Arch. Hist. Exact Sci. 56 (2002) 95–150.

Between Hydrodynamics and Elasticity Theory: The First Five Births of the Navier-Stokes Equation

OLIVIER DARRIGOL

Communicated by J. Z. BUCHWALD

The Navier-Stokes equation is now regarded as the universal basis of fluid mechanics, no matter how complex and unpredictable the behavior of its solutions may be. It is also known to be the only hydrodynamic equation that is compatible with the isotropy and linearity of the stress-strain relation. Yet the early life of this equation was as fleeting as the foam on a wave crest. Navier's original proof of 1822 was not influential, and the equation was rediscovered or re-derived at least four times, by Cauchy in 1823, by Poisson in 1829, by Saint-Venant in 1837, and by Stokes in 1845. Each new discoverer either ignored or denigrated his predecessors' contribution. Each had his own way to justify the equation. Each judged differently the kind of motion and the nature of the system to which it applied.

All of these investigators wished to fill the gap they perceived between the rational fluid mechanics inherited from d'Alembert, Euler, and Lagrange, and the actual behavior of fluids in hydraulic or aerodynamic processes. A similar gap existed in the case of elasticity. The formulas established by mathematicians for the flexion of prisms were of little help in evaluating the limits of rupture in physical constructions. French engineer-mathematicians trained at the Polytechnique, such as Navier, Cauchy, and Saint-Venant, were best equipped and most motivated to fill these gaps. As a preliminary step toward a more realistic theory of elasticity, in 1821 Navier announced the general equations of equilibrium and motion for an (isotropic, one-constant) elastic body. He soon obtained the Navier-Stokes equation by transposing his reasoning to fluids. Other discoverers of the equation also started from elasticity, except Stokes who reversed the analogy. Because of this contextual and structural interdependence between elastic solids and viscous fluids, the present paper is as much on the general theory of elasticity as on the Navier-Stokes equation.

The comparison between the various proofs of this equation – or between the corresponding proofs of the equation of motion of an elastic body – brings forth important characteristics of mathematical physics in the period 1820–1850. A basic methodological and ontological issue was the recourse to molecular reasoning. Historians have often perceived an opposition between Laplacian molecular physics on the one hand, and macroscopic continuum physics on the other, with Poisson the champion of the former physics, Fourier of the latter. However, closer studies of Fourier's heat theory have shown that the opposition pertains more to the British reading of this work than to its actual content. The present study of contemporary contributions to fluid and elastic-body dynamics brings further evidence of the hybridization of molecular and continuum physics.

All investigators in these fields, be they engineers or mathematicians, agreed that the properties of real, concrete bodies required the existence of non-contiguous molecules. There were even proofs, by Cauchy, Poisson, and Saint-Venant, that were thought rigorously to demonstrate that a continuous solid was impossible. That Laplacian physics rested on an arbitrary, unjustified ontology is a retrospective, ahistoric judgement. Many at the time believed that this physics rested on a firm, non-hypothetical basis. All five authors of the Navier-Stokes equation shared a molecular ontology, but they differed considerably over the extent to which their derivations materially involved molecular assumptions.

A wide spectrum of methodological attitudes existed at the time. At one extreme was Poisson, who insisted on the necessity of discrete sums over molecules. At the other extreme was Cauchy, who combined infinitesimal geometry and spatial symmetry arguments to define strains and stresses and to derive equations of motion without referring to molecules. Yet the opposition was not radical. Poisson used Cauchy's stress concept, and Cauchy eventually did provide his own molecular derivations. Others compromised between the molecular and the molar approach. Navier started with molecular forces, but quickly jumped to the macroscopic level by considering virtual works. Saint-Venant insisted that a clear definition of the concept of stress could only be molecular, but nevertheless provided a purely macroscopic derivation of the Navier-Stokes equation. Stokes obtained the general form of the stresses in a fluid by a Cauchy type of argument, but he justified the linearity of the stresses with respect to deformations by reasoning on hard-sphere molecules.

These methodological differences largely explain why Navier's successors ignored or criticized his derivation of the Navier-Stokes equation. His short-cuts from the molecular to the macroscopic levels seemed arbitrary or even contradictory. Cauchy and Poisson simply ignored Navier's contribution to fluid dynamics. Saint-Venant and Stokes both gave credit to Navier for the equation, but believed an alternative derivation to be necessary. To this day, Navier's contribution has been constantly belittled, even though upon closer examination his approach turns out to be far more consistent than a superficial reading may suggest.

The wide spectrum of methodological attitudes, both in fluid mechanics and in elasticity theory, derived from different views of mathematical rigor, and different degrees of concern with engineering problems. Cauchy and Poisson, who were the least involved in engineering and the most versed in higher mathematics, must have been suspicious of Navier's way of injecting physical intuition into mathematical derivations. Yet many engineers judged Navier's approach to engineering problems too mathematical and too idealized. The disagreements were enhanced, and at times even determined by personal ambitions and priority controversies. Acutely aware of these tensions, Saint-Venant developed innovative strategies to combine the demands of mathematical rigor and practical usefulness.

The many authors of the Navier-Stokes equation also differed in the types of application they envisioned. Navier and Saint-Venant had pipe and channel flow in mind. Cauchy's and Poisson's interests were more philosophical than practical. Cauchy did not even intend the equation to be applied to real fluids: he derived it for "perfectly inelastic solids," and noted its identity with Fourier's heat equation in the limiting case of slow motion. Lastly, Stokes was in good part motivated by British geodesic measurements that required aerodynamic corrections to pendulum oscillations.

To Navier's disappointment, his equation worked well only for slow, regular motions. This was enough for pendulums and capillary tubes, but was nearly worthless for hydraulics. Even in the case of regular flow, applications were troubled by a boundary condition that Navier had inferred from anterior experiments on capillary tubes and that was later rejected. In the case of turbulence, there seemed to be no alternative to the empirical approach of hydraulic engineers. Saint-Venant nevertheless proposed a reinterpretation of Navier's equation that extended it to cover large-scale, average motion with an effective viscosity that depended on small-scale, irregular motion. Stokes, for his part, suggested that turbulent behavior derived from instabilities of regular solutions of the Navier-Stokes equation. These were small but important indications of how the gap between hydrodynamics and hydraulics might someday be bridged.

The first section of this paper is devoted to the weakness of Euler's hydrodynamics in respect to hydraulic problems, and to Girard's study of flow in capillary tubes, which Navier, unfortunately for him, trusted. The second section describes Navier's achievement in the theory of elasticity, its transposition to fluids, and the application to Girard's tubes. The third section discusses Cauchy's stress-strain approach and its adaptation to a "perfectly inelastic solid." The fourth recounts Poisson's struggle for rigor in the molecular approach, Cauchy's own push for rigor in the same approach, and Navier's response to Poisson's attack. The precise nature of their arguments and their impact on fluid theory should thus become more apparent than in former, otherwise fine characterizations of Cauchy's and Poisson's styles. The fifth section concerns Saint-Venant's unique brand of applied mechanics, his contributions to elasticity and hydraulics, and his strategies for including irregular and turbulent motions within his scheme. The sixth and last section deals with Hagen's and Poiseuille's experiments on narrow-pipe discharge, and their long-delayed explanation by the Navier-Stokes equation.

For the sake of conciseness and ease of understanding, vector and tensor notation is used throughout this paper. All of the figures whose work we will examine used Cartesian-coordinate notation (including Saint-Venant, who nevertheless dreamt of a better one). They had a more global perception of Cartesian formulas, as well as much greater experience, practice and patience in using them than we have today, so that the tools at their disposal were not so blunt as it might seem to modern readers. Nevertheless, tensors and vectors are not only notationally convenient; they also carry with them insights that are much harder to come by in a purely Cartesian framework. Most important for elasticity theory, relations of isotropy that are now quite obvious because we represent certain physical quantities by tensors, did not jump off the page in the early nineteenth century. The reader will be alerted where appropriate to issues of this kind.

[The following abbreviations are used: *ACP*, *Annales de physique et de chimie*; *AP*, *Annalen der Physik*; *BSM*, Société de Mathématiques, *Bulletin*; *BSP*, Société Philomatique, *Bulletin*; *CR*, Académie des sciences, *Comptes-rendus hebdomadaires des séances*; *DSB, Dictionary of scientific biography*, ed. C.C. Gillispie, 16 vols. (New York, 1970–1980); *EM*, *Exercices de mathématiques*, journal ed. by A. Cauchy; *HSPS*, *Historical studies in the physical (and biological) sciences*; *MAS*, Académie (Royale) des Sciences, *Mémoires (de physique et de mathématique)*; *SMPP*, Stokes, George Gabriel, *Mathematical and physical papers*, 5 vols. (Cambridge, 1880–1905); (*TCPS*, Cambridge Philosophical Society, *Transactions*].

1. Mathematicians' fluids versus engineers' fluids

The Euler-d'Alembert paradox

In the second half of the eighteenth century, Jean le Rond d'Alembert, Leonhard Euler, and Joseph Lagrange established a mathematical theory of fluid motion on general mechanical principles. In a famous memoir of 1755, Euler obtained the equations of motion by equating the product of mass and acceleration for a cubic element of the fluid to the resultant of the pressures and external forces acting on and in this element. In modern notation this gives

$$\rho\left[\frac{\partial \mathbf{v}}{\partial t} + (\mathbf{v}\cdot\nabla)\mathbf{v}\right] = \mathbf{f} - \nabla P, \tag{1}$$

where ρ is the density of the fluid, $\mathbf{v}$ its velocity, P the pressure, and $\mathbf{f}$ the external force density. The motion of the fluid is further restricted by what we now term the "continuity equation," which expresses the conservation of the mass of the fluid element during its motion:[1]

$$\frac{\partial \rho}{\partial t} + \nabla\cdot(\rho\mathbf{v}) = 0. \tag{2}$$

For the steady motion of an incompressible fluid subject to constant gravity, this theory yields the so-called Bernoulli equation,

$$P + \tfrac{1}{2}\rho v^2 + \rho g z = \text{constant} \tag{3}$$

along any line of flow, wherein g denotes the acceleration due to gravity and z the height of the fluid element. From this follow the theorems on efflux that Daniel Bernoulli derived in his *Hydrodynamica* (1738) by the consideration of live forces. Lagrange introduced the velocity potential φ such that $\mathbf{v} = \nabla\varphi$, and (nearly) proved its existence for any flow resulting from a motion of the containing walls or from accelerating forces which themselves derive from a potential. This condition did not seem too restrictive at the time, and it greatly eased the solution of Euler's equations. Lagrange used it to discuss two-dimensional flow patterns, and to derive the speed of small waves on the fluid's surface.[2]

Unfortunately, this splendid theory led to absurd results when applied to concrete problems of fluid resistance and flow retardation. In 1752, d'Alembert found that the resultant of the pressures on a head-tail symmetrical body that is immersed in the steady flow of his ideal fluid was always zero. Owing to the symmetry, d'Alembert reasoned, the fluid velocities had to be the same at the front and at the rear of the body; so, then,

---

[1] Cf. P. Dugas, *Histoire de la mécanique* (Paris, 1950), 278–292; C. Truesdell, "Rational fluid mechanics, 1687–1765," in L. Euler, *Opera omnia*, Ser. 2, vol. 12 (Zürich, 1954), IX–CXXV.

[2] Cf. Dugas (ref. 2), 333–334.

were the pressures according to Bernoulli's law, in which case their resultant had to vanish. In 1768 d'Alembert enunciated his famous paradox in the following words:[3]

> Thus I do not see, I admit, how one can satisfactorily explain by theory the resistance of fluids. On the contrary, it seems to me that the theory, developed in all possible rigor, gives, at least in several cases, a strictly vanishing resistance; a singular paradox which I leave to future geometers for elucidation.

Euler had already derived this paradox in 1745, without any restriction on the shape of the body. His reasoning was based on an inspired, if, in comparison with Saint-Venant's later proof of 1837 – discussed below – non-rigorous use of momentum conservation. Roughly, the momentum gained by the immersed body in a unit of time should be equal to the difference of momentum fluxes across normal plane surfaces situated far ahead and far behind the body. This difference vanishes because of the equality of velocity and mass flux on the two surfaces.[4]

Euler knew no better escape from this paradox than a partial return to Edme Mariotte's and Isaac Newton's old theory of fluid resistance. According to these pioneers of fluid mechanics, the impact of fluid particles on the front of the immersed body completely determined the resistance. Similarly, Euler cut off the rear part of the tubes of flow to which he applied his momentum balance. In such theories, the actual form of the flow, and the shape of the rear of the body were irrelevant. The experimental inexactitude and the theoretical weakness of this view were already admitted in Euler's day. Yet it remained popular until the beginning of the nineteenth century, for it explained three basic facts: that the resistance was proportional to the density of the fluid, to the squared velocity of the flow, and to the cross-section of the body.[5]

Less concerned than Euler with engineering problems, d'Alembert did not seriously try to explain fluid resistance. He merely alluded to the possible effect of an asymmetry of the fluid motion (around his symmetrical body), owing to the "tenacity and the adherence of the fluid particles." Being unsure about the truth of this intuition, he did not try to express it mathematically. Neither did his follower Lagrange.[6]

---

[3] J. le Rond d'Alembert, *Essai d'une nouvelle théorie de la résistance des fluides* (Paris, 1752), par. 27–33; *Opuscules mathématiques*, vol. 5 (Paris, 1768), 132. Cf. A. Barré de Saint-Venant, *Résistance des fluides: Considérations historiques, physiques et pratiques relatives au problème de l'action dynamique mutuelle d'un fluide et d'un solide, dans l'état de permanence supposé acquis par leurs mouvements* (Paris, 1887), 7–11, 31–33.

[4] L. Euler, Commentary to the German transl. of B. Robins, *New principles of gunnery* (Berlin, 1745), Chap. 2, Prop. 1, Rem. 3 (French transl.: 316–317). Cf. Saint-Venant (ref. 3), 29–31. A more rigorous reasoning would have required a cylindrical wall to limit the flow laterally, together with a proof that the works of pressure forces on the two plane faces of the cylinder are equal and opposite. This cancellation results from the equality of pressures on the two faces, which can itself be derived from Bernoulli's theorem or from the conservation of live force. Compare with Saint-Venant's proof of 1837, discussed below.

[5] Cf. Saint-Venant (ref. 3), 34–36. On the Newtonian theory, cf. Saint-Venant, ibid., 15–29; G.E. Smith, "Newton's study of fluid mechanics," *International journal of engineering science*, 36 (1998), 1377–1390.

[6] D'Alembert, *Opuscules mathématiques*, vol. 8 (Paris, 1780), 211. Cf. Saint-Venant (ref. 3), 10. The fluid resistance data used in 1877 by the Academic commission for the Picardie canal, to

Pipes and channels

These great mathematicians were even less concerned with the worldly problems of pipe and channel flow than with fluid resistance. Available knowledge in this field was mostly empirical. Since Mariotte's *Traité du mouvement des eaux* (1683), hydraulic engineers had assumed a friction between running water and walls, proportional to the wetted perimeter, and increasing faster than the velocity of the water. This velocity was taken to be roughly constant in a given cross-section of the pipe or channel, in conformity with common observation. Claude Couplet, the engineer who designed the elaborate water system of the Versailles castle, performed the first measurements of the loss of head in long pipes of various sections. Some fifty years later Charles Bossut, a Jesuit who taught mathematics at the engineering school of Mézières, performed more precise and extensive measurements of the same kind.[7]

So did his contemporary Pierre Du Buat, an engineer with much experience in canal and harbor development, and the author of a very influential hydraulic treatise. Du Buat's superiority rested on a sound mechanical interpretation of his measurements. He was first, in print, to give the condition for steady flow by balancing the pressure gradient (in the case of a horizontal pipe) or the parallel component of fluid weight (in the case of an open channel) with the retarding frictional force. He took into account the loss of head at the entrance of pipes (due to the sudden increase of velocity), whose neglect had flawed his predecessors' results for short pipes. Lastly, he proved that fluid friction, unlike solid friction, did not depend on pressure.[8]

Bossut found the retarding force to be proportional to the square of velocity, and Du Buat to increase somewhat slower than that with velocity. Until the mid-nineteenth century, German and French retardation formulas were usually based on the data accumulated by Couplet, Bossut, and Du Buat. In 1804 the Directeur of the Ecole des Ponts et Chaussées, Gaspard de Prony, provided the most popular formula, which made the friction proportional to the sum of a quadratic and a small linear term. The inspiration for this form came from Coulomb's study of fluid coherence, to be discussed in a moment.[9]

---

which d'Alembert belonged, were purely empirical: cf. P. Redondi, "Along the water: The genius and the theory," in Mike Chrimes, ed., *The civil engineering of canals and railways before 1850* (Aldershot, 1997), 143–176.

[7] E. Mariotte, *Traité du mouvement des eaux et des autres corps fluides* (Paris, 1686), Part 5, Discourse 1. Cf. Saint-Venant (ref. 3), 39–40; H. Rouse and S. Ince, *History of hydraulics* (Ann Arbor, 1957), 114 (Couplet), 126–128 (Bossut).

[8] P. Du Buat, *Principes d'hydraulique, vérifiés par un grand nombre d'expériences faites par ordre du gouvernement* (Paris, 1786), reed. (1816), 3 vols. (Paris, 1816), vol. 1, xvii, 14–15, 40. Cf. Saint-Venant, *Notice sur la vie et les ouvrages de Pierre-Louis-Georges, Comte Du Buat* (Lille, 1866); Rouse and Ince (ref. 7), 129–134. In 1775 Antoine Chézy had already given the condition of steady motion in an unpublished report for the Yvette canal: cf. ibid., 117–120.

[9] Cf. Rouse and Ince (ref. 7), 141–143. In 1803 P.S. Girard had used a non-homogenous $v+v^2$ formula, also inspired from Coulomb.

Fluid coherence

For Du Buat's predecessors, the relevant friction occurred between the fluid and the walls of the tube or channel. In contrast, Du Buat mentioned that internal friction was needed to check the acceleration of internal fluid filaments. He observed that the average fluid velocity used in the retardation formulas was only imaginary, that the real flow velocity increased with the distance from the walls, and even vanished at the walls in the case of very reduced flow. The molecular mechanism he suggested for the resistance implied the adherence of fluid molecules to the walls so that the retardation truly depended on internal fluid processes. Specifically, Du Buat imagined that the adhering fluid layer impeded the motion of the rest of the fluid partly as a consequence of molecular cohesion, and mostly because of the granular structure of this layer. This structure implied a "gearing" of traveling along molecules (*engrenage des molécules*), through which they lost a fraction of their momentum proportional to their average velocity, at a rate itself proportional to this velocity. Whence the quadratic behavior of the resistance.[10]

In 1800, the military engineer Charles Coulomb used his celebrated torsional technique to study the "coherence of fluids and the laws of their resistance in very slow motion." The experiments consisted in measuring the damping of the torsional oscillations of a disk suspended by a wire through its center and immersed in various fluids. Their interpretation depended on Coulomb's intuition that the coherence of fluid molecules implied a friction proportional to velocity, and surface irregularities an inertial retardation proportional to the square of velocity. In conformity with this view, Coulomb found that the quadratic component depended only on density, and that the total friction became linear for small velocities. From his further observation that greasing or sanding the disk did not alter the linear component, he concluded:[11]

> The part of the resistance which we found to be proportional to the velocity is due to the mutual adherence of the molecules, not to the adherence of these molecules with the surface of the body. Indeed, whatever be the nature of the plane, it is strewn with an infinite number of irregularities wherein fluid molecules take permanent residence.

Although Du Buat's and Coulomb's emphasis on internal fluid friction or viscosity was exceptional in their day, the notion was far from new. Newton had made it the cause of the vortices induced by the rotation of an immersed cylinder, and he had even provided a derivation (later considered to be flawed) of the velocity field around the cylinder.

---

[10] Du Buat (ref. 8), 22, 39–41, 58–59, 89–90. Du Buat's notion of fluid viscosity or cohesion was not quite identical with internal friction as we now understand it. Rather Du Buat meant an "adhesion" of the molecules that needed to be overcome to separate them, the resistance to this separation being proportional to its suddenness. Also, he believed (ibid., 41) that the microscopic structure of the surface of the pipe or channel had no effect on the retardation, for it was hidden by the adhering layer of fluid.

