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# The Unity and Diversity of Probability

Glenn Shafer[1]

Mathematical probability and its child, mathematical statistics, are relative newcomers on the intellectual scene. Mathematical probability was invented in 1654 by two Frenchman, Blaise Pascal and Pierre Fermat. Mathematical statistics emerged from the work of the continental mathematicians Gauss and Laplace in the early 1800s, and it became widely useful only in this century, as the result of the work of three Englishmen, Francis Galton, Karl Pearson, and R.A. Fisher.

In spite of these late beginnings, probability and statistics have acquired a dazzling range of applications. Inside the university, we see them taught and used in a remarkable range of disciplines. Statistics is used routinely in engineering, business, and medicine, and in every social and natural science. It is making inroads in law and in the humanities. Probability, aside from its use in statistical theory, is finding new applications in engineering, computer science, economics, psychology, and philosophy.

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[1]This article is based on the author's inaugural lecture as Ronald G. Harper Distinguished Professor of Business at the University of Kansas, delivered on November 20, 1989. In revising the lecture for publication, the author has profited from comments from Paul Benacerraf, Theodore Chang, Arthur P. Dempster, Lorraine Daston, Nell Irvin Painter, Theodore M. Porter, G. Baley Price, Stephen M. Stigler, Geoffrey S. Watson, and Sandy L. Zabell. Research for the lecture and article was partially supported by grant IRI-8902444 from the National Science Foundation.

Outside the university, we see probability and statistics at use in a myriad of practical tasks. Physicians rely on computer programs that use probabilistic methods to interpret the results of some medical tests. The worker at the ready-mix company used a chart based on probability theory when he mixed the concrete for the foundation of my house, and the tax assessor used a statistical package on his personal computer to decide how much the house is worth.

In this article, I will sketch the intellectual history of the growth and diversification of probability theory. I will begin at the beginning, with the letters between the Parisian polymath Blaise Pascal and the Toulouse lawyer Pierre Fermat in 1654. I will explain how these authors, together with James Bernoulli, Abraham De Moivre, and Pierre Simon, the Marquis de Laplace, invented a theory that unified the ideas of belief and frequency. I will explain how this unity crumbled under the assault of the empiricist philosophy of the nineteenth century, how the frequency interpretation of probability emerged from this assault, and how a subjective (degree of belief) interpretation re-emerged in this century. I will discuss how these intellectual movements have supported the amazing diversity of applications that we see today.

I will also discuss the future. I will discuss the need to reunify the theory of probability, and how this can be done. Reunification requires, I believe, a more flexible understanding of the relation between theory and application, a flexible understanding that the decline of empiricism makes possible. I will also discuss the institutional setting for reunification—departments of statistics. Departments of statistics have been the primary vehicle for the development of statistical theory and the spread of statistical expertise during the past half-century, but they need new strategies in order to be a source of innovation in the twenty-first century. We need a broader conception of probability and a broader conception of what departments of statistics should do.

## I. The Original Unity of Probability

In this section, I will sketch how the original theory of probability unified frequency, belief, and fair price.[2]

In order to understand this unity, we must first understand a paradox. The original theory of probability was not about probability at all. It was about fair prices.

Probability is an ancient word. The Latin noun *probabilitas* is related to the verb *probare*, to prove. A probability is an opinion for which there are good proofs, an opinion that is well supported by authority or evidence.

Pascal and Fermat did not use the word probability in their 1654 letters. They were not thinking about probability. They were thinking about fair prices.

Here is the problem they were most concerned with, a problem that had been posed in arithmetic books for centuries, but that they were the first to solve correctly. You and I are playing a game. We have both put $5 on the table, and we have agreed that the winner will get all $10. The game consists of several rounds. The first player to win three rounds wins the game. I am behind at the moment—I have won one round, and you have won two—and I must leave to give a lecture. My wife Nell is willing to take my place in the game, taking over my position and my chance, such as it is, of winning the $10. What should she pay me for this chance? What is the fair price for my position in the game?

You have won two rounds to my one round. So perhaps you deserve two-thirds of the $10, and I deserve one-third. Pascal gave a different answer. He

[2]For details, see *A History of Probability and Statistics and their Applications before 1750*, by Anders Hald (Wiley, 1990), *Classical Probability in the Enlightenment*, by Lorraine Daston (Princeton, 1988), and *The Emergence of Probability*, by Ian Hacking (Cambridge, 1975).

said you deserve three-fourths, and I deserve only one-fourth. The fair price for my position in the game is only $2.50.

Here is Pascal's argument. Were we to play the next round, we would have equal chances, and if you were to win, you would get all $10. You are entitled to $5 right there. If you were to lose, we would be even, with two games each. So we should split the other $5 equally. That leaves me with only $2.50.

Probability theory got started from this kind of reasoning. Pascal and Fermat's basic ideas were published in a short but very influential tract by the Dutch mathematician Christian Huygens. Huygens, together with the French nobleman Pierre Rémond de Montmort and the Huguenot refugee Abraham de Moivre, found fair prices for positions in more and more complicated games. The Swiss mathematician James Bernoulli even found fair prices for positions in court tennis, the complicated indoor ancestor of modern lawn tennis.

There was no talk about probability at the beginning of this work. Only equal chances and fair prices. There wasn't even a number between zero and one (my probability of winning) in the discussion. Probability was another topic. Probability was concerned with evidence, and it was a qualitative idea.

It was nearly sixty years after Pascal and Fermat's letters, in 1713, that their theory of fair price was tied up with probability.[3] In that year, five years after James Bernoulli's death, his masterpiece *Ars Conjectandi* was published. Most of this book is about games of chance, but in Part IV, Bernoulli introduces probability. Probability is a degree of certainty, Bernoulli says, and it is related to

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[3]Some qualifications are required here. The idea of probability was already connected to Pascal and Fermat's theory in a general way in the very influential Port Royal Logic (*l'Art de Penser*, by Antoine Arnauld and Pierre Nicole, Paris, 1662). George Hooper used the word probability to refer to a number between zero and one in work published just before 1700 (see my article, "The Combination of Evidence," in the *International Journal of Intelligent Systems*, Vol. 1, 1986, pp. 127-135). It was the intellectual grounding provided by Bernoulli, however, that bound the idea of probability irrevocably to Pascal and Fermat's mathematics.

certainty as a part is related to a whole; *Probabilitas enim est gradus certitudinus, & ab hac differt ut pars à toto*. Just as the rounds you have won and lost in a game entitle you to a definite portion of the stakes, the arguments you have found for and against an opinion entitle you to a definite portion of certainty. This portion is the opinion's probability.

Bernoulli's introduction of probability was motivated by his desire to apply the theory of fair price to problems beyond games of chance—problems *in civilibus, moralibus & oeconomicis*—problems in domains where the qualitative idea of probability had traditionally been used.

This ambition also led Bernoulli to another innovation, the theorem that is now called the law of large numbers. Bernoulli knew that in practical problems, unlike games of chance, fair prices could not be deduced from assumptions about equal chances. Chances might not be equal. Probabilities in practical problems would have to be found from observation. Bernoulli proved, within his theory, that this would be possible. He proved that if a large number of rounds are played, then the frequency with which an event happens will approximate its probability.

