Michael Eckert

The Dawn of Fluid Dynamics

A Discipline between Science and Technology

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Preface

A leading representative of fluid dynamics defined this discipline as “part of applied mathematics, of physics, of many branches of engineering, certainly civil, mechanical, chemical, and aeronautical engineering, and of naval architecture and geophysics, with astrophysics and biological and physiological fluid dynamics to be added.” [1, p. 4]

Fluid mechanics has not always been as versatile as this definition suggests. Fifty years ago, astrophysical, biological, and physiological fluid dynamics was still in the future. A hundred years ago, aeronautical engineering did not yet exist; when the first airplanes appeared in the sky before the First World War, the science that became known as aerodynamics was still in its infancy. By the end of the 19th century, fluid mechanics meant hydrodynamics or hydraulics: the former usually dealt with the aspects of “ideal,” i.e., frictionless, fluids, based on Euler’s equations of motion; the latter was concerned with the real flow of water in pipes and canals. Hydrodynamics belonged to the domain of mathematics and theoretical physics; hydraulics, by contrast, was a technology based on empirical rules rather than scientific principles. Theoretical hydrodynamics and practical hydraulics pursued their own diverging courses; there was only a minimal overlap, and when applied to specific problems, the results could contradict one another [2].

This book is concerned with the history of fluid dynamics in the twentieth century before the Second World War. This was the era when fluid dynamics evolved into a powerful engineering science. A future study will account for the subsequent period, when this discipline acquired the multifaceted character to which the above quote alluded. The crucial era for bridging the proverbial gap between theory and practice, however, was the earlier period, i.e., the first four decades of the twentieth century. We may call these decades the age of Prandtl, because no other individual contributed more to the formation of modern fluid dynamics. We may even pinpoint the year and the event with which this process began: it started in 1904, when Ludwig Prandtl presented at a conference the boundary layer theory for fluids with little friction. Prandtl’s publication was regarded as “one of the most extraordinary papers of this century, and probably of many centuries" [1]—it "marked an epoch in the history of fluid mechanics, opening the way for understanding the motion of real fluids" [3].

In order to avoid any misunderstanding: this is not a biography of Prandtl, however desirable an account of Prandtl's life might be. Nor is it a hero story; I do not claim that the emergence of modern fluid dynamics is due solely to Prandtl. If Prandtl and his Göttingen circle's work is pursued here in more detail than that of other key figures of this discipline, it is because the narrative needs a thread to link its parts, and Prandtl's contributions provide enough coherence for this purpose. The history of fluid dynamics in the age of Prandtl, as presented in the following account, is particularly a narrative about how science and technology interacted with another in the twentieth century. How does one account for such a complex process? In contrast to sociological approaches I pursue the history of fluid dynamics *not* within a theoretical model of science–technology interactions. Nevertheless, the relationship of theory and practice, science and engineering, or whatever rhetoric is used to refer to these antagonistic and yet so similar twins, implicitly runs as a recurrent theme through all chapters of this book. I share with philosophers, sociologists, and other analysts of science studies the concern to better grasp science–technology interactions, but I cannot see how to present the history of fluid dynamics from the perspective of an abstract model. My own approach is descriptive rather than analytical; I approach the history of fluid dynamics from the perspective of a narrator who is more interested in a rich portrayal of historical contexts than in gathering elements for an epistemological analysis. This approach requires deviations here and there from the main alley, so to speak, in order to clarify pertinent contexts, but I am conscious not to lose the narrative thread and regard as pertinent only what contributes to a better understanding of the theory–practice issue. I postpone further reflections to the epilogue, when this issue may be better discussed in view of the empirical material presented throughout the remainder of the book.

Many people and institutions have contributed to this work. Instead of acknowledging their help here individually in the form of a long list of names, I refer readers to the notes in the appendix, where readers may better appreciate how archives and authors of other studies helped to add flesh to the skeleton of my narrative. The only exceptions concern my colleagues from the Deutsches Museum and the Munich Center for the History of Science and Technology, whom I owe thank for years of fruitful collaboration and stimulating discussions, and the Deutsche Forschungsgemeinschaft for funding the Research Group 393, which formed the framework of this study.

Michael Eckert, Munich, May 2005

1
Diverging Trends before the Twentieth Century

The flow of water or air around an obstacle is such a familiar phenomenon that we tend to underrate its importance in the history of science and technology. Throughout the centuries, the behavior of a body in a fluid was a fundamental theoretical problem and an obvious practical concern. The motion of celestial bodies, ships, projectiles, and other phenomena involved conceptions of fluid dynamics. Although the development of science from Aristotle to Einstein is usually presented without excursions into the history of fluid dynamics, concepts about motion inevitably involve assumptions about fluid resistance.

1.1
Galileo's Abstraction

In Aristotle's natural philosophy, the medium through which a motion proceeded played a paradoxical role. In order to sustain the motion, a motive agency was required. Aristotle (384–322 BC)imagined that this motive agency resided in the medium: "We must, therefore," Aristotle wrote in Book VIII of his *Physics*, "hold that the original movent gives the power of causing motion to air, or water, or anything else which is naturally adapted for being a movent as well as for being moved" [4, p. 506]. At the start of the motion of a projectile, the medium would be displaced by the projectile, and together with this displacement, a motive force would be passed along the trajectory. Thus, the medium acquired the power to propel the projectile. At the same time the medium would resist the motion: "If air is twice as tenuous as water," Aristotle argued, "the same moving body will spend twice as much time in travelling a certain path in water as in travelling the same path in air" [5, p. 21].

