Lorraine J. Daston

Fitting Numbers to the World: The Case of Probability Theory

By almost all current philosophical accounts, the success of applied mathematics is a perpetual miracle. Neither formalist nor logicist nor Platonist (at least of the latter-day dilute variety) can provide a plausible explanation of why numbers should fit the world. (Here I use *numbers* as a convenient shorthand for all elements of mathematics, since most—but not all—examples of applied mathematics involve numerical measurements.) Why should the relationships among undefined elements or logic or purely mental objects describe a multitude of phenomena with tolerable accuracy? Short of invoking preestablished harmony or a coincidence of mind-boggling improbability, we are philosophically at a loss to account for our great—and ever greater—good fortune in applied mathematics. We believe that pure mathematics is conceptually and for the most part historically prior to and independent of applied mathematics. Indeed, the very term *applied mathematics* tells all: in order to be applied, the mathematics must already exist in its own right, just as theory is "applied" to practice.

The situation was quite otherwise in the eighteenth century, which practiced "mixed" rather than "applied" mathematics. Enlightenment philosophers of mathematics certainly had their share of problems, but they were not our problems. They pondered, for example, why mathematics should enjoy greater certainty than various parts of physics and astronomy, but the triumphs of mixed mathematics, many of them barely fifty years old then, did not perplex them. An odd hybrid of Aristotelian, Cartesian, and Lockean views about the nature of mathematics had prepared them for such happy outcomes in astronomy, mechanics, optics, and acoustics, and they anticipated further mathematical breakthroughs in areas like pneumatics and what was then known as the art of conjecture, or the calculus of probabilities.

In this paper, I shall try to do three things: first, to briefly sketch the eighteenth-century distinction between "abstract" and "mixed" mathematics, and to contrast it with our "pure" versus "applied" distinction; second, to illustrate the significance of this philosophical distinction for mathematical practice, using examples drawn from the history of probability theory during the eighteenth and early nineteenth centuries; and third, to conclude with some reflections suggested by these examples about the preconditions for applying mathematics. Throughout, I shall be primarily concerned with the problem of how a mathematical theory gains—and sometimes loses—a domain of applications.

1. Mixed Mathematics

In the *Metaphysics*, Aristotle argues that mathematical entities cannot be truly separated from sensible things, but only abstracted from them (M.2.1077a 9-20; b 12-30). With some Lockean and Cartesian elaborations, this view continued to dominate philosophical accounts of mathematics throughout the eighteenth century. The most influential of these accounts was d'Alembert's *Preliminary Discourse* (1751) to the great *Encyclopédie*. Following Locke, d'Alembert asserts that all human knowledge derives from experience; borrowing from Descartes, he assumes analysis to be the fundamental intellectual operation turned upon the raw materials of sensation. Property by property, the mind strips away the tangle of particular features that compose any sensation until it arrives at the barest skeleton or "phantom" of the object, shaped extension. It is at this rarefied level that mathematics studies the objects of experience. Although these objects have been systematically denuded of all those traits that normally accompany them in perception, they are not, d'Alembert insists, thereby denatured. Mathematics may be the "farthest outpost to which the contemplation of properties can lead us," but it is nonetheless still anchored in the material universe of experience.

Once the limits of analysis have been reached at magnitude and extension, the mind reverses its path and begins to reconstitute perception, property by property, by the reciprocal operation of synthesis, until it ultimately arrives at its departure point, the concrete experience itself. The successive stages along this route demarcate the subject matter of the various sciences. For example, add impenetrability and motion to the magnitude and extension of mathematics, and the science of mechanics is created. All sciences study the same objects but embrace their perceptual complexity to a greater or lesser extent.[1]

That mathematics could describe the external world would thus have hardly puzzled d'Alembert: mathematics came from the world, and to the world it must return, for d'Alembert believed that mathematical abstractions, as he says, "are useful only insofar as we do not limit ourselves to them." Even the "abstract" mathematics of pure number and extension (i.e., of arithmetic and geometry) were simply the endpoints of a continuum along which mathematics was "mixed" with sensible properties in varying proportions. Of course, this schema brought its own quandaries: if mathematics was joined to the natural sciences by a continuous intellectual process, why did only mathematical results enjoy certainty, and exactly where did the boundary between mathematics and these sciences lie? D'Alembert was obliged to resort to Cartesian intuitions of clarity and simplicity to answer the first question, and he tendentiously drew the boundary to include rational mechanics within mathematics—even his devoted admirer Montucla could not follow him in that.[2] But d'Alembert's account of "abstract" and "mixed" mathematics *did* explain how, for example, geometric optics or celestial mechanics was possible, and it was echoed by many other in the next fifty years.

