

# THE FOUNDATIONS OF STATISTICS RECONSIDERED

LEONARD J. SAVAGE  
UNIVERSITY OF MICHIGAN

## 1. Introduction

This is an expository paper on the evolution of opinion about the foundations of statistics. It particularly emphasizes the paths that some of us have followed to a position that may be called Bayesian or neo-Bayesian.

The intense modern growth of statistical theory of which this Symposium is a manifestation has been strongly oriented by a certain view as to the meaning of probability. I shall try to explain why another view seems now to be entering upon the scene almost of its own accord and to suggest what practical implications it brings with it.

## 2. Our frequentist background

Those who earlier in this century helped to mold the present great burst of activity in statistical thought seem to have been particularly concerned to adopt a clear and rigorous definition of probability. They were right to be so concerned, for the concept of probability has always been elusive and it lies at the heart of whatever any of us understand by “statistical theory” today. The concept of probability almost unanimously adopted by statisticians throughout the first half of the century, and the one that still seems to be regarded as fundamentally correct by the majority of statisticians today, is the frequency concept of probability, in which a probability is the relative frequency of some kind of event in a certain type of sequence of events or, according to some, in a set of events (as for example on page 109 of [12]).

It is completely understandable that a frequentist concept of probability should have come to the fore. The best known alternative concept when the modern renaissance of statistics was beginning was one I call the “necessary” concept of probability. Traditionally, this concept—apparently inspired by games of chance—represents an attempt to define probability of events in terms of the symmetry of the context in which they arise. In some modern views, probability is a logical relationship between one proposition (regarded as back-

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ground evidence) and another. According to such a view, to say that the probability of  $A$  on the evidence of  $B$  is  $3/4$  is much like saying that the proposition  $A$  is three quarters implied by  $B$ . I have called such views necessary because according to them the probability of  $A$  on the evidence  $B$  is a logical necessity to be deduced from the logical structure of the propositions  $A$  and  $B$ . This sort of view had been thoroughly and effectively criticized. It was utterly uncongenial to statisticians of the early part of the century, and so far as I know, nothing has happened to make it more congenial to us today. Necessary probability, ably sponsored by Carnap [2], [3] as a philosopher in recent years, though with considerable modification, is not altogether dead. Harold Jeffreys [16], [17], who supports it, is recognized as a productive and stimulating statistician by many of us. Nonetheless, necessary views have not been, and are not now, active in shaping statistical opinion.

The other main view of probability, competing with frequentist and necessary views, is the personalistic view, but the concept of personal probability has not, until recently, been ripe for acceptance by statisticians. In the personalistic concept, probability is an index—in an operational sense that will be explained later—of a person's opinion about an event. At first glance, such a concept seems to be inimical to the ideal of "scientific objectivity," which is one major reason why we statisticians have been slow to take the concept of personal probability seriously. Another reason is this. No matter how neat modern operational definitions of personal probability may look, it is usually possible to determine the personal probabilities of important events only very crudely, and the overhasty conclusion that such crude determinations are of little worth is often drawn.

Rejecting both necessary and personalistic views of probability left statisticians no choice but to work as best they could with frequentist views. As is well known, this has drastic consequences for the whole outlook of statistical theory. Once a frequentist position is adopted, the most important uncertainties that affect science and other domains of application of statistics can no longer be measured by probabilities. A frequentist can admit that he does not know whether whisky does more harm than good in the treatment of snake bite, but he can never, no matter how much evidence accumulates, join me in saying that it *probably* does more harm than good. Whatever else a frequentist may do with the results of investigation he cannot, as a frequentist, use them to calculate probabilities of the uncertain propositions that are under investigation. Technically, this means that he is cut off from most applications of Bayes' theorem, the algorithm for calculating what the new probability of a proposition is on the basis of its original probability and new relevant evidence. One's natural inclination to ask, "To what degree of conviction does this new data entitle me?" usually must be, and has been, regarded as a nonsense question by the frequentist. The frequentist is required, therefore, to seek a concept of evidence, and of reaction to evidence, different from that of the primitive, or natural, concept that is tantamount to application of Bayes' theorem.

