Future Directions in Turbulence Research and the Role of Organized Motion

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1. Introduction

Before focusing on the role of organized motion I believe it is appropriate to respond to the overall premise of this meeting as set forth in the green flyer which advertises it. In this vein a general discussion follows which includes remarks about the other areas of the meeting. Then general discussion provides a background for remarks on the role of the organized motion. Since this paper will be read by persons who have not attended or been involved with the meeting, the first paragraph of this flyer is reproduced here.

"Turbulence is rent by factionalism. Traditional approaches in the field are under attack, and one hears intemperate statements against long time averaging, Reynolds decomposition, and so forth. Some of these are reminiscent of the Einstein-Heisenberg controversy over quantum mechanics, and smack of a mistrust of any statistical approach. Coherent structure people sound like *The Emperor's New Clothes* when they say that *all* turbulent flows consist primarily of coherent structures, in the face of visual evidence to the contrary. Dynamical systems theory people are sure that turbulence is chaos. Simulators have convinced many that we will be able to compute *anything* within a decade. Modeling is thus attacked as unnecessary or irrelevant because it starts with Reynolds averaging or ignores coherent structures. The card-carrying physicists dismiss everything that has been done on turbulence from Osborne Reynolds until the last decade. Cellular Automata were hailed on their appearance as the answer to a maidens prayer, so far as turbulence was concerned. It is no wonder that funding agencies are confused."

We have been brought to discuss issues implied by the premise that:

modern approaches to turbulence have been overpromoted by their proponents attracting inappropriate levels of funding at the expense of traditional mean flow turbulence modeling.

Intemperate claims by mean flow turbulence modelers seem not to have occurred! Where are these sentiments coming from? What is meant by "classical approaches" and "mean flow turbulence modeling"? With a little bit of digging I was eventually able to satisfy myself that I understood the motivation for the meeting and I will try to summarize those findings here. I will take "mean flow turbulence modeling" to mean, as it is implied in the green flyer, modeling based on the time-independent Reynolds equations without consideration of the time-dependent organized part of the motion. This definition is consistent with the approach of

Shih, Lumley and Janicka (1987) who use a carefully developed algebraic stress model to solve for the mean properties of a variable density mixing layer of the type studied by Rebollo (1973) and Konrad (1976). Modeling in this case is put forth, not just as a method for solving a practical problem, but as a means of obtaining a better physical understanding of the flow. In particular the model is used to argue that, at high Reynolds numbers and low Schmidt numbers, density is transported like a passive scalar and the turbulence behaves like constant density turbulence. It is pointed out that significant details of the mean flow are reproduced by the model even though there has been no attempt to explicitly include the large eddies. This is contrary to views such as those expressed by Broadwell and Dimotakis (1986) who warn that models which do not explicitly or implicitly include the organized structure cannot capture subtle features of the mean flow particularly the asymmetry in the entrainment ratio on the two sides of the layer. It clear from the concluding remarks of Shih et al. that their work is motivated by the desire to squelch such claims although no reference is given.

My position on this particular issue is that the entrainment ratio is a consequence of the basic geometrical asymmetry of the spatially developing mixing layer and the inviscid character of the large eddies which, on dimensional grounds, assures a linear growth rate for the layer. Any model that does a decent job of reproducing the mean velocity profile (ie, the correct spreading rate, maximum velocity gradient and position of the dividing streamline) should get the right entrainment ratio. The same holds for the maximum Reynolds shear stress. By the same token this is not an especially stringent test of a model. The ability to reproduce the mean density profile and mass fraction fluctuation profile is the most impressive feature of the model. It is interesting and perhaps significant in thinking about the current state of turbulence modeling to note that, except for the streamwise normal stress, the simplified model equations in Shih et al. agree more closely with the experimental data than the full equations. This is reminiscent of something which was noted in the 1980-81 AFOSR - Stanford Conference On Complex Turbulent Flows (Kline, Cantwell and Lilley 1981); model effectiveness did not necessarily increase with increasing model complexity. From the standpoint of modeling, the more significant issue raised by Broadwell and Dimotakis is the persistence of Reynolds number effects beyond the point where the mixing layer would be considered fully developed.

Additional background for the meeting can be found in Lumley (1981) and Lumley (1989). The sentiment expressed in Lumley (1981) is that organized structure is a characteristic of low Reynolds number turbulence and probably not important at high Reynolds numbers where conventional modeling approaches are satisfactory. Much of the paper is devoted to a discussion of statistical approaches, particularly the use of proper orthogonal decomposition as an unbiased method to extract organized structure from an inhomogeneous random field. The 1989 paper reiterates and updates these ideas with notions about the role of chaos. Reference is made to the review by Guckenheimer (1986) who discusses the difficulties inherent in treating dynamical systems characterized by a high dimensional attractor and the possible need for a probabilistic rather than a geometrical approach. In the context of quoting unnamed colleagues Lumley (1989) presents visual evidence of the absence of organized motion in the form of a schleiren photograph of a round jet with a turbulent exit flow. The difficulties inherent in the interpretation of such images are not discussed. Throughout both of these papers there is a high degree of ambivalence toward organized structure; it is important, it is not; it is practical, it is academic. Methods for finding organized structure are put forward while at the same time the significance of the organized motion is called into question.

II LOOKING FORWARD

The green flyer lays down a challenge to identify the role of organized motion in the technology of turbulence. By this I mean the ability to solve practical engineering problems using methods founded on fundamental physical principles. In order to make an honest stab at responding to this challenge in the context of a debate over the basic premise described above I think it is both necessary and useful to try to imagine where turbulence will be a decade or so from now. Only in this way can we come up with a realistic set of expectations for progress and a reasonable set of recommendations for future funding priorities. The goals are to further basic understanding of turbulent phenomena, promote the efficient development of methods for solving engineering problems and to insure the general intellectual health of the field.

It is true that the movement of ideas from research to application in turbulence has been agonizingly slow and this has strained the patience of funding agencies and the credibility of basic research which has told us much about the nature of turbulence without generating new methods for solving engineering problems in turbulence. This was one of the concluding remarks in my 1981 Annual Reviews article and it still holds true today. This is not to say that there hasn't been any progress or that we haven't made significant inroads into certain areas. It is just that new methods of general applicability have not emerged. Looking forward to the world of the 1990's which promises rapid growth in the technologies of computer science, new materials, biology and the like, the technology of turbulence appears to be a backwater, stunted by the lack of progress in using new approaches to create new engineering methods.

II.1. Computation

Will the technology of turbulence still be a backwater ten years from now in 1999? I think not. I think the rate of advancement of supercomputing and especially personal computing technology will have a very significant impact on the way we approach engineering problems in turbulence ten years from now. To a certain extent I am with the simulators, mentioned in the green flyer, in that I believe that when one looks at the broad spectrum of problems where turbulence plays a significant role; from flows in turbomachines and combustors to chemical and biological processing to atmospheric and oceanic flows; many will be solved either by direct numerical simulation or by large eddy simulation. Mean flow turbulence modeling will remain a useful tool for certain cases however the range of problems requiring this approach seems likely to decrease.

