CHAPTER I

Hydrodynamical Paradoxes

1. The Practical Value of Paradoxes

As fluid mechanics comprehends the physics of two of the three most common states of matter—the liquid and the gaseous—it is superfluous to explain why one or even many books should be devoted to the subject. It is less obvious why I have thought it suitable to begin this book with a discussion of "hydrodynamical paradoxes." It might seem that this unduly emphasizes *negative* aspects of the subject. Therefore I should like to begin by asserting the importance of such paradoxes from the practical, the mathematical, and the philosophical points of view. By a *paradox* I shall mean a plausible argument which yields conclusions at variance with physical observation, as it is in this sense of an *apparent inconsistency* that the term is most commonly used in hydrodynamics.

From a practical point of view, perhaps the most important art to acquire in fluid mechanics consists in knowing when a given theory is "applicable," to use the current phrase. The fundamental importance of acquiring this art has been stressed in various recent books,[1] which have tried to bridge the wide gap between theory and experiment which exists in hydrodynamics.

Such books tend to attribute this gap to the difference between "real" fluids of small but finite viscosity, and "ideal" fluids having zero viscosity.[2] They tend to minimize the importance of logical and mathematical rigor, and to trace all discrepancies between "theory" and "experiment" to the single "unjustified" approximation of zero viscosity. By familiarizing the reader at the outset with the physical facts of separation of the boundary layer and turbulence, they

[1] See for example the prefaces of [12] and [14], or p. 2 of [6]. Numbers in square brackets refer to the bibliography on p. 181.

[2] See [6], pp. 2, 48; [12], Vol. 1, p. 3, and the introduction to Vol. 2; [14], p. x.

put him on his guard against some of the oversimplifications of classical hydrodynamics.

The art of knowing "how to apply" hydrodynamical theories can be learned even more effectively, in my opinion, by studying the paradoxes which I shall describe. Moreover, I think that to attribute them all to the neglect of viscosity is an unwarranted oversimplification. The root lies deeper, in lack of precisely that deductive rigor whose importance is so commonly minimized by physicists and engineers. They can be traced to the use of plausible arguments, which have often proved most fruitful in physical reasoning, but which are nevertheless fallible, as simple examples show. Among these are the arguments that "small causes produce small effects" and that "symmetric causes produce symmetric effects."

A clear understanding of the fallibility of these arguments in specific hydrodynamical applications, should be useful to any research worker who wishes to make reliable theoretical predictions.

2. *Their Mathematical and Philosophical Interest*

The possibility that *arbitrarily small causes can produce finite effects* in fluid mechanics has been clearly recognized by Oseen ([11], pp. 211–213). Oseen has pointed out that in systems of differential equations, the presence of arbitrarily small terms of higher order can change entirely the behavior of the solutions. It need *not* be true that as the coefficient of a certain term tends to zero, the solution tends to the solution obtained by annihilating that coefficient. Paradoxes due to this source may be called *asymptotic paradoxes.*

The possibility of asymptotic paradoxes cannot be admitted as a universal principle.[3] If it is, since every physical experiment is actually affected by countless minute influences,[4] we

[3] I have been informed by J. R. Portié and L. L. Whyte that asymptotic paradoxes are common in biology (mutations of genes, etc.).

[4] Heating and quantum reactions from cosmic rays, variations in gravity due to distant stars and the Coriolis force, seismic vibrations, impurities in the materials used, etc.

should have no chance of ever predicting correctly the result of any specific experiment. Since the results of many experiments *can* be predicted with practical certainty, it is not a universal principle; but is presumably restricted to certain types of partial differential equations.

Hence asymptotic paradoxes should interest the pure mathematician by providing suggestive evidence regarding the facts about partial differential equations—a complex subject concerning which our knowledge is still very rudimentary. Several of the other paradoxes which I shall describe will provide physical evidence of further properties of partial differential equations, and should therefore also interest the pure mathematician. Indeed, I am convinced that in order to make real theoretical progress, we must study a wide variety of physically and mechanically constructed solutions of partial differential equations of specific types.[5]

Thus there are several interesting cases in hydrodynamics where *an apparent symmetry of causes is not preserved in the effects.* Such paradoxes may be called *symmetry paradoxes,*[6] and they have an evident philosophical interest for the logic of physics.

In general, hydrodynamical paradoxes have a philosophical interest, as illustrating the profound interweaving of deductive and inductive logic in scientific research. I believe that the resourcefulness and ingenuity of the human mind in combining these methods is vastly underestimated in all existing formal theories of mathematical and physical logic.[7]

They also have an interest for the pure mathematician, by illustrating a wealth of phenomena not anticipated by classical mathematical theories. Even a superficial attempt at mathematical classification reveals what may be called paradoxes of overdeterminism, singular point paradoxes, paradoxes of approximation, and paradoxes of topological oversimplifica-

[5] This idea, with especial reference to high-speed computing machines, has been stressed repeatedly by J. von Neumann.

[6] They seem to contradict the logical axiom that "causes invariant under a group G induce effects invariant under the same group," G. Bouligand, *Théorie générale des groupes*, Paris, 1935, p. 3.

[7] In particular, mathematical logic seems to me to suffer from too exclusive preoccupation with the foundations of set theory.

tion. I shall now try to support the statements made above by a discussion of precise mathematical details.

3. Fundamental Approximations

The role of postulates in theoretical fluid mechanics is played by an "equation of state," and certain partial differential equations and boundary conditions.

One assumes that the *density* ρ and *viscosity* μ of any fluid depend on the *pressure* p and *temperature* T, so that

$$\rho = f(p,T), \qquad \text{(equation of state)} \tag{1}$$
$$\mu = f_1(p,T). \tag{1'}$$

In mathematical discussions, μ is commonly treated as a constant, which is nearly fulfilled in most practical applications, provided temperature variations are moderate.

It is then convenient to take the "Eulerian" independent variables of vector position $\mathbf{x} = (x_1,x_2,x_3)$ and time t. Assuming that all functions involved are sufficiently differentiable[8] (even the assumption of continuity is non-trivial, as we shall see!), we can express the conservation of mass as the so-called Equation of Continuity

$$\operatorname{div}(\rho \mathbf{u}) + \frac{\partial \rho}{\partial t} = 0, \tag{2}$$

where $\mathbf{u} = (u_1,u_2,u_3)$ denotes vector velocity.

It is sometimes convenient to use instead "Lagrangian" independent variables $\mathbf{a} = (a_1,a_2,a_3)$ associated with material points moving with the fluid, and t. If $\mathbf{a}$ is held fixed, the time derivative becomes the "material" derivative

$$\frac{d}{dt} = \frac{\partial}{\partial t} + \sum u_k \frac{\partial}{\partial x_k} \qquad \text{(in Eulerian variables).}$$

[8] Careful discussions of possible discontinuities have been given by Paul Duhem, "Recherches sur l'hydrodynamique," *Annales de la Faculté des Sciences de Toulouse*, Vols. 3–5 (1901–1903) and by L. Lichtenstein, *Hydrodynamik*, Berlin, 1933. I am indebted for these references, and for other helpful comments on Chapter I, to Prof. J. Kampé de Fériet.

Hence $u_i = dx_i/dt$ and (2) is equivalent to

$$\frac{d\rho}{\rho dt} + \operatorname{div} \mathbf{u} = 0. \tag{2'}$$

The other basic differential equations of fluid mechanics express Newton's momentum law for continua, and are called the Equations of Motion; they involve the vector acceleration of gravity $\mathbf{g} = \mathbf{g}(\mathbf{x})$.

In stating the Equations of Motion, one must make one of two approximations. Either one must neglect viscosity, which gives the approximation

$$\frac{d\mathbf{u}}{dt} + \frac{1}{\rho}\operatorname{grad} p = \mathbf{g} \qquad (\rho \text{ variable}) \tag{3}$$

for a so-called *non-viscous* fluid; or one must neglect compressibility, setting ρ = const., and use the Navier-Stokes equations

$$\frac{d\mathbf{u}}{dt} + \frac{1}{\rho}\operatorname{grad} p = \nu\nabla^2\mathbf{u} + \mathbf{g}, \tag{3*}$$

for an *incompressible* fluid. Here the ratio $\nu = \mu/\rho$ is called the *kinematic viscosity* of the fluid.

These approximations are made partly for simplicity—even with them, one cannot in most cases solve the mathematical problems involved. They are also made because we do not actually know how viscosity acts in a fluid under rapid compression (see [12], Vol. 1, p. 260)!

Hence if we are too impressed by the dangers of approximation, we must despair at the outset of attaining any rational understanding of fluid mechanics. However, science was not built by timid or faltering hands. Let us move boldly forward, seeing what kind of edifice one can construct on these somewhat insecure foundations.

4. Non-viscous Fluids

I shall begin (Sections 4–12) by making the approximation of zero viscosity. The *a priori* plausibility of this approximation would be hard to exaggerate. Thus ([6], p. 4) a change in velocity of 100 m.p.h. across a layer of air .01 in. thick, exerts a shear force of only one ounce weight per square foot.

Its plausibility can also be expressed in terms of the *Reynolds number*, defined as the dimensionless ratio

$$R = \frac{\rho VL}{\mu} = \frac{VL}{\nu}, \tag{4}$$

where V is a typical velocity and L a representative linear dimension. Since R expresses the ratio (inertial forces)/(viscous forces), it seems reasonable to suppose that the approximation $\nu = 0$ should become asymptotically valid for sufficiently large R, say, over one million. This supposition becomes even more reasonable if we recall (Section 2) the many other minute forces which are always necessarily neglected in making physical predictions. Without further discussion, let us therefore introduce the fiction of a "non-viscous" fluid, in which (3) is valid.

