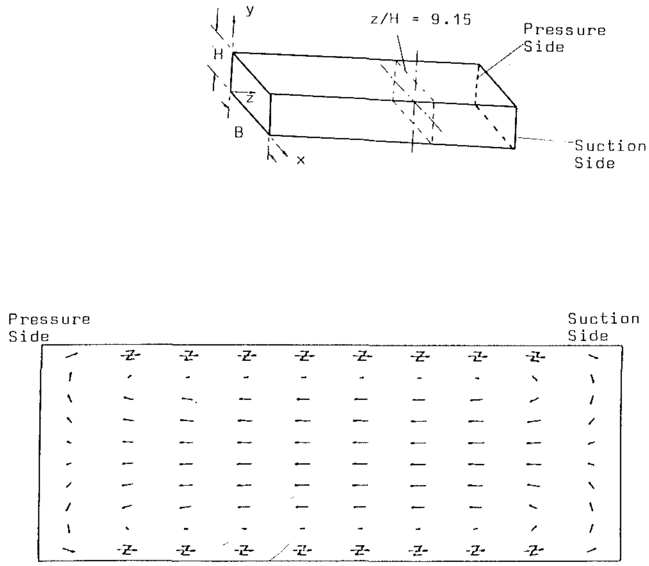

Fig. 16 Predicted secondary velocity field for $\Omega B/\bar{w} = .22$ at $z/H = 9.15$

like (*a*); but it fails to predict the situation (*b*) or any intermediate situation.

With the partially-parabolic procedure, the present authors [17] have also been successful in predicting secondary flows which are in reasonable agreement with the expectation (Fig. 16) and with the measurements (Fig. 17) of Wagner and Velkoff [18].

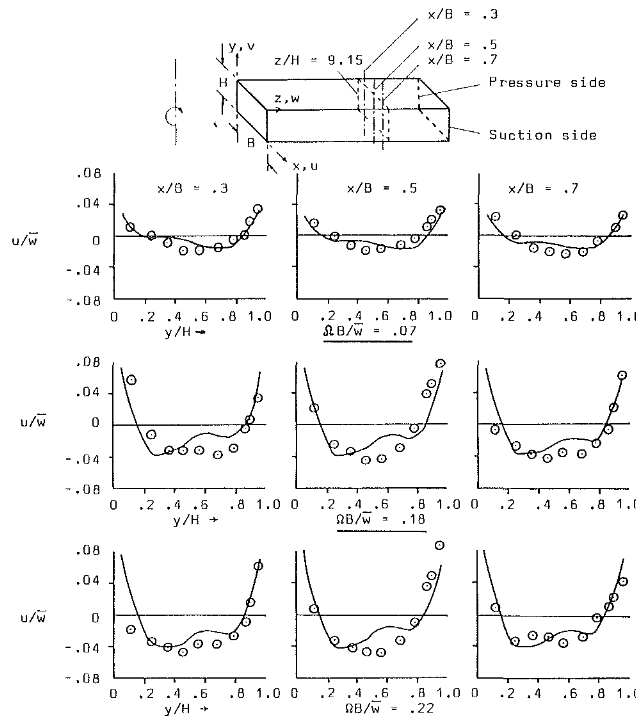

Fig. 17 Comparison of the predicted "blade to blade velocity" distribution (curve) with the measurements (⊙'s) of Wagner and Velkoff at $z/H = 9.15$

K. N. Ghia

For the present set of computations the nearest to the wall point was located at a distance 5 percent of the duct width (e.g. $(x_P/B) = (y_P/L) = 0.05$).

In the opinion of the authors, low-Reynolds-number turbulence modelling is not yet in a satisfactory state. It seems preferable in this region to concentrate on one-equation models; and the work of Hassid and Poreh [20] is suggestive.

The Dean number does not appear to be particularly relevant to the applicability of the *partially*-parabolic method; but the parabolic method should not be used when there are rapid changes in curvature, or rapid changes in Coriolis acceleration, with longitudinal distance.

J. Kotzur

The authors are in agreement with Dr. Kotzur's comment that the deflection angle depends on the ratio of tip speed and average flow speed in the duct. However, the slip factor, which is a measure of the deflection angle, can be predicted accurately without reference to any empirical input either (*a*) by prescribing an appropriate exit boundary condition (Figs. 14 and 15), or (*b*) by extending the calculation domain into the diffuser scroll. The flow in the diffuser scroll can be appropriately handled by employing a periodicity condition in the direction of rotation in a way described by Singhal and Spalding [19].

