79. From *Ars Conjectandi* (1713)*

(The Law of Large Numbers)[1]

JAKOB BERNOULLI

We have now reached the point where it seems that, to make a correct conjecture about any event whatever, it is necessary only to calculate exactly the number of possible cases,[2] and then to determine how much more likely it is that one case will occur than another. But here at once our main difficulty arises, for this procedure is applicable to only a very few phenomena, indeed almost exclusively to those connected with games of chance. The original inventors of these games designed them so that all the players would have equal prospects of winning, fixing the number of cases that would result in gain or loss and letting them be known beforehand, and also arranging matters so that each case would be equally likely. But this is by no means the situation as regards the great majority of the other phenomena that are governed by the laws of nature or the will of man. In the game of dice, for instance, the number of possible cases [or throws] is known, since there are as many throws for each individual die as it has faces; moreover all these cases are equally likely when each face of the die has the same form and the weight of the die is uniformly distributed. (There is no reason why one face should come up more readily than any other, as would happen if the faces were of different shapes or part of the die were made of heavier material than the rest.) Similarly, the number of possible cases is known in drawing a white or a black ball from an urn, and one can assert that any ball is equally likely to be drawn: for it is known how many balls of each kind are in the jar, and there is no reason why this or that ball should be drawn more readily than any other. But what mortal, I ask, could ascertain the number of diseases, counting all possible cases, that afflict the human body in every one of its many parts and at every age, and say how much more likely one disease is to be fatal than another—plague than dropsy, for instance, or dropsy than fever—and on the basis make a prediction about the relationship between life and death in future generations? Or, who could enumerate the countless changes that the atmosphere undergoes every day, and from that predict today what the weather will be a month or even a year from now? Or again, who can pretend to have penetrated so deeply into the nature of the human mind or the wonderful structure of the body that in games which depend wholly or partly on the mental acuteness or the physical agility of the players he would venture to predict when this or that player would win or lose? These and similar forecasts depend on factors that are completely obscure, and which constantly deceive our senses by the endless complexity of their interrelationships, so that it would be quite pointless to attempt to proceed along this road.

There is, however, another way that will lead us to what we are looking for and enable us at least to ascertain *a posteriori* what we cannot determine *a priori*, that is, to ascertain it from the results observed in numerous similar instances. It must be assumed in this connection that, under similar conditions, the occurrence (or nonoccurrence) of an event in the future will follow the same pattern as was observed for like events in the past. For example, if we have observed that out of 300 persons of the same age and with the same constitution as a certain *Titius*, 200 died within ten years while the rest survived, we can with reasonable certainty conclude that there are twice as many chances that Titius also will have to pay his debt to nature within the ensuing decade as there are chances that he will live beyond that time. Similarly, if anyone has observed the weather over a period of years and has noted how often it was fair and how often rainy, or has repeatedly watched two players and seen how often one or the other was the winner, then on the basis of those observations alone he can determine in what ratio the same result will or will not occur in the future, assuming the same conditions as in the past.

This empirical process of determining the number of cases by observation is neither new nor unusual; in chapter 12 and following of *L'art de penser*[3] the author, a clever and talented man, describes a procedure that is similar, and in our daily lives we can all see the same principle at work. It is also obvious to everyone that it is not sufficient to take any single observation as a basis for prediction about some [future] event, but that a large number of observations are required. There have been instances where a person with no education and without any previous instruction has by some natural instinct discovered—quite remarkably—that the larger the number of pertinent observations available, the smaller the risk of falling into error. But though we all recognize this to be the case from the very nature of the matter, the scientific proof of this principle is not at all simple, and it is therefore incumbent on me to present it here. To be sure I would feel that I were doing too little if I were to limit myself to proving this one point with which everyone is familiar. Instead there is something more that must be taken into consideration—something that has perhaps not yet occurred to anyone. *What is still to be investigated is whether by increasing the number of observations we thereby also keep increasing the probability that the recorded proportion of favorable to unfavorable instances will approach the true ratio, so that this probability will finally exceed any desired degree of certainty*, or whether the problem has, as it were, an asymptote. This would imply that there exists a particular degree of certainty that the true ratio has been found which can never be exceeded by an increase in the number of observations: thus, for example, we could never be more than one-half, two-thirds, or three-fourths certain that we had determined the true ratio of the cases. The following illustration will make clear what I mean: We have a jar containing 3,000 small white pebbles and 2,000 black ones, and we wish to determine empirically the ratio of white pebbles to the black—something we do not know—by drawing one pebble after another out of the jar, and recording how often a white pebble is drawn and often a black. (I remind you that an important requirement of this process is that you put back each pebble, after noting its color, before drawing the next one, so that the number of pebbles in the urn remains constant.) Now we ask, is it possible by indefinitely extending the trials to make it 10, 100, 1,000, etc., times more probable (and ultimately "morally certain") that the ratio of the number of drawings of a white pebble to the number of drawings of a black pebble will take on the same value (3:2) as the actual ratio of white to black pebbles in the urn, than that the ratio of the drawings will take on a different value? If the answer is no, then I admit

