THE AUTONOMY OF STATISTICAL LAW

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The most important conceptual event of twentieth century physics is the discovery that the world is indeterministic. That discovery was preceded by a metaphysical revolution, in which people became aware that law-like indeterminism is at least thinkable. In 1800 chance was still a matter of secret and concealed causes. Probabilities did not denote something real in the world, but only our limited knowledge and ultimate ignorance about the workings of the universe. Even those anti-determinists who denied that there is universal necessity never thought for a moment that law-like chances would provide an alternative to strictly universal laws of nature. By 1900 that was a real possibility. Quantum mechanics has since convinced us that there are indeed irreducible probabalistic laws. It could do that only when such laws had become a conceptual possibility. I am concerned not with the quantum mechanical end of the story, but with the conceptual beginning; the making possible of a universe of law-like chance.

At the beginning of the nineteenth century, then, chance was the superstition of the vulgar. By the end of the century a probabalistic law could be called, by Francis Galton, the law "which reigns with serenity and in complete self-effacement amidst the wildest confusion."[1]

Chance, in short, had attained the respectability of a Victorian valet. Chance had been tamed, and was ready to become the loyal servant of the natural, biological and human sciences. Statistical laws became what I call *autonomous*, describing a real thing, probability, to be found not only in the ignorance of the mind but in the regularities of nature. How did all that come about?

Before

> *Chance* in atheistical writings or discourse, is a sound utterly insignificant: It imports no determination of any *mode* of *Existence*; nor indeed to *Existence* itself, more than

to *non-existence*; it can neither be defined nor understood: nor can any Proposition concerning it be either affirmed or denied, excepting this one, "That it is a mere word."[2]

That will sound vaguely familiar to philosophers who are unlikely to have read it before. It is from the second, or 1738, edition of Abraham de Moivre's *Doctorine of Chances*, which Hume had doubtless been reading as he set down his thoughts on chance for the 1739 *Treatise*. Hume, we well know, thought "that what the vulgar call chance is nothing but secret and conceal'd cause."[3] We remember Hume as a sceptic about causation. Let us remember that even in the *Enquiry* he assures us that

It is universally allowed that nothing exists without a cause of its existence, and that chance, when strictly examined, is a mere negative word, and means not any real power which has anywhere a being in nature.[4]

Hume's rather odd positive account of probability in the *Treatise*—he dropped the whole thing from the *Enquiry*—seems based upon De Moivre's conception which just precedes my previous quote from that great probabilist:

Chance, as we understand it, supposes the *Existence* of things, and their general known *Properties*: that a number of Dice, for instance, being thrown, each of them shall settle upon one or another of its Bases. After which, the Probability of an assigned *Chance*, that is, some particular disposition of the Dice, becomes as proper a subject of Investigation as any other quantity or Ratio can be.[5]

In the approved technical usage of De Moivre or Hume, a chance is something like an opportunity, an event, a configuration of dice or a ticket on a lottery, an event which can have a definite probability. De Moivre cagily leaves open whether we are to have what we call a subjective or objective attitude to such probabilities. I have described in my book, *The Emergence of Probability*, how Newtonians like De Moivre had a hunch that God was quietly ensuring the stabilities, perhaps by arranging the causes which would ensure long term regularity. Hume of course had a more sceptical attitude, but note that for all his celebrated criticism of the concept of cause, he never for a moment suggests that we should doubt the underlying universal causal nexus. He simply believes that we shall never reveal it. In one of my favorite quotations from that great stylist, he first jeers at the presumption of Boyle who would unveil nature, but then praises the upshot of Newton's work:

While Newton seemed to draw off the veil from some of the mysteries of nature, he showed at the same time the imperfections of the mechanical philosophy, and thereby restored her ultimate secrets to that obscurity in which they ever did and ever will remain.[6]

By the end of the century Laplace gave the canonical statement of the doctorine of necessity, that "all events, even those which on account of their insignificance, do not seem to follow the great laws of nature, are a result of it just as necessarily as the revolutions of the sun."[7] Chance, he assures us, is a purely imaginary cause which we invoke only because of our ignorance:

In ignorance of the relations that unite the entire system of the universe one may have to depend on final causes or on chance, according to which they happen and succeed each other with regularity or without apparent order, but such imaginary causes have been successively driven away to the limits of our knowledge, and disappear entirely before a healthy philosophy, which sees in them only the expression of ignorance.

He used those words at the start of his lectures at the *École Polytechnique* in 1795. He repeated it in a document printed by the Bureau of Longitudes in 1810, and whose chief recipient was Napoleon. He used it in the philosophical essay that serves as introduction to the great *Theorie Analytique des Probabilités*. Laplace had higher hopes than Hume of revealing the hidden and secret causes.

An intelligence which, for a given instant, knew all the forces by which nature is animated and the respective situation of the beings which compose it. . . . will embrace in the same formula the largest bodies of the universe and that of the lightest atom; nothing will be uncertain for it and the future like the past, will be present to its eyes.

I sometimes suspect that when Laplace wrote that, he thought the supreme intelligence would be a slightly superior version of Laplace. Still, he had to employ probability because of his limitations, and he officially regards it as entirely subjective. As I note in *The Emergence of Probability*, what he calls *facility* sounds awfully like objective chance, a "real power" in nature. But the "healthy philosophy" of Laplace can find no official place for such a thing.