R. Grundmann

Dr. Grundmann correctly points out the misrepresentation in the x-direction momentum equation which does not contain the component of centrigual acceleration. However, the results are not affected because in computation the centrifugal acceleration was absorbed into the pressure field by redefining the pressure as:

$$P \equiv p - \frac{1}{2}\rho\Omega^2 r^2,$$

where r is the distance from the axis of rotation.

Therefore an incompressible flow calculation is unaffected by the centrifugal acceleration.

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The Calculation of Turbulent Boundary Layers on Spinning and Curved Surfaces[1]

P. BRADSHAW.[2] It is not clear from this very interesting paper whether the authors would recommend applying the same altera-

[1] By B. E. Launder, C. H. Priddin, and B. I. Sharma, published in the March, 1977, issue of the Journal of Fluids Engineering, Trans. ASME, Series I, Vol. 99, pp. 237–239.

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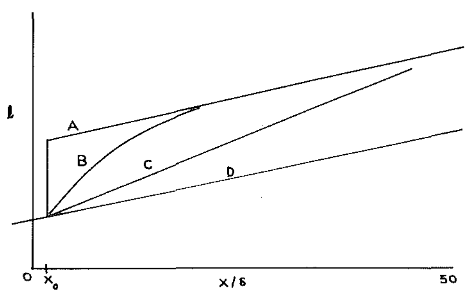

Fig. 12 Response of l to "step change" of curvature at x_0
A—correlation on local Ri.
B—correlation on Ri_{eff} (equation (D4))
C—Launder, et al.
D—growth rate in absence of curvature.

tion of the dissipation transport equation to stress-equation models, like that of Launder, Reece, and Rodi [20]. Irwin and Smith [22][3] showed that in certain cases the modelling of the stress equations would reproduce the large effects of curvature without assistance from extra terms in the dissipation equation. Does Professor Launder now share this discusser's view that this quantitative agreement was a happy accident? There seems to be no justification for using radically different modelling of the dissipation equation in different classes of calculation methods.

The use of a true turbulence time scale in the "Richardson number" was recommended in equation (8) of reference [2] and the need for a lag or transport equation to relate curvature effects to the "Richardson number" was discussed in [1]. Both refinements have been incorporated in our own calculation methods for some years (e.g. [23]) in the form of a correction to the length scale (proportional to $k^{3/2}/\epsilon$ in the present notation; it is usually simpler to think in terms of length scales rather than dissipation rates). More recently [24] we tried, and provisionally rejected, an approach very like that of the present paper.

Briefly, the curvature correction required to give good results in regions of changing curvature should cause the length scale to respond to a step change in curvature in the manner of curve B in Fig. 12. That is, the length scale should initially vary linearly with downstream distance but then settle down to a new near-equilibrium value once the turbulence structure has adjusted fully. The "popular route" described in the Introduction, namely factoring the length scale by a function of Richardson number, gives the instantaneous change to a new "equilibrium" value shown by curve A, while as will now be shown the correction suggested by Launder, et al. gives, at least for small curvature, the response of curve C, with too slow an initial response and no equilibrating mechanism within the length-scale equation.

Writing $l \equiv k^{3/2}/\epsilon$, manipulation of the transport equations for k and ϵ, equations (6) and (7), gives a transport equation for l as

$$\frac{Dl}{Dt} = \left(\frac{3}{2} - c_1\right)\frac{k^{1/2}}{\epsilon}\,\mu_t\,\frac{\partial U_i}{\partial x_j}\left(\frac{\partial U_i}{\partial x_j} + \frac{\partial U_j}{\partial x_i}\right) + c_2 k^{1/2}(1 - c_c \mathrm{Ri}_t) \quad \text{(D1)}$$

+ viscous and "turbulent transport" terms

Note that if $c_1 = 1.44$ the mean strain term in equation (D1) is negligibly small (whether the curvature is small or not) which is plausible because eddy length scales, as distinct from eddy shapes, are likely to be controlled more by turbulent mixing (the $c_2 k^{1/2}$ term) than by mean strain rates. Therefore the c_2 term in equation (D1) dominates, and the main effect of a step change of curvature from zero to $1/r$ is to increase the rate of growth of l by

$$- c_2 c_c \frac{l^2}{k^{1/2}} \frac{V_\theta}{r^2} \cos\alpha \; \frac{\partial(rV_\theta)}{\partial r}$$