So far, so Aristotelian. We recognize the kinship of Aristotle's bronze isosceles triangle and d'Alembert's impenetrable moving body: both examples "mix" physical with abstract properties. But when we turn to d'Alembert's table depicting the actual contents of mixed mathematics, we see how much enlarged beyond the Aristotelian canon it had become by 1750 in the mind of one of Europe's leading mathematicians.[3] "Mixed" or "Physico-Mathematics" dwarfs "Pure Mathematics," even though the latter category has been swelled by the integral and differential calculus, and each of the headings of the Aristotelian "mixed" canon—mechanics, astronomy, optics, harmonics—has greatly expanded its range of subheadings. (Harmonics, for example, now comprises only one part of "Acoustics," and optics only one part of a broader division including dioptics, perspective, and catoptrics.) Moreover, the division of space in d'Alembert's table corresponds roughly to the division of labor among eighteenth-century mathematicians: Bossut's 1810 survey of mathematics from the origins of the calculus to the year 1800 devotes 177 pages to work in abstract mathematics and 319 pages to that in mixed mathematics.[4]

Not only did mixed mathematics preponderate; in the opinion of eighteenth-century mathematicians, it bid fair to expand still further. Montucla, in the 1758 edition of his celebrated *Histoire des mathématiques*, reflected on the open-ended nature of mixed mathematics, which grew apace as the natural sciences acquired greater stores of observations that could serve as "principles" of new branches such as pneumatics (the mathematical study of gases) and the art of conjecture (probability theory). Indeed, there seemed no more auspicious augury for the future of mixed mathematics than a calculus of chance, "this kind of Proteus, so difficult to get firm hold of, [and yet] the mathematician is on the verge of chaining it and of submitting it to his calculations."[5] If chance itself could be tamed, what phenomenon, however irregular, could withstand the forward march of mixed mathematics?

This, then, was the framework within which eighteenth- and early nineteenth-century probabilists worked. Mathematics about the world was "mixed" with its subject matter rather than "applied" to it, and pure or abstract mathematics was thought as likely to be enriched by physical insights as the other way around—the intimate, two-way links between the development of the calculus and the study of motion were an object lesson to eighteenth-century mathematicians. Mixed mathematics was more than just a *façon de parler*: all attempts to apply mathematics to the phenomena of experience presume some degree of analogy between the subject matter and the mathematics employed, but for eighteenth-century practitioners of mixed mathematics, the requisite degree of analogy bordered on congruence. Mathematical models were conceived not merely as analogues that shared certain key features with the phenomena they described, but rather as mathematical "portraits"—highly schematic ones, to be sure—of the phenomena and/or the underlying mechanisms that produced them. Moreover, mixed mathematics absorbed the greater part of mathematicians' energies in the eighteenth century. However, they revered pure mathematics as the only inexorably progressive part of the discipline. As Montucla noted, the very hybrid nature of mixed mathematics, partaking both of the "clarity and evidence" of abstract mathematics and the "uncertainty and shadows" of physics, put it at risk of error and (Montucla's term) "retrogression."[6] I shall now turn to the calculus of probabilities, which was at once emblematic for the distinctive character of mixed mathematics, of its vast horizon, and, ultimately, of the retrogression Montucla feared.

2. The Art of Conjecture

Probability theory has been described as simply the sum total of its applications. Until the late eighteenth century introduction of generating functions, it had no techniques of its own; until Kolmogorov's axiomatization of 1933, it had no independent standing as a mathematical theory. Early twentieth century mathematicians like Hilbert and Borel still narrowly identified probability theory with specific applications, and this was a fortiori true of the eighteenth century art of conjecture, which existed solely as mixed mathematics, like geometric optics and celestial mechanics. As such, it stood or fell with its treatment of the problems deemed specific to it—problems concerning rational belief or action under conditions of uncertainty—hence the "art of conjecture," after Jakob Bernoulli's 1713 treatise of that title. I shall outline three examples from the eighteenth-century corpus of probability theory: the St. Petersburg gambling problem; the probability of judgments; and insurance. I have deliberately chosen examples that are both familiar (insurance) and, to twentieth-century eyes, outlandish (the probability of judgments); examples that are still central to the probabilistic corpus (gambling and expectation) and others that have migrated to other disciplines (the St. Petersburg paradox and its solutions now belong properly to economics). Taken together, the examples illustrate the special constraints placed upon mixed mathematics to somehow "match" the phenomena, and also how the empire of numbers could contract as well as expand.