Computer scientists are talking seriously, and I am told by my colleagues reliably, about teraflop (10^{12} floating point operations/sec) computing capabilities by the latter part of the next decade. With a relatively modest investment (< 40 K) one can buy desktop computing capability exceeding 20 megaflops today. Sun Microsystems has committed itself to doubling the speed of its processors every year for the next five years. Intel recently announced a new 64 bit RISC microprocessor which runs at 150 million instructions per second. It is probably not unrealistic to envision desktop supercomputing capability approaching 2 - 10 gigaflops by the late 1990's. We are likely to see similar advancements in memory capacity and high volume, local storage capabilities as well. Stunning high resolution imaging of data will be a routine tool. Significant advances in CFD software availability will also occur as the interest in "desktop engineering" takes on commercial dimensions. Moderately capable packages are already available. What this means is that the capability we see concentrated at a few centers today will, in the relatively near future, be available to most researchers and industry engineers while the capability of those centers will be greatly enhanced. Today we have a direct simulation of an incompressible turbulent boundary layer at $R_\theta = 1410$ (Spalart 1988) with a barely discernible logarithmic layer. Ten years from now, with the increased power predicted above, we should expect to have simulations at $R_\theta > 10{,}000$ corresponding to the upper end of available experimental data. Direct numerical simulations with a well developed turbulent spectrum with widely separated scales will exist for an increasing number of elementary flows including cases with compressibility and chemical reactions and most workers will have the capability of storing the information from these flows locally as well as the ability to carry out moderate Reynolds number simulations of their own. It is important to note that to a large degree the field of turbulent simulation has been motivated and supported by experimental observations of organized structure.

However simulation alone is not enough; it generates too much information. Simulations need to be accompanied by systematic methods for flow interpretation which can reduce complex flow patterns to basic elements in order to identify, summarize and relate significant features of the data.

II.2. Experiment

In the next decade numerical simulation will be increasingly used as a laboratory tool. A researcher doing fundamental experiments may well have available data from a high Reynolds number direct simulation of the flow under study and will also have the capability of carrying out numerical simulations in the laboratory. The ability to couple experimental measurements of organized structure with visualization of simulation - generated data for difficult to measure variables will be of great use in identifying new effects and developing explanations of underlying physical mechanisms. The use of the computer in the laboratory for data acquisition <u>and</u> flow simulation will greatly broaden the prospects for basic experimental research which leads to significant technological applications as well as improved understanding. For example, direct simulations will be used to develop control strategies based on the sensitivity of the large eddies and their interactions to perturbations applied to the transition region. The same computer generated information could then be used as a command function to control the laboratory flow. Flow control based on modifications of the organized motion will probably occupy an increasingly important place in turbulence research. The researcher will be concerned with using basic understanding to generate methods for improving flow behavior in a coordinated program of computation and experiment. Flow control is recognized today as an important subject for basic research and there is no doubt that interest in this area will grow.

Broadly speaking current experimental turbulence research can be broken down into two categories. There are experiments directed at identifying and exploring new physical phenomena and there are experiments directed at confirming computations. Unfortunately support for experiments of discovery has been on the wane while support for experiments of code verification, particularly NASA support, has increased. True enough, measurements of this type serve in the development of improved standards of computation; an issue which will grow in significance with the democratization of computational power. However at present the coordination between experiments and computations is less than ideal often involving an experimenter who mistrusts the code and an analyst who doesn't understand the intricacies of measurement. I have been shocked at meetings to witness presentations of computational results with comparisons to experimental data with no acknowledgement of the source of the data. I consider this an abberation of the current dichotomy between computation and experiment. The sharp intellectual and social divisions which exist today will have no place in the laboratory of the future. Closely coordinated experiments and computations will be the hallmark of the best research and I believe that this will hold the key to advancements, not only in understanding, but also in the technology of turbulence. In an integrated research environment code verification will be a benchmark in any project. In this instance the emphasis will shift away from experiments where code verification is a primary objective and toward innovative high risk experiments directed at using basic understanding to develop methods for improving flow behavior.

The tensions which exist between computation and experiment arise partly from a lack of confidence. For various reasons, universities, including Stanford, do a poor job of instilling in their fluid mechanics PhD graduates the professional confidence required to move comfortably between theory, computation and experiment. There has been a considerable expansion of the curriculum to include CFD, hypersonics and other important new areas at the expense of an equally considerable drop in the time devoted to graduate level laboratory education. Associated with this has been a considerable amount of fragmentation in the way we teach fluid mechanics and the associated mathematics. Finally we have to deal with a natural human tendency to develop narrow interests in response to an increasingly diverse world. It is a little hard to predict where, in the future, advancements in mathematical and computational methods will be developed. The centers of computation which exist today will still exist with greatly enhanced capability in the late 1990's. However it seems to me that if necessity is the mother of invention then an increasing source of such methods will be the laboratory where computations and experiments are being carried out side by side by individuals versed in experimentation, mathematics and numerical analysis. Today's PhD graduate in fluid mechanics has a considerable amount of self education to do if he or she is to be effective in the fully integrated research environment of the late 1990's.

II.3. Modeling

There are many technologically significant problems where turbulence modeling can be used to generate useful engineering solutions and a broad range of examples can be found in the Proceedings of the AFOSR -Stanford 1980-81 Conference on Complex Turbulent Flows. Indeed mean flow turbulence modeling is presently the only method of solving certain practical problems and the problems of today can't wait for new approaches to gain practicality. There is a need today for improved turbulence models just as there was in 1981 (cf. the position paper by Bradshaw, Cantwell, Ferziger and Kline in Volume I of the 1980-81 proceedings). If this were 1981 with the future described above eighteen years off I would probably support the idea of putting more research money into improved mean flow turbulence modeling if only to resolve some of the issues raised during this conference concerning the mixing of numerical errors with modeling errors. Because of these problems we didn't get a very accurate picture of the real capabilities of turbulence models. But it is now 1989 and progress in simulation has moved rapidly forward. Has mean flow turbulence modeling made similar progress ? Can we expect that, with a large infusion of research funding, progress in mean flow turbulence modeling will outpace the progress in computation driven by advancements in computer hardware? I would put my money on the hardware.

In many circumstances modeling works; the difficulty I see is that when it works we usually don't know why and when it fails we don't have the basic physical understanding required to correct the problem. But will this remain so ? By the mid 1990's a design engineer needing to solve a turbulent flow may well have the capability of carrying out a direct or large eddy simulation of the flow. Even in large scale engineering projects involving high Reynolds numbers and complex geometries it seems likely that some large eddy simulations will be computed for at least a few cases. One could envision using a limited number of simulations to develop a time-averaged, flow - specific turbulence model tailored to the flow in question. Lilley (1983) has pointed out the need for mean flow turbulence models which reflect the changing character of the large eddy structure from flow to flow. The single flow model would be used in cases where repetitive calculations are needed. When a regime is encountered where the turbulence model fails, simulations can be carried out to develop a physical understanding of why the failure occurred and to suggest adjustments for the model. If such a capability is developed then it reduces the need for developing universal mean flow turbulence models. It may well be that Reynolds averaged turbulence models of the late 1990's will not be significantly different from the models of today (which are not significantly different from the models of 1981). What will have changed dramatically will be our ability to understand why models work when they do and why they fail when they don't. Model failure is not such a disconcerting thing if one can determine where and why the failure occurred and has the tools available to correct it.

