**DISCUSSION**

S. J. Gallimore and N. A. Cumpsty

**Note.** The following discussion was written on the paper circulated prior to the oral presentation at the 32nd International Gas Turbine Conference and Exhibit in Anaheim, June 1987. The discussion was made available to the authors in time for them to respond to the comments made. The paper as presented was modified to take account of some of the suggestions made and this is true of the paper to appear in the journal as well. To some extent, therefore, our discussion has been superseded by the paper which now appears. For completeness, it seems more appropriate to submit the original discussion, modified only where we wish to change the emphasis, and allow the evidence of the paper to show where the alterations have taken place.

The authors are to be congratulated on a fine piece of work and an excellent paper. Their research represents a very thorough investigation into mixing and secondary flow in multistage axial flow compressors and provides us with many valuable data. We do, however, have some reservations about the interpretation of the data and these are set out below.

In reference [2] of the present paper we presented evidence of the mixing of ethylene in two compressors. Contours for the better compressor, Compressor A, were presented but for convenience they are reproduced again here as Fig. A1. The similarity between this and Fig. 6 of the paper is very striking. As noted by Wisler et al. the efficiency of Compressor A was not very high at the time when the ethylene tests were carried out, about 86 percent. It should also be noted that the design point results of Wisler et al. were taken at higher flow than for peak efficiency and that at their increased loading condition the conclusions at which they arrive regarding turbulence level across the span are similar to those we came to using Compressor A.

The authors have used the measurement of ethylene concentration contours to infer the relative importance of secondary flow and turbulent mixing. In discussing their ideas we will concentrate on the results obtained near the pressure surface and the outer casing, since this is where the effects were largest. The problem arises from the differences in interpretation based on looking at the core of the ethylene contours and at the shape of the contours. The contours for test 4 shown in Figs. 5(a) and 5(b) and also Figs. 6(a) and 6(b) show considerable distortion of the contour with a distortion radially inward near the pressure surface for the low concentration contour. The arrows drawn in the figures and the discussion emphasize the authors' conclusion that this distortion is predominantly the result of secondary flow. What is very noticeable, however, is that the core of contour 4 has moved very little in the radial direction; what radial motion there has been is in the radially outward direction, the opposite sense to the arrows drawn on the figure. For test 24 with injection near the pressure surface-casing corner there is no radial movement of the core at design point and a small movement from 10 to 14 percent immersion at the operating point with increased loading. Because the injection for test 24 was close to the pressure surface this is a key observation.

One would certainly expect to see the secondary flow reflected in the movement of the core, since the secondary flow will bring about a convection of the entire concentration field, and one does see this in the circumferential movement for many of the tests, but in every case with no more than a small radial movement. We also showed [2] that there was significant circumferential transport by secondary flow but very little in the radial direction. The large arrows in Figs. 5 and 6 are, we believe, very misleading indeed, for the radial component of the secondary flow is small. It follows that its contribution to the radial mixing by the mechanism proposed by Adkins and Smith must likewise be small.

If the secondary flow is not producing the radial distortion of the ethylene contours for tests such as number 4 shown in Figs. 5 and 6 some other explanation must be offered. We believe that the authors have provided just such evidence in their test number 29, shown in Fig. 6(a), in which ethylene was injected from a tapping on the stator pressure surface at 2.5 percent chord. The core of the contours measured downstream has hardly moved radially from the 10 percent immersion point at which it was injected. The radial spread of the contours very close to the surface is very pronounced, being much greater than that in the circumferential direction, and the radial spreading is similar in both the radially inward and outward directions. The absence of a radial shift and of any very pronounced asymmetry of the contours indicates that some mechanism other than convection by radial secondary flow is responsible for the pattern. For injection at the same position but at the increased loading, test 35 shown in Fig. 6(b), the entire pattern is shifted radially inward by about 3 percent of span, and this movement can be realistically attributed to radial secondary flow. The extensive radial spread...
of the contours in tests 29 and 35 must be attributed to locally anisotropic turbulence with larger components in the radial direction than the circumferential. The comparison shown in Fig. 7 is probably very misleading because it compares cases with different, but unknown, levels of turbulence anisotropy.

The anisotropy suggested by the ethylene test 29 is not reflected in the unsteady velocities measured with a hot wire and presented in Table 2(A) which, as the authors rightly point out, indicate very nearly isotropic turbulence. The anisotropy is probably occurring very near to the blade surface so that it could not be measured by the hot wire. The spread of ethylene seen for test 4 in Fig 6(a) is explainable as a circumferential spreading near the casing (partly turbulent and with a significant secondary flow contribution) followed by increased radial spreading by turbulence close to the blade surface. The ethylene spreads shown by the contours are the integrated effect through the whole blade passage and cannot be inferred only from the velocity or turbulence quantities measured downstream of the trailing edge.

Contours for test 4 show that very near to the casing there is a marked circumferential spreading in the direction away from the pressure surface whereas the core has moved toward the pressure surface. The measured velocities in Fig. 11 show that the secondary flow near the casing is toward the pressure surface. We believe that this is further evidence of local anisotropy, this time in the immediate vicinity of the casing wall with the larger component of turbulence being in the circumferential direction. The effect again probably occurred too close to the wall to be measured with the hot wire.

Figure 11 shows the secondary velocities measured downstream of the stator deduced from hot-wire measurements. This shows very high radial velocities in the wake region; at only 5 percent in from the casing wall the radial velocity is about 50 percent of the free-stream velocity. This seems somewhat improbable. We would be interested to know what checks were made in order to verify that the slant hot wire is valid in the very steep velocity gradients which would be found in the wake region. If the measurement of mean velocity is inaccurate does it mean that the turbulence measurements are also suspect in this region?

The radially inward velocities measured at 10 percent immersion near the pressure surface appear very similar to the design-loading conditions, Figs. 11(a) and 11(b). This would seem to be inconsistent with the lack of movement of the core at the design flow (test 24 in Fig. 5(a) and test 29 in Fig. 8(a)) and the radially inward movement at the increased loading (test 24 in Fig. 5(b) and test 35 in Fig. 8(b)). Would the authors care to comment on this?

The authors used the method of Adkins and Smith in a data-match mode to analyze their experimental results. Figure 15 shows excellent agreement between predicted and measured flow exit angles from stator 3. This combined with the reported good agreement with total and static measurements implies that the secondary flow effect on the under/overturning has been predicted well. There are, however, some points that it would be useful to have clarified. The Adkins and Smith model of mixing relies on calculation of radial secondary velocities, some deduced from consideration of radial flow in the blade boundary layer and some derived from the calculated cross passage (i.e., circumferential) flows. Figure 15 shows that the cross passage flows have been well predicted but it does not follow the radial velocities deduced from them are also correct. Considering results of prediction of radial secondary velocities for Compressor B of [2], kindly provided for us by Dr. L. H. Smith, we concluded that the radial components were overestimated, even though the match with measurement of other variables in the flow was good. It would therefore shed some light and perhaps clarify the importance of the secondary flow to radial mixing if the authors would provide a picture of the predicted secondary flow field for comparison with the measurements given in Fig. 11 of their paper. (This also assumes that some confirmation can be given or obtained that the radial velocities measured and shown in Fig. 11, in particular in the wake close to the casing, are correct.)

The calculation of exit total pressure profiles in a throughflow method is affected by several inputs. In particular the input loss distribution and spanwise mixing interact to provide the apparent outlet loss profile across a blade row. This is even true if the loss is derived from measurements upstream and downstream of a blade row; there is no unique combination of loss and mixing. This is illustrated by the fact that for some cases the measured outlet pressure profiles can be predicted satisfactorily by throughflow methods with and without mixing included, but with different input loss distributions. It is therefore necessary to consider the mixing level and the spanwise loss distribution together. Would the authors indicate how they tackled this problem in using the Adkins and Smith method for Figs. 14, 15, and 16?