We must not imagine that all clever people around Laplace were healthy necessitarians. To begin with, Laplace speaks of planets, particles, and, in general, extended things in the spatio-temporal world. A few years before Laplace lectured, Kant had found room for noumenal freedom of the will in a world of phenomenal necessity. Some readers find that to be a continuation of the Cartesian doctorine of two realms of substance. Without being Kantian, Laplace could well be Cartesian. Perhaps he was a necessitarian about spatial events, and not about mental events.

More to the point, while Laplace was telling his pupils at the *École Polytechnique* that every motion, every event, is determined by iron necessity subject to precise calculation, Xavier Bichat, down the street at the *École de Médecine*, was busy inventing histology and telling his students that so far as concerns the vital functions of the human body,

It is impossible to foresee, predict, or calculate anything with regard to their phenomena.[8]

Other vitalists would make even stronger denials of the doctorine of necessity, but such anti-determinism was not pro-chance. It was an assertion of the individuality of living matter and of the existence of organic laws that are different in kind from the laws of a mechanistic world of falling bodies. There was no implication whatsoever that chance had any real power somewhere in nature. There was no suggestion that some of the laws of nature might be inherently probabilistic. I think that was literally inconceivable in 1800. Even someone who had wished to recall the anti-Democritean dream of Epicurus, or the swerving atoms of Lucretius, would have been denying the doctrine of necessity, not invoking the law-likeness of chance.

More cogent than Lucretius is the account of chance as resulting from independent causal chains. It is found in Aristotle and elaborated by Aquinas, and was familiar at least to anyone with a decent scholastic education. To say that you and I encountered each other at the market by chance, luck, or accident is not, on this theory, to say there is a real power called chance in the world. The events in my life form a causal sequence. So do the events in your life. The description, "You and I ran into each other at the market" is one description of an event. But that same event has another description, in which it fits into a causal chain of events in my life. It has yet another description in which it fits into a causal chain of events in your life. The event is caused, and indeed occurs in two different causal chains, although it may also be described in a way that has no transparent place in any causal chain. Such an event we call chance. A. A. Cournot, the important probabilist and economist, did try to import this idea into his 1843 book on probability theory, but it never caught on.[9] This is because he was a conservative, flailing about for a way to preserve a role for universal causal laws in a world that, he saw with prescience, was becoming increasingly chancy.

After

I have already quoted the biometrician Galton, writing in 1889 about the Normal or Gaussian distribution as a supreme law of nature. Among philosophers we inevitably turn to C. S. Peirce who was convinced that we live in a universe of chance. He invented one of his odd names for this thesis: he called it tychism. In 1892 he published "The Doctrine of Necessity Examined" which concludes by saying, "I believe I have thus subjected to fair examination all the important reasons for adhering to the theory of universal necessity, and have shown their nullity."[10] That is part of the story of what I call *the erosion of determinism*, the passage from necessitarian Laplace to anti-necessitarian Peirce. In my paper "Cracks in Nineteenth Century Determinism," I describe some of the zany arguments being produced in the 1860's, arguments so kooky that they prove something is going bad with the concept of determinism.[11] I say "determinism" but following Ernst Cassirer I urge that the word and even the concept actually surface in French and English just when determinism is pretty well eroded, i.e., about 1860.

Peirce did more than deny necessity. He thought that we live in a world of chance which generates, by some law of large numbers, the perception of a rather stable and regular world. On the one hand he claims that "all the diversity and specificalness of events is attributable to chance." On the other hand, stable laws of nature result from a vast number of chance events, as is already suggested by the work of Maxwell, Boltzman and especially Peirce's former colleague whom he never mentions in this connection, namely Gibbs. It is a familiar fact that Peirce had a lot of good ideas about probability. We should also remember that he worked at his father's Observatory in Harvard and in the U.S. Coast and Geodetic Survey. He measured mundane things and also made a few interesting determinations of the constants of nature. Thus he knew by experience how variation in individual measurements adds up to a definite stable number in the long run.

At the same time, but writing in a pretty different tradition, we find Emile Durkheim convinced that there are statistical laws that act on society from above. The rates of suicide, divorce, and other kinds of social deviancy are objective measures of the health or pathology of a society. They are not mere summaries of individual behavior. They are intrinsic social laws which are not reducible to the sum of more fundamental facts about individuals. They are themselves fundamental, and we should act directly at the level of social laws in order to produce a more healthy society. Robert Nye has shown how intimately all this is connected with political concerns of fin-de-siècle France, obsessed with degeneracy and declining birth statistics.[12] There is an addition a lot of sheer empirical data—analogous to Peirce's experience with measurement—that motivates Durkheim. We may think of his book *Suicide* as inaugurating a certain style of sociology, but we must not imagine it a pioneer work on suicide. In my essay "La Statistique du suicide au XIX^{e} siècle," I study French work on suicide. I show how intense and frequent is the garnering of French suicide statistics from the 1820's on.[13] There are literally dozens of pre-Durkheim books (let alone articles) on suicide numbers. There were philosophical debates about what was called "statistical determinism." It was easy to believe, before Durkheim, that suicide rates constitute autonomous laws of society, not dependent upon more fine-grained laws about individuals. In Durkheim's words,

