Miracles, Experiments, and the Ordinary Course of Nature

*By Peter Dear**

> They say miracles are past, and we have our philosophical persons, to make modern and familiar, things supernatural and causeless. Hence is it that we make trifles of terrors, ensconcing ourselves into seeming knowledge when we should submit ourselves to an unknown fear.
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> —Lafew, in Shakespeare's *All's Well That Ends Well,* II, iii.

THE TERM "EXPERIMENTAL PHILOSOPHY" is associated with a characteristically English approach to studying nature in the second half of the seventeenth century. English experimentalism was a self-conscious attitude toward natural philosophy that such men as Robert Boyle, Robert Hooke, Joseph Glanvill, and other Fellows of the Royal Society deliberately promoted, but it was more than mere posturing: it reflects something genuine about English natural philosophical practice in that period. The basic unit of knowledge that the fellows recognized was what Steven Shapin and Simon Schaffer have identified as the "matter of fact."[1] "Matters of fact" were supposedly theory-neutral statements of bald phenomena, "facts" about what someone had actually experienced. The constitution of a matter of fact in the public domain, however, took place at the level of literary construction. The writing of a matter of fact gave it a meaning that enabled it to form part of natural philosophical discourse.

The empirical content of writings by Boyle or Hooke, or of the early *Philosophical Transactions,* takes the form of historical reportage of events—accounts of what the author witnessed as a result of chance observation or, more typically, as a result of deliberate contrivance, often including place, date, and even the names of witnesses.[2] The historical relation of an event wherein a contrived situation gave rise, on that occasion, to a particular result is the hallmark of a true "experiment" in the characteristic sense of seventeenth-century English "experimental philosophy." That event, furthermore, legitimates its associated

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[1] Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton, N.J.: Princeton Univ. Press, 1985); see also Simon Schaffer, "Making Certain," review of Barbara J. Shapiro, *Probability and Certainty in Seventeenth-Century England,* in *Social Studies of Science,* 1984, *14*:137–152.

[2] See Peter Dear, "*Totius in verba:* Rhetoric and Authority in the Early Royal Society," *Isis,* 1985, *76*:145–161.

knowledge claim rather than merely illustrating it. By contrast, reported experiences are much less common, and appear to serve a more restricted function, in the Continental science of the period.[3]

Observations of systematic differences between Protestant, especially English, science in the seventeenth century and its Continental, Catholic counterpart were made by Robert Merton in his famous monograph of 1938 and in a refined form more recently by Thomas Kuhn.[4] Kuhn based his refinement on the observation that the Protestant nations of England and the Netherlands (as well as French Huguenots) tended to work in qualitative, "experimental" areas of natural philosophy, whereas Catholics (such as Descartes, Galileo, and Pascal) tended to excel in the quantitative, "classical" mathematical sciences. The implication is that this phenomenon resulted from deep sociocultural patterns that both religious and scientific behaviors made manifest. Individual exceptions notwithstanding (the Protestants Newton and Huygens are certainly on the mathematical side of the divide), Kuhn's overall picture of divergent tendencies between Catholic and Protestant societies has some prima-facie plausibility, as the particular characteristics of English experimental philosophy suggest. Understanding why the correlations exist, however, requires more than just empirical observation; and the picture is certainly more complex than Merton's "Puritan ethic" approach suggests.

A point of entry into these issues may be found in Thomas Sprat's *History of the Royal Society* of 1667. Sprat maintained that, for the experimental philosopher, the Christian revelation has "all bin prov'd to him his own way. Had not the appearance of *Christ* bin strengthen'd by undeniable signs of *almighty Power,* no age nor place had bin oblig'd to believe his Message. And these *Miracles* with which he asserted the *Truths* that he taught (if I might be allow'd this boldness in a matter so sacred) I would even venture to call *Divine Experiments* of his *Godhead.*"[5] Christ's miracles were experiments that confirmed his doctrinal assertions.

Sprat's remarks prompt the following questions. How, in seventeenth-century England, did the experiment as an element of natural philosophical argument receive sociocultural legitimation—a legitimation so unquestionable that it could even, without fear of blasphemy, be applied to talking about Christ? What did miracles have to do with experiments? And how did forms of natural philosophy different from that dominant in England relate to alternative, non-Anglican assumptions about miracles? This essay will argue that the patterns of behavior found in the handling of miracles in everyday French life involved a set of inferential practices that privileged universalized experiences of precisely the sort found in the mixed mathematical sciences over the singular experiences found at the heart of English experimental philosophy. Those inferential practices implicated ideas about the ordinary course of nature, the standard against which

[3] See Section I, below. I use the blanket term *science* to denote both natural philosophy and the classical mathematical sciences, which were treated as distinct fields in most contexts (especially pedagogical) during the seventeenth century despite their increasingly perceived alliance as tools in confronting nature.

[4] Robert K. Merton, *Science, Technology and Society in Seventeenth-Century England* (1938; rpt., New York: Harper Torchbooks, 1970); and Thomas S. Kuhn, "Mathematical versus Experimental Traditions in the Development of Physical Science," *Journal of Interdisciplinary History,* 1976, 7:1–31 (reprinted in Kuhn, *The Essential Tension* [Chicago: Univ. Chicago Press, 1977]).

[5] Thomas Sprat, *History of the Royal Society* (St. Louis: Washington Univ. Press, 1958, facs. of 1667 ed.), p. 352.

miracles were judged. By contrast, the absence of miracles in everyday life in England (formalized in the Anglican doctrine of the cessation of miracles) corresponded to a different set of inferential practices that granted the singular experience a foundational rather than incidental relation to authentic knowledge of nature. The "ordinary course of nature" was not in England the preexisting condition of possibility for natural knowledge that a confrontation with actual miracles would have required.

I. CONTRIVED EXPERIENCES AND LAWS OF NATURE

The dominant ideal of natural knowledge in the seventeenth century involved the crucial assumption that true knowledge was shared or shareable, that knowledge was common property. Alternatives to this ideal certainly existed, and they lay at the heart of conflicts between it and alchemical and secretist models of knowledge—such as the disputes Andreas Libavius had with Oswald Croll and Tycho Brahe, those Johannes Kepler and Marin Mersenne had with Robert Fludd, and those Boyle had with Thomas Hobbes; much the same distinction plays a role in Newton's taking care not to publish his alchemical work.[6] But within the ideological framework that did emphasize the openness and accessibility of knowledge, knowledge claims had to be socially rooted—that is, natural knowledge needed, by definition, to be seen as the common property of a nonexclusive group rather than being a private, personal conviction or the creed of a secret sect.

At the beginning of the seventeenth century, the dominant form of this ideal of shareable, public knowledge about nature was Aristotelian. Accordingly, nonsecretist natural philosophers before about 1650 required of true knowledge that it be demonstratively certain. Galileo, Descartes, Pascal, even Francis Bacon, as well as Jesuits like Christoph Scheiner and Giovanni Battista Riccioli, all wanted formal scientific structures capable of guaranteeing the certainty, or universal accreditation, of conclusions regarding the way things are or happen in the world.[7] The constitution of experience and experiential statements corresponding to this requirement found its definitive formulation in the Aristotelian texts of the universities and colleges.[8] In the *Posterior Analytics* Aristotle had defined "experience" as something that arises from the accumulated memory of repeated sensory instances, and he seems to have been followed by commentators and textbook writers well into the seventeenth century. We know by "experience," for

[6] Owen Hannaway, *The Chemists and the Word: The Didactic Origins of Chemistry* (Baltimore: Johns Hopkins Univ. Press, 1975); Hannaway, "Laboratory Design and the Aim of Science: Andreas Libavius versus Tycho Brahe," *Isis,* 1986, *77*:585–610; Peter Dear, *Mersenne and the Learning of the Schools* (Ithaca, N.Y.: Cornell Univ. Press, 1988), pp. 109–112; Shapin and Schaffer, *Leviathan and the Air-Pump* (cit. n. 1); Richard S. Westfall, "Newton and Alchemy," in *Occult and Scientific Mentalities in the Renaissance,* ed. Brian Vickers (Cambridge: Cambridge Univ. Press, 1984), pp. 315–335; J. V. Golinski, "The Secret Life of an Alchemist," in *Let Newton Be!* ed. John Fauvel, Raymond Flood, Michael Shortland, and Robin Wilson (Oxford: Oxford Univ. Press, 1988), pp. 147–167; and Carlo Ginzburg, "High and Low: The Theme of Forbidden Knowledge in the Sixteenth and Seventeenth Centuries," *Past and Present,* 1976, No. 73, pp. 28–41.

[7] Hypotheticalism or probabilism only became properly respectable after mid-century; in the first half of the century astronomical or cosmological systems formed a focus of speculations on the possibility of demonstrative certainty regarding nature. See Nicholas Jardine, *The Birth of History and Philosophy of Science: Kepler's* A Defence of Tycho against Ursus, *with Essays on Its Provenance and Significance* (Cambridge: Cambridge Univ. Press, 1984).

