CHAPTER III

Experimental Physics

By the end of the Enlightenment, experimental physics had come to mean the use of a quantitative, experimental method to discover the laws governing the inorganic world. The original meaning of the term *physics,* however, had been quite different; and as a result the word continued to be used ambiguously throughout the eighteenth century. The discipline of physics had originally been created by Aristotle, and it had nothing to do with experiment or quantitative measure, nor was it limited to the inorganic world. Aristotle's *Physics* treated form, substance, cause, accident, place, time, necessity, and motion through a priori arguments that could then be used to explain the phenomena of the world, both organic and inorganic. In fact Aristotle was more successful in his description of the animal world (also part of physics) than he was in his writings on cosmology or terrestrial motion.

Experiment was almost unknown in antiquity. An experimental tradition did begin in Western Europe during the Renaissance, but it was called "natural magic," not physics. There was also a tradition of applied mathematics, but it was not physics either. It was called "mixed mathematics." During the seventeenth century, physics, as part of speculative philosophy, continued to be taught in the schools in Latin, whereas mathematics, a practical subject with mostly military applications, was taught in the vernacular. Descartes, for example, graduated from college with the impression that mathematics was useful only in the mechanical arts.

Jean d'Alembert argued in the "Preliminary discourse" to the *Encyclopédie* that mathematics was basic to all of physics:

> The use of mathematical knowledge is no less considerable in the examination of the terrestrial bodies that surround us [than it is in astronomy].

> All the properties we observe in these bodies have relationships among themselves that are more or less accessible to us. The knowledge or the discovery of these relationships is almost always the only object that we are permitted to attain, and consequently the only one that we ought to propose for ourselves.[1]

Thus, for the mathematically inclined, experimental physics had value only to the extent that its laws could be made quantitative. The importance of mathematics for experimental physics was debated throughout the century. Diderot, the Comte de Buffon, and even Benjamin Franklin (1706–90) condemned the excessive use of mathematics in physics, claiming that it led the scientist away from nature and into a false reliance on abstract forms. The Marquis de Condorcet sided with d'Alembert and claimed that except for the mathematicians, nobody at the French Academy of Sciences was doing any useful work. It was all worthless "physicaille" – parlor tricks and experimental busywork that led nowhere. The debates were about the proper balance between experiment and mathematics. Both were essential for arriving at knowledge, and both were regarded as being in the province of reason. As Voltaire argued in his *Philosophical letters,* the English philosophers Bacon (1561–1626), Locke, and Newton had shown convincingly that knowledge about the physical world could not be obtained from first principles without resort to experiment. Reason dictated a middle course, combining experiment with quantitative measure to allow a constant check on theory.

There had, of course, been famous experimenters in the seventeenth century: Edmé Mariotte (d. 1648) in France, Robert Boyle in England, and Newton, who had combined experiment and mathematics in an especially impressive way in his *Opticks* of 1704. Although Newton was admired both by Continental scientists and by his own countrymen, it was on the Continent, especially in Holland, that natural philosophers elaborated on the experimental Newtonian method. With the installation of William of Orange on the throne of England in 1688, intellectual contact between England and Holland increased. Beginning in 1715, Hermann Boerhaave, Willem 'sGravesande, and Pieter van Musschenbroek, all at the University of Leiden, followed Newton's lead in organizing experiments. Boerhaave, who became a famous doctor and chemist, initiated the Dutch program in his oration of 1715 entitled "De comparando certo in physicis." In that same year 'sGravesande, who was originally trained as a lawyer, went to England as secretary to the Dutch ambassador and met Newton and other British scientists. In 1717 he became professor of mathematics and astronomy

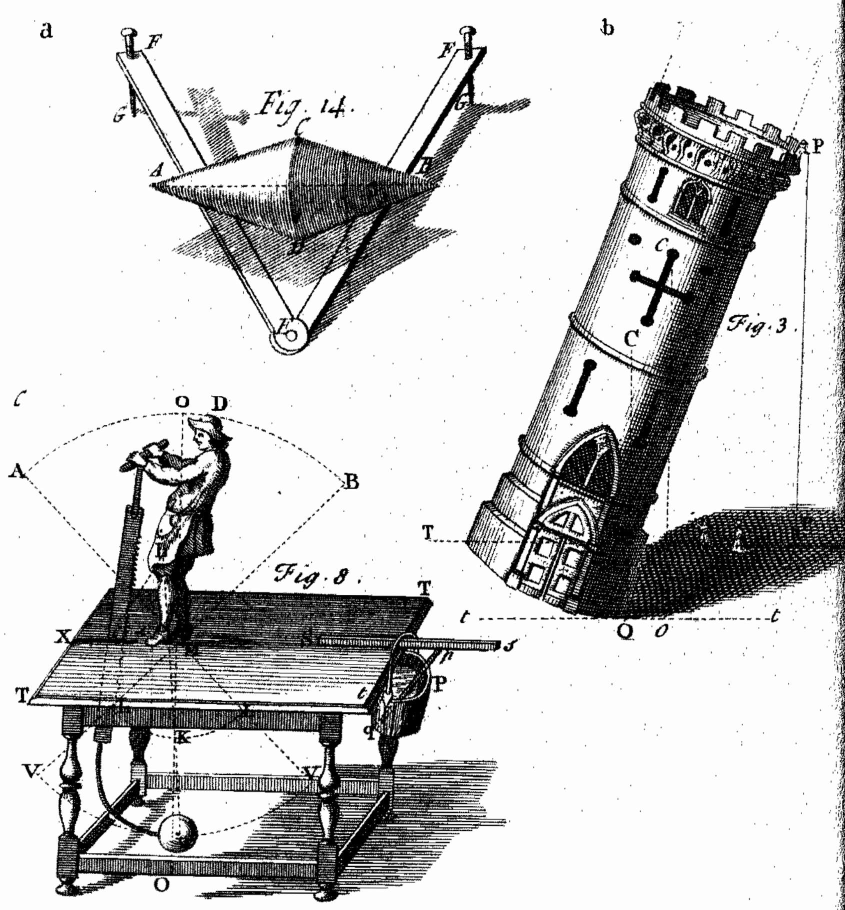

Fig. 3.1. Apparatus for physics demonstrations. A favorite subject for physics demonstration was centers of gravity. The double cone (a) appears to run uphill, but as it rolls toward the separated ends of the tracks its center of gravity is actually being lowered. The model tower (b) will fall over if its center of gravity is not over its base. And a hidden weight helps the sawyer (c) saw his plank. These pieces of demonstration apparatus are still used to instruct and entertain physics students. *Sources:* John Theophilus Desaguliers, *A course of experimental philosophy* (London, 1744), vol. I, pl. IV, fig. 14, and pl. V, fig. 3. By permission of the Syndics of Cambridge University Library.

at the University of Leiden and in 1720 he published his *Physices elementa mathematica experimentis confirmata sive, introductio ad philosophiam Newtonianam* [*Mathematical elements of physics confirmed by experiments, or introduction to the philosophy of Newton*]. It was immediately translated twice by two competing English Newtonians, John Desaguliers and John Keill, and continued through two Latin editions and four more English editions during 'sGravesande's lifetime. 'sGravesande emphasized the importance of mathematics in experimental physics, but it played only a minor role in his work. The emphasis was on experiments and demonstration apparatus, all of which he described in great detail. It is always a surprise to look through the copper engravings at the ends of textbooks by 'sGravesande, Desaguliers, Musschenbroek, Nollet, and other experimenters of the Enlightenment and find demonstration apparatus that with only minor design changes is still used in introductory physics classes (Figure 3.1). The earliest demonstration apparatus set a style that persisted for centuries.

The work of 'sGravesande and the other Dutch physicists redefined physics by making it experimental and by narrowing it to what we now recognize as physical science. From 1720 on, experimental physics commonly included the study of heat, light, electricity, and magnetism but excluded anatomy, medicine, natural history, and chemistry. The French translation of 'sGravesande's book was titled *Eléments de physique* [*Elements of physics*] (1747), Musschenbroek wrote an *Essai de physique* [*Essay on physics*] (1737), and many other books with the word *physics* in the title appeared during the Enlightenment. All of them excluded or greatly diminished the role of the life sciences and emphasized demonstration experiments.

In Germany the situation was slightly different. Experiment thrived there as well, but it was based on the philosophical foundation of Leibniz as altered and interpreted by Christian Wolff. Wolff's *Allerhand nützliche Versuche, dadurch zu genauer Erkentniss der Natur und Kunst der Weg gebahnet wird* [*Generally useful researches for attaining to a more exact knowledge of nature and the arts*] (1721–3) was published the same year as 'sGravesande's *Mathematical elements of physics.* Like 'sGravesande's work, Wolff's book described demonstration experiments and gave detailed instructions for making and using the apparatus, but unlike the Dutch physicists Wolff attempted to create a single rational systematic philosophy, after the model of Leibniz. Wolff's philosophy made little headway in England. In France, however, Madame du Châtelet accepted it in her *Institutions of physics,* and the *Encyclopédie* made use of it in the articles on experimental physics.