Mean flow turbulence models will probably always enjoy some degree of success in predicting simple as well as complex turbulent flows. Mean flow fields are smooth and, in the absence of shocks, continuously differentiable to a high order. Any carefully drawn, dimensionally-consistent, mean flow turbulence model which conserves mass, momentum and energy and is invariant under basic groups of translation, rotation and stretching should give reasonable answers in circumstances where it is applicable. Therefore the question of whether mean flow turbulence modeling is useful or not will probably never have a clear cut answer. There will always be situations where it is useful and others where it is not and so it will always remain as one among a number of tools for solving problems in turbulence. The real question from the standpoint of this meeting is whether the range of applications of mean flow turbulence modeling will grow or diminish in the future. If the advances described above come to pass it seems certain that simulation techniques which incorporate the organized structure will continue to encroach upon the domain of mean flow turbulence modeling. One exception may be in the area of supersonic boundary layers where there will be an increased interest in turbulence research across the board.

I think mean flow turbulence modeling will always be a tool for a few specialists whereas simulation is likely to have broader appeal. There are several reasons for this. The process of model construction is complex and lacking in physical - intuitive concepts for guiding the uninitiated; it is very hard to get into. When I study equations typical of mean flow turbulence models the first question that comes to my mind is: what physical picture can I form which will help me understand the various terms in these equations? Nothing comes. Direct simulation techniques are not for the fainthearted either but at least one has confidence that the basic equations one is trying to solve are correct. Large eddy simulations probably lie somewhere in between although significant improvements in sub-grid-scale models are needed. The main reason why turbulence models are under attack from some quarters today, why they divide the community so deeply and why they will probably never gain wide acceptance in the future is simply their sheer lack of a sound physical and theoretical foundation. The fact that they work in certain circumstances is not enough. When one looks at the breadth of problems which need to be solved and, as we shall do shortly, at the list of cases which likely cannot be handled it is hard to envision the widespread acceptance of mean flow turbulence models by engineers and scientists of the late 1990's especially when more attractive and powerful methods are available. The complexity and absence of a sound theory are fundamental drawbacks of mean flow turbulence modeling which should not be underestimated.

There are currently a number of efforts to include organized structure in models of turbulence. Closure schemes based on the use of a superposition of inviscid hairpin vortices have been developed by Perry (1987), Perry, Li and Marusic (1988) and Perry, Li, Henbest and Marusic (1988). The method has been used to model wakes, mixing layers and wall turbulence. In the case of wall turbulence the model has shown the connection between properties of the organized structure and classical eddy viscosity models. The recent review of Liu (1988) discusses models of coherent structure as nonlinear instability waves

II.4. Toward a Theory of Turbulence

By a "theory of turbulence" I mean a theory which tells us about the nature of solutions of the Navier-Stokes equations at high Reynolds number. I do not mean a theory which is disconnected from the Navier-Stokes equations although it may turn out that the equations will be viewed as imbedded in a more general form.

Chaos theory provides a current example in turbulence research where inadequate education impedes progress and understanding. If you were to ask a physicist working in chaos to discuss the state of our understanding of shear layer mixing, or an engineer working on shear layers to explain the notion of strange attractors it is likely that neither response would be particularly edifying. Circular-Couette flow, and Bernard cell experiments aside, the connection between chaos theory and the general problem of turbulent shear flow has not yet been made although I think it eventually will be. Lack of understanding in this area is one of the reasons why chaos theory remains largely a curiosity to the average engineer. The situation is not helped by the fact that chaos has found its way into the popular literature where the technical treatment of turbulence is often superficial and misleading. See for example the discussion of turbulence in the otherwise interesting book by Gleick (1987).

Yet here is a field which has a significant potential for improving our understanding of the nature of nonlinearity and for providing a setting within which unresolved issues can be precisely stated. If a theory of turbulence is developed in the next decade my guess is that the wellspring of this theory will be a conjunction of three current areas of active research; research concerned with the topology of unsteady flow solutions in coefficient space (chaos), research concerned with the topology of unsteady flow patterns in physical space (organized motion) and research concerned with mathematical and numerical techniques which can be used to relate the two. The recent work of Aubry, Holmes, Lumley and Stone (1988) is an effort in this direction. Such a theory, if it is developed, would tell us much about the nature of solutions of the Navier-Stokes equations as the Reynolds number goes to infinity; it would be known whether the solutions remain regular or whether singularities could develop; constants in the Kolmogorov theory and the law of the wall and their possible dependence on Reynolds number would be determined, the existence or nonexistence of asymptotic growth rates for elementary shear flows would be known and rates of growth would be predicted. In a number of areas, empirical knowledge would receive a theoretical underpinning.

We might ask what contribution would such a theory make to the technology of turbulence ? Would it be of direct use in the design of, say, a wing-body junction on an airplane ? My guess is that for large scale complex flows the existence of a theory of turbulence would probably not have a very large direct impact on engineering practice. The situation would be somewhat analogous to the situation today in quantum mechanics. We feel very comfortable about our understanding of solutions of the Schrodinger equation and for very simple atomic and molecular systems we can solve for energy levels and transition probabilities from first principles. But for complex systems one still has to resort to numerical analysis coupled with empirical models. The impact of the theory is that it provides a guide for building the models. Similarly I think the main contribution of a theory of turbulence would be to provide fundamental principles which would guide the development of models. It would greatly enhance our understanding of why models work and why they fail. The limitations of a given model would be known a priori instead of a posteriori.

III. THE ROLE OF ORGANIZED MOTION

A number of reviews related to organized motion in turbulence are available including Willmarth (1975), Roshko (1976), Cantwell (1981), Ho and Heurre (1984), and Liu (1988). In addition the role of organized motion in turbulence has been discussed by Hussain (1981) and Coles (1981) and someone wishing further information should consult these references. In the context of this meeting I have chosen to frame this paper not as a review but as both a response to the basic premise of the meeting and an attempt to articulate my views of where the field of turbulence is headed. The previous sections did this in general terms, touching slightly on all the areas of the meeting. I would now like to turn to the role of organized motion.

III.1. The Role of Organized Motion in Transport

Townsend (1956) emphasized the importance of the large eddies in controlling turbulent transport and recognized that the eddies ought to have a quasi-deterministic form. Over the past thirty years numerous studies have have revealed aspects of the structure of the organized motion in a wide variety of flows. The topology and range of scales of the organized part of the motion varies widely from flow to flow. For example in the plane mixing layer the most apparent part of the organized motion appears to be a double structure consisting of strongly interacting two-dimensional rollers which span the layer plus a relatively well organized streamwise superstructure of smaller scale associated with regions of high strain which occur between the rollers. In the turbulent boundary layer the presence of a wall leads to a structure which is strongly three-dimensional, highly intermittent and in general much more complex than in free shear flows. The most intense motions are at a scale which is small compared to the width of the boundary layer.