[8] Such texts embodied many varieties of Aristotelianism, usually with non-Aristotelian admixtures, but I am aware of no controversy over the present point. See esp. Charles B. Schmitt, *Aristotle and the Renaissance* (Cambridge, Mass.: Harvard Univ. Press, 1983).

example, that night follows day, because we are used to seeing it happen. Aristotelian experience is not about having witnessed a singular event; it was therefore perfect for incorporation into a body of shared, established, public knowledge—"demonstrated" knowledge.[9] A statement of experience was acceptable because, at least ideally, it was what everyone knew. It was a universal statement of common experience, and could therefore be used as a premise in a scientific, syllogistic demonstration: just like the axioms of geometry, it was evident, and so required no formal proof. This was the standard philosophical meaning of "experience" in the first half of the seventeenth century.[10]

If, therefore, in the course of an argument or discussion, a natural philosopher referred to a behavior or appearance in nature that happened routinely and with which everyone was familiar from daily experience, no difficulty of assent would arise. If, however, the behavior or appearance was one to which the natural philosopher was privy by virtue of having created it himself, and which was therefore outside the experience of everyone else, that private experience would have to be communicated and established as an authentic item of knowledge by means of a report. Now, the mere demand that other people lend credence to one's report could not in itself constitute the establishment of true knowledge about nature. For the knowledge claim of an individual to become an authentic—that is, a publicly constituted—knowledge claim, certain techniques of presentation were needed, and those techniques turned on an appeal to assumptions and cognitive criteria generally taken as evident and appropriate to such authentication.

Blaise Pascal's management of the experiences that he deployed in his natural philosophical writings provides an illuminating example. Pascal presents his pamphlet *Expériences nouvelles touchant le vide* (1647) above all as an attempt to establish his priority in the invention of many of the experiences he describes. We know how Pascal wanted his work to be perceived from statements in his letters to the Jesuit Noël and also from his more general reflections in his *L'esprit géométrique*. He wished to present certain demonstration, based on logical inference from experience. But how did he think that he could achieve this? Pascal writes that his *Expériences nouvelles* is the outline of a projected fuller work, an outline which he has decided to publish in part because of the following consideration:

> Having made experiences with much expense, care, and time, I feared that another, who would not have employed the time, the money, or the care, anticipating me, would give to the public things that he had not seen, and which in consequence he would not have been able to report with the exactitude and order necessary to interrelate [*déduire*] them properly: there being no one who has had tubes and siphons of the length of mine, and few who would wish to give themselves the trouble necessary to get them.[11]

[9] Not necessarily in a mathematical sense, although appeals to the geometrical paradigm of demonstration became increasingly common in the seventeenth century, as is discussed below.

[10] This is more fully discussed in Peter Dear, "Jesuit Mathematical Science and the Reconstitution of Experience in the Early Seventeenth Century," *Studies in History and Philosophy of Science*, 1987, *18*:121–164. The concept of experience in the sense described is of course found throughout the second half of the century in scholastic-Aristotelian texts. The word had a different sense in secretist philosophies, meaning, as it did for Roger Bacon, personal illumination, analogous to religious experience.

[11] Blaise Pascal, *Oeuvres complètes*, ed. Louis Lafuma (Paris: Editions du Seuil, 1963) (hereafter **Pascal, *Oeuvres***), p. 196. All translations, unless otherwise noted, are mine; I here translate "expéri-

Two things emerge from this passage. The first is that Pascal acknowledges that the experiences that form his subject are ones that only he has properly had—and that very few people would be likely to go to the trouble of acquiring. The second, in consequence, is that he expects his reports of those experiences to act as the basis of proper inferences about nature. The properties of the reports are therefore crucial in generating knowledge; Pascal himself testifies to this in saying that other people could not be relied upon to report the experiences "with the exactitude and order necessary to interrelate them properly."

Pascal's concern is therefore with turning particular contrived events into publicly available—and in some sense "evident"—experiences. They need, that is, to function as if evident to enable them to undergird demonstrations. Pascal's little treatise begins with eight itemized "experiences."[12] These are not, however, historical reports of individual trials, or "experiments"; they are instead descriptions of what happens under particular contrived conditions. They are universal statements about how things happen, suitable for Pascal's use in deriving other universal statements about nature. His later writings on hydrostatics and the weight of the air exhibit exactly the same literary form.[13] Bearing in mind Pascal's explicit expectation that most people will lack the opportunity or inclination to verify his experiential claims, the reader must accept his assertions on faith. But Pascal needed to make those assertions rhetorically effective to win that faith. There seems to have been no question of anticipating challenges to the actuality of the experiences he asserts, and in fact objections, most famously those of Father Noël, concerned only the subsequent inferences and reasonings built upon them. That is because Pascal's experiences, and the prototypical Torricellian "experiment" itself, did not in themselves contradict anything already generally accepted. The trouble arose only when they were used to argue for the possibility of a void in nature. Pascal could confidently assert them as true experiences regarding the behavior of nature so long as he presented them as philosophically neutral appearances;[14] and the step from particular experimental events to statements about how things always turn out under these circumstances could be effected silently—a surreptitious shift from "this is what I saw when I did this" to "this is what happens when this is done."

A similar example, this time from the second half of the century, provides an

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ences" as "experiences" rather than "experiments" to avoid begging some of the questions with which this article deals.

[12] Pascal, *Oeuvres,* pp. 196–197. For an illuminating discussion of these experiences (involving barometer tubes, siphons, and syringes with water, wine, and mercury), see Thomas More Harrington, *Pascal philosophe: Une étude unitaire de la pensée de Pascal* (Paris: Société d'Edition d'Enseignement Supérieur, 1982), pp. 48–51. Harrington describes how each item builds on presuppositions set up by its predecessors, such as moving from small transparent pieces of apparatus to larger opaque ones; the behavior of liquids inside the opaque apparatus can be taken as evident, even though not visible, because of implied analogies with the earlier observable arrangements.

[13] This form of presentation was used routinely in such matters, as in mixed mathematical treatises generally: cf., e.g., barometric material in Pierre Guiffart's *Discours du vuide* (Rouen, 1647); Jean Pecquet's *Experimenta nova anatomica* (Paris, 1651); Emmanuel Maignan's *Cursus philosophicus* (Lyons, 1673; original edn. 1653). The occasional appearance of historical reportage in these texts should be compared with the analysis given below of Pascal's Puy-de-Dôme "experiment."

[14] Cf., on the importance to Boyle of the "matter of fact," Shapin and Schaffer, *Leviathan and the Air-Pump;* and Schaffer, "Making Certain" (both cit. n. 1). The social-epistemological function of Pascal's matters of fact seems somewhat different, however. For a discussion of Pascal's "experiences" in this text and their idealized nature see Alexandre Koyré, "Pascal savant," in Koyré, *Metaphysics and Measurement: Essays in Scientific Revolution* (Cambridge, Mass.: Harvard Univ. Press, 1968), pp. 131–156.

explicit contrast with what became standard experimental practice in England. Robert Boyle's eponymous "law" entered the world in the form of a detailed account of particular experimental trials that included precise measurements (even differing somewhat from the supposed ideal) that were the outcome of an attempt to demonstrate the spring of the air. When, however, in 1676, Edmé Mariotte published his version of Boyle's law and its justification, he proceeded in a quite different manner.[15] Where Boyle had made experimental trials the core of his presentation, Mariotte set up his own account so as to allow minimal justificatory significance to historical reportage of specific events.

Mariotte starts out by postulating that the condensation of air occurs in direct proportion to the weight exerted on it. He gives an argument in support of that suggestion (drawn from considerations of the compression of the air by its own weight in the atmosphere) and only then proceeds to discuss apparatus and experimental procedures. The discussion takes the form of detailing how a mercury barometer that also contains air, employed in a specified way, would manifest the postulated relationship between incumbent weight and compression. Then comes the test: "To find out if this consequence was true [*véritable*], I tried it [*j'en fis l'expérience*] with Mr. Hubin, who is very expert at making barometers." Mariotte describes the resulting measurements, which accord with the postulate; gives another historical narration of a similar affair; and proceeds to set the seal of accomplishment on his account: "I furthermore made some other similar experiences, leaving more or less air in the same tube, or in others more or less large, and I always found that after the experience was made," the height of mercury rested where it ought according to the postulate: "which makes sufficiently evident [*fait connoître suffisamment*] that one can take for a certain rule or law of nature, that air condenses in proportion to the weight by which it is loaded."[16]

There follows a page of instructions on how to make "des expériences plus sensibles," consisting of tips and suggestions concerning apparatus and procedures. Mariotte rounds out the discussion with three problems based on his "law of nature" that show the calculation of outcomes (resultant heights of mercury columns) given particular initial conditions, presented with solutions in the manner of geometrical problemata.[17] The disappearance at this stage of historical reportage of specific events underlines the role of such reportage as the mere occasion for knowledge creation and legitimation, rather than as constitutive of them. Mariotte's treatise deploys such reportage very sparingly, whereas a work such as Boyle's *New Experiments Physico-Mechanicall touching the Spring of the Air* is largely constructed from it. Mariotte's historical accounts serve only to signify novelty, whereas the legitimation of that novelty consists in arguing for its likelihood in advance and rendering its production routine through the constitu-

[15] Robert Boyle, *A Defence of the Doctrine touching the Spring and Weight of the Air . . . against the Objections of Franciscus Linus* (London, 1662), Ch. 5, pp. 57–68; and Edmé Mariotte, "Discours de la nature de l'air," in *Oeuvres de Mr. Mariotte* (Leyden, 1717), pp. 149–182. See also Charles Webster, "The Discovery of Boyle's Law, and the Concept of the Elasticity of the Air in the Seventeenth Century," *Archive for History of Exact Sciences,* 1965, *2*:441–502. For details and diagrams of Mariotte's procedures see Pierre Costabel, "La loi de Boyle-Mariotte," in *Mariotte, savant et philosophe (d. 1684): Analyse d'une renommée* (Paris: J. Vrin, 1986), pp. 65–73.

