TCM-2011

SESSION 2 – Shear and Wake Flow Turbulence

Co-presided by Bob Antonia and Charles Williamson.

The session comprised 3 parts, each of approximately one hour, as follows:

(1) INVITED PAPER. The invited paper was "Turbulent Shear Layers and Wakes" by Garry Brown and and Anatol Roshko. The paper was presented by Garry (Anatol was unfortunately not able to attend).

(2) PRESENTATIONS. Two presentations. The first contribution "Stability of Coherent Vortex Structures in Shear and Wake Flows" (by Paolo Luzzatto-Fegiz and Charles Williamson) was presented by Paolo. The second contribution "Scale-by-Scale Energy Budgets which account for the Coherent Motion" (by Luminita Danaila and Robert Antonia) was presented by Luminita.

The second half of this part was devoted to the viewing of posters – this was combined with a coffee break.

(3) GENERAL DISCUSSION. A discussion (a highly comprehensive edited transcript of this discussion, first generated from recordings and notes by Paolo Luzzatto-Fegiz, is attached) which included brief presentations by Kathepalli Sreenivasan, Jim Wallace, Fazle Hussain and Bill George.

(1) INVITED PAPER. *" Turbulent Shear Layers and Wakes"*. Garry Brown and Anatol Roshko.

This very interesting presentation delivered by Garry Brown reviewed the significant research that has been carried out, since the 1961 Marseille colloquium, on coherent structures in mixing layers and wakes. One of the major highlights of this 50-years period is, without doubt, the discovery of coherent structures in high-Reynolds number turbulent mixing layers (Brown & Roshko, *Journal of Fluid Mechanics* 1974) comprising large-scale, predominantly 2-D, spanwise vorticity rollers that, via the Biot-Savart interaction, amalgamate to form even larger scale vortices. This picture differs drastically from Townsend's large eddy hypothesis. Their 1974 paper also highlighted the role of the braids between the large scale vortices in amplifying the streamwise vorticity, which triggered a series of key papers studying the evolution and significance of streamwise vortices in such shear flows. Garry discussed various aspects of the mixing layer, such as the relationship between the Reynolds stress and the vorticity flux, the entrainment, mixing and mixing transition as well as the possibility of forcing/controlling this flow. He stressed the importance of the Biot-Savart mechanics for the generation and coalescence of the large structures. He also identified issues that need to be studied further, perhaps the most major one relating to the universality of the reverse cascade. He went on to discuss the plane wake using available experimental and numerical results. He pointed out that there is strong evidence for structure rescaling in the far wake, based on relatively low Reynolds number data. This raises the issue of whether this rescaling will continue at much higher Reynolds numbers and whether it will lead to an asymptotic state characterized only by the drag of the body and the distance from the body.

(2) PRESENTATIONS.

In her presentation, Luminita pointed out that scale-by-scale energy budget equations illustrate the equilibrium between different physical processes through which energy transits at that scale: turbulent advection, diffusion, production, molecular dissipation. At finite Reynolds numbers, not only all these phenomena have to be taken into account, but the routes towards universality ("4/5" and "4/3" laws) depend very much on whether the flow is decaying or whether it is forced; for any given flow, the effect of initial and boundary conditions may also determine the route to the asymptotic state. The available data indicate that, at the same Reynolds number, forced flows tend to be closer to the asymptotic state than decaying flows. She then focused on a particular type of decaying flow -the wake behind a cylinder- where however the forcing due to the coherent motion (CM) cannot be ignored. She then went on to use the scale-by-scale budget equations written for both the CM and random/turbulent motion (RM), with the aim of quantifying the energy exchange between these two types of motions. Preliminary phase-averaged results suggest that the kinetic energy transferred by the random motion only, inferred from the total kinetic after subtracting the kinetic energy of the CM, closely follows the route that decaying "isotropic" turbulence is expected to take.

Paolo's presentation focused on the stability of certain coherent vortex motions. There is of course a large body of work linking instabilities of coherent structures (such as merging or tearing) to the dynamics underlying the energy cascade in 2D turbulence. Paolo noted that, even for remarkably simple vortex configurations, there has been much debate on their stability properties (as established, for example, by linear analysis or simulation). Paolo proposed a different stability approach, which links the number of unstable modes, for a family of steady vortices, to a simple diagram involving velocity and impulse. He also showed how introducing imperfections in these flows enables one to discover hidden bifurcated branches. He illustrated the use of this approach by considering superharmonic instabilities of a Karman street of finite-area vortices, finding that the "imperfect-velocity-impulse" (IVI) diagram methodology gave stability boundaries in agreement with linear analysis, while revealing new families of lower-symmetry vortex streets. Paolo noted that this approach can be used also for other conservative fluid problems, including nonperiodic flows, therefore enabling the analysis of other fundamental instabilities.

