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APPENDIX

Boundary-Layer Integral Parameters

In order to permit the evaluation of the integrals in equation (4), it is necessary to assume a shape for the streamwise and cross-flow velocity profiles. As explained in the foregoing, the profiles suggested by Coles and Mager have been used in this paper. When those profiles are substituted into the expressions for the integral parameters and integrated, the integral parameters become functions of the profile constants.

For example, consider the cross-flow displacement thickness, δ_2^*, given by:

$$\delta_2^* = -\int_0^\delta \frac{\rho w}{\rho_\delta U}\, dy,$$

and since

$$\frac{w}{U} = (1-\eta)^2 \left(\frac{u}{U}\right) \tan \epsilon_w,$$

we have

$$\delta_2^* = -\delta \int_0^1 \alpha(1-\eta)^2 \left(\frac{u}{U}\right) d\eta,$$

$$= -\delta\alpha \int_0^1 (1-\eta)^2[1 + C_1 \ln \eta + C_2(1 - 3\eta^2 + 2\eta^3)]d\eta$$

which gives

$$\delta_2^* = -\delta\alpha\{0.333 - 0.611\, C_1 + 0.2667\, C_2\}.$$

Similarly, for the other parameters, we have,

$$\frac{\theta_{11}}{\delta} = C_1 - 2C_1^2 - 0.5\, C_2 - 0.3714\, C_2 + 1.583\, C_1C_2,$$

$$\frac{\theta_{12}}{\delta} = \alpha[0.611\, C_1 - 0.2667\, C_2 - 1.574\, C_1^2 - 0.2302\, C_2^2 + 1.1067\, C_1C_2].$$

and

$$\frac{\theta_{22}}{\delta} = -\alpha^2[0.20 - 0.9133\, C_1 + 0.3571\, C_2 + 1.335\, C_1^2 + 0.1636\, C_2^2 - 0.8635\, C_1C_2].$$

Since

$$\theta_{12} = \theta_{21} - \delta_2^*,$$

all the integral parameters are known.

DISCUSSION

R. P. Dring[2]

The author is to be congratulated for both the technical merit of the work he is presenting and also for his clarity of presentation. The paper is a description of the analytical development of a useful design tool. It does not become involved with exotic new approaches to the problem. In this context, however, an observation is made as to the improvements that can be gained in the results when the meridional shape factor is used in the entrainment calculation, as opposed to the streamwise shape factor. This observation will have an impact on a wide variety of axisymmetric three-dimensional boundary-layer applications. The reduction of the equations to an equivalent axisymmetric meridional form is another significant result.

The comparisons with experimental and other analytical results appear reasonable. The application to separated and reattaching flows, however, is probably optimistic. The method appears to be successful in getting through a bubble without catastrophic results (e.g., the last example) but as the author points out "the model requires further evaluation." Many aspects of the model are highly questionable in this region where, as can be seen in Fig. 10(*b*), the meridional velocity is negative and the total flow angle (β) does exceed 90°.

D. Japikse[3]

The author is to be congratulated for a serious effort to predict inviscid core and viscous boundary-layer performance in a variety of annular turbomachinery geometries. The author carried out a very fine piece of work for the unseparated boundary-layer case and has introduced a number of new modeling assumptions which appear to be useful, so far as the limited examples demonstrate.

On the other hand, it is felt that the number of modeling assumptions required to treat the separation/reattachment problem, both physically and numerically, is excessive. The single comparison between analysis and experiment for the separated flow problem is too weak to provide any valid confirmation of the techniques presented in this paper. This is not to say that the author's work should be dismissed, but rather that substantial additional experiments and careful comparison are necessary before the techniques can be considered useful. Perhaps the most bothersome physical assumption is the assumption of negligible entrainment in the separated regime and the most troublesome numerical technique is the seemingly arbitrary damping scheme used in the separated flow regime.

The author presented a comparison to Hoadley's [13] annular diffuser study and indicated that poor agreement was obtained probably due to a complicated downstream physical boundary condition. This situation is surprising; proper specification of the boundary condition for swirling flow into a dump chamber is comparatively straightforward and should be manageable with the author's potential flow calculation procedure. This point needs clarification.

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The interstage bend problem was certainly interesting and the difficulty in finding truly useful experimental data is quite understandable. Data with different inlet and exit states would have been quite useful. Did the author attempt to calculate beyond the nominal exit shown in Fig. 9? If so, was he able to predict the reattachment of the hub surface separation?

