BIOGRAPHY

DAVID BLACKWELL

BORN: April 24, 1919, Centralia, Illinois

EDUCATION: Centralia Public Schools 1925-35
University of Illinois 1935-41: AB 1938, AM 1939, Ph.D. 1941, Mathematics.

POSITIONS HELD:

1941-42	Rosenwald Fellow, Institute for Advanced Study
Summer, '42	Assistant Statistician, OPA
1942-43	Instructor, Southern University, Baton Rouge, LA.
1943-44	Instructor, Clark College, Atlanta, Georgia
1944-46	Assistant Professor, Howard University, Washington, DC
1946-47	Associate Professor, Howard University, Washington, DC
1947-54	Professor and Head, Mathematics Department, Howard University, Washington, DC
Summers, 1948-50	Mathematician, Rand Corporation
1950-51	Visiting Professor, Stanford University
1954-55	Visiting Professor, University of California, Berkeley,
1955-73	Professor of Statistics, University of California, Berkeley,
1957-61	Chairman, Department of Statistics, University of California, Berkeley
1970-71	Faculty Research Lecturer, University of California, Berkeley,
1973-75	Director, University of California Study Center, United Kingdom and Ireland (Education Abroad Program)
1973 to present	Professor of Mathematics and Statistics, University of California, Berkeley

HONORARY DEGREES

1966	D.Sc. (Honorary) University of Illinois
1969	D.Sc. (Honorary) Michigan State University
1971	D.Sc. (Honorary) Southern Illinois University
1980	D.Sc. (Honorary) Carnegie-Mellon University
1987	D.Sc. (Honorary) National University of Lesotho
1988	D.Sc. (Honorary) Amherst College
1988	D.Sc. (Honorary) Harvard University
1990	D.Sc. (Honorary) Howard University
1990	D.Sc. (Honorary) Yale University
1990	D.Sc. (Honorary) University of Warwick
1991	D.Sc. (Honorary) Syracuse University
1992	D.Sc. (Honorary) University of Southern California

PROFESSIONAL SERVICE, HONORS

1955	President, Institute of Mathematical Statistics
1965	Elected to National Academy of Sciences
1968	Elected to American Academy of Arts & Sciences
1968-71	Vice-President, American Mathematical Society
1972-73	Chairman, Faculty Research Lecture Committee
1973	President, International Association for Statistics in the Physical Sciences
1974	W.W. Rouse Ball Lecturer, University of Cambridge, UK
1975-78	President, Bernoulli Society for Mathematical Statistics and Probability
1975-77	Vice-President, International Statistical Institute
1976	Elected Honorary Fellow, Royal Statistical Society
1977	Wald Lecturer, Institute of Mathematical Statistics

1978-	Vice-President, American Statistical Association
1979	TIMS/ORSA John Von Neumann Theory Prize
1986	R.A. Fisher Award, Committee of Presidents of Statistical Societies
1988	The Berkeley Citation

MEMBERSHIP IN PROFESSIONAL SOCIETIES (partial list)

National Academy of Sciences

American Association for Advancement of Science

American Academy of Arts and Sciences

Institute of Mathematical Statistics

American Mathematical Society

Royal Statistical Society

American Philosophical Society

1/93

Statistical Science
1986, Vol. 1, No. 1, 40–53

A Conversation with David Blackwell

Morris H. DeGroot

David Blackwell was born on April 24, 1919, in Centralia, Illinois. He entered the University of Illinois in 1935, and received his A.B. in 1938, his A.M. in 1939, and his Ph.D. in 1941, all in mathematics. He was a member of the faculty at Howard University from 1944 to 1954, and has been a Professor of Statistics at the University of California, Berkeley, since that time. He was President of the Institute of Mathematical Statistics in 1955. He has also been Vice President of the American Statistical Association, the International Statistical Institute, and the American Mathematical Society, and President of the Bernoulli Society. He is an Honorary Fellow of the Royal Statistical Society and was awarded the von Neumann Theory Prize by the Operations Research Society of America and the Institute of Management Sciences in 1979. He has received honorary degrees from the University of Illinois, Michigan State University, Southern Illinois University, and Carnegie-Mellon University.

The following conversation took place in his office at Berkeley one morning in October 1984.

"I EXPECTED TO BE AN ELEMENTARY SCHOOL TEACHER"

DeGroot: How did you originally get interested in statistics and probability?

Blackwell: I think I have been interested in the concept of probability ever since I was an undergraduate at Illinois, although there wasn't very much probability or statistics around. Doob was there but he didn't teach probability. All the probability and statistics were taught by a very nice old gentleman named Crathorne. You probably never heard of him. But he was a very good friend of Henry Rietz and, in fact, they collaborated on a college algebra book. I think I took all the courses that Crathorne taught: two undergraduate courses and one first-year graduate course. Anyway, I have been interested in the subject for a long time, but after I got my Ph.D. I didn't expect to get professionally interested in statistics.

DeGroot: But did you always intend to go on to graduate school?

Blackwell: No. When I started out in college I expected to be an elementary school teacher. But somehow I kept postponing taking those education courses. [Laughs] So I ended up getting a master's degree and then I got a fellowship to continue my work there at Illinois.

DeGroot: So your graduate work wasn't particularly in the area of statistics or probability?

Blackwell: No, except of course that I wrote my thesis under Doob in probability.

DeGroot: What was the subject of your thesis?

Blackwell: Markov chains. There wasn't very much original in it. There was one beautiful idea, which was Doob's idea and which he gave to me. The thesis was never published as such.

DeGroot: But your first couple of papers pertained to Markov chains.

Blackwell: The first couple of papers came out of my thesis, that's right.

DeGroot: So after you got your degree . . .

Blackwell: After I got my degree, I sort of expected to work in probability, real variables, measure theory, and such things.

DeGroot: And you *have* done a good deal of that.

Blackwell: Yes, a fair amount. But it was Abe Girshick who got me interested in statistics.

DeGroot: In Washington?

Blackwell: Yes. I was teaching at Howard and the mathematics environment was not really very stimulating, so I had to look around beyond the university just for whatever was going on in Washington that was interesting mathematically.

DeGroot: Not just statistically, but mathematically?

Blackwell: I was just looking for anything interesting in mathematics that was going on in Washington.

DeGroot: About what year would this be?

Blackwell: I went to Howard in 1944. So this would have been during the year 1944–1945.

DeGroot: Girshick was at the Department of Agriculture?

Blackwell: That's right. And I heard him give a lecture sponsored by the Washington Chapter of the American Statistical Association. That's a pretty lively chapter. I first met George Dantzig when he gave a lecture there around that same time. His lecture had nothing to do with linear programming, by the way. In fact, I first became acquainted with the idea of a randomized test by hearing Dantzig talk about it. I think that he was the guy who invented a test function, instead of having just a rejection region that is a subset of the sample space. At one of those meetings Abe Girshick spoke on sequential analysis. Among other things, he mentioned Wald's equation.

DeGroot: That's the equation that the expectation of a sum of random variables is $E(N)$ times the expectation of an individual variable?

Blackwell: Yes. That was just such a remarkable equation that I didn't believe it. So I went home and thought I had constructed a counterexample. I mailed it to Abe, and I'm sure that he discovered the error. But he didn't write back and tell me it was an error; he just called me up and said let's talk about it. So we met for lunch and that was the start of a long and beautiful association that I had with him.

DeGroot: Would you regard the Blackwell and Girshick book (*Theory of Games and Statistical Decisions*. New York, John Wiley & Sons, 1954) as the culmination of that association?

Blackwell: Oh, that was a natural outgrowth of the association. I learned a great deal from him.

DeGroot: Were you together at any time at Stanford?

Blackwell: Yes, I spent a year at Stanford. I think it was 1950–1951. But he and I were also together at other times. We spent several months together at Rand. So we worked together in Washington, and then at Rand, and then at Stanford.

"I WROTE 105 LETTERS OF APPLICATION"

DeGroot: Tell me a little about the years between your Ph.D. from Illinois in 1941 and your arrival at Howard in 1944. You were at a few other schools in between.

Blackwell: Yes. I spent my first postdoctoral year at the Institute for Advanced Study. Again, I continued to show my interest in statistics. I sat in on Sam Wilks' course in Princeton during that year. Henry Scheffé was also sitting in on that class. He had just completed his Ph.D. at Wisconsin. Jimmie Savage was at the Institute for that year. He was at some of Wilks' lectures, too. There were a lot of statisticians about our age around Princeton at that time. Alex Mood was there. George Brown was there. Ted Anderson was there. He was in Wilks' class that year.

DeGroot: He was a graduate student?

Blackwell: He was a graduate student, just completing his Ph.D. So that was my first postdoctoral year. Also, I had a chance to meet von Neumann that year. He was a most impressive man. Of course, everybody knows that. Let me tell you a little story about him.

When I first went to the Institute, he greeted me, and we were talking, and he invited me to come around and tell him about my thesis. Well, of course, I thought that was just his way of making a new young visitor feel at home, and I had no intention of telling him about my thesis. He was a big, busy, important man. But then a couple of months later, I saw him at tea and he said, "When are you coming around to tell me about your thesis? Go in and make an appointment with my secretary." So I did, and later I went in and started telling him about my thesis. He listened for about ten minutes and asked me a couple of questions, and then he started telling *me* about *my* thesis. What you have really done is this, and probably this is true, and you could have done it in a somewhat simpler way, and so on. He was a really remarkable man. He listened to me talk about this rather obscure subject and in ten minutes he knew more about it than I did. He was extremely quick. I think he may have wasted a certain amount of time, by the way, because he was so willing to listen to second- or third-rate people and think about their problems. I saw him do that on many occasions.

DeGroot: So, from the Institute you went where?

Blackwell: I went to Southern University in Baton Rouge, Louisiana. That's a state school and at that time it was *the* state university in Louisiana for blacks. I stayed there just one year. Then the next year, I went to Clark College in Atlanta, also a black school. I stayed there for one year. Then I went to Howard University in Washington and stayed there for ten years.

DeGroot: Was Howard at a different level intellectually from these other schools?

Blackwell: Oh yes. It was the ambition of every black scholar in those days to get a job at Howard University. That was the best job you could hope for.

DeGroot: How large was the math department there in terms of faculty?

Blackwell: Let's see. There were just four regular people in the math department. Two professors. I went there as an assistant professor. And there was one instructor. That was it.

DeGroot: Have you maintained any contact with Howard through the years?

Blackwell: Oh yes. I guess the last time I gave a lecture there was about three years ago, but I visited many times during the years.

DeGroot: Do you see much change in the place through the years?

Blackwell: Yes, the math department now is a livelier place than it was when I was there. It's much bigger and the current chairman, Jim Donaldson, is very good and very active. There are some interesting things going on there.

DeGroot: Did you feel or find that discrimination against blacks affected your education or your career after your Ph.D.?

Blackwell: It never bothered me. I'll put it that way. It surely shaped my expectations from the very beginning. It never occurred to me to think about teaching in a major university since it wasn't in my horizon at all.

David Blackwell (lower left), 1930, probably sixth grade.

DeGroot: Even in your graduate-student days at Illinois?

Blackwell: That's right. I just assumed that I would get a job teaching in one of the black colleges. There were 105 black colleges at that time, and I wrote 105 letters of application.

DeGroot: And got 105 offers, I suppose.

Blackwell: No, I eventually got three offers, but I accepted the first one that I got. From Southern University.

DeGroot: Let's move a little further back in time. You grew up in Illinois?

Blackwell: In Centralia, Illinois. Did you ever get down to Centralia or that part of Illinois when you were in Chicago?

DeGroot: No, I didn't.

Blackwell: Well, it's a rather different part of the world from northern Illinois. It's quite southern. Centralia in fact was right on the border line of segregation. If you went south of Centralia to the southern tip of Illinois, the schools were completely segregated in those days. Centralia had one completely black school, one completely white school, and five "mixed" schools.

DeGroot: Well, that sounds like the boundary all right. Which one did you go to?

Blackwell: I went to one of the mixed schools, because of the part of town I lived in. It's a small town. The population was about 12,000 then and it's still about 12,000. The high school had about 1,000 students. I had very good high school teachers in mathematics. One of my high school teachers organized a mathematics club and used to give us problems to work. Whenever we would come up with something that had the idea for a solution, he would write up the solution for us, and send it in our name to a journal called *School Science and Mathematics*. It was a great thrill to see your name in the magazine. I think my name got in there three times. And once my *solution* got printed. As I say, it was really Mr. Huck's write-up based on my idea. [Laughs].