Aristotle dominated pre-modern natural philosophy – but some of his views also served as bones of contention. How could the same medium at the same time propel and resist the motion of a projectile? Most famous among those who criticized this concept was Jean Buridan (1300–1358), who argued that the propulsive property resided in the projectile itself rather than in the medium. He called this property impetus: “Whenever some agency sets a body in motion,” Buridan wrote, “it imparts to it a certain impetus, a certain power which is able to move the body along in the direction imposed upon it at the outset (...) It is this impetus which moves a stone after it has been thrown until the motion is at an end. But because of the resistance of the air and also because of the heaviness, which inclines the motion of the stone in a direction different from that in which the impetus is effective, this impetus continually decreases” [5, pp. 49–50]. Now the medium through which the motion proceeded was left with just one property: resistance.

The impetus concept marked the emergence of the modern notions of inertia and momentum. But that did not happen at once. Even Galileo Galilei (1564–1642), with whom we associate the revolutionary turn from the medieval philosophy to the “new science” of motion, still mixed Aristotelian concepts with modern concepts of motion. Like his predecessors, Galileo struggled with the role of the medium through which a body moves. His famous *Dialogues Concerning Two New Sciences* reveals what problems were behind the effort to imagine how a body would move without the resistive property of the medium. Galileo lets Salviati ask, for example, “What would happen if bodies of different weight were placed in media with different resistances?” The answer was presented by comparing the motion in air and water: “I found,” Salviati continues, ”that the differences in speed were greater in those media which were more resistant, that is, less yielding. This difference was such that two bodies which differed scarcely at all in their speed through air would, in water, fall the one with a speed ten times as great as that of the other” [6, p. 68].

Obviously, motion in air would be closer to a motion without any influence of the medium. But there were problems in quantitatively measuring differences for bodies with different weights in air. “It occurred to me therefore,” Galileo argues with the voice of Salviati, “to repeat many times the fall through a small height in such a way that I might accumulate all those small intervals of time that elapse between the arrival of the heavy and light bodies respectively at their common terminus.” With the repetition of the free fall, he meant the repeated swings of a pendulum:

> “Accordingly I took two balls, one of lead and one of cork, the former more than a hundred times heavier than the latter, and suspended them by means of two equal fine threads, each four or five cubits long. Pulling each ball aside from the perpendicular, I let them go at the same instant, and they, falling along the circumferences of circles having these equal strings for semi-diameters, passed beyond the perpendicular and returned along the same path. This free vibration repeated a hundred times showed clearly that the heavy body maintains so nearly the period of the light body that neither in a hundred swings nor even in a thousand will

> the former anticipate the latter by as much as a single moment, so perfectly do they keep step. We can also observe the effect of the medium which, by the resistance which it offers to motion, diminishes the vibration of the cork more than that of the lead, but without altering the frequency of either; even when the arc traversed by the cork did not exceed five or six degrees while that of the lead was fifty or sixty, the swings were performed in equal times" [6, pp. 84–85].

In order to find out how the resistance of air depends on the velocity, Galileo compared the swings of pendulums with equal weights but different amplitudes. He found that the air resistance is proportional to the velocity of the moving body [6, p. 254].

Already Galileo's contemporaries noticed that these conclusions could not have resulted from actual experiments. Marin Mersenne (1588–1648) compared the swings of equal pendulums with different amplitudes: he found that one which started swinging with an amplitude of two feet differed from one with an amplitude of one foot already after thirty periods of oscillation by as much as one full period. In 1639, a year after the publication of the *Dialogues Concerning Two New Sciences*, he remarked that if Galileo had performed real pendulum experiments and only waited for thirty or forty swings, he would have noticed the difference [7]. Recent pendulum experiments confirmed Mersenne's critique [8, 11].

This and other observations of Galileo stirred considerable debate among historians of science – to what extent did Galileo actually perform experiments? Only his pendulum experiments with small amplitude are presumed "real"; those with larger amplitudes are regarded as "imaginary" or "hypothetical," i.e., they were not performed in reality, but (contrary to mere thought experiments) are based on extrapolation from empirical observations [7]. Earlier interpretations tended to categorize Galileo's style of research into one of two extremes: either as deductive in the tradition of Platonic and idealistic natural philosophy, in which the experiment only plays a role as a confirmation of insights gained by mere thinking; or as inductive, with the experiment as the origin of new knowledge. According to more recent historical studies, however, Galileo's science was more complex and does not fit neatly into one category or the other alone [9].

The question whether Galileo actually performed free fall experiments from the leaning tower of Pisa attracted particular scrutiny [10]. As with the pendulum experiments, the problem of resistance plays an important role here, too. "Aristotle says that 'an iron ball of one hundred pounds falling from a height of one hundred cubits reaches the ground before a one-pound ball has fallen a single cubit.' I say," Salviati responds to such an obvious discrepancy with reality, "that they arrive at the same time. You find, on making the experiment, that the larger outstrips the smaller by two finger-breadths" [6, p. 64]. However, a modern calculation, which takes into account the air resistance, yields a difference of 1.05 m for the free fall of a 100-lb. iron sphere (with a radius of 11.13 cm) and a 1-lb. iron sphere (with a radius of 2.4 cm) over a distance of 100 cubits (58.4 m). The lighter sphere would be more than one meter behind the heavier one – certainly much more than the "two finger-breadths" in Galileo's argument [11]. If Galileo really performed the tower experiment, why didn't he notice this discrepancy? The puzzle can be resolved by a psycho-physical argument: when an experimenter intends to release simultaneously two different weights from his outstretched hands, the palm with the lighter weight tends to open a bit earlier than the palm with the heavier weight; this difference could have compensated for the difference due to the air resistance [12], [13, Supplement 3].

But Galileo, presumably, was rather motivated by a theoretical argument. The medium had to be "thrust aside by the falling body," Salviati argued. "This quiet, yielding, fluid medium opposes motion through it with a resistance which is proportional to the rapidity with which the medium must give way to the passage of the body." By such reasoning, Galileo related the displaced mass of the medium to the resistance: "And since it is known that the effect of the medium is to diminish the weight of the body by the weight of the medium displaced, we may accomplish our purpose by diminishing in just this proportion the speeds of the falling bodies, which in a non-resisting medium we have assumed to be equal" [6, pp. 74–75].