[11] C.A. Coulomb, "Expériences destinées à déterminer la cohérence des fluides et les lois de leur résistance dans les mouvements très lents," Institut National des Sciences et des Arts, *Mémoires de sciences mathématiques et physiques*, 3 (1800), 246–305, on 261 (two kinds of resistance), 287 (quote). Cf. C.S. Gillmore, *Coulomb and the evolution of physics and engineering in eighteenth-century France* (Princeton, 1971), 165–174.

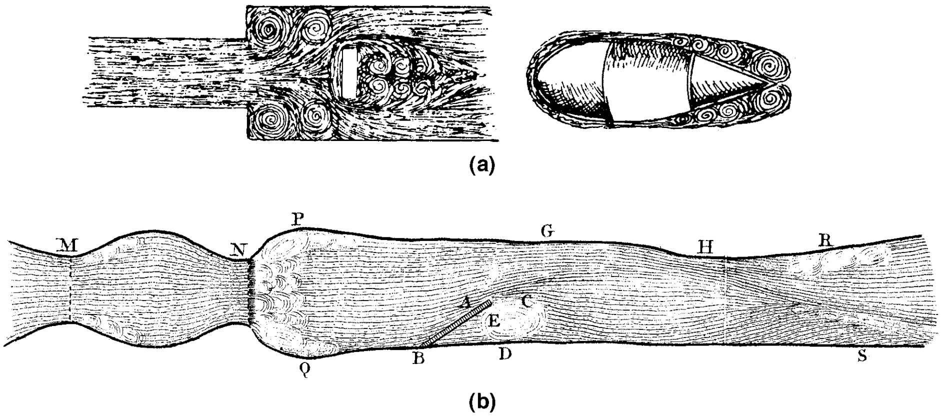


Fig. 1. Eddy formation according to Vinci (**a**), according to Venturi (**b**) (from Rouse and Ince (ref. 7), 46; and Venturi, plate, ref. 13)

He assumed (in conformity with later views) that the friction between two consecutive, coaxial layers of the fluid was proportional to their velocity difference. After a century during which this issue was nearly completely ignored, in 1799 the Italian hydraulic engineer Giovanni Battista Venturi offered experiments that displayed important effects of internal fluid friction.[12]

Venturi intended to prove "the lateral communication of motion in fluids" and to show its consequences for various kinds of flow. Some of the effects he described, such as the increase of efflux obtained by adding a divergent conical end to the discharging pipe, were in fact purely inertial effects that were already known to Daniel Bernoulli. Others, such as the formation of eddies, genuinely depended on internal friction. The eddies that Leonardo da Vinci had beautifully drawn for the flow past immersed bodies, those evoked by Bernoulli for sudden pipe enlargement, or those commonly seen in the smoke from chimneys or in rivers behind bridge pillars, were all due, Venturi explained, to "motion communicated from the more rapid parts of the stream to less rapidly moving lateral parts" (Fig. 1). Venturi also made eddy formation one of the principal causes of retardation in rivers, which current wisdom attributed to friction against banks and bottom.[13]

Venturi prudently avoided deciding whether the lateral communication of motion was occasioned "by the viscidity or mutual adhesion of the parts of the fluids, or their mutual

[12] I. Newton, *Principia mathematica*, Book 2, Prop. 51; G.B. Venturi, *Recherches expérimentales sur le principe de communication latérale dans les fluides* (Paris, 1797). Cf. Saint-Venant (ref. 3), 41–44 (Newton); Rouse and Ince (ref. 7), 134–137 (Venturi); G.J. Dobson, "Newton's errors with the rotational motion of fluids," *Archive for history of exact sciences*, 54 (1999), 243–254.

[13] Venturi (ref. 12), transl. in Thomas Tredgold (ed.), *Tracts on hydraulics* (London, 1826), 123–184, on 165.

engagement or intermixture, or the divergency of those parts which are in motion." Nor did he suggest new equations of fluid motion. As he explained in his introduction,

> The wisest philosophers have their doubts with regard to every abstract theory concerning the motion of fluids: and even the greatest geometers avow that those methods which have afforded them such surprising advances in the mechanics of solid bodies, do not afford any conclusions with regards to hydraulics, but such as are too general and uncertain for the greater number of particular cases.

Venturi's memoir enjoyed a favorable review by the French Academicians Bossut, Coulomb, and Prony. Together with Du Buat's and Coulomb's works on fluid friction, it helped to revive the old Newtonian notion of friction between two contiguous layers of fluid.[14]

Girard's capillary tubes

In 1816, the Paris water commissioner and freshly elected Academician Pierre-Simon Girard applied this notion to a six-month long study of the motion of fluids in capillary tubes. His prominent role in the construction of the *Canal de l'Ourcq* and his contribution to several hydraulic projects amply justified his interest in flow retardation. Yet Girard had the more philosophical ambition of participating in Laplace's new molecular physics. He believed the same molecular cohesion forces to be responsible for the capillarity phenomena analyzed by Laplace and for retardation in pipe flow. By experimenting on fluid discharge through capillary tubes, he hoped both to contribute to the theory of molecular forces and to the improvement of hydraulic practice.[15]

In conformity with Du Buat's observations for reduced flows, Girard assumed that a layer of fluid remained at rest near the walls of the tube. He further assumed that the rest of the fluid moved with a roughly uniform velocity. Flow retardation then resulted from friction between the moving column of fluid and the adherent layer. Girard favored experiments on capillary tubes, no doubt because measurements were easier in this case but also because he believed (incorrectly on later views) that the uniformity of the velocity of the central column would better apply to narrower tubes (because of a presumably higher cohesion of the fluid). He operated with copper tubes of two different diameters (D) around 2 and 3 mm and lengths (L) varying between 20 cm and 2.20 m. The tubes were horizontal and fed by a large water vessel under a constant height H (Fig. 2). Girard took the pressure gradient in the tube to be equal to $\rho g H/L$, where g is the acceleration of gravity and ρ the density of water. Following Coulomb and Prony, he assumed the form $av + bv^2$ for the retarding force on the unit surface of the tube, where v is the

[14] Ibid., 132–133, 129; Prony, Bossut, and Coulomb, Report on Venturi's memoir (ref. 12), Institut de France, Académie des Sciences, *Procès-verbaux des séances*, 1 (1795–1799), 271–272.

[15] Girard, "Mémoire sur le mouvement des fluides dans les tubes capillaires et l'influence de la température sur ce mouvement" [Read on 30 Apr and 6 May 1816], Institut National des Sciences et des Arts, *Mémoires de sciences mathématiques et physiques* (1813–1815), 249–380. Cf. I. Grattan-Guinness, *Convolutions in French mathematics, 1800–1840*, 3 vols. (Basel, 1990), vol. 1, 563–565.

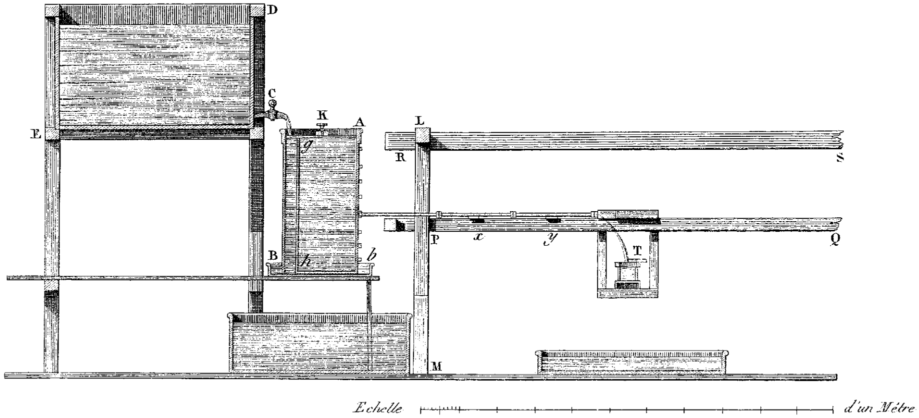


Fig. 2. Girard's apparatus for measuring discharge through narrow tubes (from Girard, plate, ref. 15). The water from the tank D is maintained at constant level in the tank A and flows through the horizontal tube (lying on xy) into the bucket T

flow velocity and a and b two tentative constants. The balance of the forces acting on a cylindrical slice of fluids then gives[16]

$$\rho g D H/4L = av + bv^2. \tag{4}$$

Girard measured the rate of discharge $\pi D^2 v/4$ for various lengths and charges, at a temperature varying with the season or controlled artificially. His first conclusion was that the quadratic friction term disappeared for tubes of sufficient length. Consequently, he assumed the friction to be fundamentally linear, and the quadratic contribution to be due to the lack of (recti)linearity of the flow near the entrance of the tube (involving the so-called *vena contracta* and its subsequent oscillations). He then focused on the linear behavior, apparently forgetting the engineer's interest in the quadratic contribution (which dominates in the case of large pipes of any length). He found that the "constant" a significantly decreased when the temperature rose, and that it varied with the diameter of the tube.[17]

Girard produced a nice molecular explanation for these effects. A temperature increase, he reasoned, implies a dilation of the fluid and therefore a decrease in the mutual adhesion of the fluid molecules. As for the dependence of a on the tube's diameter, Girard explained it by the finite thickness e of the adherent layer of fluid, which implies the substitution of $D - 2e$ for D in Eq. (4). He further argued that for high temperatures the thickness e should be negligible since the adhesion between fluid and wall molecules would be small. Then the original formula (4) (with $b = 0$) and the proportionality of the discharge to the cube of the diameter hold approximately, as his measurements with

[16] Ibid., 257–258, 265.

[17] Ibid., 285. Ibid. on 287 Girard insisted that contrary to Coulomb's case, the velocity did not need to be small for the quadratic term to disappear. Girard borrowed the expression of the accelerating force and the expression "linear motion" from Euler (cf. ibid., 307).

heated water seemed to confirm. In a sequel to this memoir, Girard used glass tubes instead of copper and various liquids instead of water, meaning to confirm his view that the thickness of the adhering layer depended on molecular forces between layer and wall.[18]

As he had little to offer to the hydraulic engineer, Girard provided something for the physiologist. The capillary dimensions of vessels and the wetting of their walls, he noted, was essential to explain blood or sap circulation in animals and plants. Otherwise body temperature could not control the circulation, and friction would wear the vessels. Girard expressed his amazement at the "simplicity of the means of Nature and the perfection of its works." One might argue that Girard should rather have considered more carefully than he did the soundness of his experimental method and theoretical conclusions: they suffer in comparison with contemporary those of the best French experimenter of his day, despite the Academy's official approbation of his work.[19]

In absence of contemporary criticism, we may only imagine which flaws a more careful contemporary could have detected in Girard's work. While estimating the charge H of the tube, Girard did not include the loss of head due to the entrance in the tube, even though Du Buat had noted the importance of this correction for short pipes. In considering the variation of the discharge rate with the diameter of the tube, he used only two different diameters and did not indicate how he had measured them. Judging from Gotthilf Hagen's later measurements, the numbers provided by the manufacturer or a simple external measurement could not be trusted.

These factors, among others, may explain why Girard did not obtain the D^4 law for the discharge, which we know to be quite accurate, why he found glass to provide a stronger discharge than copper, and why he believed that retardation would be linear for any diameter and velocity if the tube were long enough. On the theoretical side, he conflated adhesion with friction, and therefore did not appreciate the circumstances that determined the velocity profile. Nevertheless, Girard did obtain the linear bevavior in H/L for the discharge through narrow tubes, which is as well-known today as it was surprising to contemporary hydraulicians.

The rational and the practical

To sum up, at the beginning of the nineteenth century the consensus was that rational fluid dynamics could not explain practically important phenomena such as fluid resistance and flow retardation. Most knowledge of these phenomena was empirical and derived from the observations and measurements accumulated by hydraulic engineers. The concept of internal friction had been available since Newton, and was somewhat

[18] Ibid., 315–321, 328–329; "Mémoire sur l'écoulement linéaire de diverses substances liquides par des tubes capillaires de verre" [read on 12 Jan 1817], *MAS*, 1 (1816), 187–274, on 235. In this second memoir, Girard used and praised the graphic method that Prony had used of channel flow; he found that the linear term did not exist for mercury, as he expected from the fact that mercury does not wet glass.

[19] Ibid., 259.

revived by Du Buat, Venturi, Coulomb, and Girard. Yet no one used it as a basis for the mathematical determination of fluid motion.

It may seem surprising that no one before Navier tried to insert new terms in Euler's hydrodynamic equations. A first explanation for this is that the new hydrodynamics was part of a rational mechanics that valued clarity, formal generality, and rigor above empirical adequacy. Another is that Euler's equations were complex enough to saturate contemporary mathematical capability. They were among the first partial differential equations ever to have been written, and they involved the non-linearity that has troubled mathematical physicists to this day. Even if someone had been willing to modify Euler's equations, he would have lacked empirical clues about the structure of the new terms, because the concept of internal friction was as yet immature.

Last but perhaps most important, the French mathematicians who were most competent to invent new partial differential equations all accepted d'Alembert's fundamental principle of dynamics, according to which the equations of motion of a mechanical system can be obtained from the condition that the forces impressed on the system are in equilibrium with fictitious inertial forces (mass times reversed acceleration). From this point of view, the hydrodynamic equations should result directly from the laws of hydrostatics. Since the latter were solidly established, Euler's equations seemed unavoidable.[20]

2. Navier: Molecular mechanics of solids and fluids

X+Ponts

In the contemporary jargon of the *Grandes Ecoles*, Claude-Louis Navier was an "X+Ponts," – an engineer trained first at the Ecole Polytechnique and then at the Ecole des Ponts et Chaussées. He embodied a new style of engineering that combined the analytical skills acquired at the Polytechnique with the practical bent of the *Ecoles d'application*. Through his theoretical research and his teaching he contributed to a renewal of the science of mechanics that forged a much better fit between it and the needs of engineers. Navier is famed for having promoted considerations of "live force" (kinetic energy) and "quantity of action" (work) in the theory of machines, in which he was following Lazare Carnot's pioneering treatise on this subject, and preparing the way for Gaspard Coriolis' and Jean-Victor Poncelet's later developments.[21]

Orphaned at fourteen, Navier was educated by his uncle Emiland Gauthey, a renowned engineer of bridges and canals. He later expressed his gratitude through a careful

---

[20] Cournot expressed this view in his comment on Navier's equation, to be discussed below.

[21] Cf. R.M. McKeon, "Navier, Claude-Louis-Marie-Henri," *DSB*, vol. 10 (1974), 2–5. On the new style of engineering, cf. B. Belhoste, "Un modèle à l'épreuve. L'Ecole Polytechnique de 1794 au second Empire," in B. Belhoste, A. Dahan, and A. Picon, *La formation polytechnicienne. 1794–1994* (Paris, 1994), 9–30; A. Picon, *L'invention de l'ingénieur moderne: L'Ecole des Ponts et Chaussées, 1747–1851* (Paris, 1992), chaps. 8–10. On the concept of work, cf. I. Grattan-Guinness, "Work for the workers: Advances in engineering mechanics and instruction in France, 1800–1830," *Annals of science*, 41 (1984), 1–33.

edition of Gauthey's works, which was published in 1809–16. His competence in hydraulic architecture then led him to edit Bernard Forest de Bélidor's voluminous treatise on the subject, which had been a canonical reference since its first publication in 1737. In this new edition, published in 1819, Navier left Bélidor's text intact but quarreled with several of its theoretical conceptions, which continued to affect engineering practice in France and elsewhere. His footnotes and appendices constituted a virtual book within the book, including a new presentation of mechanics, a theory of machines based on live forces, and extensive corrections of the flaws that he uncovered in Bélidor.[22]

Navier found Bélidor's treatment of hydraulic problems particularly unsatisfactory. Here is his judgment of the theory of efflux found in the *Architecture*:

> The preceding theory, with which the author seems so pleased, now appears to be one of the most defective of his work. In truth one had not, at the time he was writing, gathered a sufficient amount of experiments so as to establish the exact measure of phenomena; but this does not justify the totally vicious theory that he gives of it, nor the trust with which he presents it.

In order to correct Bélidor on efflux, Navier had only to return to Daniel Bernoulli and to refer to several of Venturi's experiments.[23]

Navier had much more difficulty with the problems posed by water resistance. As he well knew, the old impact theory had been disproved by numerous well-attested experiments, from Jean-Charles Borda's in the 1760s to Bossut's and Du Buat's in the 1780s and 1790s. Navier could only deplore that contemporary hydrodynamics did not permit a definitive solution to this problem. He agreed with Euler that momentum balance applied to the tubes of flow around the immersed body should yield the value of the resistance. But no more than Euler could he justify the truncation of the tubes that yielded a non-zero resistance proportional to the squared velocity. Even less could he account for the negative pressure that Du Buat had found to exist at the rear of the body.[24]

From Coulomb, Navier also knew that the resistance became proportional to velocity for very slow motion. He agreed with Coulomb that in this case the retarding force resulted from "the mutual adhesion of the fluid molecules among themselves or at the surface of the immersed bodies." In sum, he considered two causes of fluid resistance: a non-balanced distribution of pressure around the immersed body owing to some particularity in the shape of the lines of flow around the body, and friction occurring between the body and the successive layers of fluid owing to "molecular adhesion." He respected Bélidor's omission of pipe flow.[25]

---

[22] Cf. McKeon (ref. 21); Prony, biography of Saint-Venant, in Navier, *Résumé des leçons données à l'Ecole des Ponts et Chaussées sur l'application de la mécanique à l'établissement des constructions et des machines. Première section: De la Résistance des corps solides*, 3rd ed. annotated by Saint-Venant (Paris, 1864), xxxix-li; Saint-Venant, commented bibliography, ibid., lv-lxxxiii; Grattan-Guinness (ref. 15), vol. 2, 969–974.

[23] B.F. de Bélidor, *Architecture hydraulique*, ed. Navier (Paris, 1819), 285n.

[24] Ibid., 339n–356n. On the fluid-resistance experiments by Borda, Bossut, and Du Buat, cf. Dugas (ref. 1), 297–305; Rouse and Ince (ref. 7), 124, 128, 133–134.

[25] Ibid., 345n. Ibid., on 292n Navier briefly mentioned the "friction of the fluid on the [pipe] walls" (but not the internal adhesion in this case).

Laplacian physics

Another novelty of Navier's edition was the respect it payed to Laplace's new molecular physics. Imitating Newton's gravitation theory and some of his queries, the French astronomer sought to explain the properties of matter by central forces acting between molecules. His first successful attempt in this direction was a theory of capillarity published in 1805–1806. In the third edition of his *Système du monde*, published in 1808, he also indicated how optical refraction, elasticity, hardness, and viscosity could all be reduced to short range forces between molecules. In an appendix to the fifth volume of the *Mécanique céleste,* published in 1821, he gave a detailed molecular theory of sound propagation, based on his and Claude-Louis Berthollet's idea that molecular repulsion depended on the compression of an elastic atmosphere of caloric.[26]

In the foreword to his 1819 edition of Bélidor, Navier approved Laplace's idea of the constitution of solids:

> Eventhough the intimate constitution of bodies is unknown, the phenomena which they show allow us to clearly perceive a few features of this constitution. From the faculty that solid bodies have to dilate under heating, to contract under cooling, and to change their figure under effort, it cannot be doubted that they are made of parts which do not touch each other and which are maintained in equilibrium at very small distances from each other by the opposite actions of two forces, one of which is an attraction inherent in the nature of matter, and the other a repulsion due to the principle of heat.

At that time Navier used this conception of solids only to banish from collision theory the ideally hard bodies of rational mechanics. However, he also referred to Laplace's theory of capillarity in a footnote. The conditions of equilibrium of fluids, he emphasized, could not be rigorously established without the molecular viewpoint. A fortiori, fluid motion had to depend on molecular processes, as he argued in his discussion of Coulomb's fluid-friction experiments.[27]

Elastic beams and plates

In his engineering activity, Navier acted mostly as an expert on bridge construction. In the 1810s, he designed three new bridges on the Seine river, and oversaw an important bridge and embankment project in Rome. This work, as well as his edition of Gauthey's works, made him appreciate the empirical inadequacies of the existing theoretical treatments of the elasticity of solid bodies. Previous calculations of the compression, extension, and flexion of beams had for example assumed the existence of mutually independent longitudinal fibers that would resist extension or compression

---

[26] Cf. J.L. Heilbron, *Weighing imponderables and other quantitative science around 1800* (Berkeley, 1993), 1–16; R. Fox, "The rise and fall of Laplacian physics," *HSPS*, 4 (1974), 89–136; *The caloric theory of gases: From Lavoisier to Regnault* (Oxford, 1971); M. Crosland, *The Society of Arcueil* (Cambridge, 1967); Grattan-Guinness (ref. 15), chap. 7.