The local equilibrium approximation to equation (6) at high R_t and small curvature gives

$$k \approx c_\mu l^2 \left(\frac{\partial V_\theta}{\partial r}\right)^2 \quad \text{(D2)}$$

and using this result the increase of growth rate for the case of small curvature ($\partial V_\theta/\partial r >> V_\theta/r$) becomes

$$\frac{-c_2 c_c}{c_\mu^{1/2}} \frac{lV_\theta}{r} \cos\alpha$$

where the coefficient group equals 1.28, so that for $\cos\alpha \approx 1$ we get

$$\frac{Dl}{Dt} \approx 1.28 \frac{lV_\theta}{r} + \ldots. \quad \text{(D3)}$$

A small step change in curvature therefore causes l to increase linearly with x, with a hint that large destabilizing curvature might cause exponential growth.

Factoring an algebraically-specified length scale by a function of Richardson number, in a form like

$$\frac{l}{l_0} = 1 - 10\,\mathrm{Ri} \quad \text{(D4)}$$

where l_0 is the plane-flow length scale, causes l to increase immediately, on a step change of curvature, by about $-10\,l\,\mathrm{Ri}$.

The simple lag equation explained in [1] can be written approximately, in present notation, as

$$10\delta \frac{d\mathrm{Ri}_{eff}}{dx} = \mathrm{Ri} - \mathrm{Ri}_{eff} \quad \text{(D5)}$$

which defines an effective, "low-pass-filtered" Richardson number for insertion into equation (D4). The initial response to a step change in curvature is

$$\frac{Dl}{Dt} \approx V_\theta \frac{\partial l}{\partial x} \approx -10\,lV_\theta \frac{d\mathrm{Ri}_{eff}}{dx} \approx \frac{lV_\theta}{\delta}\mathrm{Ri} \approx -6.7\frac{lV_\theta}{r} \quad \text{(D6)}$$

where the last step uses the small-curvature approximation of equation (1) with $\partial V_\theta/\partial r = 0.3\,V_\theta/\delta$, a typical value in the outer part of a boundary layer where Ri is largest. The "time constant" of 10δ used in equation (D5) is not exact, but most Reynolds-stress transport equations such as equation (D6) imply values of the same order. For downstream distances of more than 20–30δ, Ri_{eff} tends to Ri and l tends to the value predicted by equations (D4), as seen in Fig. 12.

The above analysis contain many approximations in the interests of getting simple numerical results, but the main objection that can be raised to the derivation of equation (D3) and (D6)—that the natural growth of l, δ and other quantities has been neglected—is not too important in a discussion of the initial response. Fig. 12, which compares equation (D3), (D4) and (D6), would not be much altered if l/δ, rather than l, were plotted in each case. Providing c_c is not too large, the model of Launder, et al. will eventually settle down to a new equilibrium value of l/δ because increased l means increased shear stress and thus increased $d\delta/dx$, but since equation (D3) shows that a streamwise distance of about 50δ would be needed for l to change by the amount predicted by equation (D4) the response will evidently be far too slow. The curvature directly alters the shear stress in the model of Launder, et al. via (3) but the effect is the same as applying (D4) to the mixing length with a factor of 1/2 instead

[3]Number 21–25 in brackets designate Additional References at end of discussion.

of 10 and is therefore negligible. Our own numerical experiments showed that choosing c_c to give a plausibly rapid initial response caused l to grow exponentially until the calculation broke down. We did obtain successful runs with c_c chosen as a function of curvature history so as to simulate the far simpler equation (D5), but would not recommend this for general use.

The reason why Launder, et al. have obtained encouraging results is that most of their test cases do not include rapid changes of curvature. Therefore equation (D3) has had time to reach approximate equilibrium, presumably with an l change of the order predicted by equation (D4): of course (D4) would suffice in such cases. For large Ri, as in So and Mellor's experiment, (D4) gives negative l; in practice l/l_0 must limit at a small positive value and evidently the choice $c_c = 0.2$ gives approximately the correct limiting behavior for So and Mellor's flow. It would be useful to have tests of equation (D3) in flows with rapid curvature changes where some form of length-scale transport equation is really needed.