[16] Mariotte, "Discours," p. 152.

[17] *Ibid.*, pp. 153, 154–156. See below on the more general use of the geometrical model in relation to the behavior of contrived apparatus.

tion of a "law of nature,"[18] describing procedures regularly manifesting that law, and even presenting "problems" that serve, ironically, to render the law an unproblematic assumption. Mariotte is telling the reader what happens, not what happened.

Neither Pascal nor Mariotte, therefore, was talking about experiments in the sense of discrete events carried out at specific times and places. Sprat's remarks on miracles as "divine experiments," by contrast, clearly do make appeal to experiments as discrete events, because that is what miracles themselves are. However, Sprat designates as "Experiments" events that are, by definition, contrary to the ordinary course of nature, whereas "experiments" in the usual natural philosophical sense were events that exhibited the ordinary course of nature. Sprat's willingness to accept true experimental events, in apparent contrast to Pascal, connects with the nature and place of miracles in seventeenth-century culture.

II. THE MEANING OF MIRACLES

Sprat was a Restoration Anglican apologist,[19] and the orthodox Anglican then held that the age of miracles was past; the need for them had not existed since the revelations recorded in the New Testament. Christianity had indeed "maintain'd it self in its first Age, by the innocence, and miracles, and suff'rings of its Founder, and his Apostles. But after their Deaths, when Christianity began to spread into the farthest Nations . . . the power of working wonders had ceas'd."[20] This doctrine was partly directed at domestic sectaries, enthusiasts

[18] Steven Shapin, "Robert Boyle and Mathematics: Reality, Representation, and Experimental Practice," *Science in Context,* 1988, *2*:23–58, explains how, and why, Boyle did not actually enunciate Boyle's law as a law of nature. Commenting on the results of experiments measuring the condensation of air, Boyle remarks that "till further tryal hath more clearly informed me, I shall not venture to determine whether or no the intimated Theory will hold universally and precisely": Boyle, *A Defence* (cit. n. 15), p. 62. The concluding paragraph of this chapter of Boyle's work (p. 68) stresses his central concern, which is to show that Linus's "funiculus" hypothesis is unnecessary to explain the phenomena; the weight and spring of the air are sufficient. That is what the experiments and tables summarizing them are really about.

[19] P. B. Wood, "Methodology and Apologetics: Thomas Sprat's *History of the Royal Society*," *British Journal for the History of Science,* 1980, *13*:1–26; and Hans Aarsleff, "Thomas Sprat," in *Dictionary of Scientific Biography,* ed. C. C. Gillispie, 16 vols. (New York: Scribners, 1970–1980).

[20] Sprat, *History* (cit. n. 5), pp. 10–11. This paper is not the place to investigate the complex nuances of the issue, and there is comparatively little literature on it. See D. P. Walker, "The Cessation of Miracles," in *Hermeticism and the Renaissance: Intellectual History and the Occult in Early Modern Europe,* ed. Ingrid Merkel and Allen G. Debus (Washington, D.C.: Folger Books, 1988), pp. 111–124, focusing on the origins of the doctrine in the later sixteenth and early seventeenth centuries; and Walker, *Unclean Spirits: Possession and Exorcism in France and England in the Late Sixteenth and Early Seventeenth Centuries* (Philadelphia: Univ. Pennsylvania Press, 1981), esp. pp. 66–73, which discusses attempts to decide what counts as a miracle (is exorcism miraculous or natural vis-à-vis the nature of demons?). On the place of the doctrine in Restoration apologetics see Eamon Duffy, "Valentine Greatrakes, the Irish Stroker: Miracle, Science, and Orthodoxy in Restoration England," in *Religion and Humanism,* ed. Keith Robbins (Studies in Church History, 17) (Oxford: Basil Blackwell, 1981), pp. 251–273; and Daniel A. Beck, "Miracle and the Mechanical Philosophy: The Theology of Robert Boyle in Its Historical Context" (Ph.D. diss., Univ. Notre Dame, 1986), pp. 91–96. See also Keith Thomas, *Religion and the Decline of Magic* (London: Penguin, 1973), pp. 92, 126–127, 578, for scattered manifestations of the doctrine in seventeenth-century England.

Robert Boyle admitted the possibility of modern miracles in principle (as did Sprat), but in practice he wished to reserve them to Christ and the apostles: see James R. Jacob, *Henry Stubbe, Radical Protestantism and the Early Enlightenment* (Cambridge: Cambridge Univ. Press, 1983), pp. 55–56, 164–174, concerning Greatrakes; Jacob, *Robert Boyle and the English Revolution* (New York: Burt Franklin, 1976), pp. 164–176; Duffy, "Greatrakes," pp. 267–269; and Beck, "Miracle," pp. 237–241.

who accredited their claims by themselves performing miracles, but even more (as had been the case since the original promulgation of the doctrine during the Continental Reformation) at the Catholic Church, which used the continuing history of miracles as evidence of its legitimate possession of apostolic authority.[21]

The practical management of miracles was therefore much more of an everyday affair in Catholic countries than it was in England. In France, for example, ex-voto images, usually small paintings, were routinely donated to chapels by the beneficiaries of such things as deliverance from a storm at sea or a grave illness, in recognition of divine intercession. These deliverances did not usually qualify as true miracles, but their popular acceptance as forms of divine aid in the face of natural calamity expressed a similar belief that Providence often manifested itself in physical effects.[22] Francis Bacon, in the *Novum Organum,* shows a Protestant disdain for this Catholic practice by portraying it as merely pagan. It betrays the working of an Idol of the Tribe, whereby

> the human understanding when it has once adopted an opinion . . . draws all things else to support and agree with it. . . . And therefore it was a good answer that was made by one who, when they showed him hanging in a temple a picture of those who had paid their vows as having escaped shipwreck, and would have him say whether he did not now acknowledge the power of the gods,—"Aye," asked he again, "but where are they painted that were drowned after their vows?" And such is the way of all superstition.[23]

Catholics needed modern miracles, whereas most Protestant denominations, including Anglicans, wanted to put them safely into the past. Bacon's attitude portrays clearly the cultural differences at work.

One of the reforms instituted by the Council of Trent in 1563 regularized the procedure of authenticating miracles so as to bring them under proper ecclesiastical control. Any claim of the occurrence of a miracle had to be investigated by the bishop within whose diocese the alleged event had taken place, with a standard judicial procedure involving the appointment of a panel of three churchmen to take testimony and form a recommendation. The final authentication came from the pope, on the strength of the episcopal report. Post-Tridentine miracles were no longer matters of local tradition or word-of-mouth accreditation; they were legal constructs.[24]

See also Nicholas H. Steneck, "Greatrakes the Stroker: The Interpretations of Historians," *Isis,* 1982, *73*:161–177; and Barbara B. Kaplan, "Greatrakes the Stroker: The Interpretations of His Contemporaries," *ibid.,* pp. 178–185. The issue connects with Boyle's opposition to naturalistic explanations of miracles—see also Section III below for Robert Lenoble's remarks concerning Mersenne and this theme.

[21] Walker, "Cessation of Miracles"; and Duffy, "Greatrakes," pp. 253–254. Walker, *Unclean Spirits,* pp. 72–73, asserts that the cessation of miracles was nonetheless a particularly English doctrine established in the later sixteenth century. Sprat deployed it against enthusiasts in his *History,* pp. 360–362; see Michael Heyd, "The New Experimental Philosophy: A Manifestation of 'Enthusiasm' or an Antidote to It?" *Minerva,* 1987, *25*:423–440.

[22] See, e.g. (among many), Bernard Cousin, *Le miracle et le quotidien: Les ex-voto provençaux, images d'une société* (Sociétés, Mentalités, Cultures) (Aix-en-Provence: Univ. Provence, 1983), on ex-voto images from the seventeenth through the nineteenth centuries. For true (officially attested) miracles in the life of a Catholic community in the seventeenth century, see Henri Platelle, *Les chrétiens face au miracle: Lille au XVIIe siècle* (Paris: Editions du Cerf, 1968).

[23] Francis Bacon, *Novum Organum,* in *The Works of Francis Bacon,* ed. J. Spedding, R. L. Ellis, and D. D. Heath (London, 1857–1859), Aphorisms 1.46. I am grateful to Robert K. Merton for informing me that the story appears to be borrowed from Diogenes Laërtius.