DeGroot: Was your family encouraging about your education?

Blackwell: It was just sort of *assumed* that I would go to college. There was no "Now be sure to study hard" or anything like that. It was just taken for granted that I was going to go to college. They were very, very supportive.

SOME FAVORITE PAPERS

DeGroot: You were quite young when you received your Ph.D. You were 21 or so?

Blackwell: 22. There wasn't any big jump. I just sort of did everything a little faster than normal.

DeGroot: And you've been doing it that way ever since. You've published about 80 papers since that time. Do you have any favorites in that list that you particularly like or that you feel were particularly important or influential?

Blackwell: Oh, I'm sure that I do, but I'd have to look at the list and think about that. May I look?

DeGroot: Sure. This is an open-book exam.

Blackwell: Good. Let's see ... Well, my first *statistical* paper, called "On an equation of Wald" (*Ann. Math. Statist.* **17** 84–87, 1946) grew out of that original conversation with Abe Girshick. That's a paper that I am still really very proud of. It just gives me pleasant feelings every time I think about it.

DeGroot: Remind me what the main idea was.

Blackwell: For one thing it was a proof of Wald's theorem under, I think, weaker conditions than it had been proved before, under sort of *natural* conditions. And the proof is *neat*. Let me show it to you. [Goes to blackboard.]

Suppose that $X_1, X_2, \ldots$ are i.i.d. and you have a stopping rule N, which is a random variable. You want to prove that $E(X_1 + \cdots + X_N) = E(X_1)E(N)$. Well, here's my idea. Do it over and over again. So you have stopping times $N_1, N_2, \ldots$, and you get

$$S_1 = X_1 + \cdots + X_{N_1},$$

$$S_2 = X_{N_1+1} + \cdots + X_{N_1+N_2},$$

$$\ldots$$

Consider $S_1 + \cdots + S_k = X_1 + \cdots + X_{N_1+\cdots+N_k}$. We can write this equation as

$$\frac{S_1 + \cdots + S_k}{k} = \left(\frac{X_1 + \cdots + X_{N_1+\cdots+N_k}}{N_1 + \cdots + N_k}\right)\left(\frac{N_1 + \cdots + N_k}{k}\right).$$

Now let $k \to \infty$. The first term on the right is a subsequence of the X averages. By the strong law of large numbers, this converges to $E(X_1)$. The second term on the right is the average of $N_1, \ldots, N_k$. We are assuming that they have a finite expectation, so this converges to that expectation $E(N)$. Therefore, the sequence

$$\frac{S_1 + \cdots + S_k}{k}$$

converges a.e. Then the converse of the strong law of large numbers says that the expected value of each S_i must be finite, and that

$$\frac{S_1 + \cdots + S_k}{k}$$

must converge to that expectation $E(S_1)$. Isn't that neat?

DeGroot: Beautiful, beautiful.

Blackwell: So that's the proof of Wald's equations just by invoking the strong law of large numbers and its converse. I think I like that because that was the first time that *I* decided that I could do something original. The papers based on my thesis were nice, but those were really Doob's ideas that I was just carrying out. But here I had a really original idea, so I was very pleased with that paper. Then I guess I like my paper with Ken Arrow and Abe Girshick, "Bayes and minimax solutions of sequential decision problems" (*Econometrica* **17** 213–244, 1949).

DeGroot: That was certainly a very influential paper.

Blackwell: That was a serious paper, yes.

DeGroot: There was some controversy about that paper, wasn't there? Wald and Wolfowitz were doing similar things at more or less the same time.

Blackwell: Yes, they had priority. There was no question about that, and I think we did give inadequate acknowledgment to them in our work. So they were very much disturbed about it, especially Wolfowitz. In fact, Wolfowitz was cool to me for more than 20 years.

DeGroot: But certainly your paper was different from theirs.

Blackwell: We had things that they didn't have, there was no doubt about that. For instance, induction backward—calculation backward—that was in our paper and I don't think there is any hint of it in their work. We did go beyond what they had done. Our paper didn't seem to bother Wald too much, but Wolfowitz was annoyed.

DeGroot: Did you know Wald very well or have much contact with him?

Blackwell: Not very well. I had just three or four conversations with him.

IMPORTANT INFLUENCES

DeGroot: I gather from what you said that Girshick was a primary influence on you in the field of statistics.

Blackwell: Oh yes.

DeGroot: Were there other people that you felt had a strong influence on you? Neyman, for example?

Blackwell: Not in my statistical thinking. Girshick was certainly *the* most important influence on me. The other person who had just one influence, but it was a very big one, was Jimmie Savage.

DeGroot: What was that one influence?

Blackwell: Well, he explained to me that the Bayes approach was the right way to do statistical inference. Let me tell you how that happened. I was at Rand, and an economist came in one day to talk to me. He said that he had a problem. They were preparing a recommendation to the Air Force on how to divide their research budget over the next five years and, in particular, they had to decide what fraction of it should be devoted to long-range research and what fraction of it should be devoted to more immediate developmental research.

"Now," he said, "one of the things that this depends on is the probability of a major war in the next five years. If it's large then, of course, that would shift the emphasis toward developing what we already know how to do, and if it's small then there would be more emphasis on long-range research. I'm not going to ask

David Blackwell, about 1945.

you to tell me a number, but if you could give me any guide as to how I could go about finding such a number I would be grateful." Oh, I said to him, that question just doesn't make sense. Probability applies to a long sequence of repeatable events, and this is clearly a unique situation. The probability is either 0 or 1, but we won't know for five years, I pontificated. [Laughs] So the economist looked at me and nodded and said, "I was afraid you were going to say that. I have spoken to several other statisticians and they have all told me the same thing. Thank you very much." And he left.

Well, that conversation bothered me. The fellow had asked me a reasonable, serious question and I had given him a frivolous, sort of flip, answer, and I wasn't happy. A couple of weeks later Jimmie Savage came to visit Rand, and I went in and said hello to him. I happened to mention this conversation that I had had, and then he started telling me about deFinetti and personal probability. Anyway, I walked out of his office half an hour later with a completely different view on things. I now understood what was the right way to do statistical inference.

DeGroot: What year was that?

Blackwell: About 1950, maybe 1951, somewhere around there. Looking back on it, I can see that I was emotionally and intellectually prepared for Jimmie's message because I had been thinking in a Bayesian way about sequential analysis, hypothesis testing, and other statistical problems for some years.

DeGroot: What do you mean by thinking in a Bayesian way? In terms of prior distributions?

Blackwell: Yes.

DeGroot: Wald used them as a mathematical device.

Blackwell: That's right. It just turned out to be clearly a very natural way to think about problems and it was mathematically beautiful. I simply regretted that it didn't correspond with reality. [Laughs] But then what Jimmie was telling me was that the way that I had been thinking all the time was really the right way to think, and not to worry so much about empirical frequencies. Anyway, as I say, that was just one very big influence on me.

DeGroot: Would you say that your statistical work has mainly used the Bayesian approach since that time?

Blackwell: Yes. I simply have not worked on problems where that approach could not be used. For instance, all my work in dynamic programming just has that Bayes approach in it. That is *the* standard way of doing dynamic programming.

DeGroot: You wrote a beautiful book called *Basic Statistics* (New York, McGraw-Hill, 1970) that was really based on the Bayesian approach, but as I recall you never once mentioned the word "Bayes" in that book. Was that intentional?

Blackwell: No, it was not intentional.

DeGroot: Was it that the terminology was irrelevant to the concepts that you were trying to get across?

Blackwell: I doubt if the word "theorem" was ever mentioned in that book. That was not originally intended as a book, by the way. It was simply intended as a set of notes to give my students in connection with lectures in this elementary statistics course. But the students suggested that it should be published and a McGraw-Hill man said that he would be interested. It's just a set of notes. It's short; I think it's less than 150 pages.

DeGroot: It's beautiful. There are a lot of wonderful gems in those 150 pages.

Blackwell: Well, I enjoyed teaching the course.

DeGroot: Do you enjoy teaching from your own books?

Blackwell: No, not after a while. I think about five years after the book was published, I stopped using it. Just because I got bored with it. When you reach the point where *you're* not learning anything, then I think it's probably time to change something.

DeGroot: Are you working on other books at the present time?

Blackwell: No, except that I am *thinking about* writing a more elementary version of parts of your book on optimal statistical decisions because I have been using it in a course and the undergraduate students say that it's too hard.

DeGroot: Uh oh. I've been thinking of doing the same thing. [Laughs] Well, I am just thinking generally in terms of an introduction to Bayesian statistics for undergraduates.

Blackwell: Very good. I really hope you do it, Morrie. It's needed.

DeGroot: Well, I really hope you do it, too. It would be interesting. Are there courses that you particularly enjoy teaching?

Blackwell: I like the course in Bayesian statistics using your book. I like to teach game theory. I haven't taught it in some years, but I like to teach that course. I also like to teach, and I'm teaching right now, a course in information theory.

DeGroot: Are you using a text?

Blackwell: I'm not using any one book. Pat Billingsley's book *Ergodic Theory and Information* comes closest to what I'm doing. I like to teach measure theory. I regard measure theory as a kind of hobby, because to do probability and statistics you don't really need very much measure theory. But there are these fine, nit-picking points that most people ignore, and rightly so, but that I sort of like to worry about. [Laughs] I know that it is not important, but it is interesting to me to worry about regular conditional probabilities and such things. I think I'm one of only three people in our department who really takes measure theory seriously. Lester [Dubins] takes it fairly seriously, and so does Jim Pitman. But the rest of the people just sort of ignore it. [Laughs]

"I WOULD LIKE TO SEE MORE EMPHASIS ON BAYESIAN STATISTICS"

DeGroot: Let's talk a little bit about the current state of statistics. What areas do you think are particularly important these days? Where do you see the field going?

Blackwell: I can tell you what I'd like to see happen. First, of course, I would like to see more emphasis on Bayesian statistics. Within that area it seems to me that one promising direction which hasn't been explored at all is Bayesian experimental design. In a way, Bayesian statistics is much simpler than classical statistics in that once you're given a sample, all you have to do are calculations based on that sample. Now, of course, I say "all you have to do"—sometimes those calculations can be horrible. But if you are trying to design an experiment, that's not all you have to do. In that case, you have to look at all the different samples you might get and evaluate every one of them in order to calculate an overall risk, to decide whether the experiment is worth doing and to choose among the experiments. Except in very special situations, such as when to stop sampling, I don't think a lot of work has been done in that area.

DeGroot: I think the reason there hasn't been very much done is because the problems are so hard. It's really hard to do explicitly the calculations that are required to find *the* optimal experiment. Do you think that perhaps the computing power that is now available would be helpful in this kind of problem?

Blackwell: That's certainly going to make a difference. Let me give you a simple example that I have never seen worked out but I am sure could be worked out. Suppose that you have two independent Bernoulli variables, say, a proportion among males and a proportion among females. They are independent, and you are interested in estimating the sum of those proportions or some linear combination of those proportions. You are going to take a sample in two stages. First of all, you can ask how large should the first sample be? And then, based on the first sample, how should you allocate proportions in the second sample?

DeGroot: Are you going to draw the first sample from the total population?

Blackwell: No. You have males and you have females, and you have a total sample effort of size N. Now you can pick some number $n \leq N$ to be your sample size. And you can allocate those n observations among males and females. Then based on how that sample comes out, you can allocate your second sample. What is the best initial allocation, and how much better is it than just doing it all in one stage? Well, I haven't done that calculation but I'm sure that it can be done. It would be an interesting kind of thing and it could be extended to more than two categories. That's an example of the sort of thing on which I would like to see a lot of work done—Bayesian experimental design.

One of the things that I worry about a little is that I don't see theoretical statisticians having as much contact with people in other areas as I would like to see. I notice here at Berkeley, for example, that the people in Operations Research seem to have much closer contact with industry than the people in our department do. I think we might find more interesting problems if we did have closer contact.

DeGroot: Do you think that the distinctions between applied and theoretical statistics are still as rigid as they were years ago or do you think that the field is blending more into a unified field of statistics in which such distinctions are not particularly meaningful? I see the emphasis on data analysis which is coming about, and the development of theory for data analysis and so on, blurring these distinctions

Kenneth Arrow, David Blackwell, and M. A. Girshick, Santa Monica, September 1948.

between theoretical and applied statistics in a healthy way.