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that we are likely to fail in the attempt to ascertain the number of instances of each case [*i.e.*, the number of white and of black pebbles] by observation. But if it is true that we can finally attain moral certainty by this method[4] . . . then we can determine the number of instances *a posteriori* with almost as great accuracy as if they were known to us *a priori*. Axiom 9 [presented in an earlier chapter] shows that in our everyday lives, where moral certainty is regarded as absolute certainty, this consideration enables us to make a prediction about any event involving chance that will be no less scientific than the predictions made in games of chance. If, instead of the jar, for instance, we take the atmosphere or the human body, which conceal within themselves a multitude of the most varied processes or diseases, just as the jar conceals the pebbles, then for these also we shall be able to determine by observation how much more frequently one event will occur than another.

Lest this matter be imperfectly understood, it should be noted that the ratio reflecting the actual relationship between the numbers of the cases—the ratio we are seeking to determine through observation—can never be obtained with absolute accuracy; for if this were possible, the ruling principle would be opposite to what I have asserted: that is, the more observations were made, the *smaller* the probability that we had found the correct ratio. The ratio we arrive at is only approximate: it must be defined by two limits, but these limits can be made to approach each other as closely as we wish. In the example of the jar and the pebbles, if we take two ratios, 301/200 and 299/200, 3001/2000 and 2999/2000, or any two similar ratios of which one is slightly less than 1½ and the other slightly more, it is evident that we can attain any desired degree of probability that the ratio found by our many repeated observations will lie between these limits of the ratio 1½, rather than outside them.

It is this problem that I decided to publish here, after having meditated on it for twenty years. . . .

. . . If all events from now through eternity were continually observed (whereby probability would ultimately become certainty), it would be found that everything in the world occurs for definite reasons and in definite conformity with law, and that hence we are constrained, even for things that may seem quite accidental, to assume a certain necessity and, as it were, fatefulness. For all I know that is what Plato had in mind when, in the doctrine of the universal cycle, he maintained that after the passage of countless centuries everything would return to its original state.

NOTES

1. Translated from *Klassische Stücke der Mathematik*, selected by A. Speiser (Zürich, 1925), pp. 90-95. The selection is from the German translation of the *Ars Conjectandi* by R. Haussner in Ostwald's *Klassiker der exakten Wissenschaften*, Leipzig, 1899, nr. 108.
2. For "case," the correct translation of the German, one may read *result* or *outcome*.
3. *La logique, ou L'art de penser*, by Antoine Arnauld and Pierre Nicole, 1662. (Makes use of Pascal, Fragment no. 14.) There are in fact *two* authors but Bernoulli makes it appear there is only one.
4. Bernoulli demonstrates that this is true in his next chapter.

Chapter VI
The Scientific Revolution at Its Zenith (1620–1720)

Section D
The Bernoullis

JOHANN BERNOULLI (1667–1748)

Johann Bernoulli was the tenth child in his family. His father, Nikolaus, attempted to draw him into the family business. After an unsuccessful apprenticeship as a salesman, however, he received permission from his father to enroll at the University of Basel in 1683. He resided with his brother Jakob. At age 18, Johann received the master of arts degree. At his father's urging, he took up the study of medicine but privately studied mathematics and experimental physics with his brother. He went to Paris in 1691, where he participated in the mathematical circle of Nicolas Malebranche (then the foremost Cartesian). In their discussions Bernoulli disseminated Leibniz's calculus. He also gave calculus lessons to Guillaume-François-Antoine de L'Hospital—lessons which became the basis for the first textbook on the differential calculus, L'Hospital's *Analyse des infiniment petits* ("Analysis of the Infinitely Small," 1696). This text contained the method for evaluating the indeterminate form 0/0, which is incorrectly known today as L'Hospital's rule. In 1693, Bernoulli began an extensive correspondence with Leibniz.

Through the intervention of Christiaan Huygens, Johann Bernoulli was offered the professorial chair in mathematics at Gröningen (Holland) in 1695. He accepted because his quarrels with his brother were growing and because he could not hope to obtain the mathematics professorship at Basel as long as Jakob lived. In September 1695 he, his wife Dorothea Falkner, and their seven-month-old son Nikolaus left for Holland. Two sons were born later—Daniel, the most famous of the Bernoullis, and Johann II. While at Gröningen he did not curb his "Flemish pugnacity." The theologians with whom he argued about natural philosophy charged him with Spinozism, a late-17th-century word for atheism.

Upon the death of Jakob in 1705, Johann succeeded him at the University of Basel. Johann would have preferred to accept other offers extended to him by the Universities of Leiden and Utrecht, but family concerns drew him to his native city where he spent the rest of his life. He was the most distinguished member of the university faculty. In the early 1720s, he taught his greatest student, Leonard Euler. Euler was to be one of his two heroes; the other was Leibniz. His activities were not limited to university affairs, as a member of the Basel school board, he worked to reform its humanistic Gymnasium.

Influential far beyond Switzerland, Johann took part in two major conti-