In [3] one of us (SJG) showed that the results predicted by a throughflow method were not very sensitive to the level of mixing used; it was very important to include some mixing but altering the level produced surprisingly little alteration. The relevant figure from [3] is reproduced here as Fig. A2 and from it can be seen that with only 36 percent of the level of mixing assumed to be correct the predicted temperature profile is sufficiently close to the measured value to be considered satisfactory. (This is one reason that we chose to take the level of mixing as constant across the span since it simplifies the prediction of mixing level and its inclusion in a program without any obvious loss in accuracy.) The corollary to this insensitivity to the level of mixing is that using the data-match method to infer the level of mixing from measured profiles of stagnation pressure and temperature is unlikely to be accurate. In light of this we would suggest that mixing levels shown in Fig. 16 derived from the Adkins and Smith method in data-match mode may not represent either the true level of mixing or the true spanwise mixing profile. We would expect there to
be increased mixing near the hub and casing just as the measurements of the present authors show.

Conclusions

1. The authors are to be congratulated on an excellent piece of research.
2. The evidence they show for the shifts in the positions of the core of the ethylene contours indicates that radial secondary flows are small. On their evidence the radial secondary flow cannot be contributing significantly to the radial mixing.
3. Close to the blades the mixing seems to be anisotropic with larger radial components than circumferential. (Very close to the casing the mixing is also anisotropic but with the circumferential component dominant.) The hot-wire measurements would not have been close enough to the surfaces to be able to detect this.
4. The contours for test 4 shown in Fig. 6(a) are explainable in terms of the nearly isotropic mixing a small distance out from the solid surfaces and anisotropic mixing very close to the surfaces. The authors attribute this feature to deterministic secondary flow near the pressure surface of the blade although there is little evidence of such motion from the ethylene tests with injection on the blade, Fig. 8, or from the measurements of secondary velocities where the radial component seems to be most pronounced on the suction surface.
5. The radial velocities measured in the wake near the outer casing seem to be rather high and we suspect that the hot-wire measurements could be in error because of the high shear in that region. We would like to hear the authors' comments on this.
6. The use of the Adkins and Smith method to infer the mixing level is unsatisfactory because of the demonstrated insensitivity of the calculated pressure profiles, etc., to the mixing level and also because other influential inputs such as blade loss are required in the implementation of the method. We would like to have the authors' comments on this and some clarification on how they input the loss distributions to the program.

Addendum to Discussion

We would like to supplement the original discussion with the following comments:

The revised paper includes a comment dealing with conclusion number 5 about large radial velocities in the wake region.

The appendix shows contour 15 splitting to give one core moving radially and another circumferentially, which clarifies the author's arguments in favor of substantial contributions to mixing by radial secondary flow. We do not dispute the existence of radial components of secondary flow and we welcome any clarification that the authors can provide. What we believe is that the radial secondary flows are generally small and are restricted to localized regions near the blade-surface endwall corners. This is reflected by the fact that only contour 15 shows significant radial motion of the core at the design point while contours 16, 17, 18, and 30 show no substantial movement of the core, although they are very close to where it was seen for contour 15.

We also believe that the prediction method used overestimates the magnitude of the radial components of secondary flow (as was confirmed orally by Dr. L. H. Smith at the meeting) and that the overwhelming contribution to the radial mixing is from a random process, which for brevity we called turbulent diffusion. In this connection we hope that the authors will address our conclusion number 6.

We suggested that anisotropy of turbulence is relevant and hard to measure in a compressor with a hot wire. The anisotropy of turbulence close to the surface is well documented in the literature of boundary layers; it is usual for the turbulent velocities to be substantially larger in directions parallel to the surface than normal to it. As remarked in our discussion, we believe that this can provide an important contribution to the distortion of the measured contours, such as those in Fig. 8 as well as contour 4, and we hope that the authors will address this point. We also hope that they will explain the absence of any substantial radial movement of the cores of contours 29 and 30 when, by the arguments advanced on the basis of contour 15, one would expect such movement.

L. H. Smith, Jr.

I would like to thank my colleagues at General Electric and Professor Okiishi for the depth of the research presented in this paper.

When Adkins and I chose to represent the three-dimensional motions in an embedded blade row with linearized inviscid secondary flow models [1], we knew that these could not possibly account for all the important features of the real flow. We were pleasantly surprised when our method was found to match quite nicely the overturning, underturning characteristics of the circumferential-average flow as shown in our paper and again in the present paper, and use of our analysis is now commonplace in the design and development of compressors at GE.

Until recently there have been few data available on spanwise fluid motions with which to judge the accuracy of our approximations in that regard. The ethylene tracer-gas core location measurements given in the present paper indicate that the symmetric cellular flow pattern of our linearized inviscid secondary flow model is very much distorted and the spanwise velocities are generally lower than calculated, at least in a multistage compressor stator row. The measurements also show that turbulent diffusion plays a major role in mixing, which should surprise no one. But the origin and magnitude of the turbulence then come into question. It is suggested by this discusser that the secondary flows calculated by Adkins and Smith, while not always correct in detail, do spring from phenomena that are bound to agitate the flow and cause turbulent mixing, and that these agitations should be more or less proportional to the strengths of the secondary flows calculated. With this view it doesn't really matter much how the mixing is divided between secondary flow convection and turbulent diffusion; the end result is the same, and that end result has been found to be a satisfactory representation of the circumferential-average properties of the flow.

C. Weber

The authors have shown, beyond any reasonable doubt, that aerodynamic mixing in a blade row of an axial compressor is due to the combined effects of secondary flow (i.e., deviations from the flow field as would be given by a two-dimensional blade-to-blade potential flow analysis) and of turbulent diffusion. For this I believe they deserve our hearty congratulations. Their paper makes it very clear that the experimental activities required to show this constitute a task of very major proportions. I do have one question concerning the analytical method described in the paper and an alternative to it. A little background information leading up to my question follows.

The analytical method the authors used to model the mixing...
was based largely on that given by Adkins and Smith [1]. Of course, they recommend, "... the Adkins-Smith mixing coefficient be modified by adding the contribution of turbulent diffusion and re-evaluate the empirical constants in the model to reduce the effects of secondary flow to again achieve good data matches." There is an alternative method for calculating more directly the combined effects of secondary flow and turbulent diffusion; two examples of papers that illustrate the method are the reference by Hah [W1] and the reference by Rhee [W2]. Both of these papers contain a figure that is identical in spirit to the authors’ Fig. 11 (but for a stationary turbine cascade rather than for an axial compressor blade row). The Adkins-Smith approach is, most certainly, based on a great deal of empiricism. In contrast, the approach via computational fluid dynamics (CFD) exemplified by [W1, W2] is much more direct with far less empiricism (it still has some empiricism in it, though; for example, in the turbulence model). On the other hand, the CFD approach requires far more computational time; thus each method has its advantages and its disadvantages.

My question is this: Based on their experience with the Adkins-Smith type of model for mixing, can the authors comment on the relative merits of continuing work on the development of models like that of Adkins-Smith versus the three-dimensional CFD type of model? Should work continue on Adkins-Smith type models, or should we devote all resources to CFD type models, or should we continue to develop both?

References


B. R. Vittal and A. K. Sehra

The authors of this paper are to be congratulated for a very good piece of work concerning the mixing process in multistage axial compressors. Experimental results presented in this paper are not only useful for better understanding of the secondary flow, turbulence, and mixing phenomena but also provide benchmark data for developing and validating the mixing models. This work is of vital importance to compressor designers.

The basic question addressed in this paper is whether it is secondary flow or turbulent diffusion that is the key element in the spanwise mixing process. Adkins and Smith [1] initially postulated the mixing process as an inviscid phenomena resulting from the convection of fluid properties by secondary velocity field. Gallimore and Cumpsty [2], based on their experimental investigation, concluded that this is not valid and that the mixing is due to diffusion resulting from random, high-intensity turbulence. The authors of this paper, using an experimental procedure similar to that of [2], have now come to the conclusion that both secondary flow and turbulent diffusion play important roles in the mixing process.

