IDEAS IN CONTEXT

Edited by Wolf Lepenies, Richard Rorty, J. B. Schneewind and Quentin Skinner

The books in this series will discuss the emergence of intellectual traditions and of related new disciplines. The procedures, aims and vocabularies that were generated will be set in the context of the alternatives available within the contemporary frameworks of ideas and institutions. Through detailed studies of the evolution of such traditions, and their modification by different audiences, it is hoped that a new picture will form of the development of ideas in their concrete contexts. By this means, artificial distinctions between the history of philosophy, of the various sciences, of society and politics, and of literature may be seen to dissolve.

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The Empire of Chance

How probability changed science and everyday life

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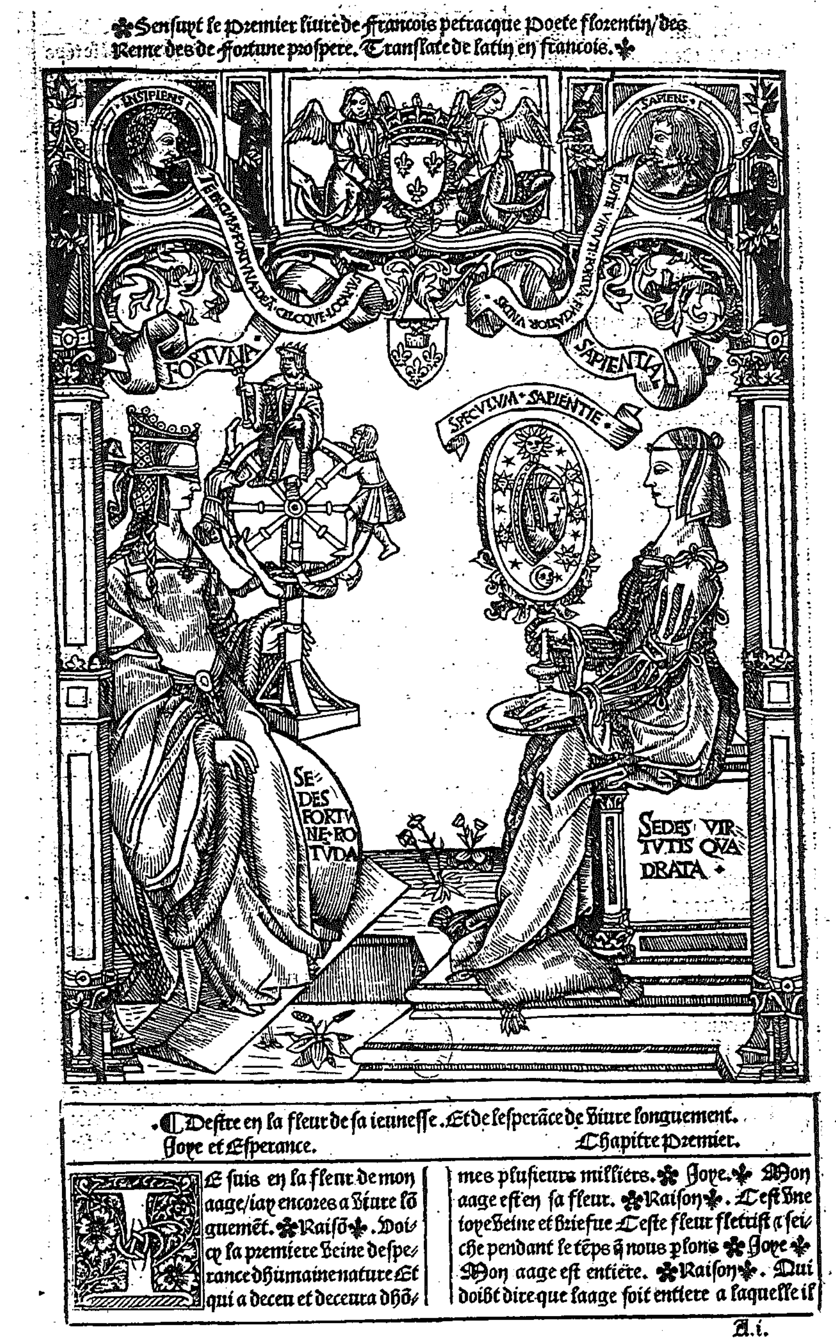

Fortuna (left) and Sapientia (right) are depicted here in traditional opposition. The slow breakdown of this opposition is the topic of this book. Source: Petrarch, *Remède de l'un et l'autre fortune prospère et adverse* (Paris, 1524); courtesy of the Bibliothèque Nationale, Paris.

God . . . has afforded us only the twilight of probability; suitable, I presume, to that state of mediocrity and probationership he has been pleased to place us in here. . .

John Locke (1690)

1

Classical probabilities, 1660–1840

1.1 INTRODUCTION

In July of 1654 Blaise Pascal wrote to Pierre Fermat about a gambling problem which came to be known as the Problem of Points: Two players are interrupted in the midst of a game of chance, with the score uneven at that point. How should the stake be divided? The ensuing correspondence between the two French mathematicians counts as the founding document in mathematical probability, even though it was not the first attempt to treat games of chance mathematically (Pascal, [1654] 1970, vol. 1, pp. 33–7; Cardano, [comp. *c.* 1525] 1966). Some years later, Pascal included among his *Pensées* an imaginary wager designed to convert sporting libertines: no matter how small we make the odds of God's existence, the pay-off is infinite; infinite bliss for the saved and infinite misery for the damned. Under such conditions, Pascal argued that rational self-interest dictates that we sacrifice our certain but merely finite worldly pleasures to the uncertain but infinite prospect of salvation (Pascal, [1669] 1962, pp. 187–90).

These two famous Pascal manuscripts, the one mathematical and the other philosophical, reveal the double root of the mathematical theory of probability. It emerged at the crux of two important intellectual movements of the seventeenth century: a new pragmatic rationality that abandoned traditional ideals of certainty; and a sustained and remarkably fruitful attempt to apply mathematics to new domains of experience. Neither would have been sufficient without the other. Philosophical notions about what happens only most of the time, and about the varying degrees of certainty connected with this unreliable experience date from antiquity, as do games of chance. But before *circa* 1650, no one attempted to quantify any of these senses of probability. Nor would the spirit of mathematical enterprise have alone sufficed, for quantification requires a subject matter, an interpretation to flesh out the mathematical formalism. This was particularly true for the calculus of probabilities, which until this century had no mathematical existence independent of its applications.

1.2 THE BEGINNINGS

The prehistory of mathematical probability has attracted considerable scholarly attention, perhaps because it seems so long overdue. Chance is our constant companion, and the mathematics of the earliest formulations of probability theory was elementary. Suggestive fragments of probabilistic thinking do turn up almost everywhere in the classical and medieval learned corpus: Around 85 B.C., Cicero connected that which usually happens with what is ordinarily believed in his rhetorical writings and called both *probabile* (Cicero, 1960, pp. 85–90). In a tenth-century manuscript, a monk enumerated all 36 possibilities for the toss of two dice (Kendall, 1956), and Talmudists reasoned probabilistically about inheritances and paternity (Rabinovitch, 1973). Yet none of these flowered into a mathematics of probability.

Several plausible hypotheses about why mathematical probability came about when it did also dissolve upon inspection. Maritime insurance expanded rapidly in Italy and the Low Countries during the commercial boom of the fifteenth and sixteenth centuries, but insurers did not collect statistics on shipwrecks, much less develop a mathematical basis for pricing premiums. It was the mathematicians who later – much later – influenced the insurers, not vice versa (Maistrov, [1964] 1974; Daston, 1987). Nor did any new recognition of chance inspire the mathematicians; on the contrary, the early probabilists from Pascal through Pierre Simon Laplace were determinists of the strictest persuasion (Kendall, 1956; Hacking, 1975). One might speculate that the mathematics of combinatorics was a precondition for the earliest versions of probability theory, but the two subjects appear to have developed in tandem, with probability theory often stimulating work in combinatorics rather than the reverse (Todhunter, 1865). The Renaissance doctrine of signatures linked the evidence of things with that of words in a way that parallels the objective and subjective senses of mathematical probabilities, but Cicero and the medieval rhetorical tradition that followed him had done so long before

(Hacking, 1975; Garber and Zabell, 1979). Similarly, the passion for gambling was hardly an invention of the seventeenth century, and so could not have been the catalyst that transformed qualitative probabilities into quantitative ones. It is, in short, easier to say where mathematical probability did *not* come from.

The very earliest writings on mathematical probability do supply some clues, however. If we return to the two Pascal musings, we discover that although they are recognizably part of what came to be called the calculus of probabilities, they are not cast in terms of probabilities. The fundamental concept was instead expectation, later defined as the product of the probability of an event e and its outcome value V:

$$P(e)V = E$$

So, for example, the expectation of someone holding one out of a thousand tickets for a fair lottery with a prize of \$10,000 would be \$10. As the definition implies, we now derive expectation from the probability, but for the early probabilists expectation was the prior and irreducible notion.

Expectation in turn was understood in terms of a fair exchange or contract. Pascal described his solution to the Problem of Points as rendering to each player what "in justice" belonged to him. In the first published treatise on mathematical probability, *De ratiociniis in ludo aleae* (1657), the Dutch mathematician and physicist Christiaan Huygens made expectation his departure point and defined it in terms of equity: equal expectations obtained in a fair game; that is, one that "worked to no one's disadvantage" (Huygens, [1657] 1920, p. 60). Since later probabilists would *define* a fair game as one in which the players possessed equal expectations, this definition of equal expectations in terms of a fair game strikes the modern reader as circular. But for the first generation of probabilists, notions of equity were intuitively clear enough to serve as the stuff of definitions and postulates.

These intuitions drew upon a category of legal agreement that had become increasingly important in sixteenth- and seventeenth-century commercial law, the aleatory contract. Jurists defined such agreements as the exchange of a present and certain value for a future, uncertain one – staking a gamble, purchasing an annuity, taking out an insurance policy, bidding on next year's wheat crop, or buying the next cast of a fisherman's net. Pascal's wager hinged upon a similar trade of the certain enjoyment of present vices for the uncertain joy of salvation. Aleatory contracts acquired prominence and a certain notoriety as the preferred way of exonerating merchants who made loans with interest from charges of usury

(Coumet, 1970). The element of risk, argued the canon lawyers, was the moral equivalent of labor, and therefore earned the merchant his interest as honestly as the sweat of his brow would have. Thus Jesuits successfully petitioned the Sacred Congregation for Propaganda in 1645 for a special dispensation for their Chinese converts, who were charging 30% interest on loans, on the condition "there is considered the equality and probability of the danger, and provided that there is kept a proportion between danger and what is received" (Noonan, 1957, p. 289).