Blackwell: I guess I'm not familiar enough with data analysis and what computers have done to have any interesting comments on that. I see what some of our people and people at Stanford are doing in looking at large-dimensional data sets and rotating them so that you can see lots of three-dimensional projections and such things, but I don't know whether that suggests interesting theoretical questions or not. Maybe that's not important, whether it suggests interesting theoretical questions. Maybe the important thing is that it helps contribute to the solution of practical problems.

INFINITE GAMES

DeGroot: What kind of things are you working on these days?

Blackwell: Right now I am working on some things in information theory, and still trying to understand some things about infinite games of perfect information.

DeGroot: What do you mean by an infinite game?

Blackwell: A game with an infinite number of moves. Here's an example. I write down a 0 or a 1, and you write down a 0 or a 1, and we keep going indefinitely. If the sequence we produce has a limiting frequency, I win. If not, you win. That's a trivial game because I can force it to have a limiting frequency just by doing the opposite of whatever you do. But that's a simple example of an infinite game.

DeGroot: Fortunately, it's one in which I'll never have to pay off to you.

Blackwell: Well, we can play it in such a way that you would have to pay off.

DeGroot: How do we do that?

Blackwell: You must specify a strategy. Let me give you an example. You know how to play chess in just one move: You prepare a complete set of instructions so that for every situation on the chess board you specify a possible response. Your one move is to prepare that complete set of instructions. If you have a complete set and I have a complete set, then we can just play the game out according to those instructions. It's just one move. So in the same way, you can specify a strategy in this infinite game. For every finite sequence that you might see up to a given time as past history, you specify your next move. So you can define this function once and for all, and I can define a function, and then we can mathematically assess those functions. I can prove that there is a specific function of mine such that no matter what function you specify, the set will have a limiting frequency.

DeGroot: So you could extract money from me in a finite amount of time. [Laughs]

Blackwell: Right. Anyway it's been proved that all such infinite games with Borel payoffs are determined, and I've been trying to understand the proof for several years now. I'm still working on it, hoping to understand it and simplify it.

DeGroot: Have you published papers on that topic?

Blackwell: Just one paper many years ago. Let me remind myself of the title [checking his files], "Infinite games and analytic sets" (*Proc. Natl. Acad. Sci. U.S.A.* **58** 1836–1837, 1967). This is the only paper I've published on infinite games; and that's one of my papers that I like very much, by the way. It's an application of games to prove a theorem in topology. I sort of like the idea of connecting those two apparently not closely related fields.

DeGroot: Have you been involved in applied projects or applied problems through the years, at Rand or elsewhere, that you have found interesting and that have stimulated research of your own?

Blackwell: I guess so. My impression though is this: When I have looked at real problems, interesting theorems have sometimes come out of it. But never anything that was helpful to the person who had the problem. [Laughs]

DeGroot: But possibly to somebody else at another time.

Blackwell: Well, my work on comparison of experiments was stimulated by some work by Bohnenblust, Sherman, and Shapley. We were all at Rand. They called their original paper "Comparison of reconnaissances," and it was *classified* because it arose out of some question that somebody had asked them. I recognized a relation between what they were doing and sufficient statistics, and proved that they were the same in a special case. Anyway, that led to this development which I think is interesting theoretically, and to which you have contributed.

DeGroot: Well, I have certainly used your work in that area. And it has spread into diverse other areas. It is used in economics in comparing distributions of income, and I used it in some work on comparing probability forecasters.

Blackwell: And apparently people in accounting have made some use of these ideas. But anyway, as I say, nothing that I have done has ever helped the person who raised the question. But there is no doubt in my mind that you do get interesting problems by looking at the real world.

"I DON'T HAVE ANY DIFFICULTIES WITH RANDOMIZATION"

DeGroot: One of the interesting topics that comes out of a Bayesian view of statistics is the notion of randomization and the role that it should play in statistics. Just this little example you were talking about before with two proportions made me think about that. We just assume that we are drawing the observations at random from within each subpopulation in that example, but perhaps basically because we don't have much choice. Do you have any thoughts about whether one should be drawing observations at random?

Blackwell: I don't have any difficulties with randomization. I think it's probably a good idea. The strict theoretical idealized Bayesian would of course never need to randomize. But randomization probably protects us against our own biases. There are just lots of ways in which people differ from the ideal Bayesian. I guess the ideal Bayesian, for example, could not think about a theorem as being probably true. For him, presumably, all true theorems have probability 1 and all false ones have probability 0. But you and I know that's not the way we think. I think of randomization as being a protection against your own imperfect thinking.

DeGroot: It is also to some extent a protection against others. Protection for you as a statistician in presenting your work to the scientific community, in the sense that they can have more belief in your conclusions if you use some randomization procedure rather than your own selection of a sample. So I see it as involved with the sociology of science in some way.

Blackwell: Yes, that's an important virtue of randomization. That reminds me of something else though. We tend to think of evidence as being valid only when it comes from random samples or samples selected in a probabilistically specified way. That's wrong, in my view. Most of what we have learned, we have learned just by observing what happens to come along, rather than from carefully controlled experiments. Sometimes statisticians have made a mistake in throwing away experiments because they were not properly controlled. That is not to say that randomization isn't a good idea, but it is to say that you should not reject data just because they have been obtained under uncontrolled conditions.

DeGroot: You were the Rouse Ball Lecturer at Cambridge in 1974. How did that come about and what did it involve?

Blackwell: Well, I was in England for two years, 1973–1975, as the director of the education-abroad program in Great Britain and Ireland for the University of California. I think that award was just either Peter Whittle's or David Kendall's idea of how to get me to come up to Cambridge to give a lecture. One of the things which delighted me was that it was named the Rouse Ball Lecture because it gave me an opportunity to say something at Cambridge that I liked—namely, that I had heard of Rouse Ball long before I had heard of Cambridge. [Laughs]

DeGroot: Well, tell me about Rouse Ball.

Blackwell: He wrote a book called *Mathematical Recreations and Essays*. You may have seen the book. I first came across it when I was a high school student. It was one of the few mathematics books in our library. I was fascinated by that book. I can still picture it. Rouse Ball was a 19th century mathematician, I think. [Walter William Rouse Ball, 1850–1925] Anyway, this is a lectureship that they have named after him.

DeGroot: I guess there aren't too many Bayesians on the statistics faculty here at Berkeley.

Blackwell: No. I'd say, Lester and I are the only ones in our department. Of course, over in Operations Research, Dick Barlow and Bill Jewell are certainly sympathetic to the Bayesian approach.

DeGroot: Is it a topic that gets discussed much?

Blackwell: Not really. It used to be discussed here but you very soon discover that it's sort of like religion; that it has an appeal for some people and not for other people, and you're not going to change anybody's mind by discussing it. So people just go their own ways. What has happened to Bayesian statistics surprised me. I expected it either to catch on and just sweep the field or to die. And I was rather confident that it would die. Even though to me it was the right way to think, I just didn't think that it would have a chance to survive. But I thought that if it did, then it would sweep things. Of course, neither one of those things has happened. Sort of a steady 5–10% of all the work in statistical inference is done from a Bayesian point of view. Is that what you would have expected 20 years ago?

DeGroot: No, it certainly doesn't seem as though that would be a stable equilibrium. And maybe the system is still not in equilibrium. I see the Bayesian approach growing, but it certainly is not sweeping the field by any means.

Blackwell: I'm glad to hear that you see it growing.

DeGroot: Well, there seem to be more and more *meetings* of the Bayesians, anyway. The actuarial group that met here at Berkeley over the last couple of days to discuss credibility theory seems to be a group that just naturally accepts the Bayesian approach in their work in the real world. So there seem to be some pockets of users out there in the world, and I think maybe that's what has kept the Bayesian approach alive.

Blackwell: There's no question in my mind that if the Bayesian approach does grow in the statistical world it will not be because of the influence of other statisticians but because of the influence of actuaries, engineers, business people, and others who actually like the Bayesian approach and use it.

DeGroot: Do you get a chance to talk much to researchers outside of statistics on campus, researchers in substantive areas?

Blackwell: No, I talk mainly to people in Operations Research and Mathematics, and occasionally Electrical Engineering. But the things in Electrical Engineering are theoretical and abstract.

"THE WORD 'SCIENCE' IN THE TITLE BOTHERS ME A LITTLE"

DeGroot: What do you think about the idea of this new journal, *Statistical Science*, in which this conversation will appear? I have the impression that you think the IMS is a good organization doing useful things, and there is really no need to mess with it.

Blackwell: That is the way I feel. On the other hand, I must say that I felt exactly the same way about splitting the *Annals of Mathematical Statistics* into two journals, and that split seems to be working. So I'm hoping that the new journal will add something. I guess the word "science" in the title bothers me a little. It's not clear what the word is intended to convey there, and you sort of have the feeling that it's there more to contribute a tone than anything else.

DeGroot: My impression is that it *is* intended to contribute a tone. To give a flavor of something broader than just what we would think of as theoretical statistics. That is, to reach out and talk about the impact of statistics on the sciences and the interrelationship of statistics with the sciences, all kinds of sciences.

Blackwell: Now I'm all in favor of that. For example, the relation of statistics to the law is to me a quite appropriate topic for articles in this journal. But somehow calling it "science" doesn't emphasize that direction. In fact, it rather suggests that that's *not* the direction. It sounds as though it's tied in with things that are supported by the National Science Foundation and to me that restricts it.

DeGroot: The intention of that title was to convey a broad impression rather than a restricted one. To give a broader impression than just statistics and probability, to convey an applied flavor and to suggest links to all areas.

Blackwell: Yes. It's analogous to computer science, I guess. I think *that* term was rather deliberately chosen. My feeling is that the IMS is just a beautiful organization. It's about the right size. It's been successful for a good many years. I don't like to see us become ambitious. I like the idea of just sort of staying the way we are, an organization run essentially by amateurs.

DeGroot: Do you have the feeling that the field of statistics is moving away from the IMS in any way? That was one of the motivations for starting this journal.

David Blackwell, 1984.

Blackwell: Well, of course, statistics has always been substantially bigger than the IMS. But you're suggesting that the IMS represents a smaller and smaller fraction of statistical activity.

DeGroot: Yes, I think that might be right.

Blackwell: You know, Morrie, I see what you're talking about happening in mathematics. It's less and less true that all mathematics is done in mathematics departments. On the Berkeley campus, I see lots of interesting mathematics being done in our department, in Operations Research, in Electrical Engineering, in Mechanical Engineering, some in Business Administration, a lot in the Economics Department by Gerard Debreu and his colleagues; a lot of really interesting, high class mathematics is being done outside mathematics departments. What you're suggesting is that statistics departments and the journals in which they publish are not necessarily the centers of statistics the way they used to be, that a lot of work is being done outside. I'm sure that's right.

DeGroot: And perhaps *should* be done outside statistics departments. That used to be an unhealthy sign in the field, and we worked hard in statistics departments to collect up the statistics that was being done around the campus. But I think now that the field has grown and matured, that it is probably a healthy thing to have some interesting statistics being done outside.

Blackwell: Yes. Consider the old problem of pattern recognition. That's a statistical problem. But to the extent that it gets solved, it's not going to be solved by people in statistics departments. It's going to be solved by people working for banks and people working for other organizations who really need to have a device that can look at a person and recognize him in lots of different configurations. That's just one example of the cases where we're somehow too narrow to work on a lot of serious statistical problems.

DeGroot: I think that's right, and yet we have something important to contribute to those problems.

Blackwell: I would say that we *are* contributing, but indirectly. That is, people who are working on the problems have studied statistics. It seems to me that a lot of the engineers I talk to are very familiar with the basic concepts of decision theory. They know about loss functions and minimizing expected risks and such things. So, we have contributed, but just indirectly.

DeGroot: You are in the National Academy of Sciences ...

Blackwell: Yes, but I'm very inactive.

DeGroot: You haven't been involved in any of their committees or panels?

Blackwell: No, and I'm not sure that I would want to be. I guess I don't like the idea of an official committee making scientific pronouncements. I like people to form opinions about scientific matters just on the basis of listening to individual scientists. To have one group with such overwhelming prestige bothers me a little.

DeGroot: And it is precisely the prestige of the Academy that they rely on when reports get issued by these committees.

