Comment

B. Efron

Five weeks into my first probability course, we proved that long-run frequencies converge to their probabilities. It was a surprisingly difficult fact to prove considering that this was the definition of probability we started out with in the first week. Professor Shafer elegantly explores the historical roots of this confusion and draws some interesting conclusions about modern statistical pedagogy. Here are a few random comments inspired by Shafer's ideas.

Statistical philosophy depends a lot upon what kind of data one sees. A bombardment of diverse problems from vastly different subject areas, such as we get in the Stanford biostatistics program, mitigates towards frequentism and against Bayesianism. Business schools, where one often sees a small amount of data acting against a considerable background of relevant prior experience, are naturally congenial to the Bayesian point of view.

Statistics can be defined as the science of accumulating information that arrives a small amount at a time, as it does in a clinical trial. Frequentist methods are at their best in such situations. (They are usually built around some form of exchangeability among the small data units.)

Statisticians are sometimes asked to make inferences in situations where the data arrives in just a few big, noncomparable chunks. An example might be a safety analysis for nuclear reactors. Bayesian methods are often the only methods of any use here.

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The fractionation of statistical theory which Shafer deplores can be viewed more kindly: as an evolutionary adaption of the statistical point of view to different data environments.

I share Shafer's preference for statistics departments that look for inspiration outside of pure statistical theory. We are fortunate to have a pressing demand for our services and an endless source of problems of genuine interest to the broader scientific community. Modern music and modern mathematics are two fields that have turned inward on themselves, so that only the initiated can appreciate the true beauty of the results. If statistics has to depend on its beauty, we may be in bad trouble.

The Stanford statistics department, and many others, continues to make joint appointments, most recently with the medical school, the math department (forget that math remark above) and the Linear Accelerator Center.

Statisticians are the only scientists who think systematically about inference. Nonstatistical inference ideas can be embarrassingly naive, even while popular, a recent example being "fuzzy sets." Statistics is not likely to go out of business as long as scientists need to make accurate inferences. Statistics departments may go out of business, though, if we don't attend to scientists' needs.

In the long run, any field is judged by the ideas it produces. We, the current bunch of academic statisticians, are living off the intellectual capital invested by Gauss, Pearson, Student, Fisher, Neyman, Wald, etc. I hope we are generating the ideas that will secure our successor's place in the academy.

Comment: In Praise of the Diversity of Probabilities

Ian Hacking

I write as a philosopher who has long been curious about probability and statistics, but who is not directly affected by the fortunes of statistics departments. I have learned much from Glenn Shafer over the years, and I am taken with his title: unity and diversity. For me, however, diversity's the thing. Not long ago, philosophers of science thought that the unity of science was a goal, a value and an essential part of rational inquiry. They meant that there is one real world, one

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interconnected body of knowledge about it, one canon of reason and one sound methodology. This vision of unity was not peculiar to philosophy or to science. From time to time an all embracing lust for oneness dominates Western and other cultures. In politics, the United Nations and world federalism were common ideals; in psychotherapy the aim was the integration of the whole person; the Aristotelian unities were the vogue in the arts; one was not supposed to build a new building that did not fit into the style of the old edifices near it.

Times change and with them, intellectual fashions. Disunity is all the rage, and although I tend to make fun of bandwagons, this is one that I am on, speaking up under titles such as "The Disunities of the Sciences" (Hacking, 1990b). To me, therefore, Shafer's inaugural lecture is a dignified voice from the past, reminding us that the unities did, after all, have things to be said for them. I shall use some of his headings to organize comments, usually favoring the "diversity" side of his title. But under Section 7, "The Conceptual Reunification of Probability," I shall urge that probability does not need reunification because it was never disunified. As I shall explain, it is properly thought of as a "radial category" and, as such, resembles many less troublesome concepts.

2. THE ORIGINAL UNITY OF PROBABILITY

Shafer reminds us that probability mathematics began with fair prices. He also rightly emphasizes "that Bernoulli and De Moivre's mathematics bound fair price, belief and frequency tightly together. The probability of an event, in their theory, was simultaneously the degree to which we should believe it will happen and the long run frequency with which it does happen." That is commendably brief but leaves out a lot of detail. Seventeenth century words cognate with "probability" were mostly in the belief business. Even in the ideal case of artificial randomizers used in gambling, I'm not sure that frequency was emphasized. Writers were concerned with objective properties of a chance set-up, founded on physical symmetries. Unequal frequencies would be a *symptom* of lack of objective symmetry, but I don't find people saying outright that the probability of an event just *was*, among other things, "the long-run frequency with which it does happen." Later, Laplace, famous for saying that probability is subjective (a matter of reasonable belief in the light of evidence), had a word for objective probabilities: *facilité*. He didn't literally mean frequency but rather the tendency or propensity to produce certain events in regular proportions. That was objective, as opposed to what he called subjective, namely reasonable beliefs that were relative in part to our knowledge, and in part to our ignorance.