> Normally, when one speaks of tendencies or of collective passions, one is inclined to see these expressions only as metaphors or manners of speaking, which denote nothing real except a sort of mean between a certain number of individual states. One refuses to regard them as things, as forces sui generis which dominate individual consciousnesses. Such is however their nature, and that is what the statistics of suicide brilliantly display for us. . . . We are obviously not intending to say that society is possible without individuals, a manifest absurdity of which no one will suspect us. But we mean: (1) that the group formed by associated individuals is a reality of a different sort than each individual taken alone; (2) that collective states exist in a group before affecting the individual as such and before organizing in the individual, in a new form, a purely interior mental existence.[14]

Characteristics of the society determine the suicide rate, and those characteristics act on individuals, a definite proportion of whom take their lives in the several months of the year (more in June than December) by a definite proportion of methods (more Carbon Monoxide poisoning in France, more self-hanging in England). This is an example of what I call the *autonomy of statistical law*.

In two of the most successful sciences of the day we now read the workings of chance: statistical mechanics and Darwinist evolution. Stephen Brush has urged that we find a certain drift towards indeterminism in the development of thermodynamics and the investigation of irreversible processes.[15] I think we should be cautious with such readings. Maxwell is very cagey as to the extent to which his mathematics commits one to real chance in nature. Boltzman proved one of his main theorems to show how a deterministic underpinning can be postulated for superficially random processes. Arguably Peirce was the first man openly to use kinetic theory as part of an argument for indeterminism. Equally I have heard it urged that Peirce was the first to insist that Darwinism illustrates how chancy a world we live in. I believe that the great success stories of 1859, i.e., Darwin and Maxwell, will not teach us much about how, in the beginning, chance was tamed. Only after it was pretty well tamed could people comfortably adopt an indeterministic attitude to those sciences. Thus only fairly late in the erosion of determinism would those great fields of research be reinterpreted. Of course probability laws are autonomous in Darwinian and kinetic theory long before 1936 when von Neumann "proved" that quantum theory is not reducible to hidden causal variables. By 1905, without being able to prove it, people well understood how probabalistic the world might be. In that year Schneider recognized that radioactive disintegration can be described by assigning a purely probabalistic constant to each atom so as to yield the exponential decay function.[16] This amounts to the *precise* denial of what Laplace said about atoms. Likewise in 1905 genetic problems begin to be solved by Monte Carlo methods, so that genetics is at least *exactly* as if all worked by chance.

My claim in the rest of this paper is that the study of societies made all this possible. It is not too daring a speculation! The very word "statistics" once meant the study of the state. Couple that with the fact that in the early days of enthusiasm it was called "moral science," and we are pointed in the right direction for serious enquiry.

Three Steps Towards the Domestication of Chance

The erosion of determinism is, as befits a metaphysical revolution, an event of unmanageable complexity. We can write a history of almost every nineteenth century branch of human knowledge as a history of the gradual—or sometimes sudden—introduction of probabalistic ideas. To grasp some of the things that are happening it is useful to impose slightly arbitrary distinctions. I think we can see three stages.

First there is what I call *the avalanche of printed numbers*. Between 1820 and 1840 there is an almost insane collecting and printing of numbers of absolutely everything, but above all of numbers connected with deviancy, disease, and death.

Then there is an era of metaphysical complacency about *laws of large numbers*. It is realized that the avalanche of printed numbers spewed forth endless probabilistic laws, chiefly connected with groups of people. But a healthy metaphysics saved us from thinking that chance has any real power in nature. On the contrary, all the stable regularities were understood as the product of tiny but fully deterministic causes. In this period, the laws of large numbers, which we now call plain theorems of mathematics, were given a metaphysical role in permitting us to use probability without really being forced to believe in it. The term "law of large numbers" originates with Poisson in 1835. So this period overlaps with the avalanche of numbers, and also it overlaps with what came third.

Third is *the autonomy of statistical law*. During the period of metaphysical complacency, and even before, probabilities were used to predict all sorts of things about populations. Mathematics was used to "explain" the appearance of statistical regularities, but statistical laws were not in turn used to explain anything about populations. Moreover it was taken for granted that there is basically one statistical law for by far the largest class of populations—none other than the Gaussian or

Normal distribution. Only by about 1875 did one begin to suspect that perhaps almost no populations are normally distributed. After then one had to invent *tests* of statistical hypotheses because for the first time alternative models had to be taken seriously. Thus statistical law becomes much more like ordinary natural law. The nineteenth century, it will be recalled, much favored what Whewell and others called "the method of hypothesis." Laurens Laudan has shown how in many ways this, rather than induction, can be regarded as the canonical method of the nineteenth century science.[17] Only when the method of hypothesis was applied to statistics could one think seriously that it was like the use of more universal laws of nature.

There is a second, connected, ingredient to the autonomy of statistical law. Only in the last quarter of the century did people use laws to explain, as well as to predict, the course of nature. In my opinion, only when you use a law to explain a phenomenon, do you regard the law as having an autonomy of its own. Galton will be a convenient witness to invoke at this juncture.

