Chapter Seven
Experiment: How to Learn Things about Nature in the Seventeenth Century

I Reconfiguring experience

Aristotle had asserted unequivocally that all knowledge has its origins in experience. He was echoed by scholastic Aristotelians, so that the aphorism "there is nothing in the mind which was not first in the senses" became a standard philosophical maxim in the later Middle Ages.[1] Despite this fact, many non-Aristotelian philosophers in the seventeenth century had taken to criticizing the approaches to learning about nature that were promulgated by scholastic learning for *ignoring* the lessons of the senses. Francis Bacon was but one among many in his stated view that Aristotle "did not properly consult experience . . .; after making his decisions arbitrarily, he parades experience around, distorted to suit his opinions, a captive."[2] Bacon's became a common view: Aristotelian philosophy was commonly represented during the century as being obsessed with logic and verbal subtleties, reluctant to grapple with things themselves as encountered through the senses. The rhetoric of the Baconian Royal Society came equally to incorporate such a picture of Aristotelianism, its spokesmen making frequent remarks dismissive of scholastic obsession with words instead of things.

Galileo too, among many others, had attempted to dramatize what he saw as the emptiness of the official school philosophy. In Galileo's *Dialogo* of 1632, Simplicio (the Aristotelian character) at one point purports to explain why bodies fall by reference to their *gravity*. Salviati, who speaks for Galileo, replies by ridiculing the use of a *word* as an explanation. What is it that moves earthly things downwards? "The cause of this effect," says Simplicio, "is well known; everybody is aware that it is gravity." "You are wrong, Simplicio; what you ought to say is that everyone knows that it is called 'gravity.' What I am asking you for is not the name of the thing, but its essence, of which essence you know not a bit more than you know about the essence of whatever moves the stars around."[3]

Why was Aristotle's natural philosophy associated by its critics with a neglect of the lessons of experience and the favouring of empty words? The answers to this question will illuminate just what the new emphasis on experimental knowledge meant in the seventeenth century. As we saw in Chapter 1, section I, Aristotle's philosophy was centrally about understanding rather than discovery. Aristotle, while in practice very interested in empirical facts of all kinds (as found especially in his zoological writings), wanted above all to solve the problem of how we are to understand ourselves and the world around us. Thus, in his more abstract philosophical writings, such as the *Metaphysics*, or in his logical writings, the specific lessons of the senses are largely sidelined in favour of analyses of how to argue, how to understand, and in what terms we must make sense of our experiences. In the *Posterior Analytics* especially, Aristotle attempts to show how an ideal science should be structured so that it would be able to account for empirical truths; the acquisition of those truths was not centrally at issue, and neither were any particular such truths themselves. Thus when Aristotle's followers considered what Aristotelian natural science should look like, the model that they examined was one in which empirically acquired truths were taken as given, with only their explanation being the truly important task. In a sense, therefore, an Aristotelian world was not one in which there were countless new things to be discovered; instead, it was one in which there were countless things, mostly already known, left to be *explained*.[4] That Aristotle himself does not seem to have believed this is beside the point; it was nonetheless the lesson that his scholastic followers in medieval and early-modern Europe tended to draw from those of his writings that they found most interesting and most teachable.

The typical expression of empirical fact for such an Aristotelian was one that summed up some aspect of how the world works. "Heavy bodies fall" is a typical example: it was a statement that acted as an unquestioned reference-point in a network of explanations that involved such things as the terrestrial elements and their natural motions, final causes, and the structure of the cosmos.[5] Such statements appeared in already generalized form, rather than in the form of singular experiences referring to historically specific events. One did not say "this heavy body fell when I dropped it"; one simply said that all heavy bodies always fall – that is how nature behaves. In the absence of the reported particular, no room was left for the denial or affirmation of a universal claim about how *all* heavy bodies behave. The assumption was that everyone, from everyday experience, already knows it to be true. The philosopher's job, according to Aristotle, was to show *why* it was true. This was a matter of giving appropriate causal explanations that would, in the ideal case, show why the fact to be explained was *necessarily* true given the attendant circumstances. Needless to say, ideal cases were seldom, if ever, met with.