The main probability ideas, "subjective" and "objective" among them, were present right from the start around 1660. In the ideal cases of artificial randomizers the subjective and objective aspects of probability were taken to coincide. But during the period 1657–1705, from the Pascal–Fermat correspondence to the death of Jacob Bernoulli, probability ideas were applied to lots of other sorts of cases. The most important were undoubtedly "subjective," having to do with legal probabilities—not only the reliability of evidence, but also the legal strength of competing claims to inheritance. Then there was probability as a measure of the credibility of witnesses, especially witnesses of miracles and other testimony of the faith. There is an excellent reason why the objective side of probability, displayed by stable frequencies, was not pressing. There were hardly any known stable frequencies outside gaming. Until near the end of the eighteenth century, empirically determined statistical frequencies were limited to the facts of life, namely birth, death and mating.

3. THE RISE OF FREQUENTISM

Shafer connects an emphasis on the frequency side of probability with positivism. "We can pinpoint," he tells us, "just when positivism entered the stage. Independently, and almost simultaneously, in 1842 and 1843, three empiricist philosophers, John Stuart Mill, Richard Leslie Ellis and Jakob Friedrich Fries, published criticisms of Laplace's classical definition of probability as degree of reasonable belief. Probability these authors declared, only makes empirical sense if it is *defined* as frequency." 1842–1843 is a promising time; and why not add a French writer, A. A. Cournot, whose 1843 *Exposition de la théorie des chances et des probabilités* is very clear about objective probabilities to which he gives the name "chances," and which are at least completely tied to stable long run frequency. It is true that unlike Ellis, who thought there was only one clear way to conceive of probability, Cournout thought there were two distinct ideas.

I have several little difficulties here. I don't find any of Shafer's three authors going so far as to "declare" that probability must be "*defined*" (Shafer's emphasis) as frequency. It is stretching things to call Fries an empiricist, critic of Kant though he was, and we note that the work of 1843 appeared in the year of Fries's death. It represented lectures Fries had been giving for ages; his most important work was published in 1800, although it hardly touched on probability. The "pinpoint" is already blunted. (And Cournot says he had hit on his ideas by 1837, when Poisson had also urged that objective probabilities should be recognized explicitly, and called chances.) More interestingly, as the historian of mathematics I. Grattan-Guinness observes, it was around 1820 that Fourier (of the transform and the theory of heat) treated one kind of probability as objective frequency (Grattan-Guinness, 1970). Fourier had the job of organizing the first systematic, official, public and officially published statistical data about population, health, disease, hospitals, madness, crime and so forth. These *Recherches statistiques de la ville de Paris et le département de la Seine* for the first time put a wide range of empirical frequencies into the hands of every thinking person. It was the beginning of an avalanche of printed numbers. After a few years, it seemed as if annual proportions of this and that were rather constant. There is not much difference in the frequency of crimes against people as opposed to property crimes; conviction rates for different types of crime are stable. So, it seemed, are the distributions of suicide by month, by sex, by method in each nation (the English shoot and hang themselves, the French drown or use carbon monoxide). And so forth, chiefly for a large amount of moral statistics. For the first time in human history, a great variety of apparently stable proportions were on view to the general reader. Fourier and his successors understood full well that they were susceptible to treatment by a calculus of probability, and one of Fourier's prefaces to an annual volume of Seine statistics is an excellent introductory text on objective probabilities. It is public tables such as these that gave rise to an idea of probability as explicable solely in terms of frequency.

Shafer is only half right in speaking of positivism here. Yes, positivism and an explicitly frequency approach surface at pretty much the same time. But they are hardly identical. It was Auguste Comte who gave us the name "positivism"; his fulminations against social statistics, frequencies and probabilities are notorious. But he lost, and what we now call positivism embraced frequencies with a passion. Yet this positivism and frequency-fetishism are only two of many consequences of the basic fact that, during and after the Napoleonic era, counting and measuring became the thing to do. The world became, for the first time, numerical. This was a transformation in Western and then human culture comparable to the so-called scientific revolution of Harvey, Galileo and the seventeenth century—to the extent that T. S. Kuhn has called it a "second scientific revolution" (Kuhn, 1976). (It is important to note that this is not one of the hundreds of revolutions within disciplines like those studied in Kuhn's famous *The Structure of Scientific Revolutions,* but a rare event ("second") altering the across-the-board feel for the kind of world we live in, a change in the texture of the universe.) In my opinion, to identify frequency theories with the rise of positivism (and thereby badmouth frequencies, since "positivism" has become distasteful) is to forget why frequentism arose when it did, namely when there were lots of known frequencies. If one wanted to give frequency statistics a bad name, one might contend that nearly all the early frequencies were frequencies of immorality and "degeneracy," and that the enthusiasm for statistics was part of an operation of information and control intended to eliminate deviance. Fascination with deviance from the norm, and even the use of "normal" to mean what usually happens, also began in the 1820s (Hacking, 1990a).