In other words, despite a flawed concept of fluid resistance in terms of buoyancy, Galileo arrived at his goal: the abstraction of a motion in a non-resisting medium. With a vanishing buoyancy, the resistance would vanish too. In this case, with no mass to be displaced, all bodies would fall in the same manner. Galileo's law of free fall certainly has to be rated among the most important accomplishments in the history of science, but it is erroneous to infer from Galileo's abstraction that he "had a correct notion of air resistance," as a widely read book on the history of aerodynamics has claimed [14, p. 8]. Galileo did not aim at a theory of aerodynamics; his predominant concern was Aristotle's natural philosophy. The abstraction of a motion in a non-resisting medium, perceived as a motion in which no medium had to be displaced, touched upon another ancient philosophical belief: Aristotle believed in the impossibility of a vacuum; for Galileo, it was the domain in which the laws of free fall hold. Maybe it is not an exaggeration to state that Galileo's elaborations on the medium through which a body moves only served to justify his abstraction of a motion in empty space.

Against this background it does not come as a surprise that it was a pupil of Galileo, Evangelista Torricelli (1608–1647), who is credited with presenting the first experimental evidence of a vacuum. Torricelli emptied glass tubes filled with mercury into a container, such that the openings of the tubes were not exposed to the air. Inside the inverted tube, above a remaining column of mercury, there was left an empty space, a "Torricellian vacuum." The height of the mercury column in the tube was found to depend on the ambient air pressure. Torricelli undertook these experiments with another pupil of Galileo (Vincenzio Viviani). Like Galileo himself, his pupils also were primarily interested in refuting Aristotelian dogmas. "Many have said [that vacuum] cannot happen," Torricelli wrote to another follower of Galileo after his experiment; yet, it "may occur with no difficulty, and with no resistance from nature." Thus, he refuted the dogma of a "horror vacui." He concluded, with a now famous quote: "We live submerged at the bottom of an ocean of elementary air which is known by incontestable experiments to have weight." [15, p. 84].

After Torricelli's experiment the old debate among "vacuists" and "plenists" seemed to be decided in favor of the "vacuists," but René Descartes (1586–1650) renewed the belief of a universal filling of space. He denied that Galileo's extrapolation of free fall in empty space was based on sound arguments. According to Descartes' doctrine, all natural phenomena resulted from the motions of infinitely fine weightless particles of an ether that pervaded the entire universe. The particles of ordinary matter, such as air or water, were supposed to have weight, so that their displacement by a moving body would retard its motion. In order to prevent a temporary depletion behind a moving object, the displacement of matter involved a flow around the object, which Descartes imagined as vortical. He extended his doctrine to the entire universe. The solar system was supposed to be an enormous vortex of matter, in which the planets orbited as smaller vortices around the center [16].

Descartes did not produce quantitative results – neither for his cosmogony nor for the domain of earthly physics. Once, he communicated in a letter a formula about the retardation of a free falling body in a medium, whereby the speed approached a limit in the form of an infinite geometrical series, but he did not provide a physical argument for this result [23, p. 110]. Nevertheless, he exerted a remarkable influence on seventeenth century natural philosophy. Christiaan Huygens (1629–1695) pursued several of Descartes' ideas, such as the concept of an attracting force due to vortical motion around a center. Such a force would keep a planet embedded in a vortex in his orbit around the sun. In order to illustrate this force, Huygens arranged a little sphere in a cylindrical vessel filled with water such that it was free to move in a radial direction only. When the vessel was rotated around its axis, the sphere moved inwards against the centrifugal force [16, pp. 76–77].

1.2 Hogs' Bladders in St. Paul's Cathedral

Descartes' concepts of motion also influenced Isaac Newton (1643–1727), but as an opponent rather than as a follower. Alluding to Descartes' *Principia Philosophiae*, Newton titled his own three-volume treatise on mechanics *Philosophiae Naturalis Principia Mathematica*. The first volume with "Newton's laws of motion" for a body in a vacuum is celebrated as the foundation of classical mechanics. It is less known that Newton also spent a lot of time developing the laws of motion for a body in a fluid. The entire second volume is dedicated to this problem. It was regarded as "the most original part of the whole work, though also largely incorrect" [17, p. 167]. As for Galileo and Descartes, the debate among "vacuists" and "plenists" was also a major issue for Newton. One of his pupils, Henry Pemberton, wrote in 1728 a book titled *A View of Sir Isaac Newton's Philosophy* about the second volume of Newton's *Principia*: "By this theory of the resistance of fluids, and these experiments our author decides the question so long agitated among natural philosophers whether the space is absolutely full of matter. The Aristotelians and Cartesians both assert this plenitude; the Atomists have maintained the contrary. Our author has chosen to determine this question by his theory of resistance" [18, p. 314].

If the universe were filled with a material substance, as taught by Descartes and his school, then the planets would encounter a resistance along their orbits around the sun. Descartes' vortex conception could not escape that fundamental problem and therefore would have given rise to contradictions if it had been formulated in a quantitative manner. Newton presented an alternative concept with his theory of universal gravitation, which assumed an empty space between the celestial bodies–or a "bodiless" medium that would not exert a noticeable resistance: "And therefore the celestial spaces, thro' which the globes of the Planets and Comets are perpetually passing towards all parts, with the utmost freedom, and without the least sensible diminution of their motion, must be utterly void of any corporeal fluid, excepting perhaps some extremely rare vapours, and the rays of light." This was Newton's conclusion at the end of the section "Of the motion of fluids and the resistance made to projected bodies" [19, vol. 2, proposition 40, pp. 161–162].

From the outset, Newton assumed: "In mediums void of all tenacity, the resistances made to bodies are in the duplicate ratio of the velocities." Galileo's relation "that the resistance is in the ratio of the velocity," according to Newton, was "more a mathematical hypothesis than a physical one" [19, vol. 2, proposition 4, p. 11]. Others had already made the same assumption of a quadratic velocity dependence, which seemed to be more in agreement with empirical observations (see below); but Newton was the first natural philosopher who attempted to justify this relation on the basis of a physical model.