The new apparatus and experiments did not immediately make experimental physics quantitative, however, because they were designed to create, not measure, phenomena. Electrical apparatus, for instance, allowed one to discover which objects could be electrified and under what conditions, but it did not measure anything. Measurement had to wait until qualitative theory had specified what it was important to measure. Thus efforts to measure electrical effects came only after experimenters had produced a wider range of new electrical phenomena and had attempted some qualitative theoretical explanations. When, toward the end of the eighteenth century, precise measurement became an important goal of experimental physics, then the imagined "subtle fluids" used to account for phenomena began to be replaced by quantitative laws that made physical phenomena more predictable, if not more understandable.

The Subtle Fluids

The concept of a subtle fluid was a necessary step in the process of quantification. A "subtle" or "imponderable" fluid was a substance that possessed physical properties but was not like ordinary matter. The movement of the subtle fluid carried the physical property with it but did not convey any mass. The best examples of subtle fluids were electricity and heat. Experimenters could easily observe the transfer of electrical effects from one object to another, but they could not detect any accompanying change in weight. Heat, likewise, flowed from a hot object to a cold one without any apparent change in mass (although some experimenters believed that they could detect such a change). It seemed easiest to explain the transfer of electricity or heat by attributing it to a weightless fluid that carried the observed properties. We still use the same image when we speak of heat and electricity "flowing" or assign a heat or electrical "capacity" to an object. Other physical phenomena that could be explained by subtle fluids were gravitation, light, magnetism, and the principle of combustion, although not all of these phenomena lent themselves equally well to explanation in terms of subtle fluids.

The subtle fluids had the added advantage of showing what should be measured in physics. They provided a theoretical framework around which one could build physical concepts like "charge," "electrical tension," "heat," "heat capacity," and "temperature." Newton had hoped to explain all of these physical phenomena by the action of forces between the atoms of matter, in the same way that he had explained the motions of the heavenly bodies by forces of gravitation acting between them. The forces of gravity could be measured, however, whereas there was no way to directly measure the forces acting between the atoms. Therefore a physics based on attractions and repulsions between atoms could only lead to descriptive theories in experimental physics.

The opposite was true of ether or fluid theories. Electricity and heat were found to be conserved, at least in certain situations. Therefore it was convenient to think of them as substances that carried physical properties with them. The density of the fluid was proportional to the intensity of the effect. For example, the thermometer measured the concentration of heat fluid in an object. If heat were not a substance, it was not clear what the thermometer was measuring. Until the concept of energy was developed, the mechanical theory of heat had no simple answer to that problem. All attempts to weigh the fluids of electricity and magnetism failed or were inconclusive, so it was assumed that the subtle fluids carried physical properties but were not themselves material. In one sense they harked back to the "sympathies" and "antipathies" of the hermetists or the "principles" of the alchemists, but unlike those ambiguous, unmeasurable qualities, the subtle fluids were the only way that experimental physics could be made quantitative in the first half of the eighteenth century.

The concept of subtle fluids made its appearance around 1740, when demonstration experiments in physics were rapidly gaining in popularity. At the same time a reinterpretation of Newton's work intended to bring his authority to the support of the new theories was under way. Newton had not made it clear whether the forces acting between the planets and between the parts of matter acted at a distance or through some intervening medium called an "ether." Earlier Roger Cotes (1682–1716) had written a preface to the second edition of the *Principia* (1713), supposedly with Newton's blessing, that described gravity as a force acting at a distance without any intervening medium. Natural philosophers universally accepted this interpretation of Newton's theory of gravity until 1740. After 1740, when the study of electricity and heat began to make progress, ether theories reappeared and were ascribed to Newton. In 1744 two famous letters from Newton to Henry Oldenburg (1676) and to Robert Boyle (1679) that described ether hypotheses were published, and three books in English on Newton's concept of ether appeared between 1740 and 1745. Suddenly Newton's gravity meant action by an ether rather than action at a distance. Thus the subtle fluids were supported in physics not only by new theories but also by a reinterpretation of the old ones.

The concept of subtle fluids was also suggested to some extent by the new interest in "air" during the first half of the century. Stephen Hales's *Vegetable staticks* (1727) ended with a long chapter on the analysis of air "fixed" in bodies. In decomposing a variety of substances by heat, Hales (1677–1761) had been able to collect large quantities of "air" from the reactions. Contemporary chemistry recognized only one element in the gaseous state, and that was the element "air," so Hales regarded all of the gases that he had collected as air. Without searching for any differences in their chemical properties, he merely measured their volumes and wondered how so much air could be contained in such a small solid. To compress the "air" collected back into the volume of the solid from which it came would require great force. How could it be contained without exploding the solid in which it was "fixed"? Hales's emphasis on the repulsive or expansive property of air led naturally to an emphasis on the expansive properties of the even more subtle fluids of heat and electricity. Heat not only caused substances to expand; it also caused solids and liquids to give off "air" that expanded without limit. Electrical fluid also repelled other electrical fluid, so that a charge spread over a conductor and leaped to another conductor that contained a lower concentration of the fluid. Thus, following the model of Hales's air, self-repulsion became a common property of the subtle fluids.

The self-repulsive or expansive property of the subtle fluids was also a property of "fire." The Dutch Newtonians always related the subtle fluid of heat to the element of fire, and "fire," like Stephen Hales's "air," was one of the four Aristotelian elements (earth, water, air, and fire). Fire was the most volatile and least substantial of all the elements; therefore it was the chief agent of change, as witnessed by its role in combustion, fermentation, decomposition, and evaporation. Heat, light, and electricity were all forms of fire, according to Boerhaave and Musschenbroek. 'sGravesande was more cautious in his theorizing but agreed substantially with his Dutch colleagues. Fire existed in all bodies. It could not be created or destroyed but could be transferred from one object to another. In some heat phenomena, such as friction and the focusing of the sun's rays by a burning lens, the fire was not actually transferred but was activated in the body from some latent or unfocused state.

The Dutch physicists also identified electricity with fire. Benjamin Franklin at first followed their lead, referring to the spark as fire until 1750. After 1750, when it became apparent that the properties of heat and electricity were very different, he made a careful distinction between them. Madame du Châtelet and Voltaire both wrote prize essays on fire, and at the end of the century Antoine-Laurent Lavoisier incurred the anger of the revolutionary Jean Paul Marat by criticizing Marat's studies of fire. Thus the old elements of fire and air lay at the conceptual foundations of the subtle fluids. It is easy for us to overlook their importance, since we are equipped with twentieth-century hindsight. Our modern physics attaches no particular significance to the Aristotelian elements, but to understand experimental physics and chemistry as the natural philosophers of the Enlightenment understood them, it is necessary to know "fire" and "air."

The theory of subtle fluids allowed physicists to create new physical concepts and to quantify them, at least to a certain extent. But ultimately the theory became more confining than liberating. Just as Newton discovered that he could describe the phenomena of gravitation mathematically without supposing any ether, so did the physicists of the late eighteenth century discover that they could quantify physical concepts such as temperature, specific heat, charge, and capacitance without assigning any specific subtle fluid to them. At that point experimental physics became more phenomenalistic and more quantitative. The theory of subtle fluids ceased to be of much value, although it had been essential in the earlier stages of theory formation and quantification.

Electricity

Of all the subtle fluids conceived of during the Enlightenment, the electrical fire was the one that caused the most excitement and attracted the most researchers. The study of electricity became the model for experimental physics, both in the kinds of experiments performed and in the construction of apparatus. Electricity had several advantages over the other fields of experimental physics. Once research began in earnest, experimenters rapidly discovered new electrical phenomena, making their work rewarding. The experiments were dramatic, especially after 1746 when the discovery of the Leyden jar made it possible to accumulate very large charges. Electrical experiments also investigated attraction. Newton had constructed an entire world system from the single idea of universal gravitation and had concluded that other attractions and repulsions between atoms would probably account for all of the phenomena of chemistry and physics (using the latter term in its modern sense). Electricity seemed to be the phenomenon that was most likely to exhibit these interatomic forces. By 1733 it appeared that all substances could be electrified by friction and that electricity

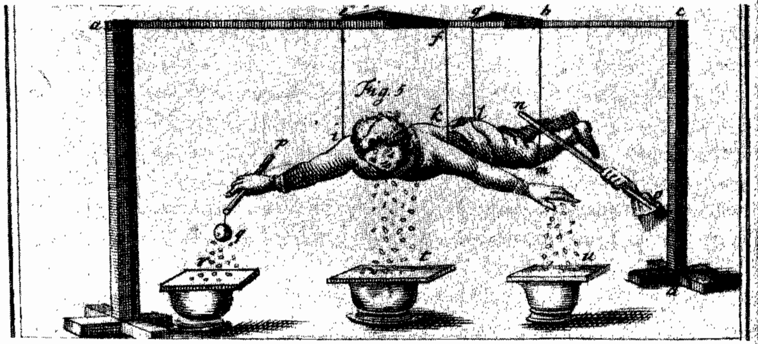

Fig. 3.2. Stephen Gray's electrified boy. Stephen Gray and Granville Wheler found that electricity could be communicated over considerable distances if proper substances were chosen for the conductors and for their supports. Among those objects that would conduct electricity was a 47-pound child suspended by silk cords. After Gray's experiment, the electrified boy became a standard part of electrical demonstrations. *Sources:* Johann Gabriel Doppelmayr, *Neu-entdeckte Phaenomena von bewunderswürdigen Würkungen der Natur* (Nuremburg, 1774). Courtesy of John L. Heilbron. Also appearing in *Electricity in the seventeenth and eighteenth centuries: a study of early modern physics* (Berkeley, 1979).

was therefore a universal characteristic of matter. The attractive power of amber rubbed by a cloth had been known to the ancients, but it had also been an artifically produced anomaly that seemed to have little value in explaining nature. The association of electricity with lightning and the discovery that all substances could be electrified moved electricity from the periphery to the center of scientific attention.

