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Coherent structures and modeling: some background comments

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When Fazle Hussain asked us to discuss how we can use the knowledge of coherent structures in boundary layers in modeling, my first thought was, "I don't know the answer to that, and hence I don't want to give a talk." My second thought was, "The lack of an answer to the question is the most disappointing aspect of the work on coherent structures, which has interested me for a long time." After losing a bit of sleep for three nights, I have to tell you I still don't know the answer to the initial question. However, thinking about the question has led me to some ideas that seem to be worth putting forward. First let me ask, "What are coherent structures in turbulent flows?" in order that we have some mutual understanding of what I am talking about. My personal answer to the question is: "Coherent structures are a sequence of events (identifiable motions) in the flow which convert significant amounts of mechanical energies of the mean flow stream, into turbulent fluctuations."

Since there are other possible definitions, let me call this coherent/p structures. These structures have three identifiable parts in time: (a) creation of coherent/p structures (larger structures) from smaller perturbations; (b) creation of smaller scale motions of many scales from the larger motions; (c) decay of the smaller scaled motions owing to viscosity (Kolmogorov scale). Part (a) is an "anti cascade-like" process; parts (b) and (c) are "cascade-like" processes. Thus cascade theories can describe parts (b) and (c) but not part (a). Cascade-like theories so dominated ideas in turbulence research for many years that when we submitted an article to a leading journal around 1970 describing an anti cascade-like process, the reviewers made us delete the remark on pain of refusing publication. So we seem to have learned at least something in the intervening years in as much as few people in this summer institute would now deny the existence of the anti cascade-like processes. It ought to have been evident all along that something, some energetic process, has to create the larger structures in turbulent flows since otherwise all turbulent motions would die out as the flow goes to downstream infinity, and we know that does not happen in cases like the boundary layer and the mixing layer where a source of energy exists to downstream infinity. In those situations, there are repeatable sequences of events, in a statistical sense, that create quasi-coherent structures from the energy of the mean motion.

If we define an instability to be a qualitative shift in the flow pattern in the advected frame, then type (a) processes appear to be "growth-like" local instabilities and type (b) processes appear to be "breakup" (or breakdown) type local instabilities and are secondary or higher order instabilities. The modifier "local" is used to indicate that the instabilities do not alter the whole flow field, as for example a flow separation often does, but rather only affect energy transfer processes within some localized part of the flow field.

It is important not only to recognize that the two types of instabilities are different, but also that it is particularly difficult to study growth type instabilities, experimentally, analytically or in the computer for a number of reasons. Such instabilities typically grow exponentially, and thus are less apt to satisfy Taylor's hypotheses than other flow structures. They change very rapidly from point to point in the flow, and being instabilities they amplify differences in initial conditions. Thus they tend to have very large "jitter" from realization to realization. For both these reasons they will be hard to identify and measure with fixed probes and will tend to have little if an R_{ij} correlation values at significant probe separations. Moreover, and this has not always been fully appreciated, the ensemble averages of the coherent/p structures will only very rarely, if ever, be seen in individual realizations. The situation with respect to ensemble averaging is precisely analogous to that of the mean velocity profile. As the data of Kim, Kline and Reynolds showed and the movie of Abernathy and other sources reconfirm, profiles of $U(y,t)$ in the boundary all deviate from the very well known mean profile $U(y)$. Not only is the situation precisely analogous for the coherent/p structures, but the implications are perhaps even more important since we are concerned with using the coherent/p structures to tell us about the details of the dynamics and

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thus the ensemble averages if taken too literally may mislead us. Yann Guezennec's remarks about ways to formulate the ensemble averages by folding structures with "strong left-handed" elements onto those with "strong right-handed" elements are an excellent example of the dangers and what needs to be done about them.

It is also in the nature of growth-like instabilities in hydrodynamics that they respond to a range of perturbations of input parameters with a range of parametric variation in the outputs. Indeed there is now clear evidence, when one begins to put it together, as S. K. Robinson of NASA Ames and I are currently doing, to show quite clearly that there are several (more than four) quite different kinds of perturbations that can set off the growth instabilities involved in the turbulence production in a boundary layer and that at least three kinds of local instabilities occur. It is not yet clear what fraction each of the three contributes to the totality of production of turbulent kinetic energy in the wall layers.