His concept is too complex for a short summary. It may suffice to hint at Newton's argument for an "elastic fluid" like air, which he conceived as a gas of particles. Based on certain assumptions about the mutual collisions of these particles, Newton obtained quantitative results about the resistance of such a fluid. "But whether elastic fluids do really consist of particles so repelling each other," he concluded, "is a physical question. We have here demonstrated mathematically the property of fluids consisting of particles of this kind, that hence philosophers may take occasion to discuss that question" [19, vol. 2, proposition 23, theorem 18, p. 79]. Newton explicitly envisioned different sources of resistance, "as from the expansion of the particles after the manner of wool, or the boughs of trees, or any other cause, by which the particles are hindered from moving freely among themselves; the resistance, by reason of the lesser fluidity of the medium, will be greater than in the corollaries above" [19, vol. 2, proposition 34, theorem 17, p. 117]. He also developed a notion of viscosity: "The resistance, arising from the want of lubricity in the parts of fluid, is, ceteris paribus, proportional to the velocity with which the parts of the fluid are separated from each other" [19, vol. 2, proposition 51, p. 184].[1]

Newton did not content himself with establishing theorems. "In order to investigate the resistances of fluids from experiments, I procured a square wooden vessel (...) this I filled with rain-water: and having provided globes made up of wax, and lead included therein, I noted the times of descents." Thus, Newton described the beginning of a series of experiments on fluid resistance. He used a pendulum with an oscillation period of a half-second for the measurement of time, and meticulously compared the various outcomes with his theoretical formulae: "Three equal globes, weighing 141 grains in air and 4 3/8 in water, being let fall several times, fell in the times of 61, 62, 63, 64 and 65 oscillations, describing a space of 182 inches," he described one of these experiments. "And by the theory they ought to have fallen in 64 1/2 oscillations, nearly." He noticed that sometimes "the globes in falling oscillate a little" and believed that for this reason the resistance was "somewhat greater than in the duplicate ratio of the velocity." But in general he regarded the outcome as an experimental verification of his square law formula for the resistance of a "globe moving though a perfectly fluid compressed medium." After a series of 12 experiments he concluded "that the theory agrees with the phaenomena of bodies falling in water; it remains that we examine the phaenomena of bodies falling in air" [19, vol. 2, proposition 40, pp. 145–155].

1) In modern terms, this is equivalent to a linear relation between shear stress and strain rate: we call fluids with such viscous behavior "Newtonian." However, Newton did not investigate the relation between stress and strain. See [20, pp. 258–259].

In order to verify his theory of the resistance of a spherical body moving in air, Newton, like Galileo, first performed pendulum experiments. He suspended a sphere by a fine thread on a hook, then varied the diameters and materials of the sphere, as well as the lengths of the thread. According to his theory, the resistance was proportional to the square of the velocity of the sphere, and this is what he "nearly" observed. But he could not account for the additional resistance of the thread "which was certainly considerable." He also compared the oscillations of the pendulum in air with those in water, but found the outcome not reliable because the vessel in which the water was contained was not large enough so that "by its narrowness [the vessel] obstructed the motion of the water as it yielded to the oscillating globe." Even less conclusive were pendulum experiments in mercury. "I intended to have repeated these experiments with larger vessels, and in melted metals, and other liquors both cold and hot: but I had not leisure to try all," Newton admitted [19, vol. 2, proposition 31, pp. 95–110].

Newton hoped to obtain more reliable measurements of air resistance with free fall experiments: "From the top of St. Paul's church in London in June 1710 there were let fall together two glass globes, one full of quicksilver, the other of air." The two spheres traversed a height of 220 English feet (67 m) before they shattered into pieces on the cathedral's floor. They were released by a sophisticated trapdoor-mechanism which ensured their simultaneous begin of fall. The time was measured by a pendulum with a period of oscillation of one second. The experiment was repeated several times with varying weights. The spheres filled with mercury had a diameter of 0.8 inches; those filled with air were between 5.0 and 5.2 inches in diameter. The time of free fall was 4 s for the heavier spheres and between 8 and 8.5 s for the lighter ones. (In a vacuum the time would have been 3.7 s.) In order to compare these results with his theory, Newton compared the experimental height of free fall with the distance they would have traversed within the measured time according to his formula. Both distances differed by less than 11 feet [19, vol. 2, proposition 40, pp. 155–157].

Newton mentioned in his *Principia* yet another series of free fall experiments from a somewhat greater height in St. Paul's cathedral: "Anno 1719 in the month of July, Dr. Desaguliers made some experiments of this kind again, by forming hogs' bladders into spherical orbs; which was done by means of a concave wooden sphere, which the bladders, being wetted well fist, were put into. After that, being blown full of air, they were obliged to fill up the spherical cavity that contained them; and then, when dry, were taken out. These were let fall from the lantern on the top of the cupola of the same church; namely from a height of 272 feet." For comparison, a leaden sphere was let fall down at the same time. The air filled hogs' bladders had diameters of about 5 inches and required about 20 s to fall down; the leaden spheres, by contrast, reached the ground in 4 1/4 s. Newton also reported about phenomena which delayed the free fall by as much as a whole second sometimes, because "the bladders did not always fall directly down, but sometimes fluttered a little in the air, and waved to and fro as they were descending." One bladder "was wrinkled, and by its wrinkles was a little retarded." Nevertheless, he found that the results agreed much better with his theory than nine years ago: "Our theory therefore exhibits rightly, within a very little, all the resistance that globes moving either in air or in water meet with; which appears to be proportional to the densities of the fluids in globes of equal velocities and magnitudes" [19, vol. 2, proposition 40, pp. 157–159].

