JETS, WAKES, AND CAVITIES

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My talk today is intended as a sequel to the reviews made by Cox and Maccoll and by Gilbarg at the 1956 Naval Hydrodynamics Symposium (1). Whereas "hydroballistics" can be loosely considered as a branch of engineering, and "free streamline theory" as a branch of pure mathematics (specifically, of potential theory), my talk will be from the standpoint of the applied mathematician.* My primary concern is thus to survey mathematical methods for predicting accurately the behavior of real jets, wakes, and cavities—especially the latter, which are the central theme of this part of the present Symposium.

If my tone is that of the "cautious critic" (1, p. 233), it is because I think a careful (not, I hope, agonizing!) reappraisal will help us to discern profitable lines of future work. In this vein, I shall point out a number of specific questions which seem tractable by available methods, provided the necessary interest, ability, and financial support are available.

I shall not try to offer any new and major challenges to intellectual giants; such men usually seek out their own problems. The level of the problems is intended to be that of high-level Ph.D. theses in applied mathematics; this is perhaps the highest level on which research can be systematically planned.

In Gilbarg's review (1), attention was focused almost exclusively on two problems of pure potential theory, which may be roughly defined as follows.

Helmholtz Problem: Given stationary obstacles, walls, etc., having fixed boundaries, find those irrotational, incompressible, steady flows bounded by streamlines which (piecewise) either coincide with these boundaries, or are free (i.e., at constant velocity).

In the Helmholtz Problem, it is understood that the flow behavior at large distances is specified. If we refer to solutions of the Helmholtz problem as "Helmholtz flows," then the second problem is the following.

Helmholtz-Brillouin Problem: Find those Helmholtz flows past given fixed boundaries, in which the maximum velocity is assumed along all free boundaries.

Before I discuss the physical significance of these problems, I shall survey some purely mathematical questions not covered adequately at the 1956 Symposium (1).

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*In the sense of Sydney Goldstein (2).

MATHEMATICAL RIGOR

Though not always attainable in applied mathematics, rigor is desirable—and one should always have a clear idea of the extent to which it has been achieved. Contrary to an impression which seems widespread among physicists and engineers, not even all pure mathematics is equally rigorous. Far from being exceptional, free streamline theory seems to be especially confused today on this fundamental issue. I should like to begin, therefore, with some questions of rigor.

Plane Flows

Even in the simplest case of plane Helmholtz flows, only a few results have been rigorously established. For example, the class of all Helmholtz flows has been rigorously determined, with full generality, only for the case of a single flat plate.* Determinations in other cases involve topological assumptions (usually tacit) about flow separation. Though these assumptions are very plausible, the analysis in the section called Cavity Separation will show that they are actually rather arbitrary.

If the flow topology is assumed known, then plane Helmholtz flows past polygonal fixed boundaries can be treated rigorously by classical methods. In the case of curved fixed boundaries it is, however, much more difficult to establish existence and uniqueness theorems. In this connection, I recall that the first rigorous general existence theorem for the Dirichlet problem was not given until 1899. This was 50 years after Kelvin and Dirichlet had attempted to found the theory on a nonrigorous minimum principle,† and 150 years after potential theory had been usefully applied to solve problems in mathematical physics.

Existence and uniqueness theorems for flows with free streamlines are very much harder to establish. Though Levi-Civita, M. Brillouin, and Villat discerned some of the most important facts in the decade 1905-15, and succeeded in formulating them mathematically‡ in the two-dimensional case, the first rigorous proofs were obtained by Weinstein and Leray in the decade 1925-35. I cannot overestimate the technical difficulty of these proofs; as extended by Kravtchenko, Huron, and others so as to cover more general cases, they run to at least 1000 pages of close and delicate mathematical reasoning. Moreover Leray has informed me that at least one extension (Oudart) is not complete.

In spite of all this work, even the Helmholtz problem has not been solved in all cases; especially little is known about the existence and uniqueness of plane jets from asymmetric nozzles. What is more striking, the only solid obstacle for which the Helmholtz-Brillouin problem has been solved, is the circular cylinder! However, I would not advise any applied mathematician to try to extend these results.

Thin Hydrofoils

More fruitful, it seems to me, would be the rigorous justification of Tulin's "linearized theory" of thin hydrofoils, dealt with elsewhere in this volume. The brilliant applications of Tulin's approximation to engineering design problems should

*E.H. Zarantonello, J. de math. pures et appl. 33:29-80 (1954)
†See O.D. Kellogg, "Potential theory," Ch. XI, 1.
‡As a singular nonlinear integral equation, to which we will recur in the next section. See Ref. 3, Ch. VII, for an exposition of some of the more accessible results. Further references are given there.

not blind us to the fact that it involves all the usual assumptions of the pure mathematician—and an additional linearization as well.