Bernoulli's ideas were quickly taken up by Abraham De Moivre, who made them the basis of his book, *The Doctrine of Chances* (London, 1718, 1738, 1756), which served as the standard text for probability during the eighteenth century. The French mathematician Laplace extended De Moivre's work further, into the beginning of mathematical statistics. Laplace's *Théorie analytique des probabilités* (Paris, 1812, 1814, 1820), served as the standard text for advanced mathematical probability and its applications for most of the nineteenth century.

I cannot trace this development here. I do want to emphasize, however, 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. It is also the fair price, in shillings, say, for a gamble that will return one shilling if it does happen.

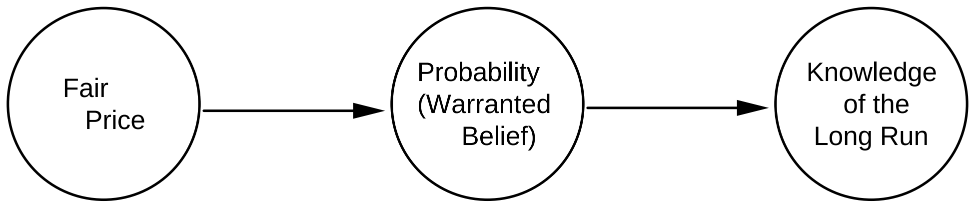


**Figure 1.**

Figure 1 summarizes the logic of the classical theory. Probability (i.e., degree of certainty or degree of warranted belief) was defined in terms of fair price, and long-run frequency (or more precisely, knowledge and belief about frequencies and other aspects of the long run) was derived in turn from probability. A whole mathematical structure goes along this route; the rules for mathematical probability derive from the properties of fair price, and the details of our knowledge of the long run derive from these rules.

## II. The Rise of Frequentism

If you had been asked, before you began to read this article, what mathematical probability means, what would you have said?

Most people would emphasize frequency in their answer. The probability of a fair coin landing heads is one-half, they might say, because it lands heads half the time. Defining probability in terms of frequency seems to be the proper, scientific, empirical thing to do. Frequency is real. You can go out and observe it. It isn't vague, mushy, and metaphysical, like “degree of certainty,” or “degree of warranted belief.”

If this is the way you think, then Figure 1 looks backwards to you. We should start with the facts, you will say. Start with the fact that the coin lands heads half the time. This is why even odds are fair. Don't start with some metaphysical idea about fair price and try to deduce facts from your metaphysics.

The pioneers of probability theory did not take this hardnosed empirical point of view, which seems so natural to you and me today. Our modern empiricism—positivism, it is sometimes called—is a fairly recent development in the history of ideas. It got started only in the nineteenth century.

In the case of probability, we can pinpoint 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 Friedrick 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. So Bernoulli's theorem, which goes through mathematical contortions to prove that probability is equal to frequency, is pure nonsense.[4]

Loath to give up Bernoulli's theorem, the mathematicians resisted this attack from the philosophers as best they could. Throughout the nineteenth century, we find probabilists defending Laplace's ideas.

Eventually, however, probability theory came to terms with the empirical spirit of the age. There are two parts to the story of this adaptation. One part of the story is about the application of probability—how a theory of statistics was developed that was suitable for the analysis of frequency data. The other part of the story is about the mathematics of probability—how the mathematical theory was adapted to the frequentist interpretation of probability, so that you could be a

[4]For a fuller account of the thinking of Mill, Ellis, Fries, and their allies and opponents, see pp. 77-88 of Theodore M. Porter's *The Rise of Statistical Thinking, 1820-1900* (Princeton, 1986).

frequentist and still prove Bernoulli's theorem. Both parts of the story take us through the end of the nineteenth century into the twentieth.

***Statistics.*** In order for probability to be empirical, it should be about actual populations and actual variation in populations. But the technical content of the mathematical theory of probability in the early nineteenth century was not well adapted to the study of variation. The ideas of correlation and regression, which statisticians use nowadays to study variation, were not worked out until the end of the nineteenth century.

At the beginning of the nineteenth century, the best developed application of probability theory was in the analysis of errors of measurement, used in astronomy, geodesy, and other areas of natural science. It was in this error theory that one found the normal distribution, the bell-shaped curve that statisticians now use in the study of variation. Adolphe Quetelet, the Belgian polymath who tried to apply the normal distribution to social statistics in series of publications from 1827 to 1870, ultimately failed because he was unable to escape from the conceptual setting of error theory. Just as the astronomer's measurement was an approximation to an ideal true value, Quetelet saw each individual in his human populations as an approximation to an ideal type. Quetelet's ideal was the average man.

The concepts of correlation and regression were finally discovered in the course of the study of heredity, by the Englishman Francis Galton. Galton's work was refined into a statistical methodology at the turn of the century by Karl Pearson and R.A. Fisher. All three of these scholars were genuinely interested in variation, because they were eugenicists. They were not content to regard the average Englishman as the ideal Englishman. They wanted to take advantage of variation to improve the race.[5]

Once the basic ideas of correlation and regression were developed, the particular problem of eugenics faded from the center of mathematical statistics. There is variation everywhere. Yet the frequentist statistical methodology developed by Pearson and Fisher is still the core of statistical theory.

***Mathematical Probability.*** The mathematical theory of probability was adapted to frequentism in a less direct way. The key to the adaptation was the philosophy of mathematics of the great German mathematician David Hilbert (1862-1943).

Roughly speaking, Hilbert believed that mathematics is a formal exercise, without any essential connection to reality. The business of mathematics, he held, is the derivation of formal mathematical statements—mere strings of symbols, really—from other formal mathematical statements. Getting mathematics right is a matter of making sure the derivations follow certain rules.

Hilbert's ideas inspired an effort to base all of mathematics on axioms, like the axioms you learned for plane geometry in high school. Most advanced mathematics, it turns out, can be built up axiomatically starting with set theory, the abstract theory of "groups of things" invented by the German mathematician Georg Cantor (1845-1918). Many of the less mathematical readers of this article will remember set theory from the late 1960s and the early 1970s, when the "new math" brought it into the elementary schools in this country.

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[5]The story I sketch so briefly here is told in depth by Porter and by Stephen M. Stigler in *The History of Statistics: The Measurement of Uncertainty before 1900* (Harvard, 1986). The influence of eugenics on the development of mathematical statistics is discussed by Donald A. Mackenzie in *Statistics in Britain, 1865-1930: The Social Construction of Scientific Knowledge* (Edinburgh, 1981).

In the case of probability, the reduction to set theory was completed only in the late 1920s and early 1930s. The definitive formulation was by Andrei Kolmogorov, the great Russian mathematician who died in 1987. Kolmogorov's axioms for probability are extremely simple. They treat events as sets, and probabilities as numbers assigned to these sets, and they require that these numbers obey certain rules. The main rule is additivity. The probabilities of disjoint events add.

Kolmogorov's axioms have been extremely successful as a basis for the further development of mathematical probability. They have freed mathematicians from all the paradoxes and confusions that bedevil the interpretation of probability, leaving them a clear view of their purely mathematical problems. The axioms have been so successful, in fact, that pure mathematicians often proclaim, with a straight face, that “probability began with Kolmogorov.”

I will not inflict on you the notation required to state Kolmogorov's axioms. I do want to point out, though, that these axioms take us far away from the setting in which Pascal and Fermat began, where repeated rounds of a game are played and prices and hence probabilities change. Kolmogorov's axioms are about a single probability space. Neither price nor repetition are fundamental now; they are both arbitrary elements added on top of the basic foundational axioms.

