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STABILITY AND INSTABILITY IN
NINETEENTH-CENTURY FLUID MECHANICS

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There is scarcely any question in dynamics more important for Natural Philosophy than the stability of motion.

W. Thomson and P.G.Tait [1867, § 346]

ABSTRACT. — The stability or instability of a few basic flows was conjectured, debated, and sometimes proved in the nineteenth century. Motivations varied from turbulence observed in real flows to permanence expected in hydrodynamic theories of matter. Contemporary mathematics often failed to provide rigorous answers, and personal intuitions sometimes gave wrong results. Yet some of the basic ideas and methods of the modern theory of hydrodynamic instability occurred to the elite of British and German mathematical physics, including Stokes, Kelvin, Helmholtz, and Rayleigh. This usually happened by reflecting on concrete specific problems, with a striking variety of investigative styles.

RÉSUMÉ. — STABILITÉ ET INSTABILITÉ EN MÉCANIQUE DES FLUIDES AU XIXᵉ SIÈCLE. — Au dix-neuvième siècle, la stabilité ou l’instabilité de quelques écoulements simples fut l’objet de conjectures, de débats et parfois de preuves mathématiques. Les motivations pour ce type de recherche variaient considérablement, de la turbulence observée d’écoulements réels à la permanence attendue dans les théories hydrodynamiques de la matière. Les mathématiques contemporaines étaient rarement en mesure de fournir des réponses rigoureuses et les intuitions des uns et des autres conduisirent parfois à des résultats faux. Néanmoins, quelques grands de la physique mathématique britannique et allemande — Stokes, Kelvin, Helmholtz et Rayleigh — développèrent certaines idées et méthodes de base de la théorie moderne des instabilités hydrodynamiques. Ils y parvinrent en réfléchissant à des problèmes spécifiques concrets, avec une étonnante diversité de styles.

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Instability of motion haunted celestial mechanics from the beginning of Newtonian theories. In the nineteenth century, it became a central question of the developing fluid mechanics, for two reasons. Firstly, the discrepancy between actual fluid behavior and known solutions of the hydrodynamic equations suggested the instability of these solutions. Secondly, the British endeavor to reduce all physics to the motion of a perfect liquid presupposed the stability of the forms of motion used to describe matter and ether. Instability in the former case, stability in the latter needed to be proved.

In nineteenth-century parlance, kinetic instability broadly meant departure from an expected regularity of motion. In hydrodynamics alone, it included unsteadiness, non-uniqueness of motion, sensibility to infinitesimal local perturbation, sensibility to infinitesimal harmonic perturbations, sensibility to finite perturbations, sensibility to infinitely small viscosity. This spectrum of meanings is much wider than a modern treatise on hydrodynamic stability would tolerate. A narrower selection would not befit a historical study, for it would artificially separate issues that nineteenth-century writers conceived as a whole.

The first section of this paper is devoted to George Stokes’ pioneering emphasis on hydrodynamic instability as the probable cause of the failure of Eulerian flows to reproduce essential characteristics of the observed motions of slightly viscous fluids (air and water). Stokes believed instability to occur whenever the lines of flow diverged too strongly, as happens in a suddenly enlarged conduit or past a solid obstacle. The second section recounts how Hermann Helmholtz (1868) and William Thomson (1871) introduced another type of instability, now called the Kelvin-Helmholtz instability, following which the discontinuity surface between two adjacent parallel flows of different velocities loses its flatness under infinitesimal perturbation. Helmholtz thus explained the instability of a jet of a fluid through a stagnant mass of the same fluid, for instance the convolutions of the smoke from a chimney. Thomson’s motivation was the theory of wave formation on a water surface under wind.

In the Helmholtz-Kelvin case, instability was derived from the hydrodynamic equations. In Stokes’ case, it only was a conjecture. Yet the purpose was the same: to save the phenomena. In contrast, Thomson’s vortex theory of matter required stability for the motions he imagined in
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the primitive perfect liquid of the world. This theory, which began in 1867, is discussed in the third section of this paper. Thomson could only prove the stability of motions simpler than those he needed. For many years, he contented himself with an analogy with the observed stability of smoke rings. At last, in the late 1880s, he became convinced that vortex rings were unstable.

Owing to their different interests, Stokes and Thomson had opposite biases about hydrodynamic (in)stability. This is illustrated in the fourth section of this paper, through an account of their long, witty exchange on the possibility of discontinuity surfaces (infinitely thin layers of infinite shear) in a perfect liquid. From his first paper (1842) to his last letter to Thomson (1901), Stokes argued that the formation of surfaces of discontinuity provided a basic mechanism of instability for the flow of a perfect liquid past a solid obstacle. Thomson repeatedly countered that such a process would violate fundamental hydrodynamic theorems and that viscosity played an essential role in Stokes' instabilities. The two protagonists never came to an agreement, even though they shared many cultural values within and without physics.

The fifth section of this paper deals with the (in)stability of parallel flow. The most definite nineteenth-century result on this topic was Lord Rayleigh’s criterion of 1880 for the stability of two-dimensional parallel motion in a perfect liquid. The context was John Tyndall’s amusing experiments on the sound-triggered instability of smoke jets. However, the strongest motivation for theoretical inquiries in parallel-flow stability was Osborne Reynolds’ precise experimental account (1883) of the transition between laminar and turbulent flow in the case of circular pipes. In 1887 Cambridge authorities, including Stokes and Rayleigh, made the theory of this transition the topic of the Adams prize for 1889. This prompted Thomson to publish proofs of instability for two cases of parallel, two-dimensional viscous flow. Rayleigh soon challenged these proofs. William Orr proved their incompleteness in 1907.

In sum, the nineteenth-century concern with hydrodynamic stability led to well-defined, clearly stated questions on the stability of the solutions of the fundamental hydrodynamic equations (Euler and Navier-Stokes). Most answers to these questions were tentative, controversial, or plainly wrong. The subject that Rayleigh judged “second to none in scientific
as well as practical interest” [RSP 2, p. 344] remained utterly confused. Besides the Helmholtz-Kelvin instability and Rayleigh’s inflection theorem, the theoretical yield was rather modest: there was Stokes’ vague, unproved instability of divergent flows, Thomson’s unproved instability of vortex rings, the hanging question of the formation of discontinuity surfaces, and two illusory proofs of stability for simple cases of parallel viscous flow.

The situation could be compared to number theory, which is reputed for the contrast between the simple statements of some of its problems and the enormous difficulty of their solution. The parallel becomes even stronger if we note that some nineteenth-century questions on hydrodynamic stability, for example the stability of viscous flow in circular pipes or the stability of viscous flow past obstacles are yet to be answered, and that the few available answers to such questions were obtained at the price of considerable mathematical efforts. This long persistence of basic questions of fluid mechanics is the more striking because in physics questions tend to change faster than their answers.

In number theory, failed demonstrations of famous conjectures sometimes brought forth novel styles of reasoning, interesting side-problems, and even new branches of mathematics. Something similar happened in the history of hydrodynamic stability, though to a less spectacular extent. Stokes’ and Helmholtz’s surfaces of discontinuity were used to solve the old problem of the vena contracta and to determine the shape of liquid jets [Kirchhoff 1869], [Rayleigh 1876]. They also permitted Rayleigh’s solution (1876) of d’Alembert’s paradox, and inspired some aspects of Ludwig Prandtl’s boundary-layer theory (1904). Rayleigh’s formulation of the stability problem in terms of the real or imaginary character of the frequency of characteristic perturbation modes is the origin of the modern method of normal modes [Drazin and Reid 1981, pp. 10–11].

As a last important example of fruitful groping, Stokes, Thomson, and Rayleigh all emphasized that the zero-viscosity limit of viscous-fluid behavior could be singular. Stokes regarded this singularity as a symptom of instability of inviscid, divergent flows; Thomson as an indication that the formation of unstable states of parallel motion required finite viscosity; Rayleigh as a clue to why some states of parallel motion were stable for zero viscosity and unstable for small, finite viscosity. Rayleigh [1892,
p. 577] even anticipated the modern concept of boundary-layer instability:

“But the impression upon my mind is that the motions calculated above for an absolutely inviscid liquid may be found inapplicable to a viscid liquid of vanishing viscosity, and that a more complete treatment might even yet indicate instability, perhaps of a local character, in the immediate neighbourhood of the walls, when the viscosity is very small.”

In the absence of mathematical proof, the value of such utterances may be questioned. Rayleigh himself [1892, p. 576] warned that “speculations on such a subject in advance of definite arguments are not worth much.” Many years later, Garrett Birkhoff [1950] reflected that speculations were especially fragile on systems like fluids that have infinitely many degrees of freedom. Yet by imagining odd, singular behaviors, the pioneers of hydrodynamics instability avoided the temptation to discard the foundation of the field, the Navier-Stokes equation; and they sometimes indicated fertile directions of research.

In sum, early struggles with hydrodynamic stability are apt to inform the later history of this topic. They also reveal fine stylistic differences among some of the leading physicists of the nineteenth century. In the lack of rigorous mathematical reasoning, these physicists had to rely on subtle, personal combinations of intuition, past experience or experiment, and improvised mathematics. They ascribed different roles to idealizations such as inviscidity, rigid walls, or infinitely sharp edges. For instance, Helmholtz and Stokes believed that the perfect liquid provided a correct intuition of low-viscosity liquid behavior, if only discontinuity surfaces were admitted. Thomson denied that, and reserved the perfect liquid (without discontinuity) for his sub-dynamics of the universe. As the means lacked to exclude rigorously one of these two views, the protagonists preserved their colorful identities.

In the following, vector notation is used anachronistically for the sake of concision. Following Thomson’s convention, by perfect liquid is meant an incompressible, inviscid fluid. In order that the present paper may be read independently, some sections of earlier papers of mine (on Helmholtz’s surfaces of discontinuity and on Reynolds’ study of pipe flow) are reproduced in abbreviated form.
1. DIVERGENT FLOWS

Stokes’ first paper, published in 1842, contains pioneering considerations of hydrodynamic stability. George Gabriel Stokes was a Cambridge-trained mathematician, First Wrangler and Smith-prize winner in 1841. In the two decades preceding his student years, British mathematical physics had undergone deep reforms that eliminated archaic Newtonian methods in favor of the newer French ones. While Fourier’s theory of heat and Fresnel’s theory of light were most admired for their daring novelty, the hydrodynamics of Euler and Lagrange was believed to provide the simplest illustration of the required mathematics of partial differential equations. The Cambridge coach William Hopkins made it a basic part of the Tripos examination, and persuaded Stokes to make it his first research topic.¹

In his first paper, Stokes [1842] studied the two-dimensional and the cylindrically-symmetrical steady motions of an incompressible, inviscid fluid obeying Euler’s equation

\[ \rho \left[ \frac{\partial \mathbf{v}}{\partial t} + (\mathbf{v} \cdot \nabla)\mathbf{v} \right] = \mathbf{f} - \nabla P\]

where \(\rho\) is the density of the fluid, \(\mathbf{v}\) its velocity, \(P\) its pressure, and \(\mathbf{f}\) an external force density (most often \(\rho g\), where \(g\) is the acceleration of gravity). From an analytical point of view, most of Stokes’ results could already be found in Lagrange or J.M.C. Duhamel. His discussion of their physical significance was nonetheless penetrating and innovative. A keen observer of nature, a first-rate swimmer, and a naturally gifted experimenter, he was struck by the departure between computed and real flows. In order to explain this discrepancy, he suggested that the possibility of a given motion did not imply its necessity: there could be other motions compatible with the same boundary conditions, some of which could be stable and some others unstable. “There may even be no stable steady mode of motion possible, in which case the fluid would continue perpetually eddying” [Stokes 1842, pp. 10–11].

As a first example of instability, Stokes cited the two-dimensional flow between two similar hyperbolas. An experiment of his own showed that the

theoretical hyperbolic flow only held in the narrowing case. He compared this result with the fact that a fluid passing through a hole from a higher pressure vessel to a lower pressure vessel forms a jet instead of streaming along the walls as the most obvious analytical solution would have it (Fig. 1). Although Mariotte, Bernoulli (Daniel), and Borda already knew such effects, Stokes was the first to relate them to a fundamental instability of fluid motion and to enunciate a general tendency of a fluid “to keep a canal of its own instead of spreading out” [Stokes 1842, p. 11].

In the case represented in Fig. 1, Stokes argued that according to Bernoulli’s theorem\footnote{According to this theorem, the sum $P + \frac{1}{2}\rho v^2 + \rho g z$ is constant along a line of flow if the motion is steady ($z$ denotes the height, and $g$ the acceleration of gravity).} the velocity of the fluid coming from the first vessel was completely determined by the pressure difference between the two vessels. This velocity was therefore homogenous, and the moving fluid had to form a cylindrical jet in order to comply with flux conservation. Dubious as it may be (for it presupposes a uniform pressure in the second vessel), this reasoning documents Stokes’ early conviction that nature sometimes preferred solutions of Euler’s equation that involved surfaces of discontinuity for the tangential component of velocity.