[27] Navier, in Bélidor (ref. 23), x-xi, 208n. Ibid., on 215n Navier rejected Bernoulli's and Bélidor's kinetic interpretation of pressure.

by a proportional tension or pressure; or they used even more drastic idealizations in which the beam was replaced with a line or blade whose curvature determined the elastic response. Navier worked to improve the reasoning in terms of fibers so as to apply it usefully to the practically essential question of rupture. He still taught this point of view in the course he began to teach at the Ponts et Chaussées in 1819, although he also told his students that the true foundation of elasticity should be molecular.[28]

In August 1820, Navier submitted to the Academy of Sciences a memoir on vibrating plates in which he still reasoned in terms of continuous deformations. The problem of vibrating plates had occupied several excellent minds since the German acoustician Ernst Chladni, with fine sand and violin bow, had presented their nodal lines to French Academicians in 1808. Whereas Sophie Germain and Lagrange still reasoned on the basis of presumptive relations between curvature and restoring force, in 1814 Laplace's close disciple Siméon Denis Poisson offered a first molecular theory. He considered a two-dimensional array of molecules, and computed the restoring force acting on a given molecule by summing the forces exerted by the surrounding displaced molecules. To perform this summation, Poisson assumed, as Laplace had done in his theory of capillarity, that the sphere of action of a molecule was very small compared to a macroscopic deformation but that it nevertheless contained a very large number of molecules. He replaced the molecular sums with integrals and retained only low-order terms in the Taylor expansion of the deformation.[29]

Poisson's analysis confirmed the differential equation used by Lagrange and Germain. Navier sought a new derivation only because he believed that the boundary conditions were still in doubt. In his memoir of 1820, he applied Lagrange's method of moments, which had the virtue of yielding simultaneously the equation of motion and the boundary conditions. For simplicity Navier assumed that the local deformation of the plate could be decomposed into flexion and isotropic extension in the plane of the plate. Then he obtained the equations of equilibrium of the plate by computing the 'moment' (virtual work) of internal pressures or tensions for a virtual deformation and balancing it with the moment of external forces. As we will see, a molecular version of this method played an important role in Navier's subsequent work on elasticity and hydrodynamics. A digression to its Lagrangian origin will help to understand how it works.[30]

---

[28] Cf. Prony (ref. 22), xliii-xliv; Saint-Venant, "Historique abrégé des recherches sur la résistance et sur l'élasticité des corps solides," in Navier (ref. 22), xc-cccxi, on civ-cix. On the history of elasticity, see also Truesdell, "The rational mechanics of flexible or elastic bodies, 1638–1788," in Euler, *Opera Omnia*, ser. 2, vol. 11, part 2 (Zürich, 1960); I. Todhunter and K. Pearson, *A history of the theory of elasticity*, 2 vols. (Cambridge, 1886, 1893); S. Timoshenko, *History of strength of materials* (New York, 1953); E. Benvenuto, *An introduction to the history of structural mechanics*, 2 vols. (New York, 1991). On Navier's course, cf. Picon (ref. 21), 482–495.

[29] S.D. Poisson, "Mémoire sur les surfaces élastiques" [read on 1 Aug 1814], Institut National des Sciences et des Arts, *Mémoires de sciences mathématiques et physiques* 9 (1812), 167–226. Cf. Saint-Venant (ref. 28), ccliii-cclviii; A. Dahan, *Mathématisations: Augustin Cauchy et l'Ecole Française* (Paris, 1992), chap. 4; Grattan-Guinness (ref. 15), vol. 1, 462–465.

[30] Navier, "Sur la flexion des plans élastiques," MS read on 14 Aug 1820 at the Academy, lithograph in the archive of the Ecole Nationale des Ponts et Chaussées; extract in *BSP* (1823), 95–102. Cf. Saint-Venant (ref. 28), cclix-cclx; Grattan-Guinness (ref. 15), vol. 2, 977–983.

Lagrange's method of moments

In order to establish the equations of equilibrium of an incompressible fluid, Lagrange applied the principle of virtual velocities, which requires the total moment $\int \mathbf{f} \cdot \mathbf{w} d\tau - \int \mathbf{w} \cdot P d\mathbf{S}$ of the external force density $\mathbf{f}$ (for instance gravitational) and of the external pressure P to vanish for any virtual displacement $\delta\mathbf{r} = \mathbf{w}$ of the elements of the fluid that is compatible with the constraint of incompressibility $\nabla \cdot \mathbf{w} = 0$. Introducing the Lagrange parameter λ, this condition is equivalent to

$$\int (\mathbf{f} \cdot \mathbf{w} + \lambda \nabla \cdot \mathbf{w}) d\tau - \int \mathbf{w} \cdot P d\mathbf{S} = 0, \tag{5}$$

where the only constraint left on $\mathbf{w}$ is that on the surface of the fluid it should be parallel to the walls of the vessel (if any). After integration by parts of the λ term, this leads to

$$\int (\mathbf{f} - \nabla\lambda) \cdot \mathbf{w} d\tau + \int (\lambda - P) \mathbf{w} \cdot d\mathbf{S} = 0. \tag{6}$$

Within the fluid this condition implies that $\mathbf{f}$ is compensated by $-\nabla\lambda$. At the free surface of the fluid, it implies equality between λ and the external pressure. Lagrange thus retrieved the conditions of equilibrium of a fluid with the appropriate boundary condition. In another section of his *Mécanique analytique,* he also introduced the moment $\int\int F \delta dS$ of the elastic tension F that arises in response to an isotropic extension of a plate. To this moment Navier added the moment of the elastic tensions or pressures that arise in response to the flexion of a plate of finite thickness.[31]

The general equations of elasticity

On the one hand, Navier admired Lagrange's method for its power to yield the boundary conditions. On the other, he approved of Laplace's and Poisson's molecular program. Within a few months of the submission of his memoir on elastic plates, he managed to combine these two approaches. He probably first re-derived the moments for the elastic plate by summing molecular moments. Having done so, he may then have realized that this procedure could easily be extended to an arbitrary, small deformation of a three-dimensional body. He thereby obtained the general equations of elasticity for an isotropic body (with one elastic constant only). In the memoir he read on 14 May 1821, he gave two different derivations of these equations: by direct summation of the forces acting on the given molecules, and by the balance of virtual moments. This second route, Navier's favorite, goes as follows.[32]

---

[31] J.L. de Lagrange, *Mécanique analytique*, 2nd ed., 2 vols. (Paris, 1811, 1815), vol. 1, 188–196, 148–151. Cf. Dahan (ref. 29), 50–51.

[32] Navier, "Mémoire sur les lois de l'équilibre et du mouvement des corps élastiques" [read on 14 May 1821], *MAS*, 7 (1827), 375–394; extract in *BSP* (1823), 177–181. Cf. Saint-Venant (ref. 28), cxlvii-cxlix; Dahan (ref. 29), chap. 8; Grattan-Guinness (ref. 15), 983–985. In the BSP "extract" of his memoir on elastic plates (ref. 30), Navier assumed a molecular foundation for the flexion moment.

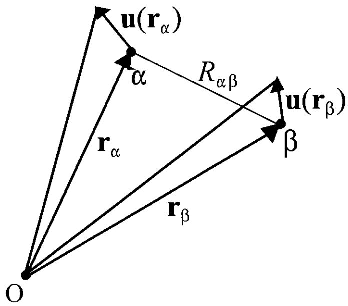


Fig. 3. Diagram for displacements in an elastic body

The solid being initially in its natural state of equilibrium, the original moment of molecular forces vanishes. After a macroscopic deformation of the solid such that the positions of its particles go from $\mathbf{r}$ to $\mathbf{r}+\mathbf{u}(\mathbf{r})$, the vector $\mathbf{R}_{\alpha\beta}$ joining the two molecules α and β is modified by $\delta\mathbf{R}_{\alpha\beta} = \mathbf{u}(\mathbf{r}_\beta) - \mathbf{u}(\mathbf{r}_\alpha)$ (Fig. 3). To first order in $\mathbf{u}$, the corresponding change of distance $\delta R_{\alpha\beta}$ is given by the projection u_R of the vector $\mathbf{u}(\mathbf{r}_\beta) - \mathbf{u}(\mathbf{r}_\alpha)$ on the line joining the two molecules. Navier assumes that for small deformations the force between two molecules varies by an amount proportional to the change in their distance, the proportionality coefficient being a rapidly decreasing function $\phi(R_{\alpha\beta})$ of their distance. This restoring force must be attractive for an increase of distance, and repulsive for a decrease of distance.

Now consider a virtual displacement $\mathbf{w}(\mathbf{r})$ of the particles of the solid. To first order in $\mathbf{u}$, the deformation $\mathbf{u}$ implies a change of moment $-\phi\delta R w_R$ for the forces between the molecules α and β, wherein w_R is the projection of the difference $\mathbf{w}(\mathbf{r}_\beta) - \mathbf{w}(\mathbf{r}_\alpha)$ on the line joining these two molecules (the indices α and β affecting R are dropped to simplify the notation; an attraction is reckoned positively). Consequently, the total moment of molecular forces after the deformation is

$$M = -\frac{1}{2}\sum_{\alpha\beta}\phi(R)u_R w_R. \tag{7}$$

Exploiting the rapid decrease of the function $\phi(R)$, Navier replaced u_R with its first order Taylor approximation $R^{-1}x_i x_j \partial_i u_j(\mathbf{r}_\alpha)$. In this tensor notation, x_i denotes the ith coordinate of $\mathbf{R}$, ∂_i partial derivation with respect to the ith coordinate of $\mathbf{r}$; summation over repeated indices is understood. With a similar substitution for w_R we have

$$\phi u_R w_R \simeq R^{-2}\phi x_i x_j x_k x_l \partial_i u_j \partial_k w_l. \tag{8}$$

Navier then replaced the sum over β by a volume integral weighted by the number N of molecules per unit volume. Since his calculation of the moment M was limited to first order in $\mathbf{u}$, he could neglect the variation of N caused by the deformation. The integral is easily effected by separating the integration over R and that over angular variables. It yields

$$\sum_{\beta}\phi u_R w_R = 2Nk(\partial_i u_j \partial_i w_j + \partial_i u_i \partial_j w_j + \partial_i u_j \partial_j w_i), \tag{9}$$

with

$$k = \frac{2\pi}{15} \int \phi(R) R^4 dR. \quad (10)$$

In order to obtain the total molecular moment M, Navier then performed the summation over α, which he also replaced with an integration. The result can be put in the form

$$M = \int \sigma_{ij} \partial_i w_j d\tau, \quad (11)$$

with

$$\sigma_{ij} = -kN^2(\delta_{ij}\partial_k u_k + \partial_i u_j + \partial_j u_i), \quad (12)$$

where δ_{ij} is the unit tensor.

In analogy with Lagrange's reasoning, Navier then integrated by parts to get

$$M = \oint \sigma_{ij} w_j dS_i - \int (\partial_i \sigma_{ij}) w_j d\tau. \quad (13)$$

The deformed solid is in equilibrium only if this moment is balanced by the moment of the applied forces. Navier considered an internal force density **f** (such as gravity) and an *oblique* surface force **P**. For virtual displacements that occur entirely within the body, the balance requires that $f_j - \partial_i \sigma_{ij} = 0$ or, in vector notation,

$$\mathbf{f} + kN^2[\Delta \mathbf{u} + 2\nabla(\nabla \cdot u)] = \mathbf{0}. \quad (14)$$

The second term represents the restoring force that acts on a volume element of the deformed solid. According to d'Alembert's principle, the equations of motion of the elastic solid are simply obtained by equating this force to the acceleration times the mass of this element. For virtual displacements at the surface of the body, the balance of the surface term of Eq. (13) with the moment $\int -\mathbf{P} \cdot \mathbf{v} dS$ of the oblique external pressure gives the boundary condition

$$\sigma_{ij} dS_j = P_i dS. \quad (15)$$

Navier of course used Cartesian notation, which makes his calculation appear to be quite forbidding to modern eyes. However, the basic structure of his reasoning was as simple as the rendering above suggests. The only step in the tensor calculation that may suggest more than Navier in fact had in mind is the introduction of the tensor σ_{ij} to prepare the partial integration of Eq. (11). Navier treated each term of this equation separately. But he did write the Cartesian version of Eq. (15) as follows:

$$X' = \varepsilon \left[\cos l \left(3\frac{dx'}{da'} + \frac{dy'}{db'} + \frac{dz'}{dc'}\right) + \cos m \left(\frac{dx'}{db'} + \frac{dy'}{da'}\right) + \cos n \left(\frac{dx'}{dc'} + \frac{dz'}{da'}\right)\right],$$
$$Y' = \varepsilon \left[\cos l \left(\frac{dx'}{db'} + \frac{dy'}{da'}\right) + \cos m \left(\frac{dx'}{da'} + 3\frac{dy'}{db'} + \frac{dz'}{dc'}\right) + \cos n \left(\frac{dy'}{dc'} + \frac{dz'}{db'}\right)\right],$$
$$Z' = \varepsilon \left[\cos l \left(\frac{dx'}{dc'} + \frac{dz'}{da'}\right) + \cos m \left(\frac{dy'}{dc'} + \frac{dz'}{db'}\right) + \cos n \left(\frac{dx'}{da'} + \frac{dy'}{db'} + 3\frac{dz'}{dc'}\right)\right], \quad (16)$$

which gives the formal structure of the response of the solid to an oblique external pressure.[33]

A new hydrodynamic equation

Soon after he presented this memoir on elasticity, Navier thought of adapting his new molecular technique to fluid mechanics. For a fluid in equilibrium, he assumed a force $f(R)$ that acts between every two molecules and that decreases rapidly with the distance R (an attraction being reckoned positively). Calling $\mathbf{w}(\mathbf{r})$ a virtual displacement of the particles of the fluid, and using the notation of the previous section, the corresponding moment is

$$M = -\frac{1}{2}\sum_{\alpha\beta} f(R)w_R \simeq -\frac{1}{2}\sum_{\alpha\beta} R^{-1} f(R)x_i x_j \partial_i w_j. \tag{17}$$

Replacing the sums with integrals, and separating angular variables in the first integration yields

$$M = -\int N^2 \lambda \nabla \cdot \mathbf{w} d\tau, \tag{18}$$

with

$$\lambda = \frac{2\pi}{3} \int_0^\infty R^3 f(R) dR. \tag{19}$$

For an incompressible fluid, Navier takes the density N to be nearly constant (he gives it the value *one*), but makes the parameter λ vary from one particle of the fluid to another. This odd assumption (it seems incompatible with the expression of λ), of which more will be said later, brought him back to Lagrange's expression (5) for the quantity whose vanishing defines equilibrium. He thereby obtained the same conditions of equilibrium as Lagrange's, so that $-N^2\lambda$ plays the role of internal pressure. In Navier's words, λ "measures the resistance opposed to the pressure that tends to brings the fluid parts closer to each other."[34]

Navier then turned to the case of a fluid moving with a velocity $\mathbf{v}(\mathbf{r})$, and he assumed that "the repulsive actions of the molecules [are] increased or diminished by a quantity proportional to the velocity with which the distance of the molecules decrease[s] or increase[s]." Calling $\psi(R)$ the proportionality coefficient, this intuition implies a new contribution of the form

[33] Navier (ref. 32), 390.

[34] Navier, "Mémoire sur les lois du mouvement des fluides" [read on 18 Mar and 16 Dec 1822], *MAS* 6 (1823) [pub. 1827], 389–440, on 395. Cf. Saint-Venant (ref. 28), lxii-lxiv; Dugas (ref. 1), 393–401; Grattan-Guinness (ref. 15), 986–992 (with a questionable chronology); Belhoste, "Navier, Saint-Venant et la création de la mécanique des fluides visqueux," *Annales des Ponts et Chaussées*, 82 (1997), 4–9.

$$M' = -\frac{1}{2}\sum_{\alpha\beta} \psi v_R w_R \tag{20}$$

to the moment of molecular forces. By analogy with the corresponding formula (8) for elastic solids, this leads to an additional force $\varepsilon\Delta\mathbf{v}$ in the equation of motion of an incompressible fluid, with

$$\varepsilon = \frac{2\pi}{15}\int N^2\psi(R)R^4 dR. \tag{21}$$

The new equation of motion reads

$$\rho\left[\frac{\partial\mathbf{v}}{\partial t} + (\mathbf{v}\cdot\nabla)\mathbf{v}\right] = \mathbf{f} - \nabla P + \varepsilon\Delta\mathbf{v}, \tag{22}$$

It is now known as the "Navier-Stokes equation."[35]

Boundary conditions

Navier provided this equation in a memoir that was read on 18 March 1822 at the Academy of Sciences and published in summary form in the *Annales de Chimie et de Physique*. There he assumed, as Girard had, that the velocity **v** vanished at the wall, in which case the balance of moments gives no additional boundary condition.[36] The resulting calculation of uniform flow in a pipe of rectangular section leads to a discharge proportional to the pressure gradient, as Girard had observed for "linear motions" (that is, laminar flow). Another consequence of this calculation is that the average flow velocity in a square tube should be proportional to the square of the tube perimeter – as it is according to Poiseuille's later law for circular tubes. Navier (wrongly) believed that this result agreed with the departure that Girard had observed in the case of circular tubes from the expected proportionality to the perimeter.[37]

However, Navier was aware of a contradiction with another of Girard's results, namely the difference between the discharge in glass and in copper tubes. He now faced the following dilemma: either he maintained the boundary condition $\mathbf{v} = \mathbf{0}$ and thus contradicted Girard's experimental results, or he gave up this condition and contradicted the most essential assumption of Girard's theory. As he indicated toward the end of his memoir, he preferred the second alternative. On 16 December 1822, he read a second memoir in which he proposed a new boundary condition based on an evaluation of the moment of forces between the molecules of the fluid and those of the wall. The form of this moment is

$$M'' = E\int \mathbf{v}\cdot\mathbf{w}\, dS, \tag{23}$$

---

[35] Navier (ref. 34), 414.

[36] However, the tangential stress must vanish at the *free* surface of the fluid. Navier did not use the method of moments in this first memoir.

[37] Navier "Sur les lois du mouvement des fluides, en ayant égart à l'adhésion des molécules" [read on 18 March 1822], *ACP*, 19 (1821) [in fact 1822], 244–260, on 259.

where E is the relevant molecular constant. This is to be cancelled by the surface term

$$\oint \varepsilon(\partial_i v_j + \partial_j v_i) w_i dS_j \tag{24}$$

of the moment M' for any displacement $\mathbf{w}$ that is parallel to the surface. The resulting condition is

$$E\mathbf{v} + \varepsilon \partial_{\perp} \mathbf{v}_{//} = \mathbf{0}, \tag{25}$$

where $\partial_{\perp}$ is the normal derivative, and $\mathbf{v}_{//}$ is the component of the fluid velocity parallel to the surface.[38]

With this new boundary condition, Navier redid his calculation of uniform square-pipe flow, and also treated the circular pipe by Fourier series. Taking the limit of narrow tubes, he found the average flow velocity to be proportional to the surface coefficient E, to the pressure gradient, and to the diameter of the tube, in rough agreement with Girard's data. Note that he no longer believed Girard's data to support a quadratic dependence of velocity on diameter. In fact, Girard's theoretical formula assumed a linear dependence, and his experimental results indicated an even slower increase with diameter. Having no reason to distrust Girard's experiments on the differences between glass and copper tubes, Navier built the old idea of fluid – solid slip into the theory of a viscous fluid.[39]

A useless equation

For large pipes, Navier's theory no longer implies a significant surface effect, but still makes the loss of head proportional to the average fluid velocity. Since Navier knew that in practical engineering cases the loss of head was nearly quadratic, he did not think it worthwhile to take the large-pipe limit of his formulas. He stated only that in this case the flow obviously did not have the (recti)linearity assumed in his calculations. Navier never did return to his theory of fluid motion. In the hydraulic section of his course at the Ponts et Chaussées, he mentioned only his formula for capillary tubes, which agreed with "M. Girard's very curious experiments." The theory on which this formula is based, he immediately noted, "cannot suit the ordinary cases of application. Since the more complicated motion that the fluid takes in these cases has not been submitted to calculation, the results of experience are our only guide."[40]

The two commissioners for Navier's first memoir, Poisson and Joseph Fourier, and the three commissioners for his second memoir, Girard, Fourier, and Charles Dupin, never wrote their reports, perhaps because Navier was elected to the Academy in 1824,

---

[38] Navier (ref. 34). In Cauchy's stress language, the condition means that the tangential stress is parallel and proportional to the sliding velocity.