The wide variation in organized structure from flow to flow is not suprising in that one would expect the motion to be the saturated nonlinear result of a basic instability driven by the boundary conditions and forces which define the overall flow. This notion underlies the so-called principal of marginal stability used by Lessen (1978) to account for trends in the turbulent Reynolds numbers of simple free shear flows. In this approach the organized structure is viewed as the result of linear instability of the mean velocity profile. It is essentially a heuristic in that the velocity fluctuations of the organized motion are not small and the mean profile is rarely, if ever, realized instantaneously nor is the instantaneous profile slowly varying. Nevertheless it is a useful idea which accounts for differences in flow geometry and forcing and can be used to argue for the regeneration of the large eddies in fully developed turbulence. The method was used effectively by Marasli, Champagne and Wygnanski (1989) to explain features of a turbulent wake in terms of the interaction of basic instability modes derived from a linear stability analysis of the mean profile. Aspects of the turbulent structure of the wake were accounted for although growth rates of various modes did not match the experiments.

There are also wide variations in the energy and stress associated with various scales of the organized part of the motion. In general studies show that eddies at the largest scale of the flow only account for a modest fraction of the stress (Hussain 1981). The most significant role of these eddies is that they differentiate the flow into regions of strongly varying strain and rotation and thus provide the setting for the first stage of coupling to finer scales. In this respect they control the overall transport. The near wake of a circular cylinder is a case where the large eddies are extremely well defined and would be expected to provide a substantial fraction of the stress. The measurements of Cantwell and Coles (1983) show that the periodic part of the motion associated with the large eddies accounts for only about fifty percent of the Reynolds stress. Hussain (1981) has attributed this to jitter in the position of the organized structure. However the observation holds within the first few diameters of the wake where such effects are not important as evidenced by relatively small values of background turbulent kinetic energy in regions where high gradients in the periodic part of the motion occur. In a frame of reference moving with the eddies the flow field is apparent as a moving pattern of centers and saddles. The saddle points are found to be regions of high shear stress and high production of turbulent kinetic energy. When the correlation coefficient of the background turbulence is formed it is found to vary widely with values near the saddle as high as 0.5 and as low as 0.1 at the centers of the vortices. Measurements in the plane mixing layer (Hussain 1980 , 1981) also indicate a peak in the production of turbulent kinetic energy at the saddles between the two-dimensional rollers. The mechanism in each case appears to involve stretching and organizing of three-dimensional vorticity by the strain field of the saddle. The stretched vorticity is aligned with the diverging separatrix of the saddle (ie. the positive direction of strain) forming an array of counter rotating vortices similar to those observed by Breidenthal (1981) and modeled by Corcos and Lin (1984). There really are no experimental studies with enough detail or simulations at high enough Reynolds number to tell us the scale at which organized structure ceases in a typical fully developed shear flow. We cannot, at this point, say definitively what fraction of the mean Reynolds stress could be regarded as contributed by motions which, by some measure, could be considered ordered. It is probable that the answer to this question, as with so many questions in turbulence, varies from flow to flow.

III.2. The Description of Organized Structure

I think it is fair to say that the observations of organized structure over the past four decades have been the motivation for virtually all new approaches to the field which have occurred during this time. From early studies of the intermittent nature of turbulence by Corrsin (1943) and Townsend (1947), to numerical simulations, to modal decompositions of flow solutions, to the theory of chaos (which is in fact the theory of *order - in - chaos*); one way or another, all have been motivated by the notion that the large scale motion in turbulent shear flows is characterized by a certain degree of order. While there is general agreement that order exists, there is a huge variety of methods for defining that part of the motion which is ordered and the subject as a whole remains controversial.

Now that simulations of turbulent flows are available, a major problem is one of understanding a vast amount of information. Simulation can compute the flow but it cannot interprete the flow solution. Despite the success of simulation there is a feeling that a true understanding of turbulence still eludes us. The problem solving engineer of the late 1990's will be faced with the same difficulty. To an important degree the usefulness of simulation for problem solving and new understanding will rest on the ability to synthesize complex flow information. The key to this will be to find an appropriate framework for describing and summarizing the organized part of the motion. A satisfactory methodology for the interpretation of complex three-dimensional data must involve more than simply an improvement in the technology of displaying the data but requires a systematic method for reducing complex flow patterns to basic elements in order to summarize and draw relationships between significant features of the data. An important point to keep in mind in thinking about this issue is that there is a vast variety of problems in turbulence and a flow interpretation scheme which satisfies one set of needs may be quite unsatisfactory for another. In this instance there are good technical imperatives for a pluralistic approach. A second point is that the structure of turbulence can be described on a number of different levels ranging from the mean or ensemble averaged flow at the highest level to the instantaneous flow at the lowest level and methods of flow field interpretation are needed at every level.

Representations of organized structure tend to fall into three catagories which could be roughly described as statistical, phenomenological, and topological with a good deal of overlap between the three. Statistical approaches have played a very important role in the experimental determination of the organized structure of turbulence. The history of this subject is one where, at each stage of development, the amount of information derived has been limited by the available techniques of the day. The evolution of statistical knowledge has moved from spatial correlation information derived from a few thousand single or two point hot-wire velocity measurements to elaborate computer controlled conditional sampling experiments involving millions of measurements over a field. Antonia (1981) has given a comprehensive review of conditional sampling techniques and the various approaches used to identify organized motions experimentally. A recurring point is the difficulty of relating Eulerian and Lagrangian information. Hussain (1981) discusses analytical tools for decomposing the flow field, various methods for characterizing coherent structures and experimental techniques for finding them in boundary layers and free shear flows. Hussain (1981, 1986) defines a coherent structure as a turbulent fluid mass connected by a phase correlated vorticity. Beside the somewhat circular nature of this definition I am not comfortable with the idea of trying to assign any sort of strict nounal definition to what is essentially an adjectival concept; ie descriptive of that part of turbulence which is ordered. I think to tie down the concept of organized motion too closely would be to cause it to lose its usefullness. For this reason I dont like the phrase "coherent structures" because it predisposes us to the idea that the organized part of the turbulent motion consists of "things"; like soft mushy rotating billiard balls and tends to ignore the elliptic field nature of turbulence which I believe must be faced head on if we are to reach an improved understanding of the subject.

The availability of direct simulations has enabled conditional averaging techniques to be applied to complete three-dimensional flow fields. Moin (1984) used the technique of proper orthogonal decomposition, suggested by Lumley (1981) as an unambiguous way of identifying organized structure, to study a simulation of turbulent channel flow. Recently Adrian and Moin (1988) have developed a rapid estimation technique which enables simulations of homogeneous shear flow to be used to study average motions conditioned on the velocity vector and deformation tensor; a study which would be impossible without simulations. They were able to identify the topology of correlated flow events which contribute to the Reynolds shear stress.