In modern terminology, Newton's formula for the resistance of a fluid is expressed as $\sim \rho D^2 v^2$, with ρ representing the density of the fluid, D the diameter of the sphere, and v its velocity. This has become known as "Newton's square law" and has been established as a valid description of fluid resistance for a wide range of flow regimes. However, although Newton's experiments seemed to corroborate this law, they bear little evidence for Newton's theory because only one quantity, the time of free fall, was observed. The particle model gave rise to contradictory results when applied to bodies of different shape in fluids such as air and water. In air, a "rare medium, consisting of equal particles freely disposed at equal distances from each other," the resistance of a sphere would be half of that of a cylinder with the same radius moving in the direction of it axis. In water, "a compressed, infinite, and nonelastic fluid," would both experience the same resistance [19, vol. 2, propositions 34 and 37, pp. 117, 135, and 141]. If Newton had compared experimental results of spheres and cylinders, he would have noticed a contradiction with his theoretical results. Similarly, if he had calculated by the same reasoning the air resistance of a flat plane oriented at an oblique angle to the flow of air, he would have found a result proportional to the square of the sine of the angle of incidence. Newton did not perform such a calculation, but among aerodynamicists "Newton's sine square law" became famous as an erroneous formula for the lift of a wing. At the beginning of the nineteenth century, this formula was even used to demonstrate the impossibility of flying, and later aerodynamicists blamed Newton for having delayed aviation at least for half a century [18, p. 311].

1.3 Ballistics

Beyond its pertinence to natural philosophy, the resistance of a body in a medium had always been a practical problem. Since antiquity, understanding the trajectory of projectiles was an obvious challenge for natural philosophers as much as for practically minded men. It was part of a science named "ballistics" (derived from the Greek word *βαλλειν*, to throw). Niccolò Tartaglia (1499–1557), a mathematician with some experience in military affairs, described the knowledge of his epoch on ballistics in a treatise *Nova scientia*. This work exerted some influence on Galileo, who spent considerable time in his youth coming to grips with ballistics problems. In Tartaglia's treatise, one could read, for example, that a projectile traveled the farthest when fired at an angle of 45 degrees; but the trajectory was no parabola: on a horizontal plane, the distance between the vertex of the projectile and the site of its impact on the ground was always shorter than the distance between the origin of its trajectory and the vertex, as depicted in Fig. 1.1. Initially, he adopted the Aristotelian belief that a trajectory starts out straight, but in a subsequent work, he argued that the trajectories are curved everywhere [23, 24].

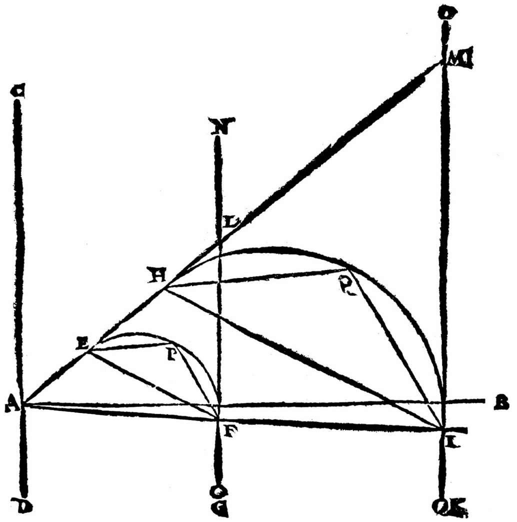

Fig. 1.1 Tartaglia imagined that a projectile's trajectory starts out straight due to the "violent" motion impressed by the shot; it is followed by a curved mixed motion, and finally becomes "natural" [21, p. 38].

As recent studies of Galileo's manuscripts have shown [25], Galileo described the trajectory of a projectile in a vacuum as a parabola *before* he arrived at his law of free fall – not the other way around, as a deductive approach would suggest. The parabola emerged in 1592, when Galileo lectured on military technology at the University of Padua. Based on his conviction that the air resistance did not exert an appreciable effect, Galileo assumed a symmetric trajectory – in contrast to Tartaglia's more realistic descriptions of asymmetric trajectories. But when Torricelli derived ballistic tables based on parabolic trajectories in 1644, an artillery officer uttered doubts: he wrote to Torricelli that if it were not for the authority of the great Galileo, whom he revered, he would not believe that the motion of projectiles is parabolic. Torricelli admitted that if there are discrepancies, one should find out what caused them, but with the experimental and theoretical tools available at the time – a hundred years before the advent of calculus – such efforts were futile. The correspondence between the practical artillery officer and the theorist (Torricelli was court mathematician at the Medicis) ended without a tangible result shortly before Torricelli died in 1647 [26].

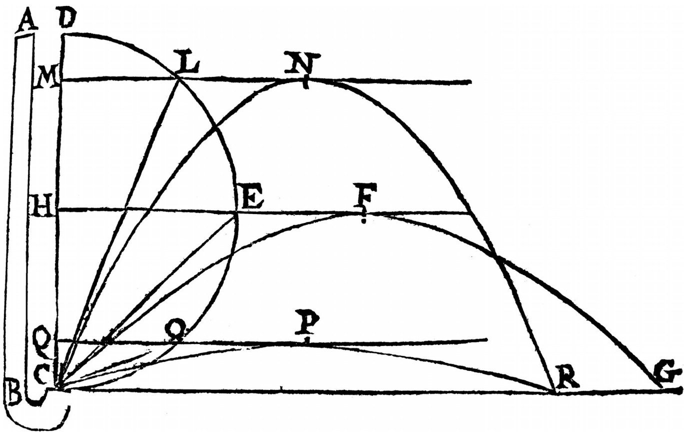

Fig. 1.2 Like the jets of a fountain [22, p. 325], ballistic trajectories were assumed to be parabolic.