The successful correlation of Tulin's formulas with those of Levi-Civita (3, Ch. VI), besides rigorizing the linearization, should have two other useful consequences. First, it should give better estimates of the errors involved in linearization, and on how to correct them.* Second, it might indicate improved ways to solve the integral equations to which Levi-Civita's formulas lead—including especially the determination of parameters.

Axially Symmetric Case

The methods mentioned above do not give any information about the axially symmetric case, of the greatest importance for hydroballistics. One is, of course, free to conjecture that natural analogs of existence and uniqueness theorems, proved for symmetric plane flows, will also hold for axially symmetric space flows. But such freewheeling conjectures, however suggestive, are not a part of science.

It was therefore most gratifying when what may be termed the "Stanford School" (Garabedian, Lewy, Schiffer, Spencer, Gilbarg, and Serrin) obtained positive results for axially symmetric space flows also. The technical brilliance of their achievements can hardly be overestimated—even after making due allowance for contributions by Riabouchinsky, Lavrentiev, Polya-Szegö, and others to the basic methods involved. However, brilliance does not imply infallibility, and the time has come for sober appraisal of what has actually been proved.

The existence proofs of Garabedian et al. (4) are based on the synthesis of a variational principle due to Riabouchinsky, Steiner symmetrization as recently exploited by Polya-Szegö, and a principle of analytic continuation due to H. Lewy. The details are again delicate and highly technical—even in the plane case, when use can be made of Schiffer's technique of interior variations.

Though I have not myself studied all the details, I have given considerable thought to many aspects of the proofs, and have discussed them with other competent analysts. On the basis of this thought and discussion, it is my impression that only the case of finite cavities has been worked through completely. For infinite cavities, I think many details must be supplied before the proof sketched in (4) can be accepted as rigorous. A complete and clearly expounded proof for infinite cavities would be very valuable.†

Weinstein has called my attention to a much simpler difficulty regarding the uniqueness proofs of Gilbarg and Serrin, with particular reference to the conditions under which "starlikeness of the obstacle implies starlikeness of the free streamline." In the plane, this can be proved rigorously; in the axially symmetric case, its proof by Serrin (5) depends on a plausible principle of potential theory, which has however itself never been rigorously proved. Thus, in the axially symmetric case, a rigorous proof of uniqueness has only been given within the class of flows with starlike free streamlines—contrary to what is implied in Ref. 3, p. 96.

*Offhand, one would expect a quadratic correction term, which could be estimated by a single exact calculation.

†In this connection, see E. Rodemich, Tech. Rep. 67, Applied Math. Statistics Lab. Stanford U., Aug. 8, 1957. A proof of uniqueness for the Helmholtz-Brillouin problem for (say) spheres would be a truly remarkable contribution.

In thinking about this difficulty, I have come to realize that the appeal in Ref. 3, p. 96, to Levinson's theorem (6) about the asymptotic shape of axially symmetric infinite cavities requires that one also assume Levinson's hypotheses. Basically, one must use these to prove the existence of a similarity transformation of one free boundary, making it lie entirely outside the other—except at one or more points of tangency—so as to make Lavrentieff's comparison method rigorously applicable. Presumably, Levinson's hypotheses and "starlikeness" can be combined into one elegant (and plausible) assumption; this would certainly seem worth doing.

In the same vein, statements have recently been made about free streamlines under gravity, which seem to me not rigorously proved. I shall comment on these later in connection with the paper by Taylor and Saffman in this volume.

NUMERICAL METHODS

The recent development of high-speed digital computing machines, with multiplication times of a millisecond or less, has enormously increased the class of economically computable formulas. The impact of this development on applied mathematics is incalculable because it is economically computable formulas that the applied mathematician requires.

Unfortunately, effective rigor in this area is hardly ever achieved, in the sense that rigorous error bounds are usually far too pessimistic. In practice, errors are usually estimated by repeating the calculations with doubled mesh length. This sometimes gives a reliable estimate of the truncation error; in other cases, the significance of the estimate so obtained is only statistical.

I shall now discuss some numerical methods which have been successfully applied to free streamline problems, with special reference to their mathematical accuracy and their potentialities for further development, using high-speed computing machines. In specifying mathematical accuracy, I mean of course the agreement with exact solutions of analytically defined Helmholtz problems—always assuming that these problems have been "well set" in the sense of existence and uniqueness. Some aspects of physical accuracy will be discussed in the following sections.

Integral Equation Methods

Perhaps the most highly developed integral equation method concerns the solution of plane Helmholtz problems by discretization of equivalent singular, nonlinear integral equations. This method has been described in Ref. 8, reviewed in Ref. 3, pp. 215-219, and briefly criticized in Ref. 1, pp. 224-225. The integral equations are those of Levi-Civita and Villat, mentioned in the preceding section.