Phenomenological approaches look directly at the flow field and attempt to use visual information to identify key features of the motion and to identify the role of these features in the generation of turbulent transport. Experimental studies of this sort have been limited by the experimental techniques at hand and hampered by the conflicts which arise in the attempt to relate the instantaneous velocity and vorticity fields to the results of flow visualization which usually involved the time integrated effect of the flow on a tracer. The ambiguities of Lagrangian visualization techniques are nicely illustrated by the far wake studies of Cimbala, Nagib and Roshko (1988). Shariff (1989) studied timelines in the flow about a vortex pair subjected to an oscillating strain field. Extremely complex patterns were produced even though the underlying velocity field was quite smooth and Shariff discusses the difficulties of relating Lagrangian and Eulerian turbulence. Simulations have helped to clarify some of these problems. The recent survey of near wall structure by Kline and Robinson (1988) is an attempt to reach a consensus on a variety of disparate pictures which currently exist in the literature and to begin piecing together a single, consistent physical model of the flow. This is another example of an undertaking which would not be possible without the kind of simulation data which is now available. Using the computations of Spalart (1988), Robinson, Kline and Spalart (1988) have studied graphical images of various quantities in the turbulent boundary layer including the pressure field, stress fields, velocity vector field and vorticity. A great deal of information is emerging from these studies which are still at a relatively early stage. The physics of the motion is found to be even more complex than was previously thought and it appears that a given visual observation can have more than one underlying mechanism (Kline, private communication).

Topological methods are useful in the description of fields. They can be used in the multidimensional space of coefficients of a modal decomposition of the equations of motion, where the object of study is the region of attraction to which the solution tends at large time, or they can be used in three-dimensional physical space where the object of study is the critical behavior of the unsteady flow field. The former is the setting for theories of chaos, the latter is the setting for a topological description of organized motion. Perry and Chong (1988) have recently reviewed the use of critical point analysis in the description of unsteady flow fields. They emphasize the fact that the method provides a wealth of topological language particularly well suited to the description of fluid - flow patterns. Perry and his co-workers (Perry and Lim 1978; Perry, Lim and Chong 1980; Perry and Chong 1987) have made extensive use of critical point theory to describe complex flow patterns in steady and unsteady three-dimensional flows. Topological methods have recently been used by Lewis, Cantwell, Vandsburger and Bowman (1988) to describe the kinematics of flame breakup in an unsteady diffusion flame. This method focuses on the problem of connecting vortex structures together to complete the flow field. However there are significant conceptual problems involved in interpreting the unsteady streamline patterns as they relate to entrainment since streamlines can move across pathlines and the pattern of streamlines depends on the frame of reference of the observer.

Certain time dependent flows can be reduced to a self-similar form including the class of flows referred to below as one-parameter shear flows. In this case the topology of the flow can be described in terms of particle paths in similarity coordinates. This procedure was used by Cantwell, Coles and Dimotakis (1978) to describe the self-similar flow in the plane of symmetry of a turbulent spot. Experimental data was collapsed onto (x/t, y/t) coordinates and the phase portrait of particle paths was used to determine the rate at which fluid was entrained into various regions on the centerline of the spot. Glezer (1981) used $\left(x / t^{1/4}, y / t^{1/4}\right)$ coordinates to freeze the large-scale structure of a turbulent vortex ring. In both of these cases, an assumption of Reynolds number invariance was invoked and non-self-similar motions following a viscous timescale were averaged out through the use of the large eddy timescale for assigning phase information to the velocity data. The entrainment diagrams generated by this procedure have the useful property that they are independent of the observer. Prospects for using this approach in modeling the organized structure of turbulent shear flows are discussed by Coles (1981) who proposes an eddy viscosity model for the propagation of the turbulent-non-turbulent interface with a diffusivity proportional to the background turbulent kinetic energy. Griffiths (1986) has used a drifting Stokes flow solution in similarity coordinates to describe entrainment by a buoyant thermal. The model is used to explain laboratory observations of the distortion of dyed fluid blobs at low intermediate and high values of the Rayleigh number. Critical Reynolds numbers in the starting process for a class of impulsive jets were determined by Cantwell (1981, 1987). A complete picture of the evolution of the flow with increasing Reynolds number was deduced just from considerations of boundary conditions, integrals of the motion and the invariance properties of the governing equations. It was found that all of the significant topological properties of the solution could be conveniently represented by trajectories of the critical points in the space of invariants of the local deformation tensor. This scheme of flow representation was first used by Cantwell (1979, 1981) to classify the topological properties of various turbulent shear flows. The method has several attractive features for concisely summarizing flow fields. In an incompressible flow the first invariant of the deformation tensor is zero and therefore the trajectories of the critical points are restricted by continuity to lie in the plane of the second and third invariants even though the flow field may be three-dimensional. Thus the complete topological history of a three-dimensional unsteady flow can be represented in a plane.

Recently Chong, Perry and Cantwell (1988) have described a generalized approach to the classification of elementary three-dimensional flow patterns in compressible and incompressible flow. Although the attention in this paper is on the topology of the velocity field as determined by its associated deformation tensor, the method can be applied to any smooth vector field and efforts are currently under way to determine the topology of the vorticity and pressure gradient vector fields in the compressible wake computations of Chen, Cantwell and Mansour (1989). The vorticity field is interesting because the first invariant of the vorticity deformation tensor is zero for both compressible and incompressible flow. The pressure gradient field is interesting because the deformation tensor of this field always has real eigenvalues with orthogonal eigenvectors.

III.3. The Effects of Reynolds Number

The dynamical significance of the mean flow is often ignored. Not only is the mean field an expression of the forces and boundary conditions which define the flow, but it exhibits the most important dynamical property of turbulence; Reynolds number invariance. This is the well known property that, once the Reynolds number is large enough for turbulence to occur, the overall properties of the flow away from walls are observed experimentally to be almost independent of the Reynolds number. Most models of turbulence begin with something equivalent to an assumption of Reynolds number invariance although it does not have a strong theoretical foundation. Probably the clearest way to think about this is to imagine that the kinematic viscosity of the fluid is varied while all forces and boundary conditions are held constant. For example in a high Reynolds number jet a factor of 10 decrease in the kinematic viscosity will have relatively little effect on the mean velocity and Reynolds stress fields. The velocity fluctuation levels scale with the characteristic velocity of the flow $U' \sim U_0$ and tend to be independent of ν. The main effect is that the spectral content of the velocity widens to contain more high frequency components. The reason this is dynamically significant is that it implies that the rate of dissipation of turbulent kinetic energy is also independent of viscosity. The dissipation is linearly proportional to ν, yet observed to be independent of ν when the Reynolds number is large.

The simplest flows to consider are one-parameter turbulent shear flows. These are flows governed by a single invariant of the motion or global parameter; a momentum flux in the case of jets, a velocity difference in the case of mixing layers, a buoyancy flux in the case of plumes, a drag per unit volume flux in the case of wakes, hydrodynamic impulse in the case of vortex rings, etc. (See Cantwell 1981 for a more detailed enumeration). Once the assumption of Reynolds number invariance is invoked and viscosity is removed from the problem the solution depends only on the global parameter. In this case the existence of a similarity solution for the ensemble - averaged flow is assured. If the flow is governed by more than one parameter with units that are incommensurable the symmetry of the problem is broken and a global similarity solution does not exist although there may be regions in the flow where local similarity holds.