The St. Petersburg Problem

Although most modern expositions derive the concept of expectation from that of probability, the first published versions of the theory of the late seventeenth and early eighteenth centuries made expectation rather than probability fundamental.[7] In modern terms, expectation is defined as the product of the probability of an event m and its outcome value:

$$E = P(m) \cdot V(m), \quad \text{Where } P(m) = \text{probability of outcome } m \text{ and } V(m) = \text{value of outcome } m.$$

Early probabilists like Huygens and Jakob Bernoulli thought in terms of expectations rather than probabilities because the problems they posed were taken from contract law, and were concerned more with equity than with probabilities per se. Aleatory contracts were a long-recognized subdivision of Roman and canon law, and included all agreements involving an element of chance: gambling, annuities, maritime insurance, the prior purchase of the "cast of a net" from a fisherman—in short, any trade of here and present goods for future uncertain ones. Lawyers studied conditions of equal expectations rather than equal probabilities of gain or loss, and the first two generations of mathematical probabilists followed suit when they attempted to quantify these legal concerns.

The St. Petersburg paradox, posed by Nicholas Bernoulli in 1713,[8] sparked a long debate over the proper definition of the central concept of probabilistic expectation—a debate that threatened to undermine the foundations of eighteenth-century probability theory. Two players, *A* and *B*, play a coin-toss game. If the coin turns up heads on the first toss, *A* gives *B* \$1; if heads does not turn up until the second toss, *B* wins \$2; if not until the third toss; \$4; and so on. By conventional methods, *B*'s expectation (and therefore the fair price to play the game) would be:

$$E = 1/2\ (\$1) + 1/4\ (\$2) + 1/8\ (\$4) + \ldots + (1/2^n)\ (2^{n-1}) + \ldots .$$

Therefore, *B* must pay *A* an infinite amount to play the game. But, as Nicholas Bernoulli and subsequent commentators were quick to point out, no reasonable person would pay even a small amount, much less a very large or infinite sum, for the privilege of playing such a game.

Why was this a paradox? Unlike most mathematical paradoxes, the contradiction lay not between discrepant mathematical results deduced from equally valid premises: there is nothing mathematically wrong with this answer. In order to understand the furor triggered by the St. Petersburg problem, we must return to eighteenth-century views on mixed mathematics and the peculiar mission of the calculus of probabilities. In essence, the branches of eighteenth-century mixed mathematics corresponded to what would now be termed mathematical models. If the mathematical description diverged significantly from the phenomena, it was incumbent upon the mixed mathematician to revise this theory. In other words, mathematical probability was as "corrigible" as the mathematical theory of lunar motion. In modern parlance, the mathematical theory had no existence independent of its applications. Thus the designated field of applications (the term is anachronistic here) played a critical role in the career of a branch of mixed mathematics, and such indeed was the case with mathematical probability. The distinctive field of applications to which classical probabilists attached their theory was the determination of standards for rational thought and conduct in civil society, a field pioneered by Jakob Bernoulli in the unfinished fourth book of the *Ars conjectandi* and taken up by his nephews Nicholas and Daniel Bernoulli, Condorcet, Lambert, Laplace, and others during the eighteenth century. By isolating and mathematizing the principles that underpinned the beliefs and actions of an elite of reasonable men, the probabilists hoped to make social rationality accessible to all. Since probability theory was meant to be a mathematical model of reasonableness, when its results clashed with the judgments of *hommes éclairés*, as in the case of the St. Petersburg problem, probabilists anxiously reexamined their premises and demonstrations for inconsistencies. Hence the paradox: the contradiction lay not between incompatible mathematical tenets, but rather between the unambiguous mathematical solution and good sense. This is why the St. Petersburg paradox, trivial in itself, exercised every first-rate probabilist from Daniel Bernoulli through Poisson.

I have discussed the numerous eighteenth-century solutions to this so-called paradox at length elsewhere, and I shall not review them here.[9] Here, I shall only point out that although mathematicians reached no consensus about the correct solution to the problem, they all agreed that the paradox was both real and dangerous to probability theory as a whole, that the definition of expectation was the nub of the problem, and that a satisfactory solution must realign the mathematical theory with the opinions of reasonable men. That is, their primary loyalty lay to the field of phenomena that the calculus of probabilities purported to describe, and they were willing to sacrifice the most fundamental definition of that calculus in order to bring about a better match between mathematical results and phenomena.

The Probability of Judgments

The preceding example of the St. Petersburg paradox and the animated debate it provoked was meant to show that (1) the eighteenth-century calculus of probabilities was, as a branch of mixed mathematics, inseparable from its designated field of applications; and (2) that field of applications was broadly conceived as rational decision-making under uncertainty—be it to buy a lottery ticket, sell an annuity, believe a witness, or accept a scientific hypothesis. For eighteenth- and early nineteenth-century practitioners, probability theory was, in Laplace's famous phrase, "nothing more than good sense reduced to a calculus."[10] We have seen how the failure of mathematical results to tally with good sense prompted a searching reevaluation of even the most fundamental mathematical premises—squarely in the descriptive tradition of mixed mathematics. My second example, the probability of judgments, illustrates just how the calculus of probabilities was made to seem "congruent" to good sense, and how rapidly shifting conceptions of that "good sense" eventually dissolved the bond between the calculus of probabilities and the phenomena it was originally intended to describe. It gained a measure of independence thereby, as an autonomous mathematical theory, but it also lost an entire field of applications.