(3) GENERAL DISCUSSION.

As one might have expected, there was a fair amount of discussion in the third part of the session (as can be gleaned from the attached transcript, scribed by Paolo Luzzatto-Fegiz) . This addressed some of the key questions which emerged from the invited talk but also other aspects of free shear flow turbulence that were not covered in that talk. Kathepalli Sreenivasan started by making general comments, first on the need to understand the complex relationship between turbulence and stability, and subsequently on how Kolmogorov's 1961 Marseille colloquium paper changed our perception of the universality of the small scale motion, in particular on how the characteristics of the small scales depend on the forcing due to the large scale structures. He went on to discuss the difficulties in assessing local isotropy in a shear flow, where the two main parameters that are expected to affect the isotropy are the non-dimensional mean shear and the Taylor microscale Reynolds number. His work with Jorg Schumacher has indicated that one may reach the conclusion that local isotropy can be attained for a suitable combination of these parameters when focusing on the third –order moment of the streamwise derivative of v (the transverse velocity fluctuation) but that the picture can change drastically when higher odd-order moments are considered. It would seem that whether or not local isotropy is actually attained depends very much on the degree of scrutiny one is prepared to apply when testing it.

At the end of Kathepalli's comments, a significant amount of discussion ensued as a result of Garry Brown's comment that one of the realities of turbulent shear flows is that they are Reynolds number independent and that "precisely what happens at the small scales can't be critical" since the Reynolds shear stress, which seems to be well predicted by an entirely inviscid two-dimensional Biot-Savart driven process, dominates the mechanics of the flow. This view was not shared by some of the participants, e.g. Bill George stressed that, even if the local Reynolds number remained constant in x, viscosity does play a role, in the context of multi-point equations, in separating the dissipative scales from the energy containing scales.

Fazle Hussain emphasized that if there were no coherent structures, we would not be able to control the flow. He also pointed out some of the complexities we need to be aware of, e.g. as they arise through both 'tearing' and 'reconnection' cascades, the intricacies of the interactions between similar sized vortices as well as between vortices of quite disparate scales.

Jim Wallace gave a brief overview of phase-averaged results obtained in a mixing layer using a multisensor hot-wire probe which can determine the velocity gradient tensor to within 5 Kolmogorov length scales. He was able to examine the spatial relationships in a streamwise plane between for example the phase-averaged velocity vector field and regions of high enstrophy, or high Reynolds stress, or production of turbulent energy. Somewhat surprisingly, the high dissipation rate was found to be largely concentrated within the rollers. On the other hand, regions of large Reynolds stress and energy production occurred along the peripheries of the rollers. There was some ensuing discussion of this with no definite consensus among participants about some of the features observed by Jim.

Finally, Bill George warned us, in his own inimitable fashion, about the pitfalls of simply following the traditional line, as advocated in standard texts. He illustrated this in the context of formulating a more general similarity or self-preservation analysis for jets and wakes than the traditional one which simply assumed or imposed the same similarity scale for both the mean velocity and the Reynolds stresses. Such a generalisation quickly leads to the conclusion that for a particular type of flow, many self-preserving states may be possible, each uniquely determined by its initial conditions. There has now of course been ample documentation in support of this this idea.

In the 'wake' of this session, it may not be unreasonable to suggest that perhaps the two 'major' new contributions to the field covered by this session since the 1961 Marseille colloquium have been (1) the identification of coherent structures and appreciation of their importance in terms of furthering our understanding of free shear flows and our ability to control them; and (2) the importance of initial conditions in determining the appropriate similarity state to which these flows evolve. Points (1) and (2) are of course not unrelated. Naturally, there is still much work to be done, especially in terms of providing a plausible link between (1) and (2) in order to investigate the idea that different types of coherent structures, which arise because of different initial conditions, can lead to different "asymptotic" states. It may also be appropriate to end with one of Garry Brown's concluding thoughts : "The subject stands at the beginning of a new era in which both LES and DNS calculations can provide details of the vorticity field and the fluxes of vorticity ". It should indeed be interesting to see if at the 2061 Marseille colloquium these types of calculations will have been able to meet the challenge of predicting the underlying mechanics and momentum transport of the flow for particular initial conditions.

