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PROMISING DIRECTIONS FOR LIQUID TURBULENCE RESEARCH

A Panel Discussion Directed by Victor Goldschmidt, Purdue University

Panelists: J. H. Whitelaw, Imperial College
V. A. Sandborn, Colorado State University
Val Kibens, University of Michigan
R. E. Kaplan, University of Southern California

J. H. Whitelaw, Imperial College: We are supposed to say what we're doing and why we do it and what we would like to see done if we had more resources than we have. Well, I'm going to hedge on the question of what we are doing and take only two situations which we are working on. The two examples I want to speak about are: first, coaxial jet flow where we work with hot wires and second, the square duct, which I'll talk about tomorrow, where we are working with laser instrumentation. The coaxial jet is a comparatively simple flow which we examine because we want to look at techniques and because we want to improve our understanding of the basic features of turbulent flow. In that flow we measure the usual rms quantities, the shear stress, the spectrum, the probability functions of U, U_1, U_2, $\overline{uv}$, some auto-correlations and some filter correlations. In contrast to that, in the square duct flow, we would not try to measure all these things. We would try only to measure those quantities which would help us to improve the development of turbulence models and thereby allow us to extend the use of computational methods for the prediction of flows.

If I take these two examples, the one where we measure things because we want to improve our calculation capability, the ratio of the one to the other in the terms of various experiments is something like 1 to 5 or 1 to 6. That's an impression of what we are doing and why we are doing it.

I mentioned in passing that we're interested in developing calculation methods that we can use in real flow situations. For example, if we develop a calculation method for square-duct flow, we hope we could apply it to the flow down the core of a gas-cooled reactor. We want, in some of these flows, to measure the boundary conditions so that we can test calculation procedures: the boundary conditions might be, for instance, the three components of velocity, the three components of the fluctuating velocity or the kinetic energy and perhaps something which could give us a handle on dissipation as an initial condition, as a boundary condition.

What would we like to see done in the future? Well, I think the thing we would like to be able to do most is to make direct checks on turbulence models. We would like, for instance, to be able to measure dissipation rate, and we would like to be able to measure the fluctuating pressure correlations. We can't do these things, but we can make the checks, the indirect checks, which will allow us to validate the numerical procedures which we have. If we could persuade other people to do things, then I would like them to measure some of the quantities which will give us indirect checks, particularly in recirculating flows and more especially in three-dimensional recirculating flows. To generalize then, I would like to see less emphasis on repeating old experiments in simple flows and more emphasis on making new experiments in more complex flows. Mainly we're working on the important problem of turbulent incompressible boundary layer flows.

V. A. Sandborn, Colorado State University: We have been doing a study at Colorado State University in conjunction with one of my students, Dr. Bill Cliff, who works at NASA, Huntsville, on turbulence convective velocities. We are trying to look at some of the basic ideas to understand at least one aspect of the structure of turbulence, and in this context are trying to get some insight into the development of models of turbulence.

In looking at convective velocities a pattern of the turbulent production and diffusion for the boundary layer appears. If we take two sensors a small distance apart, we can make a space-time correlation. If we measure all frequencies, we obtain a specific correlation with a peak at a specific time delay. By filtering the signal at different frequencies for locations in the outer boundary layer, we find that the higher frequencies are traveling at a higher speed than the low frequencies. It is found that in the region of the apparent origin (close to the surface) of the fluctuations all frequencies travel at the speed of the local mean flow. Very close to the surface, the convective velocity is higher than the local mean flow, whereas, the convective velocity is slower than the local mean flow in the outer region. The convective velocities indicate how the turbulence is diffused through the layer.

V. Kibens, University of Michigan: I have been working in boundary layers and in wakes, looking at large scale structures which cannot be seen by the "naked hot-wire set". These structures exist in the far wake, in the boundary layer as well as near the object that originates the structures which eventually drift downstream in the wake. I'm trying to look at them using sampling schemes of various kinds, trying to see what is the nature of the repetitive, quasi-periodic structures that gives the characteristic flavor to a particular flow.

Recently I've had occasion to look at the edge of a low speed jet in connection with the following problem. We were trying to quiet the potential core of a jet with a six-foot diameter and a velocity of 200 fps in order to look at the acoustic far field generated by a model placed in the jet. The potential cone is contaminated by all kinds of unexpected noise. The conventional statement is that the potential cone ends 6 diameters downstream of the nozzle exit. This suggests a picture of an undisturbed conical volume which can be used as the working space. It turns out that isn't so. The formation of large scale vortex structures in the jet shear layer, as well as the presence of random motion are responsible for considerable centerline velocity fluctuation levels.