Electrical attraction, unlike gravitational attraction, could be controlled in an experiment. It could be increased, decreased, transferred, screened, hidden in bodies, made visible as a spark or a coronal discharge, used to ignite inflammable liquids, to stun animals, and to produce a host of other variable phenomena. Gravitation could not be altered in any way and was too weak to be measured between masses that would fit into the laboratory. When it was discovered that electricity both attracted and repelled, it came even closer to fitting the needs of matter theories that required both an attractive force to account for cohesion and a repulsive force to account for the expansion of gases.

Electrical experiments were popular and suitable for the scientific amateur, but it would be a mistake to assume that amateurs dominated the study of electricity. Benjamin Franklin's kite, electric spider, and lightning bells; Stephen Gray's electrified boy dangling from silk cords (Figure 3.2); the ubiquitous "electrified Venus" with her electrical kiss; and the Abbé Jean Antoine Nollet's (1700–70) spectacular electrification of 180 gendarmes for the edification of the king and queen of France might persuade one that electrical science had only recreational value during the Enlightment, but in fact even the most elaborate showmen were using their experiments to test existing theories and to suggest new ones.

The electrical experimenters found places in the universities and the scientific academies of Europe. A small but significant proportion supported themselves by giving public demonstration lectures. The Jesuits, who had been leaders in experimental physics during the seventeenth century, continued to hold a prominent place until their order was suppressed in 1773. These scientists, who made experimental physics their profession, comprised approximately half of the active electrical experimenters. The rest were artisans, professional men, and the independently wealthy who did not depend on their experiments for their income.

Income was important because scientific apparatus was always expensive. In most cases the professor of experimental physics at a university was expected to provide his own equipment, although this changed as the century progressed. Universities occasionally

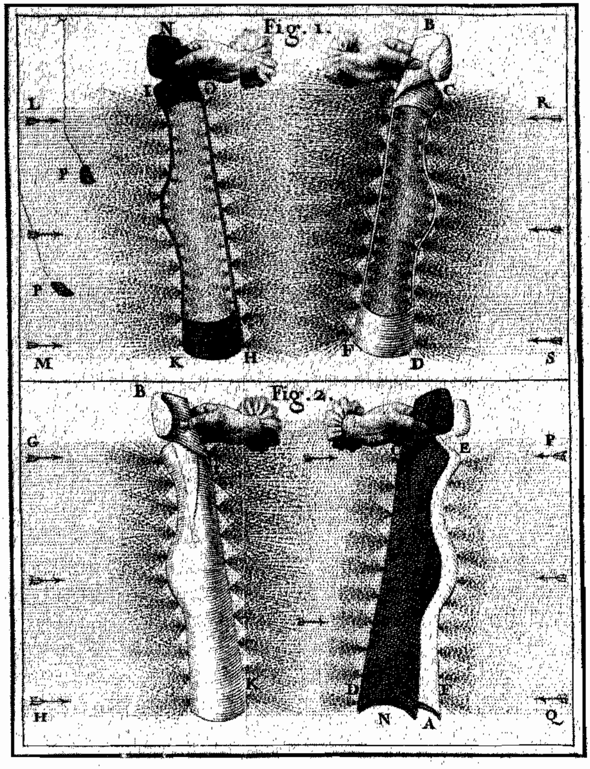

Fig. 3.3. Symmer's socks. Abbé Nollet thought that the cause of the strange behavior of Robert Symmer's silk socks was contrary jets of electric fluid. The jets inside the socks (Figure 1) were supposed to explain why the socks ballooned out when electrified, and the external jets (Figure 2) were supposed to explain why the socks attracted one another. *Sources:* Jean Antoine Nollet, *Lettres sur l'électricité (III) dans lesquelles on trouvera les principaux phénomènes qui ont été découverts depuis* 1760 (Paris, 1767). Courtesy of John L. Heilbron. Also appearing in *Electricity in the seventeenth and eighteenth centuries: a study of early modern physics* (Berkeley, 1979).

purchased a professor's apparatus for the use of his successor, but however it was financed, creating and maintaining a substantial *cabinet de physique* was an expensive undertaking. Some of these *cabinets* became very large, the most famous being the collection of the Teyler Foundation in Haarlem. In 1785 the director, Martinus van Marum (1750–1837), ordered a giant electrostatic generator from John Cuthbertson, an English instrument maker, who had moved to Holland. The English still made the best instruments, both for experimental physics and for astronomy. The great electrostatic generator at the Teyler Foundation produced a spark 2 feet long and as thick as a quill pen. Unfortunately van Marum could find few advantages for such a mighty blast in the context of eighteenth-century electrical science. The huge spark did, however, give support to the theory of a single electrical fluid, because it branched like a tree in one direction only, confounding the prevailing French theory that the spark was carried by contrary currents of electrical fluid.

Sometimes a very inexpensive piece of apparatus in the hands of an astute observer could outperform the most elaborate machines. One extraordinary case was "Symmer's socks." In November 1758, Robert Symmer (ca. 1707–63) noticed that when he pulled off his silk socks in the evening "they frequently made a crackling or snapping noise" and emitted "sparks of fire." Experimenting further with his socks, he observed that if he put both a black and a white sock on the same foot and pulled them off together, they exhibited no charge. But if he then pulled the socks apart, they crackled and bulged out as if still occupied by ghostly legs. If the socks were brought back together they collapsed, only to reinflate when separated again. Unlike the spark from van Marum's machine, Symmer's socks suggested the presence of two electrical fluids. If there were only one electrical fluid, the socks should have neutralized each other permanently when they were brought together (see Figure 3.3). Electrical phenomena discovered in increasing profusion and maddening unpredictability bewildered even the most hardy theoreticians.

The Early History of Electricity

William Gilbert (1544–1603) had made the first extensive series of investigations of electricity in his book *De magnete* [*On the magnet*], published in 1600. He was primarily interested in magnetism and investigated electricity only in order to distinguish it from magnetism. In addition to making that important distinction, Gilbert discovered many other "electrics" – substances other than amber that attracted light objects after being rubbed. His "nonelectrics" were substances that he could not electrify by friction. We would now recognize them as conductors, but of course in 1600 Gilbert knew nothing about electrical conduction.

After Gilbert, electrical experiments were carried out by the Jesuits and by members of the Italian Accademia del Cimento. Descartes attempted to include electrical attraction in his scheme of etherial vortices, and Robert Boyle investigated the behavior of electricity in a vacuum. The vacuum provided an important test for electrical theory because it was one way to determine whether air had some mechanical role in electrical attraction or whether an electrical effluvium was entirely responsible for the action. Unfortunately, electrical experiments became more complicated in a vacuum, because gases at low pressure conduct electricity, especially at the high voltages produced by friction. The electrical discharges produced in partially evacuated flasks were quite unlike ordinary sparks and therefore difficult to accommodate in any theory.

It was this glow in a vacuum, the so-called barometric light, that began the train of electrical researches that were so fruitful during the Enlightenment. Francis Hauksbee (ca. 1666–1713), "curator of experiments" at the Royal Society, began research on the luminosity of phosphorus in 1705 under instruction from members of the society. As part of his investigation he studied the phenomenon of barometric light – an occasional flashing that can be observed in a barometer in the vacuum above the mercury. Hauksbee soon discovered that the barometer was not necessary to produce the flashes: Mercury dribbled over a glass surface in a partial vacuum also caused flashes. Very small flashes could even be observed at atmospheric pressure. They grew brighter as the air was pumped out until it was about half gone, then dimmed as the air pressure dropped further. Hauksbee replaced the mercury with other materials rubbed together in a partial vacuum and still obtained the flashes. Finally he discovered that merely rubbing an evacuated globe on the outside was sufficient to produce the flashes. He mounted the glass globe on an axle and caused it to glow brightly when he placed his hands against the spinning globe.

The phenomenon was obviously electrical, although Hauksbee had little chance of explaining what was going on. He was observing a high-voltage electrical discharge through a gas that was under reduced pressure, the principle used today in mercury vapor street lights and neon signs. Although Hauksbee could not explain the barometric light, the discovery of this one unusual and inexplicable effect led to other more fruitful experiments. As we shall see, at the end of the century just such an unusual and complicated phenomenon observed by Luigi Galvani (1737–98) led to the discovery of current electricity.