S. Gopalakrishnan[4]

The author has presented a method for calculating three-dimensional boundary layers that may occur on the stationary end-walls of centrifugal compressors. The author is commended for this contribution, which is perhaps the most advanced method for such calculations. However, its value to the design engineer, in my opinion, is somewhat mitigated by the following circumstances:

(*a*) The flow in the vaneless diffuser (particularly in the large radius ratio diffusers common in industrial compressors) is often characterized by the existence of tangential non-uniformity and unsteadiness. This results from rotating stall-cell patterns in the diffuser, and significant losses in recovery are experienced. The present method, assuming axial symmetry, ignores this problem.

(*b*) The diffuser boundary-layer growth depends on the flow conditions leaving the impeller. These conditions are known only approximately. Therefore, in spite of very careful calculations in the diffuser, the final results are likely to be suspect.

As a consequence, I feel that the results using the method outlined in this paper may not be easily interpreted in terms of useful performance parameters, at least for diffusers.

The results shown in Fig. 8 indicate that the boundary layers on the two end-walls have almost merged. Some of the earlier two-dimensional boundary-layer models assume a velocity profile such that, when the boundary layers merge, the wall flow angle is 90°. Thus, separation at the wall was made to coincide with the full development of the boundary layer. In the author's calculation, did the wall angle reach 90°, at the location referred to in Fig. 8? Further, as the author states, it is typical of radial diffusers to have such a heavy influence of boundary layer on the main flow. I believe that, when the absolute flow angle leaving the impeller is greater than about 70°, the major portion of the diffuser is dominated by fully developed ("separated" flow at the wall) boundary layers. In such cases, boundary-layer theories are inapplicable.

The method in this paper is well described for attached flows. Reduction of the equations into the ordinary differential type, but still retaining the significant three-dimensional features, is an important contribution of this paper. However, the description of the method when separated flow is calculated is not clear. The author has described the mathematical procedures, but the physical interpretation of the treatment is not evident. In general, it is well known that boundary-layer methods become unstable in the vicinity of separation, and even if stability is introduced by some artificial means, the results are unreliable. Usually, the separated flows are treated, not through boundary-layer methods, but using inviscid free-streamline analyses. In this paper, as the separated flow is computed within the restrictions of the boundary-layer theory, the question arises as to whether the stability of the scheme is artificial (perhaps, because of the damping used in the calculation). If so, the general validity of the results cannot be taken for granted. Further, I believe that it will add to the clarity of the paper if some results indicating the calculated structure across the separated layer are included.

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Author's Closure

The author wishes to thank Drs. Dring, Gopalakrishnan, and Japikse for their kind remarks and discerning observations and agree with their skepticism in regard to the treatment of the separating and reattaching internal flow problem.

However, the author stressed in the paper that, in order to make progress in this area, a pragmatic approach must be taken, and that simplifying assumptions will be improved upon as additional experimental data become available. The free streamline analysis, mentioned by Dr. Gopalakrishnan, was tried for this application, a cross-over duct, and found to be extremely unstable, so it was discarded. The assumption of negligible entrainment is perhaps weak, but could easily be changed if a realistic variation could be postulated. The damping scheme used between iterations should not cause concern, because similar techniques are used in virtually all numerical iterative schemes.

The author agrees with Dr. Gopalakrishnan that the flow in vaneless, radial diffusers is dependent upon inlet conditions, subject to rotating stall, and often has fully developed, merged boundary layers. The technique is, however, useful in the design and analysis of radial diffusers in order to provide detailed flow field information up to these limiting conditions. The technique does not arbitrarily relate the wall flow angle to the boundary-layer development; the wall flow angle was not 90° in Fig. 8.

In order to clarify the discrepancy between the experimental and computed results for Hoadley's [13] diffuser, shown in Fig. 5, note the good agreement shown in Fig. 4, where the measured axial pressure distribution was used. The agreement for one iteration does not account for the large boundary-layer thickness relative to the annular height. Hoadley's [13] results were similar to the dashed line in Fig. 5. Although he examined the possibility of the hub separation being caused by the effect of dumping the swirling flow, he concluded that it probably was not caused by that phenomenon. The converged solution shown in Fig. 5 indicates that the diffusion effect alone would not have caused separation, so the author concluded that possibly the dump effect was significant. The analytical technique could not easily model the dump chamber to check out this assumption.

In response to Dr. Japikse's question regarding the interstage return bend, the calculation was not carried beyond the nominal exit. The reason was that this particular bend had vanes in the return channel; so the axisymmetric assumption breaks down.