Statistical theory has been dominated by the problem thus created, and its most profound and ingenious efforts have gone into the search for new meanings for the concepts of inductive inference and inductive behavior. Other parts of this lecture will at least suggest concretely how these efforts have failed, or come to a stalemate. For the moment, suffice it to say that a problem which after so many years still resists solution is suspect of being ill formulated, especially since this is a problem of conceptualization, not a technical mathematical problem like Fermat's theorem or the four-color problem. We Bayesians believe that the dilemma to which the frequentist position has led, along a natural and understandable path, is insoluble and reflects what is no longer a tenable position about the concept of probability.

Frequentists, of course, disagree somewhat one from another, and their individual views are subject to change and development with passing time. It is, therefore, not really careful to speak of *the* frequent view, or *the* frequency concept of probability, though I believe that what I have said thus far does apply to all frequentists. The widest cleft between frequentists is that between R. A. Fisher and those who side closely with him on the one hand and those who more or less associate themselves with the school of Jerzy Neyman and Egon Pearson.

### 3. The behavioralistic approach

One of the most valuable outgrowths of the frequentistic movement is the behavioralistic (or one might say economic-theoretic) approach to statistical problems. This approach undoubtedly has very early antecedents, but statisticians were particularly wakened to it by Neyman's suggestion in [22] that inductive behavior is a more fertile concept for statistics than inductive inference, a theme elaborately illustrated by Abraham Wald and many of us who came under his influence.

It is a vexed question whether behavior rather than inference is the very essence of statistical problems. R. A. Fisher (pages 100 and 103 of [12] and [11]) and some others maintain energetically that inductive inference serves the high, free purposes of science and that inductive behavior, which is to say the economic analysis of statistical problems, is adapted only to business and tyranny if to anything. Personally, emphasis on behavior has seemed to me an unmitigated advantage, and I believe it to be a stimulating framework for all parts of statistics. Whether the conclusion is found valid or not, there are surely many interesting, practical problems for which the behavioralistic point of view is distinctly advantageous.

The traditional idea of inference as opposed to behavior seems to me to have its roots in the parallel distinction between opinion and value. Since the Bayesian outlook reinstates opinion in statistics—in the guise of the personal probabilities of events—the concept of inductive inference has a meaning to Bayesians that is usually closed off to frequentists. Inference means for us the changes in opinion

induced by evidence on the application of Bayes' theorem. For us, a problem in analysis, as opposed to one in design, can conveniently and properly be separated into two phases. First, compute the new distribution induced by the original distribution and the data. Second, use the new distribution and the economic facts about terminal actions to pick one of the terminal actions that now has the highest expected utility. Of course, a full statistical problem typically involves design as well as analysis. The Bayesian method for such a problem is, in principle, to survey all designs, each followed by optimal terminal decision, and to select one of those with the highest expected income, taking into account the expected cost of experimentation as well as the over-all expected income of the terminal act. Even the most natural of such problems can be forbidding analytically, as is illustrated by Herman Chernoff's paper at the Symposium [5], and such problems now seem particularly important.

#### 4. Objectivity and subjectivity

The interplay between the ideas of objectivity and subjectivity in statistics is interesting and sometimes confusing. Since frequentists usually strive for, and believe that they are in possession of, an objective kind of probability (a partial exception seems to be reflected on page 33 of [12]) and since personalists declare probability to be a subjective quantity, it would seem natural to call frequentists objectivists and personalists subjectivists. I myself have succumbed to this temptation, but it is deplorable, for frequentists allocate much more to the subjective than we Bayesians do, as will now be explained.

No matter how much statistical study and sophistication have to contribute to the theory of design of investigations, the problem of design, I think everyone agrees, depends largely on subjective choices and judgments. More important for the moment, most frequentists are agreed that once the data is at hand and the moment for final action (or analysis) has come, theory leaves room for a great deal of subjective choice.