All this suggests that the coherent/p structures are not just an elephant, but are more like a zoo of various animals, each of which occurs with some variation in size and shape. It is easy to deduce examples that demonstrate this fact, but Professor Hussain's strictures on length of this material preclude those details here. In sum the physics of turbulence production in the boundary layer is of truly great complexity, unlike most physical situations in the world which are simple once they are well mapped and understood.

I summarize this "dismal view" of the problem not only because it strongly affects the way we need to conceptualize the physics, but more particularly because for this discussion it implies several things of importance about the utility of structure information in mathematical modeling.

In discussing the use of structure information in modeling, it will be useful to distinguish three "levels of help" that derive from structure information:

1. Qualitative knowledge that helps guide designs including ideas for flow control: e.g., Riblets, LEBUS, wall curvature, mixing control in jets and cyclones, ...

2. Suggestions about needed precautions in probe measurements, in LES/FTS, and in modeling.

3. Direct use of the features, the sequence of events that create turbulent kinetic energy as the source for mathematical models.

The knowledge of the coherent/p structures has already influenced all the examples of design and control functions listed under 1, and many others not listed, although just how much is hard to say accurately. There is little doubt that the information will continue to serve this function, probably increasingly so as we gain more detailed knowledge.

The knowledge of coherent/p structure at level 2 can, and sometimes has already been critical even though the number of detailed steps involved with that knowledge is small. Some examples include the difficulties with data rate and probe size in lab experiments and grid resolution in FTS that we now know occur as Reynolds number increases owing to the way the coherent/p structures scale.

The most disappointing feature of the several decades of work on coherent/p structures in the boundary layer is the near total absence of use of the information for direct building of mathematical models. So far, outside of some small guidance in the wall model of kuhn and Chapman and important use of the scale information and ensemble average structure model by Perry, Henbest and Chong, there has been very little successful use of the coherent/p information in modeling. Noteworthy attempts have been made by many, including for example Landahl and co-workers, by Beljaars and by Lilley, but thus far these have not led to computations of practical engineering significance. The situation remains that we do most practical modeling via some form of the RANS or equation with closure supplied by models or with even simpler equations such as k-ϵ.

There are then two critical questions. Will this situation continue, or will we be able to build a mathematical model directly on increased information about the coherent/p structures? Second, if we do not build such models, will the coherent/p information be important in the simpler models?

With regard to the first question, I cannot provide an answer, as I noted at the outset; one needs a crystal ball, and mine is cloudy. Only time will tell. One can say this much, the complexities of the problem, some of which are outlined above, mitigate rather strongly against the creation of a complete usable mathematical model built directly on coherent/p information. As we learn more about details in given cases and about a wider range of cases, that becomes clearer and clearer.

However, a little careful thought suggests that even if we do not use the coherent/p information to build a mathematical model directly, that information will be critical in the long run. I have not seen this argument made, and I am grateful to Professor Hussain for making me think hard enough about the problem to see the results a little more clearly.

We need to remember that the full, unaveraged, honest-to-God Navier-Stokes equations plus continuity (and when necessary the viscous energy equation) are an excellent model of turbulent flows so long as the fluid is Newtonian–at least for all the data we have thus far, and this is quite a lot for many cases. If we utilize simpler equations typically via some form of averaging, then those simple equations *must be insufficient for modeling the totality of turbulent flows precisely to the extent that we have lost information in the averaging or simplifying process.* This follows from the fact that the complete Navier- Stokes equations are a good model not only in the sense that they do describe the motion of Newtonian fluids, but *also that they describe no mode; they do not predict excess information suggesting behavior that cannot occur.* Hence, the losses of information are not trivial. This non-triviality is borne out again and again and again whenever we do careful testing of models as, for example, in the 1968 and 1980-81 AFOSR-Stanford conferences but by no means limited to those examples. An enlightening current example appears in the recent dissertation of S. Tzuoo with J. H. Ferziger and the writer. In this work Tzuoo has been able to show that one can build a "unified zonal model" using the k-e model equations which predicts all the available free-shear layer cases (wakes, jets, mixing layers; near- and far-field, planar and axisymmetric) up to the level of profiles of Reynolds stress with no discrepancy of more than 10data and computer output, IF AND ONLY IF the internal constants in the k-e model equations are made explicit functions of two external non-dimensional governing parameters, a generalized Sabin-Abramovitz parameter and a vortex stretching parameter. These results show quite clearly that the key factor is just the kind of variation in the physics from one "zone" of flow to another suggested by the metaphor above about a "zoo of animals".

