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DISCUSSION

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Through the continuing development of the turbulent boundary layer model described here the authors have made valuable contributions to the field of turbulence modelling. The results of boundary layer calculations with this model compare quite well with the available experimental data.

In evaluating any turbulence model, however, it must be remembered that the eventual goal to which most investigators, including the authors, aspire is the formulation of a universal model which will encompass all turbulent flows. Obviously an undertaking of this magnitude will require quite some time. Nevertheless the steps taken now will only lead to this objective if each step is compatible with it. The model proposed by the authors does not appear to meet such a criterion.

The simple fact is that boundary layer predictions are not as sensitive as they might be to the physical assumptions upon which they are based. The results of the Stanford Conference demonstrated this, if nothing else. Complex as boundary layer flows are, there seems to exist a set of rigidly maintained interrelations between flow variables which render physically diverse assumptions operationally equivalent. Therefore a group of methods which are fundamentally contradictory all work, in some cases, despite themselves. To have any hope of universality a turbulence model which is applied to a boundary layer should be regarded as the boundary layer approximation to a complete model just as the mean flow boundary layer equations are related to the full Navier-Stokes equations. The difficulty, of course, is to determine which interrelations between variables are truly global and which are only tenable in a restricted class of flows.

The lack of generality of the authors' model results directly from the primary assumption; $\tau/\rho = a_1 q^2$. Certainly this assumption has a decided simplistic appeal. For that matter the data for many shear flows *appear* to support it. However when considering two-dimensional shear flows it is easy to forget that the shear stress in question is only one of six independent elements in the complete Reynolds stress tensor. On the other hand the quantity to which it has been irrevocably connected is a scalar. Therefore the fundamental tensor nature of the Reynolds stress is destroyed.

Perhaps the simplest way to illustrate this problem is to view the boundary layer flow from another coordinate system as one might in treating a larger problem. Since the Reynolds stress tensor is symmetric it defines a set of orthogonal principal axes. In a boundary layer these are at angles of approximately -25 and $+65$ deg with respect to the x-axis. If a coordinate system is chosen along these principal axes, the off-diagonal shear stress terms of the Reynolds stress tensor are zero. On the other hand, the turbulent energy, q^2, since it is a scalar, is unchanged. Therefore the fundamental postulate upon which the method is based does not stand the simplest test of coordinate invariance. The fact that experimental data appear to support the authors' shear stress-turbulent energy assumption results from the universal convenience of reporting data relative to coordinate axes parallel and perpendicular to the wall and not from any physical significance that this relationship has.

Furthermore, it is reasonable to expect some difficulty with the hypothesis in regions where the coordinate system is approximately aligned with the principal axes of the Reynolds stress tensor. The authors have discussed an example of such a case; the near wake. In the vicinity of the wake center line the principal axes of the turbulence are approximately aligned with the coordinate system and the shear stress approaches zero while the turbulent energy is finite. The authors are therefore forced to make special provisions for this region. They use the relative arbitrary device of specifying a variation of a_1 which goes from $+0.15$ above through zero on the centerline and on to -0.15 below. An alternative method is used in calculating duct flows but the need for a different coordinate system on each side of the plane of zero shear is still evident. This same problem arises in other flows with velocity maxima and minima such as tangential wall jets. Of course in a complicated flow with several such features the user would have to make similar provisions

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for each one separately rather than having the shear stress—turbulent energy relation develop naturally from the equations.

The scalar behavior of the shear stress has caused other problems as well. Perhaps the most obvious of these is in the extension of the model to three-dimensional boundary layers. In order to restore the tensorial character to the shear stress which is now essential, the authors must take a very circuitous route. By comparing their two-dimensional *scalar* shear stress equation with that of the correct tensor component they ascertain the implied form of the two-dimensional pressure correlation terms. Then by analogy they infer the form of the lateral pressure correlation terms in order to complete the equation for the lateral shear stress.

The alternative to all of this is simply to deal with the Reynolds stress transport equations right from the start. Then the structure of the equations takes care of these problems which otherwise must be dealt with individually on an ad hoc basis. Of course, as the authors say, one must then model the pressure correlation terms directly. Actually this presents no additional difficulty since, as the authors have shown, they have in effect modeled them already and even used their model explicitly in the case of three-dimensional boundary layers.