Understanding the sway, in early-modern Europe, of Aristotelian ways of formulating such questions involves seeing how even the most strongly

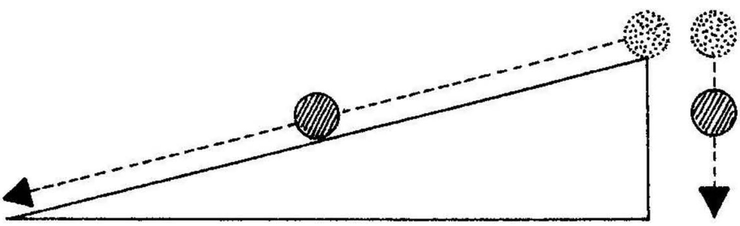

Figure 7.1 *Galileo's use of the inclined plane to slow down the acceleration of free-fall, thus making it easier to measure.*

anti-scholastic of philosophers could still take those ways for granted, as foundational aspects of their thought. For example, the dominant scholastic-Aristotelian way of conceptualizing and handling experience forms the backdrop to Galileo's famous work on the fall of heavy bodies, finally published in the *Discorsi* of 1638 although reflecting work largely completed by 1609.[6] Galileo tries at one point to establish the truth of his claimed experience that a falling body accelerates as it descends, its distance from the place of release increasing in direct proportion to the time elapsed. This experience takes the form of a standard Aristotelian generalization, describing how things behave in nature; Galileo does not describe a specific experiment or set of experiments carried out at a particular time, together with a detailed quantitative record of the outcomes. Instead, he simply says that, using apparatus of a kind carefully specified, he had found that the results of rolling balls down an incline and timing their passage yielded results that agreed exactly with his expectations, in trials repeated "a full hundred times." This last phrase (found frequently, in various forms, in contemporary scholastic writings) means, in effect, "countless times." Galileo wished to persuade his readers that the results amounted to common experience. His problem, however, was that the particular experience that he wished his readers to accept was *not* in fact one that is well known and familiar.

The subsequent rise to dominance of reported experimental events as the foundations of scientific arguments would be attended by just these difficulties. When a natural phenomenon was well known, it could be adduced as part of natural philosophical reasoning with no difficulty, because no one would be likely to contest it. But if the phenomenon were not well known, and instead brought to light only through careful and unusual experimentation, how could the natural philosopher make it acceptable for use in creating philosophical explanations? Galileo wished to have his readers believe that things behaved in nature just as he said they did. He could not rely on his readers already being disposed to accept the truth of the foundational natural behaviours that he discussed (uniform acceleration in fall), but at the same time he could not allow the matter to rest on nothing more than his say-so. Some people might have been prepared to accept his claims on the basis of his own personal and institutional authority, but that would not have made his arguments *scientific*. Galileo always adhered to a model of scientific demonstration that came straight from Aristotle: a true scientific explanation should be demonstrative, like the proofs of mathematics, and, like the mathematical theorems of Euclid, proceed on the basis of simple statements that all could accept as true at the outset. Euclid had employed starting points such as "when equals are subtracted from equals, the remainders are equal"; they were intended to be so intuitively obvious that no one could in good conscience deny them. When Aristotelian natural philosophers made arguments on the basis of empirical principles, such as "the sun rises in the east," or "heavy bodies fall," they too relied on the practical undeniability of such truths; everyone could be relied upon to accept them.[7] Experimental results, however, lacked that kind of obviousness, which is why Galileo attempted, in the present case, to render them as routine as possible as quickly as possible. Claiming results that accrued from trials repeated "a full hundred times" was a way of saying "things *always* behave in this way," and hoping that the reader would believe it.

René Descartes confronted similar problems. Like Galileo, Descartes finessed the problem of trust by refusing to acknowledge it as an issue. In the *Discourse on the Method* (1637), he invites other people to assist in his work by contributing "towards the expenses of the observations [*expériences*, which also means "experiments"] that he would need."[8] It was precisely the fecundity of his explanatory principles that required experiments, because, as Descartes himself said, for any given natural phenomenon he could usually imagine more than one possible explanation. Experiments were therefore required to determine which of them might be the true one. Descartes wanted to do all the actual work himself because, he says, receiving information about phenomena from other people would typically yield only prejudiced or confused accounts. He wanted to make the requisite experiences himself or else pay artisans to do them (since the incentive of financial gain would ensure that the artisans would do exactly what they were told). Descartes was intent only on convincing himself. He sidestepped the problem of trust by adopting a supreme selfishness: what convinced him should be good enough for anyone and everyone.

II Mathematical experimentation

These were issues that needed especial confrontation in the mathematical sciences. As various kinds of "physico-mathematics" sprang up in the course of the seventeenth century, the methodological impetus that had driven the emergence of the category served also to emphasize difficulties relating to experimental procedures.[9] The mixed mathematical sciences had often, since their ancient inception, involved the use of specially made apparatus to investigate natural behaviours that were not obvious from everyday experience. Thus astronomy used specialized sighting instruments for measuring precise positions of bodies in the heavens (well before the appearance of the telescope, an additional instrumental resource, in the seventeenth century). Optics used special devices for measuring angles in reflection and refraction. Ptolemy had written important treatises, the *Almagest* and the *Optics*, in both sciences, and he detailed the apparatus that was required for the proper conduct of work in each. The eleventh-century Islamic philosopher known in Latin Europe as Alhazen had written the most important optical treatise used in Europe prior to Kepler's studies, and he too detailed the makeup and use of optical apparatus.[10] As a result, the tradition of mathematical sciences practised by seventeenth-century Europeans involved them by its very nature in questions concerning the validation of artificially generated experience – experience that was *not generally known*.