[24] Platelle, *Les chrétiens* (cit. n. 22), pp. 32–35, gives details of the standard procedure in the

Nonetheless, miracles continued to happen in large numbers and were a common feature of life in seventeenth-century France. A miracle nearly always involved a miraculous healing,[25] but its meaning did not therefore primarily consist in the return of the subject to health (although prayers and other attempts at provoking miraculous healing, such as touching with a holy relic, were of course motivated by that goal). Rather, the meaning of a miracle resided in its role as an accreditation of a religious claim: a novel doctrinal assertion or some practical innovation, such as a new chapel or new forms of devotions.[26] Establishing that a miracle had actually occurred under the appropriate circumstances amounted to lending credence to a statement of religious doctrine or propriety. Usually somebody got cured into the bargain.

The meaning of a miracle was therefore primarily preserved and transmitted not in the body of the cured person (although scars, like ex-voto offerings, might bear witness to the miraculous event) but in historical accounts. They were the means by which the miracle did its job, and they were what ultimately issued from the ecclesiastical procedures laid down at Trent. Such accounts needed to do three things. They had to render it credible, first, that a cure had indeed taken place; second, that the cure had exceeded the capability of natural means; and third, that the circumstances, usually represented by place, were such as to show clearly what it was that the miracle certified. Once the event itself was over, an account took its place—or even, retrospectively, constituted the miracle itself.[27]

The credibility of the account hinged on the second of these three certifications, namely, that the occurrence had exceeded the capability of natural means. This was crucial, and it did not mean merely that the occurrence was wondrous or that it could not have been brought about by unaided human agency. A wonder could be an imposture caused by a demon rather than a true miracle resulting from divine action. Pascal attempted to clarify the point in a query addressed to the Abbé de Saint-Cyran: "If it is necessary, for an effect to be miraculous, that it be beyond the power of men, demons, angels, and all created nature." That indeed was so; Pascal's understanding of the matter, as Saint-Cyran assured him, corresponded to orthodox Catholic teaching.[28]

At the ontological level, therefore, the standard way of distinguishing between a miracle and a demonic imposture was to assert that demonic wonders relied on

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seventeenth century; see also *Canons and Decrees of the Council of Trent: Original Text with English Translation,* ed. and trans. H. J. Schroeder (St. Louis/London: B. Herder, 1941), p. 217 (English), p. 485 (Latin).

[25] The articles in Jacques Gelis and Odile Redon, eds., *Les miracles miroirs des corps* (Paris: Presses de l'Université de Paris VIII, 1983), examine cases from the thirteenth to the eighteenth centuries in which images of the body were implicated in behavior associated with miracles.

[26] See Platelle, *Les chrétiens* (cit. n. 22), pp. 37–43, with tables showing the chronological correlation of novelty and miracles for religious foundations at Lille in the seventeenth century.

[27] Cf. Gelis and Redon, *Les miracles* (cit. n. 25), preface, pp. 10–12; and Daniel Vidal, *Miracles et convulsions jansénistes au XVIIIe siècle: Le mal et sa connaissance* (Paris: Presses Universitaires de France, 1987), esp. pp. 33–45, "L'écriture du miracle." The issue of the credibility of the historical narrative rather than the judgment that the event described was indeed a miracle is another matter again. It is discussed, for example, by the authors of the Port-Royal *Logique: La logique ou l'art de penser* (Paris, 1662; facs. rpt., Stuttgart–Bad Cannstatt: Friedrich Frommann, 1965), Pt. IV, Ch. 13, where the miraculous properties of the purported events are taken for granted.

[28] Pascal, *Pensées* 3.32, *Oeuvres,* p. 606. See also Tetsuya Shiokawa, *Pascal et les miracles* (Paris: A.-G. Nizet, 1977). "Created nature" appears to refer generally to the physical world and not to be restricted to the "phenomena" or "works" of nature as distinct from nature as an active principle, as has been suggested by A. W. S. Baird, "Pascal's Idea of Nature," *Isis,* 1970, *61*:297–320, esp. pp. 315–318.

the manipulation of natural agencies; demons could not violate the ordinary processes of nature but only exploit them, as men do. For a miracle to serve its function as a witness to religious truth it had to be clearly a miracle: there would be no point in God's miraculously producing an effect that could also be produced naturally, because there would then be no way of telling that it had in that particular case been miraculous. True miracles involved the direct suspension or overriding of natural processes, and could only be done by God.[29] At the epistemological level, however, things were much less straightforward. Distinguishing between a miracle and a demonic act presupposed a perfect knowledge of what was and was not possible in nature. But no one could claim to possess such knowledge, and so no one could claim with certainty on the basis of its natural impossibility that a marvelous event was a miracle.[30]

Other criteria for identifying miracles were therefore needed. Pascal considered as a standard of judgment the character of the doctrine with which the candidate-miracle is associated. A false miracle ought to be associated with false doctrine, whereas a genuine miracle ought to be associated with true doctrine. That was the standard solution, but it posed a severe problem: On what basis was true doctrine to be recognized? The primary function of miracles was precisely this—to identify true doctrine. But if miracles themselves were not independently identifiable, a circle seemed inevitably to result. Agonizing over the question in his *Pensées,* Pascal expressed the central difficulty as follows: "Miracles identify [*discernent*] doctrine and doctrine identifies miracles." He went on to emphasize the necessity of a reliable criterion for identifying miracles: "There are false ones and true ones. A mark is required to know them, otherwise they would be useless," whereas, in fact, "they are not useless, and are, on the contrary, the foundation [*fondement*]." He then expanded on the problem of apparent circularity with which he had started: There would be no difficulty if it were not for the fact that "the chief end of miracles" is "the proof that true miracles give of the truth." The way out of the logical circle was therefore to apply a mark of true doctrine other than that of a miracle. Pascal considered two given in Deuteronomy but concluded that times had changed and that those rules might no longer be relevant.[31]

The problem of the criterion was one that Sprat had not seen fit to address in his remarks concerning the role of miracles in obliging belief in Christ's message. He and his like-minded contemporaries were by no means neglectful of it, however. It formed one of the strongest weapons in the armory of English opponents of miracles; defending religious orthodoxy against deistic challenge demanded

[29] Stuart Clark, "The Scientific Status of Demonology," in Vickers, *Occult and Scientific Mentalities* (cit. n. 6), pp. 351–374.

[30] Sprat, immediately following the passage on miracles as "divine experiments," continued: "What then can there be in all this *Doctrine* [i.e., "the *Evangelical Doctrine* of *Salvation* by *Jesus Christ,*" p. 351], at which a Real and impartial *Inquirer* into *Natural Things,* should be offended? Does he demand a Testimony from *Heven?* he has it: He reads effects produc'd, that did exceed all mortal skill and force: And of this he himself is a better judge than others: For to understand aright what is *supernatural,* it is a good step first to *know* what is according to *Nature":* Sprat, *History,* p. 352. Montaigne at one point maintained that "les miracles sont selon l'ignorance en quoy nous sommes de la nature, non selon l'estre de la nature," although he was certainly not consistent on the matter: *Les essais de Michel de Montaigne,* ed. Pierre Villey and V.-L. Saulnier, 2 vols. (Paris: Presses Universitaires de France, 1978), Bk. 1, Ch. 23, p. 112; cf. p. 121. Pascal adverts to Montaigne's apparent inconsistency in *Pensées* 3.34, *Oeuvres,* p. 613.

[31] Pascal, *Pensées* 3.33, *Oeuvres,* p. 607.

that the problem be confronted with more than usual urgency.[32] The arguments used by men such as Boyle, Edward Stillingfleet, and John Locke to support the credibility of biblical miracles were similar to Pascal's: Pascal, prompted by the urgings of conscience rather than of apologetical need, had gone beyond the reflections given above to indicate properties of true religion whose absence might serve at least to brand a supposed miracle as false. He identified what he regarded as criteria more useful than the Mosaic ones: "Every religion is false which in its faith does not adore a God as the principle of all things and which in its morals does not love one God as the object of all things." Elsewhere he gives an attendant practical rule: "As soon, therefore, as one sees a miracle one must either submit oneself [to it] or find strange marks contradicting it [*il faut ou se soumettre ou avoir d'étranges marques du contraire*]." Therefore, he goes on, "One must see if it denies God, or Jesus Christ or the Church."[33]

Some of the English apologists went farther than Pascal by trying to justify their choice of the hallmark doctrines of true religion rather than simply assembling a plausible list. The selection could not be justified on the basis of revelation, since the recognizability of revelation was the point at issue. As a consequence, Boyle and Locke restricted their rules of judgment to such as could be supported by "natural religion," that is, the body of religious truths that could be known through unaided reason. Since their only interest was in justifying the Christian revelation of the New Testament, assessments relating to modern miracles (which probably did not occur anyway) were irrelevant. So Boyle described the ideal procedure in this way:

> I first assent to a natural religion upon the score of natural reason. . . . And *then;* if a miracle be wrought . . . if I find by the agreeableness of it to the best notions that natural theology gives us . . . that this religion cannot in reason be doubted to come from [God]; I then judge the body of the religion to be true. And if anything yet is obscure or doubtful in that religion, a miracle should be wrought to decide the difficulty, I would submit to that decision, and not judge the miraculous matter of fact competently attested, to be false, 'cause it is repugnant to some preconceived opinions of mine.[34]

Revelation could not by its very nature be judged according to natural reason, but natural reason could judge the likelihood of a purported revelation. That

[32] R. M. Burns, *The Great Debate on Miracles from Joseph Glanvill to David Hume* (Lewisburg, Pa.: Bucknell Univ. Press, 1981), Chs. 3, 4. Among the deistic challengers was Charles Blount, in his *Miracles No Violation of the Laws of Nature* (1683): see Burns, *Great Debate,* Ch. 4; Thomas, *Religion* (cit. n. 20), p. 126; and Jacob, *Stubbe* (cit. n. 20), Ch. 8.