Blackwell: Yes. So I think it's just great as a purely honorific organization, so to speak. To meet just once a year, and elect people more or less at random. I think everybody that's in it has done something reasonable and even pretty good, in fact. But on the other hand, there are at least as many people *not* in it who have done good things as there are in it. It's kind of a random selection process.

DeGroot: So you think it's a good organization as long as it doesn't do anything.

Blackwell: Right. I'm proud to be in it, but I haven't been active. It's sort of like getting elected to Phi Beta Kappa—it's nice if it happens to you . . .

"I PLAY WITH THIS COMPUTER"

DeGroot: Do you feel any relationship between your professional work and the rest of your life, your interests outside of statistics? Is there any influence of the outside on what you do professionally, or are they just sort of separate parts of your life?

Blackwell: Separate, except that my friends are also my colleagues. It's only through the people with whom I associate outside that there's any connection. It's hard to think of any other real connection.

DeGroot: It's not obvious what these connections might be for anyone. One's political views or social views seem to be pretty much independent of the technical problems we work on.

Blackwell: Yes. Although it's hard to see how it could *not* have an influence, isn't it? I guess my life seems all of a piece to me but yet it's hard to see where the connections are. [Laughs]

DeGroot: What do you see for your future?

Blackwell: Well, just gradually to wind down, gracefully I hope. I expect to get more interested in computing. I have a little computer at home, and it's a lot of fun just to play with it. In fact, I'd say that I play with this computer here in my office at least as much as I do serious work with it.

DeGroot: What do you mean by play?

Blackwell: Let me give you an example. You know the algorithm for calculating square roots. You start with a guess and then you divide the number by your guess and take the average of the two. That's your next guess. That's actually Newton's method for finding square roots, and it works very well. Sometimes doing statistical work, you want to take the square root of a positive definite matrix. It occurred to me to ask whether that algorithm works for finding the square root of a positive definite matrix. Before I got interested in computing, I would have tried to solve it theoretically. But what did I do? I just wrote up a program and put it on the computer to see if it worked. [Goes to blackboard]

Suppose that you are given the matrix M and want to find $M^{1/2}$. Let G be your guess of $M^{1/2}$. Then your new guess is $\frac{1}{2}(G + MG^{-1})$. You just iterate this and see if it converges to $M^{1/2}$. Now, Morrie, I want to show you what happens. [Goes to terminal]

Let's do it for a 3×3 matrix. We're going to find the square root of a positive definite 3×3 matrix. Now, if you happen to have in mind a particular 3×3 positive definite matrix whose square root you want, you could enter it directly. I don't happen to have one in mind, but I do know a theorem: If you take any nonsingular 3×3 matrix A, then AA' is going to be positive definite. So I'm just going to enter any 3×3 nonsingular matrix [putting some numbers into the terminal] and let $M = AA'$. Now, to see how far off your guess G is at any stage, you calculate the Euclidean norm of the 3×3 matrix $M - G^2$. That's what I call the error. Let's start out with the identity matrix I as our initial guess. We get a big error, 29 million. Now let's iterate. Now the error has dropped down to 7 million. It's going to keep being divided by 4 for a long time. [Continuing the iterations for a while] Now notice, we're not bad. There's our guess, there's its square, there's what we're trying to get. It's pretty close. In fact the error is less than one. [Continuing] Now the error is really small. Look at that, isn't that beautiful? So there's just no question about it. If you enter a matrix at random and it works, then that sort of settles it.

But now wait a minute, the story isn't quite finished yet. Let me just continue these iterations . . . Look at that! The error got bigger, and it keeps getting bigger. [Continuing] Isn't that lovely stuff?

DeGroot: What happened?

Blackwell: Isn't that an interesting question, what happened? Well, let me tell you what happened. Now you can study it theoretically and ask, should it converge? And it turns out that it will converge if, and essentially only if, your first guess commutes with the matrix M. That's what the theory gives you. Well, my first guess was I. It commutes with everything. So the procedure theoretically converges. However, when you calculate, you get round-off errors. By the way, if your first guess commutes, then all subsequent guesses will commute. However, because of round-off errors, the matrices that you actually get don't *quite* commute. There are two ways to do this. We could take MG^{-1} or we could have taken $G^{-1}M$. Of course, if M commutes with G, then it commutes with G^{-1} and it doesn't matter which way you do it. But if you don't calculate G exactly at some stage, then it will not quite commute. And in fact, what I have here on the computer is a calculation at each stage of the noncommutativity norm. That shows you how different MG^{-1} is from $G^{-1}M$. I didn't point those values out to you, but they started out as essentially 0, and then there was a 1 in the 15th place, and then a 1 in the 14th place, and so on. By this stage, the noncommutativity norm has built up to the point where it's having a sizable influence on the thing.

DeGroot: Is it going to diverge or will it come back down after some time?

Blackwell: It won't come back down. It will reach a certain size, and sometimes it will stay there and sometimes it will oscillate. That is, one G will go into a quite different G, but then that G will come back to the first one. You get periods, neither one of them near the truth. So that's what I mean by just playing, instead of sitting down like a serious mathematician and trying to prove a theorem. Just try it out on the computer and see if it works. [Laughs]

DeGroot: You can save a lot of time and trouble that way.

Blackwell: Yes. I expect to do more and more of that kind of playing. Maybe I get lazier as I get older. It's fun, and it's an interesting toy.

DeGroot: Do you find yourself growing less rigorous in your mathematical work?

Blackwell: Oh yes. I'm much more interested in the ideas, and in truth under not-completely-specified hypotheses. I think that has happened to me over the last 20 years. I can certainly notice it now. Jim MacQueen was telling me about something that he had discovered. If you take a vector and calculate the squared correlation between that vector and some permutation of itself, then the average of that squared correlation over all possible permutations is some simple number. Also, there was some extension of this result to k vectors. He has an interesting algebraic identity. He told me about it, but instead of my trying to prove it, I just selected some numbers at random and checked it on the computer. Also, I had a conjecture that some stronger result was true. I checked it for some numbers selected at random and it turned out to be true for him and *not* true for what I had said. Well, that just settles it. Because suppose you have an algebraic function $f(x_1, \ldots, x_n)$ and you want to find out if it is identically 0. Well, I think it's true that any algebraic function of n variables is either identically 0 or the set of x's for which it is 0 is a set that has measure 0. So you can just select x's at random and evaluate f. If you get 0, it's identically 0. [Laughs]

DeGroot: You wouldn't try even a second set of x's?

Blackwell: I did. [Laughs]

DeGroot: Getting more conservative in your old age.

Blackwell: Yes. [Laughs] I've been wondering whether in teaching statistics the typical set-up will be a lot of terminals connected to a big central computer or a lot of small personal computers. Let me turn the interview around. Do you have any thoughts about which way that is going or which way it ought to go?

DeGroot: No, I don't know. At Carnegie-Mellon we are trying to have both worlds by having personal computers but having them networked with each other. There's a plan at Carnegie-Mellon that each student will have to have a personal computer.

Blackwell: Now when you say each student will have to have a personal computer, where will it be physically located?

DeGroot: Wherever he lives.

Blackwell: So that they would not actually use computers in class on the campus?

DeGroot: Well, this will certainly lessen the burden on the computers that are on campus, but in a class you would have to have either terminals or personal computers for them.

Blackwell: Yes. I'm pretty sure that in our department in five years we'll have several classrooms in which each seat will be a work station for a student, and in front of him will be either a personal computer or a terminal. I'm not sure which, but that's the way we're going to be in five years.

"I WOULDN'T DREAM OF TALKING ABOUT A THEOREM LIKE THAT NOW"

DeGroot: A lot of people have seen you lecture on film. I know of at least one film you made for the American Mathematical Society that I've seen a few times. That's a beautiful film, "Guessing at Random."

Blackwell: Yes. I now, of course, don't think much of those ideas. [Laughs]

DeGroot: There were some *minimax* ideas in there . . .

Blackwell: Yes, that's right. That was some work that I did before I became such a committed Bayesian. I wouldn't dream of talking about a theorem like that now. But it's a nice result . . .

DeGroot: It's a nice result and it's a beautiful film. Delivered so well.

Blackwell: Let's see . . . How does it go? If I were doing it now I would do a weaker and easier Bayesian form of the theorem. You were given an arbitrary sequence of 0's and 1's, and you were going to observe successive values and you had to predict the next one. I proved certain theorems about how well you could do against every possible sequence. Well, *now* I would say that you have a probability distribution on the set of all sequences. It's a general fact that if you're a Bayesian, you don't have to be clever. You just *calculate*. Suppose that somebody generates an arbitrary sequence of 0's and 1's and it's your job after seeing each finite segment to predict the next coordinate, 0 or 1, and we keep track of how well you do. Then I have to be clever and invoke the minimax theorem to devise a procedure that asymptotically does very well in a certain sense. But now if you just put a prior distribution on the set of sequences, any Bayesian knows what to do. You just calculate the probability of the next term being a 1 given the past history. If it's more than ½ you predict a 1, if it's less than ½ you predict a 0. And that simple procedure has the corresponding Bayesian version of all the things that I talked about in that film. You just know what is the right thing to do.

DeGroot: But how do you know that you'll be doing well in relation to the reality of the sequence?

Blackwell: Well, the theorem of course says that you'll do well for all sequences except a set of measure zero according to your own prior distribution, and that's all a Bayesian can hope for. That is, you have to give up something, but it just makes life so much *neater*. You just know that this is the right thing to do.

I encountered the same phenomenon in information theory. There is a very good theory about how to transmit over a channel, or how to transmit over a sequence of channels. The channel may change from day to day, but if you know what it is every day, then you can transmit over it. Now suppose that the channel varies in an arbitrary way. That is, you have one of a finite set of channels, and every day you're going to be faced with one of these channels. You have to put in the input and a guy at the other end gets an output. The question is, how well can you do against all possible channel sequences?

You don't really know what the weather is out there, so you don't know what the interference is going to be. But you want to have a code that transmits well for all possible weather sequences. If you just analyze the problem crudely, it turns out that you can't do *anything* against all possible sequences. However, if you select the code in a certain random way, your overall error probability will be small for each weather sequence. So you see, it's a nice theoretical result but it's unappealing. However, you can get exactly the same result if you just put a probability distribution on the sequences. Well, the weather could be any sequence, but you expect it to be sort of this way or that. Once you put a probability distribution on the set of sequences, you no longer need random codes. And there is a deterministic code that gives you that same result that you got before. So either you must behave in a random way, or you must put a probability distribution on nature.

[Looking over a copy of his paper, BLACKWELL, D., BREIMAN, L. and THOMASIAN, A. J., "The capacities of certain channel classes under random coding," *Ann. Math. Statist.* **31** 558–567, 1960] I don't think we did the nice easy part. We behaved the way Wald behaved. You see, the minimax theorem says that if for every prior distribution you can achieve a certain gain, then there is a random way of behaving that achieves that gain for every parameter value. You don't need the prior distribution; you can throw it away. Well, I'm afraid that in this paper, we invoked the minimax theorem. We said, take any prior distribution on the set of channel sequences. Then you can achieve a certain rate of transmission for that prior distribution. Now you invoke the minimax theorem and say, therefore, there is a randomized way of behaving which enables you to achieve that rate against every possible sequence. I now wish that we had *stopped* at the earlier point. [Laughs] For us, the Bayesian analysis was just a preliminary which, with the aid of the minimax theorem, enabled us to reach the conclusions we were seeking. That was Wald's view and that's the view that we took in that paper. I'm sure I was already convinced that the Bayes approach was the right approach, but perhaps I deferred to my colleagues.

DeGroot: That's a very mild compromise. Going *beyond* what was necessary for a Bayesian resolution of the problem.

Blackwell: That's right. Also, I suspect that I had Wolfowitz in mind. He was a real expert in information theory, but he wouldn't have been interested in anything Bayesian.

DeGroot: What about the problem of putting prior distributions on spaces of infinite sequences or function spaces? Is that a practical problem and is there a practical solution to the problem?

Blackwell: I wouldn't say for infinite sequences, but I think it's a very important practical problem for large finite sequences and I have no idea how to solve it. For example, you could think that the pattern recognition problem that I was talking about before is like that. You see an image on a TV screen. That's just a long finite sequence of 0's and 1's. And now you can ask how likely it is that that sequence of 0's and 1's is intended to be the figure 7, say. Well, with some you're certain that it is and some you're certain that it isn't, and with others there's a certain probability that it is and a probability that it isn't. The problem of describing that probability distribution is a very important problem. And we're just not close to knowing how to describe probability distributions over long finite sequences that correspond to our opinions.