[39] Ibid., 432–440.

[40] Navier, ibid., 439; *Résumé des leçons données à l'Ecole des Ponts et Chaussées sur l'application de la mécanique à l'établissement des constructions et des machines. Deuxième partie, Leçons sur le mouvement et la résistance des fluides, la conduite et la distribution des eaux* (Paris, 1838), 88–89.

well before the publication of his second memoir. However, the probabilist mathematician Antoine Cournot wrote a review for Férussac's *Bulletin* that may reflect the general impression that Navier's memoir made at the French Academy. Being Laplace's admirer and Poisson's protégé, Cournot welcomed Navier's theory as a new contribution to the now well-established molecular physics. He nonetheless suspected that there might be a few inconsistencies in Navier's basic assumptions.[41]

In his derivation of hydrostatic pressure, Cournot noted, Navier assumed incompressibility, which seemed incompatible with the molecular interpretation of pressure as a reaction to a closer packing of the molecules. In fact, according to Navier's formula (19) the coefficient λ should be a constant, which excludes a variable pressure if the density N is also a constant. Yet upon closer inspection Navier's procedure is more coherent than Cournot perceived. Here and elsewhere, Navier's formulas did not quite reflect his basic intuition. In his mind the distance R in the force function $f(R)$ did not represent the distance of the molecules in the actual state of the body, but their distance before the compression of the fluid. For a real substance, which can only be nearly, but not absolutely, incompressible, the difference between those two distances was extremely small but finite, so that Navier's f function could vary with the local state of the fluid.[42]

Another worry of Cournot's was that Navier admitted the same equations of equilibrium of a fluid as Euler and Lagrange and yet obtained different equations of motion, against d'Alembert's principle. "The matter," Cournot deplored, "does not seem to be free from obscurity." We would today solve this apparent paradox by noting that dissipative forces, such as those expressing fluid viscosity or the viscous friction between two solids, are to be treated, in the application of d'Alembert's principle, as additional, motion-dependent forces that are impressed on the system. At the molecular level, where Navier worked, the difficulty is that his calculation seems to rely on velocity-dependent forces, which is at odds with a strictly Laplacian viewpoint, which conceived only of distance dependence.[43]

Yet even here Navier's formulas do not directly reflect his deeper viewpoints. A close reading of his text shows what he intended – namely, that the distribution of intermolecular distances will be modified by the macroscopic motion of the fluid: "If the fluid is moving," Navier writes, "which implies, in general that the neighboring molecules come closer to or further from one another, it seems natural to assume that the [intermolecular] repulsions are modified by this circumstance." This occurs in the Laplacian conception of fluids, because the trajectory of an individual molecule will, as it were, undulate around the path that is overall imposed by the macroscopic motion. Consequently, at any given instant the molecules of a fluid will be in positions that deviate slightly from an equilibrium configuration, which will itself constantly change over time. The molecular

---

[41] A. Cournot, review of Navier's memoir on fluid motion, *Bulletin des sciences mathématiques*, 10 (1828), 11–14. The review is only signed A.C., but comparison with other articles in the same journal makes clear that it was not Augustin Cauchy, the only member of the redaction committee with the same initials.

[42] Ibid., 11–12; Navier (ref. 34), 392: "La force répulsive qui s'établit entre les deux molécules dépend de la situation du point M [lieu de la première molécule], puisqu'elle doit balancer la pression, qui peut varier dans les diverses parties du fluide."

[43] Cournot (ref. 41), 12.

force function in Navier's moment formula accordingly does not refer to the actual distance of the molecules, but rather to the distance that they have in the nearest equilibrium configuration; and the difference between these two distances obviously depends on the macroscopically impressed motion. This is how the fluid velocity enters the expression of Navier's molecular forces, even though the actual forces depend only on the proper distances between the molecules.[44]

Unfortunately, Navier never provided a detailed justification of his procedure – so that none of his successors (except Saint-Venant) could make sense of his calculation. In Cournot's eyes, the premisses of Navier's equation seemed arbitrary, and its applicability to concrete problems was difficult to judge:[45]

> M. Navier himself only gives his starting principle as a hypothesis that can be solely verified by experiment. However, if the ordinary formulas of hydrodynamics resist analysis so strongly, what should we expect from new, far more complicated formulas? The author can only arrive at numerical applications after a large number of simplifications and particular suppositions. The applications no doubt show great analytical skill; but can we judge a physical theory and the truth of a principle after accumulating so many approximations? In one word, will the new theory of M. Navier make the science of the distribution and expense of waters less empirical? I do not feel able to answer such a question. I can only recommend the reading of this memoir to all who are interested in this kind of application.

3. Cauchy: Stress and strain

The stress system

Like Navier, Augustin Cauchy was an "X+Ponts" with superior mathematical training and some engineering experience. However, his poor health and mathematical genius soon confined him to purely Academic activities, to the great profit of French mathematics. In 1822, his study of Navier's memoir on elastic plates led him to a new approach which still constitutes the basis of elasticity theory. If we are to trust his own account, what triggered Cauchy's main inspiration was Navier's appeal to two kinds of restoring forces produced by extension and flexion.[46]

The second kind of force could be avoided, Cauchy surmised, if the first kind were no longer supposed to be perpendicular to the sections on which it acted. Following this perception, he then imitated Eulerian hydrodynamics and reduced all elastic actions to pressures acting on the surface of portions of the body. The only difference was the non-normality of pressure. Previous students of elasticity, in particular Coulomb and Young,

---

[44] Navier (ref. 34), 390.

[45] Cournot (ref. 41), 13–14.

[46] A. Cauchy, "Recherches sur l'équilibre et le mouvement des corps solides ou fluides, élastiques ou non élastiques" [extract of a memoir read on 30 Sep 1822], *BSP* (1823), 9–13. Cf. the excellent B. Belhoste, *Augustin-Louis Cauchy: A biography* (New York, 1991), 93–102; also Grattan–Guinness (ref. 15), 1005–1013.

had already considered tangential pressures (modern shearing stresses) in specific problems such as the rupture of beams. And, in his memoir of May 1821 on a molecular derivation of the general equations of elasticity, Navier had introduced oblique external pressures and boundary conditions that would entail the Cauchy stress-system. Whether or not Cauchy relied on such anticipations, he was the first to base the theory of elasticity on a general definition of internal stresses.[47]

As in hydrodynamics, Cauchy introduced the pressures (or tensions) that act on a volume element by recourse to the forces that would act on its surface after an imaginary solidification of the element. For a unit surface element normal to the jth axis, call σ_{ij} the ith component of the pressure acting on the matter situated on the side of the element toward which the jth axis is pointing. Note that with this convention, adopted by most of Cauchy's followers, a tension is reckoned positively. Cauchy proved three basic theorems in a manner that is still used in modern texts on elasticity. The first theorem stipulates that the pressure on an arbitrary surface element $d\mathbf{S}$ is given by the sum $\sigma_{ij} dS_j$. In modern words, the stress system σ_{ij} is a tensor of second rank. This results from the fact that the resultant of the pressures acting on the pyramidal volume element $0 < \alpha x + \beta y + \gamma z < \varepsilon$ would be of second order in ε and therefore could not be balanced by the resultant of a volume force (which is of third order) if the theorem were not true. Cauchy's second theorem states the symmetry of the stress system: $\sigma_{ij} = \sigma_{ji}$. It results from the fact that the resultant of the pressure torques on a cubic element of the solid would otherwise be of third order and therefore could not be balanced by the torque of any volume force, which is of fourth order. Thirdly and most obviously, the resultant of the pressures acting on a (cubic) volume element is $\partial_j \sigma_{ij}$ per unit volume.[48]

Strain and motion

As Cauchy knew from the theory of quadratic forms which he had recently applied to inertial moments, the symmetry of the pressure system implies the existence of three principal axes for which the pressures become normal (in modern terms, the stress tensor is then diagonal). Cauchy used this property to relate the pressure system to the local deformations of the system. If $\mathbf{u}(\mathbf{r})$ is the displacement of a solid particle at the point of space $\mathbf{r}$, Cauchy showed, then the first-order variation of the distance between two points whose coordinate differences have the very small values dx_i is given by $dx_i dx_j \partial_i u_j$. In modern terms, this quadratic form is associated to the symmetric tensor

[47] Cauchy, ibid.; "Sur la pression ou la tension dans les corps solides," *EM*, 2 (1827), 42–57. Cf. Truesdell, "The creation and unfolding of the concept of stress," in *Essays in the history of mechanics* (Berlin, 1968), 184–238; Dahan (ref. 29), chap. 9. In his memoir on elastic plates (ref. 30, p. 9), Navier noted that in general the pressures would not be parallel to the faces of the element. Fresnel's theory of light was perhaps another source of Cauchy's inspiration: cf. Belhoste (ref. 46), 94–95. The stress–strain terminology is William Rankine's. Cauchy and contemporary French writers used the words *pression/tension* and *condensation/dilatation*.

[48] Cauchy (ref. 47); "Sur les relations qui existent, dans l'état d'équilibre d'un corps solide ou fluide, entre les pressions ou tensions et les forces accélératrices," *EM*, 2 (1827), 108–111.

$e_{ij} = \partial_i u_j + \partial_j u_i$. This tensor has three principal axes, which means that the local deformation is reducible to three dilations or contractions along three orthogonal axes.[49]

Cauchy then argued that for an isotropic body the principal axes of the tensors σ_{ij} and e_{ij} were necessarily identical. He also assumed that the pressures along these axes were in the same proportions as the dilations. This implies that the two tensors be proportional. Lastly, Cauchy assumed that the proportionality coefficient did not itself depend on the deformation, which is a generalization of Hookes' law. He thus obtained an equation of equilibrium similar to Navier's Eq. (14), but without the factor 2 in the $\nabla(\nabla \cdot \mathbf{u})$ term. The boundary conditions immediately result from the balance of internal and external pressures.[50]

The perfectly inelastic body

In a last section, Cauchy considered the case of a "non-elastic body" defined as a body for which the stresses at a given instant only depend on the change of form experienced by the body in a very small time interval preceding this instant. He found it natural to assume that the stress tensor was proportional to the tensor representing the velocity of deformation (again reasoning in respect to principal axes). For an incompressible body, the resulting equation of motion is the one that Navier had given for viscous fluids, save for the pressure term. Interestingly, Cauchy made no mention of Navier's result. He did not even mention that his equation could apply to real fluids. Instead he noted that for very slow motion, the linearized equation of motion was identical to Fourier's equation for the motion of heat, and announced "a remarkable analogy between the propagation of caloric and the propagation of vibrations in a body entirely deprived of elasticity."[51]

Final foundations?

Cauchy announced these remarkable results on 30 September 1822, and published them in summary form the following year. Yet he waited six more years before the complete publication in his own, personal journal, the *Exercices de mathématiques*. The reason for this delay may have been the courtesy of waiting for Navier's memoir of 1821 to be published. In the final version of his theory, Cauchy proposed the more general, two-constant relation

$$\sigma_{ij} = K'(\partial_i u_j + \partial_j u_i) + K''\delta_{ij}\partial_k u_k \tag{26}$$

---

[49] Cauchy (ref. 46); "Sur la condensation et la dilatation des corps solides," *EM*, 2 (1827), 60–69.

[50] Cauchy (ref. 46); "Sur les équations qui expriment les conditions d'équilibre ou les lois du mouvement d'un corps solide, élastique ou non élastique," *EM*, 3 (1828), 160–187. Cauchy introduced the word "isotrope" in 1839–1840, for example in "Mémoire sur les deux espèces d'ondes planes qui peuvent se propager dans un système isotrope de points matériels," *CR* 10 (1840), 905–918.

[51] Cauchy, ref. 46; ref. 50, par. 3.

between stress and deformation. This allowed him to retrieve Navier's equation of equilibrium as the particular case for which $K' = K''$. The two-constant theory is the one now accepted for isotropic elasticity.[52]

Cauchy's memoirs on elasticity were written with incomparable elegance and rigor. For this reason, and also because of their strikingly modern appearance, they have often been regarded as the first and final foundation of this part of physics. Cauchy's contemporaries thought differently. In the years following Cauchy's publication, theorists of elasticity were not satisfied with this purely macroscopic and continuist approach, even though they all did adopt Cauchy's stress. In their eyes, the true foundation of elasticity had to be molecular, as Laplace had indicated in his grand unification of physics.[53]

It would also be wrong to regard Cauchy's stress-strain approach as an indication that he himself supported a continuist view of matter. For theological reasons Cauchy was a finitist in mathematics and an atomist in physics. That he first derived the equations of elasticity without reference to the molecular level proves only that he possessed the geometrical and albebraic skills that made this route natural and easy. He himself provided the most complete and rigorous molecular theory of elasticity, even before his first theory of elasticity was published. However, his competitor Poisson undoubtedly was the most aggressive supporter of the molecular approach.[54]

4. Poisson: The rigors of discontinuity

Laplacian motivations

Siméon-Denis Poisson was an early Polytechnician, with an unusual capacity for labyrinthine mathematical analysis and a deep interest in fundamental physics. Unlike Navier and Cauchy, he did not have engineering training and experience, for he settled at the Polytechnique as a *répétiteur* and then as a professor. His interest in elasticity came from his enthusiastic embrace of Laplace's molecular program. His 1814 theory of elastic plates already was molecular. Presumably stimulated by Navier's memoirs of 1820–21, he returned to this subject in the late 1820s. His memoir read on 14 April 1828 contains his famous plea for a *mécanique physique*:[55]

---

[52] Cauchy, ref. 50.

[53] Cf. Saint-Venant (ref. 28), cliv-clv.

[54] Cauchy pleaded for discrete point-molecules in his *Sept leçons de physique générale*, delivered in 1833 during his exile in Torino and published by F. Moigno (Paris, 1868): extended molecules would be indefinitely divisible, against the principle that "only God is infinite, everything is finite except him" (ibid., 36–37). However, Cauchy never used molecular considerations in publications anterior to his molecular theory of elasticity (I thank Bruno Belhoste for this information).

[55] Poisson, "Mémoire sur l'équilibre et le mouvement des corps élastiques" [read on 14 Apr 1828], *MAS*, 8 (1829), 357–570, on 361. Cf. D.H. Arnold, "The *mécanique physique* of Siméon Denis Poisson: The evolution and isolation in France of his approach to physical theory," *Archive for history of exact sciences*, 28 (1983), 243–367; 29 (1983), 37–94; Grattan-Guinness (ref. 15), 1015–1025. Dahan (ref. 29, chap. 10.

> It would be desirable that geometers reconsider the main questions of mechanics under this physical point of view which better agrees with nature. In order to discover the general laws of equilibrium and motion, one had to treat these questions in a quite abstract manner; in this kind of generality and abstraction, Lagrange went as far as can be conceived when he replaced the physical connections of bodies with equations between the coordinates of their various points: this is what *analytical mechanics* is about; but next to this admirable conception, one could now erect a *physical mechanics*, whose unique principle would be to reduce everything to molecular actions that transmit from one point to another the given action of forces and mediate their equilibrium.

Poisson's memoir of 1828 can to some extent be seen as a reworking of Navier's memoir of 1821 on the molecular derivation of the general equations of elasticity. Both memoirs aimed at a derivation of the general equations and boundary conditions of elasticity by superposition of short-range molecular actions. However, there were significant differences in their assumptions and methods. Whereas the only molecular forces in Navier's calculations were those produced by the deformation of the solid, Poisson retained the total force $f(R)$ between two molecules. Also, Poisson avoided Navier's method of moments, and instead directly summed the molecular forces acting on a given molecule.

Cauchy worked on a similar molecular theory in the same period. Competition was so intense that Cauchy thought it necessary to deposit a draft of his calculation as a *pli cacheté* at the Academy, and Poisson to read his memoir in a still unripe form. Cauchy's assumptions and methods were essentially the same as Poisson's, which should not surprise us: they were both following Laplacian precepts without Navier's personal touch. Yet Cauchy's execution surpassed Poisson's in adherence to careful and consistent calculation, as well as in compactness. In the following, Cauchy's version of the theory is given, together with indications of Poisson's differences from it.[56]

The Cauchy-Poisson theory

Calling R the distance between two molecules α and β, x_i the coordinate i of the vector joining them, and using accents for the same quantities after the macroscopic deformation $\mathbf{u}(\mathbf{r})$, we have

$$x_i' \simeq x_i + x_j \partial_j u_i + \tfrac{1}{2} x_j x_k \partial_j \partial_k u_i \tag{27}$$

and

$$R' \simeq R + u_R \simeq R + R^{-1} x_i x_j \partial_i u_j + \tfrac{1}{2} R^{-1} x_i x_j x_k \partial_i \partial_j u_k \tag{28}$$

to first order in u and second order in R (the indices α and β are omitted to simplify notation). The force $\mathbf{F}$ acting on the molecule α is then given by the sum

$$F_i = \sum_\beta x_i' R'^{-1} f(R') \simeq \sum_\beta x_i' \left(f R^{-1} + u_R \frac{d f R^{-1}}{dR} \right). \tag{29}$$

[56] Cf. Belhoste (ref. 46), 99–100.

Cauchy retained only the parity-invariant terms, for which the number of x factors is even:

$$F_i = \lambda_{jk}\partial_j\partial_k u_i + \mu_{ijkl}\partial_j\partial_k u_l, \tag{30}$$

with

$$\lambda_{ij} = \sum_\beta \frac{1}{2} f R^{-1} x_i x_j \tag{31}$$

and

$$\mu_{ijkl} = \sum_\beta \frac{1}{2} R^{-1} \frac{d f R^{-1}}{dR} x_i x_j x_k x_l. \tag{32}$$

Cauchy then reduced the number of coefficients λ_{ij} and μ_{ijkl} by gradually increasing the symmetry of the distribution of the β molecules around the molecule α. For complete isotropy, he found

$$\lambda_{ij} = \lambda\delta_{ij} \tag{33}$$

and

$$\mu_{ijkl} = \mu(\delta_{ij}\delta_{kl} + \delta_{ik}\delta_{jl} + \delta_{il}\delta_{jk}), \tag{34}$$

as we would expect from modern tensor calculus. Poisson obtained the same relations but only after assuming that the number of β molecules in the sphere of action of the molecule α was very large so that the sum over the directions of the molecules β could be replaced with an angular integral. Cauchy emphasized, presumably contra-Poisson, that no such assumption was necessary. The resulting expression for the elastic force acting on a molecule of an isotropic solid is[57]

$$\mathbf{F} = (\lambda + \mu)\Delta\mathbf{u} + 2\mu\nabla(\nabla\cdot\mathbf{u}). \tag{35}$$

The direct force calculation does not provide the boundary conditions. For this purpose, both Poisson and Cauchy derived the internal pressure system of the deformed solid. In the molecular picture they defined the pressure acting on a surface element $d\mathbf{S}$ as the resultant of the forces between all the molecules on one side of the plane of the element and the molecules on the other side belonging to a straight cylinder based on this element. As Saint-Venant later noted, a more adequate definition makes the pressure the resultant of the forces between any two molecules such that the line joining these two molecules crosses the surface element.[58]

---

[57] Cauchy, "Sur l'équilibre et le mouvement d'un système de points matériels sollicités par des forces d'attraction ou de répulsion naturelle," *EM*, 4 (1829), 129–139; Poisson (ref. 55). Cf. Saint-Venant (ref. 28), clv-clxj; Dahan (ref. 29), chap. 11. The corresponding force density has an additional factor *N*. Poisson did not do the direct force calculation, but derived the elastic force density from the internal pressures.