This is one of many instances where the large scale structure in a particular flow is interfering with our purpose. In all of these, the control of the organized structures is desired. In fact our entire concept of turbulence has shifted from working with something that is fairly homogeneous to emphasizing the interaction of discrete, organized flow structures. This was made possible through advances in instrumentation and data processing. For many engineering purposes we now tend to group turbulent flows in terms of how these large flow structures interact. We look for discrete events and analyze their features. I would suggest that useful experiments can be devised which shed light on the behavior of a particular flow through the device of looking for ways to control and manipulate these large structures. If you successfully interfere with the formation of particular eddies then you can get rid of the noise in an objectionable part of the spectrum. If you are interested in having a small aircraft land within 10 minutes after a 747 has landed at an airport you are looking for ways to interfere with discrete vortex structures. If you want to enhance mixing processes in a chemical process, you look for means to modify or control the behavior of flow structures.

A paper that I've enjoyed very much along this line is that by Crow and Champagne (J. Fluid Mech., 48, 547 (1971)), on how to excite a jet to enhance its tendencies for forming such structures. In my opinion one profitable direction in experimental turbulence research may be to attempt to control, to modify, to enhance, to reshape or somehow influence the large eddies, and that it may be profitable to try a classification of various flow geometries in terms of the kind of control possible.

R. E. Kaplan, University of Southern California: I worked out a list of what we have been doing and it seems extremely long and I really don't know if we do all these things. We've been working very heavily on jet flows, looking at the structure, the fluid dynamics and the noise fields. We are making the same kind of investigations for shear layers and noise fields generated from shear layers (for example, as you drive with the window open in the car). We're going to be doing more work in the future on flow generated noise. We have the facilities for these studies and we have many ideas on that topic.

A group at Southern California is continuing the work in turbulent boundary layers looking at the large scale structures characterized by the bursting phenomena that Prof. Kline talked about today and that Prof. Brodkey and many other people have been investigating. We're also looking at other problems associated with the turbulent boundary layers, one of which is the motion of the passive contaminant in the turbulent boundary layer. Similar work was done at Marseille several years ago by Fulachier, and we're doing a similar type of experiment to take another look at the outer and inner structures. I think we can get more detailed information than before. We're also going to start an effort on transition, and I don't know where this will lead us. We'd like to start with the transition process and move back into the turbulent boundary layer. We've been in the turbulent region of the boundary layer and we think that if we could have moved upstream a little, we would have learned some more.

I personally have been using many of the same techniques in our computer lab. I'm one of these computer fanatics who does a lot of things digitally. We have a very active Image Processing group at the university and we have been working together with them. In addition to the turbulence , they've been doing enhancement of biological images, x-rays and indirect tumor detection schemes. We are communicating with them on the generation and detection of bursts. We have been doing work in "sonar-like" imaging based upon the work we did in the jet studies picking up sound fields by large reflectors and we found out that sonar people have been doing the same things for a long time, so we're trying to educate ourselves.

And we play with the computer! The computer gives us a great deal of flexibility once you get over that initial frustrating stage where everything progresses so slowly. As you learn to use it, you can apply computer techniques profitably to a large number of problems.

There are many questions raised by the initial experiments we started and every time we try to resolve one we open up five more areas. The major probelm is we really don't have enough people to do all these things. We have eight senior experimentalists working in these areas and need more. We're trying to get relationships with other universities in other countries and the United States, trying to share some of the work in parallel, sometimes the same problems to see if we've reached the same solutions and interpretations of the data.

Technically, we haven't faced too many difficulties. We did a lot of planning several years ago in setting up our instrumentation and the computer system and I haven't personally felt that it has restricted our ability to make the measurements and to get the results we like.

You have to sell someone the ideas that interest you. What we choose to measure is what we can convince others to support.

V.A. Sandborn, Colorado State University: Let me be a devil's advocate and pose the question of "Do we really need to measure another turbulence intensity? Do we really need to measure another spectrum: Are such things as this really going to give us new insight?" Everybody seems to be looking for a new flow. All of my Ph.D. students are happy if they can make a new set of measurements on a little different rough surface. You can meausre u', w' and maybe v' and u'v'; and yet, I would suppose that I could sit down and with a little "guess-timation" come up with curves that would look as good as the measurements. What we want to measure is something that will give us a brand new insight into what's going on, and this is becoming tougher and tougher.