## III. The Role of the Statistics Department

Probability and statistics have become so broad that in order to understand their development in the twentieth century, we must focus on institutions rather than on individual scholars.

Though the new statistics was invented in Britain, it was taken up as a practical methodology more quickly in the United States than in Britain itself.

Leadership in statistical theory, on the other hand, remained in Britain until the Second World War. American strength in statistical methodology might be attributed to our practical spirit, but it was also due to the flexible organization of American universities.[6] American weakness in statistical theory can be attributed, paradoxically, to our relatively impractical mathematics. The American drive to match the best mathematics of Europe had led by 1900 to a dominant role for pure mathematics in American mathematics, and that dominance has persisted within our mathematics departments throughout the century.[7]

What were the reasons for this dominance of pure mathematics? Folklore tells us that the Americans did not feel they could compete with the Europeans in applied mathematics. Our graduate students were unwilling to spend the time needed to master both mathematics and a field of scientific application, and our universities lacked the depth in science of the European universities. We could make a mark on world mathematics only by working as far as possible from applications.[8]

The Second World War did bring leadership in statistical theory, along with leadership in most scientific fields, to the United States. Many of the leading

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[6]Joseph Ben-David, in *The Scientist's Role in Society: A Comparative Study*, (Prentice-Hall, 1971) discusses how the departmental organization of American universities allowed the rapid development not only of statistics but also of other new fields.

[7]Garrett Birkhoff lists Thomas S. Fiske, E. H. Moore, William F. Osgood, Maxime Bôcher, and Henry Burchard Fine as the most prominent of the pure mathematicians who took over the leadership of the American Mathematical Society around 1900 ("Some Leaders in American Mathematics: 1891-1941," in *The Bicentennial Tribute to American Mathematics, 1776-1976*, edited by Dalton Tarwater and published by the Mathematical Association of America, 1977). For further information on the development of American mathematics, see the three volume collection, *A Century of Mathematics in America*, edited by Peter Duren and published by the American Mathematical Society in 1989.

[8]This folklore deserves serious historical examination. This would require both assessment of the possibilities in applied mathematics at the turn of the century and much archival work. In their public declarations, Fiske and his colleagues expressed strong support for applied mathematics.

European theoretical statisticians, including Jerzy Neyman and Abraham Wald, immigrated to the United States before or during the war, and our military invested heavily in statistical theory. Our universities accommodated this move into statistical theory not by changing the character of their departments of mathematics but by creating departments of statistics.

The rationale for the statistics department was worked out in the late 1930s and early 1940s by a remarkable group of American statistical statesmen, including Harold Hotelling, Jerzy Neyman, W. Edwards Deming, Burton H. Camp, S. S. Wilks, Walter Bartky, Milton Friedman, and Paul Hoel. It was articulated by Hotelling in two famous lectures, "The Teaching of Statistics," delivered at Dartmouth in 1940, and "The Place of Statistics in the University," delivered at Berkeley in 1946.[9]

In Hotelling's design, the statistics department is a bridge between mathematics and the disciplines in the university that use statistical methods. This bridging role can be seen in the undergraduate curriculum of the department, in its graduate curriculum, and in its faculty's research.

Most statistics departments have relatively few undergraduate majors; they play a service role at the undergraduate level, while relying on mathematics departments to train undergraduates for their own graduate programs.

The graduate curriculum is divided between mathematical probability, with at least a few courses taught at the most austere level, and statistics, with a few basic courses taught abstractly and others in a more practical spirit. Thus each doctoral student is forced to make for him or herself the journey from mathematics to applications.

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[9]The written versions were published in 1940 and 1949, respectively. They were reprinted, along with comments by some of today's leaders in statistics, on pp. 57-108 of *Statistical Science*, Vol. 3, No. 1, February 1988.

The faculty for courses in mathematical probability often have joint appointments with the mathematics department; sometimes they are simply drawn from the mathematics department. More importantly for the university, the statistics department seeks joint appointments with other departments that use statistics, from electrical engineering and geology to psychology and educational research. The faculty with joint appointments in these user departments generally have degrees in statistics and regard statistics as their primary home. Their role is to transfer the latest statistical methodology to potential users. They also provide for communication in the opposite direction; by consulting in particular applied fields, statisticians develop interests in new statistical problems in those fields, and they communicate these problems, along with their own attempts at solutions, to their statistical colleagues.

Hotelling's design has been very successful. There are now over sixty statistics departments in this country, generally at the larger public and private universities. Smaller colleges cannot afford statistics departments; but they have followed the lead of the statistics departments with various joint departments and degree programs. All told, degrees in statistics are given by over two hundred colleges and universities in the United States.

## IV. The Revival of Subjective Probability

Statistics departments are a product of frequentism, and the teaching in statistics departments is still predominantly frequentist in philosophy. Yet frequentist statistical theory has its difficulties and limitations, and these have become increasingly obvious with age. I cannot detail these shortcomings here, but I must point out that they have led to a resurgence of subjective ideas within statistics during the past thirty years. Since the publication of L.J. Savage's *Foundations of Statistics* (Wiley) in 1954, a minority of statisticians (the

"Bayesians") have revived the view that probability means degree of belief. The Bayesians have had a great impact not only in statistics, but also in economics, psychology, computer science, business, and medicine.

The intellectual foundation for this subjectivist revival was laid earlier, in the 1920s and 1930s, by the English philosopher Frank Ramsey and the Italian actuary Bruno de Finetti. Ramsey and de Finetti saw a way to make degree of belief, as opposed to frequency, respectable within positivist philosophy. We can give degree of belief an empirical, behaviorist interpretation by insisting that people be willing to bet on their beliefs. A degree of belief of 2/3 in rain, for example, can be interpreted as a willingness to take either side of a 2-to-1 bet on rain.

The revival of the subjective interpretation was facilitated, paradoxically, by Kolmogorov's axioms. Though these axioms were meant by Kolmogorov as a mathematical foundation for the frequentist interpretation, their formality makes them equally susceptible to a subjective interpretation. Indeed, since they do not require a structure for repetition, the axioms play into de Finetti's contention that repetition is not necessary for mathematical probability to be meaningful. In Kolmogorov's framework, structures for repetition are built on top of the axioms and are therefore optional. In the new subjective theory, repetition is optional in the interpretation of the theory as well.

Within statistics, Bayesianism amounts to a minority view about how to solve the standard problem of modelling statistical variation. We just add to the class of models we are considering some prior subjective probabilities about which model is correct. But beyond statistics, Bayesianism cuts a wider swath. During the past thirty years, it has allowed probability to penetrate into areas where statistical modelling is inappropriate because statistical data is unavailable, but where evidence is sufficiently complicated to make quantitative judgments useful.

The best known practical Bayesian technique is the decision tree, which originally appeared in Abraham Wald's frequentist statistical decision theory, but which, since the late 1950s, has been used more and more with subjective probabilities. Subjective decision trees have long been a standard topic in the undergraduate business curriculum, and now they are spreading to many other fields, including medicine and engineering.

Bayesian decision theory, the abstract version of subjective decision trees, has also become influential in philosophy and psychology. Philosophers debate whether Bayesian methods constitute a standard of rationality, and psychologists study the extent to which they describe actual human behavior under uncertainty.

The greatest impact of the revival of subjective probability has come in theoretical economics. The Bayesian model of rationality has found a role in a plethora of micro-economic models during the past twenty-five years, and in the past ten years it has had a growing role in macro-economics as well.