This conviction reappears in a mathematical paper that Stokes published four years later [Stokes 1846b, pp. 305–313]. There he considered the motion of an incompressible fluid enclosed in a rotating cylindrical container, a sector of which has been removed (Fig. 2). For an acute sector, the computed velocity is infinite on the axis of the cylinder. Stokes judged that in this case the fluid particles running toward the axis along one side of the sector would “take off” to form a surface of discontinuity. For the
rest of his life Stokes remained convinced of the importance of such surfaces for perfect-fluid motion. Yet he never offered a mathematical theory of their development.

Soon after publishing his first paper, Stokes became interested in a more concrete problem of fluid motion: the effect of the ambient air on the oscillations of a pendulum. The head of the British project of a pendulum determination of the shape of the earth, Captain Sabine, had suggested that the viscosity of the air could be relevant. The remark prompted Stokes’ interest in “imperfect fluids.” His first strategy, implemented in a memoir of 1843, was to study special cases of perfect fluid motion in order to appreciate how imperfect fluidity affected results.\(^3\)

Among his cases of motion, Stokes included oscillating spheres and cylinders that could represent pendulum parts. The most evident contradiction with observation was the absence of damping of the oscillations by the perfect fluid in which the bodies were immersed. Stokes considered three possible explanations of the observed resistance. First, he imagined that the fluid particles along the surface of the sphere would come off tangentially at some point, forming a surface of discontinuity. Second, he mentioned Poisson’s inclusion of a surface friction term, but only to criticize his neglect of the necessary reaction on the fluid’s motion. Third, he evoked instability as the most likely cause [Stokes 1843, pp. 53–54]:

“It appears to me very probable that the spreading out motion of the fluid, which is supposed to take place behind the middle of the sphere or cylinder, though dynamically possible, nay, the only motion dynamically possible when the conditions which have been supposed are accurately

\(^3\) Cf. [Darrigol 2002a].
satisfied, is unstable; so that the slightest cause produces a disturbance in the fluid, which accumulates as the solid moves on, till the motion is quite changed. Common observation seems to show that, when a solid moves rapidly through a fluid at some distance below the surface, it leaves behind it a succession of eddies in the fluid."

Stokes went on to ascribe fluid resistance to the *vis viva* of the tail of eddies, as Jean-Victor Poncelet and Adhémar Barré de Saint-Venant had already done in France. To make this more concrete, he recalled that a ship had least resistance when she left the least wake.

In the following years, Stokes realized that none of these supposed mechanisms of fluid resistance applied to the pendulum case. The true cause of damping was the air’s internal friction, as expressed in an equation discovered by Claude-Louis Navier in 1822 and re-derived by Stokes in 1845:

\[
\rho \left[ \frac{\partial \mathbf{v}}{\partial t} + (\mathbf{v} \cdot \nabla)\mathbf{v} \right] = \mathbf{f} - \nabla P + \mu \Delta \mathbf{v}
\]

where \(\mu\) is the viscosity parameter and \(\mathbf{f}\) the external force density. In his pendulum memoir of 1850, Stokes solved this equation for the oscillating sphere and cylinder that represented the bulb and the thread of the pendulum. The excellent agreement with experiments left no doubt on the correctness and stability of Stokes’ solutions.\(^4\)

For the sake of completeness, Stokes examined the case of a uniform translation of the sphere and cylinder, which corresponds to the zero-frequency limit of the pendulum problem. He assumed the motion to be so slow that the quadratic term \((\mathbf{v} \cdot \nabla)\mathbf{v}\) could be neglected in the Navier-Stokes equation. Stokes thus derived the resistance law that bears his name in the sphere case, but encountered a paradox in the cylinder case. In the cylinder case, the resulting equation does not have a steady solution (in a reference system bound to the cylinder) that satisfies the boundary conditions. Stokes [1850, p. 65] explained:

“...The pressure of the cylinder on the fluid continually tends to increase the quantity of fluid which it carries with it, while the friction of the fluid at a distance from the sphere continually tends to diminish it. In the case of the sphere, these two causes eventually counteract each other, and

\(^4\) Cf. [Darrigol 2002a].
the motion becomes uniform. But in the case of a cylinder, the increase in the quantity of fluid carried continually gains on the decrease due to the friction of the surrounding fluid, and the quantity carried increases indefinitely as the cylinder moves on.”

Therein Stokes perceived a symptom of instability [Stokes 1850, pp. 65–66]: “When the quantity of fluid carried with the cylinder becomes considerable compared with the quantity displaced, it would seem that the motion must become unstable, in the sense in which the motion of a sphere rolling down the highest generating line of an inclined cylinder may be said to be unstable.” If the cylinder moved long enough in the same direction (as would be the case for the thread of a very slow pendulum) “the quantity of fluid carried by the wire would be diminished, portions being continually left behind and forming eddies” [ibid., p. 67]. Stokes also mentioned that in such an extreme case the quadratic term of the Navier-Stokes equation might no longer be negligible. According to a much later study by Wilhelm Oseen, this is the true key to the cylinder paradox.  

Air and water were not the only imperfect fluid that Stokes had in mind. In 1846–1848 he discussed the motion of the ether in reference to the aberration of stars. In his view the ether behaved as a fluid for sufficiently slow motions, since the earth and celestial bodies were able to move through it without appreciable resistance. But its fluidity could only be imperfect, since it behaved as a solid for the very rapid vibrations implied in the propagation of light. Stokes [1846a, 1848] explained the aberration of stars by combining these two properties in the following manner:  

He first showed that the propagation of light remained rectilinear in a moving medium the velocity of which derived from a potential. Hence any motion of the ether that met this condition would be compatible with the observed aberration law. Stokes then evoked a theorem by Lagrange, according to which the motion of a perfect liquid always meets this condition when it results from the motion of immersed solid bodies (starting from rest). For a nearly spherical body like the earth, Stokes believed the Lagrangian motion to be unstable (for it implied a diverging flow in the rear of the body). But Stokes’ ether was an imperfect fluid, with

5 Cf. [Lamb 1932, pp. 609–617].
6 Cf., e.g., [Wilson 1987, pp. 132–145].
tangential stresses that quickly dissipated any departure from gradient-flow: “Any nascent irregularity of motion, any nascent deviation from the motion for which \( v \cdot d \mathbf{r} \) is an exact differential, is carried off into space, with the velocity of light, by transversal vibrations” [Stokes 1848, p. 9].

In the course of this discussion, Stokes noted that his solution of the (linearized) Navier-Stokes equation in the case of the uniformly moving sphere did not depend on the value of the viscosity parameter and yet did not meet the gradient condition. Hence an arbitrarily small viscous stress was sufficient to invalidate the gradient solution. Stokes regarded this peculiar behavior as a further symptom of instability of the gradient flow.

To sum up, in the 1840s Stokes evoked instability as a way to conciliate the solutions of Euler’s equations with observed or desired properties of real fluids, including the ether. He regarded a divergence of the lines of flow (in the jet and sphere cases) and fluid inertia (in the cylinder case) as a destabilizing factor, and imperfect fluidity (viscosity or jelly-like behavior) as a stabilizing factor (explicitly in the ether case, and implicitly in the pendulum bulb case). His intuition of unstable behavior derived from common observation of real flows and from the implicit assumption that ideal flow behavior should be the limit of real fluid behavior for vanishing viscosity.

Stokes did not attempt a mathematical investigation of the stability of flow. He did offer a few formal arguments, which today’s physicist would judge fallacious. His deduction of jet formation was based on an unwarranted assumption of uniform pressure in the receiving vessel. The steady flow around a cylinder, which he believed to be impossible, is in fact possible when the quadratic terms in the Navier-Stokes equation are no longer neglected. The argument based on the zero-viscosity limit of the flow around a sphere fails for a similar reason. Stokes’ contemporaries did not formulate such criticisms. They rather noted his less speculative achievements: new solutions of the hydrodynamic equations that bore on the pendulum problem, and rigorous, elegant proofs of important hydrodynamic theorems.

2. DISCONTINUOUS FLOW

In the late 1850s, as part of his project on the perception of sound, Hermann Helmholtz worked on the theory of organ pipes. Through
pioneering methods of scattering or diffraction theory, he derived the fundamental frequency of an open pipe without abusive simplification of the motion around the opening. The resulting formula agreed well with experiment, except for narrow tubes. Helmholtz surmised that in the latter case the internal friction of the air played a role. Unaware of the Navier-Stokes equation, he first studied the motions of a perfect liquid that would result from internal friction (after it has been turned off). For these, no velocity potential needs to exist or, equivalently, the instantaneous rotation $\frac{1}{2} \nabla \times \mathbf{v}$ of a fluid element does not need to vanish.\(^7\)

From Euler’s equation for a perfect liquid, Helmholtz derived the evolution of the instantaneous rotation and gave it the following geometric interpretation. Call “vortex line” a line everywhere tangent to the axis of the instantaneous rotation. Call “vortex filament” a tubular volume made up of the vortex lines that cross a given surface element of the fluid. Helmholtz’s theorems of vortex motion then read [Helmholtz 1858, pp. 111–114]:

- A vortex line “follows” the motion of the fluid.
- For a vortex filament at a given instant, the product of the area of a normal section by the rotation velocity within it is a constant along the tube. This constant is called the intensity of the filament.
- The intensity of a vortex filament does not change in time.

In short, vortex filaments represent stable structures of a perfect liquid.

Helmholtz focused on the vortex structure and came to regard the fluid velocity as derived from this structure. The double rotation $\nabla \times \mathbf{v}$ (now called vorticity), Helmholtz noted, is to the velocity what the electric current density is to the magnetic field. Therefore, the velocity field corresponding to a given vortex structure is the same as the magnetic field of the corresponding current.\(^8\) Helmholtz astutely relied on this analogy to analyze simple vortex motions without any calculation [Helmholtz 1858, pp. 114–121].

The simplest example of vortex motion that Helmholtz could think of was a “vortex sheet” made of contiguous vortex lines [ibid., 1858, 1858, 1858].

\(^7\) Cf. [Darrigol 1998].

\(^8\) Helmholtz’s more rigorous statement takes into account the boundary conditions and the fact that the vorticity determines the flow only up to an irrotational component.
In this case the electromagnetic analogy gives tangential discontinuity of the fluid velocity across the sheet. Helmholtz thus realized that Euler’s equation admitted special discontinuous solutions. Stokes already knew this, but had not provided a proof. As Helmholtz noted, the possibility of such motions is almost obvious, for they can be obtained by bringing together in thought two masses of fluid moving with different parallel velocities (the contact of the two masses does not alter their motion, since a perfect fluid has no shear stress). No one had discussed such motions before Stokes and Helmholtz, presumably because the use of differential equations seemed to presume continuity.  

In Helmholtz’s paper on vortex motion, the discontinuity surfaces were hardly more than a mathematical curiosity. They later became a central concept of his fluid mechanics. The incentive was again the problem of organ pipes. Helmholtz wondered how the continuous stream of air from the organ’s bellows could produce an oscillatory motion in the pipe. From a mathematical point of view, the air’s motion is analogous to the flow of electricity in a homogenous conductor, in which case a steady source can only produce steady currents [Helmholtz 1868, pp. 146–147].

While reflecting on this analogy, Helmholtz realized that it failed whenever a fluid flows into a larger space through a sharply delimited opening: the fluid forms a jet, whereas electricity spreads in every direction. Helmholtz proposed that surfaces of discontinuity were formed at any sharp angle of the walls along which the fluid moved. Near such an angle, he reasoned, the (irrotational) electric kind of flow leads to enormous velocities for which the pressure must be negative according to Bernoulli’s theorem. Helmholtz [1868, pp. 148–149] believed that in such cases the fluid would be torn apart [zerrissen] to form discontinuity surfaces.

From their equivalence with vortex sheets, Helmholtz derived two important properties of the discontinuity surfaces: they follow the average motion of the fluid, and they are highly unstable. “Theory,” Helmholtz [1868, pp. 152–153] wrote, “allows us to recognize that wherever an irregularity is formed on the surface of an otherwise stationary jet, this

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9 From a modern point of view, the theory of distributions allows the application of differential operators to discontinuous functions. Stokes and Helmholtz rather conceived their discontinuous distributions as limiting cases of continuous distributions.