DeGroot: Is there hope for getting such descriptions?

Blackwell: I don't know. But again it's a statistical problem that is not going to be solved by professors of statistics in universities. It might be solved by people in artificial intelligence, or by researchers outside universities.

"JUST TELL ME ONE OR TWO INTERESTING THINGS"

DeGroot: There's an argument that says that under the Bayesian approach, you have to seek the optimal decision and that's often just too hard to find. Why not settle for some other approach that requires much less structure and get a reasonably good answer out of it, rather than an optimal answer? Especially in these kinds of problems where we don't know how to find the optimal answer.

Blackwell: Oh, I think everybody would be satisfied with a reasonable answer. I don't see that there's more of an emphasis in the Bayesian approach on optimal decisions than in other approaches. I separate Bayesian inference from Bayesian decision. The inference problem is just calculating a posterior distribution, and that has nothing to do with the particular decision that you're going to make. The same posterior distribution could be used by many different people making different decisions. Even in calculating the posterior distribution, there is a lot of approximation. It just can't be done precisely in interesting and important cases. And I don't think anybody who is interested in applying Bayes method would insist on something that's precise to the fifth decimal place. That's just the conceptual framework in which you want to work, and which you want to approximate.

DeGroot: That same spirit can be carried over into the decision problem, too. If you can't find the optimum decision, you settle for an approximation to it.

Blackwell: Right.

DeGroot: In your opinion, what have been the major breakthroughs in the field of statistics or probability through the years?

Blackwell: It's hard to say . . . I think that theoretical statistical thinking was just completely dominated by Wald's ideas for a long time. Charles Stein's discovery that $\bar{X}$ is inadmissible was certainly important. Herb Robbin's work on empirical Bayes was also a big step, but possibly in the wrong direction.

You know, I don't view myself as a statesman or a guy with a broad view of the field or anything like that. I just picked directions that interested me and worked in them. And I have had fun.

DeGroot: Well, despite the fact that you didn't choose the problems for their impact or because of their importance, a lot of people have gained a lot from your work.

Blackwell: I guess that's the way scholars *should* work. Don't worry about the overall importance of the problem; work on it if it looks interesting. I think there's probably a sufficient correlation between interest and importance.

DeGroot: One component of the interest is probably that others are interested in it, anyway.

Blackwell: That's a big component. You want to tell somebody about it after you've done it.

DeGroot: It has not always been clear that the published papers in our more abstract journals did succeed in telling anybody about it.

Blackwell: That's true. But if you get the fellow to give a lecture on it, he'll probably be able to tell you something about it. Especially if you try to restrict him: Look, don't tell me everything. Just tell me *one or two* interesting things.

DeGroot: You have a reputation as one of the finest lecturers in the field. Is that your style of lecturing?

Blackwell: I guess it is. I try to emphasize that with students. I notice that when students are talking about their theses or about their work, they want to tell you everything they know. So I say to them: You know much more about this topic than anybody else. We'll never understand it if you tell it all to us. Pick just one interesting thing. Maybe two.

DeGroot: Thank you, David.

DAVID BLACKWELL

Interviewed by Donald J. Albers

At sixteen, David H. Blackwell enrolled at the University of Illinois to earn a bachelor's degree so that he could get a job as an elementary school teacher. Six years later, he had his Ph.D. in mathematics and a fellowship to the Institute for Advanced Study at Princeton.

Today, he is a much honored professor of statistics at the University of California, Berkeley. He is a theoretician, noted for his rigor and clarity, who has made contributions to Bayesian statistics, probability, game theory, set theory, dynamic programming, and information theory. He says, "I've worked in so many areas—I'm sort of a dilettante. Basically, I'm not interested in doing research and I never have been. I'm interested in *understanding*, which is quite a different thing."

Professor Blackwell was interviewed in his office at the University of California in April of 1983. While being interviewed, he went to the board several times to "share something beautiful with somebody else." In a short time, it became clear that David Blackwell, the theoretician, is also a natural-born teacher.

MP: *You were born in Centralia, Illinois, back in* 1919, *just after World War I.*

Blackwell: That's right, in Centralia, a small town in Southern Illinois with a population of about 12,000.

MP: *You whizzed through elementary and secondary school, graduating at the young age of* 16. *What remembrances do you have of your childhood in Centralia and the influences on you? Were your mother and father mathematically inclined?*

Blackwell: No, they weren't. My grandfather ran a store. I had an uncle who could add numbers, three columns at a time, and that always impressed me. He never went to school at all; my grandfather taught him.

MP: *Did your family come from Illinois?*

Blackwell: No, my grandfather came from Ohio where he was a schoolteacher and then became a storekeeper.

MP: *To whom do you trace your mathematical abilities?*

Blackwell: To my grandfather, I suppose. I never knew him. Apparently he was a well-educated man—he certainly left a large library of books. The first algebra book I ever saw was in his library. I don't think he graduated from college but I know he was a schoolteacher and, in fact, that's how he met my grandmother. She was a student in his class while he was teaching in Tennessee. The reason that his son, my uncle, never went to school was that my grandfather never let him. He was afraid he would be mistreated because he was black.

MP: *But your grandfather went to school.*

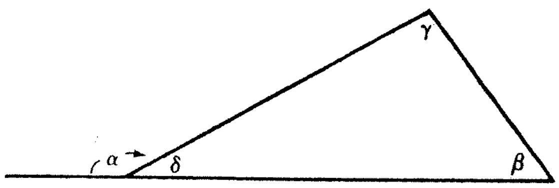
Blackwell: But that was in Ohio, not in Illinois! Southern Illinois was probably fairly racist even when I was growing up there. The school I went to was integrated, but there was also a segregated white school in that same town. There were in fact two segregated schools, one that only blacks could attend and one that only whites could attend. But I was not even aware of these problems—I had no sense of being discriminated against. My parents protected us from it and I didn't encounter enough of it in the schools to notice it.

"Geometry is a Beautiful Subject!"

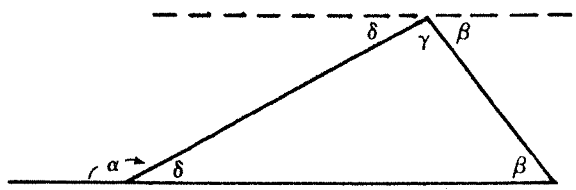
MP: *Were there teachers along the way who made a particular difference, who made learning exciting?*

Blackwell: Oh, there were many. A couple of years ago my first-grade teacher came to see me here. She's about 82 now and living in Southern California. I hadn't heard of her in a number of years and of course I was in her class in 1925, but she somehow knew where I was and looked me up here in Berkeley.

But there were a couple of mathematics teachers in particular. My high-school geometry teacher really got me interested in mathematics. I hear it suggested from time to time that geometry might be dropped from the curriculum. I would really hate to see that happen. It is a beautiful subject. Until a year after I had finished calculus it was the only course I had that made me see that mathematics is really beautiful and full of ideas. I still remember the concept of a *helping line*. You have a proposition that looks quite mysterious. Someone draws a line and suddenly it becomes obvious. That's beautiful stuff. I remember the proposition that the exterior angle of a triangle is the sum of the remote interior angles. When you draw that helping line it is completely clear.

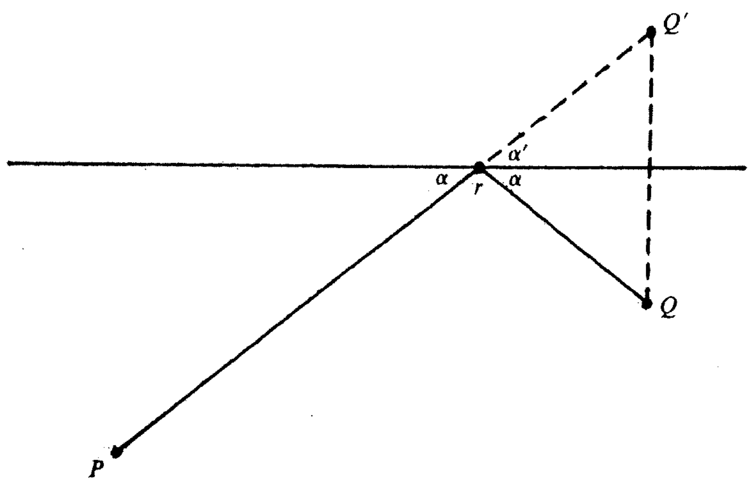


$\alpha = \beta + \gamma$—**why?**



The helping line makes it obvious.

And then there's the river problem where if you want to go from P to the river and then to Q, you have to have equal angles at R to minimize the path. Why is that so?



Again, you construct the mirror image Q' and then it's clear what the shortest path is. The construction of this one extra point changes the problem from something that is mysterious to something that is obvious.

MP: *How about your other high-school courses such as Algebra II and Trigonometry?*

Blackwell: I could do it and I could see that it was useful but it wasn't really exciting. When I went to college I knew that I was going to major in mathematics because I liked it and it was easy for me. But through calculus I thought it wasn't particularly interesting. The most interesting thing I remember from calculus was Newton's method for solving equations. That was the only thing in calculus I really liked. The rest of it looked like stuff that was useful for engineers in finding moments of inertia and volumes and such.

MP: *That's curious in a way, because you are generally portrayed as a theoretician who presents his ideas in beautiful and elegant ways. The first two examples that you give from geometry seem to enforce that. But Newton's method, finding approximations to roots of equations, seems inconsistent with the other examples.*

Blackwell: But it's also geometrical. By the way, I just always understood limits. For some students that's a difficult concept.

MP: *Do you mean epsilon-delta proofs?*

Blackwell: No. There probably were epsilons and deltas in the book, but I don't know whether I understood it in that way. The next year I really fell in love with mathematics. I had a course in elementary analysis. We used Hardy's *Pure Mathematics* as a text. That's the first time I knew that serious mathematics was for me. It became clear that it was not simply a few things that I liked. The whole subject was just beautiful.

Planned to Be an Elementary School Teacher

MP: *Was that in your junior year?*

Blackwell: It was my combination junior and senior year; I was an undergraduate only three years. I took some summer courses and proficiency exams. One of the reasons I went into mathematics is that I have never been especially ambitious. When I went to college I expected to be an elementary school teacher.

MP: *But you had gone through high school and found it pretty easy. You still expected to teach in elementary school?*

Blackwell: That's right. I think the reason may have been that my father had a very good friend who was influential on the school board of a town in Southern Illinois and even before I went to college he had told my father that when I finished he could get me a job. It was all laid out that that's what I was going to do. That was about 1935 or 1936 when jobs were scarce. In order to teach you had to take courses in education and I just kept postponing those courses. Before I had to make up my mind, though, it became clear that I was going to get a master's degree in four years, so then I raised my sights a little. I thought I might teach in a college or in high school.

MP: *Was there ever any doubt that you were going to end up in teaching?*

Blackwell: No, never any doubt about that.

MP: *How did your father respond to your postponing the education courses?*

Blackwell: He had so much confidence in me that he thought that whatever I did was right. He himself was not an educated man—he had gone through only the fourth grade in school. He didn't know much about what went on in a university.

MP: *Your father must have been a pretty good guy.*

Blackwell: Oh, he was a great guy. I found out at the end of my freshman year that he had been borrowing money to send me to college. At the start of my sophomore year I told him that he didn't have to send me any more money because I could support myself. At the time I told him that, it wasn't quite clear how I was going to do it, but I just didn't want him to borrow any more money. I had several jobs, as a waiter and as a dishwasher. I had an NYA job, the equivalent for college students of the WPA. I had a job cleaning cases in the entomology lab and filling vials with alcohol. I did that for a couple of years.

MP: *You realized that your university education was a pretty big sacrifice for the rest of the family. What did your mother think of your university experience?*

Blackwell: She was somewhat more concerned about the specifics of what I was doing. She wondered whether I would be able to get a job once I graduated, but she pretty much left it up to me.

MP: *I am interested in your plans to be an elementary school teacher.*

Blackwell: I don't think I could have remained an elementary school teacher but it wouldn't have surprised me at all had I remained a high-school teacher. I think I could very easily have done that. In fact, after I got my master's degree I suspect that if I had gotten a job as a high-school mathematics teacher I would have taken it.

