7 Phillips, A., "The Inelastic Theory of Metal Deformations. Some Thoughts on Its Experimental Foundations," *Proceedings, NSF Sponsored Workshop on Inelastic Constitutive Equations for Metals*, RPI, 1975, pp. 211–230.

8 Phillips, A., "The Foundations of Plasticity," Lectures given at ICMS Udine, Oct. 1974, Springer, (in press).

Author's Closure

The author thanks Professor Phillips for shedding additional light on this work and only wishes to add his own perspective to the comments. The model is intended to represent the gross behavior as mentioned by Professor Phillips, and to be used in large structural computer programs. The loading surface would be fitted to stress values corresponding to plastic strain levels of at least 50 microstrains, a marginally reproducible result from a good industrial materials lab. This would give drastically different subsequent yield surfaces than those reported [1–4] at 3E-6 plastic strain yield surface from an exceptionally careful research lab. For example, the first subsequent yield surface for the S-12 specimen at 70 F in reference [3] would then enclose the origin and have roughly four times the small strain yield surface radius reported. It would not be symmetric about the origin, however, so that a nonisotropic hardening of the loading surface is still needed. An isotropic hardening of the limit surface may be quite reasonable, though, as suggested by Professor Phillips and by Hecker.[4]

The author regrets the failure to mention more than the single citation ([6] of the Discussion) of Professor Phillips' work on two-surface plasticity theories.

[4] Hecker, S. S., "Influence of Deformation History on the Yield Locus and Stress-Strain Behavior of Aluminum and Copper," *Metallurgical Trans.*, Vol. 4, Apr. 1973, p. 985.

Numerical Solution of the Three-Dimensional Navier-Stokes Equations With Applications to Channel Flows and a Buoyant Jet in a Crossflow[1]

A. J. Policastro[2] and W. E. Dunn.[2] The authors deserve congratulations on their careful development of a three-dimensional numerical model for the solution of the Navier-Stokes equations. Their undertaking was indeed a challenging one considering the full Navier-Stokes equations solved and the application of the model to problems of engineering utility. The limited results obtained with the model can be considered successful, in light of the present state of the art. Our comments to follow first involve the model formulation and then the applications, with the purpose of providing a better perspective.

The velocity, vorticity, and temperature formulation of the model avoids the difficult explicit calculation of pressure but requires the solution of seven variables instead of five, had the equations been solved in their primitive form. The authors' method solves more equations with a greater computer storage requirement. The Gauss-Seidel overrelaxation procedure does mitigate the computer requirement somewhat as only the latest value of each variable at a grid point is stored. The tradeoff of vorticity and primitive variable formulations is more favorable in two dimensions since three primitive variables are replaced by only two variables, the stream function and the scalar vorticity.

The uncertainty in the boundary conditions on pressure have been transferred to uncertainty in the vorticity boundary conditions in the authors' formulation. Zero-second derivatives on the velocity components at free stream are probably more realistic than zero first derivatives used, since the open-water boundaries were not here chosen far from the source of discharge (submerged jet case). The rigid-lid surface boundary condition is probably adequate for thermal discharges at the surface but is questionable for submerged shallow water heated discharges. The surface boil and cascading effect may require a free surface boundary condition. It is precisely the shallow-water submerged discharges that provide the greatest interest for model application at the present time.

There are a number of additional issues concerning the model formulation that need further attention and investigation.

1 The present numerical technique should be analyzed theoretically and practically for its stability.

2 The model should be tested for numerical dispersion.

3 A study should be made for the purpose of optimizing the overrelaxation technique.

4 Other differencing schemes should be tested for their stability and accuracy.

5 Other methods for solving the Poisson equation should be tested for possible saving in computer time.

6 An investigation should be made to determine under what conditions the often-used hydrostatic approximation is valid. Under these conditions, a considerable simplification of the model would be possible.

7 The feasibility of making the model time-dependent should be investigated. This would permit more cases of practical interest to be analyzed such as a plume in a tidally influenced current.

8 Other semiempirical turbulence models and more complex turbulence theories (solving the turbulent energy equation) should be considered.

The prediction of square channel entrance flow was indeed quite successful. In heated jet case, the computational grid was too small for the plume had not reached ambient conditions at the grid boundary, ≈32 dia downcurrent. Limited resolution due to finite computer limitations is typically a problem for such three-dimensional applications. The model appears to overpredict dilution as it underpredicts buoyancy and overpredicts bending.

Further applications of the model with complete sets of data should be carried out. Comparison of predictions to competitive models considered to be state-of-the-art should be encouraged. The Chien-Schetz model has considerable merit and deserves further attention and further work.