For symmetric flows past obstacles having curvature of constant sign, very consistent results have been obtained with this method. There is even a rigorous theory* of its convergence (9), though the assumptions involved are not easy to interpret. This is not to say that error bounds have been calculated, or even that error estimates are available. However, the method always gives a potential flow with free streamlines past an obstacle whose shape differs slightly from that of the given obstacle. Therefore to get error estimates, it would suffice to solve the following problem.

*In particular, the singularity at the separation point causes no difficulty, contrary to the assertion in Ref. 1, p. 224.

Problem: Given two functions defining the curvature $K = K_i(\theta)$ as functions of the slope θ, $i = 1,2$, estimate the changes in the flow pattern and pressure distribution due to $\Delta K(\theta) = K_1(\theta) - K_2(\theta)$.

The method involved is capable of various extensions. Thus, various classical problems involving plane flows with free streamlines under gravity, and straight fixed boundaries, can also be transformed into nonlinear integral equations (3, Ch. VIII, §11). Though the singularities involved are nastier than in the case treated in Ref. 3, the problem should be tractable.* We had also originally hoped to apply simple modifications of the method, to calculate asymmetric flows and compressible flows in the Chaplygin approximation (3, p. 191). Adaptations of the method to jet calculations should also be feasible.

By using slope-length equations $\theta = \Theta(\theta)$ in place of curvature-slope relations $K = K(\theta)$, one can interpret plane cavity flows past obstacles having points of inflection (curvature of variable sign) in terms of integral equations (3, p. 136). It would be interesting to perfect a technique for solving such integral equations numerically; in principle, this would seem not too difficult.†

Thin Hydrofoils

An especially timely extension would be to the calculation of asymmetric cavity flows past thin hydrofoils, such as are considered elsewhere in this volume by Drs. Tulin, Johnson, Timman, Cohen, and Di Prima. Such calculations would give a clearer idea of the correction to be made for finite thickness. Conversely, a study of methods effective for the linearized approximation might suggest more effective methods for solving the exact nonlinear equations.

The general case of a smooth asymmetrical hydrofoil in an infinite stream involves three parameters (3, Ch. VI, §7): M, T_o, θ_o. These are presumably determined by the pitch angle of the hydrofoil and the two points of flow separation—though the relation between these and the location θ_o of the stagnation point may be hard to determine, unless the hydrofoil has a sharp leading edge. The angle of pitch should presumably be carried as a free parameter in calculations—but the angles θ_1, θ_2 of flow separation or the condition of "smooth separation" used to eliminate the other two parameters.

For fixed M, T_o, θ_o, the iterative methods described in Ref. 3, Ch. IX, should converge rapidly, provided the hydrofoils involved have a sharp leading edge, since the curvatures involved (hence M) are then small. However, for nonconvex hydrofoils, one would presumably‡ have to use the Villat integral equation.

Axially Symmetric Case

Even more valuable would be the perfection of numerical techniques for solving axially symmetric Helmholtz problems.§ Two approaches to this problem are

*For one case, see Ref. 7 and P. Garabedian, Proc. Roy. Soc. (London) A241:769-80 (1957). The radius of curvature at the bubble vertex seems hard to determine accurately.

†Persons interested in this problem should consult with Dr. Hans Bremermann of Stanford University, who spent some time on it.

‡Perhaps not, if the curvature had constant sign on each side of the stagnation point. Alternatively, one could treat the first six coefficients a_o, a_1, . . ., a_5 of the Levi-Civita function $\Omega(t)$ as free parameters.

§Especially because no analytical solution is known.

reviewed in Ref. 1. In both approaches "certain similarity principles between ... two-dimensional and three-dimensional problems are involved" (1, p. 222).

Approaches based on assumed analogies between plane and axially symmetric flows have long been used to make engineering estimates. For cavity flows, the recent assumptions of Plesset and Shaffer are perhaps best known. The evidence reviewed in Ref. 1, pp. 225-227, shows again that such estimates have a purely engineering significance.

Variable Dimension Number

The preceding analogy can also be put on a scientific basis, as first shown by Garabedian (10). The correct approach is to consider one-parameter families of flows satisfying

$$U_{xx} + \frac{p}{y} U_y + U_{yy} = V_{xx} - \frac{p}{y} V_y + V_{yy} = 0 \tag{1}$$

in $p + 2$ dimensions, with p a continuous parameter. This idea of considering flows in fractional dimensions,* which would not normally occur to a physicist or engineer, illustrate the practical value of pure mathematical abstractions.

Garabedian's methods and conclusions are summarized in Ref. 3, pp. 288-289, except that Gilbarg fails to emphasize Garabedian's plausible but arbitrary interpolation with respect to $\delta = p/(p + 2)$. Because of this arbitrariness, though I believe that Garabedian has obtained genuine estimates for corrections to the widely quoted results of Plesset and Shaffer (and of Trefftz for jets), I am not convinced that these estimates can be relied on beyond ±30 percent.