10 Negative pressure, or tension, is in fact possible as a metastable condition of an adequately prepared fluid: cf. [Reynolds 1878] and earlier references therein.
irregularity must lead to a progressive spiral unrolling of the corresponding portion of the surface, which portion, moreover, slides along the jet.” Helmholtz never explained how this peculiar instability resulted from theory. As can be judged from manuscript sources, he probably used qualitative reasoning of the following kind.\textsuperscript{11}

Consider a plane surface of discontinuity, with fluid running in opposite directions on each side. The velocity field is completely determined by the corresponding vortex sheet. Let a small irrotational velocity perturbation

\textsuperscript{11} The behavior of discontinuity surfaces under small perturbations is a difficult problem, which is still the object of mathematical research, \textit{e.g.} [Caflisk 1990]. Helmholtz’s relevant manuscripts are “\textit{Stabilität einer circulierenden Trennungsfläche auf der Kugel}” [HN, item 681]; “\textit{Wirbelwellen}” [HN, item 684], and calculations regarding a vortex sheet in the shape of a logarithmic spiral [HN, item 680].
cause a protrusion of the surface. For definiteness, the surface is taken to be horizontal, and its protrusion is directed upwards. We further assume the perturbation to be such that the distribution of vorticity on this surface is approximately uniform. Then the curvature of the vortex sheet implies a drift of vorticity along it, at a rate proportional to the algebraic value of the curvature. Indeed, at a given point of the vortex sheet the velocity induced by the neighboring vortex filaments is the sum of the velocities induced by symmetric pairs of neighboring filaments, and each pair contributes a tangential velocity as shown on Fig. 3a. Consequently, vorticity grows around the inflection point on the right side of the protrusion, and diminishes around the inflection point on the left side of the protrusion (Fig. 3b). The excess of vorticity of the right slope induces a clockwise, rotating motion of the tip of the protrusion (Fig. 3c). The upwards component of this motion implies instability. The rightwards component initiates the spiraling motion observed in actual experiments (Fig. 3d–3e).\textsuperscript{12}

Helmholtz thus interpreted the degradation of smoke jets into chaotic whirls, and the astonishing sensitivity of such jets to sound that his British friend John Tyndall [1868, pp. 152–153] had demonstrated. He also used the instability to explain how the oscillations of an organ pipe were maintained by the air stream from the bellows. The mouth of the pipe produces an air blade that would hit the lip if no perturbation occurred (Fig. 4). Now suppose that the air column in the tube is already oscillating. Owing to this motion, air streams back and forth perpendicularly to the blade and alternatively forces it in and out the tube (since the corresponding vortex sheets must follow the motion of the air). Being unstable, the blade dissolves spirally into the surrounding air and thus reinforces its motion synchronously [Helmholtz 1877, pp. 154–157, 629–631].

Much later, in 1888, Helmholtz used the formation and instability of vortex sheets to elucidate a paradox of atmospheric motion. The then standard theory of trade winds, based on the combined effect of thermal convection and the earth’s rotation, gave an absurdly high velocity of the upper trade winds (high altitude winds blowing from the equator). In 1886, Helmholtz chanced to observe the spiral unrolling of a vortex sheet in the Swiss sky. He inferred that surfaces of discontinuity could be

\textsuperscript{12} A more precise argument of the same kind is in [Batchelor 1967, pp. 511–517].
formed in the atmospheric air. By complex reasoning, he argued that a surface of discontinuity indeed was formed below the upper trade winds.
The spiral unrolling of this surface provided the desired mechanism to check these winds [Helmholtz 1888, p. 308]:

“The principal obstacle to the circulation of our atmosphere, which prevents the development of far more violent winds than are actually experienced, is to be found not so much in the friction on the earth’s surface as in the mixing of differently moving strata of air by means of whirls that originate in the unrolling of surfaces of discontinuity. In the interior of such whirls the originally separate strata of air are wound in continually more numerous, and therefore thinner layers spiraling about each other; the enormously extended surfaces of contact allow a more rapid exchange of temperature and the equalization of their movement by friction.”

The atmospheric air strata implied in Helmholtz’s reasoning had different densities. They were therefore analogous to wind blowing over water. Helmholtz [1889, pp. 316–322] used this analogy, with proper re-scaling, to relate the unknown properties of atmospheric waves to the observed properties of sea waves. He was thus led to discuss the behavior of a plane water surface submitted to a uniform wind [Helmholtz 1889, 1890].

His friend William Thomson had already made the first steps on this difficult ground.

Besides mathematics and physics, William Thomson enjoyed sailing on his personal yacht, the Lalla Rookh. While cruising slowly and fishing with a line, he observed very short waves or “ripples” directly in front of the line, and much longer waves following steadily in its wake [Thomson 1871b]. The two sets of waves advanced at the same rate as the line, and therefore had the same propagation velocity. By simple hydrodynamic reasoning, Thomson [1871b] showed that the combined action of gravity and capillarity implied the bivaluedness of the length of periodic surface waves, as well as the existence of a minimum velocity. (The propagation velocity $v$ and the wave number $k$ are related by $kT + g/k = v^2$, where $T$ is the superficial tension and $g$ the acceleration of gravity). On Thomson’s yacht, Helmholtz helped his friend measure the minimum velocity [Thomson 1871c, p. 88].

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13 Helmholtz’s main purpose was to determine the height and wave-length of the waves produced by a wind of given velocity. Cf. [Darrigol 1998, pp. 47–51].

Thomson also took into account the effect of wind over the water surface, and showed that the waves grew indefinitely when the wind velocity exceeded a certain, small limit that vanishes with the surface tension. In other words, the plane water-surface is unstable for such velocities. The calculation goes as follows [Thomson 1871a].

A solution of Euler’s equation (1) is sought for which the separating surface takes the plane monochromatic wave form

\[ y = \eta(x) = ae^{i(kx - \omega t)} \]

the \( x \)-axis being in the plane of the undisturbed water surface, and the \( y \)-axis being normal to this plane and directed upwards. Neglecting the compressibility of the two fluids, and assuming irrotational motion, their motions have harmonic velocity potentials \( \phi \) and \( \phi' \). Thomson guessed the form

\[ \phi = C e^{ky + i(kx - \omega t)} \]

for the water, and

\[ \phi' = -vx + C' e^{ky + i(kx - \omega t)} \]

for the air, wherein \( v \) is the wind velocity.

A first boundary condition at the separating surface is that a particle of water originally belonging to this surface must retain this property, or, differentiating Equation (3) with respect to time,

\[ \frac{\partial \phi}{\partial y} = \frac{\partial \phi}{\partial x} \frac{\partial \eta}{\partial x} + \frac{\partial \eta}{\partial t}. \]

A similar condition must hold for the air. The third and last boundary condition is the relation between pressure difference, surface tension, and curvature. For simplicity, capillarity is neglected in the following so that

15 W. Thomson, “The influence of wind and capillarity on waves in water supposed frictionless,” and “Ripples and waves,” [TMPP, vol. 3, pp. 76–92, on pp. 79–80 (fishing-line), p. 88 (Helmholtz’s assistance)], p. 79 (instability of plane surface). Some commentators, including [Lamb 1932, p. 449], have Thomson say that the plane surface is stable for lower velocities, which leads to an absurdly high threshold for the production of waves (about twelve nautical miles per hour). Thomson did not and could not state so much, since he only considered irrotational perturbations of perfect fluids.
the pressure difference vanishes. The water pressure $P$ is related to the velocity potential $\phi$ by the equation

$$P + \frac{1}{2} \rho (\nabla \phi)^2 + \rho g y = \rho \frac{\partial \phi}{\partial t}$$

(7)

obtained by spatial integration of Euler’s equation. A similar relation holds for the air.

Substituting the harmonic expressions of $\phi$, $\phi'$ and $\eta$ in the boundary conditions and retaining only first-order terms (with respect to $a$, $C$, and $C'$) leads to the relations

$$Ck = ia\omega, \quad C'k = -ia(\omega - kv)$$

(8)

and

$$\rho(ga + iC\omega) = \rho'[ga + iC'(\omega - kv)].$$

(9)

Eliminating $a$, $C$, and $C'$ gives

$$\rho \omega^2 + \rho'(\omega - kv)^2 = gk(\rho - \rho').$$

(10)

The discriminant of this quadratic equation in $\omega$ is negative if

$$v^2 > gk\frac{\rho^2 - \rho'^2}{\rho \rho'}. $$

(11)

Hence there are exponentially diverging perturbations of the separation surface for any value of the velocity: the water surface is unstable under any wind, no matter how small.\(^{16}\)

This conclusion only holds when capillarity is neglected. As Thomson [1871a, p. 79] showed, the surface tension implies a wind-velocity threshold for the exponential growth of short-wave, irrotational perturbations. Thomson did not discuss the limiting case of equal densities for the

\(^{16}\) Note that to every growing mode corresponds a decaying mode by taking the complex conjugate solution of Equation (10). This seems incompatible with the growth derived in the above-given vortex-sheet consideration. In fact it is not, because Thomson’s harmonic perturbations imply an initially heterogenous distribution of vorticity on the separating surface, whereas the vortex-sheet argument assumes an initially homogenous distribution (to first order). For Thomson’s decaying modes, the initial distribution has an excess of vorticity on the left slope of every positive arch of the sine-shaped surface, and a defect on the right slope.
two fluids. This limit could not mean much to him: as will be seen in a moment, he did not believe in the possibility of discontinuity surfaces in homogenous fluids.\footnote{See Helmholtz to Thomson, 3 September 1868, quoted in \cite{Thompson1910, p. 527}.} Rayleigh \cite[pp. 365–371]{Rayleigh1879}, who had no such inhibition, considered the limit and derived the existence of exponentially growing perturbations at any wave length. He thus showed the similarity of the instabilities discovered by Helmholtz and Thomson. The modern phrase “Kelvin-Helmholtz instability” captures the same connection, with an unfortunate permutation of the names of the two founders.

3. VORTEX ATOMS

Even though Thomson observed and measured waves while sailing and fishing, his main interest in hydrodynamics derived from his belief that the ultimate substance of the world was a perfect liquid. His earliest use of hydrodynamics, in the 1840s, was merely analogical: he developed formal analogies between electrostatics, magnetostatics, and the steady motion of a perfect liquid, mainly for the purpose of transferring theorems. His correspondence of this period contains letters to Stokes in which he enquired for the hydrodynamic results he needed. Conversely, he offered new hydrodynamic theorems that his development of the energetic aspects of electricity suggested.\footnote{See the letters of the period March–October 1847, in \cite{Wilson1990}. Cf. \cite[pp. 219–227, 263–275]{SmithWise1989}, \cite[chap. 3]{Darrigol2000}.}

One of these theorems \cite{Thomson1849} is worth mention, for it played an important role in Thomson’s later discussions of kinetic stability. Consider a perfect liquid limited by a closed surface that moves from rest in a prescribed manner. If the equation of this surface is $F(r, t) = 0$, the boundary condition for the fluid motion reads

$$v \cdot \nabla F + \frac{\partial F}{\partial t} = 0.$$  

According to a theorem by Lagrange, the motion $v$ taken by the fluid derives from a potential $\phi$. Now consider any other motion $v'$ that meets the boundary condition at a given instant. The kinetic energy for the latter motion differs from the former by

$$T' - T = \int \frac{1}{2} \rho (v - v')^2 d\tau + \int \rho v \cdot (v' - v) d\tau.$$
Partial integration of the second term gives

\[(14) \quad - \int \rho \nabla \phi \cdot (v' - v) \, d\tau = \int \rho \phi \nabla \cdot (v' - v) \, d\tau - \int \rho \phi (v' - v) \cdot dS.\]

Therein the volume integral vanishes because the fluid is incompressible. The surface integral vanishes because the surface element \(dS\) is parallel to \(\nabla F\) and both motions meet condition (12). Consequently, \(T' - T\) is always positive: The energy of the motion that the fluid takes at a given time owing to the motion impressed on its boundary is less than the energy of any motion that meets the boundary condition at the same time.\(^1\)

At that time Thomson did not speculate on a hydrodynamic nature of electricity or magnetism. His attitude changed around 1850, after he adopted the kinetic conception of heat. In this conception, the elasticity of a gas results from hidden internal motion: an apparently potential form of energy turns out to be kinetic. Thomson and other British physicists speculated that every energy might be of kinetic origin.\(^2\)

The kind of molecular motion that William Rankine and Thomson then contemplated was a whirling, fluid motion around contiguous molecules. Gas pressure resulted from the centrifugal force of molecular vortices. Thomson elaborated this picture to account for the rotation of the polarization of light when traveling through magnetized matter, for electromagnetic induction, and even for the rigidity of the optical ether. He imagined an ether made of “rotating motes” in a perfect liquid. The gyrostatic inertia of the whirls induced by these motes was supposed to provide the needed rigidity.\(^3\)

In 1857 Thomson confided these thoughts to his friend Stokes, with an enthusiastic plea for a hydrodynamic view of nature [ST, 20 Dec. 1857]:

“I have changed my mind greatly since my freshman’s years when I thought it so much more satisfying to have to do with electricity than with hydrodynamics, which only first seemed at all attractive when I learned

\(^1\) Thomson stated two corollaries (already known to Cauchy): 1) The existence of a potential and the boundary condition completely determine the flow at a given instant; 2) the motion at any given time is independent of the motion at earlier times. See also [Thomson and Tait 1867, §312, pp. 317–319].

\(^2\) Cf. [Smith and Wise 1989, chap. 12].

\(^3\) Cf. [Smith and Wise 1989, pp. 402–412], [Knudsen 1971].
how you had fulfilled such solutions as Fourier’s by your boxes of water.  

Now I think hydrodynamics is to be the root of all physical science, and is at present second to none in the beauty of its mathematics.”

A year after this pronouncement, Helmholtz published his memoir on vortex motion. In early 1867, Thomson saw the “magnificent way” in which his friend Peter Guthrie Tait could produce and manipulate smoke rings. He became convinced that Helmholtz’s theorems offered a fantastic opportunity for a theory of matter based on the perfect liquid [Thomson 1867]. Instead of rotating motes, he now considered vortex rings, and assimilated the molecules of matter with combinations of such rings. The permanence of matter then resulted from the conservation of vorticity. The chemical identity of atoms became a topology of mutually embracing or self-knotted rings. Molecular collisions appeared to be a purely kinetic effect resulting from the mutual convection of two vortices by their velocity fields. In a long, highly mathematical memoir, Thomson [1868] developed the energy and momentum aspects of the vortex motions required by this new theory of matter.