The probability of judgments emerged in the late eighteenth and early nineteenth centuries largely through the work of Condorcet, Laplace, and Poisson.[11] The central problem was the optimal design of a jury or tribunal of judges so that, by adjusting the number of judges and the plurality required for conviction, one could minimize the probability of an erroneous decision. The mathematicians adopted both the mathematical format of the probability of causes based on Bernoulli's and Bayes's theorems, and the concomitant assumptions concerning the uniformity and independence of trials in their treatments of the probability of judgments. (Recall that Bernoulli's and Bayes's theorems are the inverse of one another: roughly speaking, Bernoulli's theorem tells you how to find the probability that *n* drawings from an urn with replacement will closely approximate the known ratio of black-to-white balls contained in the urn; Bayes's theorem tells you the probability that the unknown ratio of black-to-white balls will closely approximate the known results of *n* drawings with replacement.) In the probability of judgments, each judge or juror was likened to an urn containing so many balls marked "true," corresponding to a correct decision, and the rest marked "false," to denote an incorrect decision. The decisions of the judges were further assumed to be independent of one another, just as drawings from separate urns would be. Since the individual "truth" probabilities of the judges could not be ascertained a priori, the probabilists resorted to inverse probabilities, and, after the publication of French judicial statistics starting in 1825, to assumptions that the guilt or innocence of the defendants operated as an unknown cause of the observed acquittal and conviction rates, and that a certain prior probability of guilt obtained.

Why did late eighteenth- and early nineteenth-century probabilists accept the verisimilitude of this probabilistic model for judgment? Later critics such as the mathematician Louis Poinsot and the philosopher John Stuart Mill found it all but incomprehensible that thinkers of Laplace's stature could have compared judges to, in Poinsot's words, "so many dice, each of which has several sides, some for error, others for truth."[12] The nineteenth-century probabilist Joseph Bertrand objected that decision-making was intrinsically particular, governed by determinate but fluctuating factors: if a judge erred, it was for a specific reason, not because he had "put his hand in an urn" and made an unlucky draw.[13] Yet for the classical probabilists, these assumptions did not seem so outrageous. Condorcet and Laplace admitted that the conditions of independence, equality, and constancy for individual probabilities were simplifications, but they argued that all mixed mathematics involved idealizations (witness Newton's laws of dynamics), and further maintained that such approximations "founded on the data, indicated by good sense" were preferable to nonmathematical specious reasoning.

The eighteenth- and early nineteenth-century probabilists could be confident in assumptions that their successors found absurd because they subscribed to psychological theories that described mental operations in terms congenial to their mathematics. According to Locke, Hartley, Hume, and others, the sound mind reasoned by the implicit computation and comparison of probabilities.[14] Or as Montucla put it apropos of crack gamblers:

> The gambler's mind ["l'esprit du jeu"], that mind that seems to capture fortune, is nothing more than an innate or acquired talent for seeing at a glance all the chance combinations that could lead to gain or loss; human prudence is ultimately nothing other than the art of appreciating the probabilities of events, in order to act accordingly.[15]

The association of ideas in principle mirrored the regularity and frequency of events culled from experience: in an unbiased mind, associations of ideas corresponded to real connections between the events and objects represented by the ideas. The very workings of the human understanding, when undistorted by strong emotion or uncritical custom, imitated Bernoulli's theorem, which Hartley had claimed was "evident to attentive Persons, in a gross general way, from the Common Methods of Reasoning."[16] Associationist psychology also emphasized the combinatorial operations of the mind; indeed, all intellectual novelty owed to the mental combination and recombination of simple ideas by, as Condillac wrote, "a kind of calculus." Condorcet affirmed Condillac's claim that the best intellects were those that excelled in "uniting more ideas in memory and in multiplying these combinations."[17] If good minds worked by "a kind of calculus," then the combinatorial calculus of probabilities could be viewed as the mathematical expression and extension of the psychological processes that constituted right reasoning—in particular, the right reasoning of judges.

Eighteenth-century jurisprudence also supplied the probabilists with suggestive analogies to their mathematical techniques and concepts, thereby inviting new applications in that field. Continental jurists had developed an elaborate hierarchy of so-called legal proofs that assigned the evidence procured from both witnesses and things a fixed fractional value. These fractional "probabilities" (a usage that antedates the mathematical term) corresponded to degrees of assent in the mind of the judge and were summed to obtain the complete or "full" proof required for conviction.[18] Classical probabilists like Jakob Bernoulli, Condorcet, and Laplace adopted the legal interpretation of probability as a "degree of certainty" apportioned to the probative force of various types of evidence, and they also attempted to convert the legal "probabilities" into a mathematical probability of testimony and conjecture.