Throughout the seventeenth century it was fashionable for practical gunners to assume, like Galileo and Torricelli, that the parabola is the true trajectory of a projectile – despite air resistance. Francois Blondel (1617–1686), a field marshal of the Royal French Army, published a treatise on *L'Art de Jetter les Bombes* in which he addressed the problem of air resistance but assumed that it could be neglected. He pointed to fountains with their nearly parabolic jets as evidence for this assumption – see Fig. 1.2. Another treatise on *The Genuine Use and Effects of the Gunne*, published in 1674, also claimed that air resistance is negligible and, therefore, a parabola describes the real trajectory of a projectile. It is ironic that those who believed that air resistance does exert a considerable influence and that the resulting trajectory is different from a parabola were not the gunners with experience with "real" trajectories, but men like Huygens and Newton, who based their arguments on mathematics rather than practical observations. Newton derived from the square law for air resistance that a parabolic trajectory would require that the air density not be constant but become negative along part of its trajectory. A hyperbolic trajectory would not result in such a blatant contradiction. Therefore, "It is evident that the line which a projectile describes in a uniformly resisting medium, approaches nearer to these hyperbola's than to a parabola" [23, pp. 120–127, 140–141].

In retrospect, it is not astonishing why ballistic theory and practice diverged to such an extent before the eighteenth century. Only in 1742, with the treatise by Benjamin Robins (1707–1751), *New Principles of Gunnery*, did a method become known by which it was possible to measure the velocity of a projectile in the beginning of its trajectory: the ballistic pendulum (see Fig. 1.3).

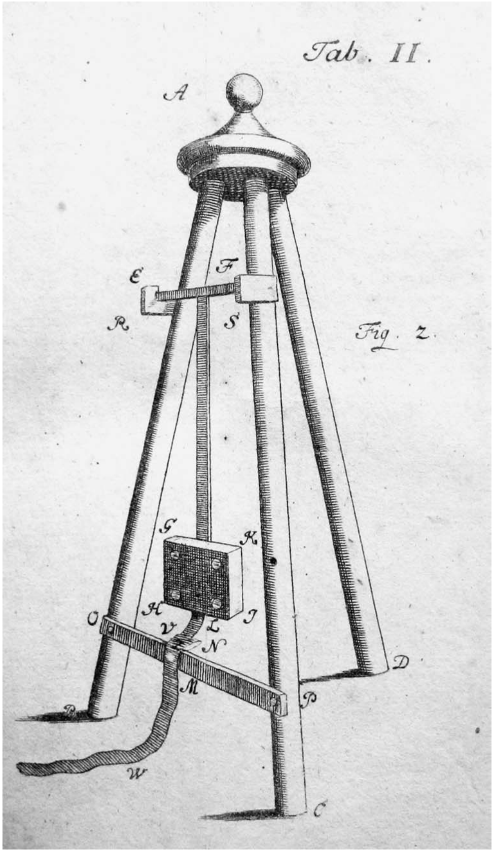

Fig. 1.3 Robins's ballistic pendulum.

Robins's innovative contributions to experimental ballistics made him famous as "Father Gunnery." He experimented with projectiles that left his gun with velocities as high as 1,700 feet per second (559 m/s). If such a projectile, when fired at an angle of 45 degrees, would follow a parabolic trajectory, it would hit the ground 17 miles away – in contrast to an actual range of only about half a mile. Projectiles with such a high starting velocity obviously experienced an enormous resistance if their range was so much shorter. "The track described by the flight of shot or shells is neither a parabola," Robins concluded, "nor nearly a parabola, unless they are projected with small velocities." Robins also invented a whirling arm technique to measure the air resistance of objects with small velocities. Based on his experiments, he found "that all the theories of resistance hitherto established, are extremely defective" [27, pp. 153–154]. But Robins's mathematical abilities were limited. It was left to Leonhard Euler (1707–1783), in his German translation of Robins's *New Principles of Gunnery*, to elaborate a theory of ballistic trajectories [20, pp. 211–220].

Besides Euler, mathematicians and natural philosophers like Johann Bernoulli (1667–1748) and Jean le Rond d'Alembert (1717–1783) became deeply engaged in ballistic calculations. The problem to find a projectile's trajectory became a proving ground for the newly developed calculus.[2]

1.4 D'Alembert's Paradox

The same eighteenth century thinkers who had recognized that air resistance posed a serious problem for calculating the trajectory of a projectile also formulated the laws of motion for ideal, i.e., inviscid, fluids, and found a strange result. D'Alembert published a treatise in 1768 titled "Paradoxe proposé aux Géometres sur la Résistance des Fluides" in which he asserted that a body moving through an ideal fluid does not experience a resistive force. D'Alembert, from his efforts in ballistics, knew about the practical importance of air resistance, but neither he nor other theorists were able to derive Newton's square law or any other law of fluid resistance from the laws of mechanics. In retrospect, d'Alembert's paradox does not appear so paradoxical because it was derived under the assumption of an inviscid fluid. Yet, it is difficult to understand why the displacement of the fluid does not involve a force. Euler had expressed this strange result many years before d'Alembert, after whom the paradox finally became named. If each fluid particle flowed around the body in such a way that it maintained the direction it had when it was in front of the body, argued Euler in one of his comments to Robins's *New Principles of Gunnery*, then there is no net force "and the body would not experience any resistance" [20, p. 245].

D'Alembert's paradox, therefore, should have entered the history of fluid mechanics more appropriately as the "Euler–d'Alembert paradox." Both had approached these problems as theorists. Their mutual relation was often one of fierce rivalry, which gave rise to some legendary stories. D'Alembert's rep-

2) The major problem, however, was due to yet unknown physical processes rather than an unavailability of mathematical tools. At the high (usually supersonic) velocities of projectiles fired from cannons and guns, the density of the air around the moving body is no longer constant. The study of air resistance in varying air density had to await twentieth century gas dynamics.

utation has been overrated, claimed one historian of mechanics, while Euler's role in this history was not appreciated enough.