The key to the problem is the need for a self-equilibrating mechanism to represent the approach to a new equilibrium turbulence structure in the curved flow. Irwin and Smith's set of equations [22] do contain such a mechanism, the "turbulence structure" being represented by ratios of normal stress. Although the extreme sensitivity of their shear-stress equation to the normal-stress ratios does not give one confidence in its general applicability, it does seem likely that the effects of extra strain rates should be represented in the Reynolds-stress equations rather than in the length-scale equation. As stated above, real eddy length scales are likely to be relatively insensitive to mean strain rates as such, and the undeniably spectacular behavior of artificial length scales like $l \equiv k^{3/2}/\epsilon$ in highly-curved flows (e.g. [25]) is unlikely to be reproduced by any equation based on ideas of what real length scales do. Unattractive though the prospect may be, it may prove to be expedient to retain a "real length scale" equation and use auxiliary transport equations—perhaps crude ones like (D4)—for the empirical coefficients in the modelled Reynolds stress transport equations.

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GEORGE MELLOR.[4] I was happy to respond to a request to comment on this paper by my fellow researchers from Imperial College. Dr. Launder and his colleagues are leaders in the field of turbulent flows and, in particular, in the modeling of turbulent flows. However, the present paper has serious problems, in my view.

In assessing a model, it seems to me that it should: predict data—the more flow cases a model covers, the better it is; it should be consistent with generally accepted physical concepts; it should be consistent with other models produced by the same researchers; empirical content should be minimized where possible.

The present model obviously works fairly well—the authors are old pros in the modeling fraternity (in which I, presumptuously, include myself) whose members are adept at making more or less workable models out of any one or two equations of an ensemble of dimensionally homogeneous equations, particularly if one of them includes the turbulent energy equation.

The present model is not consistent with another model by Dr. Launder [26].[5] That model is a simplification of a full Reynolds stress model and is similar to models we have developed [27, 28, 29, 30]. A salient feature is that the stabilizing or destabilizing effect of density stratification in a gravity field can: 1.) be predicted with little empirical adjustment (in our case, none) beyond that required for neutral flow; 2.) for the case where production very nearly equals dissipation, prediction of a critical Richardson number does not depend on the turbulent length scale or time scale and therefore the principal effect would not reside in equation (9) of the present paper; 3.) As interpreted through nomenclature of the present paper, the principal effect of density stratification as revealed in reference [26] or our work is expressed through a dependence of C_μ on density stratification.

The model of reference [26] should automatically include the effects of curvature (as does our model, [29]) a fact made transparent when terms written in rectangular coordinates are written in streamline coordinates. Comments 1.) and 2.) and 3.) above should then prevail for the curved flow case. Therefore, the present paper is inconsistent with reference [26]. Stated another way, either the present paper or reference [26] is wrong.

I have continued difficulty understanding how equation (9) can be justified. A generally accepted physical concept is that, if one relies on a single length scale (or time scale) to describe a turbulent field, it must be a macroscale (or macrotime) which is causative and responsible for the cascade of energy from the energy containing scales to the dissipative end of the spectrum whose scale adjusts to accommodate varying viscosity. However, equation (9) is an equation all about the dissipative end of the spectra—the veritable tail of the dog. All of this can be argued in terms of the large Reynolds number, asymptotic structure of spectra, but the conclusion is the same. I would be most interested in the authors' comments on this matter.

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Authors' Closure

We should like to thank Mr. Bradshaw and Professor Mellor for their contributions. To deal first with Mr. Bradshaw's opening query, we wish to make it absolutely clear that the model

[4]Professor, Aerospace and Mechanical Sciences Department and the Geophysical Fluid Dynamics Program, Princeton Univ., Princeton, N.J.

[5]Numbers 26–30 in brackets designate Additional References at end of discussion.

proposed here is what might be termed a "package deal." That is, the dissipation rate equation is to be applied in conjunction with the Boussinesq turbulent viscosity formula—it is *not* to be used with the stress-transport equations of Launder, et al. [21]. We do not share his view about "... no justification ...". The modification to the ϵ equation is designed to compensate for shortcomings in the Boussinesq stress-strain hypothesis. The range of important flow situations for which satisfactory agreement is secured suggests that the modification possesses sufficient generality to be useful. Even if a satisfactory aternative closure, based on a solution of Reynolds-stress transport equations, were available it would not necessarily be preferable to the present one because it would entail at least twice as much computational effort. In our view it is always good engineering practice to adopt the simplest level of closure that may be relied on to provide sufficiently accurate predictions: in some cases this may entail use of a Reynolds-stress closure, in others the mixing length hypothesis. Our experience suggests that the 2-equation level of closure is the simplest one that will allow all the flows tackled in the present paper to be adequately predicted.