There probably does not exist a real flow which is completely governed by only a single global parameter. Virtually all flows involve a variety of length and velocity scales related to the effects of transition, geometry of the apparatus, presence of free stream turbulence, noise, etc. Once the flow is fully developed to a point where Reynolds number invariance can be invoked, the global parameter of the motion dictates the power of time or space with which the overall velocity and length scales of the flow develop nevertheless the effects of local parameters of the problem can creep in through modifications of the rates of growth or decay. Although one-parameter flows are relatively simple they are an important class in that they form the basis for much of what we know about turbulence and a tremendous amount of research has been devoted to their study. Their geometrical simplicity helps to focus basic unsolved issues of turbulence. Historically they were the genesis of simple zero and one equation models of turbulence and with the input of a single empirically determined constant (a spreading rate or mean velocity decay rate constant) useful engineering solutions were produced.

However modeling shed no light on how this constant could be determined and it is still so today that we do not have a theory which will enable us to solve, from first principles, the simplest conceivable turbulent flow with the simplest conceivable boundary conditions: Why ? In a sense Reynolds number invariance is both a simplification, because in so far as the mean is concerned viscosity can be approximately ignored, and a source of great complexity because the role of viscosity is subtle and cannot be ignored completely. Viscosity plays a central role in the time evolution of the flow through instability. With the observations of organized structure and the recognition that the time-dependent motion needs to be included in models of the flow, mean flow turbulence models were replaced by other approaches. Vortex methods, for example, do a reasonable job of simulating the spreading rate of turbulent shear flows but the solutions depend on how the vortices are defined and on how the vorticity is introduced into the flow. Among all the possible solutions which the Euler equations might admit for a given flow geometry and a given source of vorticity, viscosity limits the possibilities in ways we are only beginning to understand.

III.3.a Transition

In recent years beginning with the work of Bradshaw (1966) we have come to appreciate the importance of the transition region in determining downstream flow behavior. The spectral content of the initial region and the relative phases of various modes have a very strong effect on the interactions of the developing large eddies in turbulent shear flows and thus on the way the mean flow develops, even though the initial amplitudes may be very small. The question has been raised as to whether the high Reynolds number growth rate of the plane mixing layer is unique. This issue has been studied carefully by Browand and Latigo (1979) who used a large facility to study a plane mixing layer with laminar and turbulent splitter plate boundary layers. The turbulent case was found to grow more slowly at first, due the time required for the turbulence to adjust from a boundary layer structure to that of a free shear layer, but eventually approached the same spreading rate as the laminar case. Recent measurements by Wygnanski and Weisbrot (1987) show that coherent forcing of the mixing layer can increase the distance required for the flow to approach the asymptotic spreading rate. Nonuniqueness in the similarity structure of fully developed wakes has been observed by Wygnanski, Champagne and Marasli (1986) who studied the far wakes of various shaped two-dimensional bodies which were designed to have the same drag. They found that, while the wakes they studied were self similar when normalized by their own velocity and length scale, the evolution of the characteristic velocities and lengths depended on the geometry of the wake generator. Different bodies generated different dimensionless wakes.

III.3.b Mixing and Combustion

Viscosity plays a particularly subtle role in the problem of scalar mixing. Beyond the usual velocity transition region lies a so-called mixing transition associated with the onset of three-dimensionality (Breidenthal 1981). A conceptual model of this three-dimensional motion has been described by Bernal and Roshko (1986) and a theoretical model has been developed by Corcos and Lin (1984). The detailed structure of three-dimensional disturbances upstream of the mixing layer have been shown by Lasheras Cho and Maxworthy (1986) to influence the subsequent development of streamwise vorticity. In the case of a chemical reaction, the dynamics of the reaction are most strongly coupled to the flow at scales where the scalar gradients and strain are the largest; scales where viscosity dominates. Chemically reacting flows push the limits of full simulations but a few studies in relatively simple geometries are beginning to appear (McMurtry, Jou, Riley and Metcalfe 1985, Jou and Riley 1987, Rutland and Ferziger 1989, Mahalingham, Cantwell and Ferziger 1989). A fast numerical method for computing large eddies in reacting flow fields is described by Oran, E. S. and Boris, J. P. 1987. This method relies on numerical dissipation to stabilize the computation of solutions of the Euler equations on a coarse grid. While the method is incapable of treating viscous effects except by analogy, basic features of the large eddies and their interactions are reproduced rather faithfully. Models of shear layer mixing with chemical reaction which implicitly include the organized structure have been developed by Broadwell and Breidenthal (1982), Dimotakis (1989) and Broadwell and Mungal (1988). These models account for the effects of Reynolds, Schmidt and Damkohler number on the overall reaction rate. These studies are motivated by the recognition that an understanding of the effects of viscosity, scalar diffusion and chemistry on combustion requires an understanding of how the flow responds to the straining and rotational motions induced by the large eddies. One of the important results of this experimental work which has implications for modeling is the observation that significant molecular diffusion effects persist to high Reynolds numbers at which the flow would ordinarily be regarded as Reynolds number independent.

III.3.c Jet Noise

The role of organized motion and the possible effects of viscosity comes up in connection with the problem of jet noise. Inviscid models of vortex ring dynamics produce a large variety of solutions and instability modes only a few of which are observed in experiments. In the case of the azimuthal instability of vortex rings for which an inviscid theory has been provided by Widnall and Tsai (1977) two modes have nearly identical growth rates (within two-tenths of a percent) but, apparently due to viscous effects, only one of them is experimentally observed (Shariff - private communication). Kambe (1986) has studied the head-on collision between two vortex rings. He points out the importance of viscous effects in the noise produced in the late stages of the collision when the cores come into contact and their radii increase rapidly. Experiments by Hussain (1983) with controlled excitation revealed that vortex pairing can be an important source of sound but it is pointed out that in the natural jet clean vortex pairing events are relatively rare and other sound generation mechanisms must be sought. Vortex structures in the near field of the jet undergo azimuthal breakdown starting at about two diameters from the exit and this led Bridges and Hussain (1987) to suggest that vortex filament cut-and-connect processes, which occur during vortex breakdown, are a more important sound generation mechanism than vortex pairing in practical turbulent jets. On the other hand Michalke (1983) has demonstrated that azimuthal coherence is necessary for sound production and only low-order azimuthal modes can radiate efficiently. Michalke notes that the sound field of a jet depends significantly on the axial and azimuthal source coherence and that the coherence length scales of the sound radiating turbulence increase when the turbulence is excited artificially. These studies suggest that phase relationships between the large eddies and coupling to finer scale motions through instabilities governed by viscosity may play a significant role in the generation of noise.

III.3.d Separated Flows

Mean flow turbulence models are often concerned with complex geometries where viscous effects of the type described above are likely to be particularly difficult to handle. Regions where separation occurs at low Reynolds number may diminish or disappear altogether at high Reynolds number. Should we therefore expect mean flow models to work better as the Reynolds number becomes very large ? Can we ever expect to be free of the effects of transition ? The classic example of flow past a circular cylinder illustrates well the persistence of transition to be expected in the flow about any smooth bluff shape. Even with fixed separation points, bluff body flows show the effects of transition to Reynolds numbers well beyond the point where we expect the flow to be fully developed (Roshko and Fizsdon 1969). Recently Schewe (1986) has studied jumps and hysteresis effects in the flow about a circular cylinder in the critical Reynolds number range 300,000 - 400,000. Sudden changes in flow structure triggered by small disturbances in the surface boundary layer can cause asymmetric lift with lift to drag ratios as large as two. Recently Williamson and Roshko (1988) have documented a whole range of resonances, jump phenomena and hysteresis in the vortex wake of a circular cylinder subjected to controlled oscillations. Similar complex phenomena were observed by Nakamura and Nakashima (1986) in the wakes of self excited bluff prisms. Although both sets of experiments were at relatively low Reynolds numbers it is not unreasonable to expect that aspects of the same phenomena will occur in the fully turbulent case. The general class of problems with large scale overall unsteadiness would appear to be one which is clearly outside the scope of mean flow turbulence modeling but amenable to simulation. The problem of determining the lift and drag of a bluff body for truly high Reynolds numbers ($Re>10^{10}$) will continue to be beyond the power of simulation, and for that matter experiment, for decades to come. The problem studied by Schewe may be just barely in range of simulation by the end of the next decade.