[58] Cauchy, "De la pression ou tension dans un système de points matériels," *EM*, 3 (1828), 213–236. Cf. Saint-Venant, lectures 21 and 22 of Moigno, *Leçons de mécanique analytique, rédigées principalement d'après les méthodes d'Augustin Cauchy et étendues aux travaux les plus récents* (Paris, 1868), 617–620.

First consider the state of the body before deformation. The contribution of a given molecular pair $\alpha\beta$ to the component P_i of the pressure is $x_i R^{-1} f(R)$ in the same notation as before. In order to perform the double sum over relevant pairs, Poisson and Cauchy counted the number of ways in which a given value of the intermolecular vector x_i can be achieved. This is given by the number of molecules $N x_i dS_i$ contained in the oblique cylinder based on the element dS_i and generated by the vector x_i. Hence, the pressure P_i is given by the sum of $N x_i x_j dS_j R^{-1} f(R)$ over all molecules β such that $x_i dS_i > 0$ (the molecule α being kept at the center of the element $d\mathbf{S}$). It has the form $\sigma_{ij} dS_j$ required by Cauchy's first stress theorem. Through a symmetry with respect to the plane of the element, σ_{ij} is unchanged but the restriction on the β sum becomes $x_i dS_i < 0$. This remark leads to the simpler formula

$$\sigma_{ij} = \frac{N}{2} \sum_{\beta} x_i x_j R^{-1} f(R), \tag{36}$$

with no restriction on the β sum.

The effect of a deformation $\mathbf{u}$ on the stress system is easily obtained from the previous formula by replacing x_i and R with their accented counterparts (27) and (28), and changing N into $N(1 - \nabla \cdot \mathbf{u})$ (the first-order volume compression being $\nabla \cdot \mathbf{u}$). To first order in u and first order in R, this leads to

$$\sigma_{ij} = \frac{N}{2}(1 - \nabla \cdot \mathbf{u}) \sum_{\beta} (x_i + x_k \partial_k u_i)(x_j + x_l \partial_l u_j) \left(R^{-1} f + R^{-1} x_m x_n \partial_m u_n \frac{d f R^{-1}}{dR} \right), \tag{37}$$

or

$$\sigma_{ij} = N(\lambda_{ij} - \lambda_{ij} \partial_k u_k + \lambda_{ik} \partial_k u_j + \lambda_{jk} \partial_k u_i + \mu_{ijkl} \partial_k u_l). \tag{38}$$

In the isotropic case, this reduces to

$$\sigma_{ij} = N[\lambda \delta_{ij} + (\mu - \lambda)\delta_{ij} \partial_k u_k + (\lambda + \mu)(\partial_i u_j + \partial_j u_i)]. \tag{39}$$

Cauchy thus retrieved the formulas he had already obtained by purely macroscopic reasoning, save for the pressure λ in the original state. In contrast, Poisson found

$$\sigma'_{ij} = N[\lambda \delta_{ij} + \mu \delta_{ij} \partial_k u_k + (\lambda + \mu)\partial_i u_j + \mu \partial_j u_i], \tag{40}$$

because his pressure system referred to the orientation and extension of the surface elements *before the deformation*, which implies the disappearance of the terms in Eq. (37) corresponding to the change of the product $N x_i$. This convention leads to an asymmetrical pressure system, and requires a more difficult proof of the balance of torques than Cauchy's. It also leads to complexities in the physical interpretation of the pressures and in the boundary conditions. Poisson overlooked this difficulty. Fortunately for him, if the original state of the body is the natural state for which the internal pressure vanishes, then the coefficient λ must vanish. In that case Poisson's and Cauchy's stress systems become identical and lead to Navier's one-constant theory.[59]

---

[59] Cauchy (ref. 58); Poisson (ref. 55). Cf. Saint-Venant (ref. 28), clix-clx. Some commentators have seen a contradiction between Cauchy's molecular force calculation and his molecular stress,

Sums versus integrals

Poisson and Cauchy both investigated the limiting case of a continuous medium, in which the sums (31) and (32) expressing the coefficients λ_{ij} and μ_{ijkl} can be rigorously replaced with integrals. As Cauchy (but not Poisson) saw, isotropy follows without further assumption, and the coefficients λ and μ are given by

$$\lambda = \frac{2\pi}{3} N \int_0^\infty R^3 f dR, \tag{41}$$

and

$$\mu = \frac{2\pi}{15} N \int_0^\infty R^5 \frac{d f R^{-1}}{dR} dR. \tag{42}$$

Integrating by parts the latter expression yields the relation

$$\lambda + \mu = \lim_{R \to 0} R^4 f(R). \tag{43}$$

Poisson and Cauchy both assumed the limit to be zero. Then the medium loses its rigidity since the transverse pressures disappear. As Cauchy further observed, the continuous limit can be taken directly on the expression (36) of the unperturbed stress system to yield the proportionality

$$\sigma_{ij} \propto N^2 \delta_{ij}, \tag{44}$$

which means that the body is an elastic fluid whose pressure varies as the square of the density. This result did not require consideration of the limiting value of $R^4 f$.[60]

Poisson reasoned somewhat differently. With his special convention for the stress system, the vanishing of $\lambda + \mu$ does not in itself imply the lack of transverse pressures. However, with the additional assumption that the original state of the medium is a pressure-less natural state, λ and μ both vanish, so that the medium is entirely devoid of elasticity. Poisson used this conclusion to dismiss Navier's theory and to denounce the general impossibility of substituting integrals for molecular sums. Poisson claimed to be the first to have offered a genuinely molecular theory of elasticity, and referred to Navier only to declare that his assumptions should have led him to zero elasticity.[61]

---

arguing that the former leads to a bi-constant theory and the latter to a mono-constant theory. In fact there is no such contradiction, because in the first calculation Cauchy did not require that the body should originally be in its natural, pressure-less state (and could not do so without knowing the pressures!).

[60] Cauchy (ref. 58), 266.

[61] Poisson (ref. 55), 397–398, 403–404.

Navier's defense

There followed a long, bitter polemic in the *Annales de Chimie et de Physique*. Navier first recalled that Poisson and Laplace had had no qualms replacing sums with integrals in their past works. The newer emphasis on a supposed rigor could only betray a desire to belittle Navier's own achievement. It was he, Navier, who in 1821 "conceived the idea of a new question, one necessary to the computation of numerous phenomena that interest artists and physicists." It was he, who "recognized the principle on which this solution had to rest." But this principle was not what Poisson thought it should be: it was the assumption that the variation of intermolecular forces during a deformation of a solid body depended linearly on the variation of molecular distances, not the assumption that molecules interacted through central forces only. Hence Navier thought that his theory was immune to Poisson's arguments on sums versus integrals.[62]

For good measure, Navier critiqued Poisson's own approach. In his view, Poisson had failed to provide a description of the force function $f(R)$ that would account for stability and elastic behavior. For example, he had required the vanishing of the sum $\Sigma Rf(R)$ without exhibiting a choice of f that could meet this condition. If Poisson were willing to presuppose this much in respect to f, Navier went on, then why not assume a non-zero value of the limit of $R^4 f$ when R reaches zero? This would avoid the fatal $\lambda + \mu = 0$, and allow the use of integrals instead of sums.[63]

From this extract of Navier's defense, one may judge that he was hesitating between two strategies. The first option was to deny the general applicability of the Laplacian doctrine of central forces, and to deal only with forces that arise out of the disruption of an equilibrium of unknown nature. This option agreed with Navier's positivist sympathies and with the style of applied physics that he embodied at the Ponts et Chaussées. It could accommodate later, unforeseen changes in molecular theory.[64]

The second option was to admit the general Laplacian reduction to central forces and to show that appropriate results could nevertheless be obtained by substituting integrals for sums. Here Navier erred, because a Laplacian continuum, that is, a continuous set of material points subjected to central forces acting in pairs, cannot have rigidity. First, the escape that the limit of $R^4 f$ might not itself vanish is unavailable, despite Navier's assertion, because that would imply the divergence of the integral $\int R^3 f d\mathrm{R}$ (the remark is mine). Second, Cauchy's proof of fluidity through relation (44) does not, despite what

[62] Navier, "Note relative à l'article...[Poisson, ref. 55]," *ACP*, 38 (1828), 304–314, on 312; "Remarque...[about Poisson's reply]," *ACP*, 39 (1828), 145–151; Navier to Arago, with a closing note by Arago, *ACP*, 40 (1829), 99–107; "Note relative à la question de l'équilibre et du mouvement des corps solides élastiques," *BSM*, 11 (1829), 243–253; Poisson, "Réponse...[to Navier's note]," *ACP*, 38 (1828), 435–440; Poisson to Arago, *ACP*, 39 (1828), 204–211. Cf. Saint-Venant (ref. 28), clxi-clxvii; Arnold (ref. 55), parts 6, 8.

[63] Navier (ref. 62), *ACP*, 40 (1829), 99–107; and *BSM*, 11 (1829), 243–253. One point of the polemic was Navier's occasional assumption that in the natural state of the body the forces between any two molecules vanished. I leave this question aside, since Navier himself did not regard the assumption as necessary to his derivations.

[64] Physicists today regard the existence of the equilibrium state of a solid as a quantum property but they nevertheless allow a classical treatment of small perturbations of this state.

Navier thought, require the consideration of any relation between λ and μ nor of the limiting value of $R^4 f$. Third, the lack of rigidity is an immediate consequence of the symmetry properties of a central-force continuum. Neither Cauchy nor Poisson apparently saw this last point (which is in any case immediately apparent only to a modern physicist trained to exploit symmetries). It was Saint-Venant who first remarked that the lack of shear stress in a perfectly continuous body resulted from the perfect invariance of the central forces acting in such a body for a large class of internal, shearing deformations. For example, a global shift of the half of an (infinite) body situated on one side of a fixed plane is such a deformation.[65]

Another of Poisson's objections to Navier was that the method of moments, which Lagrange had successfully used for continuous media, did not apply to molecular systems. This is a surprising statement, since the principle of virtual velocities does not presuppose the continuity of the material system to which it is applied. Poisson probably meant that Navier's estimate of the total moment did not properly include the contribution of molecules whose sphere of action intersects the surface of the body. Indeed the moments of the forces between such a molecule and all other molecules of the body do not sum to the full value (7). However, the contribution of such molecules is to the total moment what the radius of action is to the average radius of the body. It can therefore be neglected in the condition of equilibrium. Although Navier never gave this justification, his intuitive estimate of the total moment was correct.[66]

Navier's method of moments can in fact be applied to the Cauchy-Poisson system of molecules acting through central forces, with a considerable gain in simplicity. By analogy with Navier's formula (17), the total moment in the original state of the body is

$$M = -\frac{1}{2}\int N d\tau \sum_{\beta} R^{-1} f x_i x_j \partial_i w_j. \tag{45}$$

This may be reexpressed in terms of Cauchy's stress system (36) as

$$M = -\int \sigma_{ij} \partial_i w_j d\tau. \tag{46}$$

The effect of a deformation **u** on the moment therefore agrees with the effect of this deformation on the stress system as derived by Cauchy. Then the Navier-Lagrange procedure of balancing this moment with the moment of impressed forces and pressures

---

[65] Saint-Venant, MS (1834), discussed below (ref. 82), sect. 2; "Mémoire sur la question de savoir s'il existe des masses continues, et sur la nature probable des dernières particules des corps," *BSP* (1844), 3–15. By varying Poisson's central forces around equilibrium, Navier's elastic force ϕ is easily seen to be related to Poisson's f (in the notation of this paper) by $\phi = R^{-1} f + R d(R^{-1} f)/d\mathrm{R}$, which implies that the integral of $R^4\phi$ and Navier's elastic constant vanish. Saint-Venant's argument may have been inspired by Fresnel's remark, in his molecular ether-model of 1821, that resistance to the shift of a slice of ether required molecular constitution with intermolecular distances much smaller than this shift: A. Fresnel, "Sur le calcul des teintes que la polarisation développe dans les lames cristallines" (1821), in *Oeuvres complètes*, 3 vols. (1866, 1868, 1870), vol. 1, 609–653, on 630–632.

[66] Poisson (ref. 55), 400.

yields the interpretation of $\partial_i \sigma_{ij}$ as the elastic force density and σ_{ij} as the system of pressures.

Navier's methods were more coherent than Poisson believed, and they had considerable advantages. They minimized assumptions concerning the nature of molecular forces, and they provided a direct link between these assumptions and macroscopic properties. For this reason, several modern commentators have seen in Navier's theory an anticipation of George Green's potential-based theory of elasticity of 1837. About the necessity of preserving discrete sums, Poisson was essentially correct. However, he exaggerated the problem: in the isotropic case the substitution of integrals for sums does not affect the structure of the equations of motion as long as the integration over distance is not explicitly performed.[67]

Fluids as temporary solids

In 1829, Poisson, the self-styled champion of molecular rigor was forced to correct several flaws in his 1828 memoir that Cauchy's memoir had made apparent. He took the opportunity to offer a theory of fluid motion based on the following assumption: a fluid, like a solid, experiences stresses during its motion, but these stresses spontaneously relax in a very short time. In this picture, the liquid goes through a rapid alternation of stressed and relaxed states. Poisson further assumed that the average stress system of the fluid is to the fluid's rate of deformation what the stress system of an isotropic solid is to its strain. This leads to the Navier-Stokes equation, with some additions to the pressure gradient term that depend on the compressibility of the fluid.[68]

Poisson did not think it necessary to mention Navier's memoir on fluid motion, which he probably judged incompatible with sound Laplacian reasoning. Neither did he mention Cauchy's "perfectly inelastic solid," despite the similarity between his and Cauchy's ways of relating the desired stresses to those in an isotropic elastic solid.

5. Saint-Venant: Slides and shears

Le pont des Invalides

Navier's and other Polytechnicians' efforts to bridge the gap between theoretical and applied mechanics had no clear effect on French engineering practice. Industry

---

67 Reference to Green is found, e.g., in Dahan (ref. 29). One way to save Navier's procedure, is to introduce a finite lower limit in his integrals: see R. Clausius, "Über die Veränderungen welche in den bisher gebräuchlichen Formeln für das Gleichgewicht und die Bewegung elastischer fester Körper durch neuere Beobachtungen nothwendig geworden sind," *AP*, 76 (1849), 46–67, on 56–58.

68 Poisson, "Mémoire sur les équations générales de l'équilibre et du mouvement des corps solides élastiques et des fluides" [read on 12 Oct 1829], *Journal de l'Ecole Polytechnique*, cahier 20 (1831), 1–174, fluids on 139–174. Stokes showed that for small compressions, Poisson's additional gradient term is $(\varepsilon/3)\nabla(\nabla \cdot \mathbf{v})$, as in Stokes' fluid model.

prospered much faster in Britain, despite the less-mathematical training of its engineers. Some of Navier's colleagues were eager to ridicule the use of transcendental mathematics in concrete problems of construction.[69] In the mid-1820s, a spectacular incident seemed to justify their disdain. Navier's chef d'oeuvre, a magnificent suspended bridge at the Invalides, was ordered dismantled in the final stage of its construction.

Navier had learned the newer technique of suspension during official missions to England and Scotland in 1820 and 1823. At the end of his ministerial report, he argued for a new suspended bridge of unprecedented scale, across the Seine river facing the Invalides (Fig. 4). According to Prony's and Saint-Venant's judgement, Navier's innovative design was based on sound experience and calculation. However, as the bridge was nearly finished, an accidental flood caused displacement of one of the rounded stones on which the suspending chains changed direction before anchoring (Fig. 4b). As Saint-Venant explained, Navier had misestimated the direction of the force exerted by the chain on the stone – a kind of oversight that frequently occurs in engineering construction and that is easily corrected on the spot. Yet hostile municipal authorities obtained the dismantlement of Navier's bridge.[70]

According to Saint-Venant, the incident meant more than a local administrative deficiency:[71]

> At that time there already was a surge of the spirit of denigration, not only of the *savants*, but also of science, disparaged under the name of *theory* opposed to *practice*; one henceforth exalted practice in its most material aspects, and pretended that higher mathematics could not help, as if, when it comes to results, it made sense to distinguish between the more or less elementary or transcendent procedures that led to them in an equally logical manner. Some *savants* supported or echoed these unfounded criticisms.

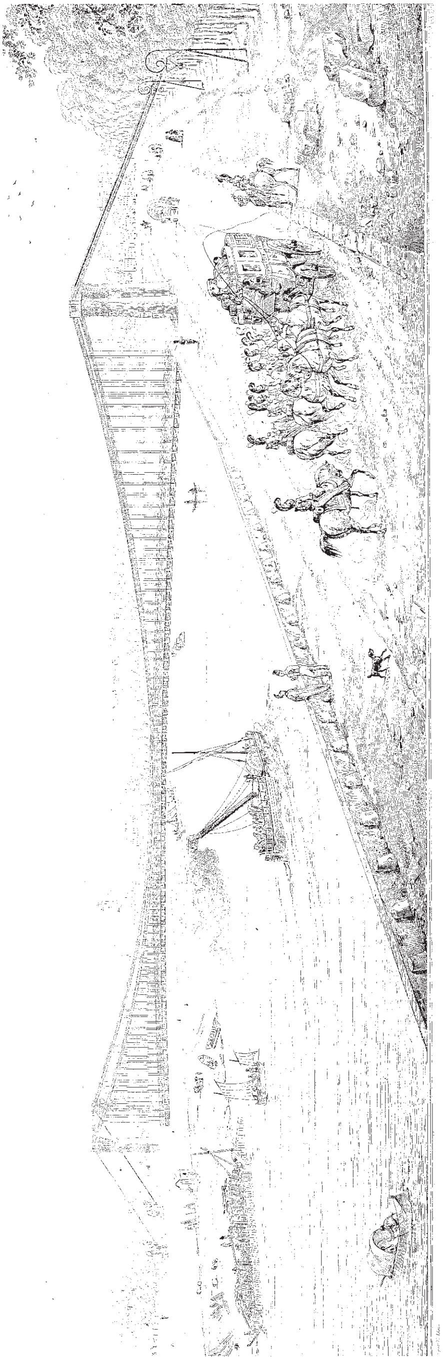
Indeed some engineers were openly hostile toward the theoretical approach that Navier embodied. In 1833, the *Ingénieur en chef des Ponts et Chaussées,* Louis Vicat, already acclaimed for his improvement of hydraulic limes, cements, and mortars, performed a number of experiments on the rupture of solids. His declared aim was "to determine the causes of the imperfection of known theories, and to point out the dangers of these theories to the constructors who, having had no opportunity to verify them, would be inclined to lend them some confidence." He measured the deformations and the critical charge for various kinds of loading, and observed the shape of the broken parts. He thought to have refuted Coulomb's and Navier's formulas for the collapse of pillars, as well as Navier's formulas for the flexion and the torsion of prisms.

---

[69] Cf. Belhoste (ref. 21), 24–25. Belhoste explains how this state of affair prompted reforms at the Ecole Polytechnique and at the Ecoles d'application.

[70] Navier, *Rapport à Monsieur Becquey, Directeur Général des Ponts et Chaussées et des Mines; et Mémoire sur les ponts suspendus* (Paris, 1823); 2nd ed. *augmentée d'une notice sur le pont des Invalides* (Paris, 1830). Cf. Prony (ref. 22), xlv-xlvii; Saint-Venant (ref. 22), lxv-lxix; Grattan-Guinness (ref. 15), 994–1000; Picon (ref. 21), 372–384.

[71] Ibid., lxviii.