MP: *But you were demonstrating big mathematical talent at that time—you had received a fellowship.*

Blackwell: But I think I would have done it. You go to college for four years and you go out and you get a job. Some people go on. I knew I could do the course work—there's no question about that—but I didn't know whether I could write a thesis. Does anyone really know whether he can write a thesis until he does?

During my first year of graduate work I knew that I could understand mathematics. I could take a graduate mathematics text, read it and do the problems, and with great difficulty I could read a research paper and a journal. I knew I could do that. But whether I could do anything original I didn't know. I didn't mind trying it but it was not the only path in the world for me. I think I would have been perfectly content being a good high-school mathematics teacher.

MP: *I'm sure you would have been active reading mathematics.*

Blackwell: Oh yes, and I would have been active in something like NCTM. Times are different now, too. One of my high-school teachers went on to become a college mathematics teacher after the war and my high-school physics teacher had a Ph.D. in physics. In those days people wanted to get a job they liked.

Graduate School and the Institute for Advanced Study

MP: *You finished your bachelor's degree in 1938 and then stayed on at Illinois for a master's degree?*

Blackwell: I continued working and for the last two years had fellowships from the University.

MP: *Were fellowships commonly awarded to black students at that time?*

Blackwell: If there was any difficulty I never heard a word of it. During my first year of graduate work a couple of my teachers encouraged me to apply for a fellowship. Let me tell you a story about that. Before the fellowships were announced, one of my fellow graduate students told me that I was going to get a fellowship. I said, "How do you know?" He said, "You're good enough to be supported, either with a fellowship or a teaching assistantship, and they're certainly not going to put you in a classroom." That was funny to me because the fellowships were the highest awards; they gave one the same amount of money and one didn't have to work for it. I have no doubt, looking back on it now, that race did enter into it.

MP: *Were there any black faculty members of the department?*

Blackwell: No, not even in the whole university.

MP: *So it turned out to be a lucky break, and you continued through to the Ph.D. as a student of Joe Doob.*

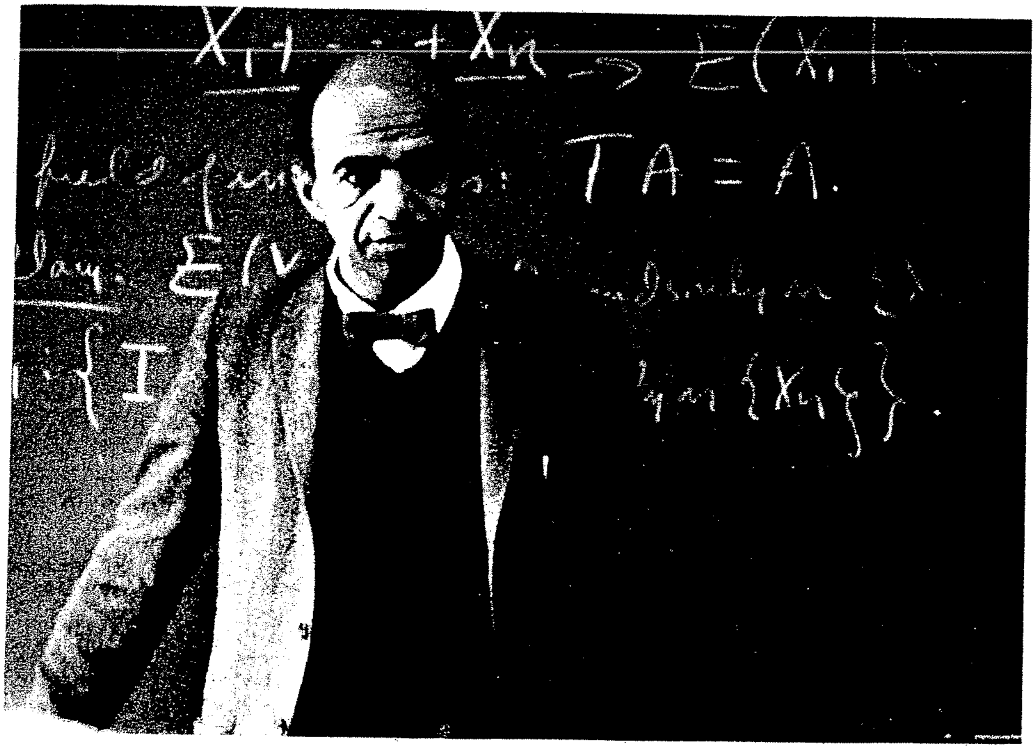
Blackwell: And he was, I would say, clearly the most important mathematical influence on me.

MP: *When did you first come in contact with him?*

Blackwell: My first meeting with him was when I asked him whether I could work with him.

MP: *You hadn't taken a course from him?*

Blackwell: No. Don Kibbey, who was chairman at Syracuse for a number of years, was a teaching assistant at Illinois at that time. One day he asked me whom I was going to work with. I told him I



Blackwell, a distinguished professor at the University of California, Berkeley, went to college expecting to become an elementary school teacher.

didn't know, I hadn't thought much about it, but it was time I started. He said, "Why don't you try to work with Doob? He's a very nice guy." Don was working with Doob. I had a lot of confidence in Don's judgment so I just went up to Doob and asked him if I could work with him and he said yes.

MP: *Was Halmos there at that time?*

Blackwell: Oh yes, he was a year ahead of me. He was also working with Doob.

MP: *Yet you ended up in very different areas.*

Blackwell: Rather different. I have stuck closer to probability than Paul has. I learned a lot from Paul by the way. Many of the papers Doob gave to me, he had given Paul to read a year before. They were on measure theory. Paul had learned them and was anxious to tell someone about them. I was of course anxious to hear them. I suspect I was the first one to hear the first version of Paul's measure theory book.

MP: *Doob took you along with him to the Institute for Advanced Study.*

Blackwell: Now I may not get this story just right, but I think something like this happened. I think it was the custom that members of the Institute would be appointed honorary members of the faculty at Princeton. When I was being considered for membership in the Institute, Princeton University objected to appointing a black man as an honorary member of the faculty. As I understand the story, the Director of the Institute just insisted and threatened, I don't know what, so Princeton withdrew its objections. Apparently there was quite a fuss over this, but I didn't hear a word about it.

MP: *At that time, you were doing measure theoretic work with Doob. But today you're thought of as a statistician. How do you think of yourself? Are you a statistician or a mathematician?*

Blackwell: I don't even try to classify myself and I try not to classify other people. To me that's a fruitless and limiting occupation. In statistics we distinguish between probability and statistics, between theoretical statistics and applied statistics, between Bayesian and non-Bayesian statistics, data analysts and other kinds of statisticians. People try to categorize other people and even themselves; they put themselves in pigeonholes. As I say, I think that's stultifying. You can get good mathematics at all levels of abstraction.

Statistical Beginnings

MP: *How did you get into statistics?*

Blackwell: Let me tell you how I got interested in statistics. In 1945, I was teaching at Howard University. The mathematics department there was small and not very lively. I looked around Washington, D.C., to find mathematics wherever I could. I happened to attend a meeting of the Washington Chapter of the American Statistical Association. Abe Girshick gave a lecture on sequential analysis. To me it was a very interesting lecture. The most interesting part was a theorem that he announced that I just didn't believe. Indeed, I went home to see if I could construct a counterexample, and not believing the theorem it was easy for me to think that I had found a counterexample. I wrote it up and sent it to him—he was working for the Department of Agriculture at that time as a statistician. My counterexample was wrong, but instead of just dimissing this counterexample as the misguided effort of somebody who didn't know statistics, he invited me over to his office to talk about it. He didn't tell me it was wrong—he just asked me over to talk about it and this established a personal relationship and collaboration that lasted until his death. And that's how I got started in statistics, just listening to that one lecture by Abe Girshick. In fact, my first paper in sequential analysis was on this very equation that I didn't believe.

MP: *What is sequential analysis?*

Blackwell: It is the analysis of an experiment where the number of trials is not specified in advance. It's the analysis of sequential experiments—that's the only difference between what is called sequential analysis and fixed sample-size analysis. You can either start out with how many subjects you're going to have and how many trials you're going to make, or you can say, I'm going to keep looking until I reach a conclusion. Of course, people had been doing that informally for a long time, but Wald was the first one to formulate that idea and study it systematically. Sequential analysis is no longer considered a distinct branch of statistics. For example, we do not have any course called sequential analysis any more. Its importance at the time was that it led to a re-examination of many things. If concepts for fixed sample-size analysis turn out to be less appropriate for sequential analysis, then that sort of suggests that maybe they weren't appropriate for fixed sample-size analysis either. I'm sure that it's Wald's work in sequential analysis that led to his work in general decision theory and that was very important in the development of statistics.

"I'm Sort of a Dilettante."

MP: *Of the areas you worked in, which do you think are the most significant?*

Blackwell: I've worked in so many areas—I'm sort of a dilettante. Basically, I'm not interested in doing research and I never have been.

MP: *What are you interested in, then?*

Blackwell: I'm interested in *understanding*, which is quite a different thing. And often to understand something you have to work it out yourself because no one else has done it. For example, I have gotten interested in Shannon's information theory. There are many questions that he left unanswered that were just crying out to be answered. The theory was incomplete so I worked on it with a couple of my colleagues because we wanted to know what happens in this case or that case. The drive was not to find something new. It would have been nicer if it had all been done. But since it hasn't been done, you just want to fill out the theory and make it complete. That's what I mean by being a dilettante. When I feel that my understanding of something has been rounded out pretty well, then I'm ready to move on to something else.

MP: *Then maybe that accounts for this long list where you have made contributions: Bayesian statistics, probability theory, game theory, set theory, dynamic programming, information theory.*

Blackwell: But just about everything that I've worked on involves either probability theory or set theory. And of course since the measure theory model for probability involves set theory, I haven't really gone very far away from where I started. I have just looked out in many different directions from it.

Duels

MP: *You are cited as one of the pioneers in the theory of duels. How did you get interested in duels?*

Blackwell: I recall it very well. That happened at the Rand Corporation when I was a consultant for them. One day some of us were talking and this question arose: If two people were advancing on each other and each one has a gun with one bullet, when should you shoot? If you miss, you're required to continue advancing. That's what gives it dramatic interest. If you fire too early your accuracy is less and there's a greater chance of missing. It took us about a day to develop the theory of that duel. I did it and Abe Girshick did it and John Williams did it. Then I got the idea of making each gun silent. With the guns silent, if you fire, the other fellow doesn't know, unless he's been hit. He doesn't know whether you fired and missed or whether you still have the bullet. That turned out to be a very interesting problem mathematically.

MP: *Did you ever go beyond two-person duels?*

Blackwell: I've never gone beyond two-person, zero-sum games at all. They're the only ones I understand. It's regrettable that those are the games for which the theory is clear and beautiful because those are the least important games. One person wins; the other loses. But they're just not the kind of games that are played in the world. For example, the game being played between the United States and the Soviet Union is a much more important game and it is not a zero-sum game. Both sides can win or both sides can lose. But I've never understood those other games. Only the zero-sum games have a clear theory.

MP: *Have you tried to understand those other games?*

Blackwell: I did try for a long time to understand non zero-sum games but I did not succeed and it became clear to me that I was not going to succeed. I was very impressed and I still am impressed by the so-called "sure thing" principle. It was formulated by Jimmie Savage. The "sure thing" principle says this: "If you have to choose between two acts, A and B, and how much you're going to make depends on some unknown situation—it might be S_0, or it might be S_1. Suppose that if you knew it was S_0, then you would choose A over B, and that if you knew it was S_1, you would choose A over B. The "sure thing" principle says that even if you don't know, you should choose A over B. That seems like such a plausible principle, but let me show you what it leads to. The "sure thing" principle leads to the prisoner's dilemma. You and I are playing a game and I can either cooperate with you or double-cross you. And you can either cooperate with me or double-cross me.

		You: C	You: D
Me	C	(2, 2)	(0, 5)
	D	(5, 0)	(1, 1)

The first coordinate in each box shows how much I get, and the second coordinate in each box shows how much you get. So I'm wondering, should I cooperate with you or double-cross you? Maybe you're going to cooperate. If I cooperate I get two; if double-cross I get five. So if I knew that what you're going to do is cooperate, then I would double-cross. But maybe you're going to double-cross, then if I cooperate I get zero and if I double-cross I get one. So again it's better for me to double-cross. So I'm going to double-cross. It's actually symmetric. The five is bigger than the two and the one is bigger than the zero so you should double-cross. So we both believe in the "sure thing" principle and we both double-cross. So we each get a dollar, whereas, if we had cooperated, we would each get two dollars. In fact, the situation with the Soviet Union has elements like this in it. To cooperate is to disarm and to double-cross is to re-arm with bigger and bigger weapons. That takes a lot of resources and we would both be better off disarming. But each is afraid that if he throws away his weapons, the other one will not and he will be at a great disadvantage. So when I saw that this "sure thing" principle led to an armaments race, so to speak, I realized I was not the one to come up with a satisfactory theory for non zero-sum games. I keep on encouraging other people to work on it, though.