DISCUSSION FROM SESSION 2

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DISCUSSION COMPREHENSIVELY SCRIBED BY DR. PAOLO LUZZATTO-FEGIZ.

Bob Antonia (Showing one slide): I am putting up this slide mainly to get some of the discussion going. The two main issues we might like to discuss are obviously not unrelated. The first is concerned with the general importance of coherent structures in free shear flows, their dependence on initial conditions , how they affect the transfer of energy from scale to scale and their implications with respect to our ability of controlling these flows. Do we believe we have suitable analytical tools to address some of these aspects? For example, DNS scale-by-scale two-point energy budgets would be particularly helpful as we are still struggling to put together reliable one-point energy budgets for these flows. The second issue concerns the small scale motion and in particular whether we are now convinced that the departure from isotropy of the small scale scalar field, due to the effect of the large structures, is greater than that of the small scale velocity field. Sreenivasan has done quite a bit of work on this and I was hoping he might be able to comment on this aspect. I remember talking to Stan Corrsin in the early eighties, and Stan thought that, in the context of wakes for instance, that local isotropy was 'alive and well' in the velocity field at large enough Reynolds numbers but he felt that it may not be valid for temperature and concentration fields that are convected by locally isotropic turbulence. I believe that we now have sufficient evidence that points to the temperature fronts being much sharper than the velocity fronts, thus implying that their impact on the anisotropy of the temperature field should be greater than on that of the velocity field. Another idea is the possibility of relaxing the assumption of local isotropy, replacing it by assuming local axisymmetry. I was hoping Bill George might comment on this although he may also wish to comment on the effect of initial conditions on the large structures. Charles and I have lined up four people to address various aspects of this, or maybe aspects of Garry's talk: Fazle Hussain, Jim Wallace, Bill George, and Katepalli Sreenivasan. Each has been allocated about five minutes.