In studying the flows of "non-viscous" fluids from homogeneous reservoirs, it is usually also assumed that the fluid expands and contracts *adiabatically*, so that

$$p = f(\rho). \quad \text{(In perfect gases, } p = k\rho^{\gamma}.) \tag{1'}$$

This assumption is reasonable if viscous forces are negligible, because heat conduction and viscosity are both manifestations of molecular diffusion. It can also be justified by separate computations, if we admit the principle that *small variations in density exert a small influence in fluid flow.* This principle, unlike the corresponding principle for viscous forces, appears to be entirely supported by the physical evidence.

5. *Mathematical Properties*

From the preceding approximate assumptions, several fundamental mathematical consequences can be derived, and I should like to emphasize the fact that these consequences do in fact explain observed resistance to acceleration from rest (see Chapter V) and many other physical phemomena.

Thus, in the case of "barotropic" fluids satisfying (1′), one can prove ([12], Vol. 1, p. 191) that the "circulation" $\oint \Sigma u_k dx_k$ around any closed "material" curve moving with the fluid,

is constant. Hence if the fluid comes from a reservoir without circulation (i.e. at rest in some non-rotating reference frame), and if the curve remains closed at all times, the circulation should remain zero. This is mathematically equivalent to the statement that there should exist a locally single-valued scalar *velocity potential* $U(\mathbf{x};t)$, such that

$$\mathbf{u}(\mathbf{x};t) = \operatorname{grad} U, \quad \text{or} \quad u_i = \frac{\partial U}{\partial x_i} \quad [i = 1, 2, 3]. \tag{5}$$

In a *simply* connected domain, such as the exterior of a solid or half the symmetric exterior of a circular cylinder, U must therefore be single-valued in all space. In any case, this is the simplest possible assumption; it was universally accepted for over a century; and we shall make it until Section 8, where the possible exceptions will be discussed.

Further consequences of the basic hypotheses (1)–(3)–(4) can be deduced in the case $U = U(\mathbf{x})$ of "steady" fluid motion. Consider the case of the flow of a stream of fluid (such as air or water) moving at constant speed past a fixed obstacle—to which the case of a rigid body moving at nearly constant speed through an infinite fluid initially at rest can be reduced, by the use of moving axes.

Since the boundary conditions and differential equations are independent of time, one then naturally writes $U(\mathbf{x})$. One can prove that, under these circumstances, the equations of motion (3) are equivalent after one space integration to the *Bernoulli equation*

$$\frac{1}{2}\nabla U \nabla U + \int \frac{dp}{\rho} + G = p_0, \quad \text{or} \quad \sum u_i du_i + \frac{dp}{\rho} + dG = p_0. \tag{6}$$

Here G denotes the gravitational potential, so that $\mathbf{g} = -\nabla G$.

An important special case is that of (nearly) incompressible flow, in which case we set $\rho = \text{const.}$ In this case, (2) is simply $\operatorname{div} \mathbf{u} = 0$, which is equivalent by (5) to

$$\nabla^2 U = 0, \tag{7}$$

i.e. to the existence of a *harmonic velocity potential.* For such flows, (6) assumes the familiar simple form

$$p = p_0 - \tfrac{1}{2}\rho\nabla U\nabla U - \rho G. \tag{6'}$$

In fact, one can prove more generally, in the case of "unsteady" or "accelerated" motion of an incompressible non-viscous fluid, initially without circulation, that the pressure must satisfy

$$p = p_0 - \rho\left\{\frac{1}{2}\nabla U\nabla U + \frac{\partial U}{\partial t} + G\right\}, \tag{6''}$$

which is equivalent to the equations of motion, just like (6).

So much for the internal conditions satisfied by flows of an incompressible, non-viscous fluid from a still reservoir: we have the basic kinematic condition (7), from which pressure can be determined by (6′) in the case of steady motion. In this case, the equation of continuity clearly becomes

$$\frac{\partial U}{\partial n} = 0 \qquad \text{on the boundary,} \tag{8}$$

where $\partial/\partial n$ denotes the outward normal derivative. Similarly, the condition that $\mathbf{a}$ is the "free stream" velocity is clearly

$$\lim_{\mathbf{x}\to\infty} \operatorname{grad} U = \mathbf{a}. \tag{9}$$

Now by potential theory ([7], pp. 211, 311), with bodies of reasonable shape, equations (7) to (9) have *one and only one solution.* This rather deep mathematical result is in perfect accord with the intuitive idea that there should be exactly one flow around a given fixed obstacle, having a given relative velocity $\mathbf{a}$. The flows obtained in this way are called Dirichlet flows.

The theory of Dirichlet flows is completely satisfactory from a logical and mathematical point of view. Let us now apply it to the study of fluid resistance.

6. *The D'Alembert Paradox*

One of the simplest examples of a Dirichlet flow is the steady flow of an incompressible non-viscous fluid past a sphere of radius L centered at the origin. We may suppose that the

free stream velocity has magnitude V, and is parallel to the x-axis. The solution is found by superposing a "dipole field" on a uniform flow, so that

$$U = Vx_1 + \frac{\alpha x_1}{r^3} \qquad \left[\alpha = \frac{VL^2}{2},\ r^2 = \sum x_i^2\right]. \tag{10}$$

It is easy to compute by (6′) the theoretical pressure distribution around a sphere under the flow (10). One finds that, apart from the hydrostatic buoyancy (cf. Chapter III, Section 12), the pressure distribution has complete fore-and-aft symmetry, and hence gives a pressure thrust zero.

This theoretical conclusion, called the d'Alembert Paradox, is not limited to spheres; one can prove generally[9]

Theorem 1. *The hydrodynamic force system on any finite obstacle B, under steady Dirichlet flow in an infinite stream, reduces to zero or a pure couple.*

Proof. The assertion is that the integral $\mathbf{F}_B$ of the pressure differentials over the surface of B is zero. To prove this, we choose the x_1-axis in the direction of flow, and let V be the free stream velocity. By hypothesis, $\nabla(U - Vx_1) \to 0$ as $\mathbf{x} \to \infty$; moreover $\iint(\partial U/\partial n)dS = 0$ on any closed surface containing the obstacle. It follows by potential theory ([7], p. 270) that $U - Vx_1 = 0(1/r^2)$, meaning that it tends to zero as rapidly as $1/r^2$, where $r = |\mathbf{x}|$. Further

$$\mathbf{u} = (V,0,0) + 0\left(\frac{1}{r^3}\right), \tag{10'}$$

whence $\nabla U\nabla U = V^2 + 0(1/r^3)$. Neglecting hydrostatic buoyancy as before, we find that the hydrodynamic pressure satisfies, by Bernoulli's equation (6′),

$$p = p_0 - \frac{1}{2}\rho V^2 - 0\left(\frac{1}{r^3}\right) = p_f - 0\left(\frac{1}{r^3}\right). \tag{11}$$

Here p_0 is the "stagnation pressure" at zero velocity; the constant $p_f = p_0 - \tfrac{1}{2}\rho V^2 = \lim_{\mathbf{x}\to\infty} p(\mathbf{x})$ is called the "free stream pressure."

[9] For a recent discussion, see M. Manarini, "Sur paradossi d'Alembert e di Brillouin nella dinamica dei fluidi," *Atti Accad. Naz. Lincei*, Rend. 4 (1948), 427–433, reviewed in *Math. Reviews*, *10* (1949), p. 335.

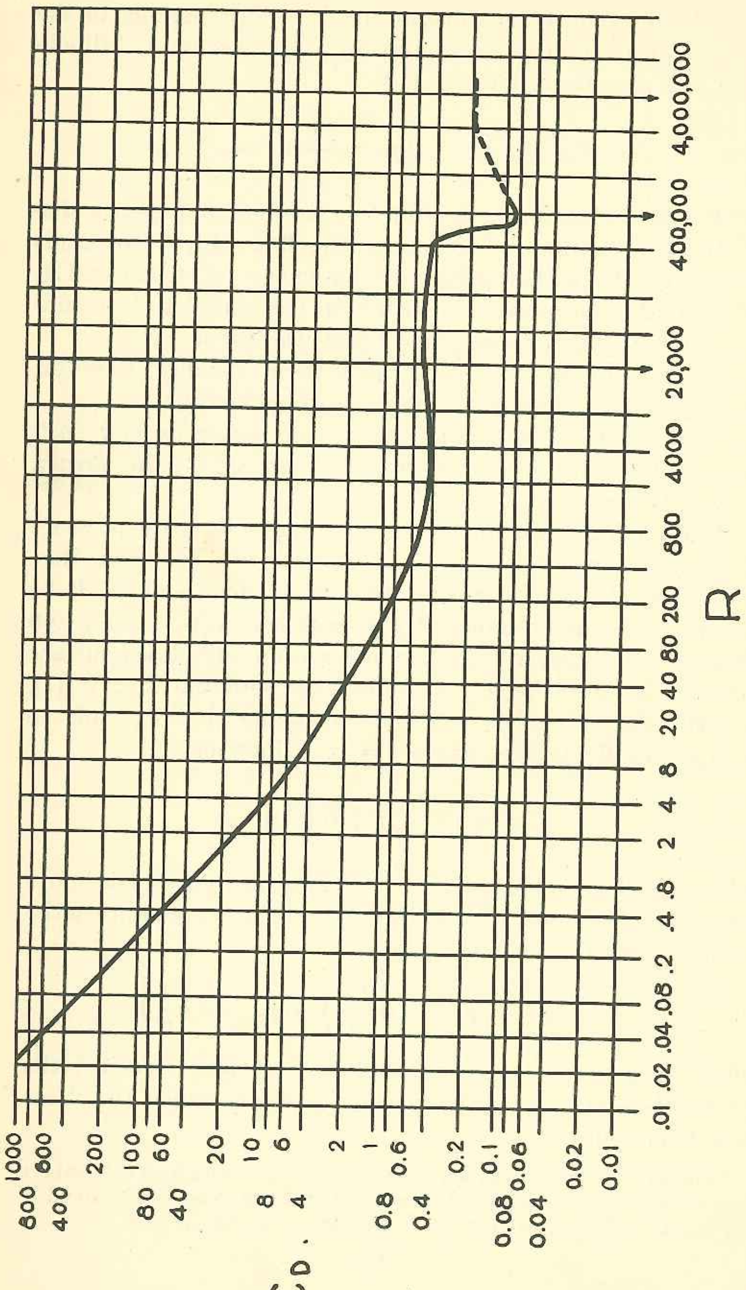

FIG. 1. Drag coefficient of sphere.