It would be very valuable if Garabedian could extend his method so as to obtain asymptotic estimates of the relative length ℓ/d and diameter d_m/d of cavities behind discs of diameter d as functions of the cavitation number Q. The extension to other obstacle shapes seems to involve formidable difficulties.

Difference Equation Methods

Most promising, in my opinion, are attempts to calculate Helmholtz flows approximately, by covering the domain involved by a fine mesh, and replacing the Laplace equation over this mesh by a suitable difference equation. In both the plane and axially symmetric case, this procedure (with due attention to boundary conditions) leads to vector equations $A\vec{u} = \vec{k}$ involving well-conditioned symmetric matrices. Clearly, such equations are most tractable for small domains—e.g., for flows having finite cavities in finite channels. By way of contrast, the methods based on integral equations and variation of the dimension number $n = p + 2$ are most effective for infinite cavities in an unbounded stream. Therefore, their ranges of application are essentially complementary: neither is to be thought of as a substitute for the other.

Relaxation Methods

The approximate numerical solution of Helmholtz flow problems, through approximating difference equations, was first accomplished (using hand "relaxation methods")

*Previously applied to other problems by Beltrami, Bers and Gelbart, and Weinstein (Trans. Am. Math. Soc. 63:342-354 (1948), where earlier references are given).

by Sir Richard Southwell and Miss Vaisey (11), in an important pioneer paper. Though not mentioned in Ref. 1, Ref. 11 seems to deserve further study at the present time, for two reasons.

First, many of the approximate solutions obtained in Ref. 11 can now be compared with results obtained by other methods, and this will give a better idea of the truncation errors involved—a subject about which little information is now available.* Thus, such a comparison is possible for the two-dimensional Borda mouthpiece in a channel: Example 1 of Ref. 11 is tractable analytically as described in Ref. 3, Ch. V, § 8. Though a numerical parameter (ratio of mouthpiece width to channel width of 1/6) must be determined a posteriori in the exact analytical formulas, the amount of work involved is not really great. Again, as to the cusped cavity behind a circular cylinder, treated as Example 10 in Ref. 11, one can use the formulas of Ref. 3, Ch. VI, § 10, to reduce the problem to the solution of a nonlinear integral equation involving two parameters M and Q, connected by an implicit relation. For one value of Q, this equation and the relation have been solved numerically (8, Case 22a); it would be interesting to make a comparison with the relaxation solutions† for $Q = 0.234, 0.562, \ldots$, as well as for the infinite cavity of zero drag.

Analytically, the various plane flows with free streamlines under gravity treated in Ref. 11 as Examples 4-8 are also reducible to mildly singular integral equations, which should be tractable by iterative numerical methods, as explained above.

Second, the numerical labor required to "relax" has been so shortened in many cases with the help of ultrafast computing machines, that it seems reasonable to hope that this will also be true of the "free streamline" flows treated in Ref. 11. The general procedure followed, in both hand and machine computations, is to solve a sequence of problems involving fixed boundaries, which are adjusted by successive trials until the condition of constant velocity is met on the (then) free streamline. Since the difference equations are the same, the only question is as to how effectively one can substitute regimented automation for free human intuition.‡

Very substantial contributions to the automatic solution of Helmholtz problems are embodied in Ref. 12. Using a square mesh, to which the successive overrelaxation (extrapolated Liebmann) method of Young and Frankel is ideally adapted,§ "Riabouchinsky" flow past a disc in a channel was treated on the NORC at Dahlgren.

Surprising though it may seem, the NORC calculations did not give results which were clearly better than those previously obtained by Miss Vaisey (3, p. 232). In fact, no convergence proof or error estimate is available for either method, and somewhat variable results were obtained by both, so that the problem is still unsolved. I should like to make some specific comments in this connection; I understand that Dr. Arms of the Naval Proving Ground also has some ideas. I believe that reconsideration of convergence speed and truncation errors, in the light of these comments and ideas, would make a successful attack possible in the near future, using new and more powerful machines.

*Even for solutions of the Dirichlet problem; the case of free boundaries is much more difficult!

†See R.V. Southwell, "Relaxation Methods in Theoretical Physics," Oxford, p. 226, 1946. A concise revision of much of Ref. 11 is presented in pp. 212-26.

‡Human intuition has many advantages of flexibility--as in the easy introduction of local mesh refinements suggested by experience.

§Thus, with the PDQ-03 Bettis Code, and rectangular meshes (with independent spacings in x and y), in rectangular domains, fixed boundary problems involving 7500 points can be solved on the IBM-704 in 10 to 15 minutes!