Before presenting our observations/comments on the work presented in this paper, we would like to mention, in addition to the above question, several other questions that need to be addressed for modeling the mixing phenomenon in multistage axial compressors. These include: (1) How accurately can we predict the secondary flows in a multistage compressor; (2) what phenomena are responsible for generating the high level of turbulence (or superturbulence) observed in multistage compressors; and (3) is it necessary to accurately model secondary flows and turbulent diffusion for predicting the mixing effect on the spanwise redistribution of temperature and pressure?

Our experience with endwall boundary layer/secondary flow calculations indicates that the predicted boundary layer thickness and associated deviation angles, downstream of the front stage, are considerably different from the experimental measurements. This discrepancy becomes more pronounced in the case of highly loaded, nonrepeating stages. In our view, the current endwall boundary layer and secondary flow models are not able to predict the secondary flow field in a multistage compressor accurately. Furthermore, computing secondary flows due to various phenomena, patching them, and then tracing the fluid properties is a rather cumbersome task. This is not ideally suited for the typically large number of iterations required for establishing stagewise and spanwise work distributions.

As far as the generation of the superturbulence is concerned, we feel that the chopping of the wakes (generated by the upstream blade row) is one of the phenomena responsible for it. Secondary flows, due to tip clearance and endwall boundary layers, are the other candidate phenomena that can play an important role in generating the high level of turbulent intensity observed in multistage axial compressors. This also implies that the superturbulence, generated by any or all of the above phenomena, is perhaps the final process that causes spanwise mixing. Having said this, we would like to present the following observations in support of this hypothesis:

- Through the stator passage, Figs. 5 and 6 do show substantial core migration in the endwall regions of the flow, but these core migrations are primarily restricted to the crossflow direction. In fact, these figures seem to suggest that the fluid particle is not really able to move from the endwall region to the midspan, or vice versa. A possible explanation is that the onset of the crossflow causes a substantial increase in the turbulent diffusion leading to the spanwise mixing. The mixing model presented in [1] is based on the assumption of physical motion of the fluid particle in the spanwise direction and is, therefore, not consistent with the experimental observations presented in this paper.

- In the rotor passage, as shown in Fig. 10, the secondary flows do not seem to be playing a dominant role in spanwise mixing because the rotor chops the incoming wakes and distributes them circumferentially. The spanwise mixing process is then completed by turbulent diffusion. Again this suggests (although the onset of the mixing process is initiated by wake chopping) the final process responsible for spanwise mixing is the turbulent diffusion.

- The authors have inferred from the turbulent intensity levels shown in Fig. 13 that the secondary flows are making a substantial contribution to the spanwise mixing. This is not as clear to us. The turbulent intensity levels are different at hub, midspan, and tip regions at stator 3 inlet for both the design point condition as well as for increased loading. Even at the stator 3 exit, in the case of a design point run, there exists a spanwise gradient of turbulent intensity. Only for the increased loading case, at the exit of the stator, is there no appreciable gradient. This suggests that the skewed contours at the endwalls may very well be due to nonuniform gradients in turbulent intensity.

- The authors have tried to show that there is a fair amount of difference between the mixing coefficients calculated based on turbulent diffusion and secondary flows, with the model of [1] showing better agreement with test values than that of [2].
We are not able to comment on this figure since the exact details of the input used for computing mixing coefficient with the model of [1] are not very clear from this paper. A better comparison on the validity of these models could have been made if the authors had presented the spanwise distribution of temperature instead of mixing coefficient. Gallimore and Cumpsty have shown that the absolute level of mixing coefficient used in the throughflow calculation is not critical. It would have been interesting if the authors had presented additional results comparing the two models on different multistage compressors.

Based on the above observations we feel that although secondary flows play an important role in initiating the spanwise mixing process, the turbulent diffusion is the dominant process that causes the transport of energy and momentum in the spanwise direction. We also feel that Gallimore’s procedure [3] for modeling the spanwise mixing is more suitable for integration with throughflow analysis.

We would again like to congratulate the authors for this valuable contribution for improving the understanding of the mixing phenomenon in multistage axial flow compressors.

G. J. Walker

1 Introduction

The authors have reported a careful and comprehensive study which convincingly demonstrates that both secondary flow and turbulent diffusion may contribute significantly to mixing in multistage axial-flow compressors. This work puts in perspective the relative importance of these two mixing mechanisms and points the way to further refinements in the modeling of mixing in axial turbomachinery.

The principal aim of this contribution is to discuss in more detail the circumferential transport of fluid particles arising from wake dispersion and the relative motion within individual wakes.

2 Wake Dispersion

An idealized model for the dispersion of inlet guide vane (IGV) wakes by an axial compressor rotor, as proposed by Smith [D1], is shown in Fig. D1. Downstream of the rotor, the IGV wake fluid is spread over an avenue of discontinuous segments terminated by the wakes of the rotor blades which have produced their dispersion. Each segment is oriented at an angle to the local mean flow direction due to the longer residence time in the rotor of fluid particles passing over the rotor blade pressure surface. Fluid particles emanating from any fixed source upstream of the rotor would produce a similar streakline pattern (sometimes referred to by British workers as “the old school tie effect”).

The difference in time required for initially adjacent particles to pass over the upper and lower surfaces of an aerofoil in cascade is given approximately [D2, D3] by

\[ \Delta t = \frac{\Gamma}{W_m^2} \]  

where \( \Gamma \) is the circulation per blade and \( W_m \) is the vector mean relative velocity. The circumferential extent of the wake dispersion (indicated by length AC in Fig. D1) is then

\[ \Delta y = \Gamma U/W_m^2 \]  

where \( U \) is the rotor speed. Finally substituting

\[ \Gamma = s_{rotor} W_s (\tan \alpha_1 - \tan \alpha_2) \]  

where \( s \) is the circumferential blade spacing, \( W_s \) is the axial velocity, and \( \alpha_1, \alpha_2 \) are the relative flow angles to axial upstream and downstream of the rotor, gives

\[ \Delta y/s_{rotor} = (1/\phi) \cos^2 \alpha_m (\tan \alpha_1 - \tan \alpha_2) \]  

where \( \phi = W_c/U \) is the local flow coefficient and \( \cos \alpha_m = W_c/W_m \).

Equation (4) may be recast in terms of lift coefficient \( C_L \) by writing

\[ C_L \sigma = 2 \cos \alpha_m (\tan \alpha_1 - \tan \alpha_2) \]

where \( \sigma \) is the solidity, which yields

\[ \Delta y/s_{rotor} = (C_L \sigma_{rotor} \cos \alpha_m)/2\phi \]  

Thus the relative circumferential extent of the dispersion of fluid elements by a rotor is seen to depend on the blade configuration, blade loading, and flow coefficient. For a fixed configuration, \( C_L, \sigma_{rotor} \) and \( \phi \) will vary in a related manner along the machine characteristic and it is valid to say that the dispersion is dependent on blade loading. At the initial design stage, however, there is some latitude for independent variation of the parameters on the right-hand side of equation (5).

Some experimental observations of IGV wake dispersion in a single-stage compressor by Lockhart and Walker [D4] are shown in Fig. D2. Using the observed values of flow angles for this test, equation (4) predicts a circumferential dispersion of 0.39 \( s_{rotor} \). The circumferential spacing of the parallel IGV wake segments (distance AB in Fig. D1) is computed to be 0.28 \( s_{rotor} \); this agrees well with the observed spacing of IGV wake segments in Fig. D2. Smith [D1] reached similar conclusions about the accuracy of this simple model from his observations of flow in a first stage of the General Electric Low-Speed Research Compressor.