It was this "proportion between danger and what is received," the element of equity fundamental to all contracts, that the mathematicians attempted to quantify in almost all of the early applications of probability theory. Both the problems they addressed – gambling stakes, annuity prices, future inheritances – and the terms in which they did so – using the concept of equal expectations – bear witness to the seminal influence of the law of aleatory contracts.

Pascal's wager is an example of how reasoning by expectations had become almost synonymous with a new brand of rationality by the mid-seventeenth century. His libertine interlocutor must be led back into the Christian fold by uncertain wagers rather than theological certainties. In the sixteenth century, Reformation controversies between Protestants and Catholics on the one hand, and the revival of the sceptical philosophy of Sextus Empiricus and his school on the other, combined to undermine the ideal of certain knowledge that had guided intellectual inquiry since Aristotle. In its place gradually emerged a more modest doctrine that accepted the inevitability of less than certain knowledge, but maintained nonetheless that it was still sufficient to guide the reasonable man in precept and in practice. Aristotle's dictum from the *Nicomachean Ethics* (1094b 24–25) was much quoted: "it is the mark of an educated man to look for precision in each class of things just so far as the nature of the subject admits: it is evidently equally foolish to accept probable reasoning from a mathematician and to demand from a rhetorician demonstrative proofs."

The ultimate result of the Reformation and Counter-Reformation clashes over the fundamental principles of faith and their justification, and of the radical scepticism of Michel de Montaigne and other sixteenth-century thinkers was vastly to erode the domain of the demonstrative proof and to expand that of probable reasoning. Their immediate impact was more devastating, challenging all claims to any kind of knowledge whatsoever. Religious apologists who sought to undercut the other side's claims to legitimacy on the basis of either (ambiguous) revelation

or (dubious) authority soon discovered that their destructive arguments were a double-edged sword. The revived pyrrhonism of the "libertins érudits" denied the reliability of even sense impressions and mathematical demonstrations; Descartes' *Meditations* began with a sceptical reverie of this extreme variety. Thus all of the traditional sources of certainty, religious and philosophical, came simultaneously under attack. Confronted with a choice between fideist dogmatism on the one hand and the most corrosive scepticism on the other, an increasing number of seventeenth-century writers attempted to carve out an intermediate position that abandoned all hope of certainty except in mathematics and perhaps metaphysics, and yet still insisted that men could attain probable knowledge. Or rather, they insisted that probable knowledge was indeed knowledge (Popkin, 1964; Shapiro, 1983).

In order to make their case for the respectability of the merely probable, these "mitigated sceptics" turned from rarified philosophical discourse to the conduct of daily life. The new criterion for rational belief was no longer a watertight demonstration, but rather that degree of conviction sufficient to impel a prudent man of affairs to action. For reasonable men that conviction in turn rested upon a combined reckoning of hazard and prospect of gain, i.e. upon expectation. Pascal's wager is about neither the bare probability of God's existence, nor the infinite bliss or misery that awaits saint or sinner, respectively. Rather, it is about the product of the two, significantly conceived in terms of a gamble, and the relationship between certain stake and uncertain pay-off, and thus a sterling example of the new rationality. Pascal's Port Royal colleagues Antoine Arnauld and Pierre Nicole made such mixed reasoning the *sine qua non* of rational judgment in their influential *Logique*, cautioning their readers that it is not enough to consider how good or bad an outcome is in itself, but also the likelihood that it will come to pass (Arnauld and Nicole, [1662] 1965, pp. 352–3). English and Dutch spokesmen for the new rationality of expectation preferred commercial analogies, but the idea was the same. John Wilkins, Anglican bishop and founding member of the Royal Society of London, argued in his *Of the Principles and Duties of Natural Religion* (1675) that just as merchants were willing to risk the perils of a long voyage in the name of profit, so "he that would act rationally, according to such Rules and Principles as all mankind do observe in the government of their Actions, must be persuaded to do the like" in matters of science and religion (Wilkins, [1675] 1699, p. 16). The emphasis upon action as the basis of belief, rather than the reverse, was key to the defense against scepticism, for as these writers were wont acidly to observe, even the most confirmed sceptic took his meals just as if the external world existed.

Expectation was thus central to the new rationality or "reasonableness," as it was sometimes called. The mitigated sceptics were less interested in equity than in rational belief, but they drew heavily upon the doctrine of aleatory contracts for examples to show that it was accepted practice and therefore reasonable to exchange a present, certain good – be it money to invest, a long-accepted scientific theory, or the indulgence of our lusts and passions – for a future, uncertain one – more money, a better theory, salvation. Mathematicians seeking to quantify the legal sense of expectations inevitably became involved in quantifying the new rationality as well. So began an alliance between mathematical probability theory and standards of rationality that stamped the classical interpretation as a "reasonable calculus"; as a mathematical codification of the intuitive principles underlying the belief and practice of reasonable men. The identification of classical probability theory with reasonableness was so strong that when the results of the one clashed with the other, it was the mathematicians who anxiously amended definitions and postulates to restore harmony, as we shall see below.

1.3 THE CLASSICAL INTERPRETATION

Thus the calculus of chance was in the first instance a calculus of expectations, and thereby an attempt to quantify the new, more modest doctrine of rationality that surfaces almost everywhere in seventeenth-century learned discourse. The first published works on the subject, from Huygens' little treatise of 1657 to Jakob Bernoulli's definitive *Ars conjectandi* of 1713, covered a range of topics that cohere only against this background. Aleatory contracts like gambling (Huygens, Pierre de Montmort, Jakob Bernoulli) and annuities (Johann De Witt, Halley, Nicholas Bernoulli), and later evidentiary problems like the evaluation of historical or courtroom testimony (John Craig, George Hooper, Nicholas and Jakob Bernoulli) constituted the domain of applications for the new theory. By the end of this period, probability had emerged as a distinct and primitive concept, although most of the applications continued to revolve around questions of expectation for some time thereafter.

Just what these probabilities measured was ambiguous from the outset, and remains a matter of controversy to this day. Originally the word "probability" had meant an opinion warranted by authority (Byrne, 1968); hence the Jesuit doctrine of probabilism, which casuists wielded to

absolve almost every transgression on the grounds that one theologian or another had taken a mild view of the matter (Demain, 1935). However, the mitigated scepticism of the early seventeenth century modified even this qualitative sense of probability. The proponents of reasonableness spoke not of certainty but of certainties, ranging from the highest grade of "mathematical" certainty attained by demonstration, through the "physical" certainty of sensory evidence, down to the "moral" certainty based on testimony and conjecture. The precise descriptions of these levels varied slightly from author to author, but the notion of such an ordered scale, and the emphasis that most things admit only of moral certainty, remained a staple of the literature from Hugo Grotius' *De veritate religionis christianae* (1624) to John Locke's *Essay Concerning Human Understanding* (1690) and thereafter. When Bishop Joseph Butler claimed in 1736 that "probabilities are the very guide of life," he was by then repeating a cliché (Butler, 1736, p. iii).

In the context of these discussions, the very meaning of the word "probability" changed from its medieval sense of any opinion warranted by authority to a degree of assent proportioned to the evidence at hand, both of things and of testimony (Locke, [1690] 1959, IV. xv–xvi). These probabilities were qualitatively conceived, and owed much to the language and practice of legal evidence, as the numerous courtroom examples and analogies make clear (Daston, 1988, chapter 2). However, mathematicians like Gottfried Wilhelm Leibniz and Jakob Bernoulli seized upon the new "analysis of hazards" as a means of quantifying these degrees of certainty, and in so doing, converting the three ordered points into a full continuum, ranging from total disbelief or doubt to greatest certainty (J. Bernoulli, 1713, IV.i). Indeed, Leibniz described the fledgling calculus of probabilities as a mathematical translation of the legal reasoning that carefully proportioned degrees of assurance on the part of the judge to the kinds of evidence submitted (Leibniz, [comp. *c.* 1705] 1962, pp. 460–5). The fact that these legal probabilities were sometimes expressed in terms of fractions to create a kind of "arithmetic of proof" (for example, the testimony of a relative of the accused might count only ⅓ as much as that of an unimpeachable witness) may have made them seem mathematically tractable.

The mathematicians who set about trying to measure these probabilities in some non-arbitrary fashion came up with at least three methods: equal possibilities based on physical symmetry; observed frequencies of events; and degrees of subjective certainty or belief. (Other seventeenth-century meanings of "probability," such as the appearance of truth or the strength of analogy, were not successfully quantified.) The first was well suited to gambling devices like coins or dice but little else; the second depended on the collection of statistics and assumptions of long-term stability; and the third echoed the legal practice of proportioning degrees of certainty to evidence.

The various senses emerged from different contexts, and suggested different applications for the mathematical theory. Sets of equiprobable outcomes based on physical symmetry derived from gambling and were applied to gambling – very few other situations satisfy these conditions in an obvious way. Statistical frequencies originally came from mortality and natality data gathered by parishes and cities from the sixteenth century onwards. In 1662 the English tradesman John Graunt used the London bills of mortality to approximate a mortality table by assuming that roughly the same fraction of the population died each decade after the age of six (Graunt, [1662] 1975, pp. 29–30). (Since the bills of mortality registered only cause, not age at death, Graunt's table was based on informed guesswork about what diseases killed whom at what age, and the faith that mortality was regular.) Eighteenth-century authors collected more detailed demographic data and enlisted probability theory in order to compute the price of annuities, and later life insurance, and to argue for divine providence in human affairs.The epistemic sense of belief proportioned to evidence arose from legal theories about just how much and what kind of evidence was required to produce what degree of conviction in the mind of the judge, and inspired applications to the probabilities of testimony, both courtroom and historical, and of judgment.