The most basic property of matter being stability, Thomson naturally faced the stability of vortex rings. Helmholtz’s theorems only implied the permanence of the individual vortex filament of which the rings were made. They did not exclude significant changes in the shape and arrangement of these filaments when submitted to external velocity perturbations. Thomson had no proof of such stability, except in the case of a columnar vortex, that is, a circular-cylindric vortex of uniform vorticity. He could show that a periodic deformation of the surface of the column propagated itself along and around the vortex with a constant amplitude [Thomson 1867, p. 4; 1880a]. An extrapolation of such behavior to thin vortex rings did not seem too adventurous to him. Moreover, Tait’s smoke-ring experiments indicated stability as long as viscous diffusion did not hide the ideal behavior.

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22 This is an allusion to Stokes’ calculation [Stokes 1843, pp. 60–68] of the inertial moments of boxes filled with perfect liquid and his subsequent experimental verification of the results by measuring the torsional oscillations of suspended boxes of this kind.

23 Thomson to Helmholtz, 22 Jan. 1867, quoted in [Thompson 1910, p. 513].


25 As John Hinch tells me, the relevance of the latter observation is questionable: the smoke rings may not indicate the actual distribution of vorticity, because the diffusivity
During the next ten years, Thomson had no decisive progress to report on his vortex theory of matter. The simplest, non-trivial problem he could imagine, that of a cylindrically symmetrical distribution of vorticity within a cylindrical container, proved to be quite difficult. In 1872–1873, he exchanged long letters with Stokes on this question, with no definite conclusion. Thomson’s arguments were complex, elliptic, and non-rigorous. As he admitted to Stokes, “This is an extremely difficult subject to write upon” [ST, 19 Dec. 1872]. A benevolent and perspicacious Stokes had trouble guessing what his friend was guessing at. I have fared no better.

A stimulus came in 1878 from Alfred Mayer’s experiments on floating magnets. The American professor had shown that certain symmetrical arrangements of the magnets were mechanically stable. Realizing that the theoretical stability criterion was similar to that of a system of vortex columns, Thomson [1878] exulted: “Mr. Mayer’s beautiful experiments bring us very near an experimental solution of a problem which has for years been before me unsolved – of vital importance in the theory of vortex atoms: to find the greatest number of bars which a vortex mouse-mill can have.” Thomson claimed to be able to prove the steadiness and stability of simple regular configurations, mathematically in the triangle and square cases, and experimentally in the pentagonal case.

These considerations only brought light on the stability of a mutual arrangement of vortices with respect to a disturbance of this arrangement, not on their individual stability. They may have prompted Thomson’s decision to complete his earlier, mostly unpublished considerations on the stability of cylindric vortices [ST, 1872–1873]. In harmony with the energy-based program developed in his and Tait’s Treatise on Natural Philosophy [1867], Thomson formulated an energetic criterion of stability. In problems of statics, stable equilibrium corresponds to a minimum of vorticity is much more efficient than that of smoke particles.

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27 The subject is further discussed by Alfred Greenhill in 1878, J.J. Thomson in 1883, William Hicks in 1882. Cf. [Love 1901, pp. 122–125].

28 Cf. [Smith and Wise 1989, chap. 11]. Thomson and Tait [1867, §346] wrote: “There is scarcely any question in dynamics more important for Natural Philosophy than the stability and instability of motion.”
the potential energy. In any theory that reduces statics to kinetics, there should be a similar criterion for the stability of motion. For the motion of a perfect liquid of unlimited extension, Thomson stated the following theorem: *If with vorticity and impulse given, the kinetic energy is stationary (a “minimax” in Thomson’s terms), the motion is steady. If it is a (local) minimum or maximum, the motion is not only steady but stable* [Thomson 1876; 1880b].

Some thinking is necessary to understand what Thomson had in mind, since he did not care to provide a proof. For simplicity, I only consider the case of a fluid confined in a rigid container with no particular symmetry. Then the condition of given impulse must be dropped, and “steadiness” has the ordinary meaning of constancy of the velocity field. The condition of constant vorticity, Thomson tells us, is the constancy of the number and intensity of the vortex filaments (it is not the steadiness of the vorticity field). A more rigorous definition would be that the distribution of vorticity at a given time can be obtained from the original distribution by pure convection.

The variation \( \delta v = \omega \times \delta r \), with \( \nabla \cdot \delta r = 0 \), of the fluid velocity meets this condition, since it has the same effect on the vorticity distribution \( \omega \) as does a displacement \( \delta r \) of the fluid particles. Therefore, the integral

\[
\delta T = \int \rho v \cdot (\omega \times \delta r) \, d\tau = \int \rho \delta r \cdot (v \times \omega) \, d\tau
\]

must vanish for any \( \delta r \) such that \( \nabla \cdot \delta r = 0 \). This implies that

\[
\nabla \times (v \times \omega) = 0.
\]

Combined with the vorticity equation (the curl of Euler’s equation)

\[
\frac{\partial \omega}{\partial t} + \nabla \times (v \times \omega) = 0,
\]

this gives the steadiness of the vorticity distribution. The fluid being incompressible, this steadiness implies the permanence of the velocity field, as was to be proved.

Thomson declared the other part of his theorem, the stability of the steady motion when the kinetic energy is a maximum or a minimum, to be “obvious” [Thomson 1876, p. 116]. Any motion that differs little from an
energy-extremum motion at a given time, Thomson presumably reasoned, would retain this property in the course of time, for its energy, being a constant, would remain close to the extremum value. Metaphorically, a hike at a constant elevation slightly below that of a summit cannot lead very far from the summit. Thomson did not worry that the proximity of two fluid motions was not as clearly defined as the proximity of two points of a mountain range.

At any rate, his energetic criterion helped little for determining the stability of vortex atoms. The energy of a vortex ring turned out to be a minimax, in which case the energy consideration does not suffice to decide stability [Thomson 1876, p. 124]. To sustain his claim of stability, Thomson only had smoke-ring experiments and the limited analogy of thin rings with columnar vortices. Presumably to prepare a later attack on this difficult problem, he dwelt on the simpler problem of cylindrically-symmetric motions within a tubular container. In this case, a simple consideration of symmetry shows that a uniform distribution of vorticity within a cylinder coaxial to the container corresponds to a maximum energy in the above sense. Similarly, a uniform distribution of vorticity in the space comprised between the walls and a coaxial cylinder has minimum energy. Those two distributions are therefore steady and stable [Thomson 1880b, p. 173].

Thomson had already studied the perturbations of the former distribution, the columnar vortex, in the absence of walls. He now included a reciprocal action between the vortex vibration and a “visco-elastic” wall [ST, 19 Dec. 1872], [Thomson 1880b, pp. 176–180]. He thus seems to have temporarily left the ideal world of his earlier reasoning to consider what would happen to a vortex in concrete hydrodynamic experiments for which the walls of the container necessarily dissipate part of the energy of the fluid motion.

Thomson [1880b, p. 177] described how, owing to the interaction with the visco-elastic walls, “the waves [of deformation of the surface of the vortex] of shorter length are indefinitely multiplied and exalted till their crests run out into fine laminas of liquid, and those of greater length

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29 For a given vorticity and a given impulse the energy of a thin vortex ring (with quasi-circular cross-section) is decreased by making its cross section oval; it is increased by making the ring thicker in one place than in another.
are abated.” The container thus became filled with a very fine, but heterogeneous mixture of rotational fluid with irrotational fluid, which Thomson called vortex sponge.\textsuperscript{30} At a later stage, the compression of the sponge led to the minimum energy distribution for which the irrotational fluid is confined in an annular space next to the wall. A few years later, George Francis FitzGerald and Thomson himself founded a reputed theory of the ether on the intermediate vortex-sponge state.\textsuperscript{31}

Some aspects of the dissipative evolution of a columnar vortex are relatively easy to understand. According to Helmholtz’s vortex theorems, the rotational and irrotational parts of the fluid (which have, respectively, the vorticity $\omega$ of the original vortex column and zero vorticity) behaves like two incompressible, non-miscible fluids. Since the original configuration is that of maximum energy, the dissipative interaction with the visco-elastic wall leads to a lesser-energy configuration for which portions of the rotational fluid move toward the walls. As the fluid is incompressible, this evolution implies a corrugation of the vortex surface. As Thomson proved in his study of columnar vortex vibrations, the corrugation rotates at a frequency that grows linearly with its inverse wave-length (and linearly with the vorticity $\omega$). The energy-damping effect of the walls being proportional to the frequency of their perturbation, the energy of the smaller corrugation waves diminishes faster. As for these special waves (unlike sea waves) smaller energy corresponds to higher amplitude, the shorter waves must grow until they reach the angular shape that implies frothing and mixing with the rotational fluid.\textsuperscript{32} On the latter point, Thomson probably reasoned by analogy with the finite-height sea-wave problem, which he had been discussing with Stokes.

Thomson naturally expected a similar degradation to occur for any vortex in presence of visco-elastic matter. “An imperfectly elastic solid,” he noted in 1872, “is slow but sure poison to a vortex. The minutest portion of such matter, would destroy all the atoms of any finite universe” \textit{[ST, p. 378]}. Yet Thomson did not regard this peculiar instability as a

\textsuperscript{30} In his correspondence of 1872, Thomson imagined a different process of “labyrin-thine”, “spiraling” penetration of the rotational fluid into their irrotational one.

\textsuperscript{31} Cf. [Hunt 1991, pp. 96–104]. FitzGerald first wrote on the vortex-sponge ether in 1885. Thomson first wrote on this topic in (1887c). Cf. [Darrigol 2002b].

\textsuperscript{32} According to Thomson [1880b, pp. 176–177], this process only occurs if the canister is set free to rotate (so that the angular momentum of the fluid is constant).
threat to his vortex-theory of matter. Visco-elastic walls did not exist at the scale of his ideal world fluid: every matter, including container walls, was made of vortices in this fluid [ST, p. 379]. Perhaps, Thomson admitted, the interactions of a dense crowd of vortices resembled the visco-elastic degradation, but only to the extent needed to explain the condensation of a gas on the walls of its container.

For a few more years, Thomson satisfied himself with the observed stability of smoke rings and with the demonstrated stability of the columnar vortex. By 1889, however, he encountered difficulties that ruined his hope of a vortex theory of matter. This is attested by a letter he wrote to the vortex-sponge enthusiast FitzGerald: “I have quite confirmed one thing I was going to write to you (in continuation with my letter of October 26), viz. that rotational vortex cores must be absolutely discarded, and we must have nothing but irrotational revolution around vacuous cores” [Thomson 1889, p. 202]. He adduced the following reason: “Steady motion, with crossing lines of vortex columns, is impossible with rotational cores, but is possible with vacuous cores and purely irrotational circulations around them.”

Crossing lines of vortex columns occurred in FitzGerald’s and Thomson’s vortex ether. They also were a limiting case of the mutually embracing vortex rings that Thomson contemplated in his theory of matter. Their unsteadiness was therefore doubly problematic. Thomson was altogether pessimistic: “I do not see much hope for chemistry and electromagnetism” [Thomson 1889, p. 204]. Although vacuous-core vortices with zero vorticity still remained possible, Thomson was much less eager to speculate on vortex atoms than he had earlier been. In subsequent letters, he tried to persuade FitzGerald to abandon the vortex ether.33

Considerations of stability also played a role in Thomson’s renunciation. Since 1867, his friend Stokes had been warning him about possible instabilities: “I confess,” Stokes wrote on 8 January 1873 [ST], “I am skeptical about the stability of many of the motions which you appear to contemplate.” In a letter to Stokes of 27 December 1898 [ST], Thomson described the frittering and diffusion of an annular vortex, with the comment:

“I now believe that this is the fate of vortex rings, and of every kind of

33 Cf. [Hunt 1991, p. 102].
irrotational [rotational?] motion (with or without finite slips anywhere) in a limited portion of an inviscid mass of fluid, which is at rest at great distances from the moving parts. This puts me in mind of a thirty-year-old letter of yours with a drawing in black and red ink suggesting instability of the motion of a columnar vortex, which I did not then believe. I must see if I can find the letter.”

According to Thomson’s own recollections [Thomson 1904, pp. 370n–371n], he became aware of the instability of vortex rings in unpublished work of 1887:

“It now seems to me certain that if any motion be given within a finite portion of an infinite incompressible liquid originally at rest, its fate is necessarily dissipation to infinite distances with infinitely small velocities everywhere; while the total kinetic energy remains constant. After many years of failure to prove that the motion in the ordinary Helmholtz circular ring is stable, I came to the conclusion that it is essentially unstable, and that its fate must be to become dissipated as now described. I came to this conclusion by extensions not hitherto published of the considerations described in a short paper entitled: “On the stability of steady and periodic fluid motion”, in the *Philosophical Magazine* for May 1887.”

In this little paper, Thomson [1887b] proved that the energy of any vortex motion of a fluid confined within deformable walls could be increased indefinitely by doing work on the walls in a systematic manner. More relevantly, he announced that the energy of the motion would come of itself to vanish if the walls were viscously elastic. It is not clear, however, why this result would have been more threatening to vortex atoms than the degradation of a vortex column surrounded by viscously elastic walls already was.

