laminar Flow Theory. Theory of Laminar Flows, F. K. Moore, ed., Princeton Univ. Press, 1964, pp. 20-285.

Let us now try to speculate about the physical process through which such an interchange might come about. The term in question can be rewritten as follows:

$$\overline{u_i' \frac{\partial p}{\partial x_i}} = \frac{\partial}{\partial x_i} \overline{u_i' p} - \overline{p \frac{\partial u_i'}{\partial x_i}}$$

where the first term on the right-hand side of the equation represents a spatial gradient transport of energy and our attention should be concentrated primarily on the second term that represents interchange between internal and kinetic energy. In incompressible flows this term is usually referred to as the "tendency toward isotropy" term since it expresses the action of the pressure due to which the u component of the turbulent kinetic energy (generated by the $\overline{\rho u'v'}$ stress) is partially transformed into the v and w energy components making the net value of this term vanish; that is,

$$-\overline{p \frac{\partial u'}{\partial x}} = \overline{p \frac{\partial v'}{\partial y}} + \overline{p \frac{\partial w'}{\partial z}}$$

It is conjectured that in a compressible flow such a balance does not take place. Using a somewhat descriptive rather than exact terminology, one may say the flow is stiffer and more resistive to changes in the direction of large Mach number gradients; consequently, less energy is transferred into the v component. This fact has then the important consequence that the Reynolds stress $\overline{\rho u'v'}$ becomes smaller and, therefore, the turbulent transport process becomes less efficient. Furthermore, the nonzero value of the dilatation $\frac{\partial u_i'}{\partial x_i}$ brings about changes in the density. These density changes can be seen in a most dramatic fashion on a holograph picture of the outer edge of a turbulent jet. It is known that at the outer edge the occasionally outward moving (with v velocity) "turbulent bumps" interact with the ambient field producing pressure fluctuations. At low speeds these fluctuations are clearly incompressible. As the jet velocity is increased, the spreading angle decreases and clearly distinguishable density variations appear around the turbulent bumps. It is conjectured that these density fluctuations arise due to the dilatation effect of the compressibility which inhibits motion in the radial direction.

I have to emphasize that the above discussion is quite speculative; it is given mainly with the hope that it will stimulate more thorough studies on this question.

<u>M. V. Morkovin</u>: I would like to thank Dr. Laufer. Obviously, I asked him to comment on the modeling of the pressure-velocity correlation terms, especially at higher Mach numbers. Remember, it is with respect to the spreading that all of our methods were having

difficulties. The high Mach number spreading was really the one thing that was most off, and the possibility that we are not modeling right in that direction is why we are talking about it now.

J. A. Schetz: I would like to make two comments – one technical and one a formalism about the evaluation. If we are going to tabulate the comparisons of all the predictors with the data, I think it would be worthwhile to make some unified scoreboard of other features of the method. After all, their relative accuracy of prediction is not the only evaluation that a potential user might make. Other factors might be types of problems that can't be treated (some of the eddy-viscosity models will obviously fall into this category) and typical time for computation. There are a lot of other measures of the usefulness of procedure which perhaps should be tabulated in this publication.

H. H. Korst: Is it your feeling that the questionnaire that has been passed out to the predictors at this conference may have been incomplete or composed in haste and should it be revised and made more complete, or do you have any other suggestions?

J. A. Schetz: Well, I think that most people put down approximate times. Perhaps we should be asked how long it took us to do a couple of representative cases. I mean, most people guessed at the time it took them or estimated the time.

D. M. Bushnell: We will be corresponding with the authors anyway and we will send along another sheet which they can fill out or, if they wish, we will use the original.

J. A. Schetz: I would like to mention again that we have measured rather substantial static pressure gradients in situations which are perhaps surprising, in line with Dennis Bushnell's comments. We have measured large static pressure gradients in the wake behind a circular cylinder, for example, up to 25 to 30 percent of the free-stream static pressure. We have also done the same thing at supersonic speeds where it is perhaps not so surprising. This is an area which requires some attention, even if we are using the simplest type of approach, and where I think further experiments and some analytical work is needed.

M. V. Morkovin: Where does this static pressure gradient come from? I mean, is it part of the model? We all know that on the backside of a cylinder we have low pressure. We have the drag, right?

J. A. Schetz: We have observed that this static-pressure variation persists for surprisingly long distances downstream (20 or 30 diameters). If in the same wind tunnel you take a cylinder of a certain size and measure the normal pressure gradient and then you take a sphere of exactly the same size and put it in the same wind tunnel, you will find a very much smaller static pressure gradient. So it is not as simple as that.