Latter-day probabilists view these three answers to the question, "What do probabilities measure?" as quite distinct, and much ink has been spilt arguing their relative merits and compatibility (Nagel, 1955). In particular, a bold line is now drawn between the first two "objective" meanings of probability, which correspond to states of the world, and the third "subjective" sense, which corresponds to states of mind. Yet classical probabilists used "probability" to mean all three senses, shifting from one to another with an insouciance that bewilders their more nice-minded successors.

Why were classical probabilists able to conflate these different notions of probability so easily, and often very fruitfully? In part, because the objective and subjective senses were not then separated by the chasm that yawns between them in current philosophy. Legal theorists of the sixteenth and seventeenth centuries found it plausible to assume that conviction formed in the mind of the judge in proportion to the weight of

the evidence presented, and Locke repeated the assumption in a more general context, invoking the qualitative probabilities of evidence: the rational mind assents to a claim "proportionably to the preponderancy of the greater grounds of probability on one side or the other" (Locke, [1690] 1959, vol. II, p. 366). At least two further elements were required to connect the objective and subjective senses of qualitative probabilities. First, precept had to be guaranteed in practice. It was not enough that the mind *should* apportion assent in strict relation to the evidence; it had to be shown that it actually did so. Second, the evidence had to be quantified.

The empiricist philosophy-*cum*-psychology of the late seventeenth and eighteenth centuries satisfied both desiderata. John Locke, David Hartley, and David Hume created and refined a theory of the association of ideas that made the mind a kind of counting machine that automatically tallied frequencies of past events and scaled degrees of belief in their recurrence accordingly. Hartley went so far as to provide a physiological mechanism for this mental record-keeping: each repeated sensation set up a cerebral vibration that etched an ever deeper groove in the brain, corresponding to an ever stronger belief that things would be as they had been. Hume notoriously rejected the rationality of such inferences to the future based on past experience, *pace* Locke and Hartley, but he retained the psychology that made them inevitable. Images of past experiences conjoin to heighten the vivacity of a mental impression, each repetition being "as a new stroke of the pencil, which bestows an additional vivacity on the colours." (Hume, [1739] 1975, p. 135). Since the mind irresistibly conferred belief in proportion to the vivacity of an idea, the more frequent the conjunction of events in past experience, the firmer the conviction that they would occur again. Locke and Hartley contended that this matching of belief to frequencies was rational (Hartley appealing explicitly to the calculus of probabilities; Locke, [1690] 1959, IV.xv; Hartley, 1749, vol. I, pp. 336–9). Hume replied that it was merely habitual, although his "Essay on Miracles" elevated belief based on unexceptioned past experience to at least a kind of reasonableness (Hume, [1758] 1955, chapter 10). All however concurred that the normal mind, when uncorrupted by upbringing or prejudice, irresistibly linked the subjective probabilities of belief with the objective probabilities of frequencies.

They also showed an increasing tendency to reduce all forms of evidence whatsoever to frequencies, in contrast to the legal doctrines that had originally been the prototype of degrees of belief proportioned to evidence. For the judge, the probative weight of eye-witness testimony that the accused had been seen fleeing the scene of the murder with unsheathed bloody sword derived from the quality of the evidence, not its quantity. It mattered not how many times in the past similar evidence had led to successful convictions. Locke remained very close to this legal tradition in his discussion of the kinds of evidence that create probabilities: number of witnesses, their skill and integrity, contradictory testimony, internal consistency, etc. He told the cautionary tale of the King of Siam, who dismissed the Dutch ambassador as a liar because his tales of ice-skating on frozen canals ran counter to the accumulated experience of generations of Siamese that water was always fluid. The King erred in trusting the mere quantity of his experience, without evaluating its breadth and variety. Yet Locke also made a place for "the frequency and constancy of experience" and for the number, as well as the credibility of testimonies (Locke, [1690] 1959, IV.xv). Later philosophical writings on probabilities narrowed the sense of evidence to the countable still further. Hume represents the endpoint of this evolution, in which evidence has become the sum of repeated, identical events. According to Hume, the mind not only counted; it was exquisitely sensitive to small differences in the totals: "When the chances or experiments on one side amount to ten thousand, and on the other to ten thousand and one, the judgment gives the preference to the latter, upon account of that superiority" (Hume, [1739] 1975, p. 141).

The guarantee that subjective belief was willy-nilly proportioned to objective frequencies and also, according to some authors, to physical symmetries allowed classical probabilists to slide from one sense of probability to another with little or no explicit justification. Only when associationist psychology shifted its emphasis to the illusion and distortions that prejudice and passion introduced into this mental reckoning of probabilities did the gap between subjective and objective probabilities become clear enough to demand a choice between the two. It was not so much the development and triumph of a thoroughgoing frequentist version of probability theory that marked the end of the classical interpretation, as the realization that a choice must be made between (at least) two distinct senses of probability. The range of problems to which the classical probabilists applied their theory shows that their understanding of probability embraced objective as well as subjective elements: statistical actuarial probabilities happily co-existed with epistemic probabilities of testimony in the work of Jakob Bernoulli or Laplace.

1.4 DETERMINISM

But the writings of these two towering figures in the history of mathematical probability also contained the manifestoes that, rightly or wrongly, led to the standard view of the classical interpretation as incorrigibly subjective. Both maintained that probabilities measure human ignorance, not genuine chance; that God (or Laplace's secularized super-intelligence) had no need of probabilities; that necessary causes, however hidden, governed all events. Therefore probabilities had to be states of mind rather than states of the world, the makeshift tools of intellects too feeble to penetrate immediately to the real nature of things. Theirs was an epistemological determinism that maintained that all events were in principle predictable, and that probabilities were therefore relative to our knowledge. Bernoulli remarked that backward peoples still gambled on eclipses that European astronomers could now predict; some day gambling on coins and dice would seem equally primitive when the science of mechanics was perfected (J. Bernoulli, 1713, IV.i).

The very mathematicians who had carved out a place for chance in the natural and moral sciences insisted to a man that chance, in Abraham De Moivre's words, "can neither be defined nor understood" (De Moivre, [1718] 1756, p. 253). They did concede that certain statistical rates varied from year to year and from place to place, but they were confident enough in the underlying regularity of phenomena like mortality to simplify and adjust the unruly data accordingly (Pearson, 1978, pp. 319–29). Variability, they believed, would prove just as illusory as chance when fully investigated.

In order to unknot the apparent paradox of the ardent determinism of the classical probabilists, we must look beyond probability theory to the panmathematical spirit of the period in which it emerged. Classical probability arose and flourished during a time of spectacular successes in fitting mathematics to whole new domains of experience, from rainbows to vibrating strings. Natural philosophers like Galileo assumed that if nature spoke the language of mathematics, this was because nature was fully determined, at least from God's viewpoint: the glue that connected causes and effects must be as strong as that which connected premises and conclusions in a mathematical argument. Determinism thus became a precondition for the mathematical description of nature.

At first glance, chance events therefore seemed the least likely candidates for mathematical treatment; even Pascal admitted that there was something paradoxical about a "géométrie du hasard" (Pascal, [1654] 1970, vol. I, part 2, p. 1034). The very earliest mathematical attempts to analyze gambling problems stumbled over just this problem. In his manuscript on the subject (composed *c.* 1525), the Italian physician, mathematician, and inveterate gambler Girolamo Cardano felt mathematically obliged to assert that each face of a die occurred once in every six rolls, although this flew in the face of his own experience at the gaming tables. He resolved the conflict with an appeal to the intervention of luck (he was a great believer in his own), which disrupted the necessary connection between the underlying probabilities and the actual events in favor of particular players (Cardano, [comp. *c.* 1525] 1966, pp. 264–5). He thus relinquished his claim to founding the mathematical theory of probability. Classical probability theory arrived when luck was banished; it required a climate of determinism so thorough as to embrace even variable events as expressions of stable underlying probabilities, at least in the long run. Determinism made a "geometry of chance" conceivable by anchoring variable events to constant probabilities, so that even fortuitous events met what were then the standards for applying mathematics to experience. Those standards were not compatible with older notions of chance as real, or with what we might call genuine randomness in the world.

"Chance" and "fortune" had been part of the philosophical vocabulary since Aristotle, meaning variously coincidence (meeting someone who owes you money on the way to the market), absence of purpose (often identified with necessity, as in the "blind necessity" of Epicurean atoms), or an ample endowment of the "external" goods of good health, wealth, beauty, and children (Sorabji, 1980). All of these meanings survived in ordinary usage, but only one played an important role in classical probability theory. This was the opposition of chance and purpose, particularly divine purpose, of which natural theologians and their probabilist allies like De Moivre made much. John Arbuthnot remarked upon the tiny probability that, year after year, male should exceed female births in a disproportion neatly arranged (or so Arbuthnot argued) to guarantee the future of the institution of monogamous marriage (Arbuthnot, 1710); Daniel Bernoulli pointed to the close alignment of the planets in the plane of the ecliptic as evidence for a single cause of the solar system (D. Bernoulli, 1752). Almost any symmetry or stability unlikely to have come about by "mere chance" – the intricate construction of the human eye; the regular mortality rate – became an argument "from design" for the existence of an intelligent and beneficent deity. The mathematical versions of the argument from design like Arbuthnot's were criticized by contemporaries like Nicholas Bernoulli and d'Alembert, who noted that

all other irregular arrangements of planets or birth ratios were just as improbable. However, the natural theologians persisted until Darwin in seeing chance refuted everywhere by the traces of divine handiwork (see chapter 4). Indeed, such beliefs inspired many of the eighteenth-century statistical demographers, who, like the German pastor Johann Süssmilch, saw in rates of natality, marriage, and mortality, "a constant, general, complete, and beautiful order." (Süssmilch, [1741] 1775, vol. 1, p. 49). Only in the mid-nineteenth century, when the alleged statistical regularities were examined against the background of very different aims and controversies, was variability given its due and chance a new lease on life. But for the classical probabilists "chance" and "luck" that stood outside the causal order were superstitions. If we could see the world as it really was, penetrating to the "hidden springs and principles" of things, we would discover only necessary causes. Probabilities were merely provisional, a figment of human ignorance and therefore subjective.