Twenty years ago I started experimentally looking at how the terms in the equation of motion vary to get a new insight. What is worrying me is that we really don't know what to measure.

G. K. Patterson, University of Missouri-Rolla: I am concerned with how we are going to correlate things, and this always makes me think about how we are going to come up with models which will allow us to either predict how certain flows are going to behave or models which will, even in an engineering sense, allow us to design things. So the question which I would like to pose is what should we be measuring which will allow us to proceed further along these lines. This, of course, brings us to what at the present time seems to be the most profitable direction that people are taking in making such models, for instance, models for predicting shear flows. First of all, the velocity profile, then the turbulence which exists, and then - possibly more importantly for a lot of processes and design - what diffusion rates occur and what mixing processes occur in the given boundary layer. This is the line that I think could be pursued very profitably and the line along which it seems to me a lot of experimental efforts should be directed.

Whitelaw: You are saying exactly what I was trying to say. Do you try to improve your ability to calculate in simple flows in terms of engineering quantities or do you try to improve your ability to calculate the very complex flows? It is unlikely that understanding of the flows will allow us to calculate accurately, but I think it will allow us to calculate with sufficient precision for many engineering applications. My contention is that we need to go a little bit further in the second direction.

Patterson: This is not rebutting, it is just another comment on the same line. I think really that these kind of things should go along parallel with one another. Not necessarily everyone jump into complex flows, but the most progress seems to be made when some people are still interested in improving the model building techniques which seem to be usually best tested-out with flows that we really understand; the other people attempting to apply what can be approximated from these techniques to more complex, usually engineering circumstances. An example of this that I have been working on for a while is a problem of mixing in stirred tanks, an industrially important problem. This is an area where you can take some of the things which are known about turbulence for simple situations and attempt to apply them to the tank as an approximation to what is actually happening. This could be improved probably after even more is known about the accurate prediction of what happens in the simple flows.

Kaplan: I just want to extend that a bit to say that one of the effects of having the capacity to keep track of a great many variables and of being able to use the computer, is the capability of both working with more complex equations numerically and keeping track of much more data. That means that we can go to more complicated situations than we could before. We had to stick to the maxim that you should pick the simplest possible geometry to understand it, to model it, and then to perhaps use it as a component in building up a more complex situation. I think that we are in a position to use more complicated models while still requiring that we understand them completely. In the same way, I think we can also do experiments in more complicated flows and still obtain and keep track of significant data better than before.

I stated that we work on those things that we get paid for. Maybe some of us may be more likely to admit it than others but the whole machine is fueled by problems that are real and that need answers.

R. S. Brodkey, The Ohio State University: Let me play the devil's advocate a bit and paraphrase a statement of George Batchelor's on working on simple problems. It can be true that in approaching the simple problem rather than the complex one, we studied a part of the problem which may be very complex, and then when we go to the complex problem, the complexities may not b really be all that complex but are the overriding factors. The problems are in reality totally different.

Whitelaw: I tend to agree with the implication but we don't know whether it is true or not. My contention was that we have to find out. So we have to make some more measurements in the complicated flows. I am not suggesting that everyone working on simple flows move into complex flows and make more simple measurements. The emphasis needs to shift just a little bit.

Sandborn: I would say that, no doubt, there are some measurements that will look a little different, and indeed it would always be nice to have more accuracy. One of the problems that struck me when I began my sabbatical at NASA-Ames was that they have the new, big computer coming on line. It will have 100 computers all tied in together and they are programming it to solve the turbulent boundary layer. So what are they going to use? They are going to use a mixing length model. No doubt this is the best engineering solution we have, but we had that one before we had any hot wires. The point to be made is that turbulence measurements have not been of great value for engineering predictions.

A. Brandt, Johns Hopkins University: This feeds into my comment that the people doing practical studies aren't using the latest models we have. I am working on combustion problems in engines which have recirculating zones and high shear rates plus combustion and concentration distributions. Some information on concentration and velocity distributions, that might help in models such as Spalding's can be obtained; but it seems that the people who are doing the practical studies are not putting sufficient emphasis on measuring the quantities that would help in solving the systems of equations developed from even the simpler models. At a recent meeting on combustion systems I observed that none of the basic turbulence quantities were measured, so that when the researchers wanted to model the flow fields they had no choice but to use oversimplified eddy viscosity or mixing length models.