Now consider the momentum equation ([12], Vol. 1, Chapter XIV) for the fluid occupying, at variable time t, a fixed large sphere S of radius a. Clearly, for any a large enough so that S includes B, $\mathbf{F}_B$ is equal to the difference $\mathbf{E}(a) - \mathbf{F}_S(a)$ between the rate $\mathbf{E}(a)$ at which momentum flows outwards through S, and the pressure thrust $\mathbf{F}_S(a)$ exerted inwards across the surface of S. Hence

$$\mathbf{F}_B = \lim_{a\to\infty}\{\mathbf{E}(a) - \mathbf{F}_S(a)\}.$$

But by equations (10′)–(11), the pressure on the surface of S differs by $0(1/a^3)$ from the constant p_f; therefore, the area of S being $4\pi a^2$, $\mathbf{F}_S(a)$ differs by $0(1/a)$ from zero: $\mathbf{F}_S(a) = 0(1/a)$. Similarly, the flux of mass per unit area through S is $\Sigma u_k x_k/a$, and so by (10′) differs by $0(1/a^3)$ from that of a "uniform flow" with constant velocity $(V,0,0)$. Again, the vector momentum transported through a unit area per unit time, is $\mathbf{u}\Sigma u_k x_k/a$; this also differs from that of a uniform flow by $0(1/a^3)$, using (10′) again. But for a uniform flow, clearly $\mathbf{E}(a) = 0$; hence actually, multiplying once more by $4\pi a^2$, $\mathbf{E}(a) = 0(1/a)$. Substituting above, we conclude $\mathbf{F}_B = 0$, as desired.

The preceding theoretical prediction, of zero resistance or "drag" D and lift L, is obviously contradicted by everyday experience; thus in Figure 1 one sees the drag coefficient $C_D = D/\frac{1}{2}\rho V^2 A$ (A = cross-section area) of a sphere, plotted as an empirical function of the Reynolds number R. I shall discuss shortly (in Section 9) the rationalizations of this contradiction between theory and experiment, but first I should like to describe some closely related paradoxes.

7. *The Reversibility Paradox*

Although it is probable that the d'Alembert paradox is also valid for subsonic, compressible flow of a non-viscous fluid, this seems hard to prove rigorously.[10] However, a very simple argument shows that fluid resistance can never be

[10] A "proof" has been given by U. Cisotti, *Comptes Rendus*, *178* (1924), p. 1792. But as there is no rigorous analog applying to nonlinear partial differential equations of the result proved on p. 270 of [7], much stronger hypotheses regarding the behavior at infinity must be made.

explained from the hypothesis of a non-viscous fluid as formulated above (Sections 4–5).

DEFINITION 1. By the *reverse* of a given flow, described in Lagrangian coordinates, we mean that flow obtained from the given flow by the substitution $t \to -t$. In Eulerian coordinates, this corresponds to the reversal $u_i \to -u_i$ of velocity direction as well as reversal $t \to -t$ of time, but pressure p and density ρ are still preserved at corresponding points $(x_1, \cdots, x_n; t)$ of space time.

DEFINITION 2. A condition on flows is reversible if, whenever it holds for a flow, it holds for the reverse of that flow. A theory of fluid dynamics is a *reversible theory* if all the conditions which it imposes on flows are reversible.

Actually, all the familiar conditions on flows are reversible, except those which involve viscosity (friction), thermal conduction, diffusion, or shock wave fronts.

DEFINITION 3. A theory of fluid dynamics will be called *incomplete* if its conditions do not determine the steady flow around a body uniquely; *overdetermined* if no flow is compatible with it; *incorrect* if its predictions do not agree closely with experimental fact.

THEOREM 2. (Reversibility Paradox.[11]) *Any reversible theory of fluid dynamics is either incomplete, overdetermined, or grossly incorrect, so far as its predictions of steady state lift and drag are concerned.*

PROOF. Such a theory will predict by (6′) that a steady flow and its reverse will give the same pressure thrust on an obstacle, whereas it is a matter of common experience that a flow and its reverse ordinarily give pressure thrusts in approximately opposite directions.[12]

[11] In general, physicists seem to ascribe only philosophical interest to the fact that the concept of non-viscous fluid motion is reversible. Thus the fact is not mentioned at all in [9], [10], or [12].

[12] The preceding discussion is borrowed from [1]. Recently, M. Munk has extended the reversibility principle to linearized supersonic airfoil theory, in *Naval Ordnance Lab. Memo. 9624* (1948). A reversibility principle for theoretical ship wave resistance has been derived by T. H. Havelock, *Proc. Roy. Soc., A149* (1935), p. 423; the discrepancy with experiment is analyzed by W. C. S. Wigley, *Trans. Inst. Nav. Arch., 86* (1944), 1–20. Similar considerations have recently been used by A. Busemann to show that no existence and uniqueness theorem can be hoped for in the case of transsonic flow.

Now the approximations of Sections 4–5 are certainly compatible, because they permit Dirichlet flows around objects of (almost) arbitrary shape. Hence any discussion of fluid resistance which is based on them must either be incorrect, or else involve further assumptions. It is generally believed that both alternatives are possible.

Thus for *subsonic* flow,[13] it is often stated that no physically correct theory of resistance is possible, which is based on the differential equations of Sections 4–5. Whereas for *supersonic* flow it is known that these equations are compatible with more than one flow, and hoped that resistance can be explained by using these equations and qualitative physical considerations.

I shall now discuss in more detail the incompressible case, which is generally supposed to be typical of subsonic flow, because the partial differential equations involved are of elliptic type in both cases.

8. *Paradoxes of Airfoil Theory*

It is well-known that, if one is willing to admit discontinuous functions—that is, to abandon the assumption that all "reasonable" functions are differentiable—the differential equations (1)–(3)–(4) permit many steady flows around an obstacle. These flows are of various topological types, and most of them avoid the d'Alembert paradox. As such "discontinuous fluid motions" form the main theme of Chapter II, I shall not discuss them here, except to mention that they, too, will be found to be subject to paradoxes.[14]

Instead, I shall discuss *two-dimensional airfoil theory*, which gives the best known theory of lift—although it does not give a basis for estimating resistance. In this theory, the multiple-valued velocity potentials, excluded in Section 5, are considered. Mathematically, this amounts to replacing (10′) by the multiple-valued potential

[13] See Th. von Karman, "The problem of resistance in compressible fluids," *Reale Accad. d'Italia* (1936); U. Cisotti, *op. cit.* supra; and Section 12 infra.

[14] Attention is called to the Hadamard paradox, and to the paradoxes of wake underpressure, of indeterminacy, of surface seal, and of downnosing, discussed in Chapter II.

$$U = \Gamma \frac{\theta}{2\pi} + \sum a_k x_k + U_1, \tag{12}$$

where $\nabla^2 U_1 = 0, \quad U_1 = 0\left(\frac{1}{r^2}\right).$

Here $\Gamma = \oint \Sigma u_k dx_k = \oint dU$ is called the *circulation*, the integral being taken along a contour going once around the "airfoil" (or obstacle) B in question. Since any real harmonic function $U(x,y)$ is the real part of a complex analytic function $W(z)$ of $z = x + iy$, we can write equivalently ($c = \Gamma/2\pi$ being real and c' complex),

$$U = Re\{-ic \ln z + c'z + W_1\}, \tag{12'}$$

where $W_1 = 0\left(\frac{1}{r^2}\right).$

We shall find it convenient to use such "complex potentials" W below, and again in Chapter II.

The introduction of the term $\Gamma\theta/2\pi$ in (12) sacrifices the determinism principle established at the end of Section 5: there is a solution of (12) for each choice of Γ. One can rescue determinism in the case that the airfoil R has a sharp "trailing edge" (but not otherwise), by assuming the so-called "Joukowsky Condition," that "velocity is finite at the trailing edge." By assuming that the sharp edge is *trailing*, one avoids the Reversibility Paradox. A Dirichlet flow with circulation which satisfies the Joukowsky Condition may be called a "Joukowsky flow." Thus the Dirichlet flow without circulation past a flat plate inclined at 15° to the main stream is sketched in Figure 2a; the Joukowsky flow past the same plate is sketched in Figure 2b; the "cavity flow" (cf. Chapter II) past it is shown in Figure 2c.

Knowing U, one can then get a theoretical estimate of the thrust on an "airfoil" of arbitrary cross-section, using the following "Kutta-Joukowsky Theorem."

THEOREM 3. *In any Dirichlet flow with circulation, the drag*[14a] *is zero, and the lift is the product* $L = \rho v_f \Gamma$, *of the fluid density, the free stream velocity, and the circulation.*

[14a] The "drag" is as usual the component of force parallel to the flow; L is the component perpendicular to the flow. In Theorem 1, $D = L = 0$.