Consequently, the ideal of an Aristotelian science, wherein the phenomena to be explained were taken as established from the outset, did not in these cases apply. The issue became especially pressing by the beginning of the seventeenth century among people such as the Jesuit mathematicians, who wanted to show that the mathematical disciplines were genuine sciences according to Aristotelian criteria (like Galileo, they were concerned about their status as mathematicians *vis-à-vis* the natural philosophers). Experimental apparatus gave them trouble because of its unobviousness.

Galileo's was a popular solution to this problem among mathematicians. Thus Jesuit mathematical scientists, such as the astronomer Giambattista Riccioli, reported experiments that involved dropping weights from the tops of church towers to determine their acceleration. While, unlike Galileo, Riccioli gave places, dates, and names of witnesses to underwrite his narratives, the way he used those narratives was always to turn them into authoritative assertions of how such matters *always turn out*. Another, especially famous, example of this presentational trick took place in 1648. The mathematician Blaise Pascal, perhaps best known for the famous "Pascal's Triangle," wrote from Paris to his brother-in-law, Florin Périer, in the Auvergne district of provincial France, requesting him to carry out an experiment. Pascal asked him to carry a mercury barometer up a nearby mountain, the Puy-de-Dôme, in order to see whether the mercury's height in the glass tube would change as the trial was conducted at different altitudes. Pascal hoped and expected that it would, because he was convinced that it was the pressure of the air that sustains the column of mercury in the tube, and that air-pressure decreases the higher one goes.[11] The apparatus was itself novel, having been devised in the 1640s in Florence by Evangelista Torricelli, who had been a protégé of Galileo's in the latter's

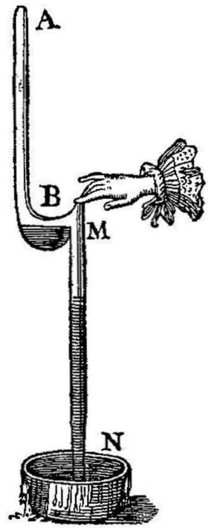

Figure 7.2 *Torricelli's experiment, in a variant by Blaise Pascal. The double arrangement is intended to demonstrate that the mercury is indeed supported by the pressure of the air.*

last years. Like Pascal, Torricelli ascribed the phenomenon to the weight, or pressure of the air (disputes also existed over which of the two, weight or pressure, was the correct way to speak of these matters).

Pascal published a narrative account of the experiment not long afterwards, a report written by Périer with Pascal's introduction and commentary. Périer provides a detailed account of his ascent and descent of the mountain, in the company of named witnesses, and records the height of the mercury that was found each time the apparatus was set up at various stops along the way. At the end of the story, which indeed showed that the mercury stood lower in the tube the higher up the mountain it was measured, Pascal proceeds to turns Périer's narrative into the keystone of a universal philosophical truth. First of all, Pascal uses Périer's results to produce a quantitative correlation of change in height of mercury with change in altitude, already taking it for granted that what Périer had recorded held true of all such measurements. Pascal then predicts the smaller changes in mercury height to be expected if similar apparatus were to be lifted up from the ground to the much lower elevations provided by church towers found in Paris – a more everyday setting than that of Périer's elaborate exploit. Finally, having made specific numerical predictions of the changes to be expected, Pascal then asserts that actual trials confirm the predictions. Like Galileo with his inclined-plane experiments on falling bodies, Pascal gives no details or particularities of these ecclesiastical experiments; they just agree with expectations, as good natural regularities should.

The two central difficulties raised up by experimental procedures, that of establishing trust in experimental narratives and that of establishing universality, or representativeness, for specific experimental outcomes, thus demanded answers with especial urgency in the mathematical sciences because these sciences often sought out unusual or unobvious phenomena. Opinions differed on what would happen to the height of mercury in the glass tube at increasing altitude, before Pascal's brother-in-law ascended the Puy-de-Dôme in an attempt to answer the question – a question that did not already possess a generally accepted answer. The mathematical sciences (which subsumed the work of Pascal and others on mercury barometers) provided their practitioners with specialized knowledge that was hard to use as the basis for a demonstrative science because it was not rooted in universally accepted experience. Somehow, therefore, specialized knowledge had to be *made* into common knowledge. A frequent recourse for astronomers and other kinds of mathematicians was to rely on their individual reputations as reliable truth-tellers. In many cases (such as that of the Jesuit mathematicians), corporate reputations could also be drawn upon: professorships in universities and colleges, or, as in Galileo's case, association with powerful sources of patronage, could lend subtle weight to empirical claims: challenge the result and you were challenging the institution that implicitly certified it.