Thus in the first stage, there is a wild scrambling for numbers. In the second stage the resulting statistical regularities are digested. In the third stage the regularities are regarded as laws of nature in their own right. Chance had been tamed.

The Avalanche of Printed Numbers

I have described this at length in a companion essay, "Biopower and the Avalanche of Printed Numbers."[18] In brief, between 1820 and 1840 there was an exponential explosion in the number of numbers being printed, while there was a merely linear increase in the number of words being printed. Counting different kinds of people stands out in these fetishistic tabulations.

If we look in the eighteenth century for predecessors of the idea of statistical law we shall find it among the philosophical economists, such as Turgot and J. B. Say. The other figure to mention is Condorcet whose 1785 study of majority votes provided a model for specific later enquiries, and whose marvellous testament, the 1793 essay on the progress of the human spirit, would serve as model for Saint-Simon, Comte, and one version of the new sociology.[19] Moreover Condorcet arguably got Laplace himself to take up the study of probability. So great minds laid the seeds, but they simply had no numbers with which to cultivate them. For example Condorcet wrote on voting procedures, and so in turn did Laplace. But neither had a shred of empirical data with which to vindicate his calculations. As I show in my paper "Models for Juries," only Poisson in 1837 could draw any conclusions about real voting behavior.[20] That was because, by then, there had been an avalanche of printed numbers to which the French Ministry of Justice had been contributing, in a massive way, since 1826.

As befits an avalanche there were little trickles before it gathered momentum. Madame de Stael is a perfect illustration. In a characteristically perceptive statement of 1796, she says:

> A collection of men is composed of a certain number of characteristics, and from all those types which give a more or less similar result, it is necessary to notice that

> circumstances which are the most dependent on hazard can be submitted to a positive calculus (*un calcul positif*) when the chances are multiplied. In the Canton of Berne, for example, it has been remarked that every ten years there are approximately the same number of divorces; there are towns in Italy where one calculates exactly how many assassinations are regularly committed each year. Thus the events which result from a multitude of diverse combinations have a periodic return, a fixed proportion when the observations are the result of a large number of chances.[21]

Note that it is murders and divorces. Statistics was called moral science partly because it was the study of immorality: suicide, prostitution, monomaniacs, crimes against the person, crimes against property, vandalism, drunkenness, onanism. In fact the term "moral science" has a slightly odd history. It has originally to do with second level of the French Academy, class II of the *Institut* Nationale for "moral and political science." The first rank of the *Institut* was for mathematical and physical science, while the *idéologues* frequented the second level. Napoleon abolished Class II in 1803, perhaps because he found the *idéologues* seditious, although he said it was because they talked too much and acted too little. At any rate, with their Lockeist psychology and their theory of economic Newtonian laws of nature there was a certain threat to a self-crowned emperor. But then the intellectual offspring of those very *idéologues*—the philanthropic students of society such as Quetelet, Saint-Simon, Comte, Villermé, and so forth, began to count people and in particular count deviants. They called that moral science. Oddly this term moral science acquired a certain sense in English, so that when Cambridge University introduced a new faculty of economics, politics, psychology and philosophy in the 1850's it was called the Moral Sciences Faculty. Gradually all subjects were hived off, leaving only philosophy, and then in 1969 the name was changed to Faculty of Philosophy. I rather like the fact that as a student of arcane statistics I am also one of the last people ever to have taken a degree in moral science (a term which Hilary Putnam has recently reintroduced in his book, *Meaning and the Moral Sciences*).

To return, however, to the high times of moral science, I would not pretend that all the interest in counting people was prurient. In 1832 Charles Babbage printed a small pamphlet "On the Advantages of a Collection of Numbers to be entitled the Constants of Nature and Art."[22] Many years ago Churchill Eisenhart of the Bureau of Standards suggested to me that this pamphlet is the origin of our very concept of a "numerical constant," the mighty numbers of nature that we call g, G, and e and so forth. Not that one did not know some of these numbers before 1832. Cavendish had weighed the earth, and so in 1798 he knew the gravitational constant G, and actually Newton had a pretty canny estimate of G as soon as he had invented it. All the same Babbage represents that time when it became taken for granted that the world is somehow constituted by a set of numbers which must be published in one book. The astronomers with their ephemera and the chemists with their atomic weights are suddenly seen as doing the same thing. A new kind of book is needed, a book "which ought to contain all those facts which can be expressed in numbers in the various arts and sciences."

What an array of numbers Babbage proposed! There are 20 categories. The list begins in a plain enough way: constants about the solar system, about atomic weights, then constants of metals such as specific gravity and specific heat. But as we pass from physics it becomes less clear that we are concerned with a natural kind at all, except simply "whatever can be measured or counted." Average heights, lifespan, pulsations per minute, proportions of the sexes—these are to be recorded for the various species of animals. Even the standard weights of their various bones shall go down. We need to know the quantity of air consumed per hour by a man, and by a woman. We want propositions of the form "A man labouring ten hours will saw (x) square feet of deal, ditto (y) elm. . . . ditto (z) square feet of Portland stone. . . . every kind of labour—raising water one foot high, horses, oxen, cows and camels ditto." On we go to the vegetable kingdom, books in libraries, heights of notable buildings, frequency of letters of the alphabet in the human languages.