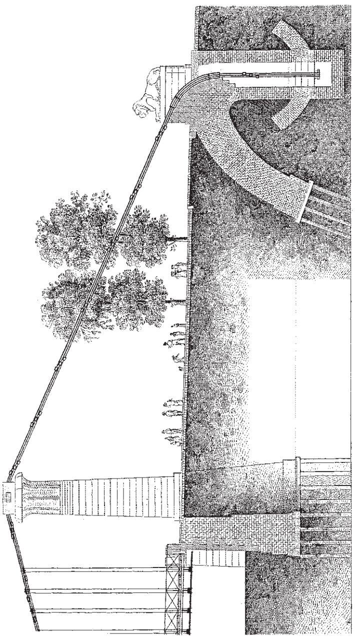


Fig. 4. Navier's projected *Pont des Invalides* on the Seine river (**a**), and the anchoring system for the chains (**b**). From Navier (ref. 70), plates

Moreover, he charged Coulomb and Navier with erroneous conceptions of the mode of rupture.[72]

Vicat's ruptures

Vicat distinguished three ways in which the aggregation of a solid could be destroyed: pull (*tirage*), pressure (*pression*), and sliding (*glissement*). He called the corresponding forces pulling force (*force tirante*), sustaining force (*force portante*), and transverse force (*force transverse*). He defined the third kind of disintegration as "the effort which tends to divide a body by making one of its parts slide on the other (so to say), without exerting any pressure nor pull outside the face of rupture." This effect was usually ignored, even though it controls the rupture of short beams under transverse load. The most important exception was Coulomb, whose theory Vicat however disapproved.[73]

Vicat published his memoir in the *Annales des Ponts et Chaussées* but ventured to send a copy for review to the Academy of Sciences. The reviewers, Prony and Girard, defended their friends Coulomb and Navier, arguing that Vicat had used granular, inflexible materials and short beams for which the incriminated formulas were not intended. They judged that Vicat's measurements otherwise confirmed existing theories. They also emphasized that only Coulomb's theory could justify the use of reduced-scale models, on which Vicat's conclusions partly depended.[74]

In his response, Vicat compared the two Academicians to geometers who would judge the law "surface equals half-product of two side lengths" to apply to any triangle because they have found it to hold for rectangular triangles. In a less ironical tone, he showed that some of his measurements did contradict the existing theories in their alleged domain of validity. Navier himself did not respond to Vicat's aggression. However, some modifications in his course at the *Ponts et Chaussées* suggest that he took Vicat's conclusions on the importance of slides and transverse forces seriously. His former student Saint-Venant certainly did.[75]

Molecules, slides, and approximations

Adhémar Barré de Saint-Venant had an "X+Ponts" training, and an exceptional determination to conciliate engineering with academic science. His mathematical fluency

---

[72] L. Vicat, "Recherches expérimentales sur les phénomènes physiques qui précèdent et accompagnent la rupture ou l'affaissement d'une certaine classe de solides," *Annales des Ponts et Chaussées* (1833), 201–268, on 202. On Vicat, his work on limes, cements, and mortars, and his implicit criticism of Navier's conception of suspended bridges, cf. Picon (ref. 21), 364–371, 384–385.

[73] Ibid., 201. Cf. E. Benvenuto, "Adhémar Barré de Saint-Venant: The man, the scientist, the engineer," Accademia Nazionale dei Lincei, *Atti dei convegni Lincei*, 140 (1998), 7–34, on 18–19.

[74] Prony and Girard, "Rapport...," *Annales des Ponts et Chaussées* (1834), 1st semester, 293–304.

[75] Vicat, "Observations sur le rapport...," ibid., 305–312.

and his religious dedication to the improvement of his fellow citizens' material life determined this attitude. He rejected both the narrow empiricism of a Vicat and the arbitrary idealizations of French rational mechanics. His own sophisticated strategy may be summarized in five steps:[76]

i) Start with the general mechanics of bodies as they are in nature, which is to be based on the molecular conceptions of Laplace, Poisson, and Navier.
ii) Determine the macroscopic kinematics of the system, and seek molecular definitions for the corresponding macroscopic dynamics.
iii) Find macroscopic equations of motion if possible by summation over molecules, or else by macroscopic symmetry arguments; the molecular level thus being, as it were, "blackboxed" in adjustable parameters.
iv) Develop analytical techniques and methods of approximation to solve these equations in concrete situations.
v) Test consequences and specify adjustable parameters by experimental means.

Saint-Venant developed this methodology while working on elasticity and trying to improve on Navier's methods. He regarded the first, molecular step as essential for a clear definition of the basic concepts of mechanics and for an understanding of the concrete properties of matter. In his mind, the most elementary interaction was the direct attraction or repulsion of two mass points. Consequently, following Poisson's and Cauchy's argument of 1828 there could be no continuous solid. Matter had to be discontinuous, and all physics had to be reduced to central forces acting between non-contiguous point-atoms.[77]

In the second, kinematic step Saint-Venant characterized the macroscopic deformations of a quasi-continuum in harmony with Vicat's analysis of rupture. Cauchy had introduced the quantities $e_{ij} = \partial_i u_j + \partial_j u_i$, but only to determine the dilation or contraction $(1/2)e_{ij}dx_i dx_j$ of a segment $d\mathbf{x}$ of the body. While studying a carpentry bridge on the Creuze river in 1823, and later in his lectures at the Ponts et Chaussées, Saint-Venant gave a precise geometrical definition to Vicat's slides and took them into account in a computation of the flexion of beams. According to this definition, the jth component of slide (*glissement*) in a plane perpendicular to the ith axis is, at a given point of the body, the cosine of the angle that two lines of the body intersecting at this point and originally parallel to the ith and jth axes make after the deformation (see Fig. 5). To first order in u, this is the same as Cauchy's e_{ij}. Saint-Venant used the slides not only to investigate the limits of rupture, but also to develop a better intuition of the internal deformations in a bent or twisted prism.[78]

---

[76] Cf. J. Boussinesq and M. Flamant, *Notice sur la vie et les travaux de Barré de Saint-Venant* (Paris, 1886); C. Melucci, *Scienza, spiritualità, visione politica in A.J.C. Barré de Saint-Venant: Contributi teorici e applicativi nella dinamica dei fluidi e nella scienza del miglioramento del territorio*, doctoral diss. (Università degli studi di Genova, 1996); Darrigol, "God, waterwheels, and molecules: Saint-Venant's anticipation of energy conservation," *HSPS* 31 (2001), 285–353.

[77] Saint-Venant (ref. 64).

[78] Saint-Venant, *Leçons de mécanique appliquée faites par intérim par M. de Saint-Venant, Ingénieur des Ponts et Chaussées, de 1837 à 1838*, lithographed course (Paris, 1837); "Mémoire sur le calcul de la résistance et de la flexion des pièces solides à simple et à double courbure;

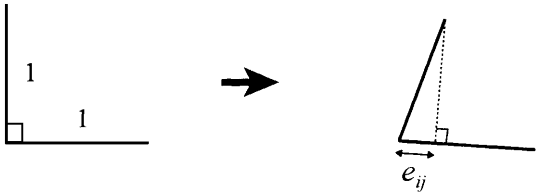


Fig. 5. Geometrical meaning of Saint-Venant's slide e_{ij} with respects to the orthogonal axes *i* and *j* (in the plane of the figure)

Saint-Venant then defined the stress system σ_{ij} in the molecular manner that has already been described, and found its relation to the strains e_{ij} in one of Cauchy's manners. The first manner, based on symmetry only, was simplest. But Saint-Venant believed the second, molecular manner indispensable to limit the number of independent elastic constants (only one in the isotropic case). Lastly, he applied or invented methods of resolution or approximation to solve engineering problems on this basis. He appreciated the variety of available strategies of approximation:

> Between mere groping and pure analysis, there are many intermediaries: the methods of false position, the variation of arbitrary constants, the solutions by series or continuous fractions, the methods of successive approximations, integration by the computation of areas or by the formulas of Legendre and Thomas Simpson, the reduction of the equations to more easily soluble ones by the choice of an unknown of which one may neglect a few powers or some function in a first approximation, graphical procedures, figurative curves drawn on squared paper, the use of curvilinear coordinates, etc. etc.

Yet this what not enough for the outstanding problem of the engineer of wood and iron structures: the flexion and torsion of prisms. For some twenty years, Saint-Venant worked hard to avoid the simplifications used in previous solutions: absence of slides, small deformation, perpendicularity of longitudinal fibers and transverse sections, flatness of transverse sections, etc.[79]

His most impressive achievement was the "semi-inverse" method that he developed in the 1830s. The "direct" problem of elasticity, which is the determination of impressed

en prenant simultanément en considération les différents efforts auxquels elles peuvent être soumises," *CR*, 17 (1843), 942–954, 1020–1031, on 943: "Je fais entrer dans le calcul les effets de glissement latéral dus à ces composantes transversales dont l'omission à été l'objet principal d'une sorte d'accusation portée par M. Vicat contre toute la théorie de la résistance des solides." Cf. Boussinesq and Flamant (ref. 76), 560 (bridge on the Creuze river); Todhunter and Pearson (ref. 28), vol. 1: 834–836, 843, vol. 2: 394–395; Benvenuto (ref. 73), 20–24.

[79] Saint-Venant, "Sur la définition de la pression dans les corps fluides ou solides en repos ou en mouvement," *BSP* (1843), 134–138; citation from "Mémoire sur les eaux courantes considérées dans un lit de figure variable," MS (1834–35), Archives de l'Ecole Polytechnique, Fond Saint-Venant, carton 21. Cauchy approved Saint-Venant's definition of pressure in "Note relative à la pression totale supportée par une surface finie dans un corps solide ou fluide," *CR*, 20 (1845), 1764–1766. For the successive steps of Saint-Venant's work on the flexion and torsion on prisms, see his own *Notice sur les travaux et titres scientifiques de M. de Saint-Venant* (Paris, 1864).

forces knowing the deformation, is easily solved by applying the stress-strain relations. In contrast, the practically important "inverse" problem, which is the determination of deformations for given impressed forces, leads to differential equations whose integration in finite terms is usually impossible. Saint-Venant's important idea was to solve a tractable mixed problem in which the deformation and the impressed forces were both partly given. He found that for a proper mixture an exact solution could be given whose difference from the practically needed solution to the inverse problem was negligible.[80]

On fluid motion

Although Saint-Venant is most famous for his work on elasticity, he also had a constant interest in hydraulics. Early in his career, he reflected on waterwheels and the channels and weirs through which they were fed. He also began to think about the scientific control of waters in rural areas, which he later called *hydraulique agricole.* In this field as for elasticity, Saint-Venant avoided narrow empiricism. He wanted to base the determination of channel and pipe flow on fundamental hydrodynamic knowledge. Since Navier's attempt in this direction had failed, the field remained wide open.[81]

In 1834, Saint-Venant submitted to the Academy of Sciences a substantial, but never published, memoir on the dynamics of fluids. Therein he first expressed his approbation of the *Mécanique physique* by citing Poisson: "It is important for the progress of sciences that rational mechanics should no longer be an abstract science, founded on definitions referring to an imaginary state of bodies." He rejected ideal solids, argued for central forces and point-atoms, and proved the discontinuity of matter in the above-mentioned manner. He defined the average "translatory" motion observed in hydraulic experiments and the invisible "non-translatory" motion that molecular interactions necessarily implied. Then he gave his molecular definition of internal pressures (which he called "impulsions"), and showed the existence of transverse pressures in moving fluids by a detailed consideration of the perturbation of the translatory motion by molecular encounters. In harmony with his kinematics of elastic bodies, he characterized the transverse pressure as opposed to the sliding of successive layers of the fluid on one another.[82]

This pressure depended on the microscopic non-translatory motion of the molecules, which propagated through the whole fluid mass "and got lost to the outside by producing, in the walls and in the exterior air foreign agitation and other effects foreign to

---

[80] Saint-Venant introduced this method in 1847 and 1853. His fullest study of the torsion and flexion of prisms is "Mémoire sur la torsion des prismes, avec des considérations sur leur flexion, ainsi que sur l'équilibre intérieur des solides élastiques en général, et des formules pratiques pour le calcul de leur résistance à divers efforts s'exerçant simultanément," Académie des Sciences de l'Institut Impérial de France, *Mémoires présentés par divers savants*, 14 (1855), 233–560.

[81] Cf. Melucci (ref. 76); Darrigol (ref. 76).

[82] Saint-Venant, "Mémoire sur la dynamique des fluides," MS (1834), Archives de l'Académie des Sciences, *pochette de séance* for 14 Apr 1834, sects. 1 (molecular mechanics), 2 (no continuous matter), 4 (undulated motion of molecules), 5 (definition of impulsions), 6–7 (transverse pressures); Poisson, *Nouvelle théorie de l'action capillaire* (Paris, 1831), 130.

the translatory [macroscopic] motion of the fluid." The live force of the macroscopic motion thus diminished at the price of hidden microscopic motion. Later, in the 1840s, Saint-Venant identified the non-translatory motions with heat.[83]

Deterred by the complexity he saw in the friction-related molecular motions, Saint-Venant renounced a purely molecular derivation of the pressure system. Instead he appealed to a symmetry argument in the spirit of Cauchy's first theory of elasticity. He assumed that the transverse pressure on a face was parallel to the fluid slide on this face, and (erroneously, he later realized) took the slide itself to be parallel to the projection of the fluid velocity on the face. This led him to an equation of motion that is far more complicated than Navier's, with five parameters instead of one, and with variations of these parameters depending on the internal, microscopic commotions of the fluid. Saint-Venant applied this equation to flow in rectangular or semi-circular open channels and described a new method of fluid-velocity measurement. He thus wanted to prepare the experimental determination of the unknown functions that entered his equations.[84]

A first-class burial

The commissioners Ampère, Navier, and Félix Savary approved Saint-Venant's memoir. Yet Savary, who was supposed to write the report, never did so and instead expressed disagreements in letters to the author. From Saint-Venant's extant replies, we may infer that Savary ignored the contradiction between Du Buat's results and Navier's equation, and that he condemned the recourse to adjustable parameters in fundamental questions of hydrodynamics. In his defense Saint-Venant clarified the purpose of his memoir: "My principal goal is all practical: it is the solution of the open-channel problem for a bed of variable and arbitrary figure." He then gave an interesting plea for a semi-inductive method:[85]

> My equations contain indeterminate quantities and even functions; but is it not good to show how far, in fluid dynamics, we may proceed with a theory that is free of hypotheses (save for *continuity*, at least *on average*), that brings forth the unknown and prepares its experimental determination? A bolder march may sometimes quickly lead to the truth [...]. However, you will no doubt judge that in such an important matter it may be advantageous to consider things from another point of view, to avoid every supposition and to appeal to experimenters to fix the values of indeterminate quantities by means of *special* experiments prepared so as to isolate the effects that the theory will later try to explain with much more assurance and to represent by expressions that are as free of empiricism as possible.

[83] Saint-Venant (ref. 82), sect. 7. For the identification with heat, cf. ref. 3 [1847], 73n.
[84] Saint-Venant (ref. 82), sects. 11 (hypothesis), 15 (equation), 18–24 (consequences); 25–28 (suggested experiments).
[85] Saint-Venant to Savary, 25 Aug 1834, Bibliothèque de l'Institut de France, MS 4226; see also the letters of 27 Jul and 10 Sep 1834, ibid.

Re-founding Navier's equation

Three years later, Saint-Venant discovered his error about the direction of slides, and ceased to require a report from Savary. Instead he inserted a new argument freed of this flaw in the manuscript deposited at the Academy. He still assumed that the transverse pressure on a face was parallel to the slide on this face, or, equivalently and even more naturally, that the transverse pressure was zero in the direction of the face for which the slide vanished. But he now used the correct expression $\partial_i u_j + \partial_j u_i$ of the slides (per unit time) corresponding to the fluid velocity $\mathbf{u}$ and the orthogonal directions i and j. He further noted that $\sigma_{ii} - \sigma_{jj}$ represented twice the transverse pressure along the line bisecting the ij angle, and $\partial_i u_i - \partial_j u_j$ the slide along the same line. Granted that the components of slide must be proportional to the components of transverse pressure, the ratios $\sigma_{ij}/(\partial_i u_j + \partial_j u_i)$ and $(\sigma_{ii} - \sigma_{jj})/2(\partial_i u_i - \partial_j u_j)$ are all equal for every choice of i and j. Calling ε their common value at a given point of the fluid and μ an undetermined isotropic pressure, this implies

$$\sigma_{ij} = \varepsilon(\partial_i u_j + \partial_j u_i) + \mu\delta_{ij}. \tag{47}$$

As Saint-Venant noted, this stress system yields the Navier-Cauchy-Poisson equation in the special case of a constant ε, with a gradient term contributing to the normal pressure.[86]

For a modern reader familiar with tensor calculus, Saint-Venant's reasoning may seem to be just another proof of the fact that the expression (47) is the most general symmetrical second-rank tensor that depends linearly and isotropically on the tensor e_{ij}. Yet this is not the case, because Saint-Venant did not assume the linearity. His hypothesis of the parallelism of slides and tangential pressures implies more than mere isotropy. For instance, it excludes terms proportional to $e_{ik}e_{kj}$. Most important, it allows for an ε that varies from one particle of the fluid to another, and from one case of motion to another.[87]

Saint-Venant believed a variable ε to be required by Du Buat's and others' experiments on pipe and channel flow, and to express the effects of local "irregularities of motion" on internal friction. The velocity $\mathbf{u}$ in his reasoning referred to the average, smooth, large-scale motion. Smaller-scale motions only entered the final equation as a contribution to tangential pressures defined at the larger scale. Whether or not Saint-Venant regarded Navier's equation with constant ε as valid at a sufficiently small scale is not clear. In any case, he believed that the value of ε should be determined experimentally without prejudging its constancy from place to place or from one case of motion to another.[88]

---

86 Saint-Venant to Savary, 13 Jan 1837, ibid.; Saint-Venant (ref. 82), new version of sect. 15; "Note à joindre au mémoire sur la dynamique des fluides, présenté le 14 avril 1834," *CR*, 17 (1843), 1240–1243.

87 Ibid., 1243, for variable ε. Ibid., on 1242n, Saint-Venant noted that Cauchy's pressure theorems were valid to second order in the dimensions of the volume-elements, "which allows us to extend their application to the case when partial irregularities of the fluid motion forces us to take faces of a certain extension so as to have regularly varying averages."

88 Saint-Venant, ibid.; Saint-Venant to Savary (ref. 85), 27 Jul 1834 (on Du Buat); Saint-Venant (ref. 82), new sect. 15: "It is experiment that should determine whether ε is constant or variable."

In the mid-1840s, the military engineer Pierre Boileau undertook a series of experiments on channel and pipe flow. Unlike most hydraulicians, who were only interested in the global discharge, Boileau planned measurements of the velocity profile of the flow. Saint-Venant congratulated him for this intention, because such knowledge was necessary to estimate the friction between successive fluid filaments, or the variable ε of his equation of fluid motion. He also advised Boileau on the most suitable channel and pipe shapes and on the technique of velocity measurement. The best measurements of that kind were done in the 1850s and 1860s by the *Ingénieur des Ponts* Henry Darcy and his assistant and successor Henry Bazin. Saint-Venant's protégé Joseph Boussinesq then developed the method of the effective ε at great length and with impressive analytical depth, so as to match these engineers' results.[89]

Extraordinary friction

In Saint-Venant's memoir of 1834, what justified a variable ε was the irregularities of motion that the macroscopic fluid slides entailed. At that time Saint-Venant confined the irregularities to mere undulations of molecular paths. However, he soon came to include the whirling motions triggered by larger slides and described by da Vinci, Daniel Bernoulli, and Venturi for sudden pipe enlargement or for the flow behind a solid obstacle. In a study of pressure losses in the pipes of steam engines, Saint-Venant emphasized the role of "*extraordinary friction*, usually called *loss of live force*, and determined by the whirling of fluids especially at points where the section of the flow suddenly increases."[90]

In 1839, the military engineer Jean-Victor Poncelet published the second edition of his celebrated course for the workers and artists of Metz, in which he gave much importance to the whirling motions observed during the sudden alteration of a flow. These motions, he noted, were "much more complicated than one usually thought." They involved pulsations, intermittences, and conversion of large-scale whirling to smaller-scale whirling, perhaps thus cascading to the molecular level. Poncelet, like Saint-Venant, regarded these intricate motions as "one of the means that nature uses to extinguish, or rather to dissimulate the live force in the sudden changes of motion of fluids, as the vibratory motion themselves are another cause of its dissipation, of its dissemination in

---

Perhaps Saint-Venant did not believe in a constant-ε small scale, because for the tumultuous flows observed in rivers channels and occurring in pipes of not too small diameter Saint-Venant believed that any irregularity of motion cascaded to smaller and smaller scale by "molecular gearing."

[89] P. Boileau, "Etudes expérimentales sur les cours d'eau," *CR*, 24 (1847), 957–960; *Traité de la mesure des eaux courantes* (Paris, 1854). Saint-Venant to Boileau, 29 March 1846, Fond Saint-Venant, reproduced and discussed in Melucci (ref. 76), 65–71. On Darcy, Bazin, and Boussinesq, cf. Rouse and Ince (ref. 7), 169–177, 201–206.