Boundary layer flows are an important special case of the entire spectrum of turbulence phenomena that require attention. An accurate boundary layer prediction method is undeniably a very useful tool. However boundary layers are only a special case. Considering the amount of time and effort devoted to boundary layer turbulence research by the authors, it would be most efficient to be able to apply this approach to more general flows. In this regard the authors' choice of a basic assumption with such severe limitations seems unfortunate.

Authors' Closure

Firstly, we would like to thank Tuncer Cebeci for presenting this paper, in his own inimitable style, at the Winter Annual Meeting.

Secondly, we are grateful to Dr. Herring for his comments. His remarks on the desirable universality of turbulence models will be helpful to people who are currently starting development of such models. Our own turbulence model was conceived in 1964 as an improvement over integral methods of the Abbott and von Doenhoff type then in engineering use, and we were the first workers—as far as we are aware—to use a transport equation for Reynolds stress without invoking an arbitrary eddy viscosity. We hoped that this innovation would permit us to calculate a variety of thin shear layers, and we have done so. Whether or not one should seriously consider the potential universality of a *new* calculation method in 1972, such considerations were not relevant at the time of conception of methods like ours which have now reached a fully developed state. However, it was always clear to us that not only more refined assumptions, but also more transport equations, would be needed in flows obeying the thin shear layer approximation ("complex" turbulent flows [32, 33]).[3] In particular one would need equations for all the Reynolds stresses (or at least all those with significant gradients), so that our empirical factor a_1 *would not be needed.*

Dr. Herring's remarks about invariance are therefore not relevant. At the risk of offending *both* Princeton groups, we would comment that invariance arguments can lead to very strange conclusions: for instance, Dr. Herring's arguments apply equally well to the (noninvariant) boundary layer equations, so the last sentence of Dr. Herring's comments would presumably apply to Prandtl!

Our reasons for using the turbulent energy equation are set out in the section on "Analytical Background." That equation was, *and is*, the only turbulence transport equation whose terms have been measured. Let us say again that the qualitative hypothesis needed to close the exact shear-stress transport equation in a thin shear layer is the same as our hypothesis italicized in "Analytical Background." We simply chose the better-documented equation. The resulting closure can be regarded as a closure of the exact shear-stress equation, and we did so regard it in our three-dimensional work.

In recent years the accumulation of indirect information about the individual Reynolds-stress transport equations has encouraged several groups to model them. We have not been so encouraged, believing that the confident extension of calculation methods to complex turbulent flows requires far more data than are at present available. However, the calculation method of Hanjalic and Launder appears to us—if not to its authors—to be a logical extension of our own ideas. It uses the turbulent energy equation, a shear-stress equation and a length-scale equation; in the thin-shear-layer version described in [34], various Reynolds-stress ratios are taken as constants, but in the complex-flow version all the Reynolds stresses would be obtained from transport equations. We regard Hanjalic and Launder's method as the most promising of the new, post-Stanford generation [35].

Dr. Herring's point about the insensitivity of boundary layer predictions is a good one, although we do not entirely understand his explanation. A simpler explanation is that most of the rise in velocity from wall to free stream is accomplished in the region of validity of the law of the wall, used in some form in nearly all calculation methods. Therefore the assumptions made about the outer layer are not critical. Near separation this is not true and different methods can give widely differing results (Fig. 3). Unfortunately separation predictions were not carefully examined at Stanford because of the unreliability of the data. Jet and wake flows are a more severe test. Mr. T. Morel of Illinois Institute of Technology has further developed our "interaction" technique for multiple shear layers (which does *not* require any special coordinate system) and is presenting results at the NASA Langley meeting on prediction of free turbulent flows.

Finally, we welcome and share Dr. Herring's views on the importance of "complex" turbulent flows. As a result of the rapid progress made in recent years in the use of transport equations for Reynolds stress in simple shear layers, it is now possible for developers of calculation methods to ask sensible questions about complex flows—but the answers to those questions must await an experimental documentation of complex flows at least as thorough as our existing documentation of boundary-layer flows. We feel that the next step toward the goal of a universal calculation method must be taken by the experimenters.

[3] Numbers 32–35 in brackets designate Additional References at end of closure.

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