III.3.e Flows with buoyancy

Zeman (1981) reviews the status of turbulence modeling of planetary boundary layers and discusses the use of second-order closure schemes. The complexities of modeling buoyancy driven turbulence which can support countergradient transport and the lack of information on the contributions of buoyancy to the pressure-velocity terms in the Reynolds stress transport equations are discussed. Zeman points out the importance and difficulties of modeling molecular diffusion terms and similar comments on the modeling of these terms can be found in Shih et al (1987). The viscous terms involve correlations of fluctuating strain rates which receive their largest contribution from the fine scale motions. The hope is that these motions will exhibit universal character. Antonia, Anselmet and Chambers (1986) have studied the local isotropy of various fields in a heated plane jet at moderate Reynolds numbers and they review the general state of knowledge of local isotropy. They point out that available data indicates that mean square derivatives of velocity and temperature are anisotropic and suggest the need to include this in models.

Recent research in buoyancy driven flames has focused on the coupling between the flow field and the reaction field and facets of this issue have been studied by a number of investigators. The effect of pressure variation on the structure of a low speed, co-flowing jet diffusion flame was studied by Strawa and Cantwell (1989). This type of flame is subject to a classical flickering instability manifested as strong, self-excited, longitudinal oscillations driven by, but not solely dependent upon, buoyancy. The flame was found to be extremely sensitive to the frequency of velocity perturbations applied to the jet exit flow. When the frequency of excitation was close to the flickering frequency the flame was seen to break up into a sequence of turbulent flamelets which exhibited an extremely repeatable three-dimensional structure. In an effort to understand some of these effects in a simpler flow configuration with approximately the same density ratio, Subbarao (1987) carried out an extensive experimental study of a co-flowing jet of helium into air subject to self-excited oscillations similar to the flickering oscillations of the low speed flame. In a buoyancy dominated range of Richardson numbers above one the helium jet was also found to exhibit an extremely regular and repeatable structure over a wide range of scales at Reynolds numbers where the jet would ordinarily be considered turbulent. However the helium jet was very insensitive to perturbations of the jet exit flow. The conclusion drawn from these studies is that the downstream development of the flow is strongly dependent on the details of how buoyancy is released. In the flame buoyancy is produced in a spiky fashion in the flame sheet near the jet exit. This flame sheet is very sensitive to small fluctuations of the jet exit velocity. In the helium jet the buoyancy flux is produced across the entire jet exit and the flow is much less sensitive.

III.4 Flow Control

In the integrated research environment described in Section II the use of simulation in conjunction with experiments to accomplish improvements in flow behavior will be of increasing interest. In one of the earliest examples of flow control Roshko (1954) demonstrated that a splitter plate placed in the wake of a bluff body caused a significant increase in the base pressure and consequently a reduction in drag. The splitter plate interferes with the transverse flow in the near wake. The strength of the vortices is reduced and they are forced to form further downstream relieving the base of the body from being subjected to the very low pressures associated with unencumbered vortex formation. The basic idea of interfering with or modifying the organized structure of the flow to achieve a more desirable flow state forms the basis for a whole variety of experiments directed at flow control. A summary of recent work in this fast growing field may be found in Liepmann and Narasimha (1987). A recent review of turbulence control in wall-bounded flows is given by Bushnell and McGinley (1989). Most of the attention in these references is on open loop flow control in which there is no attempt to use feedback.

From the body of research which has been carried out thus far, most of which is devoted to the open loop response of flows to external forcing, it seems clear that the controllability of a flow is intimately connected to its stability. For this reason attention has been focused on the transitional region of the flow and its role in determining the downstream development. In the few cases where feedback and control has been attempted such as the work of Liepmann and Nosenchuck (1982) this has been the case. In their work feedback was used to suppress a pure harmonic in the linearly unstable region of a flat plate boundary layer. In a more complex situation involving say mixing in a free shear layer, one can conceive of using combinations of unstable modes to achieve a certain desired effect on overall flow behavior in the presence of external disturbances.

Research on flow control will increasingly involve coordinated experimental and computational efforts. In this approach experiments are used to search for and identify physical mechanisms which can then be examined in detail using simulations. The recent thesis work of Mittlemans (1989) is an effort to use flow simulation to control the wind tunnel response of a delta wing at high angles of attack. Experiments will need to access more realistic flow conditions which are not achievable in simulations. It is critical to be able to establish whether the mechanisms identified for control at, say, low Reynolds number still play a role at high Reynolds number and future research in active flow control has to be capable of addressing this question.

III.5 Modeling

In the last decade or so studies of organized motion have led to a recognition that viscous and molecular diffusion effects persist to higher Reynolds numbers than was previously thought and play an important role in determining the properties of fully developed turbulent flows. These effects are felt directly through the influence of viscosity on three-dimensional breakdown and scalar transport processes and indirectly through the influence on transition and the phase relationships between the developing large eddies which affect downstream flow development. These are the common threads which run through the examples discussed above. The list of flows known to be subject to viscous effects grows longer as our fundamental understanding of the nature of turbulence improves. It is likely to grow longer in the future as flow control becomes a central theme of turbulence research. How will mean flow turbulence models incorporate these effects ? Will they do it before the power of simulation overtakes them?

IV. WHAT IS NEEDED FOR THE FUTURE

IV.1. Pluralism

The field of turbulence is described in the green flyer as "rent by factionalism". But what some may term factionalism I would call pluralism. It is true that turbulence is being studied along a number of different lines of approach and in recent years there has been an influx of physicists to the field which has been traditionally dominated by engineers. Physicists bring a somewhat different point of view to bear; more inquiring of fundamental questions, less interested in practical applications, more critical of models which lack underlying theoretical justification. The problem of turbulence is presently being approached along a broad front and in my view this is very appropriate given the importance of the subject, the breadth of problems which need to be solved, and the current lack of basic understanding. It seems to me that so far, funding agencies have been fairly enlightened about their willingness to fund new approaches and this should continue. Support for research into cellular automata as a means of flow simulation is in this spirit. The computationally intensive nature of these calculations presents a significant challenge which needs to be overcome and it is probably too early to tell how this approach will contribute to the problem of turbulence simulation. It may gain in significance with the advent of massively parallel machines. In any case I believe a pluralistic approach is essential for the future progress of the field and that it will remain so into the 1990's and beyond. By this token continued funding should be available for mean flow turbulence modeling as long as it does not represent a significant shift away from more promising areas of research.