I must also mention the new and growing influence of subjective probability in artificial intelligence. Since its inception in the 1950s, this branch of computer science has seen symbolic logic as its principal mathematical tool. But in the past ten years, the desire to build expert systems in areas where uncertainty must be explicitly managed has inspired new interest in subjective probability judgment, and new work in probability theory. This new work provides a new perspective on probability, a perspective that puts much more emphasis on the structure of conditional independence than on numbers. It has also stimulated new work on the theory of belief functions, an alternative theory of subjective probability on which I have worked for many years.[10]

## V. The Balkanization of Probability

I have been painting a picture of intellectual vitality. The mathematical theory of probability has been flourishing, spilling over all disciplinary and institutional boundaries. But this wild growth has its negative aspects. Conceptually and institutionally, probability has been balkanized.

Twenty-five years ago, the statistics department was clearly the intellectual center of probability. Those in other disciplines who wanted to use probability or statistics came to the statistics department to study these subjects. Those who were concerned about the meaning of probability came to the statistics department to hear the debate, then still fresh and stimulating, between frequentists and Bayesians. Today, the picture has changed. On both the practical and philosophical sides, many of the new developments in probability are now taking place outside the statistics department.

Within statistics, we still treat introduce probability theory either as a prelude to statistical modelling or as a prelude to probability as pure mathematics. "Probabilist," to us, still means mathematician. We have not adapted our teaching to serve students who want to use probability in computer science, engineering, or theoretical economics. Consequently, new traditions for teaching probability are growing up within these disciplines. Whereas our approach once provided a common language for all areas of application, it is now in danger of being reduced to one voice in a tower of Babel.

[10] See *Readings in Uncertain Reasoning*, edited by Glenn Shafer and Judea Pearl (Morgan-Kaufmann, 1990).

On the philosophical side, the debate between frequentists and Bayesians within the statistics department has calcified into a sterile, well-rehearsed argument. The real debate has moved outside the statistics department, and the main divisions over the meaning of probability now follow disciplinary lines. Frequentists predominate in statistics and in the experimental sciences, while Bayesians predominate in the professional schools, theoretical economics, and artificial intelligence. To most Bayesians, the debate in statistics now seems parochial—it is concerned only with statistical modelling, not with the larger issues. Most frequentist statisticians, on the other hand, see Bayesians in other disciplines as cranks. Today business and engineering schools, in their brashness and practicality, may do more than statistics departments to bring together the broad range of interpretations of probability.

In addition to failing to occupy the new ground of probability, the statistics department is also losing much of the ground it did occupy. Its role as a bridge from mathematics to users of statistics in engineering and the sciences has declined over time. This is due in part to the growth of statistical expertise within these disciplines, which makes the outside specialist less needed.

Hotelling argued that students in all fields that use statistics should take their first course in statistics from the statistics department. Only the statistical specialist, he argued, would have the mathematical grasp of statistical theory needed to teach the subject well. We have clung to the element of truth in Hotelling's argument, but in most universities we have lost the argument. This is only partly because the level of mathematical competence in the other disciplines has risen. It is also because progress has changed what the disciplines want their students to learn in the introductory course. Students in the social and biological sciences who come to learn statistics now surely deserve to be taught not only the logic of the subject but also the decades-long record of its successes and failures in their discipline. If the statistics department cannot undertake this task, the disciplines must.

The growing isolation of the statistics department is due in part to its mathematization. Perhaps any discipline that serves as a bridge between mathematics and an area of application will tend, once the leadership of its founders is gone, to move back towards mathematics. It is clear that this has happened in statistics. Today most of the articles in the leading statistics journals are so mathematical that the postwar founders of our statistics departments would not be able to read them, and so impractical that they would not want to. The joint appointments that made statistics departments so influential in the 1950s and 1960s have become difficult to replicate. When we look at the departments where these appointments were most successful, such as Stanford and Wisconsin, we find that few such appointments were made in the 1970s and 1980s. Younger statisticians have had to concentrate on their mathematics in order to be recognized as first-rate.

I want to mention one more area in which the leadership role held by statisticians through the 1970s has been wrested from us. This is in our own history. When ours was a young and brash field, we controlled our own history by default. No one else cared. We didn't really know much about this history, but we told what we knew with authority. This, too, has changed. Starting with the philosopher Ian Hacking's book *The Emergence of Probability* (Cambridge) in 1975, we have seen our history taken over by philosophers and professional historians of science. In the past decade, we have seen more books and articles on the history of probability and statistics than were published during the entire preceding existence of these subjects. The more technical of these works still tend to be written by statisticians, but most of the books that try to describe the big picture are now written by historians with relatively little technical training.

The story they tell sounds very different from the story we once told. Whereas we saw probability and its progress from the inside, the historians see probability and its vicissitudes as the result of larger cultural and historical forces.

I do not believe that the balkanization of probability is a good thing. We need ways of understanding the unity that still exists in probability, and we need an institutional center for probability and statistics. We need institutions that can bring together divergent tendencies in philosophy and application, so that these tendencies can learn from each other.

In the remainder of this article, I will be concerned with how probability can be reunified. On the conceptual side, I will sketch how we can recreate an understanding of probability that has room for both frequentist and Bayesian applications, without the worn-out dogmas of either group, and also room for the newer applications. On the institutional side, I will advance some theses about what our statistics departments must do to recover their leadership role.

## VI. The Conceptual Reunification of Probability

On the conceptual side, I believe we can go back to the original unity of belief and frequency. The positivism that drove these two aspects apart no longer holds quite the sway that it held in the nineteenth century. For the positivists, every element of a theory—every object in the theory and every relation between objects—had to have a definite, verifiable, empirical reference. Today, it is possible to be more flexible about the relation between a theory and its application.

The point is that we can apply a mathematical theory to a practical problem even though it does not model that problem empirically. In order to apply a theory to a problem, it is sufficient that we relate the problem, perhaps even very indirectly, to another problem or situation that the theory does model.

The only situation that the mathematical theory of probability models directly is still the very special situation studied by Pascal and Fermat, the special situation where we flip a fair coin or play some other game with known chances. In this special situation, which I call the *ideal picture of probability*, the unity of belief and frequency is unproblematic. If we know the frequency with which a coin lands heads, this known frequency is a sensible measure of the degree to which we should believe it will land heads on any particular flip.

Whenever we use mathematical probability in a practical problem, we are relating that problem, in one way or another, to this ideal picture. In my forthcoming book, *The Unity and Diversity of Probability*, I argue that the different ways Bayesians, frequentists and others use probability should be thought of as different ways of relating problems to the ideal picture. Much standard statistical modelling amounts to using the ideal picture as a standard of comparison. Statistical arguments based on sampling or experimental randomization depend on artificially generated random numbers which simulate the ideal picture, and they relate this simulation to real problems in clever ways. Bayesian arguments can be thought of as arguments by direct analogy to the ideal picture. And arguments based on the theory of belief functions involve analogies that are less direct.

Recognition of this continuing centrality of the ideal picture will allow us to move back to unified understanding that we find in Pascal, Fermat, Bernoulli, and De Moivre. We can insist on the unity of belief and frequency in the ideal picture even while admitting that they go their separate ways in many applications.