[33] Pascal, *Pensées* 3.33, *Oeuvres,* pp. 607–608, 611, 612.

[34] Quoted in Burns, *Great Debate* (cit. n. 32), pp. 55–56, from Boyle Manuscripts, Royal Society, Vol. 7, fol. 122. See also Beck, "Miracle" (cit. n. 20), pp. 96–99; for Edward Stillingfleet on this issue, taking off from the same Mosaic passage as had Pascal, see *ibid.,* pp. 91–92. For Locke, see his essay "A Discourse of Miracles" (1701–1702; posthumously published 1706), in *The Works of John Locke in Ten Volumes: The Eleventh Edition,* Vol. 9 (London, 1812), pp. 256–265. Locke does not demand that an event be an extraordinary operation performable by God alone in order to function as a miracle, that is, as a guarantor of revelation, since he recognizes that such a demand would put true miracles beyond the powers of discernment of "the simple and illiterate," whatever might be the case with philosophers (pp. 264–265). Nonetheless, he asserts firmly that "God having discovered to men the unity and majesty of his eternal godhead, and the truths of natural religion and morality by the light of reason, he cannot be supposed to back the contrary by revelation; for that would be to destroy the evidence and the use of reason, without which men cannot be able to distinguish divine revelation from diabolical imposture" (pp. 261–262).

likelihood then helped to certify as genuine a miracle performed in support of the truth of the revelation; and the certified miracle then guaranteed the revelation itself. That explains why Sprat said that no one would be obliged to believe Christ's teachings were it not that they had been assured by the supernatural means of miracles.

And yet despite the similarities that the writings on miracles of Pascal have with those of Boyle and other Englishmen, the actual place of miracles in the daily life of Protestant England contrasted starkly with their place in the daily life of Catholic France. Insofar as religious behavior stood at the center of European life in the seventeenth century, this difference denotes a fundamental cultural divergence—to exactly the degree that the division between Protestant and Catholic does—one expressed in rather than caused by the difference in theological doctrines regarding the cessation of miracles.[35]

The management of miracles in daily life embodied certain inferential practices, moving from the accredited event to the doctrinal claim, from the particular to the general, from the singular to the universal. The same inferential habits appear in the creation of experimental knowledge in the seventeenth century. Like experimental claims, miracles appealed to ideas and assumptions about the "ordinary course of nature."

III. PASCAL'S ACCOUNTS OF THE NATURAL AND THE MIRACULOUS

Robert Lenoble argued that Mersenne's opposition to Renaissance naturalism and espousal of mathematical "mechanism" was motivated by his need to maintain the integrity of miracles in the face of challenges to the stable, lawlike scholastic-Aristotelian account of nature.[36] Without a regular nature graspable by reason, there would have been no ontological space for miracles, that is, events characterized by their deviation from regularity. And miracles were essential for Catholic apologetics. Conversely, modern miracles were not essential to any but enthusiastic Protestant sects; they were, in fact, a threat to Anglicans like Boyle or Locke or John Wilkins. In that regard, therefore, the imperative for a stable

[35] As Robert K. Merton put it, in *Science, Technology and Society* (cit. n. 4), p. 55, "Religion is one expression of cultural values—and in the seventeenth century a clearly dominating expression." On the theoretical suppositions involved in Merton's use of "values," see Steven Shapin, "Understanding the Merton Thesis," *Isis*, 1988, *79*:594–605.

[36] Robert Lenoble, *Mersenne ou la naissance du mécanisme* (Paris: J. Vrin, 1943; rpt. 1971). This view of miracles was not unequivocally sanctioned in Christian tradition, especially among the church fathers—Augustine, for example, characterized miracles as part of the natural order: "a miracle is not contrary to nature but to what is known of nature." See Robert M. Grant, *Miracle and Natural Law in Graeco-Roman and Early Christian Thought* (Amsterdam: North Holland, 1952), p. 217, and Ch. 13 in general; see also Jean Céard, *La nature et les prodiges: L'insolite au XVIe siècle, en France* (Geneva: Librairie Droz, 1977), pp. 21–25: Augustine allows an "ordinary course of nature" from which there can be deviations, but the category seems less clear-cut than that operative in our period. The differing perspectives in the seventeenth century centered on the distinction between *potentia Dei ordinata* and *potentia Dei absoluta*: see esp. Francis Oakley, *Omnipotence, Covenant, and Order: An Excursion in the History of Ideas from Abelard to Leibniz* (Ithaca, N.Y.: Cornell Univ. Press, 1984), Ch. 3; Amos Funkenstein, *Theology and the Scientific Imagination from the Middle Ages to the Seventeenth Century* (Princeton, N.J.: Princeton Univ. Press, 1986), pp. 121–152; and Eugene M. Klaaren, *Religious Origins of Modern Science: Belief in Creation in Seventeenth-Century Thought* (Grand Rapids, Mich.: W. B. Eerdmans, 1977). For a general survey of the theory of miracles up to the thirteenth century see Benedicta Ward, *Miracles and the Medieval Mind: Theory, Record and Event* (rev. ed., Philadelphia: Univ. Pennsylvania Press, 1987), Ch. 1. For a discussion complementary to Lenoble's see also Shiokawa, *Pascal* (cit. n. 28), Ch. 1.

and regular nature was that much less.[37] At the same time, England was the home of experimental philosophy.

An experiment—or more properly, an experimental report— need not necessarily be able to stand for "what happens" in nature under specified circumstances. It might function only to stand for "what happened" on a particular occasion. In order for the singular to stand for the universal, an appropriate set of inferential assumptions (or habits), explicit or implicit, must be in place. Pascal's most fully accomplished treatises on hydrostatics and the weight of the air—the latter being effectively a subsuming of the behavior of air to the behavior of liquids, that is, to hydrostatics itself—fall squarely into the genre of the mixed mathematical sciences: Pascal's hydrostatics is, generically speaking, simply an extension of the classical mathematical science of mechanics. His *Traités de l'équilibre des liqueurs et de la pesanteur de la masse de l'air* (composed in the early 1650s but published posthumously in 1663) incorporate statements of contrived experiences couched in a universalized form appropriate to the establishment of general experiential premises within formal scientific argument. In common with the long-standing tradition of the mixed mathematical sciences (found most typically in optical treatises), this incorporation of contrived experience is accomplished by presenting details of procedure as sets of instructions or conditionals. This was routine practice in mathematical treatises, presenting confident statements of what happens when the detailed steps are accomplished.[38] Thus the *Traité de l'équilibre des liqueurs,* referring to naturalistic diagrams of apparatus, commences: "If a number of vessels are attached against a wall, one like that of the first figure; another leaning . . . ; another very large . . . ; another narrow . . . ; another that is only a small tube . . . ; and one fills them all with water to the same height, and makes all the openings equal at the bottom, which one plugs to retain the water: experience shows [*l'expérience fait voir*] that the same force is required to prevent all the plugs from coming out."[39]

For Boyle, this sort of thing was not good enough. As he noted in his *Hydrostatical Paradoxes* of 1666, the problem was that Pascal failed to vouch for actually having tried these things himself; there was no clear demarcation between real events and thought experiments: "Though the Experiments he mentions be delivered in such a manner, as is usual in mentioning matters of fact; yet I remember not that he expressly says that he actually try'd them, and therefore he might possibly have set them down as things that *must* happen, upon a just confidence that he was not mistaken in his Ratiocinations."[40] From Pascal's

[37] But cf. Shapin, "Robert Boyle and Mathematics" (cit. n. 18), pp. 39–41, arguing that Boyle wanted to avoid mathematically constituted "laws of nature" because they conveyed an improper impression of a priori necessity attaching to the regularities of nature, contrary to the doctrine of God's absolute power over nature, which could always subvert such laws. Although the issue of the relationship of God's causality to mathematical truths arose in the seventeenth century, especially in connection with Descartes's views (there is a large literature on this subject: see Dear, *Mersenne* [cit. n. 6], p. 55, n. 26), an absolute necessity does not seem to have been attached to the physical regularities involved in the mixed mathematical sciences. Had that been so, Catholics such as Pascal, who believed in modern miracles, might have been less fond of mathematical sciences even than Boyle. My stress, however, is on the practical import of denying modern miracles rather than on Boyle's simultaneous position of accepting in principle God's absolute dominion and power.