This line of thought , and the discussions in the session organized by Professor Hussain, lead me to what seems an important conclusion. The most important use of coherent/p information *may* be in making the critical decisions about what *terms can not be dropped* in forming model equations suitable for particular problems. This information is often relatively small in content, and used only for a moment at one point in a long analysis; its importance is consequently often underestimated. Let me give an example of central importance in fluid mechanics to make the point. Prandtl's derivation of the boundary layer equations, generally accepted as the genesis of modern fluid mechanics, depends critically and explicitly on the use of the empirical information that the shear layer thickness is small compared to its length in the streamwise direction. Mathematicians have sometimes argued this can be justified on mathematical grounds; it cannot. At least all such arguments I have seen are circular in some subtle way, and more important, the Navier-Stokes equations themselves are not mathematical in content, but rather use mathematics to provide a model of the physical world. The relevant underlying theory, including the example, are discussed in detail in my own monograph *Similitude and Approximation Theory*, reprinted in 1986 by Springer-Verlag.

In thinking about the future of our knowledge of coherent/p structures and their uses in the boundary layer, I want to make one more remark before drawing conclusions. Given the complexities of the physics described, IN PART, above it is clear why we have had so much difficulty in finding the details of the dynamics and in reaching agreement in the research community about even the kinematic details. Some years ago a strongly concerned government agency asked me if the time was right for a push on turbulence in order to get through the problem I said, "No!" I did not think so because there were not enough good ideas or research tools, and the added manpower, which could be brought to bear by more money, would not be likely to advance matters significantly faster. If asked that question today, I would give the opposite answer. I would do so precisely because the data bases now available at NASA Ames drastically alter the situation. While it is true that the data bases so far only cover a handful of cases and are restricted to unfortunately low Reynolds Numbers, nevertheless they do provide in central, exemplary cases, a means for investigating questions that lay entirely beyond feasibility a decade ago and equally important they allow this to be done with several orders of magnitude increase in speed and decrease in manpower compared with conventional laboratory work. More specifically, when we bring together the existing knowledge on boundary layer structure available in the various research groups such as those of C. R. Smith at Lehigh, R. E. Falco at Michigan State, R. Blackwelder at USC and H. Eckelmann and others at Gottingen and add in the heat transfer results in the groups of R. J. Moffat at Stanford, H. Kasagi at Tokyo, and M. Khabahkpasheva in Novosibirsk, to mention only a few of the more active groups, some clear questions emerge. When we take these questions to the computer data bases of Spalart and of Moin/Kim, as a number of workers in this conference, including S. K. Robinson (whose dissertation will focus on these question), have begun to do, answers very quickly begin to emerge. Some of these results answer questions that have been troubling me for a decade or two, and more important, the reasons why we had not been able to resolve them before are also clear. All this is very promising and augurs very well for rapid progress over the next few years on the question of the kinematics and dynamics of coherent/p structures in turbulent boundary layers.

CONCLUSIONS

1. Understanding of the physics cannot hurt. History tells us it almost always "helps" in important, and sometimes surprising ways.

2. "Help" has been and may remain more at levels 1 and 2 than at level 3. But even at level 3, in direct mathematical modeling, the use of coherent/p information may be critical in deciding what terms *can not be dropped* in simplifying model equations for particular flow zones.

3. These potential gains more than justify the current level of research efforts on understanding coherent/p structures.