Despite his theoretical leanings, Euler was very open-minded about practical problems. Nevertheless, his practical work was regarded largely as a failure. When Frederick the Great, king of Prussia, gave orders to decorate his Royal Garden of Sanssouci with water art, Euler became involved with hydraulic calculations about pumps and pipes required to raise water into an elevated water reservoir from where it was supposed to feed the fountains in the park. The king, however, never came to enjoy a fountain. He blamed Euler for having failed miserably: "My mill was constructed mathematically, and it could not raise one drop of water to a distance of fifty feet from the basin. Vanity of Vanities! Vanity of mathematics." Based on this passage, historians concluded "The mathematical genius Euler was a second-rate physicist," or "Euler's theory was not applicable for practical ends." This is how Euler is seen in the history of science – as a prime example of the proverbial schism between theory and practice. However, although it is true that the water art constructions in the Royal Garden of Sanssouci were abandoned unfinished in the lifetime of Frederick the Great, this fact was not Euler's fault, but was the result of the king's stinginess. He employed cheap laborers who had no experience with such work and who completely ignored Euler's hydraulic advice, which could have prevented the sad outcome. Euler conceived a theory of pipe flow that explained why the pipes always burst before water was raised to the elevated reservoir: as a consequence of the pumping action, which accelerated the water through the pipes, the walls of the pipes had to sustain a much higher water pressure than expected from the height difference between the pumps and the reservoir [28].

This was not the first incidence that a study in fluid flow was motivated by problems with water art. The science of moving water was among those specialties that were met with the greatest interest from Royal Academies. One outstanding work on hydraulics, which resulted from the patronage of the Paris Académie Royale des Sciences, is Edme Mariotte's *Traité de Mouvement des Eaux*, published in 1686. Mariotte and other academy members performed experiments investigating the speed with which water is ejected from a pipe, the principles of raising water, the height of water jets, and the resistance of a body as a function of the flow velocity. The motivation to undertake such experiments came from ambitious projects of water constructions, such as the canals all across France and the plans for the Royal Park at Versailles, where the world's most sophisticated water art was established for the pleasure of the Sun King and his court. The flow of water in open canals and in closed pipes became the subject matter of intensive study. The law of energy conservation in fluids, Bernoulli's equation, was formulated in the context of pipe flow by Johann Bernoulli (1667–1748) and his son Daniel Bernoulli (1700–

1782). Like Euler, the Bernoullis are mainly renowned for their mathematical work, but as is evident from the father's *Hydraulica* (1732) and the son's *Hydrodynamica* (1738), their work was motivated to a large extent by practical concerns of contemporary water art. In the age of Euler and the Bernoullis, the notions of hydrodynamics and hydraulics were used almost synonymously, often with an emphasis on the "art of raising water" and "the several machines employed for that purpose, as siphons, pumps, syringes, fountains, jets d'eau, fire-engines, etc.," according to a contemporary dictionary [29].

After he had established and solved the equations of fluid motion for the special case of pipe flow, Euler formulated the general equations of motion for inviscid fluids. They were published in 1755 under the title "Principes généraux du mouvement des fluides"; with "Euler's equations," as they were called, fluid mechanics was based on a firm theoretical foundation. Although these equations are valid for ideal fluids only, which inevitably involves d'Alembert's paradox, a number of practical problems can still be solved on that assumption.

1.5 New Attempts to Account for Fluid Friction

In 1822, Claude Louis Marie Henri Navier (1785–1836) added a term to Euler's equations, which turned them into equations of motion for viscous fluids. A few years later, Siméon-Denis Poisson (1781–1840) arrived at the same result. Other contributors to this new formulation of the theory of fluid flow are Augustin Louis Cauchy (1789–1857) and Barré de Saint-Venant (1797–1886). But only in 1845 did George Gabriel Stokes (1819–1903) present a valid derivation for the Navier–Stokes equations, as they became known. The earlier theories of Navier and Poisson were based on hypotheses of atoms which, from a modern perspective, have to be dismissed as wrong, illustrating "a common phenomenon in the history of science: Falsehood $\Rightarrow$ Truth," commented a twentieth century expert on fluid mechanics and historian of mechanics on the gradual emergence of the Navier–Stokes equations, but then the treatise of Stokes appeared as "a burst of sunlight" [30, p. 316].

It is not accidental that it was mostly scientists in post-revolutionary France who paid so much attention to the mechanics of continuous media – not only fluid mechanics but also elasticity theory – in the early nineteenth century. This interest was rooted in the Laplacian program, in which all phenomena in nature were believed to be explainable in terms of an attraction or repulsion of particles. This program emerged in the tradition of Newton's natural philosophy: inspired by the model of celestial mechanics, central forces were believed to govern phenomena on a large scale as much as they do on the scale of atoms [31]. However, there was little unanimity on how to pursue this program: Navier was adhering the school of "analytical mechanics," in contrast to another faction which headed for a more "physical mechanics" approach. Institutionally these traditions were rooted in l'École Polytechnique and the special engineering schools, l'École des Mines and l'École des Ponts et Chaussées. Navier, for example, had studied at l'École Polytechnique and at l'École des Ponts et Chaussées before he became a professor himself at these institutions. From a sociological perspective, his career was described as an early example of a "hybrid career," where the realms of science and technology became entangled [32].

Stokes's effort to account for friction was also initially based on assumptions about "ultimate particles", but he became aware that his conclusions did not depend on such assumptions [33]. Like Navier, Stokes was primarily a theorist, but in contrast to Navier, he was not affiliated institutionally with engineering. As a professor at the University of Cambridge, Stokes had no official research interests devoted to experimental or technological studies. Nevertheless for Stokes "mathematics was the servant and assistant, not the master." His approach was described in an obituary: "His guiding star in science was natural philosophy. Sound, light, radiant heat, chemistry, were his fields of labour, which he cultivated by studying properties of matter with the aid of experimental and mathematical investigation" [34].