[38] See Dear, "Jesuit Mathematical Science" (cit. n. 10).

[39] Pascal, *Oeuvres,* p. 236.

[40] Robert Boyle, *Hydrostatical Paradoxes, Made out by New Experiments, (For the most part Physical and Easie)* (Oxford, 1666), pp. 4–5; see also pp. 63–64, 141, 171, 185, 243.

perspective, however, this was not only right and proper; it was, in fact, an advantage. Scientific experience needed to be universal, true generally and for everyone at all times. A historical report of a particular event, of the kind that Boyle wrote endlessly, would have been scientifically meaningless; it would be philosophical antiquarianism.[41]

The most striking feature of the famous exploit of Pascal's brother-in-law Florin Périer when he carried a mercury tube up the Puy-de-Dôme, therefore, ought to be that it is presented as a detailed historical record. Pascal's pamphlet of 1648 on the matter, the *Récit de la grande expérience de l'équilibre des liqueurs,* is designed to show that the column of mercury in a barometer tube is supported by the weight of the air. Pascal had not claimed this in his earlier pamphlet *Expériences nouvelles touchant le vide,* which was aimed only at establishing that the space at the top of the tube was actually, as far as could be judged, a void; he simply took it that nature's abhorrence of a vacuum had its limits. At the beginning of this 1648 pamphlet, Pascal explains how in the earlier work "I had employed the maxim of the 'fear of a void,' because it was universally accepted, and I had not yet any convincing proofs at all to the contrary." Contradicting common opinion was not something to be done lightly. As Pascal says in the prefatory letter to Périer asking him to make the trial, "I do not consider that we are permitted to depart lightly from maxims that we hold from antiquity, if we are not obliged to do so by indubitable and invincible proofs."[42]

Pascal notes that talk of this experience had become widespread before any account of its actually having been carried out had appeared. There were, that is, plenty of people waiting to receive a report of its having been done. The present report is, of course, Pascal's, not Périer's, insofar as it was Pascal who made it the centerpiece of his pamphlet. This centerpiece is a detailed circumstantial historical report, in Périer's first-person narrative, of events that took place on Saturday, 19 September 1648; it includes the names of various people who assisted or witnessed the proceedings.[43]

Pascal's usual appeal to universalized experience makes the Puy-de-Dôme account seem incongruous: a singular "experiment" in the English sense ought not to have suited him at all. Pascal had therefore to endow it with natural-philosophical (or, strictly speaking, mathematical-scientific) meaning. A singular miracle served to establish the violation of the ordinary course of nature and to legitimate a doctrinal claim or devotional innovation, but a singular experiment could not establish the observance of the ordinary course of nature or legitimate a knowl-

[41] Galileo claimed in 1624 to have tried (probably some twenty years earlier) the famous experiment of dropping a weight from the mast of a moving ship, and yet he clearly found it inappropriate to mention this in his discussion of the matter in the *Dialogue*: Stillman Drake, *Galileo at Work: His Scientific Biography* (Chicago: Univ. Chicago Press, 1978), pp. 84, 294. Similarly, Périer gives an account, in his introduction to the *Traités* (*Oeuvres,* p. 235), of Pascal's actual performance of the experiences that the latter presented only in universalized theorem form in the 1647 pamphlet. Périer apparently finds such an account appropriate as part of a longer "histoire" of experiences on the void—that is to say, something other than natural philosophical or mathematical discourse.

[42] Pascal, *Oeuvres,* pp. 221, 222. For a survey of discussions casting doubt on the authenticity of Pascal's letter (the central issue being whether Pascal falsified it so as to create the impression that he had suggested it earlier than he really had), see W. E. Knowles Middleton, *The History of the Barometer* (Baltimore: Johns Hopkins Univ. Press, 1964), pp. 45–48; and Bernard Rochot, "Comment Gassendi interprétait l'expérience du Puy de Dôme," in Pierre Costabel et al., *L'oeuvre scientifique de Pascal* (Paris: Presses Universitaires de France, 1964), pp. 278–301.

[43] For Gassendi's independent, second-hand account see Rochot, "Comment Gassendi interprétait l'expérience."

edge claim about it. This was an inverse relationship that rested on a logic of falsification to which Pascal himself sometimes made appeal—he lived in a universe that tolerated occasional anomalies.[44] In fact, however, Pascal took care to avoid characterizing his account of the events on the Puy-de-Dôme as a singular experiment.

Pascal's "decisive experience" to establish the "pressure and weight of the air" as the true explanation of the Torricellian experience did not comprise a single experimental event but rather consisted of making "the ordinary experience of the void several times in the same day," with the same apparatus, at the bottom and the top of a high mountain.[45] Périer's report describes making the "ordinary" experiment a number of times at different locations on the mountain and in the garden of the Minim convent at the mountain's foot—without detailing each trial separately. Périer also mentions a kind of calibration, whereby a tube like the one he carried up the mountain was left in the care of a monk in the convent garden, being set up in a vessel of mercury in the usual way. The tube Périer took with him was initially set up next to this other one, to check that the two columns of mercury were of exactly the same height, and they were compared again when he returned. In the meantime the monk kept watch over the tube in the garden; he reported that the height of the mercury had remained constant while Périer was gone.

The term that Périer and Pascal use for the calibration apparatus is itself indicative of the apparatus's epistemological status: the tube in the vessel of mercury had been left "en expérience continuelle." This terminology was also used by Pascal, Descartes, and others to describe mercury tubes left to exhibit changes in height over time as a presumed function of the weather.[46] The category of "expérience continuelle" seems to have been a means of establishing the perpetual behavior of nature in deliberately contrived situations. Thus the important thing here is contrivance rather than singularity—indeed, singularity is as far as possible denied.[47]

[44] Most notably in his public demonstration at Rouen in 1646 involving two long tubes, one filled with water and one with wine: having secured from the spectators the prediction that, when the tubes were raised, the wine would ride higher in its tube than the water (on the assumption that the space above the liquid was due to expansive vapor, more readily produced from volatile wine), Pascal then demonstrated the opposite. The falsification of the vapor explanation had greater logical (and, under the circumstances, dialectical) force than any direct positive inference to the presence of a vacuum. See *Oeuvres de Blaise Pascal,* ed. Léon Brunschvicg and Pierre Boutroux (Paris: Hachette, 1908), Vol. II, pp. 7–8; and Richard H. Popkin, "Scepticism, Theology and the Scientific Revolution in the Seventeenth Century," in *Problems in the Philosophy of Science,* ed. Imre Lakatos and Alan Musgrave (Amsterdam: North Holland, 1968), pp. 1–28, on p. 14. On scholastic responses to experiences of the void see Jean-Pierre Fanton d'Andon, *L'horreur du vide* (Paris: Editions du CNRS, 1978), Ch. 2. For the role of occasional anomalies in a universe formulated on broadly Aristotelian lines, see William A. Wallace, "Albertus Magnus on Suppositional Necessity in the Natural Sciences," in *Albertus Magnus and the Sciences: Commemorative Essays 1980,* ed. James A. Weisheipl (Toronto: Pontifical Institute of Medieval Studies, 1980), pp. 103–128; and Wallace, "Aristotle and Galileo: The Uses of *Hypothesis (Suppositio)* in Scientific Reasoning," in *Studies in Aristotle,* ed. Dominic J. O'Meara (Washington, D.C.: Catholic Univ. America Press, 1981), pp. 47–77.

[45] Pascal, *Oeuvres,* p. 222.

[46] Pascal discusses barometric readings at Dieppe, and uses this terminology, in material appended to the two treatises on hydrostatics and the weight of the air. Even there, Pascal shies away from historical reportage: he uses the future tense, saying, e.g., "On verra que dans Dieppe, quand le temps est le plus chargé, le mercure sera à la hauteur de 28 pouces 4 lignes" (*Oeuvres,* p. 259). See also Pierre Humbert, *Cet effrayant génie . . . L'oeuvre scientifique de Blaise Pascal* (Paris: Editions Albin Michel, 1947), p. 129.

[47] The term "expérience continuelle" later came to be used to refer to experimental controls.

Following Périer's letter, Pascal tells his readers that, taking Périer's reported measurements as a standard, a difference in altitude of six or seven fathoms makes a difference in height of mercury of about half a line, and he notes the ease of testing this using tall buildings in Paris. He says that he has therefore tried it and obtained results corresponding to Périer's. He gives very little detail, with no dates and only one specific location. He then concludes, "All the curious can test it themselves whenever they like."[48] A remarkable and eagerly awaited historical exploit has thus turned, in the space of a paragraph, into an easily acquired, routine philosophical experience. The ascent of the Puy-de-Dôme had served its purpose.[49]

A comparison between Pascal's use of Périer's ascent of the Puy-de-Dôme and his use of an apparently similar event in the *Traité de la pesanteur de la masse de l'air* illuminates the true character of the former. In 1649 or 1650, Pascal himself ascended the Puy-de-Dôme carrying a partially inflated bladder, which he found to expand and distend as he went up the mountain and become flaccid again as he descended.[50] If the original Puy-de-Dôme experiment had acted as the model, a historical account of this later ascent of the Puy-de-Dôme would surely have appeared. But instead, the event itself seems to have only occasional relation to the following passage: "If one takes a balloon half full of air, flaccid and limp, and one carries it at the end of a string on a mountain five hundred fathoms high, it will come about that, in proportion to one's ascent, it will swell of its own accord."[51] This is so universalized an experience that even the Puy-de-Dôme has vanished; Pascal talks generally, and airily, about "mountains five hundred fathoms high."

