CONCLUDING REMARK

The above observations only confirm the importance of the subject discussed in the paper by Professor Shafer. I feel that the paper should be read and discussed by all the statisticians who really care about the future of statistics.

I am very grateful to the Executive Editor, Professor Carl N. Morris, for inviting me to join the discussion.

Comment

David Aldous

Though asserting no interest in the foundational side of probability (thereby inviting a Keynesian riposte about being "enslaved to a philosophy long discredited"!), I found Professor Shafer's article interesting and thought-provoking. Professor Shafer uses the word "probability" in a wise sense; reflecting my own interests, I use it mean "probability and its applications, *excluding* statistics." Readers may judge for themselves whether my comments are relevant to statistics proper. Many of Professor Shafer's comments concern teaching issues, whereas mine mostly address research.

1. RESEARCH-LEVEL APPLIED PROBABILITY

It is curious that there is no phrase "[adjective] mathematics" which adequately conveys the idea

> (M) research whose conclusion is the statement and proof of a theorem

as opposed to

> (A) research whose goal is answering a science question, using mathematics as a means rather than an end.

(I use "science" very broadly to mean some academic discipline in which mathematics can be used.) Though making distinctions between theory and applications is unfashionable and politically incorrect, I do see a distinction between seeking to make money at blackjack or the stock market and proving optimal strategy theorems; between designing airplane wings and proving theorems about air flow; between building reliable systems of components and proving theorems about increasing failure rates; between understanding molecular evolution and proving theorems about measure-valued diffusions.

Although an applied mathematician or statistician might claim to be doing both (A) and (M)—posing an extra-mathematical question and then answering it by proving a theorem—the proportion of research papers that actually do both is extremely small. Most papers in (for example) *Annals of Statistics, Journal of Applied Probability, IEEE Transactions on Information Theory, SIAM Journal of Control and Optimization,* and *Journal of the Association for Computing Machinery* and much that is usually called "applied mathematics," are plainly (M) but not (A). Good applied mathematics is like the unicorn: something we can all recognize but seldom actually see.

The part of (M) that is not traditional "pure mathematics" needs a name: I call it "theory-motivated-by-applications" (TMA) mathematics. Of current research involving probability, much more is TMA than is either (A) or pure mathematics. The key problem with research-level applied probability is the lack of agreed standards for evaluating TMA research. While this is not a pressing issue for most of us, it is for Mike Steele (as Editor of the new *Annals of Applied Probability*) and his associate editors. It would be unreasonable and divisive to erect high threshold standards for "serious math" and for "serious science" and insist that research exceeds one threshold or the other. There is a spectrum: at one extreme is serious math theory (at the level of *Annals of Probability*) with a rather vague connection to an application; at the other extreme is a serious science question which is solved by (to an expert theoretician) rather routine mathematics. Linear interpolation between these extremes is fine and constitutes what I regard as worthwhile applied probability. What concerns me is that, once a dozen people write papers on "probability methods in subject S," a continuing subdiscipline is likely to be established. At best, this subdiscipline will produce results of interest to both the mainstream nonmathematical scholars in subject S and to workers in broader areas of theoretical and applied probability. At worst, it becomes an inward-looking clique ignored by everyone else. Of course this worst case also happens within theoretical disciplines, but there it is easier to detect. A cynic might say that applied probabilists can get away with claiming to theoreticians that they are solving science questions, and claiming

David Aldous is Professor, Department of Statistics, University of California at Berkeley, Berkeley, California 94720.

to scientists that they are solving math questions, and thereby avoid critical assessment from either! This concern is related to Professor Shafer's discussion of balkanization. However, the name of the Department in which (say) queueing theorists are located does not seem to me the important issue. Professor Shafer worries that the existence of such subdisciplines with their own styles detracts from the conceptual unity of probability; I worry that they become self-perpetuating areas of toy theory lacking contact with either mathematical probability or the concrete real-world phenomena they were intended to model.

While it may seem trite, my "linear interpolation" notion of applied probability is a little different from that of a generation ago. In the 1960s, David Kendall and others founded the Applied Probability Trust and its journals. At that time, probability was often viewed as a narrow subject running between measure theory and theoretical statistics. The Trust's journals provided an invaluable nursery for the growth of the kind of "applicable" theory then unfashionable among theoretical probabilists (e.g., Markov chains, branching processes, renewal theory) as well as probabilistic modeling. And in an understandable zeal to nurture this growth, there was a tendency to regard any work in probability that could be labeled "applicable" as *ipso facto* worthwhile, without regard to the traditional standards of theoretical mathematics or science. Twenty-five years later, the situation is quite different: there are more journals, more research activity and more job openings in applied probability than in theoretical probability. So it is surely time to be less protective. A tough policy would be the following. Look at a paper, and ask its motivation: typically the motivation comes from a previous paper. How many such steps are needed to get out of "applied probability" and back to an actual solid application? If more than two or three, you are in an established subdiscipline and should judge by the standards of theoretical mathematics.

This sounds harsh. My earlier cynic might say that theoretical statisticians and theoretical probabilists like Statistics Departments, and applied probabilists like O.R. Departments, precisely because there they can spend their (research) lives proving theorems in peace without the bother of meeting the standards of pure mathematicians. This is partly justifiable, in that there is a narrow-minded variety of pure mathematician who judges only by "mathematical depth" (which is like judging a painting only by technique). Certainly a broader notion of what constitutes worthwhile theoretical mathematics is needed. But dressing up theory as applications is not the answer.

2. TEACHING

When in casual conversation with an educated American I confess to being a professor in Statistics, the most common reaction is "I had to take that in College, and hated it/couldn't understand a word of it." This is the dull reality of failure that we face, just as the communist elite of the Soviet Union who talk to their citizens must face the reality of failure of their system: addressing the former failure by attempting "the conceptual and institutional reunification of probability" seems as irrelevant as addressing the latter failure by instituting a unified version of different Marxist theories.

Of course this is a caricature of Professor Shafer's remarks. Much of what he writes in Section 7—joint appointments, and the need to look at ways for other disciplines to contribute to statistics—is surely as uncontroversial as motherhood and apple pie. But there will always be administrative lines dividing Departments, at arbitrary points on the spectrum from theory to applications. To emphasize where these lines are drawn—which seems the thrust of "making the statistics department once again the intellectual center of probability"—and to dismiss "we should teach better" as a well-worn answer, reverses ends and means. General Motors' product is cars, and ours is teaching. Focusing directly on the quality of the product, rather than on institutional reforms, seems in each case more likely to ensure independent survival.