Hence there would have been no a priori objections to the probability of judgments, which computed the probability that a tribunal composed of a given number of members, each with a postulated probability of judging aright, would arrive at a correct decision by a certain majority, as an inappropriate application of mathematical probability. Despite the later criticisms of the probability of judgments as the "scandal of mathematics," classical probabilists viewed the theory as reasonable and indeed quite useful, given the urgent interest in judicial reform at the time. Their optimism stemmed in large part from the "congruence" between the phenomena of right reasoning, particularly the weighing of legal evidence, and the mathematics itself, a congruence prepared by contemporary psychology and jurisprudence. Geometric optics "fit" the phenomena of reflection and refraction because light rays were indeed like geometric lines; the art of conjecture "fit" the phenomena of rational decision-making because enlightened minds intuitively calculated probabilities. The probabilists' job was to make the tacit principles underlying good sense into an explicit calculus accessible to all.

Alas for classical probability theory, neither the phenomena of good sense nor the psychological and legal theories that linked them to mathematical probability survived the French Revolution unaltered. Both psychology and jurisprudence had changed considerably between the publication of Condorcet's and Poisson's treatises on the probability of judgments in 1785 and 1837, respectively. Although associationist theories were still influential, they emphasized the pathologies of reason created by habit, prejudice, self-interest, and ignorance rather than the smoothly functioning mental calculus of eighteenth-century psychologists. Good sense was no longer so clearly identified with computation and comparison of probabilities. In jurisprudence, the deliberately anti-formal, anti-analytic system of "free" proofs had replaced that of legal proofs, substituting an intuitive appeal to the "intimate conviction" of juror or judge for any formal reckoning.[19] Poinsot's objections to Poisson's work on the probability of judgments reveal how far the notion of good sense had diverged from the mental calculus of the associationists, and how completely the new free system of proofs had severed the older connection between legal and mathematical probabilities:

> It is the application of this calculus [of probabilities] to things of the moral order which offends the intellect . . . at the end of such calculations in which the numbers derive only from such hypotheses, to draw conclusions, which purport to guide a sensible man in his judgment of a criminal case . . . this is what seems to me a sort of aberration of the intellect, a false application of science, which it is only proper to discredit.[20]

Thus did probability theory lose, at least temporarily, one of its key domains of applications. Poinsot's verdict (so to speak) on the probability of judgments as an "aberration of the intellect" stuck. However, in detaching the probability of judgments from the corpus of respectable applications of mathematical probability, Poinsot was at pains to make clear that his attack on the probability of judgments in no way touched the certainty of the *theory* of mathematical probability, which he deemed to be as irrefragable as arithmetic. This careful distinction between theory and applications would have been foreign to mixed mathematics, and it shows that the opposition of "abstract" to "mixed" mathematics had been superseded by that of "pure" to "applied" mathematics by the time Poinsot took the floor of the Paris Académie des Sciences in 1835 to attack Poisson's work. The story of the ill-fated probability of judgments might serve as an object lesson in the need to exercise caution in the choice of a suitable set of phenomena to mathematize. The "good sense" of reasonable men turned out to be notoriously unstable, as the probabilists bent on mathematically describing it discovered to their chagrin. Faced with such a "retrogression," in Montucla's sense, mathematicians and philosophers protected the reputation of mathematics for certainty by sharply distinguishing untarnished pure mathematics from its sometimes dubious applications.

Insurance

My final example, actuarial mathematics, is by way of contrast an undisputed success story about applying probability theory to the world—or at least it became one after almost a century's worth of neglected mathematical efforts to make it so. The history of how mathematical probability eventually came to be applied to the insurance trade is an intricate one that I can barely sketch here.[21] My main point in doing so will be to make the converse point to my claim concerning mixed mathematics and the probability of judgments. If the probability of judgments enjoyed a bright if brief career, it was because peculiar circumstances conjoined first to suggest a striking analogy between legal judgments and mathematical probability, and later to destroy this analogy. Conversely, if eighteenth-century insurers were slow to make use of a mathematical technology that was in many ways tailor-made for them, it was because they perceived the conditions of their trade as downright disanalogous to those imposed by the mathematicians.