This theorem can be proved in the same way as Theorem 1, without assuming the Joukowsky condition. It is a corollary that the lift is

$$L = \tfrac{1}{2}\rho v_f^2\, c C_L^* \sin \alpha, \tag{13}$$

where c is the wing chord, α is the "effective angle of attack," and C_L^* depends only on the shape of the wing cross-section.

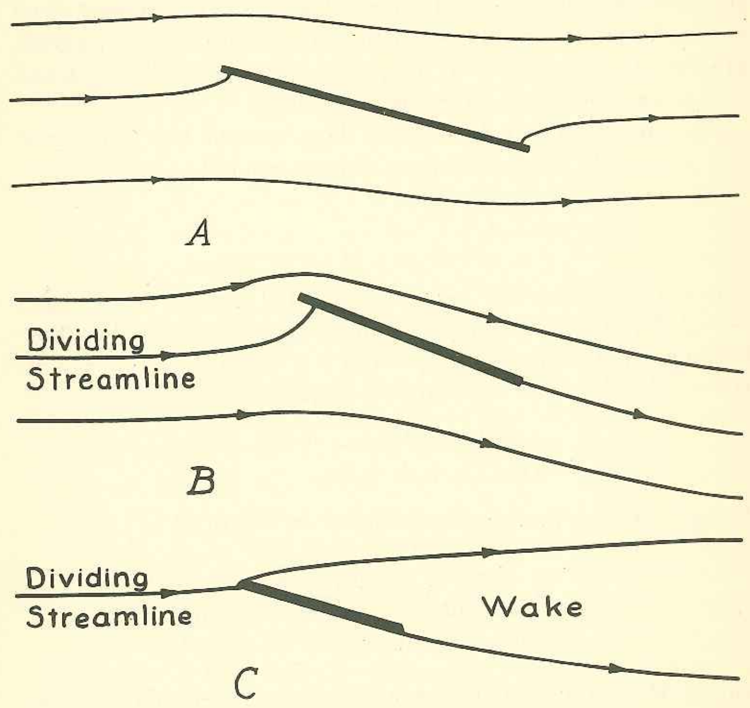

FIG. 2. Three ideal flows past inclined plate.

A similar asymptotic argument determines the moment in any Joukowsky flow,[15] and this gives the best known theoretical estimate for the variation in airfoil forces with wing cross-section. Nevertheless, the theory outlined above is subject to three paradoxes. The first of these is quite amusing

[15] The complete theory was given by R. von Mises, *Zeits. Flugtechnik und Motorluftschiffahrt*, 1917, pp. 157–163, and 1920, pp. 67–73 and 87–89. For Theorem 3, see [12], Vol. 2, p. 163; shorter proofs are known.

and purely mathematical in nature; it may be called the Cisotti Paradox (references for the full discussion may be found in [2]).

Consider the Joukowsky flow past an inclined flat plate in a horizontal wind (Fig. 2b). By the Kutta-Joukowsky Theorem, the resultant force should be vertical (i.e. pure lift). On the other hand, as all pressures are normal to the plate, the resultant force should be normal to the plate, giving a clear contradiction. The latter conclusion is confirmed by an integration along the plate of the pressure difference between the two sides; the detailed formulas follow.

The most general Joukowsky flow around the unit circle $z = e^{i\theta}$ has the velocity potential proportional to

$$U = Re\left[\frac{e^{i\alpha}z}{2} + \frac{e^{-i\alpha}}{2z} - ic \ln z\right],$$

where α and c are real. Since $2\zeta = z + 1/z$ maps $z = e^{i\theta}$ onto $-1 \leqq \xi \leqq 1$ [$\zeta = \xi + i\eta$], the most general Joukowsky flow around the segment $-1 \leqq \xi = \cos\theta \leqq 1$ has a velocity potential on this segment conveniently written as

$$\begin{aligned} U &= Re[\tfrac{1}{2}(e^{i(\alpha+\theta)} + e^{-i(\alpha+\theta)}) + c\theta] \\ &= \cos(\alpha + \theta) + c\theta. \end{aligned}$$

Using θ as a parameter, the velocity on the plate is

$$\begin{aligned} \frac{dU}{d\xi} &= \frac{-dU}{\sin\theta d\theta} = \frac{\sin(\alpha+\theta)}{\sin\theta} - \frac{c}{\sin\theta} \\ &= \cos\alpha + \sin\alpha\cot\theta - c\csc\theta. \end{aligned}$$

Hence the difference in pressure between similarly situated points on opposite sides θ, $-\theta$ is given by Bernoulli's Theorem as

$$\Delta p = 2\rho\cos\alpha[\sin\alpha\cot\theta - c\csc\theta];$$

the integral $\int_0^\pi \Delta p ds = -\Delta p \sin\theta d\theta$ is clearly convergent.

The resolution of the Cisotti Paradox leads one to the astonishing conclusion that there is a non-zero force acting on the single point at the front end! This is clearly a very pure example of a Singular Point Paradox, arising from a neglect

to study rigorously the behavior of the flow at all singular points.

A second mathematical paradox arises from the fact that, in space, the exterior of a wing of finite span is simply connected. It follows that a multiple-valued potential with zero vorticity (i.e. curl $\mathbf{u} = \mathbf{0}$) at all points is impossible;[16] hence no escape from the d'Alembert Paradox along the lines of two-dimensional airfoil theory is mathematically possible in three-dimensional space.

Even the two-dimensional theory, however plausible its assumptions, agrees doubtfully with experiment. It involves the following Fatness Paradox (see [1], p. 252). *Theoretically, the quantity* C_L^* *in* (13) *increases as the airfoil thickness increases; experimentally, it decreases as the airfoil thickness increases.*

9. *Discussion of Preceding Paradoxes*

In spite of the preceding paradoxes, the theory of an incompressible non-viscous fluid, or "ideal fluid," is extremely useful to the research engineer in dealing with fluids of small viscosity, at speeds up to one-fourth the speed of sound. Outside of a rather wide belt behind a given obstacle or airfoil B, called the "wake," and a very thin "boundary layer" near the surface of B (cf. Figs. 3a–3b), its predictions are reliable.

By accepting the wake and boundary layer as empirical facts, one can rationalize most of the preceding paradoxes. More specifically, one must recognize that, as the viscosity tends to zero, the width of the wake remains finite, and the "wake underpressure coefficient" $(p_f - p_w)/\frac{1}{2}\rho v^2$ (p_f = free stream pressure, p_w = wake pressure) does not tend to zero. This leads to a "drag coefficient" $C_D = D/\frac{1}{2}\rho v^2 A$ which does *not* tend to zero with the viscosity (see Fig. 1); and it gives a flow pattern which is clearly irreversible. Finally, the Fatness Paradox may be rationalized as the natural tendency of the

[16] This remark may be found in K. Friedrichs and R. von Mises, *Fluid Dynamics*, Brown University Lecture Notes, 1941. Nevertheless attempts have been made to compute lift forces on airships within the framework of incompressible non-viscous flow! In certain pumps, the propellers extend to the pipe walls, and so the fluid is multiply-connected; this topological fact has been said to give such pumps a superior efficiency.

wake to become "fatter" with the airfoil, a tendency which diminishes the circulation.

The preceding rationalizations are strongly confirmed by the fact that ([6], Chapter XII, or [12], Vol. 2, p. 81), by sucking away a very thin boundary layer, with a relatively minor expenditure of energy, Dirichlet and Joukowsky flows can be very closely approximated. Correspondingly, the d'Alembert and Fatness Paradoxes can be largely avoided. This suggests

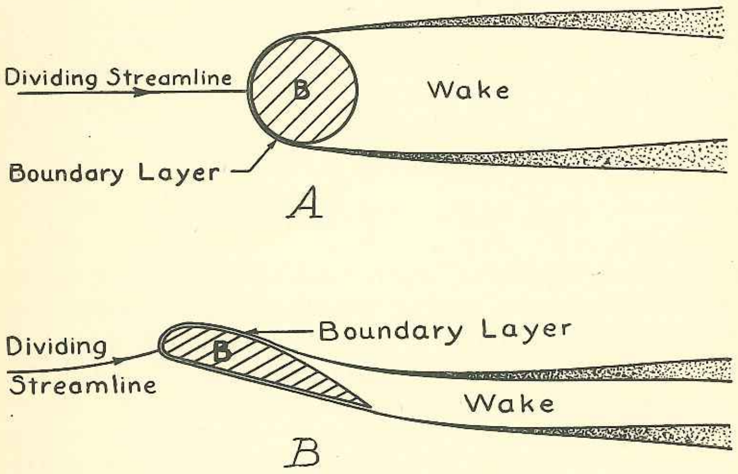

FIG. 3. Schematic diagrams of "real" flows.

that the prevailing view already mentioned (Sections 1 and 7), that the paradoxes of fluid mechanics are due to the neglect of viscosity, may be incorrect. It is conceivable that, when the Reynolds number R is very large, the concept of an ideal fluid is not incorrect, but merely incomplete[17] (cf. Theorem 2).

In support of this idea, it may be mentioned that the deduction of Dirichlet flows from the hypothesis of an "ideal fluid" involves implicitly two arbitrary topological assumptions:[18] (i) that the streamlines from the upstream side cover the whole plane, and (ii) that a locally single-valued velocity

[17] The sense in which the fundamental differential equations, etc., for an ideal fluid are "complete" is discussed in Chapter III, Section 11.