The classical interpretation of mathematical probability was thus characterized in precept by determinism and therefore by a subjective slant, and in practice by a fluid sense of probability that conflated subjective belief and objective frequencies with the help of associationist psychology. It is however somewhat misleading to call this an "interpretation" of the mathematical theory, for to the classical probabilists the interpretation *was* the theory. The "doctrine of chances," or "art of conjecture," as probability theory was variously called in the eighteenth century, was a part of "mixed mathematics," a term deriving from Aristotle's explanation of how harmonics or optics mixed the forms of mathematics with the matter of sound and light (*Physics*, 193b22–194a15). In contrast to the more modern applied mathematics, mixed mathematics did not necessarily presuppose a prior and independent mathematical theory to be applied to various subject matters. Classical probability theory had no existence independent of its subject matter, *viz.* the beliefs and conduct of reasonable men. As Laplace put it in a famous passage, mathematical probability was in essence "only good sense reduced to a calculus" (Laplace, [1814] 1951, p. 6). Its status was less that of a mathematical theory with applications than that of a mathematical model of a certain set of phenomena, like the part of celestial mechanics that described lunar motion. As such, it was held up to empirical test. If astronomical theory failed to predict lunar perturbations, so much the worse for the theory. When the results of classical probability theory did not square with the intuitions of reasonable men, it was the mathematicians who returned to the drawing board.

1.5 REASONABLENESS

The protracted controversy over the St. Petersburg problem was just such a clash between reasonableness and the dictates of probability theory, and illustrates how seriously mathematicians took their task of modeling "good sense." The problem was first proposed by Nicholas Bernoulli in a letter to Pierre de Montmort, and published in the second edition of the latter's *Essai d'analyse sur les jeux de hasard* (1713). Pierre and Paul play a coin toss game with a fair coin. If the coin comes up heads on the first toss, Pierre agrees to pay Paul \$1; if heads does not turn up until the second toss, Paul receives \$2; if not until the third toss, \$4, and so on. Reckoned according to the standard method, Paul's expectation (and therefore the fair price of playing the game) would be:

$$E = (\tfrac{1}{2} \times \$1) + (\tfrac{1}{4} \times \$2) + (\tfrac{1}{8} \times \$4) + \ldots + [(\tfrac{1}{2})^n \times \$2^{n-1})] + \ldots$$

Since there is a small but finite chance that even a fair coin will produce an unbroken run of tails, and since the pay-offs increase in proportion to the decreasing probabilities of such an event, the expectation is infinite. However, as Nicholas Bernoulli and all subsequent commentators were quick to observe, no reasonable man would pay even a small sum to play the game. Although the mathematicians labeled this a paradox, it contained no contradiction between results derived from assumptions of equal validity. The calculation of expectation is straightforward, and there is nothing in the mathematical definition of expectation that precludes an infinite answer. Rather, it struck them as paradoxical that the results of the mathematical theory could be so at odds with the manifest dictates of good sense. Applied mathematicians in the modern sense might simply have questioned the suitability of the mathematical theory for this class of problems, but that route was not open to the mixed mathematicians of the eighteenth century. In their eyes the clash between mathematical results and good sense threatened the very validity of mathematical probability.

This is why the St. Petersburg problem, trivial in itself, became a *cause célèbre* among classical probabilists. In 1738 Nicholas' cousin Daniel Bernoulli published a resolution of the paradox in the annals of the Academy of St. Petersburg, the first of many such attempts. Daniel's memoir not only named the problem after the Academy; it also raised the fundamental issues of the definition of probabilistic expectation and its relationship to reasonableness that animated the subsequent controversy. In essence, he proposed a new notion of reasonableness, and a redefinition of expectation to match. Observing that the standard definition of expectation was like an impartial judge who ignores the individual characteristics of the risk-takers, Bernoulli argued that in situations like the St. Petersburg problem more than equity was at stake. Here the players acted more out of prudence than of fairness, and the definition of expectation had to be modified accordingly. In contrast to the "mathematical" expectation of equity Bernoulli proposed the "moral" expectation of prudence, defined as the product of the probability of the outcome and what later became known as its utility. That is, Bernoulli substituted values relative to individual preference for monetary values, with the understanding that the richer you are, the more it takes to make you happy. By making utility a logarithmic function of monetary wealth, he was able to derive reassuringly small expectations, depending on one's fortune, for the St. Petersburg game. He was also able to show that moral expectation harmonized with other widely accepted practices and beliefs: for example, sober men of affairs knew to avoid the gaming tables and to distribute their cargo among several ships, and moral (but not mathematical) expectation gave results that confirmed their wisdom (D. Bernoulli, [1738] 1954).

Significantly, Daniel Bernoulli's examples were drawn from the world of trade and commerce, in contrast to the legal examples that had dominated the earlier discussions of expectation. Nicholas Bernoulli, who was professor of both Roman and canon law at the University of Basel as well as an accomplished mathematician, objected that moral expectation failed "to evaluate the prospects of every participant in accord with equity and justice." His cousin Daniel replied that his new definition of expectation "harmonize[d] perfectly with experience." What was at issue between the Bernoulli cousins was not whether probabilistic expectation should model reasonableness, but rather wherein such reasonableness consisted. Nicholas sided with the older sense of equity derived from aleatory contracts; Daniel with the increasingly important sense of economic prudence, derived from commerce. The prototypical reasonable man was no longer an impartial judge but rather a canny merchant, and the mathematical theory of probability reflected that shift.

Daniel Bernoulli's solution to the St. Petersburg problem was by no means universally accepted by other classical probabilists. Some, like Jean d'Alembert, thought the problem lay not with the monetary values but with the probabilities – was it really physically possible for a fair coin to continuously turn up tails? (see Swijtink, 1986). Others, like Siméon-Denis Poisson, pointed out that the length of the game was limited by the wealth of the two players. Still others, like M. J. A. N. Condorcet, argued that mathematical expectation was indeed correct, but only when applied as an average to many repetitions of the game (Daston, 1980; Jorland, 1987). However, all agreed that the paradox was a real one, and struck at the very foundations of the mathematical theory. The fact that classical probabilists were willing to tinker with definitions and postulates as fundamental as expectation in order to realign their calculus with good sense attests to their commitment to the mixed mathematical goal of quantifying the reasonable.

Given this goal, the classical probabilists always ran the risk of superfluity. If their calculus yielded results that echoed what the enlightened had known all along – as preface after mathematical preface emphasized was the case – then all the elaborate machinery of equations and calculations did seem a belaboring of the obvious. The probabilists replied that, in Voltaire's words, common sense was not that common. Only a small elite of *hommes éclairés* could reason accurately enough by unaided intuition; the calculus of probabilities sought to codify these intuitions (which the probabilists believed to be actually subconscious calculations) for use by *hoi polloi* not so well endowed by nature. This mathematical model of good sense could be compared to spectacles. By applying the same optical principles responsible for normal eyesight it was possible to extend vision artificially; similarly, the calculus of probabilities formalized the good sense that came naturally to the fortunate few to help out the befuddled many. And several probabilists suggested that even *hommes éclairés* could sometimes benefit from mechanized reasoning when the issue at hand was extremely complicated or obscured by sophistry.

The ideal of a calculus of reasoning, a set of formal rules independent of content, exerted a certain fatal attraction for many seventeenth- and eighteenth-century thinkers. The probabilists' hope of turning the "art of conjecture" into such a calculus echoes the seventeenth-century fascination with method taken to an extreme. In the end, the methods of Bacon, Descartes, and a host of lesser lights always relied to some extent on judgment, and therefore were of limited use in truly perplexing situations. As Leibniz quipped apropros of Descartes' famous rules of method, one takes what one needs and does what one ought. Leibniz also best captured the allure of a formal calculus that eliminated personal discretion, and with it, strife. Observing that among the learned only mathematicians ever resolved their problems to everyone's satisfaction, he envisioned a kind of "universal characteristic" that would somehow assign numbers to fundamental ideas and invent arithmetic-like operations to combine them. Henceforth, whenever a controversy arose a call to pen and paper would replace a call to arms: "Let us calculate, Sir; and thus by taking to pen and ink, we should soon settle the question" (Leibniz, [1677] 1951, p. 15). Classical probabilists from Jakob Bernoulli through Siméon-Denis Poisson wielded their calculus as an instrument of consensus in uncertain matters, attempting to make the new-style reasonableness as coercive as the old-style demonstration.

Classical probability theory was thus at once a description of and prescription for reasonableness. Mathematicians checked their results against the intuitions of the elite of reasonable men, and when the two did not jibe, it was the mathematicians who amended their theory, as in the case of the St. Petersburg "paradox." The theory was so far descriptive. But the probabilists also aimed to instruct the great majority in rational decision under uncertainty, be it about whether to accept a theory about the formation of the solar system, purchase an annuity, or believe the testimony of a witness. Here the theory was prescriptive, and sometimes presumptuously so, as when Condorcet sent Frederick II of Prussia a weighty tome of calculations on how to improve the judicial system (Condorcet, 1785).

Two ambiguities, neither ever clearly recognized, confused and ultimately undermined the classical program to render reasonableness mathematical. The first surfaced early on in the debate over expectation sparked by the St. Petersburg problem: there were several distinct brands of reasonableness, and they sometimes led to very different solutions of the same problem. The fair judge and the shrewd merchant did not agree on the proper definition of expectation, but both belonged to the select company of *hommes éclairés*. However, the probabilists persisted in believing that reasonableness was monolithic, despite endless debate over just how to define it. It took an upheaval of the magnitude of the French Revolution to shatter their faith in the natural consensus of the enlightened few. No doubt their dream of a calculus that would widen that consensus contributed to their tenacity in the face of clear evidence that reasonableness was of several sorts.