H. H. Korst: Yes, but you address yourself to the wake problem in a more general sens than just mixing when you say that the wake problem may involve nonconstant pressure mixing.

M. V. Morkovin: I'm trying to pursue this thing. Is this divorced from entrainment? Do you have entrainment without such a gradient? How does entrainment come about? Isn't a static-pressure field concomitant with the entrainment?

J. A. Schetz: My only comment is that you can have entrainment without such a big static pressure gradient. We get entrainment in a jet. You get entrainment in the wake behind a sphere and we don't measure anything like these large pressure gradients. We found out that this is not a new discovery on our part. If you examine Schlichting's thesis (1930), you will find he measured similar large static pressure gradients. It may have something to do with the rate of entrainment. It is surprising that when you go from a cylinder to a sphere you find such a big difference.

M. V. Morkovin: As you know, in laminar boundary layers and in other laminar flows (jets or otherwise), you can proceed by iteration and compute your displacement thickness to get a pressure gradient. I think Van Dyke[2] has shown, at least for laminar boundary layers, that even in complex situations like laminar entry into a duct (which is much more tricky because the displacement accelerates the flow), the simple next approximation $\left(\text{the } 1\frac{1}{2} \text{ approximation}\right)$ is a relatively good one. When I was referring to the ellipticity, I was not being party to the predictors, I was still thinking that perhaps with the marching techniques there is a possibility of putting a $1\frac{1}{2}$ type of step into the calculation.

J. Ito: I guess Dennis Bushnell more or less intimidated me right at the very beginning in that he said that this conference will not account for mixing with static pressure gradients and it will not account for mixing with chemical reaction effects. Now in industry, as has been pointed out before, we can't necessarily solve ideal problems or classical problems. We have to solve problems as they occur and we do have a model which I have mentioned to Dennis Bushnell and to Dr. Birch, which does take these factors into account and is based on physical insight. It's not a model which is based on mathematical derivations. Now the reason I didn't speak up earlier in this conference is that I didn't want to disrupt the tone of this conference by bringing up additional factors which were beyond the scope of this present conference. If anyone is interested in some of the details of this, I would be willing to discuss it with them privately.

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[2]Van Dyke, Milton: A Survey of Higher-Order Boundary-Layer Theory. SUDAAR No. 326 (Contract No. AF 49(638)-1274), Dep. Aeronaut. & Astronaut., Stanford Univ., Sept. 1967.

H. H. Korst: I think that was a self-imposed restriction of the conference. I am quite sure that not only in industry but also at other places, of maybe less practicality, these problems have found considerable interest and are worked upon and you may find here interested partners to discuss it right away.

S. F. Birch: I would like to invite comments from some of the predictors on the modeling of length scale. In many of the papers details of the other terms and constants have been elaborated upon considerably. The question I am asking is as follows: In these complex flows where initially you have boundary layers with different length scales for different regions of the flow, where the length scales are in some cases defined differently and where differences in length scales that have cropped up between planar and axisymmetric flows, do you feel that these differences represent real physical differences between these regions of the flow? Also, how do you feel they should be dealt with?

D. B. Spalding: I would just like to say that it is our opinion that if one is interested in flows of any generality at all, one can forget about formally describing the length scale. The only practical way forward (the only economical way forward) is to deduce the length scale by solving an appropriate differential equation. We need to think of only simple separated flows like the flow downstream of a sudden enlargement in a pipe and we immediately see that the length scale just downstream of the enlargement and in the neighborhood of it must be similar to that in a mixing layer, because there is a mixing layer there. Far downstream the length-scale distribution of a pipe flow must be approached. Then, in the eddy region, there is some kind of length scale which perhaps close to the wall is proportional to the distance. You can see some of the limits but you can't at all tell how to propose the length-scale distribution. I think it is not worthwhile. If you can solve any differential equations at all, you can solve those two extra ones – one for the energy and the other for an equation which will lead to the length scale. I would argue that's what all engineers ought to do.

D. M. Bushnell: I would like to ask the Imperial college people if they have ever presented the results of their length-scale calculations. I've seen quite a few predictions of mean profiles. I've never seen plots of your computed length-scale developments in various flows. It might be of some interest to see just what these things are doing.

D. B. Spalding: I cannot be absolutely certain about the papers, but I can recall length-scale distribution for that sudden enlargement flow and also for certain film cooling flows, film cooling downstream of a slot. Certainly we have presented there a length-scale distribution in the form of profiles.