[18] Much of modern clarification of the foundations of geometry is due to the explicit consideration of Euclid's implicit topological assumptions—as in D. Hilbert's classic *Grundlagen der Geometrie*, Berlin, 1930.

potential U is single-valued in the large. There is no valid mathematical reason for excluding from consideration Joukowsky flows, flows with wake (see Chapter II), and many other topological types of flows.[19] Hence the paradoxes of ideal fluid theory may be, in part, *paradoxes of topological oversimplification.*

In any case the preceding examples make it clear that *the theory of non-viscous flows is incomplete.* Indeed, the reasoning leading to the concept of a "steady flow" is inconclusive; there is no rigorous justification for the elimination of time as an independent variable.

Thus though Dirichlet flows and other steady flows are mathematically possible, there is no reason to suppose that any steady flow is *stable.* It is perfectly conceivable that, in an "ideal" fluid, initially departing slightly from Dirichlet flow, irregularly varying turbulent "eddies" are built up mathematically in the "wake" of an obstacle—reproducing mathematically what is observed physically at large Reynolds' numbers R. Indeed, such a model has already been constructed by von Karman (*loc. cit.* supra), for the alternating vortices shed by cylindrical obstacles at intermediate Reynolds numbers, $50 < R < 1000$.

To admit this possibility, we must reject the idea that there is a necessary tendency towards symmetry in natural phenomena,[20] and admit the possibility that *a symmetrically stated problem may not have any stable symmetric solution.*

Extensive hydrodynamical evidence of this possibility is presented in Section 14–15. Admitting it provisionally, we

[19] For example: The "multiple solutions" of H. Villat (see *Leçons sur l'hydrodynamique*," Paris, 1929, Chapter VIII); the solutions with two symmetric vortices in a finite wake, introduced by Föppl (see [9], p. 223, or H. Bateman, *Partial Differential Equations*, 1932, p. 251, and refs. given there); Karman's flows with infinite vortex trails ([9], p. 225; or [10], p. 341); and the reverses of the preceding flows (see E. Picard, *Comptes Rendus*, *159* (1914) 639). It should be noted, however, that circulation or a wake of infinite length and breadth is needed to avoid the d'Alembert Paradox.

[20] This idea has been advanced by various philosophers of science; see L. L. Whyte, "Tendency towards symmetry in fundamental physical structures," *Nature*, *163* (1949), p. 762. For a general discussion, see P. Duhem, *La théorie physique, son objet et sa structure*, Paris, 1907, Part II, Chapter III.

are led to the conclusion that the paradoxes of ideal fluid theory are, in part at least, paradoxes of topological oversimplification and symmetry paradoxes, in which "apparently symmetric causes do not have symmetric effects." (The latter is especially evident in the case of the Reversibility Paradox, if one considers wake theory.)

At all events, the most obvious paradoxes can be attributed as much to lack of deductive rigor as to faulty "physical" assumptions or approximations. In further support of this view, I shall now show that the theories of compressible fluids and of viscous fluids (so-called "real" fluids) have their full share of paradoxes.

10. The Earnshaw Paradox

I shall first treat the paradoxes which *specifically* concern compressibility effects arising in the high-speed flow of non-viscous fluids, beginning with a paradox due to Earnshaw.[21]

EARNSHAW PARADOX. *Plane sound waves of finite amplitude and stationary form are mathematically impossible, in a gas which vibrates adiabatically.*

PROOF. We assume that the form of the sound waves is stationary, and that the train of waves moves with constant velocity normal to the wave fronts. Hence if we change to axes moving with the waves, the fluid motion will be not only one-dimensional but *steady*, and we can write $\rho = \rho(x)$, $u = u(x)$, etc. Neglecting gravity, Bernoulli's equation (6) reduces to $udu + dp/\rho = 0$. Moreover the equation of continuity (1) reduces to $\rho u = \text{const.} = C$, or $u = C/\rho$. Substituting back we get

$$-\frac{C^2 d\rho}{\rho^3} + \frac{dp}{\rho} = 0, \qquad \text{or} \qquad dp = \frac{C^2 d\rho}{\rho^2}. \tag{14}$$

Hence such a wave motion is possible only if the fluid satisfies an equation of state (2′) of the special form

[21] S. Earnshaw, *Phil. Trans.*, *150* (1860), 133–148. See also Stokes, *Phil. Mag.*, *33* (1848), 349; Rankine, *Phil. Trans.*, *160* (1870), 277; [13], Vol. V, p. 573, or *Proc. Roy. Soc.*, *A84* (1910), 247–284; [9], §283; [60], §51. Of course, the paradoxes of Sections 6–8 also apply to compressible non-viscous fluids.

$$p = p_0 - \frac{C^2}{\rho}. \tag{15}$$

There is no known gas whose adiabatic equation of state has the form (15).

We have here a typical paradox of overdeterminism; such paradoxes arise typically when too *many* approximate hypotheses are made. It would be hard to say that any one approximation was at fault; indeed, like many other paradoxes,[22] the Earnshaw Paradox contains the germ of an important truth. For waves of *large* finite amplitude, it leads to the correct and remarkable conclusion that the dense portions of such waves gain progressively on the more rarefied portions, until eventually *shock waves* occur where there is a near-discontinuity of density, pressure, and velocity.

Shock waves are now such a commonplace that it may be of interest to recall that Riemann treated them incorrectly, and that Lamb, even in the last edition of his classical *Hydrodynamics*, casts doubt on the validity of the now accepted Rankine-Hugoniot theory.[23]

From the Earnshaw Paradox one can easily derive a second paradox, noted independently by Mr. H. D. Ursell and myself. "Everybody knows" that there is a close analogy between the theory of shallow surface waves, and compression waves in a gas whose equation of state is $p = k\rho^2 + c$, with $\gamma = 2$ (cf. Chapter III, Section 15); this analogy has been effectively exploited recently by J. J. Stoker.[24] Yet it was proved by D. J. Struik in 1925 that shallow surface waves of finite amplitude were possible; and we have just proved that waves of

[22] Thus the d'Alembert paradox contains the germ of the following useful engineering principle: By "sucking away the boundary layer," in a fluid of sufficiently low viscosity, resistance can be reduced to nearly zero, by an operation which involves very little energy.

[23] Lamb ([9], p. 486) states that "no physical evidence is adduced in support of the proposed law," and that "in actual fluids, the equation of energy cannot be satisfied consistently. . . . " I am indebted to Mr. R. H. Kent for this reference.

[24] See *N.Y.U. Communications in Applied Mathematics*, *1* (1948), 1–87 and refs. given there. The analogy was apparently first noted by E. Jouguet, *Jour. de math. pures appl.*, *3* (1920), p. 1. For Struik's results, see *Rendic. Lincei*, *1* (1925), 522–527.

finite amplitude are impossible for $\gamma \neq -1$. What is the explanation? Nobody knows.

11. *Von Neumann's "Triple Shock" Paradox*

Since shock waves are now in the center of popular attention, it may be of interest to call attention to a new paradox regarding shock waves.

The mathematical theory of plane shock waves can be derived simply from the laws of conservation of mass, momentum, and energy (heat plus kinetic energy), *provided* one

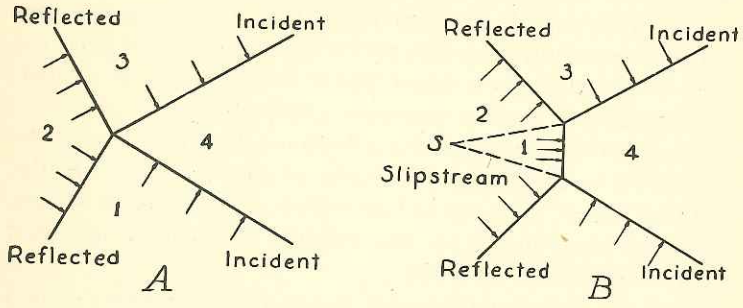

FIG. 4. Shock wave reflections.

assumes that on each side of the shock wave all relevant physical variables (temperature, density, pressure) assume limiting values. The predictions of the Rankine-Hugoniot theory derived from these paws seem to be closely confirmed experimentally, as regards *single* shock waves.

Furthermore, corresponding mathematical predictions are possible in the case of double "regularly" reflected shock waves (see Fig. 4a) and of "triple shocks" or "Mach Y" waves (see Fig. 4b), provided it is assumed that in each relevant sector (1,2,3,4) physical variables assume limiting values near the singular point at the shock "vertex." Again, many of these predictions are confirmed experimentally, so that the theory has a highly respectable status.

However, in the case of "weak" shocks, regular reflections occur for angles of incidence somewhat greater than those permitted by theory, and *the predicted limits for triple shocks seem to differ grossly from those observed.* This discrepancy

was apparently first discovered by J. von Neumann (1945); in spite of various attempts, based on the idea that this is a singular point paradox no satisfactory explanation of it has yet been advanced.[25]

12. *Kopal's Paradoxes of Linearized Projectile Theory*

Time does not permit me to give the details here of the interesting and useful "linearized theory" for forces on slender projectiles moving at supersonic speeds, developed by von Karman and Moore.[26] The basic approximation is the assumption that velocities are perturbed relatively little from the free stream velocity. An analogous theory exists for wing forces (see also Chapter III, Section 15).

Now in the case of a slightly yawed cone-headed projectile, a comparison with a more rigorous calculation, based on the exact Taylor-Maccoll theory (Chapter IV, Section 4) for cones with zero yaw, is possible.

The linearized equations originally proposed by Karman, Moore, and Tsien (*op. cit.*) underestimate by ten to fifty per cent the head drag and pressure coefficients deduced from the exact equations in case of moderate velocities (technically, when the projectile head is much more slender than the "Mach cone" starting from the projectile tip). Moreover in the case of yawed cones, the cross-forces predicted by the perturbation methods constituting the "linearized theory" decrease with increasing Mach number, whereas those predicted by perturbation of the Taylor-Maccoll exact solutions, increase with increasing Mach number.