Astronomers, however, had additional, more concrete ways of bolstering their claims. This is because, traditionally, astronomers did not as a rule publish their raw astronomical data. They did not present lists of observational results, such as measurements of planetary positions, which would then have required acceptance based solely on the astronomer's authority (unless, extraordinarily, similar measurements had been made by others at exactly the same times).[12] Instead, astronomers used their raw data to generate predictive tables of planetary, solar, or lunar positions, using geometrical models designed to mimic apparent celestial motions. This work was presented in such a way as to efface any formal distinction between *observational* astronomy (writing down the numbers that were measured using observational instruments) and those parts of the enterprise centred on the *calculation* of predictive tables from geometrical models – models that were themselves initially justified by their correspondence to the data.

This latter work was the part that might be deemed suitable for publication, but not the former. The predictive tables, rather than the original raw data, served as the *public* warrant for the goodness of the models from which they were computed, since anyone could check at any time to see how accurate those predictions were. In the sixteenth century, after all, Nicolaus Copernicus's reputation as an astronomer rested on his mathematical abilities, not his presumed competence as an observer; astronomers were *mathematicians*. Later on in the century, Tycho Brahe, although famous as an indefatigable observer, did not publish his vast accumulation of observational results; instead, he published mathematical treatments, employing his observational data, of such things as the paths of comets, or of his new earth-centred astronomical system. Tycho hired Johannes Kepler to compute a more accurate model for the motion of Mars on the basis of his raw data, without at the same time allowing Kepler free access to his complete observational records. These records were so far from public that Kepler himself had great difficulty in gaining control of them from Tycho's widow following Tycho's death.

"Experimentation" in the mathematical sciences, then, called on problems related both to trust and to the meaning of results relating to specific times and places. Astronomical practice already addressed such difficulties, as well as potential problems relating to the use of instrumentation in gathering data. In the latter case, instrumentation and apparatus, while usual for the mathematical sciences, were more problematic for areas of inquiry related to qualitative sciences. Francis Bacon's refusal to accept the legitimacy of a distinction between natural and artificial processes (as processes produced with artificial apparatus would be) thus plays an important role in the rhetoric, logic, and practice of experimental science in the seventeenth century.[13]

III "Baconian" experimentation

As we saw in the previous chapter, Bacon's writings were used as an important resource for justifying experimental investigations, especially by the Royal Society of London. Bacon's own position on experiment as a scientific tool is, however, more ambiguous than it at first appears.

Bacon, like Aristotle, stressed the importance of experience in learning the ways of nature. The examples that Bacon used to illustrate a proper use of deliberately contrived experience in making (his kind of) natural philosophical knowledge show exactly the same features of generality, or universality, that characterize the writings of scholastic philosophers. In Book II of the *New Organon* (1620), Bacon presents two worked examples of his new logic of investigation (usually referred to as his "method," although he never called it that). One of these examples concerns the nature of heat: among the listed "Instances meeting in the nature of heat" we find "the sun's rays, especially in summer and at noon"; "solids on fire"; "quicklime sprinkled with water"; and "horse shit, and similar excrement, when fresh."[14] Notice how every one of these is an assertion of a general truth applying to every case of each "instance"; Bacon evidently sees no need to adduce specific observations. This (in its own context, unremarkable) habit is seen again when he refers to some instances of variation in the degrees of heat found in varying circumstances. In giving examples, Bacon sometimes proposes tests the outcomes of which he already knows:

> Try an experiment with burning glasses in which (as I recall) the following happens: if a burning glass is placed (for example) at a distance of a span [i.e. nine inches] from a combustible object, it does not burn or consume as much as if it is placed at a distance of (for example) a half-span, and is slowly and by degrees withdrawn to the distance of a span. The cone and the focus of the rays are the same, but the actual motion intensifies the effect of the heat.[15]

The universality of this description of an experiment is part of its very effectiveness. By describing a trial the outcome of which Bacon claims to know, from the warrant of personal experience ("as I recall"), he tells the reader about something that happens in nature without actually tying it down to a specific event, an occasion on which this was tried with this outcome. Presenting experience in such a manner served to bypass, at least rhetorically, the difficulties that would arise if Bacon's argument had depended on taking his word for an historical event that lacked corroborating witnesses (recall, too, that Bacon was a lawyer). By telling you what *happens* rather than what *happened*, and by giving an account in the form of instructions as to what to do to produce this claimed effect, Bacon can create the illusion of having revealed to his reader a fact about the natural world, one that can then be used to undergird a philosophical argument about the nature of heat.

The form of "Baconianism" adopted, or asserted, or claimed, by the Fellows of the early Royal Society was one centred on the notion of *utility* rather than of *experiment*. Although the early Royal Society is often regarded as a bastion of experimentalism, the kind of experimentalism that it practised was different from that of Bacon, in the same way that it was different from Aristotle's. Where the hallmark of Aristotle's, or Bacon's, kind of scientific experience was the universal generalization, the attempt to appeal to common experience, the hallmark of the Royal Society's was the particular event. When a Fellow of the Royal Society told his audience about an experiment, he did not usually provide a recipe that purportedly revealed a regular feature of the world, as Bacon might have done. Instead, he typically told a story about an event that had happened in the past, to him, at a specific time and place. He did not, that is, make an immediate jump from a particular personal experience to an account of how some aspect of nature habitually behaves.