Perhaps the major computational difficulty centers around the (mild) separation singularity. The square mesh usually used in Ref. 12 is not well adapted to treating the local behavior near this singularity.* Successive overrelaxation is also compatible with rectangular subdivisions with unequal spacings, which could be used to concentrate mesh points near the separation point. On the other hand, the type of local mesh refinement used in Ref. 11—which seems better adapted to the problem—does not satisfy Young's property A: that net-points can be divided into "red" and "black" points, such that adjacent net-points have opposite colors. Other mesh refinements having property A might be tried, such as those sketched in Fig. 1.

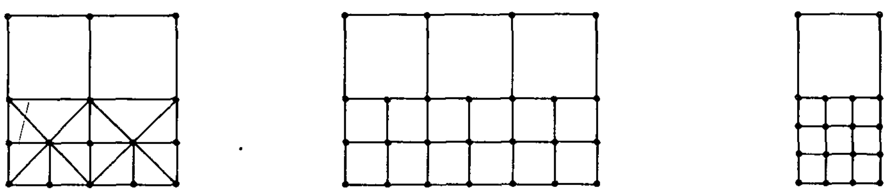

Fig. 1 - Mesh refinements having property A

One alternative would be to apply second-order Richardson or Chebychev polynomial methods to meshes having local refinements of the kind used in Ref. 11. Though these methods require twice as much storage and computing time with a square mesh† as successive overrelaxation, I am sure they are still much faster (using ultrafast machines) than hand computation. Or, one could try successive overrelaxation, hoping that it would converge rapidly even without theoretical justification.‡ As a third alternative, one could use linear interpolation during mesh transitions, as proposed by Arms.

PHYSICAL RIGOR

Solutions of the Helmholtz problem are in equilibrium under inertial forces and, in the case of wakes, under gravity. Unfortunately, as realized by Helmholtz himself (1868), this equilibrium is very unstable in single-phase flow. Gravity is neglected in representations of (two-phase) cavity flows, but the equilibrium is much more stable (14, p. 54). Moreover viscous forces, turbulence, surface tension, evaporation, air content, and impurities in solution are neglected in any case.

It is obviously important to know how best to approximate physical reality by Helmholtz flows, and also how to allow for the various forces neglected. I reviewed this question ten years ago;§ in ideal cavitation, one might expect to observe the nominal equation of state (3, p. 7)

$$\rho = \rho_o, \quad \text{if } p > p_v \tag{2a}$$

$$\rho = \rho_v, \qquad p = p_v \text{ elsewhere.} \tag{2b}$$

*A local analytical expansion was generally used instead of local mesh refinement.
†R.S. Varga, J. Soc. Ind. Appl. Math. 5:39-46 (1957)
‡Such justification may actually be implicit in recent work by Kahan and Varga. (Comment of Dr. Young.)
§G. Birkhoff, Proc. 7th Int. Congress Appl. Mech., London, 2:7-16 (1948), also Ref. 14, p. 51 and Ref. 3, Ch. I. The use of Eqs. (2a) and (2b) goes back at least to Riabouchinsky, Demtchenko, Ackeret, and Weinig (1926-32).

If inertial forces alone are considered, this equation leads naturally to the Helmholtz-Brillouin Problem.*

Reentrant Jet Models

Unfortunately, one can show that Helmholtz-Brillouin flows past a single obstacle necessarily involve infinitely long cavities and zero cavitation number Q, whereas real cavities have finite length and positive Q! To avoid this paradox, it is customary to use the Riabouchinsky and reentrant jet models, and to take

$$Q = \frac{p_a - p_c}{1/2\ \rho v^2}$$

as an empirical constant. (Here p_a is the local ambient pressure; p_c is the (mean) cavity pressure, which is p_v in ideal cavitation.)

These models give almost identical predictions for drag and cavity dimensions in two-dimensional flow.† However, it is very hard to find accurate published statements about the conformity of such models and reality; usually, authors are content to mention the qualitative occurrence of reentrant jets.

This is partly due, of course, to the limited practical interest of two-dimensional cavity flow, and partly to experimental difficulties. Anyway, the most frequently seen‡ comparisons refer to integrated, artifically rotated (Plesset-Shaffer) pressure distributions on wedges for ideal Bobyleff flows, as compared with drag measurements on cone heads in water tunnels. Since the theoretical interpretation of the calculated drag coefficients is obscure, these comparisons have questionable scientific significance.

The published experimental values $C_D(Q) = 0.55 + 0.4Q$ for flow past a circular cylinder agree, at $Q = 0$, with the numerical value calculated by Brodetsky. It would seem desirable to redo these experiments, and to check the values for $Q > 0$ against calculations recently performed§ on the MANIAC.

But agreement between calculated and measured values of $C_D(Q)$, however useful, does not tell the whole story. Especially in cases where, as with cones, the separation point is determined by a sharp corner, such agreement is almost built in. As observed nearly 15 years ago by L. B. Slichter, the difference between the stagnation and separation pressures outside the boundary layer is fitted by the choice of Q. Again, the pressure distribution is a convex even function in most cases treated. One can therefore expect fair engineering estimates of $C_D(Q)$, without knowing much about the flow as a whole. Especially good estimates may be expected when, as with cones (following Armstrong (1, pp. 226-227)), one can match the local pressure distribution near the vertex and the separation point to a higher order.