Katepalli Sreenivasan (Using blackboard): Ever since I visited Garry Brown, when he was in Adelaide, the question is, it seems like a very important question to ask, what's the relation between turbulence and stability? It's clear that there is some connection, but it is clear that the connection is very complicated, I mean, it is not a very simple 2D stability, and nonorthogonal eigenfunctions, not even that simple. It is very much more complicated. So what is the issue? What is the relation between stability and turbulence, and exactly what problems does it entail? I think it is a very important question. Whatever one has to do, one has to start with this scalar problem somehow, by subsuming the small scales into the dynamics somehow. This is what Large Eddy Simulations do, by and large, and I want to say a little bit about what we know about the small scale, about the so-called universality, and then make some comments about what Bob was asking me to do. This is universality in quotation marks. The history of it is very elementary. It started with a very nice analysis, particularly by Lee and others. Assume it's at equilibrium, you get a spectrum that scales like k^2, not k^{-2} but k^2. We thought it obviously didn't make any sense, so we resolved that very quickly. And Kolmogorov 41 said that in the inertial range, only the spectrum matters, ε and ν are both required, and you got this fantastic form for the inertial range. But what can K62 do? Which is exactly what Marseille 61 did- since ε is intermittent, it effectively brought in the large scales. The forcing scale becomes very important. I have in mind something like this, I'm talking about boundary layers for the moment, I have a disturbance by an object of length scale L, and its power is u_0^3/L, u_0 is a velocity scale. So, what K62 or intermittency brought, it introduced the large-scale lengthscale forcing, into the description of the small scale. And as a result, you have this [see board], for instance, some constant, you have the -5/3 law, times kL to the power of minus something, and this is something you will not compute dimensionally, you really have to do some real stuff. And what is known is if you go from second order statistics, such as the spectrum, to higher-order statistics, this power, equivalent power, will just keep increasing, and it's not simply proportional to this, and that's really what multifractal scaling, and multiscaling is all about. If the velocity of the forcing is strong, not only does the lengthscale of the forcing enter the picture, but also the velocity scale does. Unless you are close to u_0 = 0, that is some small velocity, u_0 also makes an entry. So what it means is that, if this is true, the Reynolds number is not the entire story, how you create the Reynolds number becomes an issue. For example, you take a very large scale, and stir it very slowly, that situation is very difficult from having a small scale and stirring it very fast. The two are not identical, once you have u_0 also coming into the picture. In fact, if you go into more and more details, it looks like more and more details of the forcing need to be taken into account, in order to describe the small scales. So, how much of this forcing do the small scales remember? I mean, that's the question, and as I said, we started off taking absolutely nothing, remember, equilibrium, equipartition of energy around that, and that's nonsense, and slowly, as we refine our analysis, more and more the driving force becomes an issue. Now to the particular worry which Bob Antonia wanted me to talk about; he wanted me to discuss it with respect to scalars, but let me discuss it with respect to velocity. So, if v is in the direction orthogonal to x, by symmetry, if you have isotropy in the large scale, this quantity $\langle(\partial v/\partial x)^3\rangle$ must be zero, if you have a lot of isotropy. You can measure this in a shear flow, and you find it is not zero, it is of the order of 0.5 in magnitude, something like that. This tells you that the small scale, because the gradient refers to the small scale, departs from isotropy. You'd like to understand how this anisotropy is coming, and how it is related to the shear, as Bob had tried to ask. So, Schumacher, with some involvement from me, made some numerical simulations, of a homogeneous shear flow, where he varied the shear of the flows, from small to large to larger, for different Reynolds numbers, and he produced a 2D plot indicating regions where the skewness of the velocity derivative was zero, regions where it was nonzero, and then zero again. What he said was, for any given shear, if you increase the Reynolds number, the shear becomes isotropic, I mean, the velocity gradient becomes isotropic, viz. you recover universality, in some sense. The plot I have drawn here (it's not, of course, as nice as I have drawn), but basically there is a bar there. So, for any given shear, you come to an Re^*, and it is essentially isotropic or universal. On the other hand, if you go to the next order, the fifth order for instance, the corresponding boundary is no longer here, but it is here. So what it means is that, for any given Reynolds number, suppose you have this Reynolds number somewhere here, the third-order moment is zero, that means that if you look at third-order moments you may conclude that you are isotropic, but if you look at fifth-order moments, it's not. So, for any given Reynolds number, there always exists a moment that is nonzero. So, depending on the detail you are willing to buy, depending on the detail which you are able to assess in your flow, you will get a different conclusion on the behaviour of the small scales and on the universality. So if you are interested in second-order, you can say it is roughly so, it is roughly universal, but you go to the next order, and the situation becomes more involved. So, there is no simple answer "the stuff is universal", or "the stuff is not universal", but it really depends on the detail on which you are willing to compromise, or ask the questions. Ultimately it is therefore that universality is simply a figment of one's imagination. Perhaps to some order of approximation, you can fortify it and say that it is (or it is not) useful for your purpose. Thank you.

Garry Brown: I think that one of the great realities of turbulent shear flows, is that they are Reynolds number independent. Precisely what happens at the small scales can't be critical. I mean, what if it was totally inviscid? If it was totally inviscid, and you talk about a whole lot of velocity fluctuations, how would you distinguish it then from heat? The flow is independent of Reynolds number, and it is a remarkable fact, I think, that in the mixing layer, the Reynolds stress dominates the mechanics of the flow, the Reynolds stress is given by the large scale vorticity field, and it is independent upon the small-scales, or the three-dimensionality of the flow, and it's a remarkable fact, that this shear stress is very well predicted by an entirely inviscid, two-dimensional, Biot-Savart driven process. That's amazing. Amazing. I don't say that it's exactly the same, what I say is that the mechanics is largely driven by that process. Then so, for free shear flows, I think the emphasis on whether we have homogeneous isotropic turbulence at the smaller scales, is not so significant, to the fundamental mechanics, which is the Reynolds stress.

William George: I'd like to disagree with that, Garry. And I think that part of my confusion arises from two of the flows that we look at most often, namely the plane wake and the axisymmetric jet. If we look at the local Reynolds number, the problem in both of those two flows, is that this value is constant, so whatever you impose at the source, is the same Reynolds number all the way down, so you think it is behaving as though it were inviscid, only because the Reynolds number is constant. In other words, the relative effect of viscosity does not change, as you move downstream. So the problem is that you've been fooled into thinking that it's Reynolds number independent, but in fact the equation for the mean of the single-point moments is relatively inviscid. This is not the case for the multi-point equations, and viscosity plays a very important role in separating the dissipation range from the energy range, and the Reynolds stress, for that matter. I think we've been really misled into thinking that these things are inviscid, but they aren't. Viscosity very much affects the second-order moments.