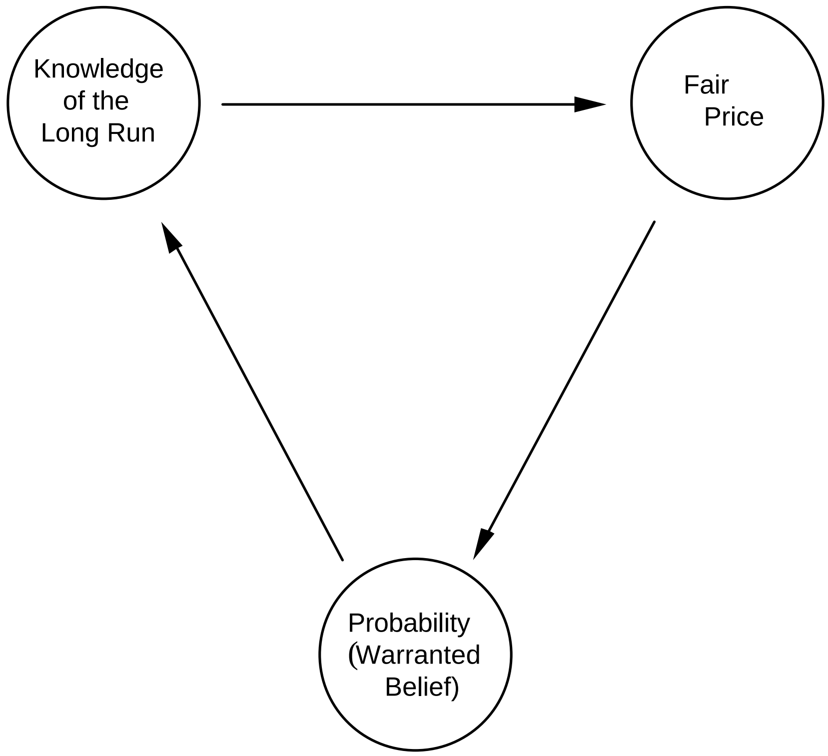


**Figure 2.**

We cannot simply return to the mathematics of the seventeenth and eighteenth century, for we have learned much since that time. We can, however, reformulate the mathematical foundations of probability in a way that incorporates rather than ignores the pioneers' insights into the fundamental role of fair price and repetition. In *The Unity and Diversity of Probability*, I argue for reformulating Kolmogorov's axioms in the framework of a sequence of experiments, in which the mathematics of Figure 1 can be recaptured.

We do need to go beyond Figure 1 in one important respect. Work in the twentieth century by the frequentist scholars Richard von Mises, Jean Ville, and Abraham Wald has shown that the theory of probability can be developed mathematically starting with knowledge of the long run, which includes both knowledge of long-run frequency and knowledge of the impossibility of gambling schemes.[11] Thus Figure 1 can be expanded to Figure 2, which shows fair price, belief, and frequency bound together in a triangle. From a purely mathematical point of view, any point in this triangle can be taken as an axiomatic starting point, but from a conceptual point of view, none of these starting points can stand on its own. The axioms or assumptions that we must set down when we start from any one of the starting points can be justified only by reference to the other ideas in the triangle.[12]

## VII. The Institutional Reunification of Probability

What can be done to make the statistics department once again the intellectual center of probability?

The well-worn answer is that we should try harder to live up to Hotelling's design. We should teach better, so that other departments will send their students to us rather than developing their own probability and statistics courses. We should play university politics better, so that these departments are not allowed to develop their own courses. We should examine the mathematics we do more critically, to make certain it is relevant to applications. We should produce more statisticians who are so bright that they can meet today's standards for mathematical accomplishment while continuing the tradition of being involved in practical problems.

Our best statistics departments are doing these things. This has not been enough, however, to stop the balkanization I have described. I believe the time

[11]See Per Martin-Löf's article, "The Literature on von Mises' Kollektivs Revisited," in *Theoria*, Vol. 35, No. 1, 1969, pp. 12-37. I should also mention closely related recent work on using complexity as a foundation for probability. See "Kolmogorov's Contributions to Information Theory and Algorithmic Complexity," by Thomas M. Cover, Peter Gacs, and Robert M. Gray, *The Annals of Probability*, Vol. 17, No. 3, July 1989, pp. 840-865.

[12] See my article, "The Unity of Probability," on pp. 95-126 of *Acting Under Uncertainty: Multi-disciplinary Conceptions* (George M. von Furstenberg, ed., Kluwer, 1990).

has come to address the problem directly. We need a new conception of the statistics department, one that suits our times.

The new statistics department should assess and absorb into its teaching and research what other disciplines have learned about probability and statistics. The department's introductory teaching of probability, at both undergraduate and graduate levels, should be comprehensive enough to serve all users. The department should teach not only the logic of statistics, but also the issues involved in its application. In our undergraduate statistics courses, we should try to assess past performance and future prospects for statistics in each of the disciplines we serve. Our graduate teaching should include comparative assessment of the possibilities for statistics in different fields, including both the disciplines we serve within the university and fields that we serve outside the university, such as the census.

Our best statisticians have been willing to evaluate the uses to which other fields put statistical ideas.[13] The discipline of statistics has failed however, to produce broad assessments of the use of statistics in different fields, with examples of successes as well as failures, and with lessons for practitioners. Such assessments should be a major research and teaching goal for the new statistics department.

Where can we find the faculty for these tasks? We do not have to look far. We must co-opt the historians, computer scientists, philosophers, economists, psychologists, and others who are contributing to our understanding of probability and statistics. We must enlist faculty from these disciplines to help us in our teaching mission, at both the undergraduate and graduate levels. We

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[13]See, for example, the debate between John Tukey and the economists Heckman and Robb in *Drawing Inferences from Self-Selected Samples*, edited by Howard Wainer (Springer-Verlag 1986).

must make dissertations and careers concerned with aspects of probability and statistics that have been developed in these other disciplines possible within statistics.

In some cases we should recruit people trained in these disciplines as full-time members of the statistics department. In other cases, we should ask them, in their role as faculty members in another department, to serve on an advisory committee for statistics and teach courses in the statistics department. In other cases, we should seek joint appointments. In the past, we have thought of joint appointments as a way for statistics to contribute to other disciplines. We must now think of them also as a way for other disciplines to contribute to statistics.

Computer science is one of the first disciplines with which we should seek joint appointments. Probability has begun to play a whole spectrum of roles in computer science, from a tool in the evaluation of algorithms to a model for distributed processing to a model for learning and inference in artificial intelligence. This, together with the ever increasing role of computing in both theoretical and applied statistics, makes it essential that ties between statistics and computer science be cultivated.

History is another field with which we need reciprocal ties. Historians need our help, for in recent decades they have joined the social sciences as users of statistics. We need their help in order to carry out the assessments of statistical practice that I am advocating. Our task is to write the history of probability and statistics in the twentieth century.

I subscribe to David S. Moore's thesis that statistics belongs among the liberal arts.[14] I believe, moreover, that we cannot teach statistics as a liberal art unless we practice it as a liberal art. The research and graduate program in the

[14]See Moore's contribution to the discussion cited in Footnote 9.

statistics department should include real attention to the history and philosophy of probability and statistics.

The proposals I have just made are far-reaching. Their implementation will not be easy or painless. It will take many years to reshape our curriculum in the directions I have suggested, and when this has been accomplished, we will have to deal with much more diverse colleagues and students than we have dealt with in the past. Evaluation of students and faculty will be more difficult and possibly more contentious. The new statistics department will not work without leadership.