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The performance of this model in the multistage environment is examined by comparing with the observations of the present study at midpassage. At the design flow (where \( \phi \) is based on average axial velocity and \( U_i = 0.52 \)) the local pitchline value of \( \phi \) is 0.62; the corresponding rotor relative flow angles at inlet and outlet are 58 and 41 deg, respectively [D6]. (Note from [14] that the rotor blades typically operate at large negative incidence to simulate high-speed flow conditions.) The circumferential dispersion is then computed to be 0.6 \( s_{\text{stator}} \), and this may be compared with the spreading of Core 51 across Rotor 4 shown in Fig. 10(a). Assuming the core spreading to be caused by a combination of isotropic turbulent diffusion and circumferential dispersion by the rotor, the contribution from dispersion is given by the difference in circumferential and spanwise extent of the concentration contours. For the lowest concentration contour in Fig. 10(a) this gives 0.5 \( s_{\text{stator}} \), which is of the correct order but lower than the calculated value. Non-isotropic diffusion in the stator blade boundary layer would have contributed to a lower experimental dispersion value; Agreement would be less good for the higher concentration contours, which are more symmetric.

3 Relative Flow Within Rotor Wakes

Downstream of a rotor there will be a circumferential transport of fluid particles in the absolute frame associated with the relative motion within the rotor blade wakes (directed toward the rotor trailing edge as shown in Fig. D2). This flow is assumed to dominate over those associated with the more highly decayed wakes of blade rows further upstream. Assuming the maximum defect in relative velocity within the rotor wake \( (W_{\text{daw}}) \) is small, and neglecting any interference from upstream blade wakes and downstream stator vanes, the maximum circumferential dispersion which can occur as the flow convects an axial distance \( \Delta z \) is approximated by

\[
\frac{\Delta \gamma}{\Delta z} = \left( \frac{W_{\text{daw}}}{W_2} \right) \tan \alpha_2 \tag{6}
\]

There will be an associated mean flow displacement, but this will be smaller in magnitude by a factor principally dependent on the ratio of rotor wake displacement thickness to rotor blade spacing (typically only a few percent).

There are two significant mechanisms which may modify the relative flow within the rotor wakes and reduce the circumferential transport below that given by equation (6). First, the mixing due to chopped upstream stator wake segments impinging on the rotor wakes acts to restrict the relative flow within the rotor wakes; the avenues of chopped stator wake segments effectively form barriers against which low-energy rotor wake fluid accumulates. This phenomenon, which was examined by Lockhart and Walker [D4] in a single-stage compressor, can be seen occurring at location A in Fig. D2. The relative flows will also be restricted by interposition of a downstream stator causing accumulation of rotor wake fluid on the stator vane pressure surfaces (as at location B in Fig. D2). The latter phenomenon was investigated by Kerrebrock and Mikolajczak [D5] in a single-stage rotor-stator unit. These two mechanisms, although similar in effect, are distinctly different physical processes. They may interfere to some extent and their relative importance will depend on the circumferential location of the upstream stator wake avenues relative to the downstream stator vanes.

Finally, the above model of secondary flow within the rotor wakes is compared with the observations of ethylene core migration through Stator 3 presented in Fig. 5 of the paper. In this case the axial distance between injection and sampling planes is about 1.3 \( s_{\text{stator}} \) and the effective mean value of \( \alpha_2 \), allowing for rotation of rotor wake segments through the stator passage, is about 60 deg. The relative rotor wake defect \( W_{\text{daw}}/W_2 \) is likely to be in the range 0.1 to 0.2 and should increase with loading as the rotor wakes thicken and take longer to decay. On these assumptions, the maximum circumferential dispersion predicted by equation (6) varies from 0.22 to 0.45 \( s_{\text{stator}} \). As in Section 2 above, the calculated circumferential dispersion is compared with the excess in circumferential spreading over spanwise spreading for the lowest concentration contours of core 9. This gives about 0.2 \( s_{\text{stator}} \) for the design loading case in Fig. 5(a) and 0.4 \( s_{\text{stator}} \) for the increased loading case in Fig. 5(b). These figures are comparable in magnitude to the calculated values, and the contour skewing indicates a secondary flow in the expected direction toward the stator pressure surface. The agreement is less favorable for the higher concentration contours, but this is to be expected due to the variation in velocity defect across the wake.

The associated mean core movement would be of order 0.01 \( s_{\text{stator}} \) and quite negligible. The mean core movements at the middle of the stator passage in Fig. 5(a) are generally in the direction of the suction surface, indicating the dominant effect of other opposing secondary motions in the bulk flow. The latter effects are even more marked for the increased loading case in Fig. 5(b).

4 Concluding Remarks

The foregoing discussion of the particular secondary flows associated with wake dispersion and relative motion within wakes supports the authors' hypothesis regarding the relative importance of secondary flows and turbulent diffusion as mechanisms for mixing in axial turbomachines. The simple models presented here are in fair agreement with experiment and indicate that the wake dispersion is the more significant of the two circumferential transport mechanisms. The analysis further emphasizes the dependence of secondary flows on configuration and blade loading that was noted in the paper.

References


K. D. Papailiou

The authors must be congratulated for an excellent work, which, I am sure, will be very useful for future development in wall shear layer research.

Their work concerns, as well, previous work cited in references [1-3] of the subject paper. Consequently, my remarks will necessarily concern these works as well.

Generally speaking, the mechanisms from the literature that result in spanwise mixing are also recognized by the authors and put into evidence by their experimental results. In this respect, I would like to mention that blade-to-blade cross flow and dihedral effects contribute indirectly to intensifying the spanwise mixing mentioned by the authors.

Personally, I would like to make the following remarks concerning the experiments and the experimental setup:

1. The experimental compressor has a high hub/tip ratio, so that the dihedral effects on secondary flows are reduced and, in any case, not really mentioned by the authors.

K. D. Papailiou

Professor, National Technical University of Athens, Athens, Greece.
2 The transport of low-energy material along the blade surfaces or inside the blade wakes (blade boundary layers, wakes, and part of secondary flows) is caused by spanwise static pressure gradients. The corresponding radial movements were recognized very early and experimental evidence of their existence and magnitude can be found as early as 1954 (P1), Figs. 284–287. It is interesting to note that there have been proposed secondary flow optimization procedures [P2, P3] based on preventing this mechanism to function and that the authors' measurements show that, for increased loading, where the spanwise static pressure gradient is stronger, this transport is also stronger.

3 The contribution of the secondary vorticity to secondary flow and the radial and blade-to-blade movements associated with it must be, in my opinion, considered separately from the contribution mentioned in (2) above. For the compressor case, inlet skew (inlet secondary vorticity) is an important parameter, independent of spanwise static pressure gradients. Concerning the authors' measurements, my opinion is that, if the experiments were conducted for nominal speed, the secondary flow effects would be more pronounced, because the contribution of the rotating speed to the upstream row secondary vorticity would have been different.

In addition, it must be pointed out that for other cases (higher camber compressor rows or turbines) the secondary vorticity and the resulting spanwise mixing may be stronger.

4 With respect to spanwise mixing due to turbulence, in addition to any attempt to model it, I would like to point out that it doesn't depend only upon the magnitude of the existing time-wise fluctuations, be they random or not, but also upon their frequency. Bradshaw [P4] pointed out this effect when he evaluated the dramatic difference between the influence of external flow unsteadiness and external flow turbulence on shear layer behavior. Consequently, it could be important to state, along with the time varying quantities, the rms of the part that corresponds to frequencies above, say, 8000–10,000 Hz. Additionally, it would be helpful to possess some knowledge about the effect of frequency upon the spreading rate of ethylene.

5 I do not have at my disposal a complete set of the experimental results, but I would suspect from the general experimental layout that the inlet casing boundary layer is very thick and that it occupies a good part of the blade height. Additionally, in the case of the test stage, I suspect that the hub and tip shear layers are merged, especially for the increased loading case for which they seem to interact strongly. Consequently, free-stream turbulence is a term that cannot be applied here as there is no free stream. In addition the high turbulence levels measured for increased loading seem to be present as a consequence of the strong interaction of the hub and tip shear layers.

With respect to the interpretation of the measurements and the corresponding modeling of the flow, I would like to observe the following:

6 It is important to bear in mind that our model is a circumferentially averaged one (meridional plane model) and that it must, necessarily, contain all contributions to the spanwise mixing. In addition, a turbulent mixing process depends not only upon the value of the diffusion coefficient, but also upon the existing local gradients of the flow quantities that are transported. Consequently, a calibration of the mixing process model is necessary, as, during the circumferential averaging, information is lost and certain terms are neglected. Even when the basic equations are used, as in the case of the calculation method presented in [P5], semi-empirical information is necessary in order to replace this information loss.