Having concluded the description, Pascal proceeds to display its significance: "This experience proves everything that I have said about the mass of the air with a totally convincing force: so it was necessary to establish it well, because it is the foundation of this entire discourse."[52] The establishment of the experience is not the presentation of a credible report of a specific historical event, Pascal strolling up the Puy-de-Dôme with a bladder; rather, it is an apparently self-justifying presentation of a universal experience, as if everyone were familiar with this kind of behavior of bladders on high mountains. The step from here to Boyle's claim that Pascal talked of impossible experiments as if he had actually performed them is a short one.

From a formal point of view, the closest parallel to the report of Périer's Puy-de-Dôme ascent in all of Pascal's writings is a document brought to light only in 1952: Pascal's deposition concerning the miracle of the holy thorn, in 1656. Once again, Florin Périer is involved. His daughter Marguerite, Pascal's niece, was suddenly cured of a chronic and potentially fatal pustule eating into her nose next to her left eye; the cure occurred after she was touched by a reliquary containing a fragment from the crown of thorns. Pascal wrote as a witness, testifying to the girl's medical history for the preceding three or four years, the pronouncements of various doctors, and her sudden recovery. This

[48] Pascal, *Oeuvres,* p. 225.

[49] Cf. Périer's description of Pascal's Parisian versions of the Puy-de-Dôme exploit in his introduction to the two treatises published in 1663: *Oeuvres,* p. 236.

[50] *Oeuvres de Blaise Pascal,* ed. Brunschvicg and Boutroux (cit. n. 44), Vol. III, p. 200n.

[51] Pascal, *Oeuvres,* p. 245.

[52] *Ibid.*

miracle became a powerful propaganda weapon in the Jansenist cause because it took place at the convent of Port-Royal in Paris, the Jansenist headquarters; Pascal himself was at the center of the Jansenist controversies in this period.[53] His deposition takes the form of a sworn account—with names, dates, and places—of what he knew at first hand of the affair. Like the Puy-de-Dôme report, therefore, the deposition is an eyewitness's attestation of events together with the naming of other witnesses. The significance of this comparison arises from the cognitive assumptions legitimating such reports.

If Périer had murdered someone instead of carrying a glass tube and some mercury up a mountain, the significance of a report of the event would have been quite different. But the Puy-de-Dôme "experiment" displays a means of taking a historical report about nature and creating from it evidential generality. The miracle of the holy thorn shows how the cognitive assumptions involved in that transformation already existed, in a culturally entrenched way, in Pascal's society. Miracles were accepted as discrete events that had a particular, determinate, and self-evident relationship to the ordinary course of nature. That relationship, furthermore, was such as to allow the use of those events in the legitimation of knowledge claims. To that extent, we appear to have a parallel with experimental philosophy, where singular events were also seen as being related to the ordinary course of nature and were similarly used to legitimate knowledge claims. But miracles were not just singular events; they were events directly contrary to nature. The knowledge claims that they legitimated were religious or theological; they could not, of necessity, be natural philosophical.

A report of the occurrence of a miracle therefore had to do two things: to establish that the event had occurred and to establish the ordinary course of nature that the event violated. Most of Pascal's deposition is devoted to describing Marguerite's condition at various times over a period of years, how it gradually deteriorated, and what various named physicians said at various times about her condition; only at the end is there a very brief hearsay description of the touching with the reliquary and a quick account of her perfect health following the alleged event and the general amazement that greeted it. Most of the report establishes the normal course, as it were, of her worsening condition, so that her sudden return to health at the end may immediately be established as miraculous. The force of the event is in violating the ordinary course of nature, whereas the force of the events in the Puy-de-Dôme narrative is in determining it. Pascal explained in advance what he expected the Puy-de-Dôme trials to show and why they would do so; the miracle of the holy thorn, by contrast, was a miracle precisely because it was not expected. But the intimate connection between the ordinary course of nature and the meaning of the events was the same in each case. Only because the meaning of a miracle derived from a presumption of the ordinary course of nature could the Puy-de-Dôme report itself be so easily transformed into an instantiation of the ordinary course of nature.

English experimental philosophers faced a problem structurally similar to the difficulties involved in authenticating a miracle. Not all apparent miracles could be authenticated as true miracles: a true miracle had to be distinguished from an imposture if it was to serve its purpose. In the same way, experimental trials had

[53] Henri Gouhier, *Blaise Pascal: Commentaires* (Paris: J. Vrin, 1971), Ch. 3, gives a full account of the miracle and its aftermath; see also Shiokawa, *Pascal* (cit. n. 28), Ch. 3.

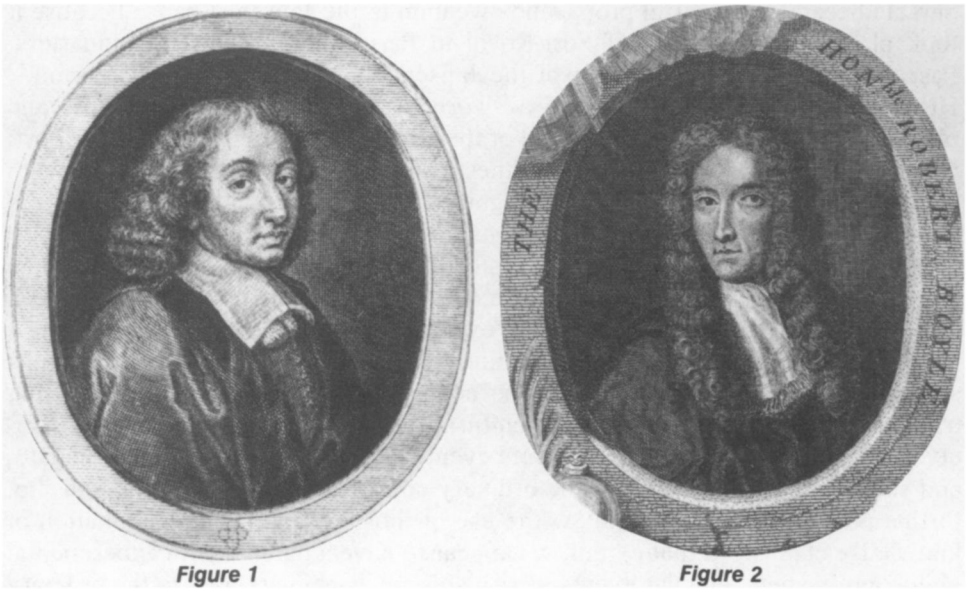

Figure 1 Figure 2

The work of Blaise Pascal (left) and Robert Boyle (right) exemplifies the difference between French mathematical universalism and English experimentalism. (See page 636 for picture credits.)

to be authenticated as having been competently performed before they could be presented as experiments that might serve as the basis for knowledge claims. Newton could rebuff the critics of his optics by accusing them of having failed to perform their prism experiments correctly; Robert Boyle dismissed Huygens's claims regarding anomalous suspension in barometers by impugning the "goodness" of his air pump and therefore the competence of his experiments.[54] If we relate these parallel difficulties to the ontological distinction between miracles and experiments, it becomes evident that while competent experiments required authentication in a similar way to genuine miracles, they were at the same time like false miracles in that they instantiated the ordinary course of nature rather than violated it.

IV. NATURAL PHILOSOPHY AND MATHEMATICS

An examination of Pascal's cultural setting serves to point up the degree to which Périer's Puy-de-Dôme expedition, one of the most celebrated of all seventeenth-century "experiments," was not really an experiment in the English sense at all.[55] It nonetheless exhibits contrivance, as do Pascal's descriptions of "expéri-

[54] Simon Schaffer, "Glass Works: Newton's Prisms and the Uses of Experiment," in *The Uses of Experiment: Studies in the Natural Sciences*, ed. David Gooding, Trevor Pinch, and Simon Schaffer (Cambridge: Cambridge Univ. Press, 1989), pp. 67–104; and Shapin and Schaffer, *Leviathan and the Air-Pump* (cit. n. 1), Ch. 6, esp. pp. 241–255.

[55] In this setting, the Jansenist "heresy" remains perfectly Catholic; it involves no repudiation of miracles and their legitimating function. Jean Orcibal, "La signification du miracle et sa place dans l'ecclésiologie pascalienne," *Chroniques de Port-Royal*, 1972, *20–21*:83–95, characterizes the miracle of the holy thorn as a typical move in religious controversy in this period in France, just as Pascal's

ences" generally. That fact indicates how Pascal's work, despite its incorporation of an Aristotelian ideal of scientific demonstration, was not "natural philosophy"; it was "mathematics."