Here is a quite typical example from the writings of Robert Boyle:

> We took an open-mouthed glass, such as some call jars, and ladies often use to keep sweetmeats in, which was three inches and a half, or better in diameter, and somewhat less in depth, and had the figure of its cavity cylindrical enough. Into this having put some water to cover the protuberance wont to be at the bottom of such glasses, we took a convenient quantity of bees-wax, and having just melted it, we poured it cautiously into the glass, warmed before-hand to prevent its cracking, till it reached to a convenient height.[16]

And so the account continues, circumstantially and with considerable detail, describing an experiment that was intended to refute some criticisms levelled against Boyle's earlier experimental work by Henry More. Boyle's exposition concludes in similar style: "And lastly, we took off by degrees the grain weights that we had put on, till we saw the wax, notwithstanding the adhering lead, rise, by degrees, to the top of the water, above which some part of it was visibly extant."[17]

This style is quite standard for Royal Society publications, including articles in its unofficial journal, the *Philosophical Transactions*. The style went along with a determination on the part of the Fellows to steer clear of speculation or hypothesis, in favour of reporting solid facts. The purpose of such an ethic was not to prevent anyone from making conjectures about natural phenomena and their causes, but to avoid the appearance of a dogmatic adherence to any particular hypothesis on the part of the Society itself. Thus the Society's Curator of Experiments, Robert Hooke, wrote to the Society at the start of his *Micrographia* (1665), that in the book

> there may perhaps be some *Expressions*, which may seem more *positive* then [*sic*] YOUR Prescriptions will permit: And though I desire to have them understood only as *Conjectures* and *Quæries* (which YOUR Method does not altogether disallow) yet if even in those I have exceeded, 'tis fit that I should declare, that it was not done by YOUR directions.[18]

And like Hooke himself, Boyle and other Fellows typically couched such cautious explanations in the terms of corpuscles and their behaviour.

The Royal Society used talk of a Baconian eschewal of hypotheses (which Bacon had decried as "Anticipations of Nature") to retain the integrity of its enterprise: their work was to rely on building up solid accumulations of facts. For this purpose, the particularities of reported, historical experiments, with no positive guarantee that attempted replications would be successful, were the simplest and safest things to discuss. The work of building up reliable theories to subsume and explain those facts was not thereby abandoned, but Boyle and others often spoke of that following stage of their work as residing in the future, to be tackled only when enough solid facts had been accumulated.

The approach of the Royal Society was not to the liking of all natural philosophers in this period, even in England. One of the fiercest critics of the Society was the philosopher Thomas Hobbes, later best known for his political philosophy. Hobbes had served as a secretary to Francis Bacon towards the end of the latter's life, yet despite this personal history, he was dismissive and scornful of the kind of "experimental philosophy" advocated and practised by Robert Boyle and his kind. Hobbes's reasons for this came out most strongly in his critique of Boyle's experiments with air-pumps, in which Boyle had conducted and written about the behaviour and properties of the space left inside an air-pump "receiver" (the glass globe from which the air was pumped). Hobbes poured scorn on Boyle's contention that he had, in these trials, removed practically all the air from the receiver, and in so doing, Hobbes also denigrated the value of such experimental investigation in general.

Hobbes's central objection was that the performance of experiments was not *philosophical*. Knowledge about nature was supposed to be natural philosophy, after all, and yet the kind of knowledge proposed by Boyle and others failed to achieve the universality and necessity that true scientific explanations by definition required. In this, in other words, Hobbes remained wedded to the Aristotelian understanding of what made a true science. Boyle spoke about experiments as historical events, whereas Hobbes wanted to produce demonstrations that would prove their conclusions with necessity, like mathematical demonstrations. Furthermore, Boyle's air-pump experiments consisted of trials conducted using complicated apparatus; why, Hobbes wanted to know, would you examine the behaviour of complex situations before you could make sense of simple, everyday ones?