Is it necessary to take so difficult a path? Many statisticians do not share my conviction that the survival of the statistics department is threatened by the balkanization of statistics. They are willing to cede the new applications of probability and the more mundane topics of applied statistics to other departments, confident that the statistics department will remain indispensable as a home for those at the forefront of research in mathematical statistics. The need for this research seems to guarantee the survival of the statistics department.

I believe this is true in the short run. But those who would rely on the prowess of a mathematical elite for the survival of statistics as a separate discipline should look over their other shoulder. Since the David Report in 1984,[15] the mathematics community in this country has taken remarkable strides in broadening its conception of its subject. Incredible as it seems to those of us who studied mathematics in the heyday of American fascination with the mythical

[15]"Renewing U.S. Mathematics: Critical Resources for the Future." This report was prepared by the National Research Council's Ad Hoc Committee on Resources for the Mathematical Sciences, under the chairmanship of Edward E. David, Jr., the former Science Advisor to the President of the United States. It was published on pp. 434-466 of the *Notices of the American Mathematical Society*, Vol. 31, No. 5, August 1984. It put strong emphasis on the applications of mathematics, and it has been credited with reversing a decades-long slide in federal funding for mathematics.

French pure mathematician Bourbaki, many American mathematicians are now broadly interested in applications. It is conceivable that in the next generation we will see mathematics departments capable of interacting with a broad range of disciplines. Were this to happen, the independence of elite departments of very mathematical statisticians would no longer make sense. Such departments would be reabsorbed into mathematics.

As stewards of a legacy from a line of giants stretching from Pascal to Hotelling, we should not relish such an outcome. Statistics, as a discipline, has proven fruitful because it has had an intellectual basis broader than mathematics. Because it has been rooted in the practical and philosophical problems of inference as well as in mathematics, statistics has been able to play a leadership role extending throughout the sciences. Our goal today should be a renewal of statistics that will keep it in this position of leadership.

# Rejoinder to Comments

I am honored that *Statistical Science* has published my inaugural lecture, together with thoughtful comments from a very distinguished group of readers. These readers include Ian Hacking, who is probably the most prominent living philosopher of statistics, and six prominent members of leading statistics departments. I want to thank the editor, Carl Morris, for recruiting these readers, and I want to thank them for the thought and care they have devoted to reading and discussing my lecture.

My chair in the School of Business at the University of Kansas was endowed by Ronald G. Harper, a Tulsa businessman who designs and sells expert systems that combine statistical and artificial intelligence methods to help retailers locate stores, control inventory, and set prices. In appointing me to this chair, the university was recognizing my work on probability in expert systems. My first thought, when I was asked to acknowledge the appointment by giving a lecture to a general audience, was that I should discuss the relation between probability and statistics on the one hand and artificial intelligence on the other. I soon realized, however, that my audience knew so little about these topics that I could not cover them all. Much of the audience would not even be aware that some universities have whole departments devoted to statistics. So I left out artificial intelligence. I talked about the intellectual history of probability and statistics and the why and how of statistics departments. I closed with some suggestions for reinvigorating statistics departments.

It takes some chutzpah for a business professor from the provinces, who has not taught in a statistics department for fourteen years, to give prescriptions for reforming statistics, and I think it is a tribute to the openness of our discipline that my ideas have been taken seriously by prominent statisticians.

In their comments, the statisticians have focused on the role of applied statistics. These comments seem very important to me, for applied statistics is crucial to the future of our discipline, and the discussants have the stature to help shape this future. In this rejoinder, I hope to raise some questions that will encourage yet further discussion.

I will begin by responding to Ian Hacking's critique of my sketch of the history and philosophy of probability. Then I will turn to the central questions we should ask about applied statistics: What is its intellectual content, and how can we raise its perceived stature vis-à-vis the mathematics of statistics?

## The History and Philosophy of Probability and Statistics

Ian Hacking's work on the philosophy and history of probability and statistics has been an inspiration to me since my graduate work in the early 1970s, and I have prized his personal encouragement of my own work in these fields. I am honored, therefore, by his serious and extended response to my lecture.

Hacking quite rightly takes me to task for my simplifications of history. It is true that we cannot compress the rise of frequentism into the years 1842 and 1843, that positivism was not a simple phenomenon, and that hints of frequentism can already be found in Laplace and even more clearly in Poisson. He has passed over in silence some other equally egregious simplifications. I should mention in particular my simplified account of David Hilbert's ideas on the foundations of mathematics, for several people have protested to me privately that this account comes closer to popular caricature than careful analysis. (For careful analysis, see Benacerraf and Putnam (1983).)

Hacking's most important point is that the rise of frequentism had complex roots, going beyond positivism. He sees the early nineteenth century's emphasis on measurement as something broader than positivism, and he points to the proliferation of published statistics as a source of frequentism. I find myself only half convinced by his arguments on these points. Why did people in the nineteenth century want to measure frequencies instead of beliefs? And why did statistics on suicides seem stable? The Ian Hacking of fifteen years ago would have seen here the influence of theory on desires and perceptions, and intellectual fashion notwithstanding, I think that theory is still part of the story. A complete understanding of the rise of frequentism must take into account the mathematical possibilities of probability theory and the internal logic of the mathematician's drive to apply this theory. Bernoulli's motivation for the law of large numbers was his desire to apply probability theory to social and economic problems, and this same motivation encouraged nineteenth-century frequentism. Probability theory seems to have a very narrow scope of application if every probability must be both a frequency and a belief, as the probabilities in fair games are. It has a much wider scope of application if every frequency is a probability.

I agree with Hacking that probability is similar to many other concepts, in that it has unified prototypes that generalize in different directions. But when so much effort has been devoted to denying either the subjective or objective side of probability, it seems Polyannaish to say that the concept has always been unified.

The desire to apply probability widely still impels people to narrow its definition. Just as nineteenth-century frequentists saw opportunities to apply probability in situations where there are frequencies but no beliefs, twentieth-century subjectivists see opportunities in situations where there are reasons to believe but no frequencies. I point out the decline of positivism not simply to encourage my readers to send frequentism down with it. Rather, I am arguing that this decline permits us to escape from the idea that broadening the application of a concept requires us to narrow its meaning. We can now take a broader view of the relation between theory and application. We can describe a theory itself in a more unified way and see its diversity as a diversity of uses.

George Box's views about distinct ways of using probability in statistical inference fit here. In fact, his views have influenced my formulation. But there are more than Box's two ways of using probability. I listed several in my lecture.

The standard axioms for mathematical probability, formulated by Kolmogorov, do not presuppose any collection of data or structure of repetition; instead they appear to deal with the probability of a single event. As Hirotugu Akaike points out, this makes mathematical probability an awkward foundation for statistics.

Yet Kolmogorov's axioms are not faithful, in this respect, to the original thinking of Pascal, Fermat, and Huygens. My proposal for reunifying probability theoretically (or re-emphasizing its unity, if you prefer) involves bringing repetition back into the axiomatic framework. As David Moore notes, I have not given enough detail about the axiomatization I have in mind for my readers to evaluate it. I wish I could report that I have corrected this by completing my book on the unity and diversity of probability. Unfortunately, this book is still “forthcoming.” However, I did present my axiomatization in one form in an appendix to Shafer (1985).

## Who Should Do History?

Hacking makes a plea for leaving history to the historians. Philosophers make bad historians, he says, and so do statisticians. Historical research is best done by historians trained in archives.