The second ambiguity concerned just where to draw the line between description and prescription. The debate between Daniel Bernoulli and d'Alembert over smallpox inoculation dramatized the difficulty. During the middle decades of the eighteenth century smallpox ravaged Europe, carrying off as much as one-seventh of the populations of Paris and London. The reports of Lady Wortley Montagu in England and Voltaire in France on the technique of inoculation against the disease, long practiced in Turkey, fanned popular and medical hopes that the scourge might be curbed. However, inoculation itself was not without its dangers, and there was a small (about one in two hundred) chance that recipients would die from the treatment in the space of a month or so. In 1760 Daniel Bernoulli submitted a memoir to the Paris Academy of Sciences that used what data could be had on smallpox mortality and probability theory to calculate the average gain in life expectancy from inoculation for any given age. Bernoulli's statistics were scanty and his simplifying assumptions dubious, but these were not the principal reasons why d'Alembert, himself a proponent of inoculation, opposed Bernoulli's favorable conclusions. Rather, d'Alembert claimed that Bernoulli's conventional probabilistic treatment of the problem ignored the actual psychology of risk-raking: confronted with a choice of a small short-term risk (inoculation) and a large long-term risk (smallpox), many reasonable people prefer in effect to bet on the long-term. D'Alembert did not altogether condone this preference, which he called the "common logic . . . half good, half bad." But he held probability theory to its task of describing the conduct of people otherwise acknowledged to be reasonable, whereas Bernoulli saw this as an opportunity for mathematicians to reveal to the benighted public where its own self-interest lay (Daston, 1979). Over the course of its long career, the emphasis within classical probability theory slowly shifted in Bernoulli's direction, from the descriptive to the prescriptive, as a result both of disillusionment with the ideal of reasonableness and of the widening gap between objective and subjective probabilities. By the time Laplace published his *Essai philosophique sur les probabilités* in 1814, probability theory had become a tool rather than a model of enlightenment.

These, then, were the hallmarks of the classical interpretation of mathematical probability: a fruitful conflation of subjective and objective senses of probability; a thoroughgoing determinism that firmly denied the existence of real chance and that highlighted the subjective sense of probability in programmatic statements; a commitment to the mixed mathematical goal of modeling phenomena; and above all an identification of the theory with that form of practical rationality that came to be known as reasonableness. To those schooled in twentieth-century distinctions, the mathematical theory is independent of both its innumerable possible interpretations and its applications, but for classical probabilists they were all of a piece. They believed that their calculus succeeded or failed on the strength of its applications, and the applications they attempted reflected their vision of the theory and its proper subject matter.

1.6 RISK IN GAMBLING AND INSURANCE

Gambling was the paradigmatic aleatory contract, and the very first problems solved by the mathematicians were of this sort. Pascal and Fermat's correspondence and Huygens' treatise were exclusively devoted to gambling problems, which continue to be the staple of elementary probability texts to this day. Between Huygens' *De ratiociniis in ludo aleae* (1657) and the first edition of De Moivre's *Doctrine of Chances* (1718), the complexity of the problems and the sophistication of the mathematical methods had advanced from simple computations of expectation using a bit of algebra to involved computations of duration of play using what were then the most advanced techniques of analysis (Schneider, 1968). Interestingly enough, these early writers on the mathematics of gambling made no hard-and-fast distinction between games of pure chance and those of mixed chance and skill, albeit with a rather arbitrary assignment of probabilities for the latter. Jakob Bernoulli, for example, undertook a long analysis of the probabilities of a game of tennis, and several eighteenth-century manuals on cock-fighting appended calculations of betting odds assuming equal chances, contradicting the message of the text that factors like breeding, size, and past performance gave some cocks a definite edge over the others.

Despite rapid progress, most probabilists were ill at ease with their stock-in-trade gambling problems and their disreputable associations. Some crusading spirits, Daniel Defoe among them, hoped that mathematicians might cure the reckless of their passion for cards and dice with a strong dose of calculation (Defoe, 1719). The mathematicians preached the folly of such pursuits along with the moralists, but apparently most gamblers had little appetite for either sort of edification. Those few who did turn to the probability texts for guidance were likely to be cruelly disappointed, like the hero of Thomas Smollett's *The Adventures of Ferdinand Count Fathom* (1753). Fathom learned to "calculate all the chances with the utmost exactness and certainty," only to be fleeced by a team of sharpsters. The eighteenth-century lottery craze that began in England and the Netherlands and swept Europe provided the mathematicians with problems but very little employment. Neither the designers nor the patrons of the several national lotteries in Britain, France, Italy, and the German states consulted the probabilists for an expert opinion. (Frederick II was a notable exception: he wrote Euler several anxious letters about the Genoan-style lottery proposed to raise money to pay Prussia's war debts.) Indeed, there was little reason on either side to do so. The traditional nonprobabilistic pay-off schemes gave governments a sizable profit margin, and the majority of those who bought lottery tickets pitted the improbability of winning against the still greater improbability that they could alter their situation in any other way. Talleyrand may have been correct in criticizing the French national lottery as a tax upon the poor, who favored the ruinous high risk/big win combinations (Talleyrand, 1789, p. 6). But given the rigidity of the social order, it is at least arguable that this was a rational if desperate strategy for the ambitious. A few lessons in probability theory were therefore unlikely to influence either the buying or the selling end of the lottery business (Daston, 1988, chapter 3).

Feeling themselves thus at once neglected and despised for their interest in the mathematics of gambling, classical probabilists eagerly turned their attention to other, more respectable types of aleatory contracts. The writings of Jakob Bernoulli and his nephew Nicholas abound with such applications: wine futures, annuities, maritime insurance, the expectation of an inheritance, dowry funds, and usufructs were all grist for their mill. However, the Bernoullis recognized that probability values were not easily derived for these more interesting examples of aleatory contracts, in contrast to games of pure chance.

This was not an insuperable obstacle for the Dutch statesman and mathematician Johann De Witt, who in 1671 presented the Estates General of Holland and West Friesland with the first mathematically based annuity scheme (never implemented) to raise money for the war with England. De Witt applied Huygens' new calculus of expectations to the pricing of annuities, simply assuming that the chances of dying in any six-month period between ages 3 and 53 were equal, and that they decreased in a regular fashion for subsequent intervals until age 80. He did nonetheless avail himself of the opportunity to check his assumptions against the data on mortality on purchasers of Amsterdam annuities with the help of mayor and fellow mathematician Johannes Hudde, although he did not revise his original price estimates in the direction suggested by these records (De Witt, 1671). In general, mathematicians all over Europe were quick to recognize the relevance of mortality tables like John Graunt's to extensions of probability theory beyond gambling, and in 1693 Edmund Halley published the first truly empirical mortality table, compiled from the data gathered by the Protestant pastor of Breslau at Leibniz' instance (Halley, 1693).

Statistics during this period took the form of demographic data on births, marriages, and deaths because this was information that governments had been requiring parishes to register since the first half of the sixteenth century, for reasons having nothing to do with probability theory. English and French parishes were ordered to record christenings, weddings, and burials to provide official proof of age and status before the law; and as early as 1562 the city of London published bills of mortality to keep track of plague deaths as a warning of a major outbreak. These bills listed only place and cause of death, and only after 1625 any cause of death other than plague. The London bills were not kept continuously until 1603, and ages at death were first recorded in 1728. This is why Graunt had to resort to shrewd guesswork and regularizing hypotheses to construct his mortality table from the London bills, and why a number of readers, including Huygens and Hudde, were wary of his figures. Governments differed greatly in the alacrity and thoroughness with which they assembled such data: Sweden led the way, with France close behind, but several central European nations did not register birth and death until well into the nineteenth century (Meuvret, 1971).

Mathematicians unhesitatingly read these statistical frequencies as probabilities, and saw in them the means of advancing from gambling to more reputable kinds of aleatory contracts. Halley immediately put his mortality table to use calculating the price of annuities, claiming that traditional methods inequitably ignored different life expectancies. Other mathematicians like Nicholas Bernoulli and De Moivre also emphasized that equity in such matters was best served by the new probabilistic techniques, an indication that they still viewed annuities within the context of contract law. By the mid-eighteenth century, there existed a sizable mathematical literature on the subject in English, Dutch, French, German, and Latin. De Moivre's *Treatise of Annuities* (1725) was perhaps the most mathematically distinguished of these works, although his methods and assumptions were sometimes contested by other probabilists. Two conceptual issues in particular exercised the probabilists, in addition to the empirical dilemma of which mortality statistics to trust: the definition of life expectancy, and the shape of the mortality curve.

Life expectancy entered mathematical probability on direct analogy to expectation. Christiaan Huygens' brother Lodewijk was among the first to grasp the significance of Graunt's table for the fledgling calculus of chance, and in 1699 he wrote to Christiaan posing the problem of how to compute "the expectation of life" using Graunt's table. (Graunt himself had no mathematics beyond his "shoppe Arithmetique," and apparently knew nothing of Huygens' 1657 treatise: for over a century, the borrowing between mathematical probability and statistics was almost all in one direction.) Lodewijk's own solution was closely patterned on Christiaan's definition of probabilistic expectation in *De ratiociniis in ludo aleae*: multiply the number of people in each of Graunt's age brackets by the average number of years they survive, and divide by the total number of people, giving 18 years, 2 months. Christiaan however proposed a different method, based on a graph he had drawn from Graunt's figures: use the graph to find the age by which half of the original cohort has died – about 11 years. Drawing a distinction that roughly paralleled that between mathematical and moral expectation, Christiaan recommended Lodewijk's method for annuities, where equity prevailed, and his own for wagers, where profit was all that counted (Société Générale Néerlandaise, 1898, pp. 57–69). His successors found it more difficult to decide which method applied when, and wrangled over the issue throughout the eighteenth century. Nicholas Bernoulli for example preferred Lodewijk's method, while De Moivre sided with Christiaan.

When the mortality curve had a certain shape – when death carried off an equal proportion of the population in equal time intervals – these two methods gave the same answer for annuities on single lives, though not on joint lives. Halley, De Moivre, and others assumed that mortality did indeed follow an arithmetic progression, although their initial grounds for doing so had more to do with ease of calculation and a faith in nature's proclivity for simple curves than with the data itself. Graunt's lack of data forced such sweeping assumptions on him, while Halley blithely proclaimed that had data on Breslau mortality been collected for twenty years rather than five, it would surely have approximated such a regular curve more closely. Thus encouraged, De Moivre took Halley's Breslau table as empirical confirmation for his assumption of an arithmetic progression. Not all probabilists were so sanguine – the Dutch mathematician Nicholas Struyck for example warned that "nature doesn't listen to our suppositions" (Société Générale Néerlandaise, 1898, p. 89). But the great majority of those who wrote on the mathematics of mortality believed implicitly in its regularity. Indeed, that belief was almost a precondition for collecting such statistics and *a fortiori* for subjecting them to mathematical analysis.