*Gilbarg (1, p. 285) calls Helmholtz-Brillouin flows "physically acceptable," but this seems to me a confusion of physics with metaphysics, because of all the physical variables neglected.

†First observed by D. Gilbarg and R.A. Anderson, J. Appl. Phys. 19:157-69 (1948); see also Ref. 3, Plate 11. The prediction of Q is an important unsolved problem.

‡Reference 1, p. 227, and B. Perry and M.S. Plesset, pp. 251-261 of the Riabouchinsky Jubilee Volume, Publ. Sci. Tech. Min. de l'Air. Paris, 1954. Garabedian's calculations for a disc are more significant.

§G. Birkhoff, H.H. Goldstine, and E.H. Zarantonello, Rend. Sem. Mat. Torino 13:205-23 (1954), esp. Case 23a. An unpublished rough check of Martyrer's data was made by the late R.T. Knapp, around 1946. (See the next footnote.)

Approximate agreement between theoretical and measured values of $C_D(Q)$ does not, in any case, imply agreement as regards separation angle, cavity size, or behavior at the rear end of the cavity. In particular, I think that the differences between ideal and "reentrant jets" has been insufficiently emphasized.

Thus, I take exception to the phrase "observed experimentally" in Ref. 1, p. 286, line 13. In spite of Fig. 16 on p. 230, I doubt that Fig. 15 of Ref. 1, p. 230, accurately represents the flow of Fig. 14 on p. 229. Whereas ideal reentrant jets are irrotational and steady, most real reentrant jets have vorticity and are unsteady. This seems especially likely for very small cavities, like that of Fig. 14. Again, as shown conclusively by Swanson and O'Neil (1, p. 230) (see also the paper by Campbell in this volume) one loses axial symmetry at the rear end of long cavities behind axially symmetric obstacles. This is because of gravity.

Cavity Size

In principle, the preceding models give unambiguous predictions as regards relative cavity diameter d_m/d and length ℓ/d, as functions of the cavitation number Q. Until these functions have been calculated effectively, one cannot ascertain the correctness of these predictions. My guess is that, for small Q, the predicted d_m/d will turn out to be too big, and the predicted ℓ/d much too big because of the neglect of gravity. This is, of course, important for some aspects of hydrofoil design; I would guess that linearized "thin hydrofoil" cavity theory would be subject to the same trend (again, for very long cavities).*

It seems reasonable to expect that the correction will begin to be large when the twin vortices, first observed by Swanson and O'Neil (1, p. 230), are a dominant feature at the rear end of the cavity. In this connection, one should consult Campbell's paper again.

Similarly, in the case of water entry of missiles, the Froude number $F = v^2/gd$ has always been supposed to be the primary variable determining similitude (e.g., d_m/d and ℓ/d). Since F and Q have very different dimensional dependence, while v and d can be varied independently, F and Q can hardly both determine d_m/d and ℓ/d.

To clarify the above questions, I think more accurate measurements of $(p_a - p_c)$ will be needed than are now available.

Wake Models

Before World War II, potential flows with free streamlines were taken as models for "wakes" (and for homogeneous jets†) in hydrodynamics, with only vague qualifications. This was in spite of the extreme instability of the free streamlines, already known to Helmholtz himself. As a result of this instability, viscosity and turbulence have an accentuated importance for wakes—though gravity, fortunately, has only a hydrostatic effect. It is therefore especially challenging to try to approximate real wake behavior by models involving potential flow.

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*Some experimental data bearing on the above remarks may be found in Rpt. E-73.6 of the Caltech Hydrodynamics Lab. by R.L. Waid, and in Project Rpt. 59 of the St. Anthony Falls Hydraulic Lab. by E. Silberman.

†As shown in Cornell's paper in this volume, good predictions of contraction coefficients C_c for jets can often be had. Cf. the last line of Ref. 3, Ch. I, § 7.

Since the pressure in real wakes, as in cavities, is below the ambient pressure p_a, it is natural to try to represent the flow outside them by the Riabouchinsky and reentrant jet models. Again, considerable success in estimating pressure distributions with such models is possible (3, p. 28, Fig. 3) in cases where the separation point is fixed by a sharp corner. However, unlike cavities, wakes have irregular and unstable boundaries. Hence, though the models with "dead water" wakes are in nominal near equilibrium when $R \gg 1$, real wakes are in fact highly turbulent.