Why did the practitioners of risk—particularly insurers and sellers of annuities—fail to take advantage of mathematical techniques and collections of statistics that were devised for their purposes? There seems to have been, as the mystery writers put it, both opportunity and motive. Opportunity, for there existed a well-worked-out theory for pricing annuities and (potentially) life insurance using the new mathematics of probability and the new data of mortality statistics from circa 1700 on. Moreover, the mathematicians (De Moivre, Simpson, and others) had expressly translated this literature into elementary handbooks aimed at the innumerate clerk, with all algebra converted to verbal form and most technical material relegated to appendices, plus copious tables to obviate the need for onerous calculation. Motive, for both the Dutch and especially the English annuity and insurance markets were large and bustling enough by the turn of the eighteenth century to offer the mathematically based company a competitive edge. (This is not to say that such enterprises could not be profitable without mathematics—indeed, they were *too* profitable—but as the experience of the first mathematically based company was to show, even the loosest connection between calculation and premium could cut prices while still preserving a lavish margin of profit.)

Why, then, did the practice of risk taking lag so far behind the theory of risk taking? I shall argue, in summary form, that mathematically based insurance could not be sold until insurers and their customers came to believe that phenomena like human mortality, shipwrecks, fires, and other catastrophes were regular enough to make statistical and probabilistic approaches plausible. To make this point clear, I must briefly describe the practice of insurance *without* actuarial mathematics. Certain forms of risk taking—insurance (chiefly maritime), annuities, and gambling—were widely and successfully practiced in Europe long before the formulation of mathematical probability. Although experience no doubt honed the ability of the underwriter, dealer in annuities, or gambler to estimate odds, their approach to risk could hardly be described as statistical or probabilistic, even at an intuitive level. A sixteenth-century insurer might have found such a statistical approach impractical, for it assumes conditions that are stable over a long period as well as the homogeneity of categories. Insurance manuals and legal treatises of the period emphasized that the premium in any given case depends on a judicious weighting of the particular circumstances: the cargo, the season of the year, the route taken, the condition of the ship, the skill of the captain, the latest "good or bad news" concerning storms, warships, and privateers.[22] Moreover, in commercial centers populous enough to support whole markets of insurers, premium prices also reacted to levels of supply and demand as well as to the latest news about the Barbary pirates.

Thus, annuity rates and insurance premiums certainly reflected past experience, but it was a far more nuanced experience than a simple toting up of mortality and shipwreck statistics. It was an experience sensitive to myriad individual circumstances and their weighted interrelationships, not to mention market pressures: it was not simply astatistical, it was antistatistical. Given the highly volatile conditions of both sea traffic and health in centuries notorious for warfare, plagues, and other unpredictable misfortunes, I am not persuaded that this was an unreasonable approach. In any case, it was the prevailing one—and it evidently turned a profit.

Although mathematicians and statisticians addressed insurance and annuities problems almost from the inception of mathematical probability in the late seventeenth century—Huygens, DeWitt, Leibniz, the Bernoullis, Halley, and De Moivre were all interested—and although by 1750 there existed an extensive literature in Dutch, English, Latin, and French on the subject, the impact of mathematical probability on practice was effectively nil prior to the establishment of the Equitable Society for the Assurance of Lives in 1762 (at the instance of a mathematician, James Dodson).[23] And even then, the dictates of mathematical theory were greatly tempered by other considerations. The Royal Exchange and London Assurance offices (established in 1720), both of which insured lives, charged a flat rate of 5 percent for every £100 insured, regardless of age.[24] The vast bulk of insurance trade remained maritime, and although premiums responded to decreases in risk (the disappearance of marauding Turks made insuring voyages to the Levant, Spain, and Portugal considerably cheaper), statistics played no role in pricing. Fire insurance was too new to be burdened with the weight of tradition, and clients were offered graduated premiums depending on the kind of building (brick versus wood) and trade housed therein (sugar bakers, for example, paid especially stiff rates). Yet fire offices apparently never collected statistics on the subject.[25]

Why was the practice of eighteenth-century insurance and annuities so resistant to the influence of mathematical theory? For maritime insurance and annuities, it might be argued that the inertia of an entrenched and successful practice based on nonmathematical estimates worked against the application of the new mathematical statistical methods. This is no doubt part of the answer, but it cannot explain why the new forms of insurance against fire and death, both inventions of the late seventeenth and early eighteenth centuries, did not take on a more statistical cast. The case of life insurance and annuities is particularly baffling because of the availability of mortality statistics drawn from several locales. The first mathematically based life insurance company, the Equitable, was founded by a mathematician, not an insurer, and was twice denied a Royal charter because, in the words of the Royal Council (1761), the company's proposed mathematical basis,

> Whereby the chance of mortality is attempted to be reduced to a certain standard . . . is a mere speculation, never yet tried in practice, and consequently subject, like all other experiments, to various chances in the execution. . . .[26]