[90] Saint-Venant, "Mémoire sur le calcul des effets des machines à vapeur, contenant des équations générales de l'écoulement permanent ou périodique des fluides, en tenant compte de leurs dilatations et de leurs changements de température, et sans supposer qu' ils se meuvent par tranches parallèles, ni par filets indépendants," *CR*, 6 (1838), 45–47, on 47.

solids." He also believed that the smaller-scale motion largely contributed to the effective friction between fluid filaments.[91]

Saint-Venant approved these considerations and brought them to bear on the problems of flow retardation. For example, in 1846 he derived Borda's old formula for the loss of head during a sudden enlargement of a pipe by estimating the dissipated live force. "The molecular gearing [engrènement moléculaire]," Saint-Venant wrote, "creates whirls and other non-translatory motions indicated by D. Bernoulli and by M. Poncelet, and which, after being conserved for some time in the fluid, end up being dissipated under the effect of friction and extraordinary resistance." In a discussion of practical retardation formulas and tables published in 1851, he made similar tumultuous motions responsible for the variable ε he suggested since 1834:[92]

> If Newton's hypothesis, as reproduced by MM. Navier and Poisson, and which consists in making internal friction proportional to the relative velocity of the filaments sliding on one another, can be approximately applied to the various points of the same fluid section, every known fact indicates that the proportionality *coefficient* must increase with the dimensions of transverse sections; which may to some extent be explained by noticing that the filaments do not proceed in parallel directions with a regular gradation of velocity, and that the *ruptures*, the whirls and other complex and oblique motions that must considerably influence the intensity of friction develop better and faster in large sections.

Fluid resistance

In 1846 Saint-Venant considered the old, difficult problem of fluid resistance. He first showed that the introduction of internal friction solved d'Alembert's paradox. For this purpose he borrowed from Du Buat and Poncelet the idea of placing the immersed body inside a cylindrical pipe (Fig. 6), from Euler the balance of momentum, and from Borda the balance of live force. If the body is sufficiently far from the walls of the pipe, the action of the fluid on the body should be the same as for an unlimited flow. The body being fixed, the flow being stationary and the fluid incompressible, the momentum which the fluid conveys to the body in a unit time is equal to the difference $P_0S - P_1S$ between the pressures on the faces of a column of fluid extending far before and after the body, because the momentum of the fluid column remains unchanged. For an ideal fluid, the work $(P_0S - P_1S)v_0$ of these pressures in a unit time must vanish, because the live force of the fluid column is also unchanged. Hence the two pressures are equal, and the fluid resistance vanishes. This is d'Alembert's paradox, as proved by Saint-Venant.[93]

---

[91] J.V. Poncelet, *Introduction à la mécanique industrielle, physique ou expérimentale* (Paris, 1839), 528–530.

[92] Saint-Venant, "Mémoire sur la perte de force vive d'un fluide, aux endroits où sa section d'écoulement augmente brusquement ou rapidement," *CR*, 23 (1846), 147–153, on 147; "Mémoire sur des formules nouvelles pour la solution des problèmes relatifs aux eaux courantes," *Annales des Mines*, 20 (1851), 183–357, on 229.

[93] Saint-Venant, "Solution d'un paradoxe proposé par d'Alembert aux géomètres" [read on 7 March 1846], *BSP* (1846), 25–29, 72–78, 120–121; (ref. 3), 45–49. In 1866, the Chevalier de Borda had derived the paradox in an even simpler manner, by applying the conservation of live

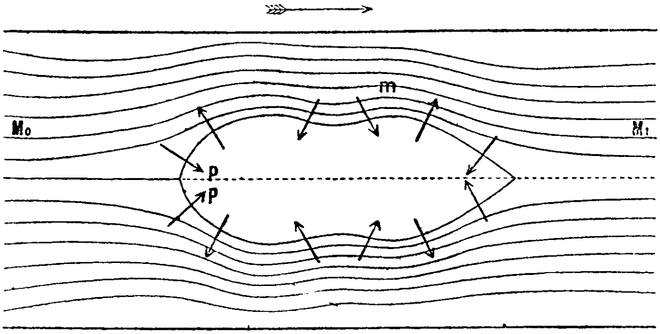


Fig. 6. Drawing for Saint-Venant's proof of d'Alembert's paradox (from Saint-Venant, ref. 3, p. 50)

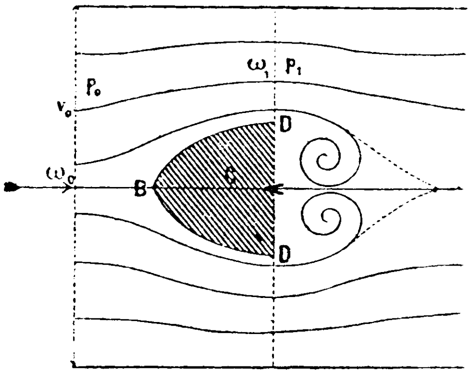


Fig. 7. Drawing for Poncelet's and Saint-Venant's evaluation of fluid resistance (from Saint-Venant (ref. 3), 89)

In a molecular fluid, the (negative) work of internal friction must be added to the work of the pressures P_0 and P_1, or, equivalently, the live force of non-translatory motions must be taken into account. Hence the pressure falls when the fluid passes the body, and the resistance no longer vanishes. The larger the amount of non-translatory motion induced by the body is, the higher is the resistance. When tumultuous, whirling motion occurs at the rear of the body, the resistance largely exceeds the value it would have for a perfectly smooth flow. After drawing these conclusions, Saint-Venant improved on a method invented by Poncelet to estimate the magnitude of the resistance, and based on the assumption that the pressure P_1 at the rear of the body does not differ much from the value that Bernoulli's law gives it in the most contracted section of the flow (Fig. 7).[94]

In sum, Saint-Venant did not accept the dichotomy between an hydrodynamic equation for ideally smooth flow on the one hand, and completely empirical retardation and

forces to a body pulled uniformly through a quiet fluid: "Sur l'écoulement des fluides par les orifices des vases," *MAS* (1766), 579–607, on 605.

[94] Saint-Venant (ref. 93), 28, 72–78, 120–121; (ref. 3), 56–192.

resistance formulas for hydraulic engineers on the other. He sought a via media that brought theoretical constraints to bear on practical flows and yet allowed for some experimental input. One of his strategies, later pursued by Boussinesq and successfully applied to turbulent flow to this day, consisted in reinterpreting Navier's hydrodynamic equation as controlling the average, smoothed out flow with a variable viscosity coefficient. Another was the astute combination of momentum and energy balances with some empirically known features of the investigated flow. For hydraulics as for elasticity, he was the most obstinate and most imaginative conciliator of fundamental and practical demands.

6. Stokes: The pendulum

A swimming mathematician

Until the 1830s at least, the production of advanced mathematical physics in an engineering context remained a uniquely French phenomenon, largely dependent on the creation of the Ecole Polytechnique. The main British contributors to elasticity theory and hydrodynamics in this period had little or no connection with engineering. Typically, they were astronomers like George Airy and James Challis, or mathematicians like George Green and Philip Kelland. Their work on elasticity was subordinated to their interest in the new wave-optics, and the aspects of hydrodynamics that captured their attention tended to be wave and tide theory. George Stokes himself was a Cambridge-trained mathematician, First Wrangler and Smith-prize winner in 1841. He nonetheless was a keen observer of nature, a first-rate swimmer, and a naturally gifted experimenter. He was quick to note the gaps between idealized theories and real processes, and sometimes eager to fill them.[95]

During the two decades preceding Stokes' studies, 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 provided the simplest illustration of the necessary mathematics of partial differential equations. The famous Cambridge coach William Hopkins made it a basic part of the Tripos examination, and persuaded Stokes to choose it as his first research topic.[96]

Stokes' first paper dealt with the two-dimensional or cylindrically symmetrical stationary motions of an incompressible fluid. From an analytical point of view, his results could be found in Lagrange or J.M.C. Duhamel. Yet his discussion of their physical significance was penetrating and innovative. He introduced the notion of stability of a

---

[95] Cf. G.G. Stokes, "Report on recent researches in hydrodynamics," British Association for the Advancement of Science, *Report* (1846), in *SMPP*, vol. 1, 157–187; E.M. Parkinson, "Stokes, George Gabriel," *DSB*, 13 (1976), 74–79; D.B. Wilson, *Kelvin and Stokes: A comparative study in Victorian physics* (Bristol, 1987); Introduction to *The correspondence between Sir George Gabriel Stokes and Sir William Thomson, Baron Kelvin of Largs*, 2 vols. (Cambridge, 1990), xv-xlvi.

[96] On the transformation of British physics, cf. C. Smith and N. Wise, *Energy and empire: A biographical study of Lord Kelvin* (Cambridge, 1989), chap. 6; On Hopkins' role, cf. Wilson (ref. 95) (1987), 132.

flow, which later became an essential part of hydrodynamics. Specifically, he argued that the possibility of a given motion did not imply its necessity, because there could be other motions compatible with the same boundary conditions, some of which could be stable, others unstable. “There may even be no stable steady mode of motion possible, in which case the fluid would continue perpetually eddying.”[97]

As a first example of instability, Stokes cited the two-dimensional flow between two similar hyperboles. An experiment of his own showed that the theoretical hyperbolic flow only held in the case for which the flows becomes narrower. He compared this result with the fact that a fluid passing through a hole from a higher pressure vessel to a lower pressure one tends to form a jet instead of creeping along the walls, as the most obvious analytical solution would have it. Although Mariotte, Bernoulli, and Borda already knew such effects, Stokes was the first to suggest their connection with special solutions of Euler’s equations involving surfaces of discontinuity. This was a first step toward a more realistic theory of fluid motion.[98]

The pendulum

Stokes’ motivation for other steps of the same kind derived from his interest in the pendulum experiments performed by Edward Sabine in 1829. This artillery officer had been responsible for a number of geodesic projects, one of which, in 1821–1822, dealt with the pendulum determination of the figure of the Earth. In 1828, the German astronomer Friedrich Bessel published a memoir on the seconds’ pendulum that brought pendulum studies, and quantitative experiment in general, to an unprecedented level of sophistication. Bessel not only improved experimental procedures and data analysis, but he also included new theoretical insights into the various effects that altered the ideal pendulum motion. Most important, he was first to take into account the inertia of the air carried along by the pendulum. His study played a paradigmatic role in defining a Königsberg style of physics. It also triggered further experimental and theoretical pendulum studies in Britain and France.[99]

In his investigation of Bessel’s inertial effect, Captain Sabine encountered the following anomaly. The correction to the mass of the pendulum was much higher for hydrogen than the density ratio between hydrogen and air would suggest. Sabine suggested that gas viscosity might explain this anomaly. The remark prompted Stokes’ interest in

---

[97] “On the steady motion of incompressible fluids,” *TCPS* (1842), also in *SMPP*, vol. 1, 1–16, on 10–11. Cf. Parkinson (ref. 95), 75.

[98] Stokes (ref. 97), 11.

[99] On Stokes’ interest in pendulums, cf. Stokes, “On the effect of the internal friction of fluids on the motion of pendulums” [read on 9 Dec 1850], *TCPS* (1850), also in *SMPP*, vol. 3, 1–141, on 1–7. On Sabine, cf. N. Reingold, “Sabine, Edward,” *DSB*, vol. 12 (New York, 1975), 49–53. On Bessel’s work, cf. K. Olesko, *Physics as a calling: Discipline and practice in the Königsberg seminar for physics* (Ithaca, 1991), 67–73. On pendulum studies in general, cf. *Collection de mémoires relatifs à la physique*, vols. 4–5: *Mémoires sur le pendule* (Paris, 1889, 1891), with a historical introduction by C. Wolf, vol. 1, I–XLII, and a bibliograpy. Bessel’s inertial effect was already known to Du Buat (ref. 8), vol. 3, in a hydraulic context.

"imperfect fluids." Stokes' first strategy, implemented in a memoir of 1843, was to study special cases of perfect fluid motion in order to appreciate departures from reality:[100]

> The only way by which to estimate the extent to which the imperfect fluidity of fluids may modify the laws of their motion, without making any hypothesis on the molecular constitution of fluids, appears to be, to calculate according to the hypothesis of perfect fluidity some cases of fluid motion, which are of such a nature as to be capable of being accurately compared with experiment.

Among his cases of motion Stokes included oscillating spheres and cylinders that were relevant to pendulum studies. In the spherical case, he confirmed the 1.5 mass correction factor that Poisson had derived in 1821, and which departed considerably from Bessel's experimental 1.9 factor. Stokes also applied Thomson's method of electrical images to show that a rigid wall placed near the oscillating sphere modified the mass correction. Lastly, he addressed the most evident contradiction with observation: that a perfect fluid does not have any more damping effect on oscillatory motion than it would have on a uniform translational motion.[101]

Stokes considered three possible causes of the observed resistance. First, he imagined that the fluid particles along the surface of the sphere could 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:

> 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 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 trails of eddies, as Poncelet and Saint-Venant had already done. To make this more concrete, he recalled that a ship had least resistance when she left the least wake.[102]

Stokes did not himself perform pendulum experiments, presumably because the required apparatus and protocol was too complex for his taste. He usually favored experiments that could be performed with minimum equipment and time consumption. For

[100] E. Sabine, "On the reduction to a vacuum of the vibrations of an invariable pendulum," Royal Society of London, *Philosophical transactions* (1829), 207–239, commentary to his eighth experiment; Stokes (ref. 99), 2 (Sabine); "On some cases of fluid motion," *TCPS* (1843), also in *SMPP*, vol. 1, 17–68, on 17–18 (quote). Stokes assumed that the motion started from rest, which implies the existence of a velocity potential. Although this implication is no longer valid for a real fluid, Stokes then hoped it would approximately hold for small oscillations (ibid., 30; this turned out to be wrong in the pendulum case).

[101] Ibid., 36, 38–49, 53. Stokes made his calculation in the incompressible case, knowing from Poisson that the effects of compressibility were negligible in the pendulum problem.

[102] Ibid., 53–54.

testing experimentally the departure of real fluids from perfect ones, he judged that the moments of inertia of water-filled boxes offered a better opportunity. However, his own experiments with suspended water-boxes could only confirm the perfect-fluid theory. They were not accurate enough to show any effect of imperfect fluidity.[103]

Fluid friction

Having exhausted the possibilities of his first strategy for studying the imperfection of fluids, Stokes tried another approach. He sought to include internal fluid friction in the fundamental equations of hydrodynamics. To Du Buat's arguments for the existence of internal friction, he added pendulum damping and a typically British observation: "The subsidence of the motion in a cup of tea which has been stirred may be mentioned as a familiar instance of friction, or, which is the same, of a deviation from the law of normal pressure." From Cauchy he borrowed the notion of transverse pressure, as well as the general idea of combining symmetry arguments and the geometry of infinitesimal deformations.[104]

Stokes' first step was the decomposition of the rate of change $\partial_i u_j dx_i$ of an infinitesimal fluid segment $d\mathbf{x}$ into a symmetrical and an antisymmetrical part:

$$\partial_i u_j dx_i = \tfrac{1}{2}(\partial_i u_j + \partial_j u_i)dx_i + \tfrac{1}{2}(\partial_i u_j - \partial_j u_i)dx_i. \qquad (48)$$

Then he showed that the antisymmetrical part corresponded to a rotation of the vector $d\mathbf{x}$, and the symmetrical part to the superposition of three dilations (or contractions) along three orthogonal axes. That $\partial_i u_j - \partial_j u_i$ represents the rotation of an element of a continuum for a small deformation $\mathbf{u}$ was known to Cauchy. No one, however, had explicitly performed Stokes' decomposition and used it to arrive at the local distortion of the medium. Cauchy and other theorists of elasticity directly studied the quadratic form $(1/2)e_{ij}dx_i dx_j$ that gives the change of the squared length of the segment $d\mathbf{x}$.[105]

---

[103] Ibid., 60–68; "Supplement to a memoir on some cases of fluid motion," *TCPS* (1846), also in *SMPP*, vol. 1, 188–196, on 196. On Stokes' experimental style, cf. G.D. Liveing, Appreciation in J. Larmor (ed.), *Memoirs and scientific correspondence of the late Sir George Gabriel Stokes*, 2 vols. (Cambridge, 1907), vol. 1, 91–97.

[104] Stokes, "On the theory of the internal friction of fluids in motion, and of the equilibrium and motion of elastic solids," *TCPS* (read in 1845, pub. in 1849), also in *SMPP*, vol. 1, 75–129, on 75–76; "Notes on hydrodynamics. III. On the dynamical equations" (1848), *SMPP*, vol. 2, 1–7, on 3 (cup of tea). Stokes refers to Cauchy as follows: "The method which I have employed is different from [Cauchy's], although in some respects it much resembles it" (*SMPP*, vol. 1, 78).

[105] Stokes, "On the theory..." (ref. 104), 80–84; Cauchy, "Mémoire sur les dilatations, condensations et les rotations produites par un changement de forme dans un système de points matériels," *Exercises d'analyse et de physique mathématique*, 2 (1841), 302–330, on 321 (cf. Dugas (ref. 1), 402–406). Stokes' reasoning did not seem too clear to Saint-Venant: see his letter to Stokes, 22 Jan 1862, in Larmor (ref. 103), 156–159. Larmor's comment, "The practical British method of development in mathematical physics, by fusing analysis with direct physical perception or intuition, still occasionally present similar difficulties to minds trained in a more formal mathematical discipline," does not seem to apply well to Saint-Venant, although it certainly applies to the continental perception of Larmor's own work.

Stokes then required, as Cauchy had done, the principal axes of pressure to be identical with those of deformation. He decomposed the three principal dilations into an isotropic dilation and three "shifting motions" along the diagonals of these axes. To the isotropic dilation he associated an isotropic normal pressure, and to each shift a parallel transverse pressure. In order to get the complete pressure system, he superposed these four components and transformed the result back to the original system of axes. So far, Stokes' procedure was similar to Saint-Venant's, except that Saint-Venant directly dealt with slides in the original system of axes, and did not require any superposition nor any transformation of axes.[106]

The analogy with Saint-Venant – whose communication Stokes was probably unaware of – ends here. Stokes wanted the pressures to depend linearly on the instantaneous deformations. He justified this linearity (including the above-mentioned superposition), as well as the zero value he chose for the pressure implied by an isotropic compression, by means of a somewhat obscure model of "smooth molecules acting by contact." His previous approach to the imperfect fluid had been deliberately non-molecular. The new, internal-friction approach was explicitly molecular. Surely, Stokes grew to be an overcautious physicist who would avoid microphysical speculation as much as he could. Yet, no more than his French predecessors could he conceive of internal friction without transverse molecular actions.[107]

Elastic bodies, ether, and pipes

Stokes' reasoning of course led to the Navier-Stokes equation, since this is the only hydrodynamic equation that is compatible with local isotropy and linear dependence between stress and distortion rate. After reading Poisson's memoir of 1829, which proceeded from the equations of elastic bodies to those of real fluids, Stokes tried the reverse course and transposed his hydrodynamic reasoning to elastic bodies. From the "principle of superposition of small quantities," he derived the linearity of the stress-strain relation. Then he exploited isotropy in the principal-axis system to introduce two elastic constants, one for the shifts, the other for isotropic compression. He thus retrieved the two-constant stress system that Cauchy had obtained for isotropic elastic body in his non-molecular theory of 1828. Stokes attributed Poisson's single-constant result to his assumption that the sphere of action of a given molecule contained many other molecules – which only shows that he had not read the memoir in which Cauchy proved this assumption to be unnecessary. More pertinently, Stokes argued that soft solids such as India rubber or jelly required two elastic constants, for they had a much smaller resistance to shifts than to compression. He also suggested that the optical ether might correspond to the case of infinite resistance to compression, for which longitudinal waves no longer exist. In

---

[106] Stokes, "On the theory..." (ref. 104), 83–84.