A better model for real wakes is probably provided by an old discontinuous potential flow model of Joukowsky, with variable $Q > 0$, recently discussed by Roshko* and Eppler.† In this model, the wake or "cavity" terminates in a strip bounded by fixed parallel streamlines, thus predicting an asymptotically constant displacement thickness downstream, as seems reasonable (3, p. 266). It would be interesting to compare this model (for the right Q) with Flachbart's photographs of the wake behind a flat plate, and to see whether agreement was also better locally than for the Riabouchinsky model (3, p. 350). Moreover, comparisons of the predicted with real displacement thickness $2\delta(Q)$ and pressure coefficient distribution $C_p(x,y,Q)$ would be interesting, as functions of the Reynolds number R too! (This plays no role in the Roshko-Eppler model.) Finally, it would be interesting to make similar comparisons for curved obstacles; calculations could be made by the methods of Ref. 3, Ch. VI.

In any event, the Roshko-Eppler model seems a definite improvement on earlier, otherwise analogous models‡ with $Q = 0$, as regards simulation of flow outside the wake.

The pressure recovery which is observed in real wakes is also obtained by another model, recently suggested by Batchelor.§ This model, which has some metaphysical plausibility as a representation of asymptotic behavior when R approaches infinity, involves the assumption of constant vorticity in the wake "bubble." In the light of observed wake instability for $R > 50$, however, it seems obvious that neither of the above models resembles physical reality at all, within the wake itself (except possibly in supersonic flow). Similar objections apply to the Karman "vortex street" model for periodic wakes (3, Ch. XIII), even in the most favorable range of Reynolds numbers, though with much less force.

Impact Forces

More critical comparisons between theory and experiment are also desirable, as regards the "impact forces" arising during the first half-diameter of entry of a missile into water. This important subject was reviewed briefly in Ref. 1, pp. 216-20, but with emphasis on favorable comparisons. I should like to record my opinion, that unfavorable comparisons should receive equal emphasis—I see no reason why such comparisons should be taboo.

For instance, consider Ref. 17, p. 403, Fig. 1, which gives the impression that all is sweetness and light. A more careful study shows that the "experimental points" plotted there represent averages of variable observations, which decrease by factors 1.5 to 3.5. Obviously, in such cases, the "experimental" k will depend on the interval of averaging—and Watanabe's data disagree dimensionally with the formula $F = Ay^2$ predicted by Wagner's similarity argument.

*A. Roshko, J. Aer. Sci. 22:124-132 (1955).
†R. Eppler, J. Rat. Mech. Anal. 3:591-644 (1954).
‡See Ref. 13, and references given there to earlier work of Prandtl, Schmieden, Squire, and others. Also, see R. von Mises, "Theory of flight," p. 101.
§J. Fluid Mech. 1:177-90 and 1:388-98 (1956). This model is remotely reminiscent of Föppl's old model of a vortex-pair (3, p. 263).

In the above case, I would guess that the discrepancy is due to experimental difficulties, but this question should be settled scientifically and not by prejudice. In the cases of the "expanding lens" and other pseudo-mathematical models also reviewed favorably in Ref. 1, the theory rests on a less firm foundation* and critical comparisons with observation of all "theoretical" predictions seems especially needed.

CAVITY SEPARATION

It is well known (1, pp. 224, 234, 285) that the prediction of the separation point for cavity flow past smooth obstaclés is especially difficult, and that potential flows past analytic profiles have their only singularity there. However, it is not clear to what extent or how viscosity, turbulence, and surface forces affect separation.

In Ref. 1, pp. 224, 295, Maccoll and Imai point to boundary layer theory as the key to the problem. I shall discuss this suggestion below, showing in particular that a laminar boundary layer theory implies a unique separation point.

For wakes, observed pressure distributions do not indicate a unique separation point (16, pp. 422, 497); the separation point is strongly turbulence dependent.

Low Velocity Effects

As shown by Eisenberg (15, pp. 10-11), the same is true of cavities at large Q: the problem is the old one of predicting conservation of the liquid-gas balance in the cavity. In a similar vein, it is known that cavity formation is drastically delayed by spinning spheres,† because of boundary layer turbulence. Again (14, p. 63), Slichter has observed a downward refraction of spheres at low speeds, when entering water at angles of 20 degrees with the horizontal, at moderate speeds, and explained this as due to the effect on separation of insufficient ventilation. Finally, Worthington himself observed effects on separation of the surface condition ("wettability")—effects recently confirmed by May.‡

As regards cavities, all these anomalies occur at relatively low velocities. I therefore repeat my suggestion (made in 1948) that solutions of the Helmholtz-Brillouin problem should give good indications of the separation angle as a function of Q for cavities behind slowly decelerating macroscopic missiles, in the range 150 fps $< v <$ 1400 fps (Mach number $M < 0.3$).

At lower speeds, one should expect delayed separation and other scale effects. Thus viscosity and surface tension, in combination, will make the cavity separate at a finite angle, and not tangentially. An approximate computation of this contact angle, as a function of the variables involved, would be very instructive.