Boyle emphasized experiment as the best way to make knowledge of nature that would command general assent. Everyone would be able to see for themselves that what was claimed was actually true. Hobbes objected that the kind of knowledge that this represented failed to yield *explanations* for natural phenomena. At best, Boyle could display natural behaviours to which everyone might assent, but there was no way in experimental work to demonstrate what the causes of those behaviours must be. Hobbes stressed the point that, whatever interpretation Boyle might provide for one of his phenomena, Hobbes could always come up with several different ones, each as likely as Boyle's. Hypothetical explanations were easy to make, but, for Hobbes, they were not sufficient to make a true natural philosophy, and he accused Boyle of asserting the existence of a vacuum (which Hobbes denied to be possible) on insufficient grounds:

> The science of every subject is derived from a precognition of the causes, generation, and construction of the same; and consequently where the causes are known, there is place for demonstration, but not where the causes are to seek for. Geometry therefore is demonstrable, for the lines and figures from which we reason are drawn and described by ourselves;

> and civil philosophy is demonstrable, because we make the commonwealth ourselves. But because of natural bodies we know not the construction, but seek it from the effects, there lies no demonstration of what the causes be we seek for, but only of what they may be.[19]

Consequently, for Hobbes, the best that could be done in natural philosophy was to postulate possible causes (he favoured mechanical ones) that were capable of explaining the observed phenomena; but the truth of those causes could never be demonstrated.

Boyle, like most of the leading Fellows of the Royal Society, was himself cautious about hypotheses. His care to avoid dogmatic talk or to ascribe causal explanations in his work led him, for example, to refuse to speak positively on whether the action of the air-pump created a true vacuum in the receiver; that is, whether the space became truly empty. Instead, he spoke of the removal of the "ordinary air," leaving open the possibility that there might be some weightless, undetectable, aetherial medium still present. Boyle used the word "vacuum" to refer to the space inside the receiver when once it was emptied of air, but he made it clear that this operational vacuum was not to be confused with a "metaphysical," true vacuum. Whether a true vacuum existed was a question on which he refused to pronounce, Hobbes's charges to the contrary notwithstanding.

Furthermore, Hobbes's own infatuation with the mathematical, demonstrative model of science was not one from which Boyle radically departed, insofar as this generally accepted ideal could be applied. As he wrote regarding work on buoyancy and displacement, "it is manifested by hydrostaticians after *Archimedes*, that in water, those parts that are most pressed, will thrust out of place those that are less pressed; which both agrees with the common apprehension of men, and might, if needful, be confirmed by experiments."[20] Thus, in establishing for practical purposes the truth of this hydrostatical principle, Boyle was as ready to use "the common apprehension of men" as his starting point as was Aristotle, or Euclid. Experimental confirmation was simply something that was available "if needful." But in matters that were novel and unobvious, special experimental contrivances and their disciplined management were central to Boyle's view of how to learn things about nature.[21]

The *Saggi* of the Accademia del Cimento, published in 1667, were subsequently translated into English by another Fellow of the Royal Society, Richard Waller, and published in 1684 as *Essayes of Natural Experiments*. The anonymity and recipe-like generality of many of the *Saggi's* experimental accounts are somewhat reminiscent of the impersonal recipes by which instruments and their proper uses were often described in mathematical treatises of astronomy or optics; but the first-person (albeit unnamed), circumstantial accounts of the conduct of very many of the experiments suited perfectly the model adhered to by the Royal Society. For example:

> To throw some light on the question, whether the cooling of a body results from the entry of some kind of special atoms of cold, just as it is believed that it is heated by atoms of fire, we had two equal glass flasks made, with their necks drawn out extremely fine. These were sealed with the flame, and we placed one in ice and the other in hot water, where we let them stand for some time. Then, breaking the neck of each under water, we observed that a superabundance of matter had penetrated the hot one, blowing vigorously out of the flask. . . . It seemed to some of us that the same thing should have occurred when the cold one was opened, should the cooling of the air in it have proceeded in the same way . . . i.e., by the intrusion or packing in of cold atoms blown by the ice through the invisible passages of the glass. But it turned out quite the other way.[22]

The centrality of experiments and experimental reports to the business of the early Royal Society resonates awkwardly, therefore, with the work of one of the Society's most celebrated members, Isaac Newton. Newton was a university mathematician, from 1669 the successor to Isaac Barrow as Lucasian Professor of Mathematics in the University of Cambridge, and a man who first came to the Society's collective attention in 1671. He was already familiar with the Royal Society and its work, having studied, among other things, volumes of the *Philosophical Transactions* in the 1660s. Newton evidently wanted to become associated with the group, and to that end sent them a small reflecting telescope of his own design and manufacture. The Fellows rewarded the young Cambridge mathematician with an election to the fellowship. Encouraged, Newton soon afterwards sent to Henry Oldenburg, in the latter's guise as the Society's secretary, a letter describing for the Royal Society some of his studies on optics that related to the ideas behind the telescope that he had sent them.