Roddam Narasimha: It's true that for the wake the Reynolds number remains constant as you go downstream, but for the mixing layer, it does not.

William George: Absolutely right.

Roddam Narasimha: It actually grows like x. So that argument won't cut it, for the mixing layer it should be very bad.

William George: No, it should become more and more inviscid, the further you go.

Roddam Narasimha: Yes. So, therefore, what would you like me to say about that?

William George: Well, but it does not have a similarity solution either, so... I mean there are a lot of things going on, right? The relative balance of everything is changing, and I would agree with you that, asymptotically, the viscous terms are negligible. I mean, eventually you'll get a fully developed 5/3 range in your spectrum, and all the usual things. But you can create a jet and a wake, that looks like it's self-preserving, from almost the beginning.

Roddam Narasimha: For a mixing layer, the calculations have been made, and what they show is that, depending on the initial condition, we have a transient, a region which is affected by the initial conditions, but in simulations that we've done, involving a hundred realizations, with 10,000 vortices, the result is very clear, the final state is in fact independent of the initial conditions.

William George: So what's the point of disagreement?

Roddam Narasimha: The point is that the growth rate is so close to the experimental values.

William George: But what's the point of disagreement? That's a very different flow from what I was describing.

Roddam Narasimha: I am reinforcing what Garry said, for the small structure...

William George: For one flow! For *a* flow! That shear layer.

Roddam Narasimha: No, I am saying that for that flow, the inviscid scaling is very precise.

William George: I agree with you. We have no point of conflict here.

Zhen-Su She: I wanted to come back to Sreenivasan's comments. I seem to agree with everybody. But I am afraid the only problem I see is that there is no theory, because any theory has to be based on some universality. Now, there is a hierarchy of symmetries here. So, I totally agree with this view. But again Garry, there are some scales where there are some universal mechanisms, and we've done theory for the pipe flow, and all this evidence seems to be universal for pipe flow, and I think for the wake as well. Now, there are instances where we have to identify those empirical constants, from place to place, and I also agree with you that all the issue is not high Reynolds number, it's how to get asymptotes, that if you go to high Reynolds number, it seems to be universal, and so, the point I am making is that there are different aspects of universality. If I speak of the Kolmogorov constant itself, it may not be universal. But how the Kolmogorov constant varies with Reynolds number, that could be universal.

William George: Let me just make a comment. The axisymmetric wake is the opposite of what you just described, because the Reynolds number continuously drops, and it shows a very string dependence on initial conditions.

Roddam Narasimha: We know that the axisymmetric wake will eventually laminarize.

William George: No, I don't know that. In fact, I would argue that's not true. From an engineering point of view I can say it laminarizes, from a turbulence point of view it doesn't. When you say it laminarizes, what do you mean? It drops below a certain amount? Or you mean you just have no production of energy to balance the dissipation?

Roddam Narasimha: If you look at what happens as you go downstream, you see that the dissipation over the production increases.

William George: I think that the numerical analyses have shown that that's not true. The work that Peter Johansson and I did shows that in fact it seems to evolve to a second state, which can sustain itself forever, at least in the continuum limit. What you get is a balance between different things, just as transport comes in, convection goes out, but it's a balance between different terms, because as the Reynolds number drops, other terms have grown into the problem. There are two simulations going on right now: one is at Southampton, and it's quite large.

Keith Moffatt: A frivolous comment, while we wait for Fazle. The cosmologists have had no difficulty moving from universes to multiverses, so we should have no difficulty moving from universality, to multiversality, if such be the needs.