Separation Curvature

Actually, the Helmholtz-Brillouin problem was first formulated to avoid the mathematical indeterminacy in the separation point which would otherwise arise for

*Even the calculations of Hillman, reported in Ref. 17, are subject to the general truncation error uncertainty I discussed earlier.

†H. Wayland and F.G. White, Proc. Symposium Fluid Mech. Heat Transfer, Stanford, pp. 51-64, 1949.

‡A. S. May, J. Appl. Phys. 22:1219-1222 (1951); Ref. 3, p. 326. For the case of circular struts, see Wetzel's paper in this volume.

Helmholtz flows past smooth obstacles. To solve it, Brillouin postulated that the free streamline curvature must be finite at the separation point (3, p. 139)—now called the condition of "smooth separation" (1, p. 285). Villat related this condition to the solution of the Levi-Civita integral equation (3, p. 139), and also assumed that this condition, of a lowest order singularity possible in potential flow, actually determined the separation of flow.

The condition of smooth separation has always seemed to me without physical basis; I cannot see why nature should abhor infinite pressure gradients. If infinite curvature is ruled out, then so is Kirchhoff-Bobyleff flow past a flat plate! My view seems to have been shared by von Mises and Schmieden who, as recalled in the previous section, proposed the "wake of zero drag" as a first approximation to wake flow with turbulent boundary layer. Indeed, it seems obvious to me that separation should entail both a mathematical and a physical singularity; thus boundary layer theory (cf. infra) even predicts a jump in slope at the separation point (16, p. 57, Fig. 22).

Much more reasonable is the condition for ideal cavitation, corresponding to the nominal equation of state (Eqs. 2a and 2b) which leads to the Helmholtz-Brillouin problem if inertial forces alone are considered.

Though significant deviations from Eqs. (2) can occur under exceptional conditions (3, Ch. XV), variations of one atmosphere in pressure change the pressure coefficient $C_p = (p - p_a)/1/2\ \rho v^2$ by less than 0.03, if $v = 200$ fps, and by less than 1 percent if $v = 400$ fps. Hence, in such cases, the Helmholtz-Brillouin problem is a priori very reasonable. Even air-filled and vapor-filled cavities should behave similarly, since we have $p_v < p < p_a$ under these circumstances.*

Unfortunately, the Helmholtz-Brillouin problem is not easy to solve mathematically, and one seldom knows whether it has a unique solution.

Boundary Layer Theory

I agree with Drs. Maccoll and Imai (1, pp. 224, 295) that the influence of viscosity on the separation point should first be studied using boundary layer theory. This theory makes it obvious why separation must occur behind the point of minimum pressure—i.e., why the Helmholtz-Brillouin problem predicts separation too far forward. It also suggests a way of improving agreement between theory and observation (a "viscosity correction"). Experimental data can be corrected by displacing the liquid-solid and liquid-gas interfaces outward by an easily calculated displacement thickness $\delta(s)$. It would be interesting to study the resulting potential flows as solutions of the Helmholtz-Brillouin problem for the displaced fixed boundary. Perhaps this would explain the discrepancy noted in Ref. 1, p. 224.

On the other hand, I do not think that boundary layer theory is an adequate substitute for Eqs. (2), as suggested by Imai.† I do not even agree that, according to boundary layer theory, the asymptotic curvature must be finite. Why should not a

*It seems unlikely that one can attain cavity overpressure by "hyperventilation"--i.e., that forcing air into the cavity will make $Q < 0$. Cf. Proc. First Symp. Appl. Math. Am. Math. Soc., p. 1, 1949; Ref. 14, p. 58; Proc. 7th Int. Congress Appl. Mech., London, 2:12 1948.

†Ref. 1, p. 295; and J. Phys. Soc. Japan 8:399-402 (1953).

brief adverse pressure gradient, followed by a mild singularity* inducing separation with infinite curvature, be compatible with conventional boundary layer theory?

It is attractive to speculate that the separation point can be located by combining free streamline and boundary layer theory, as follows. Consider the one-parameter family of cavity flows, with variable separation point, which is possible if the maximum velocity occurs before separation—as suggested by boundary layer concepts. Then find which flow is compatible with the usual boundary layer separation criteria. This could probably be calculated numerically, in the case of a circular cylinder, and I think such a calculation would be very desirable.

However, it is clear that such a calculation will predict a fixed separation point, independent of the flow velocity. For, inside the boundary layer, the equations are invariant under the group†

$$x \to x, \quad y \to y/\alpha, \quad u \to \alpha^2 u, \quad v \to \alpha v \ (\alpha > 0)\,. \tag{3}$$

Outside the boundary layer, the flow scales inertially (14, p. 97), completing the verification.

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*I know of no exhaustive mathematical study of the nature of the singularity at a separation point.

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