Here I disagree. The conclusion is wrong, and the arguments are terrible.

Many historians (my wife, for example) wince at the suggestion that learning to do research in archives is the most important part of their training. Historians do need to be able to examine and judge evidence about the past, but they also need perspectives that will enable them to make sense of the past, and their task always involves relating the past to the present. Historians say that each generation rewrites history. Each generation must redo the French Revolution in light of its own understanding of politics and economics. Each generation must also redo Bernoulli in light of its own understanding of the law of large numbers.

It is disingenuous of Hacking to claim that his writings on the past are not history, for they are history, and very influential history at that. Historians need theses of kind that Hacking formulates, and they must test these theses against the evidence. If his theses sometimes do not stand up to a careful examination of the archives, then Hacking must take his lumps; he cannot escape by taking the word “history” out of his books. Moreover, statisticians sometimes make very good historians. Stephen Stigler is one of the best historians of statistics ever—perhaps the very best. He is unsurpassed in the care and judiciousness with which he examines the archives.

Let statistics be done in many houses, Hacking proclaims. History, too, should be done in many houses. But we need ways to bring these houses together. The history of statistics, in order to be done as well as it should be, needs talents and training that we now find scattered in history, philosophy, statistics, and elsewhere.

In my lecture, I suggested that statistics departments seek joint appointments with history departments, in order to strengthen our own understanding of our past and present. Historians also need these ties. The recent burst of work on statistics by historians is valuable, but it suffers from its exclusively externalist approach. (See my reviews of Porter's *Rise of Statistical Thinking* and Gigerenzer et al.'s *Empire of Chance*.) The next step in understanding the development of probability and statistics in the seventeenth, eighteenth, and nineteenth centuries should be to integrate what these historians have done with a deeper understanding of the internal possibilities of the subject. As for the history of probability and statistics in the twentieth century, the historians have scarcely been able to get started by themselves, because their training in our discipline is not yet adequate.

In general, historians have not hesitated to reach out to other disciplines for new perspectives. History departments often have joint appointments with other departments and programs in the humanities and social sciences, including American studies, area studies, women's studies, African-American studies, sociology, and economics. I believe that historians of science would welcome opportunities for similar interactions with statistics. Joint appointments between history and statistics could lead to a crop of historians capable of writing the history of twentieth-century statistics, a result that would serve both disciplines well.

## The Stature and Intellectual Content of Applied Statistics

A prominent statistician said to me recently that the central problem facing our discipline is how to raise the stature of applications vis-à-vis mathematics. This is reflected in the comments here. Hirotugu Akaike, George Box, Art Dempster, and David Moore all demand more respect for applied statistics and the logic of statistics.

Why has this problem arisen? Why has prestige within our discipline moved in the direction of mathematics?

In truth, mathematics has always held the most prestigious position in the discipline. In Hotelling's model, mathematics was already the more or less exclusive intellectual foundation for statistics. Yes, the applied statistician needed "additional resources" (to use George Box's term), but these resources were to be obtained from experience. They were not the subject of graduate courses in the statistics department or of articles that would merit promotion in the discipline. In Hotelling's time, however, it was easier to combine mathematics flashy enough to gain prestige—within the discipline, at least—with applied concerns. As time has passed, an inevitable process of competition and theoretical progress has pushed up the requirements for mathematical recognition within the discipline, to the point where the applied statistician can no longer meet them in his free time.

In my lecture, I advance the thesis that we must make the other aspects of statistics part of its *intellectual* foundation. We must make Box's "additional resources" a subject of intellectual study—a proper topic for courses, books, and articles, a speciality in which statistics departments recruit and promote faculty. I submit that unless we do this, applied statisticians will retain the second-rate status within statistics departments that Box bemoans.

Art Dempster and David Moore insist, and I agree, that there is a logic of applied statistics. How do we get it out of the heads of applied statisticians and into books, articles, and graduate courses, where it can compete with mathematics for the attention of our brightest graduate students? We must approach the task with the same professionalism with which we approach the task of teaching mathematics. We cannot simply ask applied statisticians to bring their war stories to the classroom. We cannot simply require our graduate students to endure summer internships. We must develop scholars who teach and write about how applied statistics is done. And in order to develop such scholars, we must go outside for help. Just as we went to mathematics departments for help in developing our mathematical scholarship, we must go to history, the sciences, and the professional schools for help in developing our scholarship about the realities of applied statistics.

I am convinced that we, more than the historians, need to learn the history of twentieth-century statistics. It is natural to assume that because we did it, we already know all about it. But we do not. Understanding requires effort. It requires observation, synthesis, and criticism, an effort of many minds, even many generations. We have made a tremendous effort, over several generations, to achieve an understanding of the mathematics of statistics. We have not made a similar effort to understand, in Dempster's words, the "active logic of the process of doing statistics."

Instead of history—instead of sustained thinking about the complexity of contemporary applied statistics and probability—we now have only intriguing ideas without elaboration. Brad Efron, for example, asserts that the fractionation of statistical thinking is an evolutionary adaptation of the statistical point of view to different data environments. He is probably right, but whose doctoral dissertation has pursued the idea and subjected it to criticism?

In private conversations, some statisticians have reacted to my suggestion that we try to teach the logic of statistics with, "Oh no, not courses in methodology." One statistician buttressed his skepticism by observing that he has been impressed, over the course of his career, by how naive and vacuous prominent and accomplished statisticians—applied and theoretical—can be when they turn their attention to methodology. This I can believe. How could it be otherwise when, as statisticians, they are trained only in mathematics?

## Misunderstandings

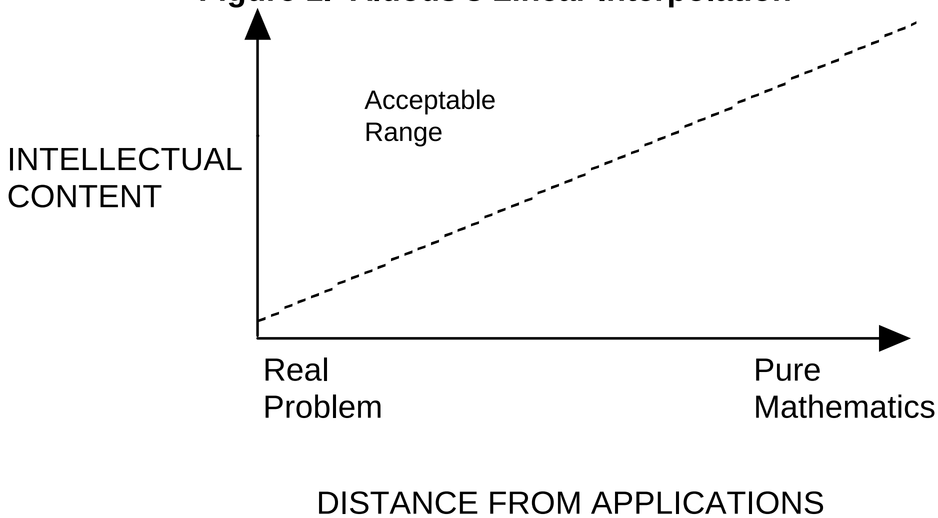
Our failure to pursue an intellectual understanding of applied statistics makes it difficult even for statisticians to talk to each other about the topic. It is difficult for applied statisticians to claim credit due, and it is difficult for theorists to avoid insulting applied statisticians. Since we know how to talk about the mathematical component of statistical reasoning and we do not know how to talk about the non-mathematical component, we can scarcely avoid unbalanced accounts—accounts full of holes that look like slights to the applied statistician.