MP: *Are there parts of your work that have given you particular pleasure? You have a couple of theorems named after you—there must be a certain amount of pleasure there.*

Blackwell: One thing that gave me a good deal of pleasure was finding a game theory proof for a theorem in topology: the Kuratowski Reduction Theorem. I was studying the proof and trying to understand it, when all of a sudden, I recognized the kind of thinking I was doing, exactly the kind of thinking I was doing some years before when I was thinking about games, infinite games. In about three minutes I realized that you could prove this theorem by constructing a certain game. That gave me real joy, connecting these two fields that had not been previously connected.

MP: *That probably surprised a few topologists.*

Blackwell: Actually, some logicians got interested in it. I may have been one of the first to show how infinite games related to set theory. Actually, I was not the first because Banach and Mazur, back in Poland, related infinite games to set theory. *(Blackwell then went to the board and outlined the proof.)*

Blackwell on Teaching

MP: *You're the first person I've interviewed who can't restrain himself, who can't keep from getting up to the board to explain something. You must like to teach. What is it that makes teaching fun for you?*

Blackwell: Why do you want to share something beautiful with somebody else? It's because of the pleasure he will get, and in transmitting it you appreciate its beauty all over again.

MP: *Are you teaching now?*

Blackwell: Yes, I teach at all levels. This quarter I have only a graduate seminar but last quarter I had a senior-level course and a very elementary course. In the fall quarter I had a course for sophomore-level engineers. There is beauty in mathematics at all levels, all levels of sophistication and all levels of abstraction.

MP: *If you were to write down a short list of desirable characteristics in a mathematics teacher, what would be on the list?*

Blackwell: I don't think that one person is a good teacher for all students. There are all kinds of styles of learning and it takes a good teacher to teach in a style that is not the style in which he learns. For example, I love pictures. The first time I had a blind student in my class, though, I realized how inexplicit my teaching is. For example, I put 5 points on the board and say, "Let's try to fit a line through them." The blind student is completely lost because I don't give to the points any coordinates. In another case, I didn't write anything except two letters, A and B. Some people prefer a somewhat more formal style than that. I think that I'm a good teacher for certain kinds of students, but not necessarily all.

"Formulas and Symbols—I Don't Especially Like Them."

MP: *What I've seen so far reminds me somewhat of Paul Halmos' style. Maybe what we see here is the common influence, Doob.*

Blackwell: My students sometimes complain because I use more than one symbol for the same thing. I forget which symbol I use because the symbols are not very important to me. It's strange to have a mathematician who doesn't especially like formulas and symbols. I remember when von

Neumann and Morgenstern's book on game theory came out. It was a very significant book and it's a big book, because they wrote it twice, once in symbols for mathematicians and once in prose for economists. I read the prose. I found it much easier to read than the symbols. If you are a mathematician it's easy to translate the ideas in prose into symbols if you want to.

MP: *What do you as a mathematician do on a day-to-day basis?*

Blackwell: Well, what I did today was to try to understand two forms of the category 0-1 law, to see if one of them is stronger than the other and to see what each one implies about the existence of what Harvey Friedman calls diagonalizations. It's one of the things I was doing today. That's at a rather high abstract level.

Another thing I was doing today was just playing around with a computer, trying out programs for minimizing a function of five variables, looking at curves and trying various techniques to see which ones work and which ones don't. I would say that the first thing I told you about is a somewhat more serious activity because if I find out the relationship between those two forms, the 0-1 law, I'll probably tell my students about it in a seminar and I may even pursue it further. The other is unlikely to result in anything more than my better understanding the problem of minimizing functions. I play quite a bit.

MP: *What do you mean by play?*

Blackwell: You know the algorithm for calculating the square root. If you want the square root of s and you start out with x, you divide s by x and take the average and that's the new x. Every positive definite matrix has a positive definite square root. It occurred to me that maybe this algorithm would work for positive definite matrices. You take some positive definite X, add to it SX^{-1} and divide by two. The question is: Does this converge to the square root of X? I decided that instead of trying to prove it I would just try it out. If you have an X you can square it and compare it to S and calculate the distance between them. I started out with the identity as my first approximation. In a particular example, the error at first was tremendous. then dropped down to about .003. Then it jumped up a bit to .02, then jumped up quite a bit to .9, and then it exploded. Very unexpected. It is not unusual to have it diverge if you start out far away from the solution, but when you start out close to a solution you expect it to converge to the solution. That's characteristic of Newton's method and this is kind of a Newton-like method. Then I started looking at the theory and it turns out that the algorithm works provided that the matrix you start with commutes with the matrix whose square root you want. You see, it's sort of natural because you have to make a choice between SX^{-1} and $X^{-1}S$, but of course if they commute it doesn't make any difference. Of course I started out with the identity matrix and it should commute with anything. So what happened?

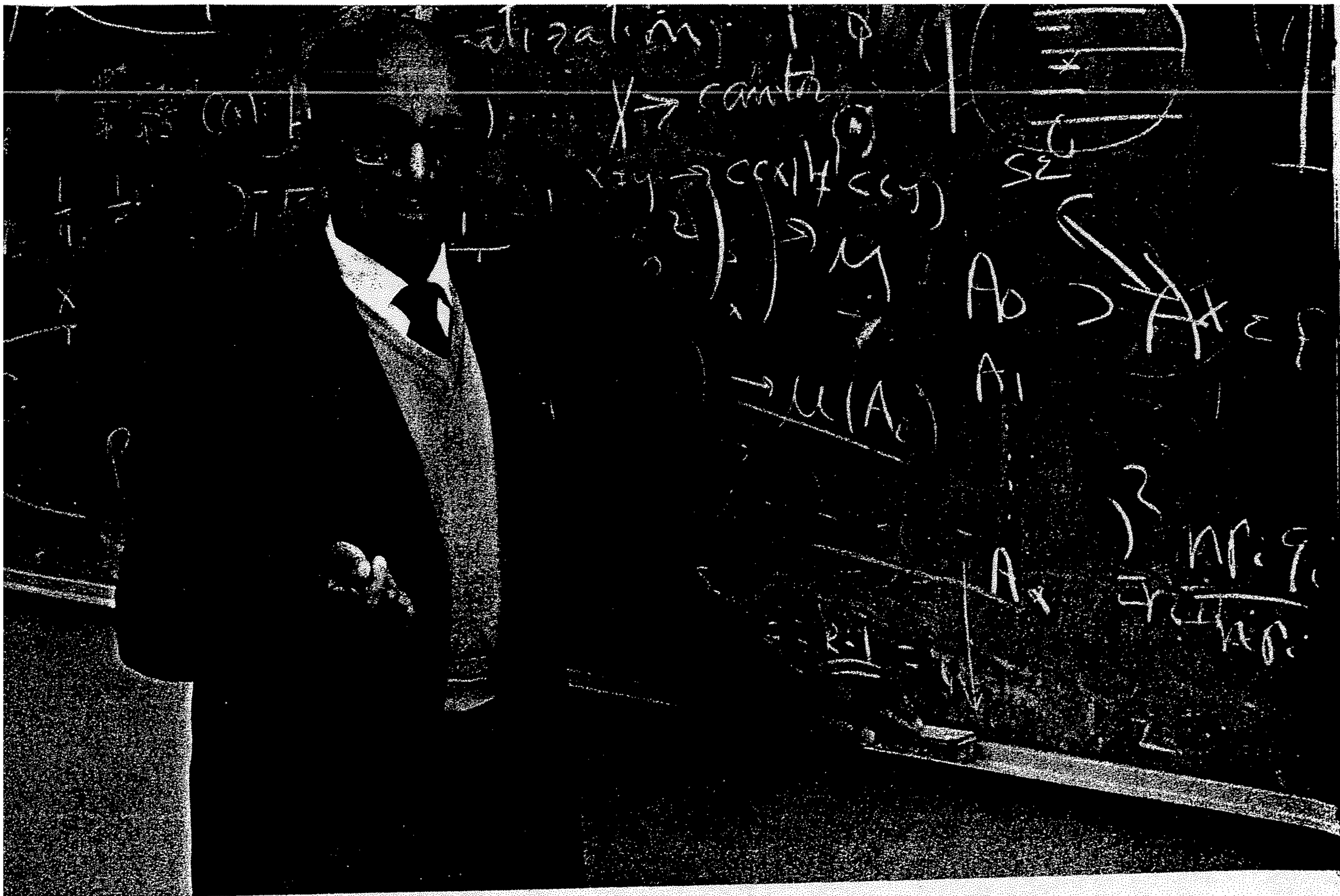
MP: *You must have been having some kind of round-off error.*

Blackwell: Exactly! If the computer had calculated exactly it would have converged. The problem is that the matrix the computer used didn't quite commute.

"I Think Proofs by Contradiction Are a Mistake."

MP: *I have been told that you have a very interesting way of proving a thoerem. How do you, in fact, prove a theorem?*

Blackwell: I don't really know. I think proofs by contradiction are a mistake. I've always found that if you start with mutually contradictory hypotheses, you're always working in never-never land. You're saying that in this land $0 = 1$ and you're just trying to show that that's where you're working. Nothing you say is true, and in a way you're not learning anything because everything you say is false. In all the cases I have looked at, you can transform a proof by contradiction into a proof in which everything you're saying is true and you learn more that way. It's not a question of making a big change. Let me give you an example. Take the proof that the set of real numbers is uncountable. The usual proof is this: Suppose you have a list of all the real numbers, the first one, the second one, the third one, and so on. Write out their decimal expansions, then you can write down one where it differs from the first one in the first place, from the second one in the second place, and so on. So you've reached a contradiction. You have claimed to have them all but you've shown one that is not in the list. It's a beautiful proof but I would formulate the theorem this way. Show me any sequence of



Blackwell, the theoretician, says, "I've never been especially interested in research—I'm sort of a dilettante."

numbers and I'll show you one that is not in the list. It's a small change but it's a positive fact. Every proof by contradiction that I have seen or studied can be recast so there's no contradiction at all. You learn something new.

MP: *The first one that we typically see as students is the irrationality of the square root of 2.*

Blackwell: That is more difficult—I would have to think about that.* But I am convinced that imbedded in that somewhere is a positive approach to the proof. In the proof that the number of primes is infinite there is, of course, a construction of a prime that is larger than any of the primes in the list. So that one is clear.

MP: *Thus we should make proofs positive and avoid proofs by contradiction.*

Blackwell: Yes, I learn things that way. It is well known that from contradictory premises you can deduce anything. Well, I'm just not interested in deducing things from contradictory premises.

"I Applied Only to Black Institutions."

MP: *Let's jump back to Howard University for a few minutes. You were there for ten years. In the Neyman biography, Constance Reid mentions that Jerzy Neyman of the University of California first saw you at the University of Illinois where you were president of the Mathematics Club. In 1942 he contacted Doob, hoping to get Doob to join the statistics department, but Doob said, "No, I cannot come but I have*

*Later, Blackwell did give a positive approach to the proof.

some good students and Blackwell is the best. But of course he's black and in spite of the fact that we are engaged in a war that's advancing the cause of democracy, it may not have spread throughout our own land." How did that incident affect you?

Blackwell: Let me tell you what happened. This is another one of those cases where I didn't know much about it until much later. Neyman wrote to me and said he wanted to interview me for a possible job in Berkeley. We met in New York and he interviewed me. He said he would let me know. He went back to California and I didn't really expect anything to happen. I had already written 104 letters of application to black colleges.

MP: *You hadn't thought of applying to other institutions?*

Blackwell: Oh no, I applied only to black institutions.