Another paper of the same year [Thomson 1887e] seems more relevant. Therein Thomson considered the symmetric arrangement of vortex rings represented on Fig. 5, as a possible model of a rigid ether. He worried [ibid., pp. 318, 320]:

“It is exceedingly doubtful, so far as I can judge after much anxious consideration from time to time during these last twenty years, whether the configuration represented [in Fig. 5] or any other symmetrical arrangement, is stable when the rigidity of the ideal partitions enclosing each ring
separately is annulled through space? The symmetric motion is unstable, and the rings shuffle themselves into perpetually varying relative positions, with *average homogeneity*, like the ultimate molecules of a homogeneous liquid."

This instability threatened not only the vortex theory of ether – on which Thomson [*ibid.*, p. 320] pronounced “the Scottish verdict of *not proven*” – but also any attempt at explaining chemical valence by symmetrical arrangements of vortex rings. After twenty years, Thomson’s anxiety was turning into disbelief.

### 4. THE THOMSON-STOKES DEBATE

When, in 1857, Thomson was contemplating an ether made of a perfect liquid and rotating motes, his friend Stokes warned him about the instability of the motion of a perfect liquid around a solid body.\(^{34}\) Thomson confidently replied [*ST*, 23 Dec. 1857]: “Instability, or a tendency to run to eddies, or kind of dissipation of energy, is impossible in a perfect fluid.” As he had learned from Stokes ten years earlier and as Cauchy had proved in 1827, the motion of solids through a perfect liquid originally at rest completely determines the fluid motion if solids and fluid are originally

\(^{34}\) This is inferred from the letter from Thomson to Stokes of 17 June 1857 [*ST*]: “I think the instability you speak of cannot exist in a perfect [...] liquid.”
at rest. Following Lagrange’s theorem, the latter motion is irrotational and devoid of eddying. Following Thomson’s theorem of 1849, it is the motion that has at every instant the minimum kinetic energy (“mike” in private) compatible with the boundary conditions. Thomson believed these two results to imply stability.\(^{35}\)

Stokes disagreed. He insisted: “I have always inclined to the belief that the motion of a perfect incompressible liquid, primitively at rest, about a solid which continually progressed, was unstable” \([ST, 12 \text{ Feb. 1858}]\). The theorems of Lagrange, Cauchy, and Poisson, he argued, only hold “on the assumption of continuity, and I have always been rather inclined to believe that surfaces of discontinuity would be formed in the fluid.” The formation of such surfaces would imply a loss of \textit{vis viva} in the wake of the solid and thus induce a finite resistance to its motion. A surface of discontinuity, he told Thomson \([ST, 13 \text{ Feb. 1858}]\), is surely formed when fluid passes from one vessel to another through a small opening (Fig. 1), which implies the instability of the irrotational, spreading out motion. Similarly, Stokes went on, the spreading out motion behind a moving sphere (Fig. 6) should be unstable. Stokes was only repeating the considerations he had used in 1842–1843 to conciliate perfect and real fluid behaviors.

In general, Stokes drew his ideas on the stability of perfect-liquid motion from the behavior of real fluids with small viscosity, typically water. In 1880, while preparing the first volume of his collected papers, he reflected on the nature of the zero-viscosity limit \([SMPP 1, \text{ pp. 311–312}]\). His remark of 1849 on the discontinuity surface from an edge, he then

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\(^{35}\) Presumably, Thomson believed that a slightly perturbed motion would remain close to the original motion because its energy would remain close to that of the minimum-energy solution. However, this is only true in a closed system for which there is no external energy input. As Stokes later argued, such an input may feed the perturbation.
noted, depended on the double idealization of strictly inviscid fluid and infinitely sharp edge:

“A perfect fluid is an ideal abstraction, representing something that does not exist in nature. All actual fluids are more or less viscous, and we arrive at the conception of a perfect fluid by starting with fluids such as we find them, and then in imagination making abstraction of the viscosity. Similarly, any edge we can mechanically form is more or less rounded off, but we have no difficulty in conceiving of an edge perfectly sharp.”

Stokes then considered the flow for a finite viscosity $\mu$ and a finite curvature radius $a$ of the edge, and argued that the limit of this flow when $a$ and $\mu$ reached zero depended on the order in which the two limits were taken. If the limit $\mu \to 0$ is taken first, the resulting flow is continuous and irrotational, and it obviously remains so in the limit $a \to 0$. If the limit $a \to 0$ is taken first, the resulting flow is that of a viscous fluid passing an infinitely sharp edge. The viscous stress is easily seen to imply the formation of a trail of vorticity from the edge. In the limit $\mu \to 0$ this trail becomes infinitely narrow, and a vortex sheet or discontinuity surface is formed. In Stokes’ view, the latter double limit was the only one of physical interest, because the result of the former was unstable in the following sense: an infinitely small viscous stress was sufficient to turn it into a widely different motion.$^{36}$

Stokes returned to his idea of the double limit in several letters.$^{37}$ In 1894 it led him to an instructive comment on the nature of his disagreement with Thomson: “Your speculations about vortex atoms led you to approach the limit in the first way [$\mu \to 0$ first]; my ideas, derived from what one sees in an actual fluid, led me to approach it in the other

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$^{36}$ As Thomson later pointed out, in this presumed instability there is an apparent contradiction between the vanishing work of the viscous stress and the finite energy difference between the two compared motions. Stokes replied with a metaphor $^{ST}$, 27 Oct. 1894: “Suppose there is a railway $AB$ which at $B$ branches off towards $C$ and towards $D$. Suppose a train travels without stopping along $AB$ and onwards. Will you admit that the muscular exertion of the pointsman at $B$ is the merest trifle of the work required to propel the train along $BC$ or $CD$? Now I look on viscosity in the neighbourhood of a sharp, though not absolutely sharp, edge as performing the part of the pointsman at $B$."

$^{37}$ $^{ST}$, 1 Nov. 1894, 22–23 Nov. 1898, 14 Feb. 1899]. In the latter letter, Stokes considers the state of things at time $t$ from the beginning of motion and distance $r$ from an edge, and argues that the limit $t \to 0$ gives the “mike” solution, whereas the limit $r \to 0$ gives a discontinuity surface.
way \([a \to 0\) first]\)” [ST, 1 Nov. 1894]. Indeed, Thomson’s reflections on stability mostly occurred in the context of his grand theory of ether and matter. He was therefore prejudiced in favor of stability, and generally expected important qualitative differences between real and perfect-fluid behavior.

In 1887, Thomson publicly rejected the possibility of surfaces of discontinuity, arguing that they could never be formed by any natural action. In his opinion continuity of velocity was always obtained when two portions of fluid where brought into contact. He now admitted, with Stokes and Helmholtz, that the flow around a solid obstacle was unstable when the velocity exceeded a certain value, but denied that this instability had anything to do with surfaces of discontinuity. For a perfect liquid, the determining effect was the separation of the fluid from the solid surface.

In the case of flow around a sphere, Thomson [1887a] described the instability as follows. The fluid separates at the equator when the asymptotic velocity of the fluid exceeded the value for which the pressure at the equator becomes negative \((\frac{5}{8} \rho V^2\) according to Bernoulli’s law applied to the irrotational solution of Euler’s equation in this case).\(^{38}\) A coreless vortex is formed as indicated on Thomson’s drawing (Fig. 7). This vortex grows until it separates from the sphere and follows the flow. The whole process repeats itself indefinitely in a “violently disturbed motion” [ibid., p. 149].

Stokes did not comment on this cavitational instability, which was known to occur on the edges of swiftly moving immersed solids, for instance ship propellers.\(^{39}\)

He did, however, contest Thomson’s assertion that discontinuity surfaces could not be formed by any natural process. In his view, a drop of perfect liquid falling on a calm surface of the same liquid led to such a surface. So did the “goring” of fluid on itself, as drawn on Fig. 8 [ST, 4, 7 Feb. 1887].

Thomson rejected these suggestions as well as Rayleigh’s idea of bringing into contact two parallel plane surfaces bounding two portions of liquid moving with different velocities [ST, 6, 9 Feb. 1887]. In every case, he argued, the contact between the two different fluid portions always begins

\(^{38}\) Regarding negative pressure, see note 10.

\(^{39}\) Cf. [Reynolds 1873]. In a real fluid, the cavities are filled with vapor.
at an isolated point, and the boundary of the fluid evolves so that no finite slip ever occurs. The drawings of Fig. 9 illustrate his understanding of the goring and rain drop cases. In Rayleigh's plane-contact process, the imperfect flatness of the surfaces does the trick.

Seven years later Thomson published another provocative article in *Nature* [Thomson 1894] against the “doctrine of discontinuity.” This time his target was the alleged formation of a surface of discontinuity past a sharp edge. The relief from infinitely negative pressure at the sharp edge, Thomson declared, never was the formation of a surface of discontinuity. This doctrine was inconsistent with his minimum-kinetic-energy theorem. The true compensatory factors were finite viscosity, finite compressibility, or yielding boundary of the fluid.\(^\text{40}\)

Thomson illustrated the compensations with the example of a thin moving disk. When the first factor dominates, a layer of abrupt velocity change, or, equivalently, a vortex sheet with small thickness, is formed behind the moving solid. When the third dominates, a succession of thin hollow rings is created behind the disk in a process similar to that which

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\(^{40}\) Thomson already expressed this opinion in a letter to Helmholtz of 3 Sept. 1868, quoted in [Thompson 1910, p. 527]: “Is it not possible that the real cause of the formation of a vortex-sheet may be viscosity which exists in every real liquid, and that the ideal case of a perfect liquid, perfect edge, and infinitely thin vortex sheet, may be looked upon as a limiting case of more and more perfect fluid, finer and finer edge of solid, and consequently thinner and thinner vortex-sheet?”
Thomson had described in 1887 for the moving sphere. Both processes imitate a surface of discontinuity when the fluid is nearly perfect. But the imitation is always imperfect.

The strict doctrine of discontinuity, Thomson went on, leads to an absurd theory of resistance. His target was Rayleigh’s “dead water” theory of resistance [Rayleigh 1876], according to which the fluid remains at rest (with respect to the solid) in the space limited by a tubular surface of discontinuity extending from the edges to infinity (see Fig. 10). The pressure in the dead water immediately behind the solid is inferior to the pressure on the front of the body, so that a finite resistance results. Whereas Rayleigh offered this picture as a solution to d’Alembert’s old paradox, Thomson denounced its gross incompatibility with experiment.
The dead water, if any, could not realistically extend indefinitely rearwards. Moreover, the resistance measured by George Dines for a rectangular blade under normal incidence was three times larger than that indicated by Rayleigh’s calculation in this case. The truncation of the discontinuity surface, Thomson showed, did not remove this discrepancy. As a last blow to the dead water theory, Thomson conceived a special case in which it gave zero resistance (see Fig. 11).

Stokes’ reaction was strong and immediate [ST, 11 Oct. 1894]. He had never supported the dead water theory, and believed instead that the main cause of resistance was the formation of eddies. But he maintained that the continuous, irrotational, and steady motion of a perfect liquid around a solid body with sharp edges was unstable. He agreed with Thomson that this motion was that of minimum energy under the given boundary conditions. “But what follows from that? There is the rub.” After this interjection, Stokes explained how instability was still possible:

“What is meant by the motion being unstable? I should say, the motion is said to be stable when whatever small deviation from the phi motion [the minimum-energy motion, for which there exists a velocity potential phi] is
Figure 11. Case of motion for which the dead-water theory gives zero resistance. The hatched tube $EA$ moves to the left through a perfect liquid, leaving a dead water wake in its rear cavity and within the cylindrical surface of discontinuity which begins at $LL$. The longitudinal resultant of pressure on the front part $E$ is very nearly equal to the pressure at infinity times the transverse section of the tube, because the cylindrical part of the tube is much larger than its curved front part. The same equality holds exactly at the rear of the tube, because the pressure is continuous across the discontinuity surface and constant within the dead water. Therefore, the net longitudinal pressure force on the tube vanishes. From [Thomson 1894, p. 228].

...supposed to be produced, and the fluid thenceforth not interfered with, the subsequent motion differs only by small quantities from the phi motion, and unstable when the small initial deviation goes on accumulating, so that presently it is no longer small. – I have a right to take for my small initial deviation one in which the fluid close to the edge shoots past the edge, forming a very minute surface of discontinuity. The question is, Will this always remain correspondingly minute, or will the deviation accumulate so that ultimately it is no longer small? I have practically satisfied myself that it will so accumulate, and the mode of subsequent motion presents interesting features."