Why mortality should have been assumed regular and not other phenomena of equal practical importance like the incidence of fires or shipwrecks is somewhat mysterious. It was an age in which all were subject to wild fluctuations due to plague, huge conflagrations like the Great Fire of London, or war on the high seas. Yet only mortality was singled out for study, a fact that greatly influenced the applications of probability

theory during the eighteenth century. It was not the case that the available data revealed the regularities; on the contrary, the data was collected only once such regularities were posited, and it was often adjusted to fit the hypothetical regularities more closely. Natural theologians like William Derham in England and Johann Süssmilch in Germany who argued from design to God's providence eagerly seized upon alleged regularities in the rates of birth and death, but natural theology does not seem to have been the original source for belief in such regularities. Although it is a recurring theme in De Moivre's work on probability, there is no trace of natural theology in the pioneering works of Graunt and Halley. The vogue for demographic theology began later, with John Arbuthnot's 1710 memoir "An Argument for Divine Providence Taken from the Constant Regularity Observed in the Birth of Both Sexes," and received its most extreme and expansive expression in Süssmilch's compendium of demographic proofs for God's handiwork in human natality and mortality rates. Seeing God's hand in the mortality curve was the result of rather than the reason for asserting simple regularities.

Two aspects of mortality may have made it a more promising candidate for regularity seekers than fires or other catastrophes: its inevitability, and its ancient connection with the continuous numerical variable of age at time of death. With luck one might escape fire or shipwreck, but death comes to all. Therefore a complete enumeration of the population was in principle possible, and long custom made age the obvious choice for the independent variable: even the Bible duly records the age to which Adam survived. Independent variables were not so easy to come by for other events. Was it building material or trade that mattered most in susceptibility to fires? Was it the experience of the captain and the crew, the seaworthiness of the vessel, or the route and season that weighed heaviest in a safe passage? And how were any of these factors to be quantified? Of course, age was not the only possible choice for mortality; state of health was also a candidate, but one not so easily numbered. Neither the inevitability nor the age-linked character of death were sufficient conditions for believing mortality to be regular. Centuries of observers had recognized both without drawing that conclusion. However, in an age that sought regularities everywhere, particularly quantitative ones, these features may have been suggestive.

By 1750 the mathematics of mortality, particularly as applied to annuities and other reversionary payments, was the cutting edge of research in probability theory. Beginners still learned their probability from gambling problems, but the best minds were more interested in the latest mortality table than in what went on in London clubs like White's or in the Paris "académies de jeu." Annuities were simply the obverse of life insurance – the annuitant pays a lump sum in return for regular payments so long as he (or some designated person) lives; the policy holder makes regular payments in return for a lump sum paid to his beneficiaries upon his death. Hence some mathematicians like James Dodson also turned their attention to the mathematics of this second variety of aleatory contract that made use of mortality tables. Armed with statistics and the most refined analytic techniques, the probabilists set about rationalizing the business of risk taking. And it was big business: in 1753 Dodson observed that "of much the greatest part of the real estates" of Great Britain depended on "the value of lives" (Dodson, 1775, vol. II, pp. vii–viii). The mathematicians wrote handbooks for men of affairs as well as treatises for their colleagues. They pitched their manuals to the barely numerate clerks with only arithmetic, translating the equations into words and appending tables to spare the reader onerous calculations. Although the lack of statistics prevented them from mathematizing other aleatory contracts like maritime insurance, the probabilists hoped to make themselves useful and perhaps prosperous by reforming the bustling trade in "values of lives."

That hope was seldom realized. By and large, eighteenth-century buyers and sellers of annuities and life insurance were no more interested in probability theory than the gamblers. Their indifference seems at first glance inexplicable, particularly in the percolating London and Amsterdam markets where purveyors of annuities and a motley assortment of insurance schemes scrambled for competitive advantage. Yet the first mathematically based enterprise of this sort, the Equitable Society for the Assurance of Lives, was established only in 1762, and then by the mathematician Dodson rather than by an insurer (Ogborn, 1962, pp. 24ff). Indeed, the project met with almost universal scepticism from the London insurance community, and went another twenty years without imitators. Most annuity and life insurance plans instead charged flat rates, regardless of age, and those few that did take some account of age did so apparently without consulting the available mortality statistics and mathematical manuals on the subject. Far from profiting from the law of large numbers, these societies strictly limited the size of their membership. The influence of the mathematical theory of risk on the practice of risk was thus effectively nil for most of the eighteenth century.

Why were dealers in annuities and insurance, not to mention gamblers, so reluctant to avail themselves of a technology custom-made (and often

custom-written) for them? Their failure to do so seems a *prima facie* case of irrationality: mindless conservatism blinded them to the manifest superiority of the new mathematical methods. It is however difficult to recognize in this description the booming, innovative business centers of eighteenth-century London and Amsterdam, which were also the centers for probabilistic research in these applications. Daniel Defoe called his "the Projecting Age" and his own *Essay on Projects* (1697) testifies to the efflorescence of schemes to make money and/or improve life by hook or crook, by "Banks, Stocks, Stock-jobbing, Assurances, Friendly Societies, Lotteries, and the Like" (Defoe, 1697, pp. 8–9). The London merchants who invented insurance against fire, cuckoldry, and even losing at the lottery may be accused of many things, but inertia was not one of them.

The context in which annuities and life insurance was sold goes a bit further towards explaining the general neglect of probabilistic techniques. Annuities were an ancient form of investment, but they acquired a new function in eighteenth-century London after loans at more than 5% interest were declared usurious. Many of the annuities taken out thereafter were essentially loans at a much higher rate of interest, made legal by the aleatory nature of the contract: annuities were sold very cheaply (mostly at six years' purchase) on the life of the *seller*, in need of quick cash even on very unfavorable terms (Campbell, 1928). These financial arrangements clearly subverted the original purpose of the annuity as a kind of pension for the lifetime of the buyer, and mortality statistics were therefore not so relevant. Life insurance at this time was also dedicated to very different ends. Since the sixteenth century, and possibly earlier, a life insurance policy was almost always a wager on the life of a third person, often a celebrity not personally acquainted with the buyer – for example, a cardinal or a sovereign. As such, it was summarily banned in almost all European countries except England, where it continued to flourish in this form throughout most of the eighteenth century. London life insurance offices made book on everything from the life of Sir Robert Walpole to the succession of Louis XV's mistresses. Those who took out such betting policies were usually as little interested in probability theory as the purchasers of lottery tickets.

There were other, still deeper reasons why the practitioners of risk resisted the mathematical theory of risk. Long before the advent of mathematical probability and statistics, parties to aleatory contracts like gambling, annuities, and maritime insurance had agreed upon the price of a future contingency on the basis of intuitions that ran directly counter to those of the probabilists. Whereas the dealers in risk acted as if the world were a mosaic of individual cases, each to be appraised according to particular circumstances by an old hand in the business, the mathematicians proposed a world of simple, stable regularities that anyone equipped with the right data and formulae could exploit.

For the practitioners of risk, accepting the mathematical theory of risk required profound change in beliefs, and in the case of life insurance, also of values. They had to replace individual cases with rules that held only *en masse*, and to replace seasoned judgment with reckoning. What the mathematicians dismissed as local perturbations that would cancel one another out in the long run, the practitioners viewed as the very stuff of their trade – the spry sixty-year-old who outlived the ailing thirty-year-old; the ship waylaid by pirates on the way to the Levant; the unusually cold winter that ruined that year's vintage. Although the sixteenth-century manuals on maritime insurance were full of detailed advice on every other particular, they were mute on the subject of premiums, except to urge the insurer to take full account of the season, the route, the cargo, the condition of the ship, rumors of pirates, etc. Only good judgment and a thorough versing in these minutiae could price the risk in question. Writers on annuities gave analogous advice. The practice of risk was not simply astatistical; it was positively antistatistical in its focus on the individual case to the neglect of large numbers and the long term. The practitioners equated time with uncertainty, for time brought unforeseen changes in these crucial conditions; the probabilists equated time with certainty, the large numbers that revealed the regularities underlying the apparent flux. For the practitioners of risk to accept the mathematical theory of risk required a new conception of the world, and it is therefore perhaps not so surprising that they accepted the new techniques and the beliefs they implied slowly, if at all (Daston, 1987).

1.7 EVIDENCE AND CAUSES

Aleatory contracts were not the only area of application that the classical probabilists took over from the jurists. They also turned their attention early on to problems of evidence, particularly those of witness testimony. Although their inspirations owed much to the courtroom, however, their most spectacular example was historical rather than legal: the Judeo-Christian lore of miracles. Several streams fed the current of Enlightenment interest in the subject. In the late sixteenth century, Protestant and Catholic apologists had clashed over the relative weight carried by scripture and tradition in matters of faith, the Protestants insisting that scripture alone carried the divine imprimatur, and the Catholics retaliating

with a sophisticated comparison of various biblical texts that threw doubt on the existence of some pristine original. During the seventeenth century the critical hermeneutics that emerged from this debate and from the sceptical challenge of the new pyrrhonists changed the attitude of historians to their sources, and the trustworthiness of witnesses became a major methodological problem. New conceptions of God and of nature, as well as a heightened sense of the gap between learned opinion and popular errors, made accounts of marvels and miracles particularly suspect. Jean Calvin declared that the age of miracles was past; Francis Bacon warned against the promiscuous mix of fact and fable in natural history; Pierre Bayle ridiculed the idea that God would resort to singular events like comets to announce his intentions; John Toland attempted natural explanations of biblical miracles. A chasm yawns between the arguments and motives of Calvin and the Deist Toland, but they both helped forge a new vision of a God more revered for rules than for exceptions. David Hume's famous "Essay on Miracles" was simply one of many contributions to the large Enlightenment literature on this topic (Burns, 1981).