"Most of the constants in the list" already exist, says Babbage. Evidently some are old, but it is striking that most of them are very very new. Consider the frequency studies for letters of the alphabet in various languages. That seems to start with the Belgian F. J. Adelman in 1829. Babbage says that rates of disease and sickness must be recorded. In my paper on the avalanche of printed numbers I show in detail that in 1823 there was zero usable information about sickness rates. In 1824 there was one datum, from Scotland. In 1825 the best professional statistician in England was testifying to a select committee of the House of Commons that in principle there could not be a law of sickness. By 1835 such laws abounded. That was because whole new categories of numbers were being established and printed. That is the sort of thing I mean by the avalanche of numbers.

The avalanche of numbers is interwoven with positivism. August Comte, who coined the word "positivism" in the 1830's, very much disliked number crunching. The original positivism was to be an historical science. He and Adolphe Quetelet both used the term "social physics" and "social mechanics" for a new discipline. But Quetelet wanted this to be a science of numbers and probabilities. So Comte thought his name had been stolen, and he coined a new name for his discipline, sufficiently nasty sounding that he hoped no one would steal it—"sociology". It was stolen. Quetelet won again. Moreover poor old Comte could not even keep hold of his own positivism.[23] By 1848 I find many passages like this paragraph from a medical book:

> It is no longer permissible in our times to seek truth in pure theory, in vain abstractions or gratuitous hypothesis. The rigorous observation of facts has become, quite justifiably, the point of departure and the basis of all knowledge. From this *positivisme eclaire* of our epoch is born the application of statistics to medicine and to the study of moral and political questions.[24]

Lest anyone think I exaggerate when I speak of a widespread fetishism for numbers, I may cite my favorite character, Alphonse Guerry, who in 1832 won a prize for his work on French crime and suicide statistics. Did you know that 35% of all murders committed by poisoning are consequent upon adultery?[25] Later Guerry did a comparative work on the "moral statistics" of France and England.[26] He would for instance consider case by case the 21,132 individuals accused of murder and sort them into 4,478 groups of motives. By the time he retired he had personally compiled elaborate index cards on 85,564 suicides with a detailed analysis of the motives.[27] Nor of course was this just a matter of counting. You cannot count unless you have categories within which to count, and Guerry was influential in forcing the police to start classifying suicides according to a scheme of his own devising. Well, they did not quite go so far as he wishes in his 1832 essay. He wanted, for each suicide, the sex, age, state of health, occupation or social status, address, place of birth, marital status, with or without children, *Etat de fortune*—(rich, comfortably off, or miserable) education, intellectual state, morality (which is divided as: arrest record ? adulterer ? prostitute ? gambler ? concubine ? alcoholic ?), religion, place of suicide, medical circumstances, time of day, meteorological conditions, who found the suicide and under what conditions, motives, suicide note ? parental history of madness insanity or suicide? Finally, *objets trouvés* (in the vicinity of the suicide, such as in his pockets).

This will be a parable for the way in which counting breeds bureaucracies which breed new classifications of people which in turn breed more numbers. It is amazing what you can count. I have found 1832 to be an oddly key year. It saw Babbage, Guerry and many others who turned their minds to numbers. How about an 1832 paper by Lueret and Mitivie "On the Pulse Rate of Lunatics (aliénés) considered as a function of atmospheric pressure, the phases of the moon, etc."[28] Such lunacy should not let us forget the political element in counting. As Guerry puts it in 1832:

> L'importance de la statistique, comme instrument de surveillance et contrôle, dans les divers branches des services publiques, ne pouvait échapper au coup d'oeil de Napoleon I ere.

Laws of Large Numbers

The basic idea starts with Jacques Bernoulli, whose *Art of conjecturing* was posthumously published in 1713. He proved that if the probability of an event S on a single trial is p, then in repeated independent trials it is increasingly probable that the relative frequency of S is close to p. As the number of trials grows without bound, the probability of any finite difference from p decreases to zero. This might provide an analysis of statistical stability in tossing coins or even in the proportion of male and female births. But as social statistics came to the fore during the avalanche of numbers, it became increasingly clear that we can have statistical stability even though we do not have repeated independent trials with constant p. Guerry in 1832 is one of a hundred who make the same point:

> Assuredly the annual harvest or the quantity of taxes collected in the several parts of the country cannot be evaluated in advance with more precision than the number of robberies, murders and assassinations.[29]

He can say that, thanks to the statistics from the ministry of justice that appeared after 1826. Poisson wrote his great tract on probability in the light of that data. In "Historical Models for Justice" I show how he was led to what he called "the law of large numbers."[30] He had a theorem that even if we do not have constant probability of S, we may still have statistical stability. So long as there is a constant probability distribution for the probability of S—so that the probability of S is itself a random variable—in the long run the relative frequency converges on the average probability of S. I think that people observed empirically before it was proven mathematically that you get stability quicker with a heterogeneous p than with a Bernoulliesque homogeneous p. That is, the variance in Poisson's problem is *less* than in Bernoulli's. Social statistics are more stable than coin tossing.[31]