These discrepancies were revealed clearly by laborious calculations made during the late war. They are purely mathematical paradoxes due to lack of rigor in computing the

[25] A comprehensive review of the subject has been given by H. Polachek and R. J. Seeger, "On shock-wave phenomena: interaction of shock waves in gases," *Proc. Symposia in Applied Math.*, Vol. I, pp. 119–144, Am. Math. Soc., 1949. See especially p. 131 and the references given there. See also W. Bleakney and A. H. Taub, *Revs. Mod. Phys.*, *21* (1949), 584–605.

[26] Th. von Karman and N. B. Moore, "Resistance of slender bodies," *Trans. Am. Soc. Mech. Eng.* (1932), p. 303; H. S. Tsien, *Jour. Aer. Sci.*, *5* (1938), p. 480.

relative orders of magnitude in the asymptotic approximations ("perturbations") made, and from failure to determine the order of magnitude of the effect on the resulting calculations.

They show up strikingly the insecure foundations of the asymptotic theory of partial differential equations, alias "perturbation theory." Recent new "linearizations" of the equations of supersonic flow have attempted to rectify the situation.[27]

13. *Viscous Incompressible Fluids*

I now come to paradoxes involving the theory of viscous incompressible fluids. In this connection, I recall that the Equations of Motion for a viscous compressible fluid have never been proved correct, and that, according to Paul Duhem,[27a] the supposition of an incompressible viscous fluid is thermodynamically incompatible with the concept of a fluid. However, these remarks need not concern us further.

In an "incompressible" *viscous* fluid, the equations of continuity and state reduce to the single condition

$$(16) \qquad \rho = \rho_0, \qquad \text{or equivalently} \qquad \operatorname{div} \mathbf{u} = \sum \frac{\partial u_k}{\partial x_k} = 0.$$

These are combined with the Equations of Motion (3*). The only kinematical limitation imposed by these equations is that curl (grad p) $= \nabla \times (\nabla p) = \mathbf{0}$, which, if $\mathbf{g} = \nabla G$, reduces to

$$(17) \qquad \frac{d\boldsymbol{\xi}}{dt} = \nu\nabla^2\boldsymbol{\xi} + (\boldsymbol{\xi}\cdot\nabla)\mathbf{u}. \qquad [\boldsymbol{\xi} = \operatorname{curl} \mathbf{u}]$$

The boundary conditions are

$$(18) \qquad \mathbf{u} = 0 \text{ on any fixed rigid surface,}$$

instead of merely that the *normal* velocity component (8) is zero.

It is commonly (but erroneously) believed that the theory of viscous fluids is free from paradoxes. One reason for this

[27] See E. V. Laitone, *Jour. Aer. Sci.*, *14* (1947), 631–642; also Broderick, *Quart. J. Mech. Appl. Math.*, *2* (1949), 98–120 and 121–128; M. J. Lighthill, *Jour. Aer. Sci.*, *16* (1949), p. 75. The perturbed Taylor-Maccoll theory was first developed by A. H. Stone.

[27a] *Traité d'energétique*, Vol. 2, p. 131, Paris, 1900.

is perhaps that the drag coefficient can often be plotted as a function $C_D(R)$ of the Reynolds number alone (cf. Chapter III, Section 13, and Figure 1). Other experimental confirmations are found in the theory of lubrication, and in some parts of the theory of turbulence.

In support of the view that the paradoxes of fluid mechanics are due to an unjustified neglect of viscosity, the following two mathematical remarks are often made. In non-viscous flow, (18) cannot be satisfied, except in the trivial case of no motion; hence the real boundary conditions are completely altered, in the approximation of zero viscosity. Again, the term of highest order in the Equations of Motion is suppressed, completely altering the type of these differential equations.

One can easily see that the preceding criticisms have some merit, by considering the solutions of the ordinary differential equation $\epsilon y'' + y = 0$, as $\epsilon \to 0$. If one uses the boundary conditions $y(0) = a$, $y(1) = b$, one gets an interesting contrast between the cases $\epsilon \uparrow 0$ and $\epsilon \downarrow 0$.

Nevertheless, I think they are inconclusive. By "sucking away the boundary layer" one can approximately realize the flow patterns predicted by the theory of non-viscous fluids. And clearly, with a sufficiently thin boundary layer,[28] (18) is approximately equivalent to the weak assertion that the normal component of velocity vanishes. I therefore think that the real question is, why does separation of the boundary layer occur? Thus it concerns the *stability* of nearly non-viscous flows.

I shall now show that similar problems of stability arise in the theory of (incompressible) viscous fluids.

14. *The Paradox of Turbulence in Pipes*

Consider the steady flow of a viscous incompressible fluid through an infinitely long straight circular pipe of radius c.

[28] More precisely, suppose we use the following test, proposed by Harold Jeffreys, for the validity of approximations: substitute the solution of the approximate equations in the exact equations, and estimate the size of the terms neglected. If ν is small, we can introduce a shear flow in a very thin boundary layer, by means of very small forces; physically, this can be done by "sucking away the boundary layer." It is clear that the preceeding test gives no information about stability.

Let x denote distance parallel to the pipe, and r distance from the axis of the pipe. Let further u_x, u_r, and u_θ be the axial, radial, and angular components of velocity, in cylindrical coordinates. We have the following paradox.

TURBULENCE PARADOX. The only flows possessing the symmetry of the hypotheses of steady viscous flow in a circular pipe are

$$u_x = a(c^2 - r^2), \qquad u_r = u_\theta = 0 \tag{19}$$

Such "Poiseuille flows" are observed if $R < 1000$, but not usually if $R > 10{,}000$.

PROOF. The hypothesis of "steady flow" means that $\mathbf{u}$ should be a function of r, x, θ alone, and not of t. The statement of the problem is invariant under reflection in every plane through the pipe axis; the flow has this symmetry only if $u_\theta = 0$ and $u_x = f(x,r)$, $u_r = g(x,r)$. The statement is also invariant under translations along the pipe axis; so are conditions (16)–(18) since ν is assumed independent of pressure; the flow has this symmetry only if $u_x = f(r)$ and $u_r = g(r)$. By (16), this implies $u_r = 0$ (equivalently, the *stream function* is a function of r alone).

We now use (3*). Since $u_2 = u_3 = 0$ identically, it gives $p = p(x)$. Now considering the x-component of (3*), we get

$$p'(x) = \mu\nabla^2 u_x = \mu\left[f''(r) + \frac{f'(r)}{r}\right]. \tag{20}$$

The right-hand side of (19) is independent of r, since the left-hand side is. Hence $(rf')' = rf'' + f' = kr$ for some constant k, and $rf' = \tfrac{1}{2}kr^2 + C$. This gives finite $u_x = f(r)$ at $r = 0$ only if $C = 0$; hence $f' = \tfrac{1}{2}kr$ and $f(r) = \tfrac{1}{2}kr^2 + b$. To satisfy condition (18) of adhesion to the pipe walls, we must have $u_x = a(c^2 - r^2)$, completing the proof of (19).

Experimentally, though (19) is usually observed if $R < 1000$, it commonly ceases abruptly near $R = 2000$. This is shown in Fig. 17, p. 101; see also [12], p. 31; [14], p. 246. Above $R = 2000$ the flow becomes turbulent (see Fig. 4b), and the instantaneous velocity field has neither the temporal nor the spatial symmetry of the hypotheses.

It may also be observed that the experimental facts just noted seem to contradict the Principle of Least Action, since Poiseuille flow involves much less expenditure of energy than turbulent flow.

15. Other Symmetry Paradoxes

It is most peculiar to have Nature's love of symmetry suddenly cease to be verified physically near $R = 2000$. This is reminiscent—though the limit is far less precise[29]—of Nature's abhorrence of a vacuum, which is limited to thirty-five feet of water. Moreover there are other cases in hydrodynamics where an apparent symmetry of causes fails to produce symmetric effects.

One such case has been carefully studied by Sir Geoffrey Taylor in a classic paper.[30] Consider a viscous liquid between two long coaxial cylinders, rotating with constant angular velocities W and W' in opposite directions. The problem described has (approximate) symmetry under translation along and rotation about the axis of the cylinders, and is independent of time. There is only one solution of the Navier-Stokes equation (3*) and (18) which also possesses these symmetries; it is called "Couette flow." At low speeds Couette flow is observed. But it is replaced at high Reynolds numbers by an unsymmetrical flow which is, however, *not* turbulent: the symmetry in time is preserved, through spatial symmetry is not.

Again, consider a small air bubble, rising in water under its own buoyancy. Under surface tension, it will be nearly spherical—and in any case there is no cause which is not symmetric about a vertical axis through the centroid of the bubble. Hence, by symmetry, the bubble should rise vertically. Yet it is a striking fact[31] that if $R > 50$, the bubble ascends in a vertical spiral instead!

[29] Nature's abhorrence of a vacuum is susceptible to some diurnal variation; her desire for a symmetrical Poiseuille flow is influenced by wall roughness and initial turbulence; cf. infra.

[30] "Stability of a viscous liquid contained between two rotating cylinders," *Phil. Trans.*, *A223* (1922), 289–343.

[31] See Nisi and Porter, *Phil. Mag.*, *46* (1923), p. 754, also [50], pp. 23–24.

An analogous phenomenon occurs in the wake behind a cylinder pulled broadside through a stream. It is not symmetrical if $R > 50$, but consists of alternating vortices (Bénard-Karman vortex street).

The above examples might seem at first sight to contradict Leibniz's metaphysical Principle of Sufficient Reason,[32] that a symmetry of causes must be preserved in the effects. A deeper view is that, *although symmetric causes must produce symmetric effects, nearly symmetric causes need not produce nearly symmetric effects:* a symmetric problem need have no *stable* symmetric solution. This possibility is the real source of Symmetry Paradoxes.