Both the philosophical and mathematical treatments of the problem took their cue from the venerable legal distinction between intrinsic and extrinsic evidence. Intrinsic evidence derives from the nature of things; extrinsic evidence from testimony. Plausibility and internal consistency bore on the one; number, integrity, and competence of witnesses on the other. Combinations of the two, duly sifted and weighted by a sagacious judge, created rational belief in varying degrees. For the probabilists, the trick was to find measures of both sorts of evidence and some function for converting them into probabilities. At first they floundered. John Craig attempted an elaborate Newtonian model of "motive forces" of argument that impelled the "velocity of suspicion" through a "space" representing "degrees of assent" in his *Theologiae christianae principia mathematica* (1699). In the same year the Royal Society of London published an anonymous article (probably by George Hooper) that analyzed the problem in terms of Huygenian expectation (Hooper, 1699). Jakob Bernoulli began part IV of the *Ars conjectandi* (1713) with a tangled exposition of "pure" and "mixed" forms of both sorts of evidence (J. Bernoulli, 1713, IV. ii–iii). Nicholas Bernoulli's *De usu artis conjectandi in jure* assumed a "true proportion" of veracity for each individual witness, to be computed from past performance (N. Bernoulli, 1709, chapter 9). No wonder the French probabilist Pierre de Montmort in 1713 wrote off the whole enterprise as doomed by an "infinity of obscurities" and arbitrary assumptions (Montmort, 1713, Préface).

Not only did the mathematicians face the problems of quantifying the probative force of evidence; they also had to incorporate a number of widely held assumptions about how such evidence was to be weighted and combined into their model. For example, jurists and historians agreed that written records were more reliable than oral tradition – but by how much? Everyone knew that eye-witness testimony of two independent witnesses counted for more than that of two who had conversed beforehand – but how to capture this in calculation? It is proof of the strong and lingering influence of the old legal "probabilities" upon the new mathematical probabilities that the classical probabilists were undaunted by these formidable difficulties. Almost every probabilist from Jakob Bernoulli through Poisson tried his hand at the probability of testimony, and Montmort was exceptional in asking whether such matters were really legitimate applications of the mathematical theory.

The question of miracles had the great advantage of drastically simplifying these vexed considerations almost beyond the need for calculation. Hume's formulation was probably the clearest: miracles are by definition violations of the laws of nature, and therefore their intrinsic probability is zero. No amount of extrinsic probability would thus suffice to make reports of miracles credible; no witness was so sincere, so reliable, as to counterbalance the overwhelming improbability of such an event. Poisson later went so far as to recommend rejecting one's own perception of a miracle as a hallucination on these probabilistic grounds (Poisson, 1837, pp. 98–9). Hume also had recourse to a guilt-by-association argument – the more ignorant and barbarous the people, the keener their appetite for marvels – but the crux of his essay was the annihilation of extrinsic by intrinsic evidence. His more astute critics, like the English probabilist and Unitarian divine Richard Price, recognized this, and tried to boost the intrinsic probability above the zero point, and to buttress the extrinsic probability by appeal to everyday intuitions and practices (Price, [1767] 1811, pp. 226–7). But even defenders of miracles like Price accepted the underlying assumption that intrinsic and extrinsic probabilities multiplied to produce degrees of rational belief.

Hume's argument that extrinsic probability derived from past experience, conceived as the repetition of identical events, stemmed from another important branch of classical probability, the probability of causes. Here the probabilists broke new ground in natural philosophy with a novel model of causation. In the late seventeenth century it was a commonplace that nature admitted of two sorts of investigation: from causes to effects and from effects to causes. The former method of "synthesis" deduced phenomena from underlying causes, as the mathematician deduced theorems from axioms and postulates. This was the royal road to natural philosophy, the one which would at once secure both clear understanding and rigor. Alas, the dimness of the human senses, the fallibility of human reason, and the intricacy of nature hid the causes of all but a very few phenomena, forcing natural philosophers back upon the complementary method of "analysis." Since the same effects could be deduced from various hypothetical causes – the phases of Venus from both the Copernican and Tychonic systems, to take a seventeenth-century example – success was no guarantee of certainty.

Beginning with Jakob Bernoulli's celebrated theorem, the probabilists addressed the problem of how much success generated what degree of certainty. This meant recasting ideas of cause and effect in terms tractable to probability theory, i.e. relating them to the ubiquitous urn model that became the hallmark of the classical interpretation. Imagine an urn filled with colored balls in some fixed proportion, from which repeated drawings with replacement are made. Bernoulli's theorem states that in the limit, as the number of drawings N approaches infinity, the probability P that the observed proportion of colored balls m/N corresponds to the actual proportion p within the urn approaches certainty.

$$\lim_{N\to\infty} P(\,|p - m/N| < \varepsilon\,) = 1, \quad \text{for any } \varepsilon$$

Here, the fixed but unknown proportion of balls in the urn corresponds to the hidden causes, and the results of the repeated drawings to the observed effects. Bernoulli's theorem amounted to a guarantee that in the long run observed frequencies would stabilize around the "true" underlying value, that regularity would ultimately triumph over variability, cause over chance (J. Bernoulli, 1713, IV.v).

This result was a curious mixture of the banal and the revolutionary. Banal, because as Bernoulli himself admitted in a letter to Leibniz, "even the stupidest man knows by some instinct of nature *per se* and by no previous instruction" (Leibniz, [1703] 1962, vol. III, part 1, pp. 77–8) that the greater the number of confirming observations, the surer the conjecture; revolutionary, because it linked the probabilities of degrees of certainty to the probabilities of frequencies, and because it created a model of causation that was essentially devoid of causes. Heretofore causes had been understood as essences, or as microscopic mechanisms. In either case, they produced their effects necessarily, if obscurely. Causes were more loosely connected to effects in Bernoulli's model of *a posteriori* reasoning, just as the results of individual drawings were only loosely connected to the actual proportion of balls. Necessity obtained only in the infinitely long run. Moreover, the new model abandoned all search for mechanisms, for the hidden springs and principles that ran the clockwork of the world. In Bernoulli's urn model, numbers generated numbers; the physical processes by which they did so were wholly inscrutable.

Bernoulli had intended his "golden theorem" to be of use in practical matters; a way of investigating human mortality, the weather, and other variable but vitally important phenomena. The common sense of the "stupidest men," though recognizing that more observations meant more reliability, did not suffice to show how *many* observations warranted what *degree* of certainty – hence the great utility of his theorem. But Bernoulli's own way of performing these computations was so conservative that even he seems to have been daunted by the enormous number of trials required for a reasonable degree of certainty. If one was drawing (with replacement) from an urn with 30 white balls and 20 red balls, and wished to know the true ratio to within $1/50$ with a probability of at least $1000/1001$, his methods required 25,550 trials. It took Abraham De Moivre's more refined methods of approximating the sums of the terms of a binomial to bring the number of trials down to a more practicable number (Stigler, 1986, pp. 72–85).

But the greatest obstacle to the use of Bernoulli's theorem for the purposes he had intended it, to plumb "the work of nature or the judgment of men," was more fundamental. The theorem was the cornerstone of the probability of causes, and yet it did not really provide a way of reasoning from known effects to unknown causes even in the restricted sense of frequencies and probabilities, although Bernoulli sometimes wrote as if it did. For one was not justified in simply reading off the underlying probability from the observed frequency in any finite number of trials: without some additional simplifying assumption, the frequencies never converge unambiguously to a single value. *Given* the probability, Bernoulli's theorem revealed how likely it was that observed frequencies would approximate that probability to any desired degree of precision. What was required was the inverse: Given the observed frequency, how likely is it to approximate the unknown probability? Or, as the problem was more often posed, given that an event has occurred so many times before, what is the probability that it will occur again on the next trial? In short, what is the probability that the future will be like the past?

These so-called inverse probabilities became the core of the probability of causes. Thomas Bayes and Pierre Simon Laplace independently proved

versions of the inverse of Bernoulli's theorem (Bayes, 1763; Laplace, 1774), whose applications remain controversial to this day (see 3.4). In modern notation, the theorem is usually stated as:

$$P(C \mid E) = P(C \cap E)/P(E)$$

or equivalently:

$$P(C \mid E) = P(C)\ P(E \mid C)/P(E)$$

In words: the probability of a cause C given an observed effect E (or of a hypothesis given certain data) equals the combined probability of C *and* E divided by the probability of E. In fact, the original statement of the original theorem was somewhat more complicated, because it involved a set of causes, $C_1, C_2, \ldots, C_i$. In order to apply the theorem, both Bayes and Laplace made the problematical assumption that in the absence of any information to the contrary, we may assume all competing causes C_i to be *a priori* equally likely, a so-called uniform prior probability distribution. Bayes appears to have been troubled by the assumption, perhaps even to the point of not publishing his results (it was his literary executor Richard Price who submitted Bayes' essay to the Royal Society of London), and he resorted to an elaborate physical analogy of a ball tossed at hazard onto a flat table top to justify his calculations. Just as the ball seemed equally likely to land anywhere on the table, so "in the case of an event concerning the probability of which we absolutely know nothing antecedently to making any trials concerning it, I have no reason to think that, in a certain number of trials, it should rather happen any one possible number of times than another" (Bayes, 1763, p. 143).

Laplace was more nonchalant in making the same assumption that ignorance could be converted into a uniform distribution of prior probabilities, offering no justification whatsoever (Stigler, 1986, p. 103). His notorious law of succession then follows from the inverse theorem, leading to calculations that the probability that an event once observed – Adam's first sunrise in the garden of Eden was a favorite example – would happen the next time was two-thirds. In the hands of Laplace and his followers, Bernoulli's theorem was a mathematical model of causation, particularly useful for detecting the existence of "weak" causes like animal magnetism, while the inverse theorem was a mathematical model of the scientific method itself, of evaluating the status of hypotheses like the preponderance of male to female births in light of new data. As ever in the classical interpretation, the beliefs of the reasonable man, in this case those reasonable men known as natural scientists, were the compass by which the probabilists steered. As late as 1837 Poisson was adjusting the probability of causes to take account of the fact that only one well-performed experiment suffices to convince scientists that phenomena like the polarization of light were real (Poisson, 1837, p. 165). Even in its applications to the natural sciences classical probability theory was meant to capture, not correct, these reasonable intuitions.