Fazle Hussain: I wish to emphasize the point that, if there were no coherent structures, you could not control the flow. If you have an elliptic jet, the axis will flip flop. If there were no coherent structures, there would be no reason for the elliptic jet cross section to flip flop. And there is a recent simulation by Gleizer, showing there is no question there is a plethora of structures. Typically, we're interested in the vortex/turbulent interactions, or coherent structure/coherent structure interactions. One kind of cascade is tearing, another is reconnection. Sreenivasan drew some beautiful pictures of reconnection. There are some other examples of superfluid reconnection, and of reconnection in the wake of an aircraft. For a 747, the Reynolds number is 10 million, for each vortex. The current limit for DNS is 10,000. So, as Sreeni showed, what does reconnection mean? That there is a topological transformation. This is a Kida simulation showing, at a Schmidt number of one, for the first time, that indeed what you see ain't what is there. The left-hand side is a scalar, and the right-hand side is the vorticity pattern, in two colliding circular vortices. In the first CTR conference, we did this simulation, of two antiparallel vortices, they connect and reconnect, but you see that part of it is left behind, and that will reconnect again. This is a more recent one, at higher Reynolds number. It turns out that contrary to the situation that Sreeni was talking about, the rate of approach, and rate of departure after reconnection, are quite different, and the departure is much more rapid. Therefore this must be the most effective part of noise generation, in the acoustic model that I proposed in 1980. No one has yet disproven it, but no one has proven it, either. We're working on it. In the picture here, we have reconnection, and then a second reconnection, and so on. So one should be able to build a reconnection cascade theory of turbulence. The other question I have, in the other slide, is the interaction between completely disparate scales. We have a column, which contains very small scales. It turns out, this nonlocal interaction, involves very different scales, is the question I think Parviz had raised. How does the structure affect -- it does, because as these fine-scale structures are stretched around, they form rings, and rings induce bending-wave modes in the column. I think this is an interesting example of completely disparate scales. Regarding the last point that Sreeni raised, a turbulent flow can have instability, so I don't know what the question is. Historically, Mark Goldstein and others were doing the instability of the time-averaged flow. This makes sense only if the turbulence timescales are much smaller than the instability timescales. But if you're looking for modes at the timescale that is comparable with the dominant timescale of the large eddy of the coherent structures, then unless you have a floquet theory, the instability of the mean profile makes absolutely no sense, even though for some wakes, people have found some connection between the instability mode, and the instability calculated from the time-averaged profile.

James Wallace: Bob asked me to say a little bit about what Garry presented about the fine scale structure riding on the large two-dimensional vortices in the mixing layer, and also the locality of the Reynolds stress. We made some measurements, some years ago, with this probe, it approximates the velocity gradient tensor (for that flow, the mixing layer) to within five Kolmogorov scales. Using a phase reference, we could extract the phase-averaged velocity field, in the streamwise plane, and you could see very clearly in the velocity vectors that are projected on the streamwise plane, large-scale rollers, streamlines drawn for those. Then, using phase averaging, we could look at fine-scale quantities, like the dissipation rate of the vorticity, the enstrophy, and so on, and spatial relationships to the positions of the large-scale rollers. This just shows the phase-averaged dissipation rate, superimposed in the colour contours onto the plots of the vectors for the rollers, and to my surprise at least, the dissipation rate is largely concentrated inside the rollers. So there are obviously very high three-dimensional activity, and fine scale activity, in the rollers themselves. The lower plot there is the covariance of the streamwise, and cross-stream vorticity, and you can pick up the presence of the streamwise so-called rib vortices here, and then this is a plot of the phase averaged Reynolds stress, and in contrast to the dissipation rate, the Reynolds stress and thus the production rate is around the periphery of the rollers, not concentrated in the centers.

Charles Menevau: Just a comment: we made measurements very similar to these, actually measuring the subgrid-scale energy dissipation, also conditioned on these structures, and also we see this kind of correlation, at scales significantly smaller than the very large scales. There are ways of analysing these coherent structures with statistical conditioning that I think illuminate quite a bit. But definitely there are correlations. I think that when you do simulations, however, those will be included already, because in those regions even the resolved scales will be more energetic, more intense. So, a lot of these things are naturally contained in the standard LES already.

Bob Antonia: Do you see the same as Jim on the dissipation?

Charles Menevau: We have a subgrid scale dissipation instead of the molecular-scale dissipation, and it peaks somewhere in between I guess, it doesn't really peak at the center.

Fazle Hussain: I have a comment. The intensity will get to a maximum in the core, because of the lack of precise alignment, but it is very hard to justify how dissipation can be maximum, because there is no mechanism to sustain the structures in the core. This is essentially vortical flow. Unless you have strain, this cannot be sustained. So most of the dissipation, and Reynolds stress, must be outside, in the braids, or in the ribs. Within the core, you can still have high fluctuation intensity, but there is no mechanism to sustain fine-scale turbulence within the core.