6. THE BALKANIZATION OF PROBABILITY

Political metaphors are dangerous. Times change, in the Balkans as elsewhere. By November 20, 1989, the day of Shafer's inaugural, someone in the audience might have stood up and shouted "Balkanization? You mean the liberation of probability!"

My praise of diversity welcomes the wonderful variety in the use of probability ideas. The tension between "Bayesian" and "frequentist" approaches to statistics was enormously beneficial. It forced reflective students to think out conceptual foundations afresh. It created a space for Shafer's own fascinatingly original *A Mathematical Theory of Evidence*. Moreover, diversity-freak that I am, I believe that the piecemeal approach to inference and decision is the right one. That eclectic attitude will not surprise people who apply statistics to novel problems. It was always commended by R. A. Fisher, many of whose insights become blurred by those who favor one unified theory of statistical inference.

But shouldn't a philosopher want the one right theory? Quite the contrary. Take Hume's celebrated problem of induction. In my opinion both Bayesian ideas and those of Neyman can't solve it but do *evade* it. The Bayesian says, "Hume, you're right, there is no foundation in reason for beliefs about the future. But there is a uniquely reasonable (on pain of incoherence) way of learning from experience, of changing your beliefs. And that is all a sensible person should want." Neyman's disciple says, "You're right Hume, there is no inductive inference, no way of attaching objective probabilities to propositions about the future, but there is still more and less reasonable inductive behavior; there are methods of inferring and deciding that are more reliable than others. And that is all a sensible person could want." I am well aware that neither of these doctrines is the end of the story. But they are fundamentally different insights that would not have arisen without the adversarial character of debates in this century.

In a lecture so full of good sense there is one very odd paragraph. Shafer mentions "one more area in which the leadership role held by statisticians through the 1970s has been wrested from us . . . [Once] we controlled our own history . . . [But now] we have seen our history taken over by philosophers and professional historians of science." He generously says I began this trend with *The Emergence of Probability.* As an aside, I should say I am not an historian and don't think of myself, when I make use of the past for my own ends, as doing history. An anonymous referee of *Emergence* began by insisting that the world "history" should be deleted from the subtitle and the rest of the book, and I'm glad I took the advice. Real historians will know why. Philosophers do their own strange things and make their uses of the past—of which every statistician should be skeptical. All that's an aside. Philosophers make bad historians—and so do statisticians. What statisticians can do is to bring peculiarly statistical insights to their past, very often with an eye to reforming the present, as in Shafer's own marvelous studies of Bernoulli or Lambert. The past in Shafer's hands is a propaganda tool for legitimating his own occasionally radical approaches to statistics. That is an established art form: compare Noam Chomsky's *Cartesian Linguistics* as legitimating the new transformational grammar of 1957 by identifying it with the general or universal grammars of the Enlightenment. Chomsky's book was fascinating. It was about the past. No historian would call it history.

Shafer's talk of "leadership roles being wrested" from statisticians, of "controlling our own history" and "our history taken over . . . ," etc., is puzzling. To begin with, the phrase "controlling our own history" is ambiguous between "controlling our own destiny" and "controlling the writing of our own history." I suspect it is the former that is supposed to make the statistical skin tingle with fear, but of course it is completely irrelevant (and luckily no science controls its own destiny, else there would be dismal stagnation).

Historical research is best done by historians trained in archives, notwithstanding the existence of wonderful hobbyists. Any of us could have found Fourier's prefaces to the *Recherches statistiques*, but in fact Grattan-Guinness made the observation about objective probabilities and could back it up by ploughing through the Fourier archive. More important, it took a certain sort of historical sensibility, in this case Kuhn's, to notice a "second scientific revolution" of numeration and measurement, a revolution of which positivism, frequentist statistics and the avalanche of printed numbers about social deviants are minor ingredients.

7. THE CONCEPTUAL REUNIFICATION OF PROBABILITY

I shall now go slightly against my principles and suggest an entirely general way to understand the unity of probability to which Shafer aspires. In brief, probability is like most other concepts.

There is a natural tendency to think of clear concepts in terms of necessary and sufficient conditions. Mathematical deduction demands that. There has long been an alternative picture in which many of our organizing ideas apply to clusters, or are like family resemblances; things that fall under them are like strands in a rope; the rope may be strong even though some strands do not overlap. The linguist George Lakoff is among those who has urged a way to bring some structure to those metaphors (Lakoff, 1987). It begins with a theory of prototypes due to the psychologist Eleanor Rosch. It holds that many concepts have most favorable, central, prototypical examples, often cross-culturally shared (or quickly transferred) and readily imaged or elicited when the word for the concept is brought to mind. Then there are other less central instances of the concept that are related by some of several standard patterns of metaphor to the prototype; these patterns constitute the structure of the concept that radiates out from the center.