I am mentioning this reference for three reasons: (a) The coefficients controlling the diffusion processes are also those controlling the dissipation of kinetic energy, as the theory sug-

B. Lakshminarayana*

The present authors as well as the authors of the earlier papers on this topic (Adkins and Smith, 1982; Gallimore and Cumpsty, 1986; Gallimore, 1986) should be commended for doing a very thorough investigation of the mixing effects in axial flow compressors. I feel that the data and the interpreta-

References


Mixing is a complex phenomenon caused by the mean flow, as well as the periodic and random unsteadiness in the flow. Momentum transport depends upon the local acceleration, convective acceleration, pressure gradient, and viscous and turbulent diffusion. If the flow is uniform (one dimensional) and steady, the mixing is mainly caused by the turbulent motion or molecular motion. Most compressors operate with large velocity gradients in blade boundary layers, wakes, and end-wall regions. In a multistage environment, the cross and radial flow component exit. The mean velocity can be decomposed into a time-averaged value, periodic components (depending upon the history of the flow, this may include components such as \( V_{lp} \) and \( V_{hp} \) due to first and second stages, respectively) and a random component \( V' \) as shown below by the time-averaged momentum equation (say blade to blade)

\[
\frac{\partial V}{\partial t} + U \frac{\partial V}{\partial x} + V \frac{\partial V}{\partial y} + W \frac{\partial V}{\partial z} = -\frac{1}{\rho} \frac{\partial p}{\partial y} + \mu_t \left( \frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} + \frac{\partial^2 V}{\partial z^2} \right)
\]

where \( V = V' + V_{lp} + V_{hp} + V'' \); \( \mu_t \) is turbulent eddy viscosity; \( V' \) is the turbulence component. Neither Gallimore and Cumpsty (1986) nor Wisler et al. (1987) decompose the turbulence component in this manner. Hence, the terminology used in these papers would include not only the random component but also the periodic component. The periodic component is a mean velocity and cannot be considered as turbulence. Hence, there is confusion with regard to the usage of the terminology "turbulent diffusion." Turbulent diffusion should be used only to identify the diffusion caused by the random fluctuation and not by the periodic fluctuation due to the upstream wake.

Vast amounts of data available for turbulent shear flows (e.g., jets and wakes) indicate that both the convection by the mean velocity and the diffusion by turbulence dominate the transport. For example, the turbulent energy budget for a jet shown in Fig. 4.8 of Tennekes and Lumley (1972) indicates that this is indeed true.

The mixing process is caused by both the mean velocity field and the turbulent field. Consider for example the wake mixing, leakage flow, and vortex formation and mixing downstream of a rotor. The wake thickening near the tip of a rotor blade and the wake thinning at the root of a blade, as well as transport of secondary vorticity toward the suction side of the blade, are all caused mainly by the mean motion and to some extent by turbulence. The data presented recently by Lakshminarayana et al. (1987) indicate that the leakage flow and vortex are transported both by mean flow and turbulent fluctuations. Both these phenomena are equally important. The absence of a well-defined leakage vortex downstream demonstrates the effect of turbulent diffusion, distortion, and mixing by radial flows present in the wake. Within the passage, the vortex is transported inward by the radial mean flow. It is well known that a vortex decays and diffuses in both laminar and turbulent flows even though the diffusion is different for the turbulent case. Hence, it is misleading to think that the diffusion is caused purely by the mean velocity field or purely by the turbulent flow field. It is caused by both effects. One effect will dominate over the other depending upon the mean velocity gradient and turbulent intensities.

Wisler et al. and Gallimore and Cumpsty [2] have measured the flow downstream of the stator. In such a case, the turbulent diffusion may dominate over the mean velocity near the midspan, since the spanwise velocities are small in the case of stators. But Adkins and Smith (1982) have included mixing downstream of a rotor, where the mean velocities in the spanwise direction are likely to be substantial. The mixing downstream of a rotor will be dominated by the mean velocity field as well as the turbulent field, depending on the magnitude of the radial velocity or the spanwise velocity. We have taken extensive measurements downstream of a rotor and found considerable radial velocities near the trailing edge (Ravindranath and Lakshminarayana, 1980, 1981; Reynolds and Lakshminarayana, 1979). Hence, both radial velocity and turbulence would be major contributors to the mixing near the trailing edge of the blade. Far downstream, the turbulent fluctuations would tend to dominate the wake mixing as well as the spanwise mixing.

If the mixing is due to turbulence alone, the mixing and distortion of ethylene contours will be random and distorted, unless the turbulence is isotropic. Our measurements in the wake and in the end-wall region suggest turbulence is not isotropic. For example diffusion inside the wake of a stator (Fig. 8) is caused by both turbulence and normal velocity in the wake, as evidenced by equation (1). Even though turbulent diffusion may dominate, the contribution by mean velocity cannot be ignored. For a rotor wake, both these effects are equally important.

What precautions did the authors take to make sure that the ethylene injection technique provided quantitative information? Ethylene injection itself creates turbulence in the field. For example, injection on the blade surface would result in mixing of the mainstream flow with the crossflow jet, resulting in turbulence production and quick spreading of the ethylene gas. How much of this spreading/mixing is caused by this phenomenon and how much of it from the mean velocity and turbulence in the undisturbed stream must be investigated. We have used ammonia for flow visualization on a blade and noticed it is very sensitive to slot shape and injection velocity. If the injection velocity is high, the ammonia jet diffuses very rapidly, providing a thick trace on ozalid paper. The sharp holes likewise accelerated the diffusion processes. It will be useful to evaluate the extent of diffusion close to the injection point as well as several diameters downstream and see what happens before it reaches the blade.

It is interesting to note that prediction of the radial temperature profiles shown in Fig. 7 of Adkins and Smith [1] and Fig. 8 of Gallimore [3] shows remarkable resemblance, yet each author attributes this to entirely different phenomena. This dilemma can be attributed to empirical constants used by these authors. Gallimore utilizes a mixing coefficient one order of magnitude higher than that for a flat plate boundary layer. Such high values are unacceptable from physical considerations. Adkins and Smith likewise use many empirical coefficients. Such empiricism masks the underlying phenomena of mixing and may lead to misleading conclusions.

What is needed at this time is some detailed measurement of the flow field including the turbulence quantities, and evaluation of the transport by mean velocity, periodic unsteady components, and turbulence intensities through the use of the equations of motion and the data. This can be done if the measurements are taken at very close intervals to include such quantities as the turbulence intensity and correlation. This is the only way to resolve the controversy generated by Gallimore and Cumpsty, and Adkins and Smith.
I am firmly convinced, based on the data of Wisler et al. as well as on Penn State data, that mixing downstream of a compressor is caused by both mean velocity components and turbulent diffusion.

References


W. B. Roberts10

Questions

1 Section 4.1 indicates that the overall performance data were obtained at 850 rpm and the ethylene and hot-wire studies were done at a speed of 600 rpm. Furthermore, the paper states that, "tests showed that the difference in Reynolds numbers did not affect the vector diagram quantities and consequently should not affect the conclusions of the work." Could the authors be more specific, i.e., what was the difference in blade chord Reynolds number between the two speeds; was at least one performance data point taken at the lower speed to compare with the higher speed data of Fig. 2; and what other hard data lead to the conclusions stated above?

2 Explain the term "unsteadiness velocities" as used in Section 6.2 and Fig. 14. Is this an approximate indicator of turbulence intensity? If so, is there an approximate relation between the two?

Comments

1 The ethylene trace and hot-wire measurements as seen in Figs. 5, 6, 8, 11, 13, and 14 show that near the design point there is a relatively large "core flow" region with secondary flow confined to the endwalls. This indicates that simple "design point" secondary flow loss and deviation models that use the concept of a core flow combined with a secondary end-wall flow are valid for multistage compressors.