Hauksbee found that his spinning globe also provided a convenient way of producing electricity. A spinning glass globe or plate became the standard electrostatic generator throughout the century. Hauksbee also rubbed a long tube of glass to produce sparks, and this apparatus became the other standard generator. Hauksbee discovered that a glowing, spinning globe could cause a nearby evacuated globe to glow as well and concluded that the electrical "effluvium" carried about by the spinning globe must be rubbing against the nearby globe, causing it to glow too. He attempted to learn more about this effluvium by hanging threads about the spinning globe (see Figure 3.4). Instead of being blown around by the effluvium as he expected, the threads stood out stiffly, pointing toward the center of the globe.

With a large glass tube he made pieces of leaf brass dance about, first being attracted to the tube, then repelled, then attracted again. Earlier researchers, including Hauksbee's mentor Newton (who was president of the Royal Society during the time when Hauksbee was curator of experiments), had observed this phenomenon but believed that the repulsion was merely a mechanical rebounding of the brass from the tube, not the action of an electrical effluvium. Huygens recognized repulsion, as did 'sGravesande, but Hauksbee was understandably baffled. There did not seem to be any conceivable effluvium, atmosphere, or ether that would explain his apparently contradictory experimental results.

In 1729, Stephen Gray (1666–1736), a dedicated amateur experimenter and occasional contributor to the *Philosophical Transactions* of the Royal Society, discovered that electricity could be communicated over rather long distances by contact. Gray used as a generator a glass tube that he kept corked at both ends to keep out dust. Keeping the dust out was a good idea, because Hauksbee had found that dust in the tube could reduce its power. While he was checking to see if the corks themselves would reduce the tube's effectiveness, Gray was surprised to see the feather that he was trying to attract with the tube move to the cork instead. Since he was preparing to investigate the possible transfer of the "emanation" to other objects, he was alert to the fact that the corks might acquire electricity from the tube. Gray then tried communicating the electricity further, through a stick stuck into the cork and surmounted with an ivory ball. When this succeeded, he fastened to the tube a fishing rod with a string attached, making a total distance of 52 feet, and again he was able to attract objects with the end of the string. Continuing his experiments a month later with a neighboring amateur scientist, Granville Wheler (d. 1770), he managed to carry the electricity 650 feet along a heavy string suspended from silk cords mounted on poles in his orchard.

The discovery of electrical conduction gave evidence of an electrical fluid and presented an opportunity for spectacular demonstrations. Gray and Wheler electrified by conduction a small boy suspended from the ceiling who could then attract objects with all parts of his body.

The next major advance in electrical science was made by Charles-François de Cisternai Dufay (1698–1739), a young infantry officer who pursued his scientific investigations with a command of previous research and a degree of organization that was completely missing in the haphazard experiments of Gray. Expanding on Gray's work in 1733, Dufay systematically experimented with different materials to see which ones could be electrified. He succeeded in electrifying everything that could be rubbed except metals, and these he electrified by induction – that is, by bringing the glass tube close to the object to be electrified (which was placed on an insulated stand), drawing off a charge from the other side of the object, and then removing the glass tube. Dufay discovered that wetting a string made it conduct better, that glass was a better insulator than silk, that attracted bodies were indeed repelled after they struck the glass tube, and, most important of all, that there appeared to be two electricities, not just one. The electricity produced by rubbing a vitreous substance like glass attracted the electricity produced by

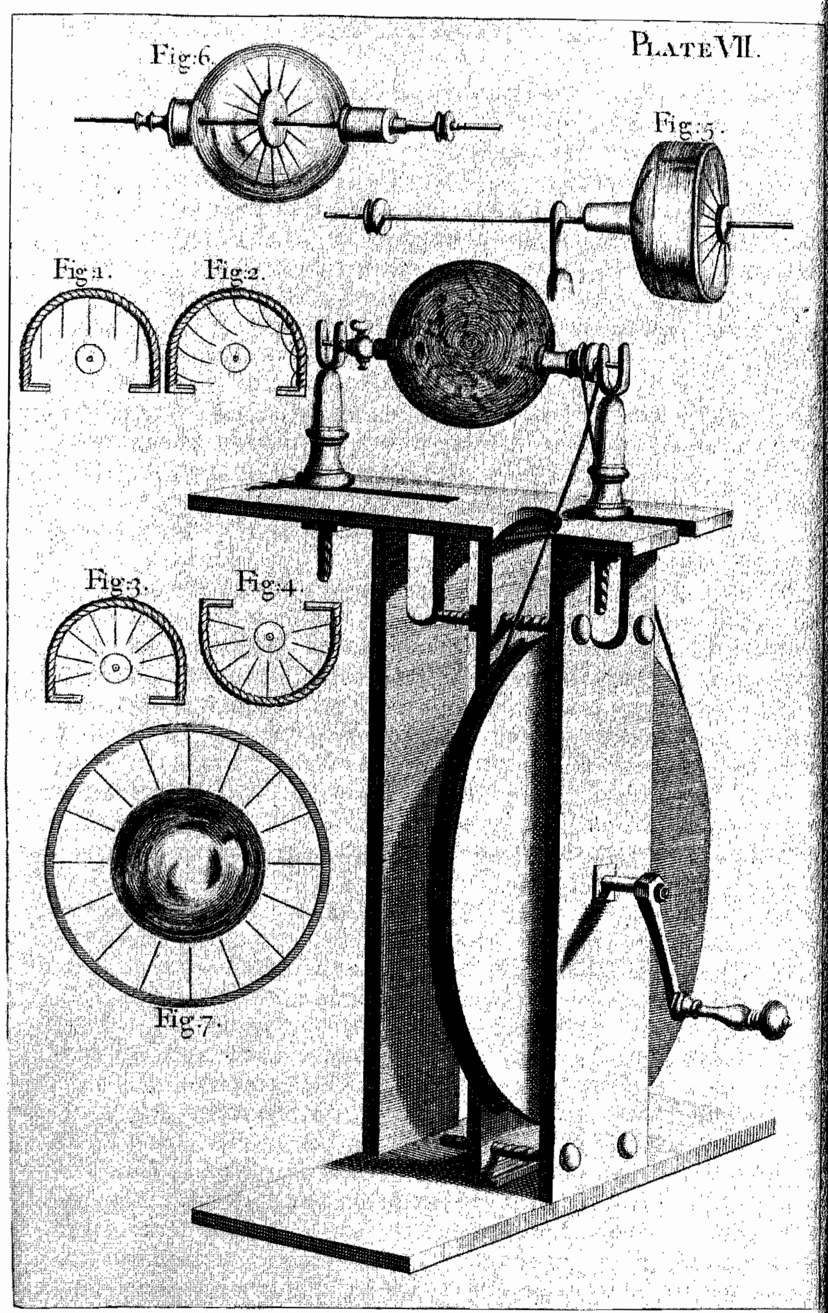

Fig. 3.4. Francis Hauksbee's investigation of the barometric light. The spinning evacuated glass globe glowed when touched with the hand. In order to detect the motion of the electrical effluvium that he believed surrounded the electrified globe, Hauksbee hung threads inside and outside the glass. But instead of being carried around by the effluvium as he had expected (Figure 2), the threads stiffened and pointed directly at the glass, a result not easily explained by the effluvial theory. After these experiments by Hauksbee, the spinning glass globe became the standard instrument used to create large amounts of electrical charge. *Sources:* Francis Hauksbee, *Physico-mechanical experiments on various subjects. Containing an account of several surprising phenomena touching light and electricity, producible on the attrition of bodies . . . together with the explanations of all the machines* . . . (London, 1709), pl. VII. By permission of the Syndics of Cambridge University Library.

rubbing a resinous substance like amber. Each kind of electricity repelled electricity of its own kind. Dufay called these electricities "resinous" and "vitreous" after the kinds of substances that were rubbed to produce them. Although Dufay never talked about electrical fluids and carefully limited his description of experiments to the phenomena observed, it was natural to assume a two-fluid theory in which each fluid repelled fluid of the same kind but attracted fluid of the other kind. Dufay's collaborator, Abbé Nollet, who became the most prominent French electrician during the Enlightenment, explained the two electricities as opposing currents of the electrical fluid emerging in jets from the electrified body. Nollet's assumption that the electrical effluvium was in rapid motion was natural because the most striking phenomenon of electricity had been from the beginning the way in which small bits of paper or leaf brass were hurled about by the presence of an electrified body. The assumption that a rapidly moving effluvium was responsible for this agitation seemed obvious.

Benjamin Franklin's One-Fluid Theory

An alternative view was that of Benjamin Franklin. Following Newton, whose ideas he had studied in the works of 'sGravesande, Musschenbroek, and Desagulier, Franklin proposed a single static electrical "atmosphere" that attracted and repelled by pressure rather than by the impact of an electrical wind. (Newton's gravitational ether, as he had described it in a letter to Boyle, had been of this type.) Franklin's theory of an electrical atmosphere was never very successful in explaining the phenomena, but Franklin freely admitted that it was nothing but a speculation. His contribution was not dependent on any particular atmosphere, and, like a good disciple of Newton, he made it his object to reduce the phenomena to rule, not to explain them.