And even the Equitable does not seem to have been entirely persuaded of the reliability of the new mathematical methods, for it was cautious in every respect: it calculated interest on investments at the lowest rates (3 percent); used the mortality table that gave the shortest lifespans; took the further precaution of insuring only healthy lives; added a flat 6 percent to all premiums, and surcharges of up to 22 percent in individual cases deemed especially risky. Nor could the company be persuaded to distribute dividends among its members, despite its almost embarrassing prosperity and the ever more regular contours of the Equitable's own mortality figures as membership increased. Year after year, William Morgan, the first mathematically trained actuary, held importunate stockholders at bay, warning that "extraordinary events or a season of uncommon mortality" might catch the Equitable unawares,[27] law of large numbers or no. Small wonder that the nineteenth-century mathematician Augustus De Morgan quipped: "We should write upon the door of every mutual office but one be *wary*; but upon that one should be written *be not too wary* and over it the *Equitable Society*."[28]

Thus, the confidence that the phenomena of human mortality revealed, in the words of German theologian and demographer Johann Süssmilch, "a constant, general, great, complete, and beautiful order"[29] stable enough to rest a trade upon was slow in coming. For some phenomena, that faith came later than for others, and the disparities are hard to explain. As early as 1662 some writers were confident enough of the regularities to collect data on mortality, but it is difficult to understand why mortality should have been assumed to be regular and other phenomena of equal practical interest like the incidence of fires not, in an age where both were subject to wild fluctuations: witness the plague and Great Fire of London in 1665-66. Whatever the reasons for such confidence that whole new realms of phenomena were indeed regular, it was a prerequisite for the transformation of insurance from an enterprise based on judgments of individual cases to one based on probability and statistics. In contrast to the case of the probability of judgments, here a new analogy was forged rather than an old one sundered.

3. Conclusion

These three examples drawn from the history of the classical theory of probability illustrate three cardinal points about applying mathematics to phenomena: (1) whether applications succeed or fail depends on the standards set, and in this respect eighteenth-century mixed mathematics differed significantly from nineteenth-century applied mathematics; (2) mathematical theories can lose as well as gain applications; and (3) the conditions that make a set of phenomena ripe for quantification—or the reverse—depend crucially on their *perceived* stability and degree of analogy with the mathematical techniques at hand. The mixed mathematics of the eighteenth century demanded a far closer fit between mathematics and the phenomena than did the applied mathematics of the nineteenth century—for example, the psychological and legal theories underlying the probability of judgments. As with applied mathematics, the proof of the pudding was in the eating, i.e. whether or not the mathematical results tallied with experience, but the consequences of a mismatch were far more dire for a branch of mixed mathematics: the St. Petersburg paradox was grave enough evidence against the calculus of probabilities for its practitioners to revise its most fundamental definitions or even to suggest abandoning it entirely. These bonds between subject matter and mathematics loosened by the end of the eighteenth century in large part because many mathematical techniques originally conceived in a rather restricted applied context (e.g., partial derivatives and hydrodynamics) had since acquired so large and varied a repertoire of applications that close analogy no longer seemed of the essence. This is certainly what occurred in probability theory: consider the diaspora of the normal distribution from astronomy to sociology to physics to psychology in the course of the nineteenth century.

As mixed mathematics became applied mathematics, probability theory shifted its characteristic domain of applications. The St. Petersburg paradox, once at the very center of probabilistic research, became a problem in economic theory that happened to employ probabilistic techniques. The probability of judgments disappeared altogether, and actuarial mathematics expanded steadily. Although mathematicians and philosophers continued to wrangle over the proper interpretation of probability, the theory achieved a measure of independence from both applications and, ultimately, interpretations. And finally, the configuration of stable phenomena was transformed: in the eighteenth century, good sense seemed uniform enough to be quantified, but not the frequency of hailstorms; in the nineteenth century, just the reverse. In a curious way, the expanding and contracting domain of applications belonging to probability theory—indeed, to mathematics in general—charts for us the changing landscape of what we believe to be the regular, the predictable, and the stable, for only at these points do we believe that numbers can fit the world.

Notes

1. Jean d'Alembert, "Discours préliminaire," *Encyclopédie, ou Dictionnaire raisonné des sciences, des arts et métiers* (Paris, 1751-80), vol. 1, pp. v ff.
2. J. F. Montucla, *Histoire des mathématiques* (Paris, 1758), vol. 1, p. 5.
3. D'Alembert, "Discours," Système figuré des connaissances humaines.
4. Charles Bossut, *Histoire générale des mathématiques* (Paris, 1802-10), vol. 2.
5. Montucla, *Histoire*, vol. 1, p. 6; 2d ed. (Paris, An VII-X [1798-1802]), vol. 3, p. 381.