Thomson replied that the would-be surface of discontinuity would “become instantly ruffled, and rolled up into an ανηριθµον γελασµα (by the last word I mean laughing at the doctrine of finite slip)” and would be washed away and left in the wake [$ST$, 14 Oct. 1894]. Stokes declared himself undisturbed by this objection [$ST$, 27 Oct. 1894]. He well knew the instability of discontinuity surfaces, but their spiral unrolling was

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O divine Sky, and swiftly-winging Breezes,
O River-springs, and multitudinous gleam
Of smiling Ocean - to thee, All-Mother Earth,
And to the Sun’s all-seeing orb I cry:
See what I suffer from the gods, a god!
not a priori incompatible with their continual formation at the edge of a body. “The rub” still was Thomson’s pretense to derive stability from his minimum-energy theorem. The theorem, Stokes explained, did not require that the actual motion should be that of minimum energy, because the additional energy needed to create the discontinuity surface could be provided by work of the external pressures that sustained the flow.42

Perhaps, Stokes conjectured, there was another “Kelvinian theorem” that truly excluded the discontinuity [ST, 27 Oct. 1894]. The only one that came out in later letters was the theorem that the angular momentum of every spherical portion of a liquid mass in motion, relatively to the center of the sphere, is always zero, if it is so at any one instant for every spherical portion of the same mass [ST, 23 Dec. 1898]. The theorem, Stokes judged, did no more exclude the formation of a surface of discontinuity than Lagrange’s and Cauchy’s theorems regarding fluid motion produced by moving immersed solids. Its proof required the continuity of the fluid motion near the walls [ST, 26 Dec. 1898].43

After a pause of four years, Stokes resumed the discussion with some considerations on the growth of a “baby surface of discontinuity” at a sharp edge [ST, 22–23 Nov. 1898]. Presumably, Thomson had objected that the continuity of pressure across the baby surface was incompatible with the discontinuity of velocity. Stokes explained that the growth of the surface and the resulting unsteadiness of the flow implied an additional term $\frac{\partial \phi}{\partial t}$ in the pressure equation (from Bernoulli’s theorem) that counterbalanced the discontinuity of $\frac{1}{2} \rho v^2$. He also repeated his conviction that Thomson’s minimum-energy theorem was not incompatible with the formation of discontinuity surfaces.

Thomson replied with a thought experiment [ST, 25 Nov. 1898] (see Fig. 12):

“To keep as closely as possible to the point (edge!) of your letter of the 22nd, let $E$ be an edge fixed to the interior of a cylinder, with two pistons clamped together by a connecting-rod as shewn in the diagram, and the space between them filled with incompressible inviscid liquid. Let the radius of curvature of the edge be $10^{-12}$ of a centimeter.”

The curvature being still finite, Thomson thought that Stokes would

42 See also Stokes to Thomson, 22–23 Nov. 1898 [ST, p. 707].
43 See also [ST, 13, 18, 19, 20 Dec. 1900; 4 Jan. 1901].
agree about the perfectly determinate and continuous character of the fluid motion induced by pushing the double piston. A moderate velocity of the piston would then imply an enormous pressure tending to break the connecting rod. Although Thomson did not say why, he probably reasoned by combining Bernoulli’s law and the impossibility of negative pressure at the edge, as he had earlier done for the flow around a globe. In the real world, Thomson went on, the connecting rod would either break, or yield slightly, thus allowing the liquid to leave the solid wall before it comes to the edge. In neither case would there be a slip of liquid over liquid.

The argument backfired. In his response [ST, 20, 21, 26 Dec. 1898], Stokes placed the cylinder and pistons vertically, and counterpoised the double piston and liquid by means of a string, pulley, and counterweight (Fig. 13).

Then a house-fly perching on the upper piston would according to Thomson suffice to break a connecting rod of large but finite resistance to trac-
tion. Stokes solution to this paradox was the formation of a surface of discontinuity past the edge, despite the lack of a strict angular point.\textsuperscript{44} In previous letters, he had only focused on the infinitely sharp edge because the instability of the “mike” solution was easiest to understand in this case. From the beginning, he believed that surfaces of discontinuity were formed behind smoothly shaped obstacles. Two days after he enunciated the house-fly paradox, he re-expressed his conviction that the “mike” solution for a uniform flow around a cylinder was unstable in the rear of the cylinder and challenged Thomson for a proof of stability in this case \cite{ST, 22 Dec. 1898}. He referred to the turbulent flow behind the pillars of a bridge as an instance of this instability. “It is hard to imagine,” he reflected, “that the instability which the commonest observation shows to exist is wholly due to viscosity, especially as an increase of viscosity seems to tend to increased stability, not the reverse.”

A week later, Stokes described how surfaces of discontinuity could be generated even without a sharp edge \cite{ST, 27 Dec. 1898}:

“I can see in a general way how it is that it is towards the rear of a solid moving through a fluid that a surface of discontinuity is formed. I find that at the point of a solid which is the birthplace of such a surface? the flowing fluid must go off at a tangent, and the fluid at the other side of the surface of discontinuity must just at the birthplace be at rest.”

In a crossing letter \cite{ST, 27 Dec. 1898}, Thomson denied instability in the perfect-liquid case, and proceeded to explain the practical instability for a real fluid of small viscosity and negligible compressibility such as water. He first considered the fluid motion induced by a sudden acceleration (from rest) of an immersed solid body:

“The initial motion of the water will be exceedingly nearly that of an incompressible inviscid liquid (the motion of minimum kinetic energy). There will be an exceedingly thin stratum of fluid round the solid through which the velocity of the water varies continuously from the velocity of the solid to the velocity in the solution for inviscid fluid. It is in this layer that there is instability. The less the viscosity, the thinner is this layer for a given value of the initial acceleration; but the surer the instability. Not

\textsuperscript{44} Cf. Stokes to Thomson, 14 Feb. 1899 \cite{ST}. Another escape would be to note that the fly cannot communicate a finite velocity to the piston, and therefore cannot induce an infinite pressure of the fluid if the “mike” solution still applies.
very logical this.”
Thomson did not say why he thought the thin layer of vorticity to be unstable. He only alluded to his earlier consideration [Thomson 1887c] on the instability of the plane Poiseuille flow (parallel flow between two fixed parallel plates), to be discussed later. He moved on to consider what would happen to the fluid if the acceleration ceased and the body (now a globe) was kept moving uniformly:

“If the velocity is sufficiently great, the motion of the fluid at small distances from its surface all round will always be very nearly the same as if the fluid were inviscid, and the difference will be smaller near the front part than near the rear of the globe.”

Here we have a description of what Ludwig Prandtl later called the boundary layer. The rest is more personal to Thomson: “If now the whole fluid suddenly becomes inviscid and the globe be kept moving uniformly, the rotationally moving fluid will be washed off from it, and left moving turbulently in the wake, and mixing up irrotationally moving fluid among it.”

Thus Thomson made viscosity responsible for the formation of an unstable state of motion, but regarded the instability of this state as being unrelated to viscosity and therefore felt free to “turn off” viscosity to discuss it. For a given state of motion at a given instant, viscosity could only have a stabilizing effect. Yet viscosity could make a stable state evolve toward an unstable one.  

In his reply to this letter [ST, 30 Dec. 1898], Stokes expressed his agreement with everything Thomson said, except for what would happen if the viscosity were suddenly brought to zero. In his opinion, “the streams of right-handedly revolving and left-handedly revolving fluid at the two sides would have the rotationally moving fluid washed away, at least in the side trails, and the streams would give place to streams bounded by surfaces of finite slip, commencing at the solid, and then being paid out from thence. The subsequent motion would doubtless be of a very complicated character [owing to the Helmholtz-Kelvin instability].”

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45 This is an anticipation of the Tollmien-Schlichting instability. Earlier anticipations by Reynolds and by Rayleigh are mentioned below.

46 The modern reader will recognize Prandtl’s separation process for the boundary layer.
Again, Stokes wanted the inviscid behavior to be a limit of the low-viscosity behavior. If a discontinuity surface was formed in the ideal inviscid fluid case, it had to play a role in the practical case of a slightly viscous fluid.

The debate continued until Stokes’ last letter to Thomson, dated 23 October 1901. In this late period the two old friends only reasserted their positions. They could not even agree on the (in)compatibility of Lagrange’s theorem and the formation of discontinuity surfaces. Stokes refined his picture of the formation of a discontinuity surface behind a moving solid sphere, so as to reach “continuity in the setting of discontinuity”: the contact line of the solid and surface began as a tiny circle around the rearward pole of the sphere, and then widened out until the surface took its final, steady shape [ST, 5 Jan. 1899]. Stokes also made the spiral unrolling of the discontinuity surface the true cause of eddying behind a solid obstacle [ST, 19–20 Dec. 1900]:

“It seems evident that the mere viscosity of water would be utterly insufficient to account for [the eddies] when they are formed on a large scale, as in a mill pool or whirlpool? Of course eddies are modified by viscosity, but except on quite a small scale I hold that viscosity is subordinate. Of course, it prevents a finite slip, which it converts into a rapid shear, but viscosity tends to stability, not to instability.”

Through their long, playful disagreement, Stokes and Thomson were driven by different interests: whereas Stokes wanted to understand the behavior of real liquids, Thomson primarily reasoned on the ultimate perfect liquid of the world. Thus they had opposite prejudices on the stability properties of the flow of a perfect liquid past a solid obstacle. As the intrinsic mathematical difficulty of the subject prevented a settling of issues by rigorous argument, they relied on intuition and past experience. Stokes appealed to the natural world and conjectured that the behavior of perfect liquids should reflect that of real liquids with small viscosity and compressibility. Thomson instead appealed to the energy-based dynamics which he regarded as the general foundation of physics. Thus he privileged the minimum-energy flow and an energy-based criterion of stability.

A few words on the modern understanding of the role of discontinuity surfaces will help to clarify some of the implied physics and to assess Stokes’ and Thomson’s anticipations of some aspects of hydrodynamic
instability.

Consider first the formation of discontinuity surfaces. As Stokes correctly argued, none of the theorems evoked by Thomson prohibits the formation of such surfaces, even in the absence of a sharp edge. These theorems presuppose the continuity of the motion. For example, the demonstration of Lagrange’s theorem requires the finiteness of the term $(\omega \cdot \nabla)v$ in the vorticity equation and therefore the continuity of the velocity.\footnote{This is emphasized in [Stokes 1845, pp. 106–113].}

If the flow is continuous at a given time, it remains so at subsequent times. If, however, a tiny surface of discontinuity is grafted on the wall, Helmholtz’s theorems and the electromagnetic analogy implies that it should grow at a rate given by the velocity-discontinuity at its origin, with a spiral unrolling of its extremity.\footnote{Jacques Hadamard [1903, pp. 355–361] gave a proof that surfaces of discontinuity cannot be formed in a perfect fluid as long as cavitation is excluded. This proof, however, does not exclude the growth of a pre-existing, tiny surface of discontinuity. Marcel Brillouin [1911] made this point, described the growth process, and extended the conformal methods of Helmholtz, Kirchhoff, and Levi-Civita to curved obstacles devoid of angular points. Felix Klein [1910] described the evolution of a surface of discontinuity formed by immersing an infinitely thin immersed blade (concretely a rudder) perpendicularly to the liquid surface, pulling it uniformly in a direction parallel to this surface, and suddenly withdrawing it. He resolved the apparent contradiction between Helmholtz’s vorticity theorems and the formation of discontinuity surfaces as follows: “Offenbar liegt dies [the contradiction] darin, dass wir das Zusammenfließen zweier ursprünglich voneinander getrennten Flüssigkeitspartien ins Auge zu fassen hatten, während bei der gewöhnlichen Begründung des genannten Satzes angenommen wird, dass Flüssigkeitsteilchen, welche einmal an der Oberfläche der Flüssigkeit liegen, immer auch an der Oberfläche bleiben.”}

In order that the discontinuity be finite, the fluid should be stagnant at one side of the origin of the discontinuity surface, and move continuously on the other side. Consequently, the surface must depart tangentially from the wall (in the case of an edge, it is at any time tangent to one side of the edge). As far as Marcel Brillouin [1911] and Felix Klein [1910] could see, there is nothing in Euler’s equations that contradicts this growth process. There is also nothing in this equation that restricts the points from which an embryonic surface would grow (at least in the two-dimensional case).\footnote{According to Brillouin [1911], in the two-dimensional case the departure point of a steady surface of discontinuity must be beyond a certain point of the surface of the body.}

In sum, in an Eulerian fluid surfaces of discontinuity can be formed...
as Stokes wished, but their departure point is more arbitrary than experiments on real fluids would suggest.

Another important issue of the Stokes-Thomson debate is the connection between inviscid and viscous behavior. According to Prandtl [1904], at high Reynolds numbers the flow of a real fluid along a solid obstacle is irrotational beyond a thin boundary layer of intense shear. Unless the solid is specially streamlined, this layer separates from the body at some point (line) of its rear part. The resulting flow resembles the surfaces of discontinuity imagined by Stokes for the Eulerian fluid. However, the separation point can only be determined through the Navier-Stokes equation (even though it does not depend on the value of the viscosity parameter!). From this perspective, Stokes was right to expect a resemblance between the low-viscosity limit of real flows and discontinuous Eulerian flow. But Thomson was also right to lend viscosity a decisive role in forming the thin vortex layers that imitate discontinuity surfaces.\footnote{50 For a viscous fluid, separation is not an instability issue. However, it is so in the ideal fluid case according to Stokes.}

5. PARALLEL FLOW

In the course of his acoustic studies, the London Professor John Tyndall learned of the sensitivity of flames to sound that his American Colleague John Le Conte had observed at a gas-lit musical party. The flame from the “fish-tail” gas burners gracefully danced as the musicians played a Beethoven trio, so that “a deaf man might have seen the harmony” [Le Conte 1858, p. 235]. In 1867 Tyndall displayed this funny phenomenon at the Royal Institution, as well as a similar effect with smoke jets, and published an account in the *Philosophical Magazine* [Tyndall 1867]. When subjected to various sounds, the jet shortened to form a stem with a thick bushy head (Fig. 14). The length of the stem depended on the pitch. High-pitch notes were ineffective. Tyndall made this instability the true cause of the dancing of flames. But he did not propose any theoretical explanation.