In 1743 Franklin saw a demonstration lecture in Boston given by a Dr. Spencer of Edinburgh who repeated Stephen Gray's trick of electrifying a small boy suspended from silk cords. Franklin came away with the impression that he had seen a demonstration revealing "fire diffused through all space." In 1745 the Library Company of Philadelphia received from Peter Collinson (ca. 1693–1768), a Quaker merchant in London, a copy of the *Gentleman's Magazine* containing a lengthy description of spectacular electrical experiments performed in Germany. The article, a translation by the Göttingen physiologist Albrecht von Haller (1708–77) of an earlier article in the Dutch journal *Bibliothèque raisonée,* described experiments at length and clearly identified them as being electrical. Collinson also included a glass tube so that the members of the Library Company could perform their own experiments. Working largely independently from the Europeans, with Franklin as their chief experimenter, the Philadelphians made two important contributions to the growing electrical science. Franklin described them in 1747 in letters to Collinson that circulated in England before they were published in 1751 in the *Philosophical Transactions.*

The first discovery concerned the peculiar power of pointed conductors to "draw off and throw off" the electrical fire. The experimenters had found that if one brought a sharp bodkin up to an electrified metal sphere, the "fire" leaked off the sphere to the sharp point for a distance of up to 8 or 10 inches. In the dark, the experimenters observed a glow at the point, and afterward the sphere was found to have lost its electricity. To the practical-minded Franklin this experiment later suggested the lightning rod, which was intended to defuse the electricity in thunderclouds by drawing it off before it could strike as lightning. It was one of the first inventions to make good Francis Bacon's promise that science would produce new and useful technology. Franklin suggested lightning experiments in 1749 and performed his famous kite experiment in 1752. By that time experimenters in France had already drawn electricity from the clouds and had even performed the kite experiment independently, so it was not original with Franklin. Franklin's kite had a pointed wire attached to the frame of the kite to draw the electricity. The wet kite string conducted the electricity to the key, which was insulated from Franklin's hand by a silk ribbon. From the key Franklin was able to draw off the "fire" and show that it acted just like electricity produced by friction.

This most famous experiment of Franklin's was extremely dangerous. In 1753 the able physicist George Wilhelm Richmann (1711–53), at the Russian Academy of Sciences, was killed when his apparatus drew a lightning bolt from the sky. A debate raged through the rest of the century over the advantages of pointed as opposed to blunt lightning rods. The proponents of blunt rods, led by Benjamin Wilson (1721–88), argued that pointed rods would attract lightning whereas blunt rods, which Wilson believed should be mounted under the roof, would merely carry away a strike but would not attract it. In fact, the shape of the rod makes little difference in its ability to carry off lightning, and Franklin was excessively bold in thinking that his rods defused storm clouds. The height reached by the rod is much more critical than its shape.

A less dramatic but more important set of experiments was one

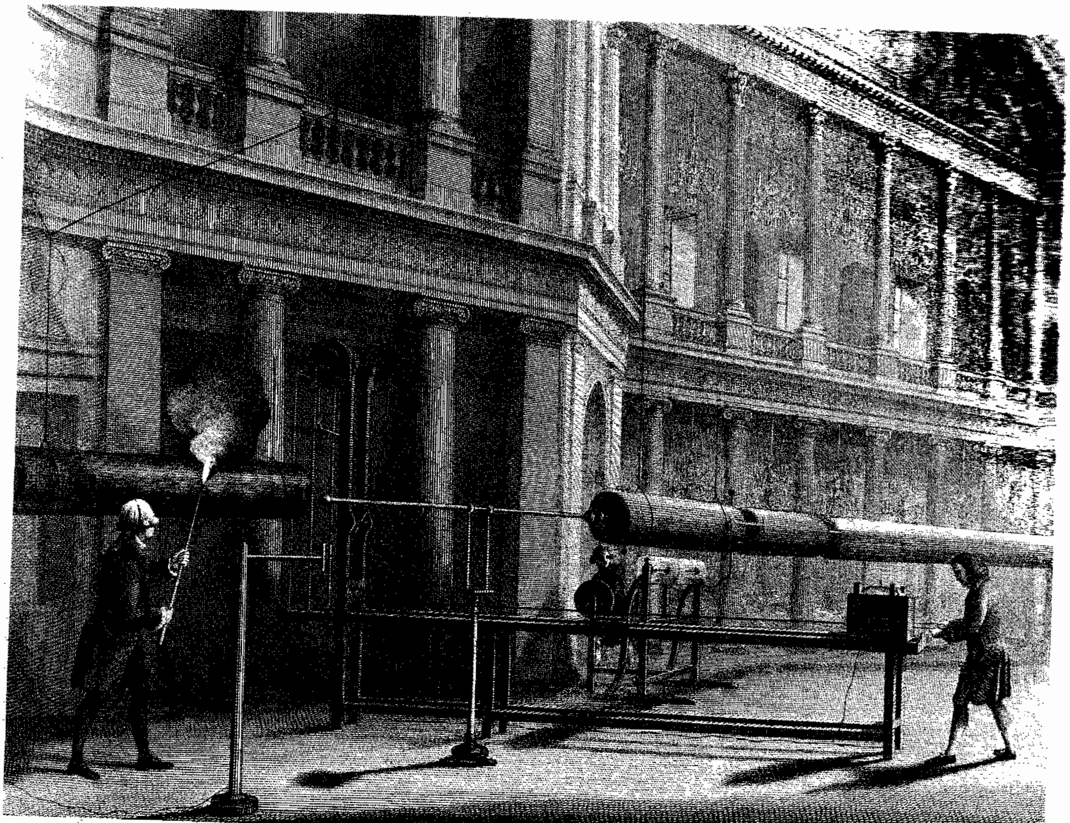

Fig. 3.5. Testing lightning rods in the great hall of the London Pantheon. In 1772 the Royal Society of London formed a commission to decide whether the Purfleet Arsenal should be protected by pointed or blunt lightning rods. Benjamin Wilson favored blunt rods; Benjamin Franklin, also serving on the commission, favored pointed rods. Franklin won. But in 1777 lightning damaged the arsenal in spite of the pointed rods. King George III supported Wilson in his experiment, shown in the figure, and ordered blunt rods to replace the pointed ones at Buckingham Palace. In the illustration, Wilson's artifical clouds (the large metal cylinders suspended from the ceiling) are menacing a model of the Purfleet Arsenal. *Sources:* Benjamin Wilson, "New experiments and observations on the nature and use of conductors," *Philosophical Transactions* vol. 68, pt. 1 (1778), p. 246. Courtesy of the University of Washington Libraries.

of the first that Franklin performed. In May 1747 he wrote to Collinson, "We had for some time been of opinion, that the electrical fire was not created by friction, but collected, being really an element diffused among . . . and attracted by other matter, particularly by water and metals." Evidence for this theory came from "the impossibility of electrizing one's self (though standing on wax) by rubbing the tube and drawing the fire from it."[2] This experiment, which indicated that the electrical fluid on the glass tube was not created by the rubbing but drawn from the body of the experimenter, suggested four other experiments:

1. A person standing on wax, and rubbing the tube, and another person on wax drawing the fire, they will both of them (provided they do not stand so as to touch one another) appear to be electrized, to a person standing on the floor; that is, he will perceive a spark on approaching each of them with his knuckle.
2. But if the persons on wax touch each other during the exciting of the tube, neither of them will appear to be electrized.
3. If they touch one another after exciting the tube, and drawing the fire as aforesaid, there will be a stronger spark between them than was between either of them and the person on the floor.
4. After such strong spark, neither of them discovers any electricity.[3]

Franklin explained these experiments by assuming that there was a single electrical fluid of which each person in a neutral state had a share. The electrified glass tube acted as a pump in which the fluid could be moved from one body to another. Therefore if person A transferred fluid to person B and both were insulated from the ground, then B would have an excess of the fluid (be charged positively) and A would have a lack (be charged negatively). Franklin's theory of a single electrical fluid that could not be created or destroyed but only transferred from one object to another explained most of the known electrical phenomena. Electrical fluid repelled other electrical fluid but attracted ordinary matter. Electrical attraction, repulsion, sparking, conduction, induction, and Dufay's vitreous and resinous electricities could all be explained by Franklin's theory. (Dufay's two electricities represented on the one hand a lack and on the other an excess of the fluid.)

Franklin missed the fact that negative charges repel, a phenomenon that his theory could not easily explain because it required that ordinary matter bereft of its electrical fluid should repel other ordinary matter, whereas the theory of gravitation required that matter be attractive. Franz Ulrich Theodosius Aepinus (1724–1802) removed this anomaly in 1756 by merely assuming a symmetry in the attractive and repulsive properties of positive and negative electricity, but he could do this only by giving up all the atmospheres and effluvia that had been part of electrical theory before him. By 1756 experimental evidence had already cast doubt on the atmospheres. The study of electricity would have to become more operational. Theory would describe more but explain less.