[107] Ibid., 84–86; Stokes mentioned Saint-Venant's proof in his British Association report of 1846 (ref. 95), on 183–184, with the observation: "This method does not require the consideration of ultimate molecules at all." Stokes's model implies a zero trace for the viscous stress tensor, so that his equation includes the term $(1/3)\varepsilon\nabla(\nabla \cdot \mathbf{v})$ (besides the $\varepsilon\Delta\mathbf{v}$ term) in the case of a compressible fluid.

sum, Stokes had both down-to-earth and ethereal reasons to require two elastic constants instead of one. With George Green, whose works he praised, he inaugurated the British preference for the multi-constant theory.[108]

Stokes' immediate purpose was, however, a study of the role of internal friction in fluid resistance and flow retardation. Boundary conditions are here essential. When in 1845 Stokes read his memoir on fluid friction, he already believed that a vanishing relative velocity at rigid walls was most natural. But this contradicted Bossut's and Du Buat's experiments on pipe and channel flow. Navier's and Poisson's condition that the tangential pressure at the wall should be proportional to the slip did not work any better, except for very small velocity, in which case the observed resistance and retardation both became proportional to the velocity. As Stokes knew, Du Buat had found a zero-velocity near the walls for very reduced flows. But Girard's measurements, as interpreted by Navier, seemed to require a finite slip. In this perplexing situation, Stokes refrained from publishing discharge calculations. He only gave the parabolic velocity profile for cylindrical pipes with zero-velocity at the walls.[109]

Back to the pendulum

In the pendulum case Stokes knew the retardation to be proportional to velocity, in conformity with both the Navier-Poisson boundary condition and the zero-slip condition. He also knew from a certain James South that a tiny piece of gold leaf attached normally to the surface of a pendulum's globe remained normal during oscillation. This observation, together with Du Buat's and Coulomb's small-velocity results, brought him to try the analytically simpler zero-slip condition. The success of this choice required justification. In his major memoir of 1850 on the pendulum, Stokes argued that it was "extremely improbable" that the forces called into play by an infinitesimal internal shear and by a finite wall shear would be of the same order of magnitude, as they should be for the dynamical equilibrium of the layer of fluid next to the wall.[110]

Neglecting the quadratic $(\mathbf{v} \cdot \nabla)\mathbf{v}$ terms in the Navier-Stokes equation, Stokes found an exact analytical solution for the oscillating sphere that represents the globe of the pendulum, and a power-series solution for the oscillating cylinder that represents the suspending thread of the pendulum. The results explained Sabine's mass-correction anomaly, and permitted a close fit with Francis Baily's extensive experiments of 1832. Ironically, Stokes obtained this impressive agreement with a wrong value of the viscosity coefficient. The explanation of this oddity is that his data analysis depended on the

[108] Stokes (ref. 104), sections 3–4.
[109] Ibid., 93–99; ref. 95(1846), 186. For large pipes, Stokes assumed a tangential pressure proportional to velocity squared at the walls, justified in Du Buat's and Coulomb's manner by surface irregularities.
[110] Stokes (ref. 99), 7, 14–15.

assumption that viscosity is proportional to density, at variance with the approximate constancy later proved by James Clerk Maxwell.[111]

Stokes also considered the case of uniform translation. For a sphere of radius R moving at the velocity v, he derived the expression $-6\pi\varepsilon Rv$ of the resistance, now called "Stokes' formula." He used it to obtain the modern explanation of the suspension of clouds: the resistance experienced by a falling droplet decreases much more slowly with its radius than its weight does. He also found that in the cylinder case no steady solution existed, because the quantity of dragged fluid increased indefinitely. This accumulation probably implied instability, in which case "the quantity of fluid carried by the wire would be diminished, portions being continually left behind and forming eddies."[112]

At that time, Stokes did not discuss other cases of non-linear resistance, such as a swiftly moving sphere. However, he later adopted the view that the Navier-Stokes condition with the zero-shift boundary condition applied generally, and that the non-linearity of the resistance observed beyond a certain velocity corresponded to an instability of the smooth-flow solution of the equation, leading to energy dissipation through a trail of eddies. This is essentially the modern viewpoint.[113]

7. The Hagen-Poiseuille law

Hagen's Besselian pipe-study

Stokes' pendulum memoir contains the first successful application of the Navier-Stokes equation with the boundary condition which is now regarded as correct. For narrow-pipe flow, Stokes (and previous discoverers of the Navier-Stokes equation) knew only Girard's results, which seemed to confirm the Navier-Poisson boundary condition. Yet a different law of discharge through narrow tubes had been published twice before Stokes' study, in 1839 and 1841.

The German hydraulic engineer Gotthilf Hagen was first to discover this law, without knowledge of Girard's incompatible results. Hagen had learned the discipline of precision measurement under Bessel and had traveled through Europe to study hydraulic constructions. As he had doubts on the methods through which Prony's and Johann Eytelwein's pipe-retardation formulas had been established, he performed his own experiments on this subject in 1839. In order to best appreciate the effect of friction, he selected pipes of small diameter, between 1 and 3 mm. The principle of the experiment was similar to Girard's. However, Hagen eliminated important sources of error that had escaped Girard's attention. For example, he carefully measured the diameter of his pipes

[111] Ibid., sections 2–3. On the wrong value of the viscosity coefficient, cf. Stokes, note appended to ref. 99, *SMPP*, vol. 3, 137–141; Stokes to Wolf, undated (circa 1991), in Larmor (ref. 103), vol. 2, 323–324.

[112] Stokes (ref. 99), 59, 66–67.

[113] Cf. Stokes's letters of the 1870s and 1880s in Larmor (ref. 103).

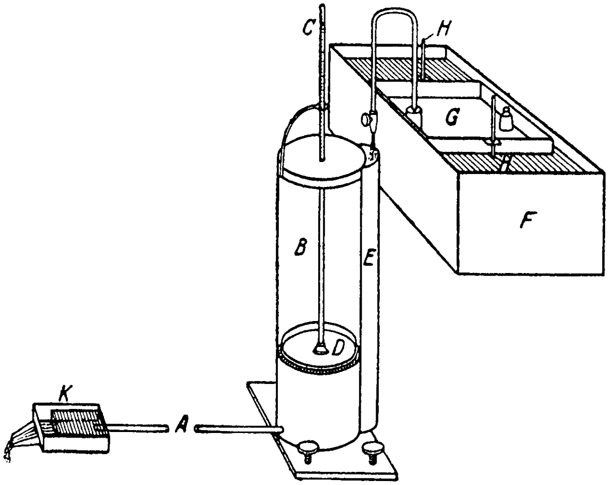


Fig. 8. Hagen's apparatus for measuring fluid discharge. The tank F feeds the cylinder B through the regulating device H. The water level in the cylinder is determined by reading the scale C attached to the floating disk D. The discharging tube A ends in the overflowing tank K. (From Hagen, ref. 114)

by weighing their water content. And he avoided the irregularities of an open-air efflux by having the pipe end in a small tank with constant water-level (see Fig. 8).[114]

Hagen first observed a surprising change in the nature of the flow for a critical pipe-flow velocity of the order $(2gh)^{1/2}$, h being the pressure head, and ρ the density:

> An essential change of the phenomena [occurs] when this limit is passed.... When I let the water flow in open air, for a small head [h] the issuing jet had a constant shape and looked like a solid glass rod; but as soon as, by increased head, the velocity exceeded the above-said limit, this jet started to fluctuate and the outflow was no longer uniform but pulsatory.

In this turbulent case, Hagen surmised that "there was no longer the tension necessary to transmit pressure." For better experimental control, he decided to operate below the critical threshold. His experimental results are summarized in the formula

$$h = \frac{1}{R^4}(\alpha L Q + \beta Q^2), \tag{49}$$

where h is the pressure head, Q the discharge, L the length, α a temperature-dependent constant, β a temperature-independent constant. In true Königsberg style, Hagen determined the coefficients and exponents by the method of least squares and provided error estimates.[115]

---

[114] G. Hagen, "Über die Bewegung des Wassers in engen cylindrischen Röhren," *AP*, 46 (1839), 423–442. Cf. L. Schiller, "Anmerkungen," in Hagen, Poiseuille, and E. Hagenbach, *Drei Klassiker der Strömungslehre* (Leipzig, 1933), 81–97, on 83–84; Rouse and Ince (ref. 7), 157–161.

[115] Hagen (ref. 114), 424, 442.

Hagen correctly interpreted the quadratic term as an entrance effect, corresponding to the live force acquired by the water when entering the tube. Assuming a conic velocity profile, he obtained a good theoretical estimate of the β coefficient. He attributed the linear term to friction, and justified the $1/R^4$ dependence by combining the conic velocity profile with an internal friction proportional to the squared relative velocity of successive fluid layers. Perhaps because this concept of friction later appeared to be mistaken, the credit for the discovery of the QL/R^4 law has often be given to Poiseuille only. Yet Hagen's priority and the excellence of his experimental method are undeniable.[116]

Dr Poiseuille's capillary vessels

Jean-Louis Poiseuille, a prominent physician with a Polytechnique education, performed his experiments on capillary-tube flow around 1840, soon after Hagen's. He had no particular interest in hydraulics, but wanted to understand "the causes for which some organ received more blood than another." Having eliminated a few received explanations in a previous memoir, he focused on the behavior of capillary vessels and decided to examine experimentally the effect of pressure, length, diameter, and temperature on the motion of various liquids through capillary glass tubes. He judged Girard's anterior measurements irrelevant, because capillary blood vessels were about hundred times narrower than Girard's tubes.[117]

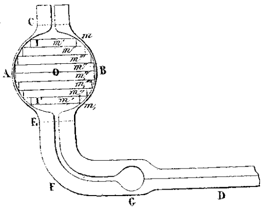
Poiseuille produced the flow-generating pressure with an air-pump and reservoir, in vague analogy with the heart of living organism (see Fig. 9). He avoided the irregularities of open-air efflux and controlled temperature by immersing his capillary tubes in a thermostatic bath. He reckoned the discharge from the lowering of the fluid level in the feeding flask. The most delicate parts of the measurements were the optical and hydraulic control of the cylindricity of the capillary tubes, and the determination of the pressure head. Like Girard, Poiseuille overlooked the entrance effect, which is fortunately negligible for very narrow tubes. But he properly took into account hydrostatic head, viscous retardation in the larger tube leading to the capillary tube, and the pressure shift in a given run. The description of his protocol was extremely meticulous, so much as to include prescriptions for the filters he used to purify his liquids. His results compare excellently with the modern theoretical expectation. They of course include the Poiseuille law $Q = KPR^4/L$ (P being the fall of pressure, and K a temperature-dependent constant).[118]

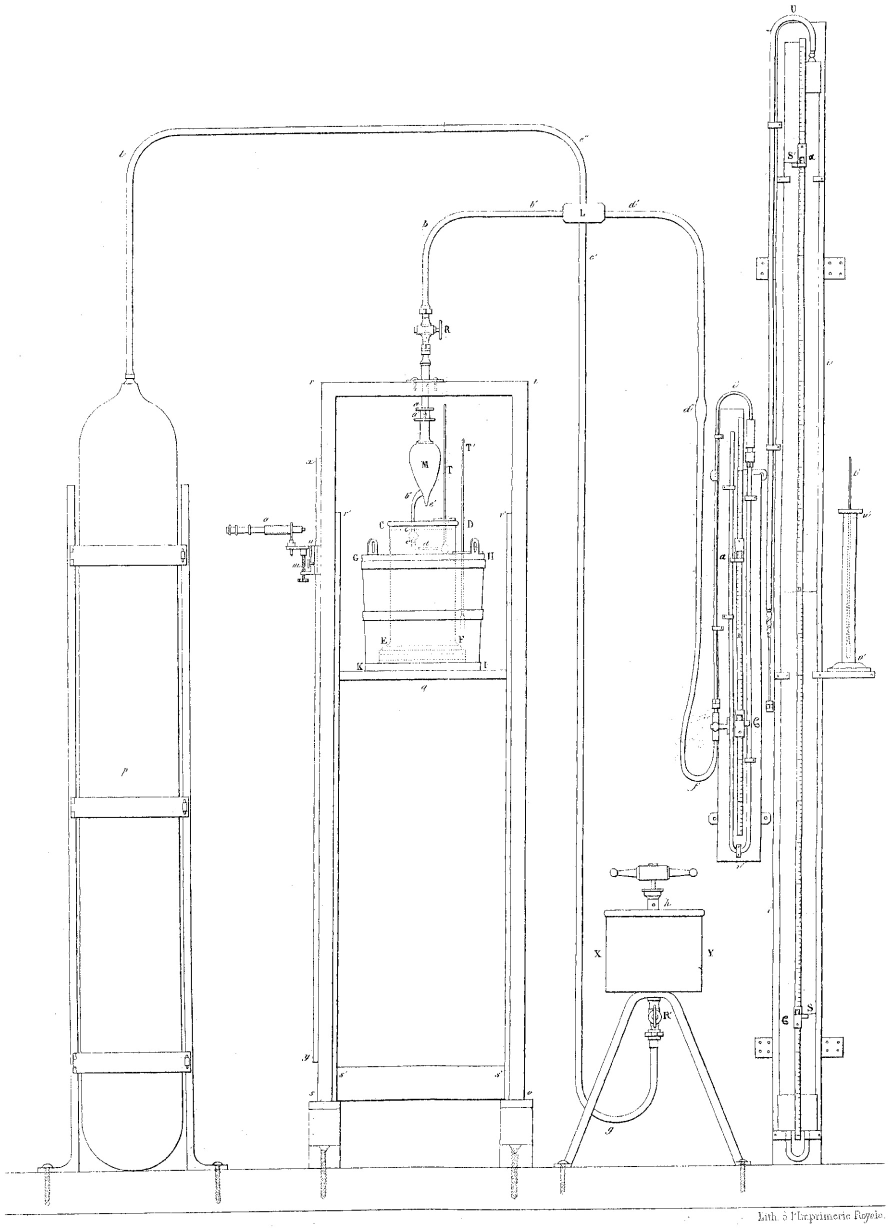
Poiseuille only mentioned Navier's theory to condemn it for leading to the wrong PR^3/L law. Unfortunately, Navier did not live enough to know Poiseuille's result. The Academicians who reviewed the physician's memoir (Arago, Babinet, and Piobert) did

[116] Ibid., 433, 437, 441.

[117] J.L. Poiseuille, "Recherches expérimentales sur le mouvement des liquides dans les tubes de très petit diamètre" [read on 14 Dec 1840, 28 Dec 1840, and 11 Jan 1841], Académie des Sciences de l'Institut Impérial de France, *Mémoires présentés par divers savants*, 9 (1844), 433–543; Report on this memoir by F. Arago, J. Babinet, and G. Piobert in *CR*, 15 (1842), 1167–1186. Cf. Rouse and Ince (ref. 7), 160–161; Schiller (ref. 114), 89.

[118] Poiseuille (ref. 117), 519. For a modern evaluation, cf. Schiller (ref. 114), 85–89.


C
A
O
B
E
F
G
D


U
L
R
M
T
T'
C
D
G
H
E
F
K
I
X
Y
R'
S
Lith. à l'Imprimerie Royale.

◀ **Fig. 9.** Poiseuille's apparatus to measure fluid discharge through capillary tubes. The reservoir P, originally filled with compressed air by the pump AXY is connected to a barometric device (on the right), and to the flask M, which in turn feeds the elaborate glass part CABEFGD. The fluid contained in the spherical bulb AB is pressed to flow through the capillary tube D into a thermostatic bath. (From Poiseuille, ref. 117)

not know that Navier had already obtained the R^4 dependence in the case of a square tube of side R with zero-shift at the walls. It was left to Franz Neumann, who had probably known Hagen in Königsberg, to give the first public derivation of the Hagen-Poiseuille law. Neumann assumed zero-velocity at the walls, made the internal friction proportional to the transverse velocity-gradient, derived the quadratic velocity profile, and integrated to get the discharge. His student Heinrich Jacobson published this proof in 1860. The Basel physicist Eduard Hagenbach published a similar derivation in the same year, with an improved discussion of entrance effects and mention of the *Erschütterungswiderstand* (agitation-resistance) that occurred for larger pipes. Lastly, the French physicist Emile Mathieu published a third similar proof in 1863.[119]

A slow integration

It would be wrong to believe that these derivations of Poiseuille's law were meant to vindicate the Navier-Stokes equation. Neumann and Mathieu did not mention Navier's theory at all. Hagenbach did, but imitated Poiseuille in globally condemning Navier's approach. Newton's old law of the proportionality between friction and transverse velocity gradient was all these physicists needed. Hermann Helmholtz may have been the first physicist to link the Navier-Stokes equation to the Hagen-Poiseuille law.

Helmholtz's interest in fluid friction derived from his expectation that it would explain a left-over discrepancy between theoretical and measured resonance frequencies in organ pipes. Like Stokes, he first studied cases of perfect fluid motion in which the departure from real fluids would be most apparent. This led him to his famous study of vortex motion, published in 1858. Then he derived a hydrodynamic equation that included internal friction. He mailed it to his friend William Thomson to ask whether it was the same as Stokes', of which he had heard without seeing it. Yes, it was.[120]

In order to determine the viscosity coefficient of liquids, Helmholtz asked his student Gustav von Piotrowski to measure the damping of the torsional oscillations of a hollow

---

[119] Ibid., 521; H. Jacobson, "Beiträge zur Hämodynamik," *Archiv für Anatomie, Physiologie und wissenschaftliche Medicin* (1860), 80–112; E. Hagenbach, "Über die Bestimmung der Zähigkeit einer Flüssigkeit durch den Ausfluss aus Röhren," *AP*, 109 (1860), 385–426; E. Mathieu, "Sur le mouvement des liquides dans les tubes de très petit diamètre," *CR*, 57 (1863), 320–324.

[120] Helmholtz to Thomson, 30 Aug 1859, Kelvin collection, Cambridge University Library; Thomson to Helmholtz, 6 Oct 1859, Helmholtz Nachlass, Akademie der Wissenschaften zu Berlin. Cf. Darrigol, "From organ pipes to atmospheric motions: Helmholtz on fluid mechanics," *HSPS*, 29 (1998), 1–51.

metallic sphere filled with liquid. Helmholtz performed the necessary integrations of the Navier-Stokes equation in order to extract the viscosity coefficient from these measurements and also from Poiseuille's older experiments on capillary tubes. The two values disagreed, unless a finite slip of the fluid occurred on the walls of the metallic sphere. When he learned about this analysis, Stokes told Thomson he rather inclined against the slip, but did not exclude it.[121]

This episode shows that as late as 1860 the Navier-Stokes equation did not yet belong to the physicist's standard toolbox. It could still be rediscovered. The boundary condition, which is crucial in judging consequences for fluid resistance and flow retardation, was still a matter of discussion. Nearly twenty years elapsed before Horace Lamb judged the Navier-Stokes equation and Stokes' boundary condition worth a chapter of a treatise on hydrodynamics. This evolution rested on the few successes met in ideal circumstances of slow or small-scale movement, and on the confirmation of the equation by Maxwell's kinetic theory of gases. Until Osborne Reynolds' and Joseph Boussinesq's turbulence studies in the 1880s, the equation remained completely irrelevant to hydraulics.[122]

Acknowledgments. I thank Jed Buchwald for useful comments and editorial assistance.

83 rue Broca
75013 Paris
France
darrigol@paris7.jussieu.fr

(Received May 4, 2001)

[121] Helmholtz and G. von Piotrowski, "Über Reibung von tropfbarer Flüssigkeiten" (1860), in Helmholtz, *Wissenschaftliche Abhandlungen*, 3 vols. (Leipzig, 1882, 1883, 1895), vol. 1, 172–222, on 195–214 (calculations in the spherical case); 215–217 (calculation for the Poiseuille flow); Stokes to Thomson, 22 Feb 1862, in David Wilson, ed., *The correspondence between Sir George Gabriel Stokes and Sir William Thomson, Baron Kelvin of Largs*, 2 vols. (Cambridge, 1990). Helmholtz was aware of Girard's measurements (ibid., 217–219), which he unfortunately trusted, but not of Hagen's.

[122] H. Lamb, *A treatise on the mathematical theory of the motion of fluids* (Cambridge, 1879), chap. 9. The verification of consequences of Maxwell's kinetic theory by viscous damping experiments required new, improved solutions of the Navier-Stokes equation: cf. W.M. Hicks, "Report on recent progress in hydrodynamics," British Association for the Advancement of Science, *Report* (1882), 39–70, on 61–70.