Fisher's school, with its emphasis on fiducial probability—a bold attempt to make the Bayesian omelet without breaking the Bayesian eggs—may be regarded as an exception to the rule that frequentists leave great latitude for subjective choice in statistical analysis. The minimax theory, too, can be viewed as an attempt to rid analysis almost completely of subjective opinions, though not of subjective value judgments. From this point of view, the minimax theory of statistics is, however, an acknowledged failure. The minimax rule has never been taken seriously as a philosophic principle for inductive behavior, and even as a rule of thumb little if any good has been found in it; the strongest apology for the rule is perhaps to be found in the latter half of my book [24], especially chapters 10, 11, and 13. Studies of the minimax rule have been stimulating for statistics, and modifications and outgrowths of the rule may prove of great value, but those of us who, twelve or thirteen years ago, hoped to find in this rule an

almost universal answer to the dilemma posed by abstinence from Bayes' theorem have had to accept disappointment.

I repeat, then, frequentists are rather well agreed that analysis is largely subjective. The one clear guide to analysis to which they have come is the principle of admissibility. This says roughly that statistical procedures that are capable of improvement simultaneously at all values of the unknown parameter are not satisfactory. I think that we all agree that the principle is basically sound, though the demonstration by Charles Stein [29] that some favorite estimation procedures are not admissible must make us circumspect in our statement and application of the rule. The usual frequentist position goes on to say that any admissible procedure might be the preferred procedure of some person, so statistical theory cannot eliminate any admissible procedure. In all this, the Bayesian agrees with the frequentist.

Both agree, too, that though they cannot eliminate any admissible procedure as the preferred procedure of a given person, it may be possible to give suggestions that will help the person to make his choice in an orderly and personally satisfying way. The frequentist seeks to do this by inventing what he often calls "nice properties," exemplified by unbiasedness, stringency, minimum mean squared error, symmetry (or invariance), a given significance level, and so on. These properties of statistical procedures are felt by frequentists to be more or less appealing, and they hope that, in a given problem, exploration of what subsets of nice properties can be attained by admissible procedures will help the person to know his personal choice.

It is just here that we Bayesians particularly hope to carry statistical theory a step forward. Our approach is not to urge the person to ask himself what qualitative properties he likes a procedure to have but to ask himself rather generally (usually going outside of the particular procedures that the experiment actually contemplated happens to make available) when he would prefer one procedure to another. A few strongly appealing principles of coherence often succeed in making this task relatively easy. These principles can easily be communicated to a frequentist with no reference to any kind of probability that he does not believe in. But, in fact, a person who succeeds in conforming to the principles of coherence will behave as though there were a probability measure associated with all events in terms of which his preference among statistical procedures is such as to maximize expected utility under this measure. In particular, he will behave in accordance with Bayes' theorem as applied to his personal probability measure.

## 5. Simple dichotomy

The whole situation too abstractly described by the last paragraph is illustrated in microcosm by the theory of a simple dichotomy. In this almost oversimplified sort of problem, a statistical procedure is characterized by two

numbers  $\alpha$  and  $\beta$ , the errors of the first and second type. The essence of the principle of admissibility here is that any change in  $\alpha$  and  $\beta$  that increases neither of them is a good thing. For a given experimental setup, the admissible procedures (in this case, admissible tests) are, according to the Neyman-Pearson lemma, the likelihood-ratio tests. What can be said to help a person in a given situation choose one among the many likelihood-ratio tests available? A typical, and thoroughly competent, frequentist answer is explicitly given by Lehmann in a recent article [21]. He discusses various "nice properties," shows that some are not so nice after all, and mildly recommends a new one.

The Bayesian approach here is to inquire what a person's preference scheme might be for all pairs of points in the  $(\alpha, \beta)$  square. Lehmann briefly considers this possibility, but he yields to the discouraging conclusion that the indifference curves describing the preference scheme might be practically any family of curves running in a generally northwesterly to southeasterly direction. Actually, the situation is anything but discouraging. Clear and simple arguments give strong reason for concluding that the indifference curves of an ideally consistent person are parallel straight lines. These arguments flow from the remark that if a person does not care which of a certain pair of experiments and tests he does then he would just as soon substitute for either of them the experiment and test that consists in executing one of the two different programs at random. Thus, where there seems at first to be freedom for a person to express his preferences by any of a vast family of curves, it can be concluded that there is good reason for him to express his preference by a single number, say the negative of the slope of his family of indifference lines. A person whose preferences among  $(\alpha, \beta)$  points are expressed by a family of parallel straight lines will use the negative of the slope of these lines as his critical likelihood ratio for any experiment referring to the simple dichotomy.