In an Aristotelian universe, the distinction between art and nature is fundamental: art—that is, human contrivance—is either a means of imitating nature or a way of circumventing nature. Natural processes are goal directed, and the natures of things are discovered by seeing how those things behave and what their ends are. If the natural philosopher were to interfere with a natural process —that is, set up contrived situations—he would be thwarting nature, and its processes would therefore remain unknown; instead of learning about nature, he would merely have learned a way of achieving his own ends.[56] This restriction did not, however, apply to the mathematician. The conventional (and curricular) division between natural philosophy on the one hand and the mathematical sciences (more specifically, the mixed mathematical sciences) on the other meant that there were two different models for talking about the world in a scientific manner: qualitative physics, concerned with the natures and causes of things, and classical mathematical sciences such as optics, mechanics, and astronomy, which supposedly dealt only with the quantitative characteristics of things. Since the latter could therefore avoid issues of process and teleology of a properly natural philosophical kind, they could also avoid the injunction against contrived situations.[57]

Seventeenth-century philosophical texts and treatises on nonmathematical subjects increasingly exhibited a conscious adoption of a mathematical, or geometrical, form of presentation, apparently as a means of co-opting the appearance of certainty associated at that time with mathematical demonstration.[58] Mariotte's version of Boyle's law exemplified the presentation of problems modeled on those of geometry, while Pascal's treatises on hydrostatics and the weight of the air are, as noted earlier, classic examples of the genre. Such a tendency was not, however, consonant with "experimental philosophy." When Boyle criticized the practices of mathematical writers on hydrostatics in his *Hydrostatical Paradoxes,* he based his objections on the central feature of the distinction between physics and mathematics: "Those Mathematicians, that, (like *Marinus Ghetaldus, Stevinus,* and *Galileo*) have added anything considerable to

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discussion of miracles reflected a Thomistic orthodoxy on the subject. In the eighteenth century, by contrast, Jansenist miracles began to assume a disreputable marginality: see Vidal, *Miracles,* pp. 24–25. The cultural differences at issue here concern in part the acceptable limits of innovation. Innovativeness, departure from the previously accepted, itself became a positive feature of the seventeenth-century European approach to natural knowledge, whether experimental or mathematical-scientific. Pascal wanted to establish property rights over his barometric experiences because they were novel; Robert Hooke's microscopical observations were published not because they were canonical, but because they were new. So the difference between English experimentalism and Continental mathematicism reduces in large part to distinguishing the different patterns of behavior available in the two societies for handling innovation.

[56] Sister Mary Richard Reif, "Natural Philosophy in Some Early Seventeenth Century Scholastic Textbooks" (Ph.D. diss., St. Louis Univ., 1962), pp. 228–241, provides a particularly useful discussion of this issue in connection with mechanical contrivances.

[57] See Dear, "Jesuit Mathematical Science" (cit. n. 10), Sect. 4.

[58] Hermann Schüling, *Die Geschichte der axiomatischen Methode im 16. und beginnenden 17. Jahrhundert* (Hildesheim: Georg Olms, 1969); Wilhelm Risse, *Die Logik der Neuzeit* (Stuttgart-Bad Cannstatt: Friedrich Frommann, 1970), Vol. 2, Ch. 8; Patricia Reif, "The Textbook Tradition in Natural Philosophy, 1600–1650," *Journal of the History of Ideas,* 1969, *30*:17–32.

the Hydrostaticks . . . have been wont to handle them, rather as Geometricians, then as Philosophers, and without referring them to the explication of the Phaenomena of Nature."[59] For Boyle, the disadvantages of the mathematical model far outweighed its advantages in putative absolute certainty over experimental natural philosophy. The latter fulfilled the role of "physics" to the extent that it sought "causes" or "reasons"; at the same time, it transcended the Aristotelian injunction against artificiality by eschewing the "vulgarly received" notion of "natures."[60]

V. EXPERIMENTAL CULTURE

We are now in a position to frame a cultural explanation for the apparent correlations noted by Kuhn between Protestant societies and "experimental" sciences on the one hand and Catholic societies and "mathematical" sciences on the other. The patterns of behavior in which experimental events served as legitimators of natural philosophical innovations ran counter to those in which miracles served as legitimators of theological or religious innovations. The relationship between event and knowledge claim was different in the two cases because the inferential practices operated in opposite directions—determining whether the event was accounted a part of the natural order or uniquely contrary to it. Those opposed sets of inferential procedures therefore meant that the use of natural philosophical experience in a society that recognized and sanctioned miracles differed from its use in a society that did not.

In France, miracles had a well-established place in daily life as indicators of divine approval or as events to be prayed for. They were events that violated the ordinary course of nature, the conviction of which (as Lenoble stressed in the case of Mersenne) therefore powerfully confirmed them. As a corollary, singular events that did not violate the ordinary course of nature were not in themselves appropriate elements in establishing it, because the universal had effectively to be prior to the singular. When Pascal, in his work on voids and the weight of the air, incorporated contrived experiences into authentic knowledge of nature, he exploited the model of the mathematical sciences to make them universal by the very form of their presentation. It served to preserve them, by a kind of disciplinary insulation, from objections that they were not normal. Pascal operated in a different kind of social space from that of the experimental philosophy associated with the Royal Society.[61]

[59] Boyle, *Hydrostatical Paradoxes* (cit. n. 40), preface. On other aspects of Boyle's antipathy see Shapin, "Boyle and Mathematics" (cit. n. 18).

[60] One of the themes of Boyle's *Free Inquiry into the Vulgarly Received Notion of Nature* (1686; written 1666).

[61] The operative distinction is between experimental events as they typically appear in the work of the early Royal Society and descriptions of contrived experiences that are not couched in terms of discrete historical events. A different use of terminology might result in the *Saggi* of the (Catholic) Accademia del Cimento being labeled "experimental," but the contrived experiences related there are chiefly universalized accounts of procedure. The long-running debate over the status of Galileo's "experiments" illustrates the niceties of this point well: what are we to make of Galileo's clear attempts in his published writings to turn the activities evidenced by manuscript notes into transparent "thought experiments"? It would be appropriate to say that, as evidenced by what he does with the contrived experiences found in his manuscripts when he comes to publish what are meant to be scientific knowledge claims, Galileo did not do experiments. In the case of Mersenne, philosophical use of discrete experimental events seems clearer, but closer examination again indicates the differ-

English experimental philosophy, with its focus on discrete historical events, made the "ordinary course of nature" something to be gradually constituted through inferential steps rooted in "matters of fact," and in effect kept it in abeyance both epistemologically and ontologically (owing to God's omnipotence). Hence the Royal Society engaged in collective suspicion of hypotheses, Boyle distrusted mathematical laws, and Glanvill doubted the immediate possibility of going beyond the mere compiling of natural histories.[62] Correspondingly, truly miraculous events, which demanded the actual rather than simply potential presence of an "ordinary course of nature," were consigned to the past, playing no practical role in everyday life.[63]

The cultural differences between English experimentalism and what might be called French "mathematicism" thus appear sharply when viewed in this context. Not all the English were experimentalists in the sense employed here, and not all the French appealed to mathematical universalism.[64] The dominant forms of "science" established in the two countries in the second half of the seventeenth century, however, have distinct characters. "Experimental philosophy" was indeed English, not French; we have seen some aspects of that differential institutionalization.

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ence from later English experimental philosophy. Mersenne's famous examinations of Galilean claims from the *Dialogue* regarding falling bodies, published in his *Harmonie universelle* of 1636–1637 ("Du mouvement des corps"), are just that: critical examinations of another's claims. They are not something on which Mersenne himself builds conclusions. The use of discrete events as elements of philosophical discourse in Mersenne's work seems close to their use by Jesuits in the early part of the century, as rather unusual reactions to controversy, not philosophical building-blocks. See Dear, "Jesuit Mathematical Science" (cit. n. 10). We have already noted the differences between Mariotte and Boyle in the use of historical reports.

[62] See, e.g., Dear, "*Totius in verba*" (cit. n. 2); and Shapin, "Robert Boyle and Mathematics" (cit. n. 18). For another perspective, focusing on legal rather than religious correlates of natural philosophical activity, see Rose-Mary Sargent, "Scientific Experiment and Legal Expertise: The Way of Experience in Seventeenth-Century England," *Studies in History and Philosophy of Science,* 1989, *20*:19–45, which, however, deals with programmatic statements rather than with practice.

[63] For a discussion of the "experimental philosophy" as it touches on matters with overtly theological significance see Simon Schaffer, "Godly Men and Mechanical Philosophers: Souls and Spirits in Restoration Natural Philosophy," *Science in Context,* 1987, *1*:55–85.

[64] Shapin and Schaffer, *Leviathan and the Air-Pump* (cit. n. 1), has surely established Hobbes as the classic English antiexperimentalist, while one can look through French philosophical texts and find some reports of experimental events. The function of these latter accounts must be carefully considered, however, as the discussions above of the Puy-de-Dôme "experiment" and Mariotte's "Boyle's law" presentation have shown.