Since I have described Poisson and what we may call the French tradition elsewhere, it will be well to present briefly the German tradition, which is *exactly* contemporary. Gauss and Laplace are peers. They corresponded about how to do the theory of errors. Gauss was a little younger but at least equally original. The chief work is *Theoria Motus* of 1809. The systematic explanation of the theory of errors in terms of little deterministic causes that add up to one probability is however more due to Hagen's monograph of 1837, published in the same year as Poisson's book.[32]

Gauss was concerned with astronomy, not society. Suppose we have a number of doubtless erroneous observations of a heavenly body on successive days, and that we know the form of the equation that describes the motion, but not the parameters. What is the best guess of the orbit? Gauss devised the theory of least squares to do the work. Unlike the slightly nutty works which I read in the course of the present research, Gauss's study is superb. It is a classic of modern literature, a breath-taking example of the ability of the human mind to sort things out from scratch. It also generated a certain philosophical position. It seemed that one could now understand how the normal distribution, "the law of errors," *must* arise as the product of a large number of small independent causes. Throughout the century we have a series of contributions to this research program, of which Poincaré's is perhaps the most famous, but is only one in a line of dozens of contributors.

Hagen's 1837 paper provides a good example of how such work was done. He based his proof on the idea that random error will consist of the algebraic sum of a large number of minute or infinitesimal errors, all of equal magnitude, and as likely to be positive as negative. By what now seems a fairly straight-forward derivation, he shows that in the limit we shall find a Gaussian distribution. This is taken as an explanation of the empirical fact, shown by experiment, that errors commonly do assume the familiar bell shaped curve. This argument is repeated, again and again, with relatively minor variations, throughout the century, right up to Czuber's magisterial survey, in 1897, of the then current state of probability theory.[33]

The theory of errors was grafted on to social statistics in a slightly eccentric way. The first great published official tables of numerical data are the *Recherches statistiques sur la ville de Paris et la département de la Seine* which began in 1821. They contain fundamentally important essays on applied probability. The essays are anonymous, but they are written by one of the directors of the Paris statistics project, the distinguished elderly mathematician Fourier, best known for his work on heat and the "Fourier transform." When the young and energetic Belgian astronomer Adolphe Quetelet became enamoured of probability, he wrote his own elementary introduction, largely cribbed from Fourier's notes.[34] But Quetelet went on to better things, and became the premier statistician of Europe. It was he who founded the great International Statistical Congresses which commenced in Brussels in 1853. He was instrumental in founding the statistical section of the British Association for the Advancement of Science when he attended its 1833 meeting. During the 1830's his own journal, the *Correspondance Mathematique et Physique*, was a prime place for the study of statistics. Thus Quetelet contributed enormously to the institutionalizing of statistics as a major independent discipline. Yet we may remember him for something else. He noticed in an old Edinburgh magazine a record of the chest circumference of some 7000 Scottish soldiers. That had been published in 1812, for no evident reason. The numbers are just *there*, without comment, betokening the avalanche of numbers that was about to cover Europe. Many years later, Quetelet was to assert that these numbers follow the distribution of the curve of errors. They are just as if an incompetent tailor had made 7000 measurements of the chest of one (average) Scots soldier.

As a matter of fact, it is not even true. Doing a chi-squared test to the Scottish data, we find a poor Normal curve. No matter. Quetelet then convinced the world that biological and social phenomena are distributed according to the curve of errors. This became an absolutely entrenched opinion, so that in due course Francis Galton and Karl Pearson were to rename the distribution as *the Normal curve*. It is the curve which nature normally follows, they thought, "the law which reigns with serenity and complete self effacement amidst the wildest confusion ... the supreme law of Unreason." In fact Pearson was soon to regret the name they had given to the curve, for he was finding that most phenomena are skewed, and so he developed a cumbersome theory of abnormal curves. But the name has stuck, in English, to this day. The news that most phenomena are not strictly normal had already been broken some time before, by Lexis, who observed in 1875 that the only well known social phenomena that are normally distributed are the distribution of male and female births. Moreover that is to be expected, because there we have a straightforward binomial distribution which even in 1718 de Moivre had shown to generate a normal distribution.

Between 1835 and 1875, however, laws of large numbers and the law of errors reigned supreme. Probability was a threat to a deterministic world view, yes, but determinism could hang on. This is because of the mathematics in the 1837 style of Hagen or Poisson. We could understand the stable regularity as arising from a large number of independent deterministic little causes. That is why I call this an era of metaphysical complacency. Quetelet does indeed form the concept of "the average man" and seems to regard the average man as an objectively existing sort of entity. But it is an entity that simply will not do the trick. In 1865 Claude Bernard tells us good enough practical reasons for this in his widely read long chapter with the title "On the use of calculation in the study of the phenomena of living beings, on averages and on statistics."[35] More powerful theoretical reasons are given by Durkheim, who sees that what we require is the concept of an autonomous statistical law of nature, rather than simply one parameter, the average, taken as a constant (which it is not). Indeed for all Quetelet's services to the statistical world,

I think he did not have the concept of law of a nature of any sort. This is consistent with the fact that he was an astronomer, for all his computations were ephemeral, in the astronomical sense of the word. But he also took an undue delight in ephemeral numbers in general. He enjoyed, for example, his discovery that Belgian lilacs burst into bloom when the sum of the squares of the mean daily temperature since the last frost adds up to 4264°C. That is surely the perfect example of what cannot be a law of nature, and is in itself a meaningless number.