The Leyden Jar

The most exciting discovery was also the hardest to explain. This was the Leyden jar, devised by the German Ewald Georg von Kleist (ca. 1700–48) but best described by Pieter van Musschenbroek at Leiden, Holland, whence the name of the device. In January 1746, Musschenbroek wrote to René-Antoine Ferchault de Réaumur (1683–1757), his correspondent at the Paris Academy of Sciences: "I would like to tell you about a new but terrible experiment, which I advise you never to try yourself, nor would I, who have experienced it and survived by the grace of God, do it again for all the kingdom of France." Musschenbroek had collected the electricity from a whirling globe in an iron tube suspended from the ceiling by silk. From the end of the tube hung a brass wire that carried the electricity into a flask containing water. Musschenbroek held the flask in his right hand and tried to draw a spark from the iron tube with his left hand. Suddenly his hand was struck with such force that his "whole body quivered just like someone hit by lightning . . . The arm and the entire body are affected so terribly I can't describe it. I thought I was done for."[4]

His courage may have been weakened by the experience but not his curiosity, and Musschenbroek soon discovered that the human hand was not essential; any conductor would do on the outside of the jar, but in order to obtain the huge shock the same person had to touch the outside conductor and the iron tube at the same time. The Leyden jar long remained the most common form of electrical condenser, but other forms, such as "Franklin squares" composed of parallel metal plates, appeared in England soon after. Several Leyden jars connected in parallel could increase the dramatic quality of any lecture demonstration. After he had electrified 180 gendarmes for the entertainment of the king, Nollet shocked 200 Carthusian monks in their monastery. "It is singular to see the multitude of different gestures, and to hear the instantaneous exclamation of those surprised by the shock."[5] Parlor tricks and practical jokes of this type approached lethality.

Franklin pursued an elegant series of experiments designed to probe the mystery of the Leyden jar. He found that just as much

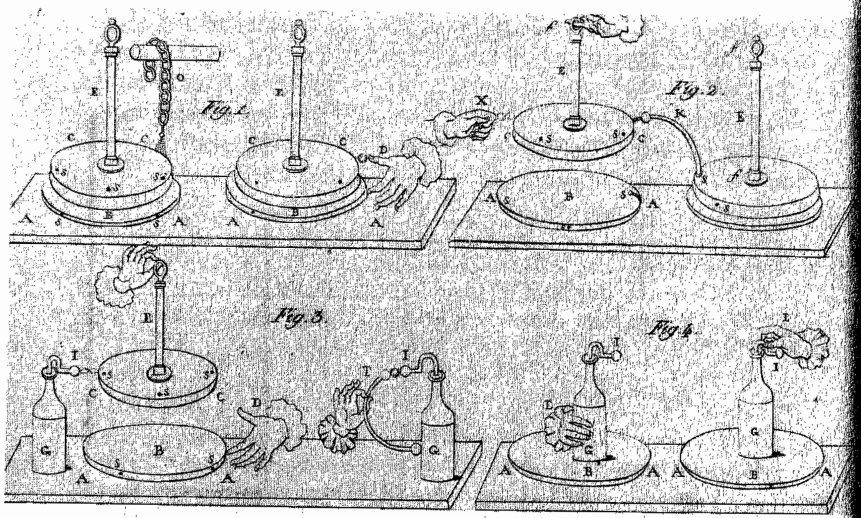

Fig. 3.6. The electrophore of Alessandro Volta. The experimenter could obtain an unlimited amount of electricity from the electrophore merely by lifting and lowering the metal plate (C) with its insulated handle, and by grounding and drawing off the charge at the proper times. With such an apparatus one could charge any number of Leyden jars (G). This version of the separable condenser effectively destroyed the effluvial theories of electricity. *Sources:* Alessandro Volta, *Collezione dell'opere del cavaliere Conte Alessandro Volta* (Florence, 1816), vol. 1, pt. 1, pl. I. By permission of the Syndics of Cambridge University Library.

electricity was pushed off the outside conductor as was taken in by the inside conductor, from which he concluded that the glass of the jar was completely impermeable to electricity. In other experiments he built condensers that could be taken apart in order to locate the residual electricity. He sought to determine whether the charge was on the conductor, in the conductor, or on the surface of the glass. The complexity of the phenomenon and the inadequacy of his theory prevented him from solving the puzzle, but experiments similar to his led to improvements in the theory.

The electrical properties of glass created great confusion for all the early experimenters. Their theories assumed that the electrical fluid existed not only on the electrified body but also in an atmosphere around it. Franklin used smoke and Nollet used fine powder to detect the presence of this atmosphere and to reveal its extent. Electrical attraction and repulsion were assumed to be caused by the direct action of this electrical atmosphere. Glass did not conduct electricity (except when heated), but it did transmit the electrical influence. Hauksbee and Gray could easily attract light objects through several layers of glass. Did the electrical effluvium go through the glass or not? A metal or even a damp cloth conducted electricity, but it screened the effect of attraction. Hauksbee was dismayed to discover that the electrical attraction that could penetrate the wall of a glass bottle could be blocked by a thin sheet of muslin. As long as electrical theorists did not make a distinction between the electrical fluid and its attractive and repulsive influence, they could not explain this particular anomaly. Atmospheres, whether static or in motion, could not be the electricity and its attractive influence at the same time.

The invention of condensers that could be taken apart soon revealed the inadequacies of the concept of atmospheres. Aepinus's air condenser (1756), Johann Carl Wilcke's dissectible condenser (1762), and Allesandro Volta's electrophore (1775) were all disassembled condensers used to locate the electricity and its effect. The electrophore attracted the most attention because it appeared to provide an inexhaustible supply of electricity. Volta (1745–1827) described his *elettroforo perpetuo* to Joseph Priestley (1733–1804) in 1775. It consisted of an insulating cake made of resin and wax set in a metal dish; a metal plate with an insulated handle was placed on top of the cake (see Figure 3.6). The experimenter first electrified the cake by rubbing it or by charging it from a Leyden jar. He then placed the metal plate on the cake and touched the top of the plate to draw off the charge induced by the presence of the charged cake. He then removed the plate by holding the insulated handle. The plate was found to be charged. That charge could be transferred to a Leyden jar and the plate replaced on the cake. The process could be repeated as many times as desired without diminishing the charge on the cake – an apparently perpetual source of electricity. From this experiment Volta concluded that "nothing real" could be passing from the cake to the metal plate; otherwise the electricity on the cake would soon be exhausted. The electricity stayed on the cake, and only a force reached the plate. No atmosphere or effluvium would be inexhaustible. Therefore atmospheres could not explain the electrophore.

With the elimination of the atmospheres, electricians gave up trying to create mechanisms to explain electrical phenomena. Instead they tried to subject these phenomena to quantitative rule. Aepinus's *Tentamen theoriae electricitatis et magnetismi* [*Examination of a theory of electricity and magnetism*] (1759) contained the first successful quantitative analysis of the condenser, but it was regarded with suspicion and little read. Attempts to measure electricity were frustrated not so much by the lack of instruments as by confusion over what should be measured. To separate the concepts of charge, force, tension, and capacitance required a theory that included these concepts. The electroscope, consisting of two threads or two pieces of gold leaf hung side by side, measured charge. The greater the separation of the threads, the greater the charge. (The gunnery metaphor was not accidental. Early electricians believed that they "charged" and "fired" their Leyden jars just as one charged and fired a cannon).

In 1747 Nollet projected the shadow of the threads on a screen containing a protractor scale that allowed him to measure the charge accurately without disturbing the experiment. In 1788 Volta suggested that the charge on a Leyden jar was probably proportional to both the "tension" (intensity of the electricity) and the "capacity" of the jar. Volta had isolated the concepts that would be the most valuable in the quantitative study of electricity. Unfortunately he could not confirm this relationship because of the nonlinearity of electroscopes.

Attempts to determine the force law were more successful. In 1769, John Robison (1739–1805), a student of Joseph Black's at Glasgow, measured the repulsion between charges with an apparatus that balanced the electrical repulsion against gravitational attraction. He was able to show that electrical forces fall off in proportion to the square of the distance between the charged objects, just as gravitational forces do. Charles Augustin Coulomb (1736–1806), a French military engineer, made his much more famous measurements of electrical attraction and repulsion in 1785 by balancing the electrical force against the torsion in a fine wire. The precision of his apparatus set a new standard in experimental physics. After Coulomb, apparatus tended to be carefully engineered for their tasks, not just put together from whatever happened to be lying around the laboratory.

The most successful quantifier of all was Henry Cavendish (1731–1810), but because he resisted publishing his results and wrote only for the fully informed, his accomplishments were not well known. He not only measured electrical force in 1771 but also added the first careful mathematical analysis of experimental error. Cavendish developed a technique for measuring the relative capacitance and the relative resistivity of conductors. The latter he measured by equalizing the jolts he felt upon placing himself in parallel with different lengths of conductors. Through this technique he obtained consistent results, with an error of less than 10 percent.

The Discovery of Current Electricity

At the end of the century the study of electricity was turned in a new direction by a discovery as complex as Hauksbee's barometric light at the beginning of the century. In 1791, Luigi Galvani, professor of anatomy at the University of Bologna, was dissecting a frog in his laboratory. He noticed that when the blade of his scalpel touched the crural nerve in the frog's leg, the leg kicked. It also kicked in unison with an electrostatic machine that was sparking in the room. The kick occurred only when he was touching the blade of the scalpel; no kick occurred when he held the scalpel by its bone handle.