To put it differently, there is every hope of convincing a person concerned with simple dichotomy that he has some rate of exchange between the two quantities  $\alpha$  and  $\beta$ ; for a reduction in one, he will trade a suitable proportional increase in the other. When the person sees this, he will want to adopt a critical likelihood ratio equal to his critical exchange ratio for any experiment that may be done.

The idea that a person has reason to choose a fixed likelihood ratio for a simple dichotomy without regard to whether the experiment is large or small or, indeed, without regard to what the errors associated with its various likelihood-ratio tests are, seems never to have emerged from frequentist studies, certainly never to have caught on and been propagated. In particular, it is not referred to in the well-informed and thorough article [21] of Lehmann alluded to above. Some of us Bayesians have been talking about this rule with many people of diverse opinions for several years, and it seems solidly to stand the test of criticism and counterexample.

To continue the Bayesian analysis a little further, suppose that the errors of type 1 and type 2 occasion numerical losses, say  $L_1$  and  $L_2$ . It is then evident, on

examination, that the person who adopts a fixed critical likelihood ratio is behaving as though he attributed a certain probability to one hypothesis and the complementary probability to its alternative.

The whole theory of simple dichotomy sketched here is easily extended to cover any problem with finite parameter and action spaces. Many will agree that this extension is basically adequate for a conceptual portrayal of the whole of statistical theory, though of course exploration of continuous situations is interesting and of practical importance.

The main purpose of this section has been to clarify the important point that the Bayesian approach is more objectivistic than the frequentist approach in that it imposes a greater order on the subjective elements of the deciding person. We Bayesians have, in fact, sometimes been thought guilty of dictatorial tendencies on this count. I hope that the language I have been using makes it clear that Bayesians are not trying to tell anyone what he must do. We are of course content to state what we think are acceptable principles of coherence and let those who agree strive to comply with them.

A second important objective which has been served by this section is to show how the Bayesian movement has a contribution to make even to those who are disinclined to take seriously any but a frequency concept of probability.

The theory of simple dichotomy sketched here was worked out by Dennis Lindley and me in the spring of 1955 under the impetus of his paper [22].

## 6. Personal probability

The preceding section brought out that the Bayesian theory of statistics can be entered by a back door, so to speak, without challenging the propriety, or even the exclusive propriety, of the frequency concept of probability, but once the edifice is truly penetrated the theory of personal probability is seen to be its main staircase. Let us then boldly enter the front door and take a brief look at this theory.

If I offer to pay you \$10.00 if any one horse of your choosing wins a given race, your decision tells me operationally which you consider to be the most probable winner. Working out the theory of this economically defined concept of "more probable than for Mr. So-and-so" on the assumption that Mr. So-and-so is ideally free of contradictory and self-negating behavior, leads to the conclusion that his preference scheme is governed by an ordinary probability measure on the class of all events. Details and references may be found in [24].

For many of us, it is more stimulating to think of the odds that Mr. So-and-so would be just willing to offer in favor of an event as measuring his personal probability through the formula,  $\text{probability} = \text{odds}/(1 + \text{odds})$ . This approach, though vivid and useful once the theory is accepted, does not lend itself smoothly to a rigorous foundation of the theory (compare page 176 of [24]).

The concept of personal probability sketched in the preceding two paragraphs seems to those of us who have worked with it an excellent model for the concept

of opinion. Of course, the concept has been subjected to much criticism and elaboration, which cannot be entered into here. See [25] for details and references.

For the purpose of using the concept of personal probability to put statistics a bit forward, it is enough to understand the concept to concede that it has some sense and promise, and to explore the consequences for statistics with imagination and temperance. I will confess, however, that I and some other Bayesians hold this to be the only valid concept of probability and, therefore, the only one needed in statistics, physics, or other applications of the idea. In particular, we radical Bayesians claim to demonstrate that all that is attractive about the frequency theory of probability is subsumed in the theory of personal probability. Before challenging that as preposterous, one ought at least study the chapter on equivalent events in de Finetti's paper [6] or in section 3.7 of [25], which is derived from that chapter. A less radical position was recently taken by Good [15].