2 The turbulence intensity data shown in Fig. 14 are a significant contribution to the body of knowledge concerning the flow in multistage axial compressors. Were any measurements made of the scale of the turbulence?

3 Section 9.0 states that, "the Adkins-Smith model was used in a data-match mode and certain constants were adjusted to give agreement with measured data..." This and other observations lead me to the conclusion that there is no current model or code that is completely independent of its database.

4 Considering the high quality and quantity of data presented in this paper, it would be greatly beneficial to turbomachinery designers, analysts, and code developers if the blading geometry used in this testing was published or otherwise made available.

Authors' Closure

We greatly appreciate the considerable time, effort, and thought expended by those who provided excellent discussions of our paper. These individuals are authorities in the field and we respect their opinions. We also appreciate having had some of these discussions in time to supply additional supporting data and clarification at the formal presentation of the paper at the 32nd International Gas Turbine Conference in Anaheim. The enthusiastic, thoughtful, and lengthy discussion following this presentation was very useful and gratifying. We are indebted to the session organizer, Dr. Herbert Law, who encouraged and facilitated this discussion. Finally, we continue to enjoy the many hours of in-depth and friendly discussions we've had with Dr. Simon Gallimore and Dr. Nick Cumpsty on this subject.

Having studied the large number of discussions thoroughly and having noted the commonality of certain questions and comments, we have organized our closure by addressing this commonality rather than addressing each discussion separately. This appeared to us to be the more logical approach. Most of the additional supporting figures presented in our closure and in the appendix of the paper were also presented at Anaheim. We believe we have successfully addressed all of the issues raised by the discussers; consequently, all of our conclusions remain unchanged.

The following eight issues were raised by the discussers: (1) data interpretation questions associated with small radial core movement but large radial contour distortions, (2) an alternative explanation for contour distortion by local anisotropic turbulence instead of secondary flow, (3) the effect of the rotor on the mixing process including circumferential transport, (4) random and periodic components and frequency of the turbulence, (5) the effects of Reynolds number on the results, (6) precautions taken to obtain quantitative ethylene measurements, (7) velocity levels in the vane wake, and (8) the Adkins-Smith data match procedure and the mixing models. Each of these issues is addressed below.

1 Data Interpretation Questions Associated With Small Radial Core Motion but Large Radial Contour Distortions. A major question is raised by Dr. Gallimore and Dr. Cumpsty (their paragraphs 4-6 and Conclusion 2) and by Dr. Vittal and Dr. Sehra (their bullet 1) about our conclusion that secondary flow causes the large radial contour distortions. Specifically, Gallimore and Cumpsty correctly point out that Contours 4 in Figs. 5(a, b) show considerable radially inward distortion along the pressure surface while Cores 4 move only a small amount in the radially outward (opposite) direction. However, they then reason that since the secondary flow should be reflected in the movement of the core and the radial core motion is small, it follows that radial secondary flows are also small and therefore cannot cause the large radial contour distortions seen in the data. Thus the secondary flow contribution to the radial mixing mechanism proposed by Adkins and Smith must likewise be small.

While we recognize the logic of the above reasoning and considered it ourselves when originally trying to understand the flow, we think our conclusion that secondary flow causes this distortion near the vane surface is supported by the data. However, this issue cannot be resolved by examining Contour 4 alone. Further insight is needed and this is found in the additional data supplied in Figs. 17 and 18 of the appendix. These data show a transition in the core motion and contour shape from large distortions to nearly circular contours as one proceeds through the endwall region from the casing to 15 percent immersion. Both pressure side and suction side contours are shown. To understand Contour 4, the contour in question, one first needs to examine design-point Contours 17 and 15.

The mirror-image similarity between the pressure-side Cont-
Contour 4 at 7 percent immersion in Fig. 17(a) and the suction-side Contour 17 in Fig. 18(a) is clearly seen in Fig. 19. Both show large, radially inward contour distortion or stretching along the airfoil surface, substantial circumferential core motion and a small amount of radially outward core motion. The cause of the stretching of Contour 17 will become clear when Contour 15 at 0 percent immersion, shown in Figs. 18(a) and 20, is examined.

Injection Point 15 was located at the vane leading edge at the casing. This important data point in Fig. 20 shows a split core motion. In the radial direction Core 15 moves from 0 to 12 percent radial immersion, nearly all the way through the endwall region. This distance is marked “A” in the figure. The radial velocity computed from this core motion is about 10 percent of rotor tip speed. The total radial excursion of ethylene from the injection point is “A + B,” where “B” is attributed to turbulent diffusion. Clearly the radial convection by secondary flow, “A,” and the diffusion by turbulence, “B,” are the same order of magnitude. The significance of the circumferential motion, “C,” was never questioned.

The reason for the spanwise stretching of Contour 17 along the vane surface now becomes clear when the radial lengths “A” and “B” from Fig. 20 are placed on Contour 17 in Fig. 21, beginning at the injection point. “A + B” accounts for the total inward radial spread of ethylene. Even though Core 17 is swept circumferentially by the cross-passage flow and a small amount radially outward, a significant fraction of the ethylene becomes entrained in the radially inward secondary flowfield near the vane surface producing the observed contour distortion. Turbulent diffusion is responsible for the additional spreading.

In fact, the distance “A + B” is shown in Fig. 18(a) to account for all of the radial motion shown inside the endwall region, while the distance “B” is shown to account for all the motion outside this region at 20 percent immersion.

The comparison in Fig. 19 suggests that the Contour 4 pattern is exhibiting similar features. Additional evidence will be presented to substantiate this under Issue 2.

Furthermore, the distance “B” in Figs. 20 and 21 is the same as the minimum distance from the core to the outermost contour. We think that this distance best represents the contribution of turbulent diffusion to mixing in any direction as described in Fig. 7 and Section 8.1.

Gallimore and Cumpsty state in the fifth paragraph of their discussion that they measured very little radial transport by secondary flow, as evidenced by their Fig. A1. In examining this figure, it seems to us that there are very few data from which to draw this conclusion in the critical region from the
casing to their 75 percent height position (see our Section 5.5). Moreover we suggest that their Contour 3 in their Figure A1 moves radially inward along the suction surface for the same reasons that our Contours 15, 16, and 17 do in Fig. 18(a).

Some more of our additional data in Fig. 22 show the full Contours 27 and 28 at the design point (see Fig. 5a). Most of the secondary flow is, as expected, circumferential. However even here, the radial diffusive motion by no means overwhelms the radial convective motion. In fact on average for Contour 27 in Fig. 22(a), the fluid is diffused from 5 to 11 percent immersion and is convected from 5 to 0 percent immersion.

The core motion, we think, shows only the localized aspects of the secondary flow motion. If the core doesn't become entrained in the secondary flow, it doesn't show the motion. Core 15 in Fig. 18(a) was injected at a point where it showed both radial and circumferential motion. Cores 16 and 17 were injected slightly farther from the vane suction surface, were immediately affected by the strong crossflow, and didn't show the radial feature. Cores 1–6 in Fig. 5 were injected at 53 percent stator pitch and were not convected into the regions of secondary flow that distorts the contours.

The contours, on the other hand, show a more global picture. As regions of the contour become entrained in the secondary flow field, they respond, as evidenced by Contours 2, 4, 5, 15, 16, and 17 in Figs. 17 and 18. We have shown in this section that: (1) Secondary flow effects and turbulent diffusion effects are of the same order of magnitude in the endwall region and (2) there can be significant radial stretching of the contours near the vane surface due to secondary flow with little corresponding radial core motion.

2 An Alternative Explanation for Contour Distortion by Local Anisotropic Turbulence Instead of Secondary Flow. Gallimore and Cumpsty (Paragraphs 6–8 and Conclusions 3 and 4) suggest that the distortion of Contour 4 along the pressure surface is caused by local anisotropic turbulence while Vittal and Sehra (bullet 3) advocate that all of the contour skewing is caused by turbulence gradients. We will conclude from the data presented in Figs. 23 and 24 that the skewing caused by these effects accounts for only about 1/4 of the total contour skewing in the spanwise direction. The rest must be attributed to secondary flow.