Kaplan: I see three types of experiments: In the first, a theory has been developed and you do experiments to see whether the theory checks out. You measure the u'v'-correlation because the theory, in its way of looking at the equation of motion, asks you to measure it. The second type of experiment is one in which someone has a specific problem (i.e., they are combusting in a complicated geometry of separation). There is no basic theoretical model helping you so you do experiments to determine how that situation develops. And while you're at it you can measure some other things which would be of general interest if you have a little extra time. You measure some spectra and some correlations. A third type of experiment that's becoming more popular now than in the past years is one in which people try to experimentally look at the fundamental nature of the flow hoping to give some structural guidance to the theoretician. So that based on the idealization that we make of some of the situations in turbulence somebody can put together the necessary mathematics to make that predictable.

I think that all three types are important and I don't think, speaking as an experimentalist now, an individual should limit himself exclusively to one of those areas, because it becomes rather sterile.

I think the future of experimentation in turbulence is really looking up. I think more people are doing a larger number of these problems and a lot greater range of insights are being brought to bear.

Sandborn: I think a problem of interest is the separation type problem. One of the questions we might pose is what would we like to measure in the separated flow. I was approached by a fellow who employed a high-powered digital system with the statement, "I really think I can predict the separating flow if you can just tell me what the turbulent shear stress is there, because it's really important." We had made one set of measurements, where we found that for turbulence separation, the turbulent shear stress was not an important term in the equation of motion. I think there's a problem here, and there ought to be more thought on what will give us new insight into this problem of turbulence.

W. W. Fowlis, Florida State University: I see two things as being quite significant. In general, computers have come on faster than anyone thought. We are now in the verge of coping with 3-dimensional problems and all turbulence is 3-dimensional. The laser Doppler seems to be very important here. In many situations it can give velocity as a function of time and when you have that you have a great deal. I don't feel so optimistic about analysis by linearized mathematics. It seems to me that notmuch is going to come there. The big problems coming up are the environmental problems and the big applied problems. The only way is the computer. Numerical schemes are not always reliable and so have to be checked. The laser-Doppler has appeared on the scene. I see a building up with bigger and bigger numerical computations plus laser-Doppler experiments in the laboratory and some of the difficulties that Prof. Sandborn has touched on perhaps being answered that way, back to the correct mixing length theory.

V. W. Goldschmidt, Purdue University: Let me pose a general question - At the Langley Research Center on mixing shear flows there were two groups: those of us who preferred to measure and those who preferred not to measure. Those who preferred not to measure but preferred proposing theories complained of the lack of data useful for constructing correct models to predict very simple things like the spreading rate of shear flows. Now we have heard from the panel that we need to get into more complicated flows. We have also heard from the panel that we need to go into more simple flows and these two things are in agreement. Now I would like to pose the following question: - Do we at this time have enough data to allow these chaps who prefer not to take measurements to go at it and predict the spreading rate of shear flows?

Brodkey: One need from a measurement standpoint in our fundamental work in turbulent shear flow is to have a probe that moves with the flow. We want to measure and photograph the same region. A laser system that can operate around zero velocity and that could be transported along the flow would be good. It would have to move at up to 1 ft/sec, maintain alignment, and stop at the end with a sudden shock. I would take just one component velocity, the U-component.

P. Iten, Brown Boveri Research Center: From the optical point of view, it should be possible. One can now buy an optically integrated system which is able to withstand high accelerations. I think this has been done. I don't know exactly where, but I'm sure this has been done. With laboratory setups of course this won't work. You have to have an item as compact as possible. I think we are not far from this.

L. N. Carter, Naval Ship Research and Dev. Center: We have just completed a brief study on the feasibility of using a multicomponent LDV system mounted on a towing carriage for survey work for determining the wake of the propellor on a ship model. It was a strictly paper study, so we don't really know how it's going to work out. It has convinced us that it's possible in principle, although it is going to be very difficult and we anticipate a lot of problems because nobody has done anything like that yet. Actually, we think that we could get all three components simultaneously by using one laser, one photomultiplier tube with Bragg cell shifting and bandpass filtering off of each of three components at the Bragg cell frequency. This would have to be done through a transparent window, but not on the hull, which would raise a big problem, interfacing between air and water due to the index of refraction problems. Another big problem with this is that it would have to be competitive with our present system of using a pitot tube rake which is able to get six measurements all at once where the other used only the one. But we feel by possibly automating this data, this could be accomplished.