D. M. Bushnell: Are these the length scales which you have computed from your $k\epsilon1$ and $k\epsilon2$ models?

D. B. Spalding: Correct.

M. L. Finson: I agree with Professor Spalding's points about the importance of solving for the length scale in many situations. I gave a paper[3] at the recent AIAA meeting, which Stan Birch alluded to briefly, having to do with the near wake behind hypersonic bodies with turbulent boundary layers. In that case, the near-wake turbulence is apparently residual boundary-layer turbulence and can have a scale size that will be off by an order of magnitude from a normal wake scale size. This can have a very large effect on the development of the wake. When you have scale sizes that are far from similarity, things like dissipation rates will be far from similarity, and it can take a very long time to recover.

H. H. Korst: I have one observation which I would like to share. While the appearance of similarity solutions was recognized and observed by many of the predictors, I was wondering why no reference was made to the levels of modified Reynolds numbers involving the virtual kinematic viscosity as published many years ago by Schlichting[4] for two-dimensional and axisymmetric wakes which could be compared with the turbulent Reynolds number defined in Dr. Peters' paper. Is there anyone who would like to comment on that?

M. L. Finson: I would like to amplify on your question because I wondered many of the same things. For instance, Professor Spalding, in analyzing the wake flows with the various models, showed that as one went downstream the lower level models diverged from the data and from the higher order models. I found this rather surprising. I would have thought that the one thing any model could reproduce would be similarity; the asymptotic self-preserving behavior. I would think that would be the first thing one would check with the models to make sure that you get the right asymptotic solution, presuming, of course, that the Reynolds number is high enough that you do have that behavior. Perhaps those particular cases did not go far enough downstream, but I was shocked by that.

D. B. Spalding: The first thing that we did do when we began all this work, long before the conference, was to look at those self-similar flows. You saw one slide, I showed it, it is one of Mr. Rodi's slides, for four or five different self-similar layers and I quite agree, one must at least be able to make predictions for self-similar layers. This is why I argued that the $k\epsilon 2$ is the one which we have to prefer, because only it can handle those well-known plane and axisymmetrical wake flows which appear in Schlichting.[4] That is why we must have a model of that kind. Now in this conference we weren't asked to compare our predictions with those data we were asked to compare them with developing flows and you saw what the results were. I entirely agree with what the speaker has just said, and we've done it.

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[3] Finson, Michael L.: Hypersonic Wake Aerodynamics at High Reynolds Numbers. AIAA Paper No. 72-701, June 1972.

[4] Schlichting, Hermann (J. Kestin, transl.): Boundary-Layer Theory. Sixth ed., McGraw-Hill Book Co., Inc., 1968, ch. 24.

H. H. Korst: I was particularly interested in whether Dr. Peters would be able to correlate his terminal and universal Reynolds number to such a value. Did you give some thought to that?

C. E. Peters: Are you referring to the value of the fully developed terminal Reynolds number R_T in a wake?

H. H. Korst: That is right.

C. E. Peters: The one thing we commented on in the written version of the paper is that the axisymmetric incompressible wake, the Chevray wake, was a situation where the length scale of the experiment, which was far from fully developed, did not go to the ultimate $x^{-2/3}$ decay or whatever is proper; it varies as x^{-1} all the way. Personally, I have never seen any incompressible axisymmetrical wake data behind a streamlined body which go to the $x^{-2/3}$ decay. Perhaps I am not aware of it. There may be a question of whether or not self-preservation exists in that case, within practical distances.

H. H. Korst: Steve Kline has mentioned and given due weight to this. The fact that we do not reach similarity does not mean that we cannot anticipate its approach as we plot consecutive data and see an asymptotic narrowing down of the gap.

C. E. Peters: In the particular flow that I mentioned before, we maintained an x^{-1} center-line velocity distribution (that is, the defect distribution) to x/D of 200 or so, far beyond where the experimental data stopped. We commented on this in the written paper.

M. V. Morkovin: I would like to have some advice on my own philosophy. On the panel, we strongly came out for motherhood; that is, there was a need for simple solutions as well as a need for more complex solutions. Maybe that wasn't right if I understand what the industrial members are saying. For one thing, Peters says his simple solution is really consuming an awful lot of time too and that no solution is really simple. If I understood Professor Spalding, the extra differential equation isn't really that much more difficult. We have made that statement partly because we felt that the computer technology is sucking us into a method pollution just like technology is sucking us into general pollution. Perhaps, that's wrong and we should use the most up-to-date method and recommend it to everybody; I don't know. I'm not in a position to judge and I'd be very happy to be corrected in terms of a yearning for the simple old days.