Like the barometric light, the cause of the kick was beyond the scope of any existing theory. Galvani thought he had discovered a new kind of animal electricity and proceeded to check his theory by hanging frogs' legs from brass hooks on an iron trellis outdoors, the purpose being to attract "atmospheric" electricity to the legs. The legs jumped when he pressed the hooks against the trellis, but not because of the atmosphere. As Galvani soon discovered, the contact between the brass hooks and the iron trellis was making the electricity. A single metal produced no kicks.

Galvani must have been extremely observant to recognize these anomalies, but he lacked any adequate theory to explain them. As a physician, he sought a physiological explanation in the anatomy of the frog. His compatriot Volta knew little about frog anatomy but much about electricity. He soon found that the frog served only as a detector of electricity, much more sensitive than any electroscope of the time. Volta soon concluded that the electricity was produced by a circuit containing two dissimilar metals and at least one moist conductor. The electricity from a single metallic junction was weak, but he hoped to multiply the effect by linking several junctions in series. No combination seemed to work until he finally hit upon the idea of stacking up disks of silver and zinc (the metal pair that produced the most electricity), separating each pair with moist cardboard, thereby creating the following series: silver, zinc, cardboard; silver, zinc, cardboard; and so forth, ending in zinc. Just as in the case of the Leyden jar, connecting the top and bottom of this pile produced electricity, but instead of the single spark given off by the Leyden jar, Volta's pile generated a *constant current* of electricity. The pile caused a sensation when it was announced in 1800. Within a year Anthony Carlisle (1768–1840) and William Nicholson (1753–1815) at the Royal Society had used the current to break down water into oxygen and hydrogen. Current electricity led to the whole field of electrochemistry, and the study of electromagnetism followed as a consequence.

The electricians of the eighteenth century had carried their subject a long way. They had refined the theory, quantified their experiments, and improved their apparatus. By the end of the century the concept of the "subtle fluid" of electricity had changed drastically. The concepts of electrical atmospheres and effluvia were gone; electrical fluid was still imagined to flow in conductors, but the forces that it exerted were no longer understood to be the mechanical action of a material in space. As the phenomena of current electricity continued to multiply in the nineteenth century, even the existence of the electrical fluid would be called into doubt. The nature of the subtle fluid changed as the theories of electricity changed, but it was a gradual process. It would be a mistake to say that the discovery of current electricity created a "new science." Even before Volta built his pile, electricians were attempting to measure resistivity, capacitance, electrical "tension," and other quantities that we usually associate with current electricity, and construction of a quantitative theory of electricity was well under way.

Heat and Temperature

Of all the subtle fluids, heat was the one that was most a part of everyday experience (with the possible exception of light). In Aristotle's scheme of things, heat was a *quality,* like color, smell, roughness, or wetness. Qualities could be more or less intense but could not be measured or expressed in numbers. Only *quantities* such as length, weight, and time had magnitude and could be measured. Medieval scholars had talked about different degrees of heat (they counted eight), and scholars at Oxford and Paris in the fourteenth century speculated about the possibility of reducing qualities like heat to numbers, but it was the thermometer, first constructed in 1592 by Galileo, that finally made possible a quantitative study of heat. Galileo's gas thermometer had no set scale and therefore was not really a measuring instrument. The expanding liquid thermometer soon replaced Galileo's gas thermometer, and by 1641 the grand duke Ferdinand II of Tuscany had constructed an expanding liquid thermometer with the end sealed, which was not affected by changes in barometric pressure or by the evaporation of liquid from the tube.

The thermometer scale was completely arbitrary. Some scales were calibrated at a single temperature, and the degree was an arbitrarily chosen distance on the thermometer stem. Others were calibrated at two set temperatures, and the space in between was divided into some number of degrees. Anders Celsius created the centigrade scale in 1742, choosing the freezing and boiling points of water as fixed and dividing the intermediate temperatures into 100 degrees. But Celsius chose 0 degrees for the boiling point and 100 degrees for the freezing point. According to our usage he had the scale upside down, but there is no particular reason why the numbers should run one way rather than the other.

In addition to this arbitrariness of scale, the thermometer *was* what it measured. The experimenter assumed that something called "heat" was proportional to the expansion of mercury. Now that we have other ways of defining and quantifying heat, we know that that assumption was correct for the range of temperatures commonly measured by liquid-expansion thermometers. If it had been an incorrect assumption, the science of heat would have made little progress in the eighteenth century. The best early evidence for the linearity of the temperature scale was the fact that when water is heated over a constant fire, the temperature goes up uniformly.

Of course the thermometer does not measure heat at all, but only temperature. Joseph Black (1728–99), professor of medicine and chemistry at Glasgow and then at Edinburgh, was the first to point out the difference. If one assumed that heat was a fluid, the thermometer measured the intensity or density of the heat fluid in an object, not the total amount of fluid. A 5-gallon container of water contained five times as much heat as a 1-gallon container of water at the same temperature. The thermometer measured merely the density of the heat fluid.

Bacon, Galileo, Descartes, Boyle, and Newton had claimed that heat was not a substance but merely the motion of parts of bodies. In most cases, however, their theories were ambiguous because they believed that the motion of heat was contained in, or was caused by, "fire particles" or some other extremely active special substance. Thus heat was the result of motion, but it was identified with a special substance.

Boerhaave, in his *Elementa chemiae* [*Elements of chemistry*] of 1732, stated the fluid theory of heat unequivocably. He believed that the thermometer measured the density of the heat fluid and that therefore the amount of heat in any object was proportional to its temperature and its *volume,* the object serving merely as a container for heat. Boerhaave was persuaded that the quantity of heat in an object was proportional to its volume by experiments performed by Daniel Gabriel Fahrenheit (1686–1736). Fahrenheit found that when he mixed three volumes of mercury with two volumes of water, the mixture reached an equilibrium temperature halfway between the two initial temperatures. According to Boerhaave's rule, in order to obtain the same equilibrium temperature, the volumes should have been equal. This discrepancy was unfortunate, but at least it was not as bad as the discrepancy between the results of the experiment and the commonly held rule, which was that the heat contained in a body was proportional to its mass. That rule would have required thirteen times as much water as mercury by volume. In an essay written in 1739, George Martine (1702–41), of St. Andrews University, described an experiment in which he heated equal volumes of mercury and water before a fire and found that the temperature of the mercury rose twice as fast as that of the water, demonstrating again that the amount of heat in an object was not proportional to its volume.

Black concluded from these results that the quantity of heat in an object was not proportional either to the volume or to the mass. He argued that different substances had different affinities for or different capacities for heat. A piece of metal felt hotter than a piece of wood at the same temperature because the metal gave off more heat. Therefore not just the volume or the mass but also the nature of the material determined how much heat it contained at a given temperature. Not knowing how the heat was held in matter, whether by chemical combination with the atoms or by saturation of the pores like a sponge, Black in 1760 simply postulated a different heat "capacity" for each substance. By mixing different substances at different temperatures and observing their equilibrium temperature, he concluded that the amount of heat in any object was proportional to the temperature, the mass, and the heat capacity of the object. His only confirmation that the heat capacity was a definite characteristic of a substance came from the discovery that the capacity varied from substance to substance but remained the same for any one substance, whatever the combination of mass and temperature in the experiment.

In 1781, Johann Carl Wilcke (1732–96) came independently to much the same conclusions in Sweden, but instead of heat "capacity" he called the phenomenon "specific heat," in analogy to "specific gravity." For both Black and Wilcke, the specific heat was a constant of proportionality giving the amount of heat required to raise the temperature of a unit mass of a given substance one degree. For every substance the relationship between heat and temperature was different. Water required the most heat for a given temperature change. Very dense materials, which were believed by some to contain the most heat, were often found to have a rather small capacity for heat, mercury being a good example.

Latent Heat

Black and Wilcke were both led to the problem of specific heat by the discovery that a great deal of heat was required to melt ice, even though its temperature remained at the melting point. The prevailing assumption was that ice melted immediately when it reached the melting point. Wilcke recognized the fallacy of this assumption when he tried to melt the snow in a small courtyard by pouring hot water on it. Much hot water melted little snow, even though the snow was at the melting point.

Black saw the same problem in 1757. If snow melted completely when it reached the melting point, the torrents caused by a spring thaw "would tear up and sweep away every thing, and that so suddenly, that mankind should have great difficulty to escape from their ravages."[6] Black placed in a warm room two flasks of water, one frozen and the other liquid, but both at the melting point. The temperature of the liquid water constantly rose, whereas the temperature of the melting ice and water remained at the melting point until all of the ice was gone. From the temperature differences he calculated that it took as much heat just to melt the ice as it took to raise the temperature of an equal amount of water 140 degrees. He checked his results in a more precise experiment in which he added a weighed piece of ice to a weighed amount of warm water.

From the equilibrium temperature, he obtained a comparable figure of 143 degrees Fahrenheit. Black said that this heat was hidden, or "latent," because it did not affect the thermometer. According to the fluid theory, the latent heat was held in combination with the atoms in such a way that the thermometer could not detect it.