Tyndall’s work attracted Lord Rayleigh’s attention. This country gentleman had an unusual taste and ability for physics, both mathematical and experimental. Coached by Edward Routh and inspired by Stokes’ lectures at Cambridge, he emerged senior wrangler and Smith’s Prize- man in 1866. Until his appointment as Cavendish Professor at Maxwell’s
death (1879), his main research interests were optics and acoustics. His elegant and masterful *Theory of sound*, first published in 1877, became one of the fundamental treatises of British physics, and remains an important reference to this day.\(^{51}\)

Rayleigh the theorist of sound was naturally interested in Tyndall’s observations and in Félix Savart’s and Joseph Plateau’s earlier experiments on the sound-triggered instability of water jets. In the latter case, the determining factor is the capillarity of the water surface, which favors a varicose shape of the jet and its subsequent disintegration into detached masses whose aggregate surface is less than that of the original cylinder.\(^{52}\)

Rayleigh [1879] determined the condition for the growth of an infinitesimal sinusoidal perturbation of the jet surface, as Thomson had done to study the effect of wind on a plane water area. Rayleigh’s theory of smoke-jet instability was even more similar to Thomson’s theory. In this case, the relevant instability is that of a cylindrical surface of discontinuity for the air’s motion. Neglecting capillarity, Rayleigh showed that on a jet of velocity \(V\) a sinusoidal perturbation with the spatial period \(\lambda\) grew as \(e^{Vt/\lambda}\).

As Rayleigh did not fail to notice, this result contradicted Tyndall’s observation that short sound waves were ineffective. The cause of this discrepancy, Rayleigh [1880, pp. 474–475] surmised, could be the viscosity

\(^{51}\) Cf. [Lindsay 1976].

\(^{52}\) Cf. [Rayleigh 1896, pp. 362–365].
of the air. In the case of two-dimensional parallel motion, the Navier-Stokes equation implies that the vorticity $\omega$ evolves according to the equation

$$\frac{\partial \omega}{\partial t} = \frac{\mu}{\rho} \Delta \omega$$  \hspace{1cm} (18)

(the convective terms vanish): vorticity is “conducted” through the fluid according to the same laws as heat. Consequently, any vortex sheet or discontinuity surface evolves to form a layer of vorticity of finite thickness. Rayleigh then examined the stability of a finite layer of uniform vorticity. Switching off viscosity, he found that the layer became stable when the thickness of the layer somewhat exceeded the wave length of the perturbation. This result made it likely that viscosity, by smoothing out the velocity discontinuity, should stabilize a jet for high-pitch sound [Rayleigh 1880, pp. 475–483].

After thus resolving the discrepancy between fluid mechanics and Tyn dall’s experiments, Rayleigh proceeded to the theoretically similar problem of two-dimensional parallel flow between fixed walls. He first studied the stability of successive finite layers of uniform vorticity with perturbed separating surfaces, using Helmholtz’s analogy between vorticity and electric current. He thus guessed that stability, for a continuous variation of the vorticity $\omega$, would depend on the constancy of the sign of the variation $d\omega/dy$ between the two walls. In other words, the curvature $d^2U/dy^2$ of the velocity profile could not change sign [Rayleigh 1880, pp. 483–484].

Rayleigh then proceeded to the more direct approach to the stability problem that soon became standard [Rayleigh 1880, pp. 484–487]. Call $0x$ an axis parallel to the flow, $0y$ the perpendicular axis, $U(y)$ the original velocity, $u(x,y)$ and $v(x,y)$ the components of a small velocity perturbation. The vorticity equation (17) gives

$$\frac{\partial \omega}{\partial t} + (U + u) \frac{\partial \omega}{\partial x} + v \frac{\partial \omega}{\partial y} = 0,$$  \hspace{1cm} (19)

with

$$\omega = \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y} - \frac{dU}{dy}.$$  \hspace{1cm} (20)

Retaining only first-order terms in $u$ and $v$, assuming that $u$ and $v$ vary as $e^{i(kx-\sigma t)}$, and eliminating $u$ by means of the continuity equation
∂u/∂x + ∂v/∂y = 0, yields the stability equation

\[ (U - \frac{\sigma}{k}) \left( \frac{\partial^2 v}{\partial y^2} - k^2 v \right) - \frac{d^2 U}{dy^2} v = 0. \] (21)

Therefrom Rayleigh obtained his stability criterion in the following ingenious manner:

The stability equation has the form \( v'' + \alpha v = 0 \), with

\[ \alpha = -k^2 - \frac{U''}{U - \sigma/k}. \] (22)

Multiplying by the complex conjugate \( v^* \) of \( v \), and integrating from wall to wall gives

\[ \int |v'|^2 dy + \int \alpha |v|^2 dy = 0. \] (23)

Hence, the imaginary part of the function \( \alpha \) must satisfy the condition

\[ \int \text{Im}(\alpha) |v|^2 dy = 0, \] (24)

or

\[ \text{Im}(\sigma) \int \frac{|v|^2}{|U - \sigma/k|^2} U'' dy = 0. \] (25)

If the sign of \( U'' \) is constant (and if the perturbation does not uniformly vanish), the integral is non-zero, so that the imaginary part of \( \sigma \) must vanish and the perturbation cannot grow exponentially. Rayleigh concluded to stability in this case.\(^53\) As he noted, the criterion is of no help in the jet case, for which \( U'' \) changes sign.

In this discussion of parallel flow between fixed walls, Rayleigh probably had in mind a two-dimensional approach to the stability of pipe flow.\(^54\)

Yet he did not discuss this application, presumably because of the lack of relevant experiments. Osborne Reynolds [1883] filled this gap, with his experimental study of the transition between “direct” and “sinuous”

\(^53\) Rayleigh [1880, p. 487] also gave without proof the criterion in the cylindric case that “the rotation either continually increases or continually decreases in passing outwards from the axis.”

\(^54\) Rayleigh says so in [RSP 3, p. 576].
flow in straight circular pipes. The distinction between two kinds of flow had been known at least since Navier used it to explain the failure of his hydrodynamic equation for hydraulic retardation. It was also known since the 1850s that Navier’s equation, with the condition of zero-velocity at the walls, applied to the flow in narrow pipes studied by Jean-Louis Poiseuille around 1840. Yet the conditions for the transition between the two kinds of flow had never been studied.\(^55\)

Reynolds’ motivation for such a study came from his earlier interest in a parallel phenomenon: the transition between the ballistic and continuous regimes for the flow of a dilute gas through a porous material. In this case, the transition depends on the size of the pores compared to the mean-free-path of the gas molecules. Reynolds suspected that the transition from direct (laminar) to sinuous (turbulent) flow would similarly depend on a dimensional property of the Navier-Stokes equation. Specifically, he had the turbulent eddying depend on an excess of the inertial term of the vorticity equation over the viscous damping term (the relevant vorticity equation is the curl of the Navier-Stokes equation). When the flow depends on only one characteristic length \(L\) (pipe diameter) and on the average velocity \(V\), the ratio between the two terms is governed by the ratio \(LV/\nu\), where \(\nu\) is the kinematic viscosity \(\mu/\rho\). This ratio is now called the Reynolds number [Reynolds 1883, pp. 54–55].

Through his colour-band experiments, Reynolds verified that the critical transition depended on this number. He thereby noted the surprisingly sudden character of this transition: violent eddying occurred as soon as the critical Reynolds number was reached. Moreover, the flow appeared to be unstable with respect to finite perturbations well before the critical number was reached: “The critical velocity was very sensitive to disturbance in the water before entering the tubes [...] This showed that the steady motion was unstable for large disturbances long before the critical velocity was reached, a fact which agreed with the full-blown manner in which the eddies appeared” [Reynolds 1883, p. 61].\(^56\)

\(^{55}\) Cf. [Darrigol 2002b].

\(^{56}\) A similar statement is found in [Reynolds 1883, pp. 75–76]: “The fact that the steady motion breaks down suddenly, shows that the fluid is in a state of instability for disturbances of the magnitude which cause it to break down. But the fact that in some condition it will break down for a large disturbance, while it is stable for a smaller disturbance, shows that there is a certain residual stability, so long as the disturbances
From casual observations of conflicting streams of water, Reynolds was aware of the existence of another kind of instability, for which the transition from direct to sinuous motion was gradual and independent of the size of disturbances. His memoir of 1883 recounts an elegant experiment in which he had a lighter fluid slide over a heavier one with a variable velocity difference [Reynolds 1883, pp. 61–62]. For a certain critical velocity, the separating surface began to oscillate. The waves grew with the sliding velocity, until they curled and broke.

Reynolds was unaware of relevant theoretical considerations by Helmholtz, Kelvin, and Rayleigh. He was therefore “anxious” to find a theoretical explanation of the two kinds of instabilities he had encountered [ibid., p. 62]. He first studied the stability of the solutions to Euler’s equation, with the result that “flow in one direction was stable, flow in opposite directions unstable” [ibid., p. 63]. As he could only imagine a stabilizing effect of viscosity, the instability of pipe flow long puzzled him. At last, he attempted a similar study in the more difficult case of the Navier-Stokes equation. He then found that the boundary condition for viscous fluids (vanishing velocity at the walls) implied instability for sufficiently small values of the viscosity [ibid., p. 63]: “Although the tendency of internal viscosity of the fluid is to render direct or steady motion stable, yet owing to the boundary condition resulting from the friction at the solid surface, the motion of the fluid, irrespective of viscosity, would be unstable.”

Reynolds further explained that “as long as the motion was steady, the instability depended upon the boundary action alone, but once eddies were introduced, the stability would be broken down.” He thereby meant that the introduction of an eddy changed the distribution of velocity and thus induced an instability of the frictionless kind, which could overcome a much higher viscous damping than the boundary-based instability. As a corollary to this view, there should be a value of the Reynolds number below which instability with respect to finite disturbances disappears. Reynolds thus inferred the existence of a second critical velocity of pipe flow, “which would be the velocity at which previously existing eddies do not exceed a given amount [...] It was a matter of surprise to me to see the sudden force with which the eddies sprang into existence, showing a highly unstable condition to have existed at the time the steady motion broke down. – This at once suggested the idea that the condition might be one of instability for disturbances of a certain magnitude, and stable for small disturbances.”
would die out, and the motion become steady as the water proceeded along the tube” [Reynolds, p. 64]. His experiments on pipe retardation were aimed at verifying this.

Reynolds never published his stability calculations. He could plausibly have handled the inviscid case in a manner similar to Rayleigh’s, though the roughness of his statement of the criterion suggests some erring. That he could derive a boundary-layer instability in the viscous case seems highly implausible, considering the tremendous difficulty of the later considerations of that sort by Werner Heisenberg, Walter Tollmien, and others.57

In his presidential address to the British Association for the Advance-
ment of Science meeting of 1884, Rayleigh praised Reynolds’ contribu-
tion to the study of the transition between laminar and turbulent flow. His view of the future of the subject was singularly optimistic [Rayleigh 1884b, p. 344]: “In spite of the difficulties which beset both the theoretical and the experimental treatment, we may hope to attain before long to a better understanding of a subject which is certainly second to none in scientific as well as practical interest.” He and Stokes were plausibly responsible for the subject of the Adams prize for 1889: “On the criterion of the stability and instability of the motion of a viscous fluid.” After a reference to Reynolds’ work, the announcement of the prize read: 58

“It is required either to determine generally the mathematical criterion of stability, or to find from theory the value [of the critical Reynolds number] in some simple case or cases. For instance, the case might be taken of steady motion in two dimensions between two fixed planes, or that of a simple shear between two planes, one at rest and one in motion.”

The only theorist to claim success in solving these two cases was no
beginner in need of £170: Sir William Thomson it was.59 In the second case (plane Couette flow)60, the simpler one because of its constant vorticity, Thomson provided a fairly explicit procedure for deriving the evolution

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57 On the latter considerations, cf. [Drazin and Reid 1981, chap. 4].
59 According to The Cambridge Review, 9 (1889), p. 156, the prized was not adjudged in default of candidates.
60 The Couette flow is the steady viscous flow between two concentric parallel cylinders, one of which is rotating at constant speed.
of an arbitrary small perturbation of the flow [Thomson, 1887c]. From the Navier-Stokes equation and the incompressibility condition, he first obtained the linearized equation

\begin{equation}
\left( \frac{\partial}{\partial t} + U \frac{\partial}{\partial x} - v \Delta \right) \Delta v = 0
\end{equation}

for the normal velocity perturbation \( v \) of the basic flow \( U = \beta y \).

As Thomson astutely noted, this equation and those for the other components \( u \) and \( w \) can be solved explicitly for any initial value of the perturbed velocity compatible with the incompressibility condition, if only the real boundary condition (vanishing relative velocity on the plates) is replaced with the sole condition of vanishing normal velocity at the plates for \( t = 0 \). Call this the relaxed solution.

Thomson next used Fourier’s method to find a forced solution of the linearized equations for which the components of the velocity perturbation on the plates are equal to prescribed functions of time. For the latter prescription, he chose the functions that vanish for negative time and that are equal and opposite to the perturbed velocity components of the relaxed motion on the plates for positive time. Thomson believed that the corresponding forced solution vanished identically for negative time. Consequently, he regarded the sum of the relaxed and the forced solution as the solution of the real initial-value problem. The relaxed solution is easily seen to decrease exponentially in time. This implies the same behavior for the forced and the complete solution. Thomson concluded for stability in this case.