The extent of mixing by turbulence was approximated in several ways as shown in Fig. 23. First, average random unsteadiness velocities were measured at the locations indicated by the solid dots in the figure (see Section 6.2). The radial components of unsteadiness velocity \( \nu_r \), measured at the stator inlet and exit, were nearly identical. Values of \( \nu_r \) for the stator exit were previously presented in Table 2. The product of \( \nu_r \) and the fluid transit time across the stator represents the average distance for diffusion by turbulence at 90 percent stator pitch near the pressure surface. This diffusive distance is plotted in Fig. 23. Note that the measurements were made at the region in question for Contour 4.

Secondly, ethylene was also injected very near the pressure surface at the locations shown for design-point Contours 24 and 29 in Fig. 24(a). These are the contours that Gallimore and Cumpsty refer to as key observation points. We agree that they are key, but for reasons that are different from those of Gallimore and Cumpsty. We think they are key because they are a direct measure of the maximum amount of spreading that could be caused by turbulent diffusion very near the pressure surface, regardless of whether or not anisotropic turbulence is present. This diffusion distance for Contour 24 is also plotted on Fig. 23.

Finally the measured spreading of Contour 25 at increased loading, shown previously in Fig. 7, is plotted in Fig. 23.
radial motion in Figs. 5(a) and 24(a) is easily explained. Very
near the pressure surface, the cross-passage flow creates a type
of stagnation point at 10 percent immersion. The radial sec­
ondary flow, shown by the arrow in Fig. 6(a), is farther from
the pressure surface for Contour 24. As secondary flows in­
crease for the increased loading point, there is stronger cross­
passage flow and more flow being pushed into the corner as
seen in Fig. 5(b). Secondary flows increase and now, in res­
ponse, Cores 24 and 35 show radial motion with Core 35 mov­ing from 10 to 17 percent immersion in Fig. 24(b). Also the
radial secondary flow, shown for contour 4 in Fig. 5(b), is
now closer to the pressure surface, as one would expect from
the above analysis. Compare Fig. 17(a) with Fig. 17(b).
In summary for Issues 1 and 2, we have shown that:

(a) There can be significant radial distortion of the con­tours
near the vane surface due to secondary flow with little cor­
responding radial core motion.
(b) Secondary flow effects were shown to be of the same order of magnitude as turbulent diffusion effects in the end­
wall region.
(c) With both secondary flow and turbulent diffusion shown to be important in the mixing process, the original con­
clusions in Section 10 of the paper remain valid.
We believe that these findings should be configuration de­
deptent.

3 Effect of the Rotor on the Mixing Process Including Cir­
cumferential Transport. Vittal and Sehra (bullet 2) have con­
cluded from the data in Fig. 10 that secondary flows in the rotor passage do not seem to be playing a dominant role in the spanwise mixing process. We understand how this conclusion would be reached from the data presented. However, the addi­tional data in Fig. 25 show that in the endwall region the rotor influence on spanwise mixing can be significant. The split-core data show that a large concentration of ethylene has been con­
verted from the casing to nearly 8 percent immersion, a radial distance halfway through the endwall boundary layer. The radial velocity computed from this core motion is about 8 per­
cent of rotor tip speed. This implies a strong secondary flow in the endwall region of the rotor.
On another point, one of the important aspects of the secondary flow that has been neglected by some is its influence on circumferential mixing and under/overturining. We have tried to emphasize this in the paper in Section 7.3 and in the last conclusion in Section 10. To this end we have entitled our paper “mixing” not “spanwise mixing.” It is satisfying to see that the results of Dr. Walker’s careful analysis of the cir­
cumferential transport of fluid particles arising from wake dispersion and the relative motion within wakes are consistent with our measurements.

4 Random and Periodic Components and Frequency of
the Turbulence. Prof. Papailiou, Prof. Lakshminarayana,
and Dr. Roberts all addressed some aspects of the turbulence structure.

We agree with those discussers who propose that details of the unsteady (periodic and aperiodic) flow field as well as the time-averaged flow are fundamentally important because of their influence on mixing in the compressor. We have ad­
dressed some aspects of each of these flow components in our paper as clarified below.

Time-averaged flow information helped clarify the in­
fuence of secondary flow on mixing. The ethylene tracer gas results we provided are obviously a time-averaged record of flow mixing that occurred between injection and sampling planes. The hot-wire data displayed in Figs. 11 and Figs. 12(b, c) were also time-averaged. Further, some of these time­
averaged hot-wire data were circumferentially averaged as well and are shown in Figs. 12(a, d).

Phase-lock averaged hot-wire information helped address the influence of the random and periodic nature of the flow on mixing. Phase-lock data appear in Fig. 13. All flow unsteadiness that was not periodic with respect to the rotor blade passing frequency was considered to be a measure of “turbulence intensity.” Turbulence intensity values (equa­tions (1)-(4)) are indicative of the combined extent of fluctua­tion or unsteadiness velocities, \( v_1 \), \( v_2 \), and \( v_3 \) on either side of the phase-lock averaged flow and do not include any unsteadiness due to periodic occurrences such as rotor blade wakes. Some third-stage rotor blade wakes are discernible as regions of higher turbulence intensity in Fig. 13. Rotor wakes from blade rows further upstream could not be detected as easily and were assumed to be mixed out. Also, third-stage rotor wake motions were much less noticeable near the casing at the stator row entrance, near both endwalls at the stator exit, and at all spanwise locations at the stator exit for increased loading flow. Average values of phase-lock averaged, unsteadiness velocity components that were not periodic with respect to rotor blade passing frequency were summarized in Table 2 for fixed locations in the stator 3 exit plane.
The flow unsteadiness data shown in Fig. 14 represent approx­imate values of “total unsteadiness” at different span­wise locations in the stator row. Since an rms average of the continuous analog signal from the hot-wire anemometer was involved, periodic (including rotor wakes) and aperiodic (including turbulence) unsteadiness were included. The result is an indication of the level of fluctuation of flow, at a point in space, about the time-averaged flow field there. The trends in ethylene diffusion we observed were greatly clarified by the unsteady flow data.

We did not attempt to assess the scale of turbulence in the LSRC. Space limitations within the compressor prevented us from using an appropriate probe for this kind of measurement.

From our time-averaged and unsteady flow data we con­
cluded that secondary flows and turbulence were both very im­
portant facets of flow mixing. We also concluded that wake chopping, a periodic flow effect, was another important aspect of mixing.

5 Effects of Reynolds Number on the Results. Dr. Roberts asked for clarification of the effects of the differences in Reynolds number on the results discussed in Section 4.1. Testing was conducted at two blade chord Reynolds numbers of 3.6 and 2.5 \( \times 10^5 \), the latter being used for the ethylene and hot-wire tests. A complete set of overall performance data was taken at each Reynolds number. Except for the expected small reduction in pressure rise capability at the reduced Reynolds number, the features of the two pressure-flow characteristics were identical.

6 Precautions Taken to Obtain Quantitative Ethylene Measurements. In response to Prof. Lakshminarayana's question, the following precautions were taken for the ethylene measurements.
An injection probe was designed following the careful work of Dr. Gallimore [2, 26, 27]. The flame ionization detector (FID) was carefully calibrated and maintained. First the FID was calibrated and tested extensively to verify signal linearity and absence of zero drift. Secondly, FID calibrations were performed often during actual testing. Thirdly, leak checks were performed each day. Lastly, voltage signals were also monitored to watch for signal abnormalities.

To determine the effects of injection rates through the probe and through the surface static pressure taps, we conducted tests in which the injection rates were varied. For example, vane surface injection rates were halved and doubled relative to the test injection flow rate. This resulted in no change in core location and ethylene spreading, both radially and circumferentially. Sampling rates were also halved resulting in no change in core location and, importantly, no change in the extent of ethylene spread.