IV.2. The Democratization of Supercomputing

By the late 1990's many workers will have desktop computing capability comparable to the largest supercomputers of today. In order for such a vision to be realized, an immense capital investment will be needed by universities, industry and the funding agencies. Advances in technology have a way of forcing themselves on us and I expect that one way or another the funds will be found to achieve this but *real increases are needed in funding for turbulence research if the technology of turbulence is to develop as it should.* The merits of widespread supercomputing are described above; but there are challenges too. The number of workers in CFD today is very large and growing and complaints about the quality and reliability of what is being done are common. If the future described above is realized the problem will be completely out of hand unless some sort of standards exist. Reliable workers in the field routinely do this today; full simulations are checked to demonstrate consistency with the results of linear stability theory, global conservation laws are checked, etc. In the case of compressible simulations the acoustic transmission properties of the grid should be carefully documented. Anisotropy of the acoustic speed or wave reflections caused by the grid can feed erroneous perturbations into the flow throwing off calculations of viscous unstable flows. I am unaware of anyone who does this rigorously at present.

The issue of maintaining computational standards is an especially important and difficult one for mean flow turbulence modeling since the problem is exacerbated by the fact that as the constitutive equations grow in complexity the numerical schemes needed to solve them also become more complex and the ability to make numerical checks with established theories diminishes. This came up in the 1980-81 AFOSR-Stanford Conference on Complex Turbulent Flows (Kline, Cantwell, Lilley Vol III 1981) where numerical problems severely limited the ability to make meaningful comparisons. One of the challenges of the 1990's will be to develop a set of standards of computation which workers in the field will adhere to and which will insure that published results are reliable while not discouraging innovation.

By the end of the 1990's high Reynolds number direct simulations of a number of elementary flows will exist and the results of these simulations will need to be made available to researchers who will have the capability of storing computed flow solutions locally or running codes to generate additional data. This will have the beneficial effect of forcing error checks and uncertainty analyses to be carried out on the numerical data similar to checks of experimental data which are released for general use today.

Software which generates software for turbulence simulation will be needed to relieve the average researcher from having to re-invent the wheel. The software packages which accomplish this in the future will resemble highly evolved versions of typical scientific software packages available today. The main difference is that they will need to be designed to allow a great deal of user intervention to try out new ideas and approaches.

IV.3. Support for Integrated Research

The funding patterns of today don't particularly encourage the kind of closely coordinated experiments and simulations which I have described above simply because the capability for such research is only beginning to occur. As the power of flow simulation moves into the laboratory, increased funding will be required to support research efforts which involve fully integrated experimental and computational investigations of turbulence. There is a number of possible ways that turbulence research might benefit from this approach.

(1) Perhaps the greatest benefit of having flow simulation capability in the lab is the possibility for rapid interpretation of experimental observations which would be used to guide the next round of experiments. The position of the experimenter would be rather like that of an amatuer sport fisherman who is suddenly handed a sonar imaging system for finding schools of fish. There is likely to be a sort of positive feedback effect where better experiments lead to better simulations which lead to better experiments and so on. Rapid turn around between the experiment and the simulation is essential for this synergism to work effectively.

(2) In - the - lab - simulation will afford flexibility in the rapid display of difficult to measure variables to understand how they relate to observables of the flow. This will aid in the efficient and unambiguous interpretation of flow visualization data. Experimentation will be made more efficient by permitting the numerical study of a large number of flow conditions punctuated by a few well chosen measurements.

(3) Advanced methods of experimental flow control are likely to be highly dependent on simulations for the generation of command and feedback information for the flow. Ultimately the simulation will be part of the control system.

IV.4. Funding Priorities

What will the funding for turbulence research look like in the late 1990's? If the last decade is any indicator it is not likely that turbulence research will receive a significant increase in real dollars. The fact is that funding for turbulence research is too low and we have taken an awful beating in recent years; the funding agencies just got tired of hearing that the solution to the turbulence problem is just around the corner. Perhaps my description of the future will be passed off as just another call for patience. I hope not. I believe today we can see the future more clearly. We have all witnessed the amazing progress in computing. There has also been significant progress in analysis and in the development of numerical methods so that today we have fairly mature methods of direct numerical simulation in hand for a limited number of flows. It is not too much of a leap to suggest that flow simulation will become an engineering tool in the future.

Turbulence should not be regarded as just another engineering discipline. Progress in almost any technical endeavor involving movement at sea or in the air or involving industrial processing of a fluid is nearly always limited in one way or another by the problem of turbulence. We have not been effective at getting this message across to the funding agencies and as a result contract monitors in fluid mechanics have not been able to compete effectively with their counterparts in other disciplines. Part of the reason is that too many people are chasing too few dollars; the inevitable result of that situation is that congress and the funding agencies do not get unbiased advice. People working in different areas tend to disagree with one another in rather uninformed ways and funding agencies dealing with fixed budgets hear a cacophony of inconsistent voices from researchers with disparate points of view. In spite of this it not my impression that the funding agencies are confused. They are just forced to work with limited funds.

For the purposes of this meeting, our discussion of the future of turbulence should probably be predicated on the assumption that any significant increase in funding in one area will have to be at the expense of another. The real question is: *How should we view funds for basic research and what is the appropriate balance between short term and long term goals*? Should basic research money be used to fund the development of methods which don't advance our understanding but do advance our capability of solving practical problems even though the actual employment of such methods in engineering practice may never occur? Or should these funds be used to develop new fundamental knowledge of lasting value? At the present time the emphasis is on fundamentals and the field is advancing rather rapidly. Given the applications that this basic knowledge will have in the future this does not seem to be the time to move toward mean flow turbulence modeling if it is at the cost of reduced funding for research in simulation, modeling via simulation, studies of organized structure, or other areas of fundamental research.

I don't think we should regard current funding levels as acceptable. Increased funding is needed. As a community we need to think seriously about trying to develop a consensus on a reasonable set of priorities and to get our message across to Congress and the funding agencies in a unified way. Physicists have done this effectively for many years. Our job is more difficult in that the list of priorities must satisfy both scientific and engineering needs.

V. CLOSING REMARKS

In this position paper I have tried to express my view of where the field of turbulence is headed and to describe some of the forces which shape current research.

(1) Research on organized structure has led to an increased awareness of the importance of viscous effects which persist to high Reynolds number.

(2) Aspects of this work form the impetus for an increasing emphasis on flow control through the use of coherent forcing to modify the complex interactions of the large eddies.

(3) At the same time powerful simulation methods are appearing which have the potential for handling these complexities.

(4) There is rapid progress in the hardware needed to use simulation methods. Things are moving vertically toward greater computational power and horizontally toward increased availability.

The future of this field is just beginning to be realized and there are exciting prospects for combining theory, experimentation and simulation to rapidly advance the technology of turbulence. Meanwhile basic questions about the physical and theoretical foundations of mean flow turbulence models remain unanswered and the overall progress of modeling has been disappointing. Although there will continue to be a need for improved mean flow turbulence models, the combined effects of the forces which drive current turbulence research seem likely to reduce the range of problems treated by these models in the future.

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