1.8 THE MORAL SCIENCES

However, it was in the moral sciences of the Enlightenment that the reasonable man of classical probability theory was most in evidence. For this reason the probabilists tried long and hard to make theirs the calculus of the moral sciences, a "social mathematics," in Condorcet's phrase (Baker, 1975). Jakob Bernoulli pioneered this vision in part IV of his *Ars conjectandi*, meant to set forth the application of the mathematical art of conjecture to "civil life," including the probability of evidence and his theorem linking statistical frequencies to degrees of certainty. The fact that part IV was never completed stands as a symbol of the unfulfilled ambitions of the classical probabilists in this domain. Their nineteenth-century successors ridiculed the program as an amalgam of the impracticable and the presumptuous, a slur upon the good name of mathematics. But to the classical probabilists nothing seemed more obvious than that their calculus should be applied to jurisprudence, political economy, and other parts of the moral sciences.

In order to understand their confidence, we must first understand the assumptions and aims of the Enlightenment moral sciences, and how these harmonized with those of classical probability theory. In contrast to the social sciences of the nineteenth century, the students of the moral sciences took the individual rather than society as their unit of analysis. Insofar as they dealt with society at large, they conceived of it as an aggregate of such individuals. Moreover, the regularities that the moral sciences sought to uncover were the result of rational decisions made by these individuals rather than of the overarching structures of culture and society. Reasoning individuals were in this sense the cause of social regularities; social order flowed from orderly individuals. Although the moral scientists greatly admired the successes of Newtonian mechanics, and aped its language of natural laws, they constantly harangued their fellow citizens to obey these laws. No physicist wasted his breath trying to persuade people to obey the law of gravitation; this was a matter of physical necessity, not free choice. But the moral sciences were well named, for here the necessity was indeed moral. Self-interest might make following the laws of the moral realm rational, yet human beings were free, if only to err. Arguments in the moral sciences often resembled those in classical probability theory: both were at bottom attempts to convince the recalcitrant – gamblers, judges, insurers, monarchs – that it was in their own best interest to mend their ways. Like classical probability theory, the moral sciences were both descriptive and prescriptive. On the one hand, they claimed to reveal the immutable order of human thought and action; on the other, they urged changes in existing social arrangements to better approximate this order.

The kind of reason postulated by the moral sciences also meshed well with classical probability theory, for it was reason conceived as implicit calculation and even as the capacity to combine and permute ideas. These mental operations were for the most part too swift and subtle to be conscious, and they were occasionally overtaxed by the complexity of the problem at hand. Hence the need for the moral sciences, with the aid of probability theory, to render them explicit. If sound reason were simply implicit reckoning, then it was not absurd to try to model it mathematically. Here the moral scientists gave the classical probabilists a free hand, all the more so because they depended mainly on persuasion rather than discovery to realize the "laws" of the moral realm. These laws exacted obedience in the sense that mathematical demonstration coerced assent, through an appeal to reason, and the authority of mathematics was a most welcome support to the arguments of the moral sciences.

The probabilists entered the moral sciences through jurisprudence, for reasons having to do with the history of the calculus itself and with the political climate of the time. Judicial reform was a campaign of the French philosophers, fueled by Voltaire's denunciations of the infamous trials of Jean Calas and the Chevalier de Barre, and by Cesare Beccaria's influential *Dei delitti e delle penne* (1765). It became an urgent political reality in the succession of French regimes between the outbreak of the Revolution in 1789 and the July Monarchy of 1830. The probability of judgments, invented by Condorcet in his *Essai sur l'application de l'analyse à la probabilité des décisions rendues à la pluralité des voix* (1785), was a mathematical attempt to redesign judicial tribunals, and conceptually much indebted to Beccaria's idea of summed fractions of individual liberty balanced against societal authority. Condorcet equated this fraction with the maximum risk an individual citizen might run of being wrongly convicted of a crime, and set it equal to a risk that anyone would take without a second thought – say, a ride on the Calais/Dover packet boat, the eighteenth-century equivalent of the New York/Washington shuttle. The *Essai* purported to quantify this maximum acceptable error in judicial proceedings, and to calculate the number of judges, their individual degree of "enlightenment" (*lumières*), and the minimum plurality required to guarantee that probability of safety (Condorcet, 1785, pp. lxxvii–ix).

As in the case of the probability of causes, far-reaching assumptions about the subject matter had to be made in order to launch the probability of judgments. Condorcet supposed that the individual probabilities of each judge were equal, and that their decisions were independent of one another. All subsequent contributions to the subject made similar assumptions to the effect that a verdict rendered by a tribunal composed of a given number of judges was mathematically equivalent to the same number of drawings from an urn containing a given proportion of balls marked "right" and "wrong." This made it possible to apply Bernoulli's and Bayes' theorems to the analysis, assimilating the probability of judgments to the probability of causes. However, other assumptions were more specific to the political predilections of the mathematicians. In Condorcet's hands the probability of judgments was a broadside for liberal reforms, including the abolition of the death penalty and the protection of individual liberties. Laplace took a more conservative view, emphasizing societal security over individual rights: each judge must assess not only the probability that the accused was guilty or innocent, but also the probability that the verdict served the interests of society. Poisson pushed the probability of judgments still further right, assuming an *a priori* probability of guilt of at least one-half. Indeed, he argued that the judges should contemplate whether public safety was better secured by a verdict of guilty or innocent rather than whether the accused was actually guilty or innocent. In the service of the moral sciences, mathematics itself took on a moral tinge (Daston, 1981).

1.9 CONCLUSION

By the time Poisson claimed to have demonstrated his conclusions with all the rigor of mathematics in 1837, the classical interpretation of probability was under attack on several fronts. Poisson's own results stirred up a storm of controversy among his colleagues, who sharply criticized the probability of judgments as "the real opprobrium of mathematics," as John Stuart Mill put it a few years later (Mill, [1843] 1881, p. 382). These critics also heaped scorn on the probabilities of testimony and of causes;

the one for attempting to quantify imponderables like veracity, and the other for substituting armchair algebra for honest empirical investigation. By 1840, the theory that had been touted as good sense reduced to a calculus struck many mathematicians and philosophers as an "aberration of the intellect." For the first time, mathematicians began to distinguish the theory of probability from its suspect applications.

The intellectual and social context which had made the classical interpretation and its characteristic applications conceivable dissolved in the early decades of the nineteenth century. The French Revolution and the social tensions that followed it shook the confidence of the probabilists in the existence of a single, shared standard of reasonableness in a way that decades of controversy over the proper definition of expectation had not. The reasonable man fragmented and then disappeared altogether, along with the consensus of the intellectual and political elites he was supposed to embody. In the first flush of romanticism, reason itself ceased to be a matter of implicit calculation, and was instead identified with unanalyzable intuitions and sensibility. The classical interpretation had lost its subject matter.

It had also lost its justification for amalgamating objective and subjective probabilities. In the late eighteenth century the associationist psychology that had initially joined the two sides of probability, frequencies and degrees of certainty, shifted its emphasis from accurate tallies to distortions and illusions. Hume's associationism had apportioned belief to experience on a probabilistic scale, but Condillac's enslaved belief to fantasy and desire (Condillac, [1754] 1798, vol. 3, p. 95n). Subjective belief and objective frequencies began as equivalents and ended as diametric opposites. When Laplace discussed how the association of ideas influenced the estimation of probabilities in his *Essai philosophique sur les probabilités* (1814), it was under the heading of "illusions" (Laplace, 1951, ch. 16). Once the psychological bonds dissolved between objective and subjective probabilties, and between the calculus of probabilities and good sense, the classical interpretation came to seem both dangerously subjective and distinctly unreasonable.

Poisson was the first to distinguish clearly between subjective and objective senses of probability in print, in 1837 (Poisson, 1837, p. 3), and in the next decade probabilists on both sides of the Channel forged a new interpretation of probabilities as observed frequencies. The statistics amassed in government offices all over Europe during this period provided a prominent model for the new interpretation. In the mouths of the frequentists "subjective" became an epithet, and they were unrelenting in their criticisms of applications that equated "equally undecided" with "equally possible," as in many classical applications of Bayes' theorem. The applications that survived this transition were those compatible with a purely objective interpretation of probability: gambling and actuarial problems, and the method of least squares (Stigler, 1986; see 3.3). In contrast, the probabilities of evidence, judgment, and causes were discredited for what now looked like bizarre assumptions and oversimplifications of their subject matter. The rise and fall of the classical interpretation of probability shows that quantification is not irreversible, and that mathematical theories can lose as well as gain domains of application.

A handful of prominent mathematicians, most notably Augustus De Morgan and W. S. Jevons in England, upheld one or another variant of the classical interpretation of probability during the middle decades of the nineteenth century, but they were an embattled minority. By then much of the classical program was the object of ridicule among philosophers and mathematicians, including John Stuart Mill, George Boole, and Joseph Bertrand. The background of assumptions about the world and the mind that had made sense of the program had eroded, taking the reasonable man of classical probability theory and most of what he represented with them. Probabilists turned from the rationality of the few to the irrationality of the many.

But the age of chivalry is gone – That of sophisters, economists, and calculators, has succeeded; and the glory of Europe is extinguished for ever.

Edmund Burke (1790)

2

Statistical probabilities, 1820–1900

2.1 INTRODUCTION

By about 1830, *l'homme éclairé* had given way to *l'homme moyen*. The same awareness of a dynamic and perhaps unstable mass society that superannuated the reasonable man as the most characteristic object of probability theory simultaneously brought into existence a new one. There was, to be sure, some continuity. The application of probability to error theory, insurance, and gambling problems survived unscathed. More abstractly, what could be subjected to mathematics was yet assumed to have a certain latent rationality, notwithstanding the dispiriting unruliness of manifest events. The allure of aggregate figures for many social thinkers lay precisely in their insensitivity to political and economic crises. The reasonable man might still exist, but he did not and could not control public life. Statistics was valued as a way of searching for the larger order that, it was hoped, would prevail nonetheless. Statisticians exulted in their ability to find such an order, for chance disappeared in large numbers, and with it the discontinuities of revolution – "always the most precarious of all games of chance," in Gustav Schmoller's words (quoted in Semmel, 1968, p. 197). The main object of probabilistic analysis became mean values. Its most important and conspicuous field of application was now statistics.

Social numbers, of course, were not invented in the nineteenth century, and the application of probability to them was almost as old as mathematical probability itself. Ironically, the great improvement in accuracy of demographic, economic, anthropometric, and social records early in the