A. R. P. van Heiningen,[3] A. S. Mujumdar,[4] and W. J. M. Douglas.[5] We wish to point out a basic mathematical limitation of the computational method used by the authors. To eliminate problems associated with the presence of the pressure term in the Navier-Stokes equations the authors transform the primitive equations into their vorticity-velocity formulation. Unfortunately the derivation of equation (1) is not presented in this paper although it is given in detail in the paper's reference [3]. The key step is the derivation of the velocity equations from the continuity equation via partial differentiation. For example, the u-velocity equation is obtained by requiring

$$\frac{\partial}{\partial x}\left(\frac{\partial u}{\partial x}+\frac{\partial v}{\partial y}+\frac{\partial w}{\partial z}\right)=0 \qquad (17)$$

[1] By J. C. Chien and J. A. Schetz and published in the September, 1975, issue of the JOURNAL OF APPLIED MECHANICS, Vol. 42, TRANS. ASME, Vol. 97, Series E, pp. 575–579.

[2] Energy and Environmental Systems Division, Argonne National Laboratory, Argonne, Ill.

[3] Graduate student, Department of Chemical Engineering, McGill University, Montreal, Canada.

[4] Assistant Professor, Department of Chemical Engineering, McGill University, Montreal, Canada.

[5] Professor and Chairman, Department of Chemical Engineering, McGill University, Montreal, Canada.

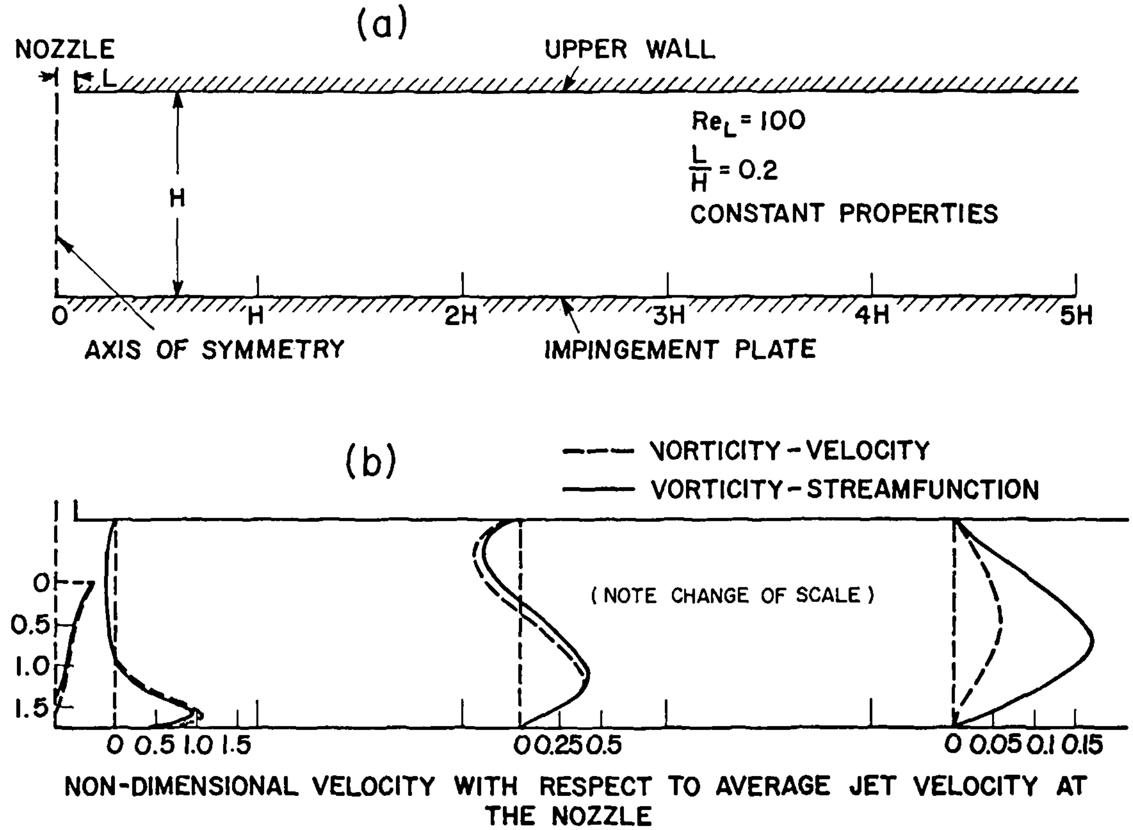

Fig. 1 Velocity profiles in a semiconfined laminar impinging jet

From the definition of vorticity components, they obtain

$$\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2} = \frac{\partial \Omega_y}{\partial z} - \frac{\partial \Omega_z}{\partial y} \tag{18}$$

Equation (18) is the u-velocity equation (No. 5 in Table 1). Equations for v and w velocities are obtained similarly by setting equal to zero the partial derivatives of the continuity equation with respect to y and z, respectively.