A few words of history. The first formal mention of the concept of personal probability seems to be by Frank Ramsey in 1926. His posthumous papers in [24] present a theory of personal probability and of utility developed together. Ramsey seems largely to repudiate his creation in a brief note (on pages 256 and 257 of [24]) not written for publication, according to page viii of [24].

Bruno de Finetti developed the concept of personal probability in great detail, beginning after, but independently of, Ramsey. De Finetti has many relevant publications, but I particularly recommend [6], though it is not recent. A recent and most stimulating work is [7], in Italian.

The work of B. O. Koopman [19], [20] is interesting also. It was done with some slight knowledge of [6].

Ramsey, de Finetti, and Koopman developed their theories in detachment from statisticians and did relatively little to explore possible application of personal probability to statistics. I. J. Good in 1950 published a small book [14] on personal probability with special reference to statistics, and I published a book in 1954 [25]. In my judgment, Good and I were both too deeply in the grip of frequentist tradition—he much less than I—to do a thorough job. In 1959 Robert Schlaifer published an elementary but important and stimulating textbook [28] on statistics written entirely and wholeheartedly from the Bayesian point of view.

Harold Jeffreys holds what I call a necessary view of the theory of probability, but such a view is Bayesian in the broad sense that it makes free use of Bayes' theorem and therefore demands a thorough practical exploration of Bayes' theorem for its application to statistics. In fact, Jeffreys' books [16], [17] as well as his many papers are at present invaluable in developing the theory of (personalistic) Bayesian statistics.

## 7. Implications of the Bayesian view

We Bayesians find that exploration of the Bayesian position stimulates our insight into every part of statistical theory. Of course, many of the criticisms

that flow from this position have been seen before. After all, the theory is but an elaboration of common sense and it is to be expected that any of its important conclusions can be referred rather directly to common sense. There is space for only a brief mention of a few of the implications of the Bayesian position already discovered. The discussion of these will perhaps bring out that many of them have been discovered by frequentists but that the Bayesian position usually allows an implication to be carried further, often supplementing a destructive criticism with a positive suggestion. From the Bayesian position, heretofore scattered ideas take on new unity and comprehensibility.

One of the most obvious, ubiquitous, and valuable consequences of the Bayesian position is what I call the likelihood principle. This principle was, so far as I know, first advocated to statisticians in a non-Bayesian work by George Barnard [2]. It is supported in R. A. Fisher's recent book [12], which is of course non-Bayesian and ostensibly frequentist. Practically none of the "nice properties" respect the likelihood principle nor apparently does Fisher's concept of fiducial probability in any of its revisions; see [1].

The likelihood principle says this: the likelihood function, long known to be a minimal sufficient statistic, is much more than merely a sufficient statistic, for given the likelihood function in which an experiment has resulted, *everything* else about the experiment—what its plan was, what different data might have resulted from it, the conditional distributions of statistics under given parameter values, and so on—is irrelevant.

Consider an illustration. A properly randomized and executed experiment to ascertain the number of red-eyed flies in a population of flies will, for each pair of nonnegative integers  $(r, n)$  and each frequency  $p$ , have some probability  $P\{r, n|p\}$  of resulting in  $r$  "reds" and  $n$  "nonreds" for a given  $p$ . In fact,  $P\{r, n|p\}$  will be of the form  $k(r, n)p^r(1 - p)^n$ , as shown in [13]. For a given outcome  $(r, n)$  this function of  $p$  represents the likelihood function of  $p$ . I say "represents" rather than "is" to emphasize that the likelihood function is defined only up to an arbitrary factor that does not depend on  $p$ . Thus, equally,  $p^r(1 - p)^n$  represents the likelihood function, and applying the likelihood principle, we conclude that the import of  $r$  "reds" and  $n$  "nonreds" is independent of the design of the experiment and, in particular, independent of the function  $k(r, n)$ . This conclusion is out of harmony with much statistical thinking and effort, for example the search for unbiased estimates of  $p$ , exemplified by [13]. The likelihood  $p^r(1 - p)^n$  retains its import even if the experiment terminated merely when the experimenter happened to get tired or run out of time—always under the proviso that the individual trials are independent under a fixed  $p$ , not for instance one that changes with fatigue or excitement.