In support of this view, it may be noted that even with turbulent flow in circular pipes, the *mean* velocity $\bar{\mathbf{u}}$ is found to satisfy $\bar{u}_r = \bar{u}_\theta = 0$, $\bar{u}_x = F(r)$. Whereas in pipes not having circular symmetry, $\bar{u}_r \neq 0$ ([14], p. 267); hence the symmetry arguments of Section 14 are physically relevant. But we do have to realize that, in order to be sure of verifying this experimentally, we must first prove that the symmetry is stable.

Now, wherever it is necessary to analyze the *stability* of a deduction, since an analysis of the stability of a mathematical deduction is far more complicated than the deduction itself, it is almost certain that the deduction will be made long before its instability can be tested. From this principle, we may anticipate a continuing stream of symmetry paradoxes.

Furthermore, in the present instance, it is by no means clear that Poiseuille flow is unstable in the usual sense, for "infinitesimal" disturbances. The available mathematical evidence for the plane analog of Poiseuille flow seems to indicate that it is stable for such disturbances.[33] Moreover

[32] A modern formulation of this has been given by George D. Birkhoff [57]. It is curious that formal systems of mathematical logic seem to have dispensed with this principle, whose applicability to physics was observed in 1894 by Pierre Curie ([4], p. 119).

[33] See J. L. Synge, "Hydrodynamical stability," *Am. Math. Soc. Semi-centennial Publications*, Vol. 2, 227–269, New York, 1938; H. B. Squire, *Proc. Roy. Soc.*, *A142* (1933), 621–627; C. L. Pekeris, *Phys. Rev.*, *74* (1948), 191–199. For recent physical data, see MM. Fortier and Comolet, *La Houille Blanche*, numéro spécial B (1949), p. 673.

this evidence is in some sense confirmed by the fact that the onset of turbulence can be greatly delayed by using sufficiently smooth walls and rounded entrances ([12], Vol. 2, p. 33; [14], p. 172), up to $R = 75{,}000$. Thus Poiseuille motion may well be stable locally, but unstable in the large.

From all these facts, one begins to get an idea of the difficulties and confusion which surround the foundations of hydrodynamics, and of the uncertainty of symmetry arguments!

16. The Eiffel Paradox

Another paradox, related physically to the Turbulence Paradox, was found in 1912 by Constanzi and Eiffel.[34] It may be stated as follows.

EIFFEL PARADOX. *Near a "critical Reynolds number" $R_c \cong 150{,}000$, the fluid resistance experienced by a sphere actually decreases as the velocity increases.*

This contradicts our general physical experience; thus it is a purely experimental paradox. It contradicts even more flagrantly the result "proved" by dimensional analysis in many college textbooks, that fluid resistance is proportional to the velocity at low speeds, and to the velocity squared at higher speeds (see Chapter III, Section 3).

It is especially astonishing if we recall that R is generally supposed to express the "ratio between inertial forces and viscous forces" (cf. [12], Vol. 2, Chap. I; and Chap. III infra). How can the change in this ratio, from 10^{-5} to 0.5×10^{-5}, cause such a striking effect? Thus, unlike the paradoxes discussed in Section 9, the Eiffel paradox seems to be a genuine Paradox of Approximation: a minute decrease in viscosity causes a large change in the actual flow, separation is delayed, and the wake becomes narrower.

Of course, the cause is now well-known: the change induces transition of the boundary layer from laminar to turbulent flow! This transition is clearly visible in good spark photographs. Thus the Eiffel Paradox is associated with the *stability* of laminar flow, and hence to the discussion of Section 14.

It seems to me that this example throws great light on the shortcomings of the conventional theory of partial differential

[34] The references are given in [12], Vol. 2, p. 98.

equations. Certainly, the classical asymptotic theory of partial differential equations ("perturbation theory") fails utterly to predict a sudden change in topological nature of the whole solution when one coefficient is changed from 10^{-5} to 0.5×10^{-5}. Hence it would seem that no treatment of "slightly viscous" fluids which is based on this theory,[35] can come close to describing the real behavior of such fluids. It will never predict a turbulent wake, a vortex street, or "transition" at R_c.

Instead, what is needed is an attempt to deduce, from *a priori* mathematical considerations, the possibility of sudden changes in regime induced by small changes in the coefficients of partial differential equations. That is, mathematics must take advantage of physical inspiration, as I have already stressed (Section 1).

The phenomenon of turbulence raises interesting questions regarding the differentiability of the functions $u_i(x_1,x_2,x_3;t)$. It seems well established that the u_i are to some extent random functions. And by Norbert Wiener's classic theorem, "almost all" purely random functions are non-differentiable.[36] This fact has led some scientists to believe that one must go back to (discontinuous) molecular considerations in fluid mechanics, in dealing with turbulence. However, the fact that R_c is the same in liquids as in gases, makes this doubtful. One recent guess, based on the statistical theory of turbulence, is that the u_i have fifth but not sixth derivatives.

17. *Stokes' Paradox*

One of the great triumphs of theoretical hydrodynamics, is Stokes derivation of the formula

$$D = 6\pi\mu c v \tag{21}$$

[35] Such as, for example, the work of the school of Oseen [11], which has been so useful in the opposite asymptotic case, $R \to 0$. The phenomenon has no analog in compressible flow, though the drag of a slender-nosed projectile has a near-discontinuity at $M = 1$ (see Karman and Moore, *op. cit.*, supra).

[36] See J. L. Doob, *Annals of Math.*, *43* (1942), 351–369, and its bibliography. The principle that solutions of the Navier-Stokes equations will tend not to be analytic, was deduced by P. Duhem on other grounds in p. 121 of Vol. 2 of his *Traité d'energétique.*

for the resistance D encountered by a rigid sphere of radius c moving with constant speed v in a fluid of viscosity. This formula, which is verified experimentally[37] for Reynolds numbers $R < 0.2$, may be deduced from conditions (16)–(18), by neglecting the "inertial" term in du/dt, and assuming that the flow is steady and has axial symmetry—i.e. that no Symmetry Paradox is involved. This simplification is said to correspond to "creeping flows."

It seems reasonable to make the same simplifications in two dimensions. However, if this is done, we are confronted with:

STOKES' PARADOX. *No steady "creeping flow" of a viscous fluid past an infinite circular cylinder is possible.*

ROUGH PROOF (I know of no rigorous proof). By (3*), neglecting du/dt and $\mathbf{g}$, we get

$$\nabla^2 p = \mu \sum \frac{\partial}{\partial x_i}(\nabla^2 u_i) = \mu\nabla^2\left(\sum \frac{\partial u_i}{\partial x_i}\right) = 0; \tag{22}$$

hence p is harmonic. Again, "by symmetry" (!), $u_1(-x_1,x_2) = u_1(x_1,x_2)$, while $u_2(-x_1,x_2) = -u_2(x_1,x_2)$; hence the total flux of momentum out of a large circular cylinder concentric with the small cylinder, is zero. Hence, by the momentum equation (cf. Section 6), the pressure-thrust on the two cylinders must be the same. Since p is harmonic by (22), we infer $p - p_\infty = A \cos \theta/\pi r + 0(1/r^2)$, and so by (3*),

$$\nabla^2 u_2 = \frac{\partial p}{\partial y} = \frac{A \sin 2\theta}{2\pi r^2} + 0\left(\frac{1}{r^2}\right).$$

But if u_2 can be expanded in a power series at infinity, with $u_2 \to 0$, we must have $\nabla^2 u_2 = 0(1/r^3)$; hence $A = 0$ and we have a contradiction.

Oseen and Lamb[38] have rationalized Stokes' Paradox as a singular point paradox, pointing out that at very large distances the acceleration terms dominate the viscosity terms and hence should not be neglected. By definition, it can also be regarded as a paradox of overdeterminism.

[37] [12], Vol. 2, p. 100. If the sphere is in a gas where the mean free path is comparable with c, a correction must be made for "slipping"; cf. [6], Vol. 2, Appendix.

[38] [11], p. 162; [9], p. 614; see also [13], Vol. VI, pp. 29–40.

Stokes' Paradox is analogous logically to the paradox of a two-dimensional underwater explosion. Though there is a simple and extremely useful theory of the underwater oscillations of spherical bubbles,[39] it is easily shown that any two-dimensional notion of a bubble involves infinite kinetic energy, with $U = \log r + 0(1/r)$. Hence no two-dimensional bubble oscillations can be induced by finite forces. The paradox of a two-dimensional underwater explosion is clearly also a singular point paradox; at very large distances the effect of compressibility in absorbing the expansion is dominant.

18. The Rising Bubble Paradox

Even in the case of spheres, the theory of "creeping motions" leads to a curious paradox. Let a small air bubble rise under buoyancy in a liquid, the bubble being so small that surface tension keeps it nearly spherical. Assume as above the approximation of "creeping motion," that $du/dt = 0$ in (3*). Since the bubble is fluid, it is logical to replace (18) by

(18′) $\mathbf{u}$ *is continuous across the surface of the drop,*

as pointed out by Rayleigh.

The mathematical problem defined in this way was solved by Riabouchinsky and Hadamard,[40] who showed that the theoretical drag was given by the formula

$$D = \frac{6\pi c v \mu(2\mu + 3\mu')}{(3\mu + 3\mu')}. \tag{21′}$$

In (21′), μ is the viscosity of the medium, and μ' is that of the bubble moving through it.