It is clear from equation (17) that this formulation does not satisfy the continuity equation since the u, v, and w equations need satisfy

$$\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z} = \text{constant (independent of } x) \tag{19}$$

Equations for the v and w components may satisfy a different set of constants.

Thus it is easy to see that equation (1) (Table 2) cannot satisfy the continuity criterion without imposing additional restraints. We are therefore surprised at the excellent agreement between the authors' computations and the experimental data of Goldstein, reference [6] of the paper. Unfortunately the data presented in this paper are insufficient to test if their predictions satisfy continuity at all locations in their flow system. We hope the authors will present this information in their reply. If indeed they encountered problems with conservation of mass (as we would expect) we would be very interested in the techniques they used to enforce continuity

In order to demonstrate the validity of our argument we solved a two-dimensional flow problem we are currently investigating employing the stream function-vorticity formulation of Gosman, et al., reference [2] of the paper, and the vorticity-velocity formulation presented by the authors. Use of the stream function guarantees mass conservation in the first method. We obtained a solution to the laminar flow field generated by a semiconfined slot jet with a parabolic velocity profile at the nozzle exit impinging on a plane wall. Fig. 1(a) of the Discussion shows a schematic of the flow geometry and the velocity profiles at and near the nozzle and the exit plane. The nozzle Reynolds number was 100, the nozzle-to-plate spacing was five times the nozzle width and the computation was carried to $x = 5H$; (see Fig. 1 of Discussion). Details about the computational procedure for the vorticity-stream function formulation are available in footnote 6.

A computer code was written to solve the two-dimensional velocity-vorticity formulation following essentially the procedure presented by the authors. A hybrid finite-difference scheme was used in preference to the upwind scheme. Fig. 1(b) shows the lack of mass conservation using the authors' scheme. The total outflow is only about 30 percent of the entrance flow through the nozzle. While there is overall annihilation of fluid, there are regions of both fluid generation and depletion. For example, as the jet penetrates into the chamber there is appreciable generation of fluid which is depleted significantly in the outflow region. This computation demonstrates that the vorticity-velocity formulation cannot conserve mass. We do not see any reason why this should not also be true in the three-dimensional situation.

Aside from the fundamental fluid mechanical problem of mass conservation we found that the velocity-vorticity formulation has a slower rate of convergence (even after 350 iterations the solution was not fully converged while the stream function-vorticity formulation converged in about 200 iterations). This is not unexpected since continuity is not satisfied at each iteration.

Authors' Closure

The authors would like to thank Drs. Policastro and Dunn for their kind and thoughtful observations. We are proceeding with

[6] van Heiningen, A. R. P., Mujumdar, A. S., and Douglas, W. J. M., "Numerical Prediction of the Flow and Impingement Heat Transfer Due to a Laminar Slot Jet," ASME Paper No. 75-WA/HT-99.

most, if not all, of the "action items" listed. The extension of the model to unsteady flows is a challenging and exciting task that we are anxious to undertake. Unfortunately, we have not yet been able to find a source of support for that effort. Perhaps a reader can suggest or provide such a source of funds.

The authors appreciate the comments of van Heiningen, Mujumdar, and Douglas, and would like to offer the following observations in reply. First, all of the formulations and finite difference techniques which have been used to solve the three-dimensional Navier-Stokes equations have some advantages and disadvantages. One disadvantage of the vorticity-velocity formulation that we employed is a nonexact satisfaction of the conservation of mass and potential errors in the solution resulting therefrom. We have made a careful study of the matter for problems of the type presented in our paper and have found the overall error to be rather small, ranging from 1 to 5 percent error on total mass conservation across a channel. This is not a negligible error, and it, therefore, represents a cause for a genuine concern. On the other hand, it is not such a severe error that the method should be dismissed out of hand. We should like to state categorically that we never encountered errors in overall mass conservation of the magnitudes reported by the commenters. We are at a loss to explain their poor results, other than to suggest that perhaps it had to do with the details of the finite-difference formulation that they employed or possibly some simple error. A second general point has to do with the suggestion of possibly employing a stream function-vorticity formulation, or, indeed the implication that perhaps primitive variables would be superior in general to the vorticity-velocity formulation employed in our paper. The stream function vorticity formulation is indeed attractive for a two-dimensional case such as the commenters present in their material, but the extension to a genuinely three-dimensional flow problem is by no means trivial. We have seen no evidence to indicate that, on balance, the stream function-vorticity formulation would be superior to that employed in our work for three-dimensional flow problems. If one wishes to go to primitive variables, other difficulties present themselves.

In summary we would like to say that we have found the commenters discussion interesting and helpful. The matter as to an optimum, if there is such a thing, formulation for three-dimensional Navier-Stokes numerical solutions has by no means been resolved, and we intend to continue our efforts in the general field, and look forward to the contributions, comments, and discussions of others toward a final resolution of this important question.