We said above "Even if ... [an] alternative closure were available." There is, in fact, not that alternative which Professor Mellor's discussion seems to imply. For, an important feature of the present model is that it extends into the *low Reynolds number region* immediately adjacent to the wall. At present we know of no stress-transport model that has been subject to more than cursory testing for this region. The calculation schemes with which both discussers are associated confine attention to regions of high turbulence Reynolds number. While, as we noted in the main text, such models (used in conjunction with the law of the wall) serve to calculate the flows over curved surfaces presented here, they are inadequate for treating boundary layers on spinning surfaces considered in Figs. 3–8. (Numerous other flow situations displaying crucial low-Reynolds-number effects are presented in the theses of Priddin [13] and Sharma [31]).[6]

Although we share Professor Mellor's view that, in due course, Reynolds-stress closures should allow a much wider range of turbulence phenomena to be predicted than effective viscosity schemes, the present status of these models is by no means as satisfactory as his remarks indicate. By way of example, we would mention that his predictions of buoyant flows [27] display entirely the wrong effect of stable stratification on the relative normal stress levels while our recent calculations [32] of swirling jets—perhaps the most complex flow yet examined with a stress transport closure—show that the predicted shear-stress field is in substantially worse agreement with experiment than when the Boussinesq relation is used. Even the "success" in predicting curvature effects is only partial. The direct production rate of shear stress by mean strain is

$$-\left\{\overline{v^2}\frac{\partial U}{\partial y}+\overline{u^2}\frac{\partial V}{\partial x}\right\} .$$

Thus, since $\overline{u^2}$ is some 5 times greater than $\overline{v^2}$ near a (plane) wall, the shear stress is indeed particularly sensitive to the secondary strain $\partial V/\partial x$. However, as Mr. Bradshaw notes, the desired factor is about 10 rather than 5. In references [21] and [22] the extra factor of 5 arises from the modeling of near-wall effects on the fluctuating pressure field (an effect consistently neglected in Professor Mellor's work). Irwin and Arnot Smith's calculations of the wall jet on a spiral surface indicate, however, that their model predicts incorrectly the effects of streamline curvature except at modest levels of curvature.

Mr. Bradshaw's discussion of our ϵ equation in terms of a quasi-length scale equation provides useful insights. One needs to be somewhat cautious in discarding mean-strain effects from the l equation, however, for although c_1 differs by barely 5 percent from (3/2), the replacement of the former by the latter would typically decrease the rate of spread of a free shear flow by 20 percent. We note his view that our correction to the ϵ equation will produce a too slow response under conditions of rapid change. We have not noticed such an effect in our predictions to date. As mentioned in the Concluding Remarks, however, our inclination would now be to apply a curvature correction to c_1 rather than c_2 (though for the situations considered here there would be little difference produced).

Finally, in response to Professor Mellor's final paragraph, our "dissipation" rate is perhaps best interpreted as the rate at which energy is being transferred out of the production range of turbulent eddies; that is, it is an equation about the *large* scale motions (Bradshaw's discussion helps emphasize this point). In equilibrium this is also equal to the viscous dissipation rate of kinetic energy. The fact that the two quantities are not exactly the same under nonequilibrium conditions identifies a weakness in the equation; one that, so far as we know, is shared by all current closures. At least the practice of *calculating* a length scale from a transport equation allows a wider range of flows to be treated than does the algebraic prescription used in references [27–30].

[6]Numbers 31 and 32 in brackets designate Additional References at end of Closure.

Additional References

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Review—Progress in Numerical Turbomachinery Analysis[1]

RAMAKANT B. DESHPANDE.[2] This paper adequately summarizes the numerical methods developed in the last couple of decades based on Wu's classical paper using the streamline curvature, finite difference and finite element procedures. The impression that one gets after reading this paper is that at least the inviscid compressible flow problem has been solved with some degree of success either by using fixed grid stream function method or movable grid methods. As pointed out by the author streamline curvature methods are in vogue among industrial companies as it is thought that they are better able to deal with high Mach number flows. In spite of the numerous computer programs available it is not known precisely whether they would obtain a converged solution for any particular blade geometry at a given flow condition. There is no guidance available in the literature regarding the "best" program for a given job. This lack of information hinders the users in either using these programs or modifying them for their particular use.

All the published computer programs use any one of the well-known numerical methods to solve the differential equations and it is general experience that most of these programs suffer

[1]By David Japikse published in the December, 1976, issue of the JOURNAL OF FLUIDS ENGINEERING, TRANS. ASME, Series I, Vol. 98, No. 4, pp. 592–606.

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