The application to what Shafer calls the "ideal picture" of probability is obvious. "We can insist on the unity of belief and frequency in the ideal picture," he writes, "even while admitting that they go their separate ways in many applications." The prototypes for probability are artificial randomizers, chance setups, with objective symmetries or other physical properties that engender stable long frequencies, and about which further knowledge is conventionally limited. Lakoff speaks of metaphors. Probability models are precise metaphors. There are two governing metaphors for the ideal cases, which lead to two kinds of extension from the prototype. In one direction the comparison is with the belief structure associated with ideal cases. That is the subjective direction. In another direction the comparison is with the objective properties of the prototype, and in particular with behavior on repeated actual or hypothetical trials.

We can even describe a fairly Lakoffian structure for this radial category, with extensions from the ideal case being close to the center when the Kolmogorov axiomatization is adequate, less central, as in many of the developments of Dempster and Shafer, when we have at most upper and lower probabilities, subject to a number of interestingly different axiomatizations.

What I have said merely repeats, in another jargon, the main idea in Shafer's part 7. I wanted only to say that there is nothing so special about probability. It is like most of our other concepts, a radial one, not characterized by necessary and sufficient conditions. I would not describe Shafer as re-unifying probability. I would say he is just reminding us what it is and has been since around 1660.

8. THE INSTITUTIONAL REUNIFICATION OF PROBABILITY

The diversity of statistics is one of its strengths. Any attempt to restore an hegemenous department of statistics could only harm the subject. Yes, let statisticians (those who identify themselves as such) again be more open, more willing to learn from other departments, more willing to think hard about the problems, both practical and conceptual, that arise whenever we try to reason with precision short of deduction, or to assess plans for deciding under uncertainty. If a department of statistics, frightened by the proliferation of its expertise, turns inward and dedicates itself to pure mathematics, it will lose its reason for existence. But statistics departments should not try to reclaim old territory. Let statistical thinking be done in many houses. Why should Shafer be so keen to "co-opt" people from other disciplines? Won't "co-operation" do? Why should there be one department that provides all the basic teaching in statistics? Contrary to the belief of Shafer and David S. Moore, statistics is not one of the liberal arts. It is part of logic, and logic, I remind you, is one third of the trivium of logic, grammar and rhetoric. I quite disagree with my own colleagues who want all students to take a basic course in logic and critical thinking in our philosophy department. I urge for others what I urge at home. Don't try to claim everything for yourself. I teach an elementary course on inductive logic and probability, which is much enriched by the fact that some of the students have picked up a little statistics in pharmacy, in physics, in archaeology, in computer science. The friction is great. Had they all learned their little statistics in the same department, from the same teachers, I would probably quit teaching the course; I don't want to teach serried ranks of bland and uniform young people.

There is all too much "reclaiming" in Shafer's vision of his subject. Most departments of statistics at research universities grant the Ph.D. Would Shafer want us philosophers to reclaim "our" degree? Shafer is something of a philosopher (rather more than something, in fact). I am delighted that such a philosopher is located in a School of Business. I do not want to co-opt him but to learn from him—as I have always done.

Comment

David S. Moore

Glenn Shafer alleges that our discipline is in some disarray, not only institutionally but intellectually. He traces this disarray to the "balkanization" of probability and urges as a solution a conceptual reunification of interpretations of probability. In presenting his case, he offers a most interesting glimpse at the recent surge of work on the history of probability and statistics. How shall we react to Shafer's diagnosis and to his proposed therapy? Subjectively, of course. For my part, I commend him for calling our attention to our history, accept with some hesitations his allegation of institutional disarray and remain unconvinced that whatever intellectual disarray (I would call it ferment) we face is a disease needing the treatment he proposes.

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OUR INSTITUTIONAL VITALITY

Shafer notes the extensive growth of both teaching and research about probability and statistics in other disciplines and the considerable contributions made by scholars in these fields. All true and all to the good. No fundamental intellectual method can be confined within a neat institutional framework.

The case of mathematics is instructive. Research that only the narrow-minded would distinguish from research in mathematics has long been carried out by scholars in many fields. A recent sample survey finds that over half of all students studying advanced mathematics are enrolled in courses taught outside of mathematics departments (Garfunkel and Young, 1990). Mathematics is simply too important to be left to mathematicians. Mathematics has undergone the fragmentation that Shafer laments institutionally as well as in research and teaching. This ought not to surprise us. The differentiation of once unified functions among diverse institutions is an essential