For Stokes, like for other nineteenth century natural philosophers, hydrodynamics was a specialty where fundamental questions about the constitution of matter sometimes went hand in hand with practical problems. This dual orientation, which led to the Navier–Stokes equations, is also apparent in the derivation of what is known as Stokes's law: a sphere of radius a moving with a constant velocity V in a fluid of viscosity μ experiences a resistance $6\pi\mu aV$. Stokes arrived at this result by simplifying the Navier–Stokes equation so that terms involving the square of the velocity were neglected. It was published in 1850 in a paper titled "On the Effect of the Internal Friction of Fluids on the Motion of Pendulums" [35, vol. 3, 1–141].

The relation to pendulums hints at the practical context that motivated this study: from Galileo via Huygens and Newton until the nineteenth century, the pendulum was the preferred instrument to measure time, but the precision that could be obtained theoretically was, in the true sense of the word, dampened by air resistance. When Stokes started to analyze the potential reasons why the swings of a pendulum would slow down, he investigated the buoyancy that the sphere at the end of a pendulum experiences in a medium as a primary cause. A second cause was the dynamic effect of the displacement of the medium, which resulted in an apparent increase of the inertia of the sphere. Stokes concluded "that the mass which we must suppose added to that of the pendulum is equal to half the mass of the fluid displaced." With regard to friction, it was unclear to what extent the density played a role.[3] Experiments commissioned by the Board of Longitude had shown that the resistance depended both on the density and composition of the gas in which the pendulum swung. In practice, medium-related influences for a pendulum designed for a certain period of oscillation were accounted for in terms of correction factors for the ideal length of a pendulum in vacuum. There were numerous theoretical and experimental studies in order to determine such correcting factors. Stokes cited studies performed by the German astronomer Friedrich Wilhelm Bessel (1784–1846) or the Frenchman Louis Gabriel Dubuat (1732–1787), whose research had been largely ignored by those interested in pendulum clocks, as Stokes argued, "probably because such persons were not likely to seek in a treatise on hydraulics for information connected with the subject of their researches. Dubuat had, in fact, rather applied the pendulum to hydrodynamics than hydrodynamics to the pendulum." The same may be said about Stokes. His goal was to derive an "index of friction," by which the experimentally determined correction factors for pendulums used for precise measurement of time could be understood in terms of hydrodynamics.

Stokes's law was of interest far beyond its original pendulum context. Stokes argued, for example, that the resistance of the water droplets in a cloud may be estimated from his law. "The terminal velocity thus obtained is so small in the case of small globules such as those of which we may conceive a cloud to be composed, that the apparent suspension of the clouds does not seem to present any difficulty," he argued. "The pendulum thus, in addition to its other uses, affords us some interesting information relating to the department of meteorology" [35, p. 10].

Stokes had also sketched another application which could be analyzed by the Navier–Stokes equations: he derived a formula for the velocity profile of a fluid in a tube. If one assumes that the velocity is zero at the inner wall of the tube (which Stokes mentioned as a possible assumption but did not pursue), one finds a parabolic increase of the velocity towards the tube's center. Integration over the tube's cross section yields the total flow as proportional to r^4 (with r being the radius of the tube), or the resistance per unit length as proportional to $1/r^4$. This law was found earlier from experiments by the German hydraulic engineer Gotthilf Hagen (1797–1884) and the French physiologist Jean Louis Poiseuille (1797–1869); it became known as the Hagen–Poiseuille law. Hagen's experiments were performed with metal tubes with a diameter of a few centimeters and were motivated by practical considerations concerning the design of water pipelines. Poiseuille experimented with

3) Stokes assumed that the viscosity μ is proportional to $\rho\mu'$, where μ' is the "index of friction," and ρ is the density of the medium; it was later shown by Maxwell that contrary to Stokes's assumption, the viscosity is independent of the density and therefore, the density does not enter into the formula of Stokes's law.

glass tubes with a diameter of only a tenth of a millimeter; he aimed at a better understanding of blood circulation [36, 37].

The theoretical explanation of the Hagen–Poiseuille law was published in 1860 in a physiological as well as a physical context, the former in the *Archiv für Anatomie, Physiologie und Medizin* and the latter in the *Annalen der Physik*. What is remarkable about these publications is that the result stemmed from such diverse disciplines – physiology and physics—which seems to indicate that after the Navier–Stokes equations were formulated and the first applications appeared, theory and practice would grow closer together. However, this was not the case. Hydrodynamics became an ever more theoretical science and hydraulics a specialty for practical men. The interest in the theory of ideal fluids did not fade away but further increased when mathematicians and physicists explored new avenues of fluid behavior in the second half of the nineteenth century.

1.6 Revival of Ideal Fluid Theory

Despite d'Alembert's paradox, there is an influence upon the motion of a body in an ideal fluid that is due to the displacement of the fluid. In 1852, the mathematician Gustav Lejeune Dirichlet (1805–1859) investigated this influence through a novel analysis of Euler's equations. He wondered whether there were specific motions in which a resistance in an ideal fluid was theoretically possible. Dirichlet analyzed the case of a sphere in a uniformly accelerated fluid. He found that the sphere experiences a constant force proportional to the ratio of the densities of the fluid and the sphere, and to the accelerating force. This "resistance" was independent of the momentary flow velocity and disappeared with a vanishing acceleration, so that for the case of uniform motion, d'Alembert's paradox was established. Dirichlet's "resistance" had nothing to do with friction but was a mere inertial effect due to the displacement of fluid by the solid body, as analyzed by Stokes in his pendulum motion experiments. It was most conspicuous when expressed in terms of the kinetic energy: compared with motion in a vacuum, the kinetic energy of the sphere in the fluid was as if the motion involved an increased mass of the sphere. That mass corresponded to the mass of the fluid which had to be displaced by the sphere [38].

Although Dirichlet's result was derived from ideal fluid theory, it was important for the understanding of fluid resistance in real fluids because it showed how to discern forces due to inertial effects from friction. One is tempted to conclude that his result stemmed from efforts to learn more about the differences between ideal and real fluids, but that was not Dirichlet's mo-