Laws of Chance and the Explanation of Phenomena

Statistical laws become autonomous laws of nature in the final quarter of the nineteenth century. I shall emphasize only two aspects of this complex part of the story. The first is plain to see. Whereas we once had one form of "statistical law," namely the Gaussian or Normal distribution, it became realized that there are many possible forms. Where Poisson had stated, in 1837, the mathematics of what we now call the Poisson distribution, I find no serious reference to it, or even recognition of it, until very much later. But when a less enthusiastic and more critical generation than that of Quetelet looked hard at numerical data, it realized that there must be non-Gaussian laws. Much of this work was German. For example in 1898, Bortkiewicz told us the now familiar story that the Poisson distribution describes rare events, such as the distribution of soldiers being kicked to death by horses in the Prussian military. The existence of alternative statistical models gave us a need for tests of goodness of fit. These are first of all German origin, although in English, Karl Pearson—who himself pointed out German antecedents—is credited for his 1900 chi-squared test, still the most widely used test of goodness of fit.

Much of that story belongs to the history of mathematics, so here I shall attempt a more tendentious and philosophical observation about the autonomy of statistical law. What do I mean by this fine phrase? I connect it closely with the reality of chance. I began by quoting authors who held that chance is nothing real, all a matter of hidden and concealed causes that must be banished by a healthy philosophy. The autonomy of statistical law goes hand in hand with chance being taken as something real in nature. But here we must be cautious. I shall distinguish several stages in the course of which chance became real. At the final stage we have *irreducible autonomy*. In the early days of quantum mechanics many workers favored versions of an ignorance interpretation, harking back to Laplace and beyond. But when von Neumann proved that there cannot be hidden variables underlying quantum statistics, he was saying that chances (or some probability related concepts) were irreducibly real parts of nature.

But let us turn to earlier stages of the journey of which irreducible autonomy is the end point. From the beginning of probability reasoning about 1660 we find statistical chances being regarded as *symptomatic*. Thus William Petty in 1674 said we should have a "scale of salubrity" for the various parishes, based upon the mortality rates for those parishes. The chances of dying at various ages are merely symptoms of whatever it is that make one parish healthier than another one.

Turning to a later medical question, there was a great debate about the efficacy of small pox innoculation. The practice of self-inflicted small pox was brought by Lady Mary Wortley Montague from Turkey in 1721. One inhaled, for example, some dried small pox puss. Some people died, while many others had a greater life-expectancy. Diderot and d'Alembert had a famous debate, the one holding that for the good of the state, innoculation should be compulsory. The other observed that a young person should, for his own sake, avoid innoculation, especially if most other people were being innoculated. They understood these issues very well, and argued on the basis of good rough estimates of the probabilities and utilities in connection with the state and with the person. (The issue was made obsolete by Jenner's invention of vaccination.) During such a debate it was well understood that you could use probabilities to make qualified *predictions* and that you could undertake procedures which would *alter* chances. I shall call this an *instrumental* attitude to statistical law. It was combined with a Laplacian healthy philosophy according to which all the probabilities indicated underlying causes. Only in ignorance did one have to work with probabilities.

There is a missing link between instrumental and irreducible statistical laws. It is the stage at which statistical laws become *effectively autonomous*. This partly means that those who employ laws are indifferent to whether the laws are mere ephiphenomena founded upon universal causes. But an attitude of mind is too hard to pin down. We need a more behavioral criterion. I think it lies in the difference between prediction and explanation. A property, phenonemon, or thing is effectively regarded as real when one is prepared to use it to explain other phenomena. Galton provides my finest example.

Some recent philosophical discussions force me to interject another distinction. There has arisen a considerable literature, to which Wesley Salmon has been the most enduring recent contributor, under the heading of "statistical explanation." He is concerned with using a statistical fact to explain what happened to some individual within a reference class. Some 90% of cholera victims given such and such a treatment survive; Mr. Patel, given this treatment, survives. The explanation is the 90% efficacy of the treatment. In what follows I bypass this kind of explanation. Perhaps I doubt that it is an "explanation" at all. Often "excuse" would be the better word. Thus to take one of the earliest examples, the callous young Tom Gradgrind, by the end of Dickens' *Hard Times*, wreaks revenge on his ghastly utilitarian father. Father Gradgrind had always said that such and such a proportion of such and such a class will be criminals. Tom, now steeped in crime, can say, father, you always told us some would necessarily take to crime. So my crimes are then not my fault.

My concern is not the explanation of an individual fact that falls under a reference class, but the use of a law to explain a phenomenon which otherwise is pretty unintelligible. Galton was long obsessed with heredity. He now has a bad press as the founder of Eugenics, the plan to improve the racial stock so that more Englishmen would have the virtues of middle class Victorians. He was surely an authoritarian figure, this inventor of the system of finger printing, and of the silent dog whistle for use by Police Dogs. He also gave us more felicitous inventions, such as the theory of correlation and regression.