In my description of how statistics departments use joint appointments, I mentioned that statisticians with such appointments often communicate new problems back to their statistical colleagues, together with "their own attempts at solutions." I wish I had written merely "their own solutions," for George Box has interpreted my words as a disparagement of those who actually apply statistics. I am surprised and chagrined by this, for I meant only to convey the standard idea that new applied problems are a stimulus to theory. It is no disparagement of those first working with such problems that those following in their footsteps can sometimes improve on their solutions.

Box is right to have wanted me to say more. It is often the new ideas that the applied statistician devises, not further mathematical elaboration of these ideas, that constitute the most important contributions to theory. But this is not the formulation that comes first to my mind. Mathematics is my intellectual framework for thinking about statistical theory, and I am apt to need reminded to distinguish between the two.

Just after having read George Box dressing me down, I read David Aldous on applied probability, and I shuddered at Aldous's failure to discuss the non-mathematical intellectual content of contributions to applied probability. I do not fault Aldous for this. None of us can discuss this non-mathematical intellectual content, for no one has given us the words with which to do it. But in the absence of such a discussion, contributions close to real problems seem to derive merit only from their location, not from their content. Figure 1 portrays Aldous's linear interpolation as it might feel to an applied statistician. As we go from real scientific problems to pure mathematics, the level of intellectual content required for publication rises from near zero (“routine mathematics”) to its maximum.

**Figure 1. Aldous's Linear Interpolation**




In order to learn to talk to each other, we must make it part of the mission of any department (or journal) that houses applied probability to exposit the non-mathematical component of probabilistic modelling and reasoning in real problems. We need books, articles, and courses that survey and organize the diverse applications of probability, describe the strategies at work, and analyze their successes and failures. When there are such courses in Aldous's department, applied applied probability, as opposed to pure applied probability, will no longer be as fantastic and indescribable as the unicorn, for both Aldous and I will have a vocabulary for praising applications that matches our vocabulary for praising mathematics.

## Who Should Do Statistics?

I agree with Ian Hacking and David Moore that it is healthy for teaching and research in statistics to flourish outside statistics departments. I do take very seriously, however, the leadership role of the statistics department.

Some years ago, in the course of an unsuccessful effort to establish a statistics department at the University of Kansas, I looked at the numbers of statistics courses offered, in and out of statistics departments, at several comparable universities. I found that the universities with statistics departments usually had more rather than less statistics taught in other departments.

I have spent my teaching career at Princeton, where statistics is now dormant, and at Kansas, where there never has been a statistics department. This gives me a different perspective than my colleagues at Stanford, Purdue, and Berkeley. I am less confident than Efron that statistics will flourish even if statistics departments do not, less confident than Moore that conversations about probability will take place among varied scientific cultures even if statistics departments do not serve as the foci, and more inclined than Aldous to believe that anyone who wants to influence the intellectual life of a university must pay attention to where administrative lines are drawn. I invite these statisticians to take a close look at Kansas and Princeton, to see how far universities can go in doing without advanced courses in statistics. The fact that there is a need does not mean it will be filled.

Having lived outside statistics departments for fourteen years, I am also freer to fantasize about a statistics department with a broad mission. Indeed, I do not see how you could start to convince a university such as Kansas or Princeton that it should have a statistics department unless you were to promise more breadth than you find in existing statistics departments. A statistician at Berkeley, Stanford, or Purdue may find my suggestions for broadening statistics more difficult. It might not be easy, for example, to convince some well-established statistics faculties that historians could deserve appointments in their departments. Hirotugu Akaike has found it easier to bring people with diverse training together under the name “statistical science” than under the name “statistics.” The name does not matter. What matters is leaders such as Akaike, who will bring ideas and people from outside statistics into statistics.

## The Limits of Probability

A serious history of twentieth-century applied probability and statistics will be more than a collection of success stories. In truth, most problems of inference in science and the professions do not lend themselves to effective probabilistic or statistical treatment. An understanding of the intellectual content of applied probability and applied statistics must therefore include an understanding of their limits. What are the characteristics of problems in which statistical logic is not helpful? What are the alternatives that scientists, engineers, and others use? What, for example, are the characteristics of problems for which expert systems should use non-probabilistic tools of inference?

Here is another way in which a better understanding of our own discipline requires expertise from other disciplines. If we are going to understand when to use probability and when to use alternatives, we must understand the alternatives. We must, for example, understand the non-probabilistic methods of inference for artificial intelligence that have been developed in computer science. Contrary to Efron's assertion, there are computer scientists who think systematically about inference. We need to absorb their expertise into our understanding of the limits of probability and statistics.

## The Quality of Teaching

Both Aldous and Moore conclude their comments with a plea for more emphasis on the quality of teaching, especially the teaching of beginners. I am sympathetic with this, and I applaud Moore's putting his ideas into textbooks that bring the excitement of the history and logic of statistics into introductory courses. In the long run, however, I do not believe that we will convey this excitement to our beginning students unless we cultivate it in our research and advanced teaching. We are all proponents of the scholar-teacher model; we believe that in the long run, the best teaching is teaching based on the teacher's own life of the mind. At least we believe it when we want to explain to administrators and taxpayers why we should have time to do our research. We should also believe it when we make decisions about the allocation of resources within statistics departments. If we want our students to have teachers who can convey excitement about the history and logic of statistics, we must appoint scholars with serious research interests in these topics.

## A Challenge

I would like to end with a plea for more discussion. In my lecture, and again in this rejoinder, I have offered a rationale for strengthening our ties to other disciplines. Other statisticians, with more experience and insight than me, will be able to improve this rationale. Such improvement, and the consensus that can result from further discussion, are prerequisites for effective action.

There are many signs of an awareness of the need for new breadth in statistics. The very existence of the journal *Statistical Science* is one of the these signs. The new vigor of the applications section of the *Journal of the American Statistical Association* is another. The recent IMS report on cross-disciplinary research (Olkin et al. 1990) is another. But there is not yet a consensus that the need for new breadth applies to the intellectual core of statistics. The applications section of *JASA* is used primarily by statisticians in other disciplines, and the very word “cross-disciplinary” de-emphasizes the interdisciplinary nature of statistics itself.

I would like to challenge the contributors to this discussion, and other statisticians as well, to address these questions: What is the intellectual content of applied statistics, and how can we make this intellectual content as central to our discipline as the mathematics of statistics?

Art Dempster has been addressing these questions squarely, in his contribution to the discussion here, in his own publications on the philosophy of applied statistics (e.g., Dempster 1990), and in his recent work on course development. I am grateful for his support and applaud the scope of his work.

I would particularly like to challenge George Box, perhaps the most accomplished living applied statistician, to address these questions. We have much to learn from him about the intellectual content of applied statistics, and we deserve more than references to "additional resources." Box may not be sure what the originators of the idea of statistics departments intended, but he must know a lot about what the founder of at least one statistics department intended, and he could teach us what he learned from that experience.

Brad Efron also has the stature to contribute very influentially to this discussion. In his comments here, he modestly hopes that we are generating ideas that will secure our successor's place in the academy. As a leader in generating ideas that have stimulated new mathematical work, he can speak with authority about how we can achieve a balance between such mathematical work and broader understandings. I hope he will do so.

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