This letter was not long after published in the *Philosophical Transactions* as "A Letter of Mr. Isaac Newton, Professor of Mathematics in the University of Cambridge; Containing His New Theory About Light and Colours."[23] One of the many features of this celebrated paper is its use of a particularistic, event-focused experimental format to present material that would normally have fallen under the heading of the mathematical science of optics. Thus Newton begins by telling a story about events that had transpired back in 1666. He tells of how he had, for no good reason, got himself a glass prism, and used it to cast a spectrum generated from the rays of the sun projected through a hole in the shutters of a darkened room. (Newton was not the first to play with prisms in an optical investigation; Descartes had used one in his essay "Dioptrics," for instance.) He says that he was "surprised" by the oblong shape of the spectral band of colours, "which according to the received laws of refraction, I expected would have been circular."[24] The length of the spectrum was, he says, five times its breadth, "a disproportion so extravagant, that it excited me to a more than ordinary curiosity of examining from whence it might proceed."[25] Newton's historical account of what happened, and what he did, leads the reader towards a general conclusion that the light from the sun spreads out into a band when refracted through a prism because it is composed of "difform rays, some of which are more refrangible [i.e. "able to be refracted"] than others: so that of those, which are alike incident on the same medium, some shall be more refracted than others, and that not by any virtue of the glass, or other external cause, but from a predisposition, which every particular ray has to suffer a particular degree of refraction."[26]

Furthermore, Newton proceeds to assert, those differing degrees of refrangibility correspond to differing colours of the light exhibiting them. Those rays which are refracted most exhibit the blue-violet colour characteristic of one extreme of the spectrum, whereas those rays which are refracted the least correspond to the red colour visible at the opposite end of the spectrum. The refrangibility of each kind of ray is an unalterable property, remaining constant throughout a number of successive refractions and reflections; furthermore, the colour associated with any particular refrangibility of ray is similarly unalterable. Thus Newton could ascribe numbers to colours, by characterizing any spectral colour in terms of the degree of refrangibility of its ray.

Newton's optical paper to the Royal Society thus goes out of its way to appear non-mathematical. Newton does not provide a geometrical diagram to assist in his preliminary exposition of these experiments; instead, he presents the first part of the paper as an experiment of the kind he knew the Royal Society preferred, an historical account of what he had seen and done on a particular occasion in the past. A shift to a more typical mathematical format, in which general conclusions are stated, occurs only after the central experimental premises have been laid out in narrative form. Fittingly, Newton incorporates into his letter remarks regarding the problems caused by the differential refrangibility of light rays for making telescopes that will focus light-sources precisely rather than blurring them, and explains how he had come to make his reflecting, instead of refracting, telescope as a consequence. The practical, operational, Baconian dimension of the new experimental philosophy was an important part of Newton's enterprise.

Newton's own work came to represent a conception of scientific experience that departed considerably from the old scholastic model, therefore. For an Aristotelian philosopher, "experience" was the proper source of knowledge about the world's habitual behaviour. For Newton and his later followers (and see Chapter 8, below), experimental philosophy was now a means for interrogating nature that yielded, above all, operational rather than essential knowledge – it told you how to do things, rather than what something truly was in itself. Experimentation, as the Royal Society understood it and as Newton refined it, became an approach to knowledge that accumulated records of natural phenomena that owed their general credibility to institutional authority or to the word of appropriate witnesses (Boyle's especial technique).

IV Physiological experimentation

William Harvey's investigations show once more the importance of the accepted, broadly Aristotelian framework for experimental studies in this period, as well as the specific difficulties of experimental study in physiology. His work also further indicates the kinds of practical means available for dealing with problems of credibility.

Harvey's *De motu cordis* of 1628 had opened, as we saw in the previous chapter, with two dedicatory prefaces, one to the king, the other to the College of Physicians. The latter preface did some important work for Harvey, because he was proposing a view of the behaviour of the heart and blood that flew in the face of long-accepted Galenic teaching. Galen (like Aristotle) had taught that the heart is a kind of repository for the blood, which is communicated out to the rest of the body through the network of blood vessels. Galen's specific version of this view distinguished between the system of the arteries, branching out from the left side of the heart, and the system of the veins, which connected to the right side of the heart but was regarded as having its "origin" in the liver. Arterial blood carried heat and *pneuma* (a kind of vitality derived from the air in the lungs) out from the heart to all parts of the body. The veins had a different function, that of distributing nutrition around the body. Venous blood was created in the liver from ingested food, which is why the veins were seen as having their origin in the liver. Blood found its way into the arterial system, where it served its quite different distributive function, by seepage through pores in the wall of the heart. This wall, called the septum, divided the left side of the heart from the right, and the pores in the septum were the only means of communication between the one side and the other that Galen could imagine. The beating of the heart helped in expressing blood out from the heart, but there was no circulatory pumping.