Galton studied the great families and noted a regular tendency of regression towards mediocrity. We now use the more neutral phrase, regression towards the mean. An exceptional person will by and large have less exceptional children. Galton's gravest interest was in men and women of talent, but he also realized that the same was true of more easily measured biometric facts such as human heights. Where Quetelet enthused that Scottish Highlanders had chest diameters distributed according to the law of errors, Galton would have been more struck by the fact that the extremely skinny ones would have slightly less skinny children, and the men of exceptionally sturdy chests had less outstanding sons.

His enthusiasm for correlation and regression arises from the fact that he was finally able to prove (with some assistance from a friend) that regression towards mediocrity follows deductively from any quantity having a Gaussian distribution. In his autobiography he movingly describes his intense exhileration as the explanation flashed upon him in a moment.[36] This was his greatest discovery. He had explained the baffling regression towards mediocrity on the basis of the law that human and biological traits have a Normal distribution. Statistical law had become effectively autonomous.

I do not say that Galton believed that chances are irreducible. In private life he was pretty much of a determinist. In the matter of heredity, his public life shows him to have sought hidden variables. He guessed that genetic material would be carried in the blood. To test this he developed a rather good system of blood transfusion, supposing that the "gemmules" would then be transmitted, say from black rabbits to white rabbits. He was delighted when his gardener reported that the first generation from surviving blood-transfer white rabbits actually showed small tufts of black hair on their paws. Delighted he carried on to the next generation but alas, not only did the project fail, but he also learned that in general the strain of white rabbits he was using tends not to breed quite true. Ever an adherent of scientific rigor, Galton abandoned his hypothesis. The story serves only to show that he did believe in an underlying more mechanistic accounts of genetics. What I call effective autonomy does not preclude such enquiries. It points not at the positive absence of hidden variables, but at the possibility of using statistical laws as effective explanations of puzzling phenomena. That is the point at which chance became something real—not merely symptomatic, not merely predictive, not merely instrumental, but a real explanatory force in nature.

Conclusion

What was required for this complex event? I have here emphasized only a few of the strands. I have not touched upon the road to Peirce's tychism. I leave to inference how the gradual accumulation of suicide statistics led on to Durkheim's effective autonomy of social laws represented in statistical form. I emphasize three stages. First, there is the fetishistic avalanche of numbers, deeply motivated by a desire for information about and control of social deviants. The amazing masses of statistical stabilities lead on to a theory of laws of large numbers, showing that regularity in the large may still result from the interaction of myriad tiny causes. This makes acceptable and even intelligible Quetelet's propaganda, that biological and social phenomena are distributed according to the law of errors. Finally acceptance of this Normal distribution as a supreme law of nature makes it possible to explain complex phenomena on the basis of this law, which has gained effective autonomy. Determinism is being eroded faster and faster, but not by making the world amenable to meaningless chance or fate. The new indeterminism brings with it a stricter necessity, the necessity of information and control. That paradox must be repeated again and again. The avalanche of numbers occurred because of a desire for control over deviants, and in the end laws of chance were found to be the very thing to do it. The taming of chance is the creation of order beyond the dreams of any ancient determinist.[37]

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20. To appear in Proceedings of a Conference for Probability (Luino, 1981), ed. by D. Costantini.
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22. *Edinburgh Journal of Science*, N.S. 12, (1832). The word "constant" seems to have acquired this usage in 1832, cf. W. Turnbull, *Treatise on Strength, Flexure and Stiffness of Cast-Iron Beams and Columns, with Tables of Constants.* (London, 1832).
23. A. Comte, *Prospectus des travaux scientifiques nécessaires pour réorganiser la societé* (Paris, 1822), Published as *Plan des travaux* in *Système de politique positive* (Paris, 1854), vol. IV, p. 82. A. Quetelet, *Recherches sur la loi de la croissance de l'homme* (1831), *Nouveaux memoires de l'Academie royale de Bruxelles*, VII (1832), p. 7, in *Sur l'homme et le developpement de ses facultés* (Paris, 1835). The 1835 work contains paragraphs identical to the 1831-2 works, but with "*mécanique social*" replaced by "physique social." See, J. Lottin, "Quetelet, son système sociologique," *Annales de l'Institut Superieur de Philosophie*, vol. 1 (1912), pp. 107-108.
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35. Claude Bernard, *Introduction a l'étude de la médecine experimentale* (Paris, 1865), 3ème partie, ch. II, sec. IX.
36. Francis Galton, *Memories of My Life* (London, 1906), p. 300, "A temporary shower drove me to seek refuge in the rock by the side of the pathway. There the idea flashed across me, and I forgot everything else for a moment of great delight."
37. A brief account of the methodological point of view underlying this paper is "How Should we do the History of Statistics?" *I&C* No. 8, Spring, 1981, pp. 15-26, with a slightly abridged version under the title, "The Taming of Chance by an Army of Statistics," *The Times Higher Education Supplement*, 30 October 1981, pp. 12-13.

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PREFACE

The essays contained in this volume were originally presented during the 1980–82 period in a lecture series sponsored by the Center for Philosophy of Science at the University of Pittsburgh since 1960. The Center acknowledges support of the Richard King Mellon Foundation in making these lectures possible. They not only enhance the varied intellectual landscape of our University but, as the reader will see, represent significant contributions to the area of learning with which they deal. The Center is grateful to the scholars concerned for issuing these products of their labors under our auspices.