Whitelaw: I would seriously ask the question of the last speaker. O.K., you can do this paper study, you may even be able to purchase or conjure up the instrumentation to make the measurements you're suggesting you make. Do you know what to do with them? I suggest that it might be quite difficult to know what to do with them.

To Prof. Brodkey's requirement, I think it is possible. I think it would require a little work, but I'm sure it can be done. However, if it were me who was going to build, I'd like some greater assurance than just the question, that it is going to lead to something which is worthwhile having. Specifically, for instance, take all the work in bursting that is so interesting and so fascinating and from which I think I've learned a great deal of information about turbulence, I still don't know what to do with it. I still can't use it. There's still this big gap between the user and the person who's trying to create an equal understanding.

H. M. Nagib, Illinois Institute of Technology: My first comment is on the question do we have the instrumentation to do it? I think we have a lot of instrumentation if it is used in the right way. I think if we learn from the problem about the bursting phenomena in the boundary layer and some other problems, that one very effective tool we haven't been using very much is the combination of point measurements and field measurements. The point measurement can be obtained from hot wires and hot films and so forth, the field measurements from visualization. I think if we combined these two tools as Prof. Kline so elegantly showed us today, we can do a lot.

My second comment has to do with some of the problems. What can we do? We have developed a philosophy recently that we call a functional approach to engineering problems through modules. I'm sure all of you are familiar with the wake of a cylinder and you know that under certain Reynolds number conditions that you have shedding. We discovered that the Karman vortex pipe has an instability but later on we discovered there is a second instability on top of this one. I think these different things interact, I would call this one one module and this another module.

I. J. Wygnanski, University of Tel-Aviv: The first thing I would like to comment on is Prof. Whitelaw's question of whether the new measurements, say in bursting, can enable us to develop new methods of calculations. I think we are just at the beginning of the road. We don't have enough data to enable us to calculate things like "mixing lengths" which we plug arbitrarily in our models.

For example let's consider the mixing layer where the doubling process of vortices occurs. If one takes an inviscid model as Winant did, one can calculate the spreading rate of the mixing layer. Isn't this better than using some mixing length hypothesis coupled with arbitrary constants?

Victor asked a question about the mixing layer - do we know enough to predict the spreading? Sometimes yes and sometimes no. We encountered an interesting problem in a simple two-dimensional mixing layer whereby introducing just a trip wire at the start (i.e., at the discontinuity), the spreading rate changed. Spreading with the trip wire was about 30% faster than without it. This introduced a spreading constant which is different from the measured one by Liepmann and Laufer. It seems that the flow was self preserving, so the initial condition should not affect the spreading. We could have thought maybe we committed a gross error but then Datt from TRW repeated the experiment and got our spreading by putting a trip wire in. Removing the trip wire, he got Liepmann and Laufer's result. Here we have different rates of spread, and the difference is quite significant. It is possible that measurements of intensities and mean velocities give us very little insight into some problems. They are necessary in order to define the flow statistically but the new methods of measurement may enable us to understand the mechanisms governing turbulent shear flows.

L. Thomas, University of Akron: It seems that there's some despair on the part of some of the panel members, from the standpoint that we still heavily rely on eddy diffusivities and mixing lengths. Comments have been made suggesting that it would be nice if we could use some of this burst information, such as in the prediction of temperature profiles, velocity profiles and heat transfer. I'd like to point out that this information can be used. Bursting information that has been obtained by a number of people here, and by Meek and Baer, myself and others, can be used in the context of surface renewal type of formulations to predict heat transfer, temperature profiles, velocity profiles and recovery factors for liquid metals, moderate Prandtl number fluids and high Prandtl number fluids, under steady or unsteady conditions.

R. L. Simpson, Southern Methodist University: There is a technique which is relatively unknown to fluids engineering which maps an entire velocity flowfield at an instant by means of a hologram. This would necessitate a holographic motion picture camera for fluid velocity measurements over a long period of time, something like a rotating prism camera. Since this technique has not been developed much, I don't think we have the types of instrumentation we ultimately need, because we need to measure entire flow fields at one point in time, and when we have that we can quit taking samples here and there and guessing about what's happening. We will have an entire flow field mapped spatially and with time. A paper on this technique was published by Mayo and Allen, Applied Optics, Vol. 10, No. 9, Sept. 1971, pp. 2119-2126. Limitations were due to a low-powered laser and the inability to make multiple pictures rapidly.