6. Montucla, *Histoire*, vol. 1, p. xxv.
7. See Christiaan Huygens, *De ratiociniis in ludo aleae* (1657), in *Oeuvres complètes de Christian Huygens*, Société Hollandaise des Sciences (The Hague: Martinus Nijhoff, 1920), vol. 14, p. 60.
8. For a complete history of the paradox, see Gérard Jorland, "The St. Petersburg Paradox (1713-1937)," in *The Probabilistic Revolution. Vol. I: Ideas in History*, ed. Lorenz Krüger, Lorraine J. Daston, and Michael Heidelberger (forthcoming).
9. See Lorraine J. Daston, "Probabilistic Expectation and Rationality in Classical Probability Theory," *Historia Mathematica* 7 (1980): 234-60.
10. Pierre Simon de Laplace, *Essai philosophique sur les probabilités* (1814), in *Oeuvres complètès*, Academie des Sciences (Paris, 1878-1912), vol. 7, p. cliii.
11. M.-J.-A.-N. Condorcet, *Essai sur l'applcation de l'analyse à la probabilité des décisions rendues à la pluralité des voix* (Paris, 1785); Laplace, *Théorie analytique des probabilités* (1812), in *Oeuvres*, vol. 7; Siméon Denis Poisson, *Recherches sur la probabilité des jugements en matière criminelle et en matière civile* (Paris, 1837). See also Lorraine J. Daston, "Mathematics and the Moral Sciences: The Rise and Fall of the Probability of Judgments," in *Epistemological and Social Problems of the Sciences in the Early Nineteenth Century*, ed. H. N. Jahnke and M. Otte (Dordrecht and Boston: Reidel, 1981), pp. 287-309.
12. John Stuart Mill, *A System of Logic* (1843), 8th ed. (New York, 1881), p. 382.
13. Joseph Bertrand, *Calcul des probabilités (Paris, 1889), p. 326.*
14. See John Locke, An Essay Concerning Human Understanding (1689; all editions dated 1690), ed. Alexander C. Fraser (New York: Dover, 1959), vol. 1, p. 529; David Hartley, *Observations on Man, His Frame, His Duty, and His Expectations* (London, 1749), vol. 1, pp. 335ff.; David Hume, *A Treatise of Human Nature* (1739), ed. L. A. Selby-Bigge (London: Oxford University Press, 1968), vol. 1, pp. 125ff.
15. Montucla, *Histoire*, 2d ed., vol. 3, p. 381.
16. Hartley, *Observations*, vol. 1, p. 331.
17. Étienne Condillac, *Essai sur l'origine des connaissances* (1746), in *Oeuvres de Condillac* (Paris, An VI [1798]), vol. 1, pp. 100 and 109; Condorcet, *Vie de Turgot* (1786), in *Oeuvres de Condorcet*, ed. F. Arago and A. Condorcet-O'Connor (Paris, 1847-49), vol. 1, p. 222.
18. See J. Gilissen, "La preuve en Europe du XVIe au début du XIXe siècle," *Recueils de la Socièté* Jean Bodin pour l'Histoire Comparative des Institutions 17(1965): 755-833.
19. From the Instruction of 21 October 1791, quoted in Gilissen, "Preuve," pp. 831-32. These instructions were reprinted in the Code of 1808.
20. Louis Poinsot, in *Comptes rendus de l'Académie des Sciences* 2 (1836): 399.
21. See Lorraine J. Daston, "The Domestication of Risk: Mathematical Probability and Insurance, 1650-1830," in Krüger et al., eds. *The Probabilistic Revolution.*
22. See, for example, Estienne Cleirac, *Les us et coutumes de la mer* (Rouen, 1671), pp. 271-72.
23. See Maurice Edward Ogborn, *Equitable Assurances* (London: George Allen and Unwin, 1962), for a detailed history of this company.
24. Barry Supple, *The Royal Exchange Assurance* (Cambridge: Cambridge University Press, 1970), p. 56; Royal Assurance Company, *Assurance Book on Lives*, vol. 1 (1733-57), MS. 8740 of the Guildhall Library, London.
25. H. A. L. Cockerell and Edwin Green, *The British Insurance Business 1547-1970* (London: Heinemann, 1976), p. 27.
26. Quoted in Ogborn, *Equitable*, p. 35.
27. Quoted in Ogborn, *Equitable*, p. 105.
28. Quoted in Ogborn, *Equitable*, p. 206. See also Augustus De Morgan, "Review of Théorie Analytique des Probabilités. Par M. le Marquis de Laplace," *Dublin Review* 2 (1837): 338-54, esp. pp. 341ff.
29. Johann Süssmilch, *Die göttliche Ordnung in den Veränderungen des menschlichen Geschlechts*, 3d. ed. (Berlin, 1775), p. 49.