Periodic FID calibration, careful design and placement of injection and sampling probes, along with proper injection and sampling flow rates yielded the accurate, quantitative ethylene data shown in the paper.

7 Velocity Levels in the Vane Wake. Gallimore and Cumpsty (Paragraph 9 and Conclusion 5) point out that the magnitudes of the radial velocities measured in the stator wake in Fig. 11 seem to be rather high, perhaps improbably high. We agree. In fact, we realized that the original statement in Section 6.1 about the hot-wire calibration limits being exceeded in the stator wake (only) was not strong enough. We added an additional clarifying sentence in this section. However, the direction of this velocity vector in the stator wake and all other velocities are correct.

8 The Adkins-Smith Data Match Procedure and the Mixing Models. Most of the discussers have commented on various aspects of the Adkins-Smith data match procedure, the resulting mixing coefficients, and/or the mixing models. Their comments generally fall into the following categories: (a) procedural, (b) empiricism, (c) insensitivity or inaccuracy with respect to level of mixing and flow details, and (d) ease of application.

8(a) Procedural matters. Some concern was raised by Gallimore and Cumpsty in their Paragraph 12 and Conclusion 6 about how loss was input during the data match procedure because of its potential influence on the results.

In applying the Adkins-Smith analysis method to any design, it is only necessary to specify base (profile) losses representative of the cascade airfoil sections involved and a number indicating the proximity to stall. In the present case, spanwise uniform values of base loss coefficient of 0.025 for rotors and 0.020 for stators were employed, and the design point loading was taken to be 85 percent of the stall loading. The endwall loss model described in the appendix of [1] was invoked automatically by the computer program and this loss was added to the base loss to obtain the total loss before mixing. No “special handling” of losses was involved in the analysis. Comments on the other influential inputs were given in Section 7.1.

The suggestion was made by Vittal and Sehra (bullet 4) that a comparison of the radial distribution of temperature rather than mixing coefficients would have been a better measure of the relative validity of the models. Although we would have liked to do this, it was not practical for at least two reasons. First, detailed temperature distributions are not normally measured in the LSRC because the temperature rise across each rotor row is not large. Instead, work input to the fluid is determined from overall measured torque. Secondly, we did not have access to the Gallimore code to predict a temperature distribution for our test condition. Some discussers have suggested that data bases in the major research organizations be accessed for mixing calculations or that additional comparisons of the two mixing models be made on different multistage compressors. In principle we agree. In practice, the task of getting data released is Herculean.

8(b) Empiricism. It was observed by Dr. Roberts (Comment 3) that there is no current model or code that is completely independent of its data base. This is our understanding also. On the one hand prudent designers must rely on models and codes that are well grounded physically. Although recognizing the importance of distinguishing the details of the flow physics for research purposes, the designer’s primary interest centers on obtaining a satisfactory representation of the circumferential-average properties of the flow. Thus for them, as Dr. Smith stated, it doesn’t really matter how the mixing is divided between convection and diffusion and whether the model is always correct in every detail. This, we believe, is the case for both the Adkins-Smith model and the Gallimore-Cumpsty model. Designers become calibrated with respect to such models. On the other hand, CFD code developers and other researchers need to understand the flow physics in detail. For these individuals detailed flow information, including level and radial variation of mixing, is very useful.

In response to Dr. Weber’s question, we think that for some time to come there will be a need for both empirical methods and CFD approaches; thus development of both should continue.

8(c) Insensitivity/inaccuracy with respect to level of mixing and flow details. The difficult issue of insensitivity to mixing level and inaccuracy, raised by Gallimore and Cumpsty in Paragraph 13 and Conclusion 6 and by Vittal and Sehra in Bullet 4, needs careful evaluation. The sensitivity issue addresses how much the predicted flowfield solution changes when mixing level is changed. The accuracy issue addresses how closely the predicted mixing levels agree with the actual mixing levels.

What we did in our research was to measure the mixing, sort it out as intelligently as we could the relative contributions of secondary flow and turbulent diffusion to this mixing, observe how well the models predict the experimental results, and recommend improvements. The models were exercised with no “special handling” and the reader is referred to Section 9.0 for details.

With respect to the issue of insensitivity to mixing level, Dr. Gallimore [3] concluded that the results predicted by his throughflow method were not very sensitive to the level of mixing used. Because of this, Gallimore and Cumpsty then conclude that levels of mixing inferred from measured profiles of total pressure and temperature by the Adkins-Smith method are unlikely to be accurate.

If only total pressure and temperature were being considered, then to a certain extent we would agree. However, there was more to our analysis than this. These other implications must be considered before conclusions are drawn about insensitivity and inaccuracy.

Several questions need to be answered. How sensitive is the Adkins-Smith method to the level of mixing used? How accurate are the computed mixing coefficients? To what accuracy do users want their answers? How do these findings impact the designer?

With the above in mind, we exercised the Adkins-Smith model by allowing the secondary flows to be computed but by inputting our own levels of mixing. We used radially constant mixing coefficients at each calculation station, with the level set to be a fraction of the mass-averaged value calculated by Adkins-Smith.
This is because the mixing calculation in the Adkins-Smith model was calibrated from a limited data base. Therefore one could obtain a good estimate of the total mixing coefficient by doubling the experimental values shown in Fig. 16 near the endwalls and leaving the remaining experimental values unchanged. After doing this, we conclude that the Adkins-Smith model is a reasonably good predictor of the experimentally determined mixing coefficient, both in mass-averaged level and in radial distribution. However, the predicted values are still high near 20 percent immersion.

Does the Adkins-Smith model overpredict radial velocities? Based on core movements shown in Figs. 5, 20, and 25 and on hot-wire measurements outside of the stator wakes, we compute maximum radial velocities in the endwall region to be about 9–10 percent of the rotor tip speed. This is about the level predicted by the Adkins-Smith model and shown in Table 4 at 10 percent immersion. The predicted radial velocities are too high between 20 and 50 percent immersion. Circumferential velocities are well predicted. The Adkins-Smith model does not predict the details of the secondary flowfield very well as measured in Figs. 5, 6, and 11. In this sense it is inaccurate.

Thus these issues return to the criteria used to judge when the solution is "good enough." This is generally determined by the designer. If the design system being used does not do well in the endwall region and designers are forced to second-guess the flowfield there anyway, then we would agree that the level of mixing doesn't much matter.

However this conclusion should not be made universal. The Adkins-Smith throughflow model has been constructed to give better predictions in the endwall region. Experience assimilated to date using this model is producing confidence that the calculated air angles in this region can be used in setting blade angles. One of the authors (DCW), who is responsible for the design of research blading, thinks that the 7.5 deg variance in incidence angle or blade setting angle described earlier in Table 5 is too large and would have a serious impact upon the design approach.

Mixing levels computed in the design process must be tied to the physics of the flow and must respond to those changes in the design parameters that change mixing levels. If this doesn’t happen, the designer is faced with problems as illustrated below. A parametric study was conducted in which vector diagrams having various levels of mixing were evaluated. For the case where spanwise gradients of circulation were significantly increased relative to a base level, the Adkins-Smith model predicted a significant increase in mixing. However the Gallimore–Cumpsty model, because of the axial velocities and stage lengths involved, predicted a decrease in mixing. The dilemma faced by a designer, who may want a design that increases mixing for example, is obvious.

8(d) Ease of application. For ease of applicability, we fully agree that the Gallimore–Cumpsty mixing model is much easier to integrate into a throughflow calculation.

General Comments

In summary, we have shown that both secondary flow and turbulent diffusion can play important roles in the mixing process in axial flow compressors. While recognizing the very valuable work of Adkins and Smith in secondary flow and Gallimore and Cumpsty in turbulent diffusion, we think that a more complete picture of mixing emerges when there is a synthesis of these two mechanisms as explained in our paper.

We are indebted to the American Society of Mechanical Engineers for providing the international forum in which the paper, the discussions, and our closure can be presented. We hope that this will provide the technical community with a better understanding of this complex mixing phenomenon.