Nagib: In 1967-68, we had to build a number of wind tunnels in which we needed to control the free stream turbulence levels. We worked with different devices that were placed in this free stream including screens, honeycombs, perforated plates and so forth. We call these things turbulence manipulators. This work came out as an Agard report. We call it experimentson the management of free stream turbulence.

This led us to an interesting result. Perforated plates or screens have instabilities very much like Champagne instabilities which we call shear layer instabilities. It turns out that these instabilities are very, very important in controlling things that are happening. Now one of the very important things that you can do with this is to select an exciting sound frequency, and building it at the test section, reduce the turbulence level downstream by 20 and 30%. You are exciting those instabilities downstream of that particular device. In this case it was just a typical honeycomb in a free stream. If you excite that honeycomb with the correct frequency, appreciating the fact that this instability is very important in the mechanisms, in the growth and the decay of that turbulence downstream of that device, then you can reduce the turbulence downstream quite a bit by adding a very small amount of energy, sound energy in this case.

Kibens: A regrouping of concepts along a different axis appeals to me, namely, the classification of flows into various units or modules, if I am to use Prof. Nagib's term, and then seeing what can be done in the way of controlling these modules in terms of either enhancing or erasing their main features. Along this line of reasoning one might ask how do we kill the sublayer bursts?

Kaplan: I would like to comment on several areas. Turbulence was controlled in an experiment on a turbulent mixing layer by Winant in which the spreading of the mixing layer was delayed substantially by driving the flow at a critical location. Kendall's experiments were performed in the wake of the flat plate by driving the wake, transforming a turbulent wake into a regular wake. Additional examples are jet noise suppressors So there are many techniques for manipulating turbulence. I may also comment that in many areas people are interested in increasing turbulence levels to help in mixing and we seem more successful at that than in reducing levels.

H. Branover, University of the Negev: I think we are missing one important possibility. The magnetic field can be used very effectively for controlling, managing and changing turbulence in electroconductive flows. The golden time of magnetohydrodynamics passed fifteen years ago when there were a lot of undiscovered questions. But the possibility to use magnetic fields as a tool for changing the properties of turbulence and for making it possible to discover hidden away properties still exists. A magnetic field can change the degree of isotropy of turbulence; we can obtain almost two-dimensional turbulence, turbulence with no momentum transfer, but still intensive heat and mass transfer and so on. All this is related closely also to transition problems because here one has to deal with the question of two-dimensional or three-dimensional instability and correspondingly with the dilemma: two- or three-dimensional transition. All those questions can be investigated by the use of the presence of a magnetic field.

J. L. Zakin, University of Missouri-Rolla: Most of the suggestions for managing turublence have dealt with mechanical ways of doing it or somehow or other influencing the turbulence from external fields. Another area that hasn't been mentioned is changing the nature of the material. One of the main reasons for studying drag reduction in liquids, aside from its practical import, is to obtain a better understanding of turbulence mechanisms, that is, to observe changes in the flow field caused by changes in the fluid.

A similar situation exists in dusty gas flows. I believe that in cases where drag reduction has been observed, the effect is probably due to electrostatic charges on the particles. That's what Ian Radin has proposed and I'm pretty well convinced that he is correct. There again we've changed the nature of the fluid which in turn changed the turbulent field. So manipulating the fluid itself is another alternative.

Brandt: Besides modifying the flow field or changing the fluid, practical goals can be attained by redesigning the system. An example of what I have in mind is the automobile engine, where the primary problem is that of reducing the nitrogen oxides. This turns out to be a fluid dynamic and a kinetic problem, and as yet no one has been able to design a combustor where the kinetics and turbulence work together to reduce the NO formation. This is a situation where we have not been able to "manage" turbulence to achieve the desired goal and perhaps an altogether new type of combustor is required. This again points out the need for studying more complicated problems.