Harvey, by contrast, saw the arterial and venous systems as two components of a larger circulatory system. Blood was pumped out from the left side of the heart through the arteries. The arteries, as they are traced out by the anatomist from the heart, branch out and become, as they do so, more numerous, smaller, and finer. Harvey held that the ultimate status of these branching arteries was as invisibly small blood vessels that gradually linked together again to form the venous system, which served to return the blood to the right side of the heart. So blood left the heart through the arteries and returned to the heart through the veins. Furthermore, there were no pores in the septum. Instead, venous blood found its way to the heart's left side by making a "pulmonary transit" from the heart's right side through special blood vessels that carried it through the soft, spongy tissue of the lungs (with the blood vessels again having subdivided into invisible tubelets), before returning through appropriate blood vessels from the lungs to the left side of the heart. The full circulation then having been completed, the blood could thereafter be sent out once again via the arteries.

The "pulmonary transit" was an idea that had already been put forward at Harvey's *alma mater*, the University of Padua, in the later decades of the sixteenth century, and was the element of his mature ideas that Harvey had presented in his 1616 Lumleian lectures.[27] The full, or "general," circulation around the body was Harvey's real, and spectacular, innovation.

Now, this picture was not one that could be demonstrated by simply opening a living animal body and looking. Its establishment required Harvey to make a large number of experiments on a wide variety of animals, from shellfish to human beings, and to elucidate what he saw by means of arguments. One of the chief difficulties of the work was in making it clear to others that he really had seen what he claimed to have seen, and that his inferences genuinely followed from that evidence. This is where the preface addressed to the College of Physicians played an important role:

> The booklet's [i.e., *De motu cordis'*] appearance under your aegis, excellent Doctors, makes me more hopeful about the possibility of an unmarred and unscathed outcome for it. For from your number I can name very many reliable witnesses of almost all those observations which I use either to assemble the truth or to refute errors; you so instanced have seen my dissections and have been wont to be conspicuous in attendance upon, and in full agreement with, my ocular demonstrations of those things for the reasonable acceptance of which I here again most strongly press.[28]

In effect, Harvey was informing potential critics that if they doubted or denied his assertions, they would at the same time be doubting or denying the "full agreement" of the members of the most illustrious medical institution in England. These sorts of social relationships, whether with a royal patron, a socially accredited professional society, or even with respected gentlemen, all served to render more plausible an individual's truth claims. Experimental assertions, in order to be treated as if they were philosophical assertions, needed as much shoring up as they could get, from whatever quarter available.

Harvey himself, when later debating his views on circulation with a critic, stressed the fundamental issue at stake: "Whoever wishes to know what is in question (whether it is perceptible and visible, or not) must either see for himself or be credited with belief in the experts, and he will be unable to learn or be taught with greater certainty by any other means."[29]

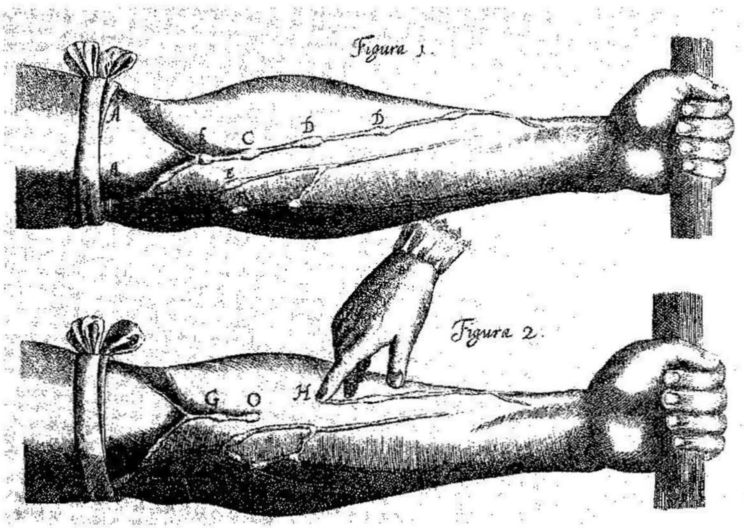

Figure 7.3 *An "ocular demonstration" of the function of the valves in the veins, from Harvey's* De motu cordis.

Harvey wanted this necessary recourse to experience and accredited testimony to be accepted as legitimate in making natural philosophy. To establish the point, he appealed to the usual touchstone of certain knowledge, mathematics: "If faith through sense were not extremely sure, and stabilized by reasoning (as geometers are wont to find in their constructions), we should certainly admit no science: for geometry is a reasonable demonstration about sensibles from non-sensibles. According to its example, things abstruse and remote from sense become better known from more obvious and more noteworthy appearances."[30] If mathematics can be accepted as certain and scientific, so too should a properly conducted experimental science – such as his own work in physiology.

The senses remained paramount in the sciences revolutionized by the new breed of philosopher in the seventeenth century, therefore, and one of the key tools for generating knowledge from them was the experiment. Experiment, understood as the making of specific trials of phenomena, typically with contrived circumstances or apparatus, was a particular kind of sensory experience that went beyond a simple inventory of what all or most people already knew was in the world. In this sense, experiment was about *discovery*, about finding out new things. As such, it had to incorporate means of protecting the discoverers from being disbelieved.