C. E. Peters: May I clarify one point about our computation times. Our program is at least an order of magnitude slower than it needs to be for these particular flows because, as I mentioned during my presentation, it is full of extraneous information going much beyond the requirements for the flows considered in this conference; therefore, I think it is faulty to say that it is extremely slow. The point is that it is still a monumental computation task even as an integral method. I mean it is not something one can do on the back of an envelope as classical integral methods have been, unless one has an awfully big envelope and an awful lot of time.

M. V. Morkovin: It has been said that the Stanford conference did one piece of damage; that is, it pushed in the direction of the proliferation of the extra complex things. Is that a correct criticism? Is the idea to use the best tool that you have because it is not really that complicated?

S. J. Kline: It seems to me that what Dr. Morkovin is saying is that we no longer need motherhood but we need population control, for one thing. With regard to the Stanford conference,[5] I remember the recommendations were that we need some simple methods. We said, in fact, that one couldn't differentiate between the integral methods and the differential methods, as some of each were good. But since then, in looking at the actual figures, critical readers of the proceedings have observed that the center of gravity of the population of the differential methods is certainly at a better place than the center of gravity of the integral methods. If you are going to go to a computer, as Peters has said, then certainly you are going to use more sophisticated methods. And in another comment, I tend to agree with Professor Spalding. It seems to me that what progress has been made toward improved calculations of turbulent boundary layers and of the class of problems we have been discussing here – I think there is some progress – is due more to the computer than to any other single factor in the last 20 years.

C. duP. Donaldson: I would like to make a suggestion in regard to when you use more sophisticated methods and when you don't. I found it is enormously instructive to do what I did or do what Professor Spalding did. If you wish to use a simpler method, you don't use all these additional equations. But really the essence of a lot of the physics, but not all of the physics, of eddy-viscosity methods comes from looking at the super equilibrium form of those equations. In particular, what is the effect of heat release by chemical reactions on eddy viscosity? You can get that effect – the first-order effect – by taking an equilibrium nondiffusive limit. This is a very helpful thing to do and it also helps tell you, when you look at the problem, whether that's the kind of problem you can do that way with any degree of confidence. If you don't feel you can, then you will have to use one of these more elaborate methods.

C. E. Peters: I would like to add one bit of clarification about integral methods. Classically, integral methods have been, I think, of two types, and we should differentiate between them, not mathematically but grammatically. The classic integral method is one where you may not input information on the intergrand but you input information on the integral quantities from empirical information and this does not give you back a detailed

[5]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.

flow-field description from the integral method. That's one approach. What we have done is a bit different: We have input information on the integrand – that is, the shape of things – and this means that we get back not just the distribution of net quantities (the solution variables, wall pressure in the case of our ducted flow) but also spatial distributions. We get back just as much information qualitatively as we would get from a differential method. We get spatial distributions of kinetic energy, of shear stress, of velocity, and so forth. So the output is not quite the same as in some simpler integral methods.

H. H. Korst: The different complexity of the problems on one side and in methods of solution on the other side can be illustrated rather simply. As topics for this conference, we have excluded certain problem types entirely by definition. That doesn't mean that we as individuals haven't been concerned with such practical problems as hydrogen burning in wakes (and have found solutions experimentally verified by simple flame sheet methods) or that we haven't utilized such things as virtual origin shift and equivalent bleed to attack problems of base bleed in rather complicated configurations. We have here concentrated on the better founded ways – not seeking simple solutions like saying what do I care for σ if I'm only interested in the dividing streamline because the dividing streamline without any bleed in its asymptotic behavior doesn't care about σ. It is just a simple similarity solution which does not require any empirical information. Therefore, I think we have gone quite a bit into more details. We always find that some people will continue to make contributions to a better physical understanding and other people, such as in industry, will be more or less forced to utilize whatever simple and maybe nonsophisticated methods that have become available and can be readily compared to and applied to problems expecting a certain degree of ball-park type accuracy for their solutions.

S. C. Lee: Any method selection should be based on what we want to calculate. If we are interested in design parameters such as a drag force or aerodynamic heating, I think the so-called integral methods would be quite sufficient for giving us this information. However, if we are interested in the fuel-air mixing for a hypersonic jet, I think the integral method would not be sufficient to give you the mixing in proper details. If we are interested in problems of meteorology, and we would like to see how the air masses move, we have to go to more sophisticated methods like those Professor Spalding proposed – maybe going even further, like analyzing the diffusion terms as well as the production and dissipation terms.