Whitelaw: I want to come back for one minute to some comments that came from Prof. Wygnawski and some others many of which I agree with. I agree that there are exceptions to many of the rules that have purported to exist within the framework of the numerical procedures in turbulence models, etc. Unfortunately, I have to eat and the bread and butter is provided by people with real problems who want to solve those problems. One example which was mentioned earlier is that of the combustor. I have no way of handling problems like, for example, the gas turbine combustor, with all its complexities, other than to take what I believe to be the best turbulence information around, and shove it into a numerical framework, and crank out the answers. I don't believe the answers, at least I don't believe them in absolute terms. But I believe that I have a chance of getting the trends right and if I can get the trends right then I have made a very, very long step forward.

Goldschmidt: Should there be a 20 year moratorium on digital computer analysis?

Kaplan: I would like to start off with a very irreverent comment. If the digital computer had been available in the seventeenth century I'm convinced that the laws of gravity would never been formulated. There would have been very complex models, simulations of the motions of the planets up to the seventeenth order of epi-cycloidal theories, which for engineering purposes would have predicted the motion of the planets for the next 200 years. The fact that a model can be useful for predicting a few of the easily measureables doesn't mean it contributed to the knowledge or the fundamental understanding of what's going on. We've taken very small steps every stage; we make minor perturbations on the situation that existed before; we can retrench and revise very quickly, but there is still a definite need for fundamental understanding.

I feel safer in saying we should put a 20 year moratorium on the digital computer than we should put a 20 year moratorium on the movie camera, that would be unjust. I am a heavy user of the digital computer so I'm attacking myself in this case. But in many cases we use the machine as a crutch, so that we don't have to make the intellectual jump that Prandtl made for example. The idealization of the situation is trying to extract from very complicated phenomena the heart of the physics. The true idealization, the true model picture, may not look like any of the measurements we see, may not look like any of the visualizations we see, but it is the great intellectual leap. So many of us are trying to debug the computer programs and fight the system and get the money to make the next thousand runs, that we may have lost our capacity for thinking.

Sandborn: I would say to a certain extent that I am in agreement. In teaching the student how to make measurements, one of the typical examples is to give him a chart recording trace of the turbulence and tell him to work up the probability distribution. He will invariably take a random signal, digitize it on the computer, and come out with some of the most beautifully skewed probability distributions you ever saw - for a perfectly symmetrical signal. He will never question the fact that he missed the point because he didn't digitize it right. I think that there are, from an experimental viewpoint, some very definite pitfalls. I can remember people saying, "I've got the computer all set up. Now I can compute 130,000 points per second." And I found myself wondering, is that 130,000 wrong points per second that we're going to have to deal with? I think the problem is that one can get the idea that the computer can do no wrong, and of course, the computer can do no more than what it was told.

Whitelaw: My answer would be yes, a selective moratorium. I wouldn't say how I'd arrange the selection. I have a lot of sympathy with the arguments against digital computers. I try not to use them. I make too many mistakes. I do feel, however, that the argument which says that our prejudices, the older generation's prejudices, against the digital computer should make us take them away from students is not a very good one. I think that what we have to do is to learn to teach the student how to use the digital computer.

Brodkey: Is it not the object of people working on turbulence to eliminate the subject by understanding it?

Goldschmidt: I think that's the answer, once you can get a set of equations which is deterministic, it solves the velocity at any point at any time. Turbulence is that which does not fall into that definition.

Now, I would like to summarize the comments made in this session. The first item to which we addressed ourselves is what measurements do we need? What should be done? What should be our philosophy there? The panel led us to the thinking that there are three different types of measurements: those in which we are trying to develop data for theory, those in which we are trying to solve immediately practical problems, and those in which we are trying to check out theories. If we first answer the question in which one of these pockets does our work fall, maybe our approach would become more efficient. Further, the consensus is that technology and instrumentation are generally available. We also did talk about instrumentation needed.

It was stressed, that we probably could get a lot of mileage out of data already available, and secondly, that it's necessary to define what will be done with that curve that we're going to plot, that paper we're going to publish.

On the question of whether we can control or manage turbulence, I believe we all felt very domineering and we decided we could indeed control turbulence. We said that we could control the effects of spreading, the influence of acoustic fields was referred to, and the possibility of noise reduction was noted. Certain gadgets could be built on the exhausts of jets to avoid noise we were told. Transfer could be enhanced, magnet fields could be brought in, and polymers and dust could be added. We do have certain abilities to control and manage turbulence.

A strong point made was the idea of thinking in modules and that a regrouping of these modules may be desirable.

On the last item, on a moratorium on computer use, the consensus was that a selective moratorium might be desirable.