This same function even persists if the experimenter quits only when he believes he has enough data to convince others of his own opinion. This leads to the moral that optional stopping (as condemned in [8]) is no sin, but that traditional methods of judging data in terms of significance level are misleading. Frequentists have certainly had an inkling of the fact that significance level

cannot safely be interpreted without regard to other information, but, so far as I know, they have not been able to treat the matter systematically.

The likelihood principle, with its at first surprising conclusions, has been subject to much oral discussion in many quarters. If the principle were untenable, clear-cut counterexamples would by now have come forward. But each example seems, rather, to illuminate, strengthen, and confirm the principle.

Another principle of great practical value is an approximation that I call the principle of precise measurement and that accounts for much practical "objectivity" in statistical inference. If prior opinion about a parameter (or set of parameters)  $\omega$  is described by a density  $\rho(\omega)$ , and if the probability of a datum  $D$  given  $\omega$  is  $P\{D|\omega\}$ , then, according to Bayes' theorem, posterior density of  $\omega$  on the datum  $D$  is  $\rho(\omega|D) = kP\{D|\omega\}\rho(\omega)$ , where  $k$  is determined by normalization. The principle of precise measurement is the recognition that if  $\rho(\omega)$  behaves gently as compared with the likelihood representation  $P\{D|\omega\}$  then  $\rho(\omega|D)$  will, in the interesting part of the range of  $\omega$ , be approximated by  $k'P\{D|\omega\}$ , where  $k'$  is also a normalizing constant.

To illustrate roughly, if your opinion about the weight of a certain sack of potatoes is insensitive to shifts of half a pound or so within some reasonable interval—an assumption that can well be palpably satisfied—then after you weigh the sack with a normal error of standard deviation one ounce, your opinion will be a density nearly normal about the weighing with standard deviation one ounce. In short, you will be justified, as an approximation, in drawing just the kind of conclusion that the theory of confidence intervals is perforce careful not to justify. It is not surprising that the theory of precise estimation illuminates many questions that have been raised about confidence intervals.

A somewhat fuller statement of the principle allows for the possibility that  $\rho(\omega)$  can be written as  $f(\omega)g(\omega)$ , where  $f$ , but not necessarily  $g$ , is gentle. This justifies, for example, approximating posterior opinion based on several normal weighings of unknown variance by a  $t$  distribution.

One question about which frequentists have disagreed is the Behrens-Fisher problem. The solution always championed by Fisher [12], for reasons inscrutable to most of us, is in fact appropriate as a good approximation in many situations according to the theory of precise measurement. The demonstration, like so many other valuable things in this area, has in effect been given by Jeffreys [18] from a somewhat different outlook.

The principle of precise measurement is not new. It has, for example, been well stated and well understood by Jeffreys (section 3.4 of [16]) and less well stated and less well understood by Fisher (page 287 in [9]).

Many other examples of Bayesian implication could be offered if space permitted; some are to be found in [26] and [27].

## 8. One present view

My own present view, which I feel safe in asserting agrees quite well with that of other Bayesian statisticians, must be largely clear from earlier sections of this

lecture, but I would like to draw the threads together here in a sort of provisional credo.

Personal probability at present provides an excellent base of operations from which to criticize and advance statistical theory.

The theory of personal probability must be explored with circumspection and imagination. For example, applying the theory naively one quickly comes to the conclusion that randomization is without value for statistics. This conclusion does not sound right; and it is not right. Closer examination of the road to this untenable conclusion does lead to new insights into the role and limitations of randomization but does by no means deprive randomization of its important function in statistics.

Exploration of the theory of personal probability is full of practical implications for statistics at all levels from that of the most elementary textbooks to the most remote pages of the *Annals*.

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