In the other case of the Adams prize (plane Poiseuille flow), Thomson could no longer obtain the relaxed solution and therefore imagined a new method. He simply argued that Fourier analysis, which in the former case he only used to derive the forced solution, could be directly applied to the real initial-value problem. He seems to have believed that both the boundary condition and the prescription of a given initial value could be met by superposing Fourier components varying as \( e^{i(\sigma t + kx + mz)} \). Accordingly, he contented himself with proving that for any non-zero value of the viscosity parameter and for any value of \( \sigma, k, m \), convergent power series expressions could be found for the \( y \) dependence of the Fourier components. From this result he concluded that the plane Poiseuille flow was also stable [Thomson 1887d].
Thomson last dealt with the practical instability of pipe flow. In conformity with Reynolds’ observation that stability could depend on the size of disturbances, he proposed that pipe flow was probably stable for infinitesimal perturbations (as in the two dimensional case) but unstable for finite ones. It would be so, he argued, if the inviscid flow with Poiseuille velocity profile was unstable, and if viscosity could only damp sufficiently small perturbations. The margin of stability would then increase for higher viscosity, as Reynolds had observed [Thomson 1887a, p. 335].

The instability of inviscid flow with a parabolic velocity profile clearly contradicted Rayleigh’s inflection theorem. Thomson [1887a, p. 334] believed, however, that a “disturbing infinity vitiate[d] [Rayleigh’s] seeming proof of stability.” As Rayleigh [1880, p. 486] himself noted, the stability equation

\[
(U - \frac{\sigma}{k}) \left( \frac{\partial^2 v}{\partial y^2} - k^2 v \right) - \frac{d^2 U}{dy^2} v = 0
\]

becomes singular wherever the velocity of the \(\sigma/k\) of the plane-wave perturbation becomes identical with the velocity \(U\) of the unperturbed flow (and \(U''\) does not simultaneously vanish). At such a point, the flow is obtained by superposing a sine-wave velocity pattern with a shearing motion. For an observer moving along the fluid, the flow has the “cat’s eye” outlook of Fig. 15, which Thomson [1880c] drew in *Nature*. From that date, Thomson attached great importance to the disturbing infinity: The ‘awkward infinity’, he wrote to George Darwin on 22 August 1880, “threatens quite a revolution in vortex motion (in fact a revolution where nothing of the kind, nothing but the laminar rotational movement, was even suspected before), and has been very bewildering” [Thompson 1910, p. 760]. Thomson believed the elliptic whirls of this flow to be the source of the turbulence observed by Reynolds. Any simple perturbation of the fluid boundary necessarily contained Fourier components for which elliptic whirling would disturb the laminar flow.\(^{61}\)

Rayleigh [1892, p. 580] defended his stability criterion against Thomson’s “disturbing infinity”:

“Perhaps I went too far in asserting that the motion was thoroughly stable; but it is to be observed that if [the frequency \(\sigma\)] be complex, there

\(^{61}\) Thomson does not address the question of the growth of the whirls.
is no ‘disturbing infinity.’ The argument, therefore, does not fail regarded as one for excluding complex values of $\sigma$. What happens when $\sigma$ has a real value such that $[\sigma - kU]$ vanishes at an interior point, is a subject for further examination.”

Equation (21) is indeed non-singular for a complex value of $\sigma$, so that exponential increase of infinitesimal perturbations and constant sign of $U''$ are truly incompatible. Rayleigh conceded, however, that the impossibility of exponential increase did not rigorously establish stability. Perhaps a less rapid increase of perturbations was still possible owing to the “disturbing infinity.” Perhaps, higher order terms in the stability equation implied a qualitative departure from the first-order behavior. In sequels to his 1880 study, Rayleigh [1887, 1895] provided arguments that made these escapes to his criterion implausible. Modern writers on hydrodynamic stability no longer question the validity of his stability criterion.\footnote{Cf. [Drazin and Reid 1981, pp. 126–147].}

In the same memoir Rayleigh [1892, p. 582] questioned Thomson’s proofs of stability of plane, viscous flow:

“Naturally, it is with diffidence that I hesitate to follow so great an authority, but I must confess that the argument does not appear to me demonstrative. No attempt is made to determine whether in free disturbances of the type $[e^{i\sigma t}]$ the imaginary part of $[\sigma]$ is finite, and if so whether it is positive or negative. If I rightly understand it, the process consists in an investigation of forced vibrations of arbitrary (real) frequency, and the conclusion depends on the tacit assumption that if these forced vibrations can be expressed in periodic form, the steady motion from which they are deviations cannot be unstable.”
Rayleigh went on to show that the tacit assumption was wrong in the case of a (rigid) pendulum situated near the highest point of his orbit. Whether he correctly interpreted Thomson’s intentions is questionable. He was right, however, to judge Thomson’s reasoning incomplete. The Irish mathematician William Orr [1907] clearly identified the gaps.

Consider first, the proof of stability of the plane Poiseuille flow found in Thomson’s second paper. This proof assumes that the superposition of harmonic solutions (with respect to \( t, x, z \)) that meet the boundary conditions is sufficient to reproduce any initial value of the velocity perturbation. This does not need to be true, because the boundary conditions might restrict the harmonic solutions too much [Orr 1907, p. 85].

In 1895, Rayleigh admitted the validity of Thomson’s “special solution” for the stability of the plane Couette flow. Yet Thomson’s proof also fails in this case. As Orr [1907, p. 85] pointed out, the forced solution in this proof does not need to vanish for \( t = 0 \), even though it is forced to vanish on the boundaries of the fluid for any negative time. Indeed in this case it is easily seen that the boundary conditions completely determine the Fourier-type solution, thus leaving no room for a further restriction of the initial motion. Consequently, the complete solution does not have to be a solution with the arbitrary initial value of the relaxed solution.

Thomson himself seems to have become aware of the weakness of his reasoning. On 27 December 1898, he wrote to Stokes [ST]: “Several papers of mine in Phil. Mag. about 1887 touch inconclusively on this question [of the stabilizing effect of viscosity].” In the meantime, Rayleigh [1892, pp. 576–577] pointed to the basic paradox of pipe flow:

“If the [Rayleigh criterion] is applied to a fluid of infinitely small viscosity, how are we to explain the observed instability which occurs with moderate viscosities? It seems very unlikely that the first effect of increasing viscosity should be to introduce an instability not previously existent, while, as observation shows, a large viscosity makes for stability.”

He offered a few suggestions to explain this discrepancy. First, irregularities of the walls may play a role. Second, instability may occur for finite disturbances even when the Rayleigh criterion gives stability. Third, the three-dimensional case of Reynolds experiments may qualitatively differ

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63 Orr’s interpretation of Rayleigh’s criticism seems unconvincing to me, however.
from the two-dimensional case studied by Rayleigh and Thomson. Fourth, “it is possible that, after all, the investigation in which viscosity is altogether ignored is inapplicable to the limiting case of a viscous fluid when the viscosity is supposed infinitely small.”

The main purpose of Rayleigh’s paper was to exclude the third possibility by extending his stability criterion to cylindrically-symmetrical flow. In retrospect, his short comments on the fourth conjecture are most interesting [Rayleigh 1892, p. 577]:

“There is more to be said in favor of this view than would at first be supposed. In the calculated motion there is a finite slip at the walls [when viscosity is ignored], and this is inconsistent with even the smallest viscosity. And further, there are kindred problems relating to the behaviour of a viscous fluid in contact with fluid walls for which it can actually be proved that certain features of the motion which could not enter into the solution, were the viscosity ignored from the first, are nevertheless independent of the magnitude of viscosity, and therefore not to be eliminated by supposing the viscosity to be infinitely small.”

Rayleigh had in mind the explanation he had given in 1883 of an acoustic anomaly discovered by Savart in 1820 and studied by Faraday in 1831: when a plate sprayed with light powder is set into vibration, the powder gathers at the antinodes of the motion, whereas Ernst Chladni’s older experiments with sand gave the expected nodal figures. Faraday traced this anomaly to the action of currents of air, rising from the plate at the antinodes, and falling back at the nodes.  

In his confirming calculation, Rayleigh [1883] assumed a plane monochromatic stationary wave motion of the plate and solved the Navier-Stokes equation for the fluid motion above the plate perturbatively, regarding the non-linear \((\mathbf{v} \cdot \nabla)\mathbf{v}\) term as the perturbation. The resulting motion is confined near the plate in a layer of thickness \((\nu/f)^{1/2}\), where \(\nu\) is the kinematic viscosity and \(f\) the frequency of the oscillations. This layer includes a periodic array of vortices as shown on Fig. 16. The maximum of the vortical motion is as \(v_0^2/V\), where \(v_0\) is the maximum velocity of the motion of the plate, and \(V\) the velocity of the progressive waves of which this motion is composed. As Rayleigh [1883, p. 246] emphasized, this

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64 Cf. [Rayleigh 1883, pp. 239–240] and [Rayleigh 1896, vol. 1, pp. 367–368]. Rayleigh also explained the air currents observed by Vincenz Dvořák in 1876 in Kundt’s tubes.
maximum does not depend on the value of the viscosity $\nu$: “We cannot, therefore, avoid considering this motion by supposing the coefficient of viscosity to be very small, the maintenance of the vortices becoming easier in the same proportion as the forces tending to produce the vortical motion diminish.”

Rayleigh anticipated a similar singularity of the zero-viscosity limit in the case of plane parallel flow. This view agreed with Reynolds’ assertion that intense shear near the walls caused the instability observed in his pipe flow experiments. Many years later, in 1947, Tollmien proved the correctness of this intuition in the plane case. In 1924 Heisenberg obtained the instability of the plane Poiseuille flow, through a method of approximation whose validity could be established only much later by Chia Chiao Lin and others. For circular pipes, the flow is probably stable at any Reynolds number, although a complete proof is still lacking. The latter problem is mathematically similar to the plane Couette flow, for which a rigorous proof of stability is now available. Nineteenth-century experts on fluid mechanics did not possess the mathematical techniques that have proven necessary even in the simplest problems of viscous-flow stability. Yet they could anticipate various mechanisms of instability: finite-disturbance effects, intense shear in boundary layers, and irregularity of walls.\(^{65}\)

A last nineteenth-century approach to parallel-flow instability is found in Reynolds [1894]. As we saw, Reynolds’ first intuition of a stability criterion was based on a comparison of the orders of magnitude of the inertial and viscous terms in the vorticity equation. In 1894 he instead considered the balance of the inertial and viscous terms of the equation

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he obtained for the variation in time of the *energy* of the eddying motion. Reynolds thereby assumed the existence of a macroscopic averaging scale for which the mean motion no longer involved turbulent eddying. Under this assumption, the energy of the eddying motion is borrowed inertially from the energy of the mean-motion [mean-mean-motion for Reynolds], and damped by viscous forces. As a stability criterion, Reynolds required the dominance of the damping term of his eddying-energy equation over the inertial one for any choice of the eddying motion. By laborious calculations he estimated the corresponding Reynolds number in the case of flow between two fixed parallel plates.\(^{66}\)

Reynolds’ method can at best yield a value of the Reynolds number below which the motion must be stable. It does not allow one to determine the Reynolds number from which certain perturbations (not necessarily of the random eddying kind) will grow. The general idea of studying the evolution of the energy of a perturbation of the laminar motion has nevertheless seduced later students of hydrodynamic instability, including Hendrik Lorentz, William Orr, Theodor von Kármán, and Ludwig Prandtl. In some cases, as Tollmien’s boundary-layer instability, it provides some physical understanding of the mechanism of instability.\(^{67}\)

Thomson’s, Rayleigh’s, and Reynolds’ mathematical studies of parallel flow show how impenetrable the caprices of fluid motion could be to the elite of nineteenth-century mathematical physics. Where stability was hoped for, for instance in Kelvin’s vortex rings, it became highly unlikely. Where instability was observed, for instance in Reynolds’ pipes, it turned out to be very hard to prove. The first failure threatened the British hope of basing all physics on the perfect liquid. The second stood in the way of concrete applications to hydraulic or aerodynamic processes. Yet the few mathematical successes obtained in simple, idealized cases, together with inspired guesses on general fluid behavior, helped formulating some of the basic questions and methods of modern fluid dynamics.

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\(^{66}\) Cf. [Darrigol 2002b].

\(^{67}\) Cf. Lin [1966, pp. 59-63].
Abbreviations

BAR: British Association for the Advancement of Science, Report.

BB: Akademie der Wissenschaften zu Berlin, mathematisch-physikalische Klasse, Sitzungsberichte.


HN: Helmholtz Nachlass, Akademie der Wissenschaften, Berlin.

HSPS: Historical Studies in the Physical and Biological Sciences.


JRAM: Journal für die reine und angewandte Mathematik.


PM: Philosophical Magazine.


PT: Royal Society of London, Philosophical Transactions.

ReP: Osborne Reynolds, Papers on Mechanical and Physical Subjects, 3 vols., Cambridge, 1900–1903.


TCPS: Cambridge Philosophical Society, Transactions.


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