Garry Brown: I would like to emphasize something that I said firstly, the remarkable picture of the mixing transition, measured by molecular mixing. Molecular mixing between reactants in the free stream, and indeed we locate the reactants at particular places within the free stream, you watch the reactants enter the large structure, and then where is all that product? The product is where the dissipation, the molecular diffusion if you like, is high, and from the pictures it's not true that it's strongly located in the periphery, nor is it true that it's strongly located in the center. At least that was our observation. What is remarkable, is that I could go to lower Reynolds numbers, where the mixing, below the mixing transition, where there is practically very very little molecular mixing, and I got the same growth rate for the layer. What that says is that there are components of the Reynolds stress that are small scale, but the largest component, that dominates the growth rate, is coming from the large-scale motion. Now that's true for the mixing layer, and I recognize that the wake is quite different, so I don't want to be confused, but at least the mixing layer that Jim is talking about, I think what he measures is not inconsistent with the measurements that we made of the product, but what I think I would disagree with is that the Reynolds stress, you could easily be confused about that, because the small scales don't actually contribute very substantially at all to the real Reynolds stress, which determines the growth rate of the layer.

Fazle Hussain: I want to inject a point, only Anatol Roshko very recently brought to my notice, that in 1993 I had published a paper with [unclear], which is a complete mixing-length theory without any assumption. You have a dynamical model of a coherent structure, and its rate of evolution, you equate to the strain of the mixing layer. With absolutely no assumptions, this gives Reynolds stress, mean profile, as of the real experiment.

Katepalli Sreenivasan: What does it not give?

Fazle Hussain: Higher order moments, many other things.

Bob Antonia: Bill George is going to make the last few comments.

Bill George: One of the questions I want to ask is: "why do we believe things?" I have told my students many times. I don't want to offend anybody. I was raised in a very conservative religious tradition, and there were certain things, in being a Conservative Christian, one of them was that you were asked to believe certain things that were not exactly... obvious. As you grow up, you start asking questions, "why do we believe this?". The first answer you get is "well the bible says...". That's a lot like turbulence. "Monin and Yaglom say...", "Tennekes and Lumley say...". And then you begin to understand that it's really important that you believe that, because if you don't believe that, you can't belong to the group. The turbulence community has a lot of the same characteristics; if you don't believe in certain orthodoxies, your life can be really difficult, in this community. Having said that, let me go on, because eventually you go to college, and take theology courses, and start learning, you start pushing back: “How did we come to believe this? why did we think it was necessary to believe this?”. Again, that’s a lot like turbulence. And one of the things that happened to me some years ago, is that one of my students asked me these questions, and much to my surprise, I found the answer wasn’t quite what I expected, because having been raised in the fine tradition of Tennekes & Lumley, and Townsend, I always had some questions, like: “in a turbulent shear flow, you always scale the mean velocity this way, and the Reynolds stress was scaled this way”. One day a student asked me: “why did we just assume that?” The answer was, of course, ‘it’s in the bible’, meaning Tennekes & Lumley, and that of course wasn’t satisfactory. So we started digging back, and finally Monin & Yaglom actually provided an explanation. They said imagine a point source of momentum, and a large distance away the only thing that these quantities can depend on, other than the radial distance, is the momentum or drag of the source, and the distance downstream. So you very quickly work your way backwards from this point source argument, and find that these both have to scale with U. Well, one of my students said “Why? Couldn’t it be something else?” And the answer is YES. And you plug that into the equations, and work it out, and find that not only it can be something else, but also it doesn’t have to be a point source of momentum, it can retain a dependence on initial conditions. So we came to believe all of these things because of an analysis that was wrong, or at least, less general than it needed to be. So the question is: “how could we have been so stupid?”. The answer is: “we couldn’t measure Reynolds stress”. So the first 30 years of this idea we only measured mean velocity profiles. And the really neat thing about this is that it is something that any undergraduate can work out really, I’ve tested it at a number of universities, any undergraduate would decide that’s what it has to be. The mean velocity profile is completely independent of r. So no matter how you studied the flow, no matter how much initial conditions affect these flows, they all go to the same mean profile, if you collapse them correctly. The problem is the Reynolds stress. We came to believe we needed independence of Reynolds stress, for all the wrong reasons, and we believed it so long, that it’s like a religion now, we’re afraid to let it go. Now, I am sure there are folks who were asking “how could it be independent of initial conditions?”, and I am sure there are other flows that are very strongly dependent on initial conditions. The wake is an example, the axisymmetric wake is an example. Probably decaying turbulence is an example. I think Sreeni gave us an example. We can argue all that at dinner.