Black also measured the latent heat required to boil water into steam. This measurement required a constant source of heat, which Black at first thought could not be obtained, but after a distiller told him that when his furnace was in good order he could tell to a pint the amount of liquor he would get in an hour, Black decided to attempt the experiment. On a constant fire he compared the rate at which water boiled into steam to the rate at which the temperature of cool water increased over the same fire. He discovered that the amount of heat required to boil away a given quantity of water would raise the temperature of that water 810 degrees Fahrenheit if it had not boiled, a figure approximately 20 percent too low but a good result for the rough experiment that Black had designed.

William Irvine (1743–87), who had been a student of Black's at Glasgow, measured the latent heat of fusion of substances such as beeswax and tin and came up with an ingenious theory suggesting a relationship between latent and specific heats. He argued that the heat capacity of an object measured the total heat that it contained, the object acting as a container for the heat fluid. Ice had a measured heat capacity substantially less than that of water. Therefore when ice melted it changed from a small heat container into a large heat container. In order to have water at the same temperature as the ice, the water, with its larger capacity, had to take on a great deal more heat. This was latent heat. Because the heat of vaporization for water was greater than the heat of fusion, Irvine's theory predicted the heat capacity of steam had to be much greater than that of water. In fact it is less. Irvine's theory survived only because it was difficult to determine the specific heat of steam.

The inadequacies of Irvine's simple theory reveal the difficulties in any theory of latent heat. Somehow the heat fluid had to be held in combination with the atoms of different substances in such a way that only a portion of it was detected by the thermometer. How this heat was held latent in the body was difficult to determine. The most simple assumption was that it was held in chemical combination with the atoms of matter, a theory that had important implications for chemistry.

If heat were an actual material substance rather than an "imponderable," it could be expected to have weight, and throughout the eighteenth century many attempts were made to measure it. Boerhaave found no change in the weight of a mass of iron when it was heated. Comte Buffon, however, found that iron gained weight on heating. John Roebuck (1718–94) found just the opposite in 1775, and John Whitehurst (1713–88) confirmed Roebuck's results the next year. They both found that iron gained weight on cooling. Whitehurst warned, however, that the heat from the hot iron being weighed might have caused air currents or uneven expansion of the arms of the balance that might account for the weight difference.

George Fordyce (1736–1802), working with the knowledge of latent heat, realized that a big difference in heat could be obtained with a small difference in temperature, if one compared water and ice. He weighed a flask of water when liquid and when frozen (near the melting point in both cases) and found that it weighed more when frozen, which agreed with Roebuck's and Whitehurst's results and indicated that heat had "levity" (a property that Aristotle had given to the element of fire). In 1787, Benjamin Thompson, Count Rumford (1753–1814), repeated Fordyce's experiment with a more precise balance and varied the experiment to detect anomalies caused by condensation on the flasks and uneven expansion of the balance arms. His results detected no weight at all for heat, which caused him to conclude that heat was a mode of motion, not a substance.

In a more famous set of experiments performed at the military arsenal in Munich of which he was director, Rumford remarked on the great amount of heat produced in boring cannon, particularly if the boring tool were dull. By enclosing the boring apparatus in a box, he was able to boil water from the heat produced as the horses drove the machine. As long as the horses kept moving, the water kept boiling, which seemed to indicate an endless supply of heat.

This experiment has often been described as crucial, since it showed – from our present perspective – that the mechanical theory of heat was correct and that the fluid theory of heat was false. The fluid theory assumed that heat was conserved, which meant that it was not available in an endless supply. Yet however persuasive Rumford's experiments may appear to us in the twentieth century, history refutes this claim. The fluid theory retained its supporters. It could account for conduction and conservation of heat and for change of state in a simple way. It permitted a quantitative science of heat using the thermometer. Even if scientists held the mechanical theory, they would think in terms of a heat fluid for the sake of convenience.

The end of the fluid theory came when scientists sought a theory that would cover radiant heat. It was not easy for either the fluid

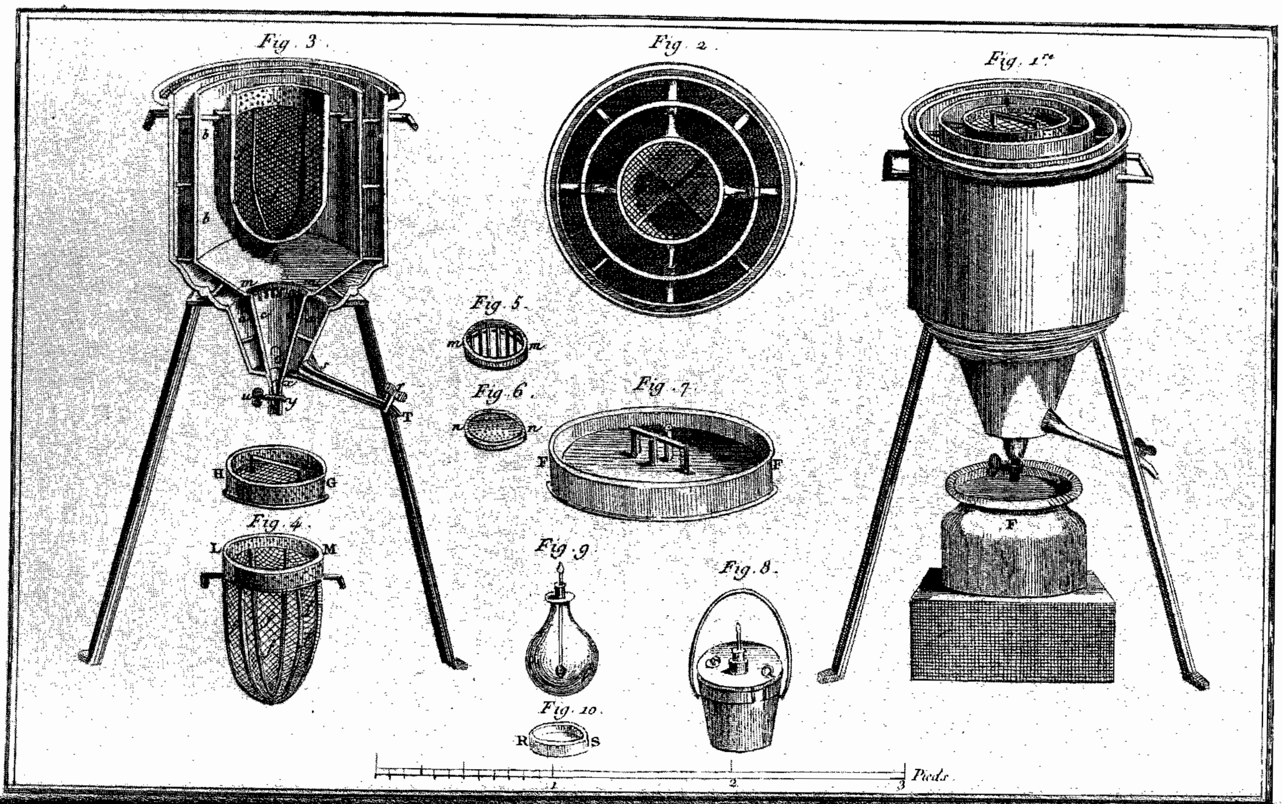

Fig. 3.7. The ice calorimeter of Lavoisier and Laplace. This calorimeter used melting ice to measure the heat given off by a body. Ice packed in the outer space of the calorimeter kept the external temperature constantly at freezing. The sample was placed in the wire basket at the center of the calorimeter and the sample's heat melted ice placed in the interior space (*bbbb*). The water from the melting ice ran out of the bottom of the calorimeter and was weighed to determine the amount of heat produced. *Sources:* Antoine Laurent Lavoisier, *Traité élémentaire de chimie* (Paris, 1789), vol. II, pl. VI. By permission of the Syndics of Cambridge University Library.

theory or the mechanical theory to explain the heat from the sun. One could scarcely believe in a fluid flowing all the way from the sun or a mechanical motion acting over that distance. In the nineteenth century the revival of the wave theory of light suggested a comparable wave theory of heat. According to this theory, all heat was radiant. Even heat conduction was merely the radiation of heat waves from one atom to the next. This erroneous theory effectively replaced the fluid theory. It enjoyed a brief reign, to be replaced in turn by the mechanical theory, now fortified by the kinetic theory of gases and the more abstract mathematical formulations of thermodynamics.

The theory of subtle fluids served its purpose well during the Enlightenment. It made possible the quantification of experimental physics and added a more abstract dimension to the prevailing mechanical philosophy. The theory was also amazingly versatile. Franklin's atmospheres and Nollet's effluvia proved inadequate to the task of accounting for all electrical phenomena, but the primary property of the subtle fluid – its conservation – remained. The subtle fluid of heat survived Rumford's experimental refutation because it was simply too valuable to give up. The mechanical theory provided no model for the conservation of heat or its flow from a hot to a cold place. In most cases, giving up the subtle fluid meant giving up the only model that could be understood in simple terms. The subtle fluids were necessary for the quantification of experimental physics in its early years, but as that quantification progressed in the nineteenth century, especially with the creation of new and more precise measuring instruments, the subtle fluids gradually gave way to even more abstract and more mathematical models.