So much for theory; experimentally, the drag seems to be given by (21) instead of (21′) (cf. [12], vol. 2, p. 115, or [14], p. 215). That is, the bubble seems to behave as if it were

[39] [9], p. 122. An analogous paradox in the theory of heat conduction is obtained if one looks for a steady temperature distribution in the plane, maintained at 100° on the circumference of a circle, and tending to 0° at infinity. See also [13], Vol. 6, pp. 29–40.

[40] D. Riabouchinsky, *Bull. Acad. Sci. Cracovie* (1911), p. 40; J. Hadamard, *Comptes rendus, 152* (1911), 1735. The experimental data are thoroughly discussed on p. 190ff. of G. Barr, *A Manual of Viscometry*, Oxford, 1931. See also T. Bryn, *Forschung. Ingenw. 4* (1933), 27–30.

rigid. This contradiction between theory and experiment may be called the Rising Bubble Paradox.

What is the explanation of this paradox? It has been suggested by Bond[41] that the cause is surface energy, and many of Bond's measurements lead to (21′) instead of (20). However, the explanation is far from clear. It may be that a rigid film forms at the interface, especially if impurities are in solution, but all that one can say with certainty is, that the mechanics of "real" surface behavior are far less simple than the classical treatments would suggest.

19. "Drift"; Magnus Effect Paradox

Artillerymen have known for over a century that spinning bullets tend to "drift" out of the vertical plane in which they are fired; and that the drift is in the direction of rotation of the top of the bullet. However, this phenomenon was not correctly understood for many years.[42]

A considerable vogue was enjoyed by the following quite fallacious qualitative "explanation" of drift, due to the great mathematician Poisson. Due to inertia, the axis of the bullet will lag behind the tangent to the trajectory, as sketched in Fig. 5a. Hence there will be more pressure on the underside; hence more friction there. Hence (cf. Fig. 5b) there will be a drift in the observed direction.

The fallaciousness of this qualitative explanation can be seen easily by applying the same argument to a spinning tennis ball: it yields the erroneous conclusion that a tennis ball with topspin should rise! Moreover a quantitative study of the gyroscopic stability of bullets reveals that the stable position of the bullet axis is (with right-handed rifling) to the right of the trajectory tangent. The drift of the bullet is thus due to the aerodynamic *cross-force*, and only indirectly to the spin.

Why is Poisson's explanation inapplicable to tennis balls? Since every college physics student is given a pat qualitative

[41] W. N. Bond, *Phil. Mag., 4* (1927), 889–898. For a modern discussion of the real facts of surface energy, see N. K. Adam, *The Physics and Chemistry of Surfaces*, Oxford, 1931, esp. Chapter II.

[42] An historical account may be found in [3], Chapter X.

explanation of the Magnus effect, I do not think I need to say. Instead, I should like to point out that the usual explanation is also dubious, in view of the following paradox.

MAGNUS EFFECT PARADOX. *At low rates of spin, the actual deflection is in the opposite direction to that predicted by the conventional explanations.*[43]

Actually, a possible qualitative explanation of the Magnus Effect Paradox can be based on Prandtl's boundary layer theory. Due to rotation, the low pressure "wake" is shifted

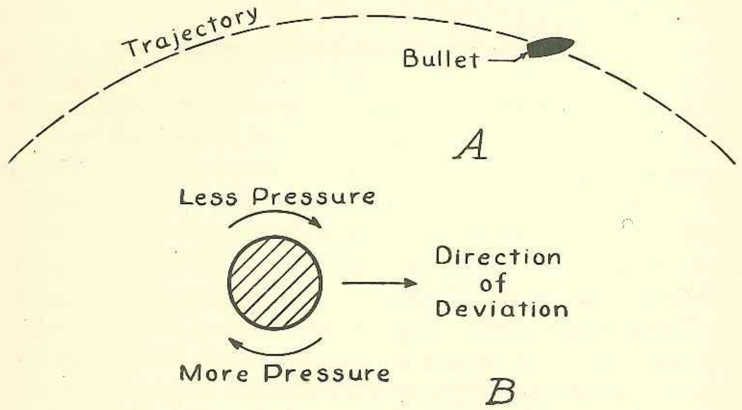

FIG. 5. Fallacious explanation of "drift."

up by the topspin (cf. [12], Vol. 2, Plates 7–9), and the stagnation point down; this would cause a lift.

I think that Poisson's fallacious explanation and the Magnus Effect Paradox both illustrate the unreliability of qualitative explanations. One can argue cynically that qualitative deductions have a fifty per cent chance of appearing verified, no matter whether they are correct or not! I think that students should be taught not to set much store by them; they are very corrupting to one's critical sense.

20. *A Role for Mathematicians*

What is the moral of the preceding paradoxes? Certainly, they show the necessity of comparing theory with experiment,

[43] See [6], p. 504; the data are due to Maccoll.

and consequently show the utter falsity of the following widely quoted statement of Eddington: "An intelligence, unacquainted with our universe, but acquainted with the system of thought by which the human mind interprets to itself the content of its sensory experience, should be able to attain all the knowledge of physics which we have attained by experiment.[44]

To my mind, they also demonstrate the need which the theory of partial differential equations has for fresh physical inspiration. Why should a small change in viscosity affect fluid flow drastically, when a small change in compressibility does not?

On the other hand, they seem to me *not* to support the impression, widely current among engineers and physicists, that mathematical deduction should be supplanted by more "physical" reasoning. Indeed, the free use of approximations, topological oversimplifications, and non-rigorous symmetry considerations, which were seen to underly these paradoxes, are more typical of "physical" than "mathematical" reasoning. I think the very phrase "physical reasoning" implies a confusion of deductive and inductive considerations, which corrupts the fundamental meaning of the word "theory." This corruption is readily recognized by the confusion of students when the "theory" is taught, and by the unreliability of predictions when the "theory" is applied. Thus it is all too common to read of "satisfactory" agreement between theory and experiment, when the agreement should give little satisfaction to any true scientist.

I agree that oversimplifications based on the "right" approximations, made possible by familiarity with experimental results, greatly accelerate technological and even theoretical progress. Such oversimplifications led to the concept of an "ideal fluid," which is still very useful. But I think that mathematicians can perform a useful service if they will analyze critically these oversimplifications, by the deductive method, and so establish their limitations more clearly.

[44] Sir Arthur Eddington, *Relativity Theory of Protons and Electrons*, Cambridge, 1936, p. 327. They might also give pause to social theorists who rely exclusively on deductive logic in the infinitely more complicated field of social relations.

I realize that this service will often be deprecated by those "scientists" who use science instead of building it. But it is encouraging to know that its value was recognized by no less a person than Lord Rayleigh. Apparently Maxime Bôcher had written Rayleigh, questioning the statements about Sturm-Liouville expansions in Section 187 of the latter's celebrated *Theory of Sound*. Rayleigh replied,[45] with characteristic candor: "I doubt if I had any special method in my mind when I wrote Section 187. At any rate, if I had then, I have not now. I think I took this case as covered by the general theory as in Chapter IV. I have often wished that mathematicians would devote themselves to improving the rigor of this and similar demonstrations, rather than to inventing special methods for the particular cases."

21. *Mathematics as Reliable as "Common Sense"; Conclusion*

Moreover mathematics is not only useful in "tidying up" reasoning, whose broad lines were suggested by physical intuition. In many cases, mathematical deductions will rapidly give results which are at variance with physical intuition, and which it would be extremely laborious to derive experimentally. I shall close by giving two examples of such results.

Experimental data must frequently be corrected for "wall effects" which are much larger than common sense would suggest. In principle, wall effects can be determined empirically by removing the wall to larger and larger distances, but this may be prohibitively expensive in practice. Thus, Ladenburg has shown that a sphere falling slowly in a cylindrical tube of viscous liquid, having 100 times the cross-section area of the sphere, encounters 20 per cent more resistance than if there were no walls.[46]

Finally consider the celebrated Taylor-Maccoll supersonic flow past a conical projectile (Chapter IV, Section 4). This flow has the peculiar property of having streamlines at constant pressure along the cone wall. It is well known that, according to the mathematical theory of non-viscous fluids, the region behind any streamline at constant pressure can be replaced theoretically by motionless fluid, as in the Kirchhoff wake theory (Chapter II). Hence, mathematically, supersonic flow past a flat disc is possible, in which an invisible conical air barrier separates the onrushing air from the disc.

This flow pattern is instantly rejected by intuition and common sense, as absurdly unstable. The rejection is on the same basis as the rejection of the reverse of the Kirchhoff flow with wake, i.e. as the rejection of a (mathematically possible) upstream wake. It seems highly plausible that the effects of an obstacle should lie on the downstream side of the cause.[47]

Yet in this case common sense seems to be more fallible than mathematical deduction! Spark shadowgraphs, taken at the Ballistic Research Laboratories in Aberdeen (see frontispiece), indicate that a very thin needle in front of the disc is actually capable of inducing something quite like this "absurd" flow.

In view of these facts, I think the mathematician need have no inferiority complex about the soundness of purely deductive reasoning (as contrasted with "physical reasoning") in fluid mechanics, *provided* he clearly understands the preceding hydrodynamical paradoxes.

[45] The letter is in my possession.

[46] B. Ladenburg, *Annalen der Physik*, *23* (1907), 447; A. W. Francis, *Physics*, *4* (1933), 403–406. Another surprisingly large wall effect, arising with cavity flows, is discussed in Chapter II, Section 11.

[47] The same intuition makes *retarded* potentials give the "physically preferred" solutions of the wave equation, and is the basis of selecting the corresponding solutions of the linearized equations of supersonic flow. In the case of shock waves, this selection is bolstered by an appeal to the Second Law of Thermodynamics; in some sense, this law also seems relevant to the development of turbulent disorder in the wake *behind* an obstacle.