MP: *You mean the door was closed?*

Blackwell: I just assumed that it was, but then Neyman wanted to interview me and I was glad to be interviewed. I would have welcomed the job. I didn't expect anything to happen and eventually I got a letter from him saying something like this: "In view of the war situation and the draft possibilities, they have decided to appoint a woman to this position." It sounded plausible to me and I wasn't expecting anything anyway. It wasn't until I came to Berkeley that I learned that there was more to the story than that. My blackness was a plus for Neyman. He had a tremendous amount of sympathy for anyone who had been oppressed or mistreated in any way. He always favored the underdog. It would have given him a special pleasure to appoint me just because I was black.

MP: *Neyman eventually had that pleasure, although some time elapsed between that first interview and your eventual appointment here.*

Blackwell: Yes, twelve years.

MP: *While you were at Howard you kept going. You did a lot of research.*

Blackwell: I was quite free at Howard. They understood the importance of attending professional meetings, for example. They were quite generous in paying expenses for attending professional meetings. I think everyone had the right to attend one professional meeting per year, more than that if you could show need for it.

MP: *That's better than a lot of colleges today.*

Blackwell: Better than Cal. Now if you are presenting a paper, you can get your expenses paid, but not if you just want to go there and learn something. It causes a lot of incomplete papers to be presented because that's how people get their expenses paid. I was able to maintain mathematical contacts with statisticians in Washington and I went to meetings all over the country.

MP: *Did the employment situation change much for blacks during your twelve years at Howard University?*

Blackwell: Oh yes. The first year I was at Howard one of my colleagues in the economics department got an appointment at the University of Chicago. Looking back on it, I feel that there must have been a big change just resulting from World War II. I think there was a big change between the 1941 attitude and the 1945 attitude.

Berkeley

MP: *You arrived in Berkeley in 1954 and shortly thereafter the Department of Statistics was formed as a separate entity from the Department of Mathematics. Would you have been happier had the division not occurred?*

Blackwell: No, in fact, Neyman had had a separate operation for some years. It was just formalizing something that was in fact already the case. Neyman had had his statistical laboratory and when he wanted appointments in what was thought of as the statistical laboratory, the mathematicians pretty much went along with it. I rather liked being in a smaller group. I think a group loses something when it grows beyond a certain size. I think one would miss that close personal relationship where everybody talks to everybody else. Now our department is a little bit too big.

MP: *You succeeded Neyman as chairman of the department. At the time, you were still a junior member of the department, at least in years of service.*

Blackwell: I had known all the people of the department before I came here. Mathematical statistics is still not a very large subject and it was considerably smaller at that time. Mathematical statisticians all knew each other and would see each other at meetings of the Institute of Mathematical Statistics.

MP: *You mentioned earlier some people who have influenced you: your geometry teacher and Doob. Was Neyman also an influence on you?*

Blackwell: He was a very good friend. He was not so much a professional influence on me but rather he had a personal influence. His statistical and mathematical ideas did not influence me very much, at least not directly. It was his character as a man—he was a warm, generous, principled man. He regarded himself as a conservative and in some ways he was a conservative person. For example, in dress, he was extremely conservative. He had rather rigid standards about proper behavior.

MP: *Was anyone else a strong influence on you?*

Blackwell: Well, Girshick influenced me. We worked together over many years. He had more good statistical ideas than I had, though I was better trained technically than he was. He would often announce some mathematical idea of his and it would turn out that it was not quite right, but almost right, and what was right was interesting. He was full of ideas and anxious to get other people to work on them.

Blackwell on Leadership

MP: *It seems that since your student days you have been a leader. At the University of Illinois, you were president of the local mathematics club. You have been president of the Institute of Mathematical Statistics and an officer of several other organizations. Do you have a flair for leadership?*

Blackwell: No.

MP: *Do you lead grudgingly?*

Blackwell: No, I don't mind doing it. I have a tendency to figure out what people want done rather than be a leader. When I was department chairman, I soon discovered that my job was not to do what was right but to make people happy. When you set about making up a teaching schedule, you know that A can teach it but he won't do a very good job, and B will do a better job, but B taught it last year so you give it to A.

MP: *So you don't miss administrative work.*

Blackwell: Not a bit! In fact, when I gave up being chairman, for about a year my first waking thought in the morning was, "I'm no longer chairman," and it made my day.

MP: *It wasn't too many years after you arrived here in Berkeley that the whole social fabric of the University was torn apart by the free speech movement. You weren't exactly a bystander.*

Blackwell: I was completely sympathetic to the students. I did not like the way they expressed their grievances, but they certainly had grievances. The students had changed but the administration had not recognized the change. You know when Adlai Stevenson was running for President—I believe that was in 1956—he was not allowed to speak on the Berkeley campus. The administration took this line: The University must not get involved in political matters. No candidates were permitted to speak. When I was going to school that would not have bothered me at all. And it wouldn't have bothered most of the other students when I was going to school. But the students in the 1960's were a different breed. A lot of them were very much interested in what was going on and these rules that may have been appropriate 40 years ago were simply completely outmoded. But the administration simply wouldn't move an inch. That's what the students were protesting. They wanted to hear all kinds of ideas discussed on the campus. Looking at it now, it's hard to believe that's the way it was, but it really was that way. I don't like loud noises. There was a lot of violence and destruction in those days, but the students really had something to protest.

Blacks in Mathematics

MP: *At mathematical meetings I still see very few black faces. The list of black mathematicians is short and it does not seem to be growing very rapidly. Do you have any explanation?*

Blackwell: Yes. Black people go in other directions. Black people are going into the professions: law, medicine, and business. I sort of understand that: there's more security. There's more certainty of having a fair income in those areas than there is in mathematics. I don't know if you know J. Ernest Wilkins. He's a black mathematician just a few years younger than I am. He's good. In fact, he's also an engineer and is a member of the National Academy of Engineering. His father was quite a good mathematics student and got his bachelor's degree at the University of Illinois. Then he went into law and became a very distinguished lawyer. He was on some President's cabinet. There are other black people who have had considerable mathematical talent but went into law.

Families and Telephones

MP: *You've been at Berkeley a long time now and you've had a very creative career here. Have you ever thought of going anywhere else?*

Blackwell: Oh no, I've been pretty happy here and a number of our children are living here now, and those who are not plan to come back here.

MP: *You have eight children?*

Blackwell: Yes.

MP: *That's a big family even by the standards of the 'forties and 'fifties. You must like children.*

Blackwell: Yes. I like grandchildren too.

MP: *Have any of your children pursued careers connected with mathematics?*

Blackwell: No, they have no particular mathematical interests at all. And I'm rather glad of that. This may sound immodest, but they probably wouldn't be as good at it as I am. People would inevitably make comparisons. My brother went to the University of Illinois and he was a freshman there about ten years after I was. He joined the same fraternity that I had belonged to. They asked him, when they found he was from Centralia, whether he was related to me. He said: "I think I've heard of him, but there is no connection." Again, he didn't want to be compared to me. He wanted to make it on his own. My name was on some sort of a plaque there. It's hard on the younger one, whether he made a very good record or whether he made a very bad record, for he gets blamed for what his older brother did.

MP: *Some years ago Life Magazine put out a book on mathematics that had your picture in it. You were shown teaching, which I now see was very appropriate. It also mentioned that you did not have a phone in your home. Do you still have no telephone in your home?*

Blackwell: Oh, no, our youngest daughter won that battle years ago. She insisted we have a telephone. But for a long time we did not have a telephone. It wasn't based on any principle at all, but one of our kids ran up an excessive long-distance bill so we decided to have the telephone discontinued for a month. One month went into two and two months when into three and we decided that there were advantages as well as disadvantages to not having a telephone.

I do not have a positive attitude toward telephones, though. During World War II a friend of mine and I were in Washington trying to get a train to New York. There were long lines and trains did not run very frequently. Furthermore, soldiers had priority. We were standing in the ticket line, just waiting to get some information, and my friend said, "Just a minute." He left the line and then I heard the telephone ring—the ticket agent stopped waiting on customers and went over, answered the telephone, and gave my friend the information he wanted to know. That's when my attitude toward the telephone changed. What a rude, impolite instrument that is. It can break in and take priority over all the people who have made the effort of coming in and standing in line.

MP: *Do you have any hobbies?*

Blackwell: No. When I have spare time I listen to music or I go into the country and work. We have some land up in Mendocino County, about 40 acres. It's beautiful—it has a creek and big redwood trees. When we bought it my dream was to go up on weekends, get a martini, sit under a redwood tree, and watch the creek go by. But when I go up there I work from the time I get there until the time I leave, planting trees, repairing fences, cutting weeds, fixing a leak in the barn. So many things go wrong that something always has to be done. When we go up, I'm not the only one who works. My wife also works from the time we get there till we leave for home. It hasn't worked out at all the way we had in mind, but it is a lot of fun.

Spring 1994
Math Times
University of Illinois
at Urbana-Champaign

Blackwell to be Honored

Dr. **David Blackwell,** emeritus professor of mathematics and statistics at the University of California at Berkeley, will be presented with an Alumni Achievement Award at the university commencement ceremonies in the Assembly Hall, May 15.

This award is for outstanding achievement and was established in 1956. It is the highest honor the U. of I. Alumni Association can bestow. Portraits of the awardees are permanently displayed in the Illini Union.

Dr. Blackwell, a native of Centralia where he attended public schools, came to the university in 1935. He received three degrees here, his A.B. in 1938, his A.M. in 1939 and his Ph.D. in 1941. He wrote his thesis under Emeritus Professor **J.L. Doob.**

Dr. Blackwell has been a pioneer in statistical decision theory and is an expert on Bayesian statistics, probability and dynamic programming, as well as information theory. He is a member of the National Academy of Sciences and the American Academy of Arts and Sciences and has received twelve honorary doctorates, including one from the University. When he received a doctorate from Harvard the citation said, "His lucid mind has emphasized new ways of describing old concepts."

Among his other awards are the Von Neumann Theory Prize, fellowship in the Institute of Mathematical Statistics and honorary fellowship in the Royal Statistical Society. He is a past president of the International Association for Statistics in the Physical Sciences and of the Bernoulli Society for Mathematical Statistics and Probability. He will be introduced at the Alumni Association's annual awards luncheon on May 14 in the Illini Union and will be a guest at dinner that evening.

David Blackwell

Effervescent Mathematics

In the early 1980s Professor Eberhard Becker of the University of Dortmund in Germany proved that, for every positive integer, the rational function $(1+t^2)/(2+t^2)$ can be written as a sum of 2k-th powers of rational functions. His proof was abstract, and he offered a bottle of champagne to anyone who could construct an explicit set of formulas.

Bruce Reznick presented a 20 minute talk at a special session on Quadratic Forms and Division Algebras at the Joint Meetings in Cincinnati last January, and for this was awarded the bottle of champagne. Professor Victoria Powers of Emory University, a collaborator of Professor Becker, made the presentation. The explicit formulas, which fit on a single sheet of paper, are available from Reznick, but the champagne is all gone.

BIBLIOGRAPHY

DAVID BLACKWELL

1. "Idempotent Markoff chains," Ann. of Math., Vol. 43, No. 3 (1942), pp. 560-567.

2. "The existence of anormal chains," Bull. Amer. Math. Soc., Vol. 51 (1945) pp. 465-468.

3. "Finite non-homogeneous chains," Ann. of Math., Vol. 46, No. 4 (1945), pp. 594-599.

4. "On an equation of Wald," Ann. Math. Stat., Vol. 17, No. 1 (1946), pp. 84-87.

5. "On functions of sequences of independent chance vectors with applications to the problem of the 'random walk' in k dimensions," Ann. Math. Stat., Vol. 17, No. 3 (1946), pp. 310-317 (with M.A. Girshick).

6. "Conditional expectation and unbiased sequential estimation," Ann. Math. Stat., Vol. 18, No. 1 (1947), pp. 105-110.

7. A lower bound for the variance of some unbiased sequential estimates," Ann. Math. Stat., Vol. 18, No. 2 (1947), pp. 277-280. (with M.A. Girshick).

8. "A renewal theorem," Duke Math. Jour., Vol. 15, No. 1 (1948), pp. 145-150.

9. "Bayes and minimax solutions of sequential decision problems," Econometrica, Vol. 17, Nos. 3 and 4 (1949), pp. 213-244 (with K.J. Arrow and M.A. Girshick).

10. "Some two-person games involving bluffing," Proc. Nat. Acad.Sci., Vol. 35, No. 10 (1949), pp. 600-605, (with R. Bellman).

11. "Comparison of experiments," Proc. Second Berkeley Symposium on Math. Stat. and Prob., University of California Press, 1951.

12. "The range of certain vector integrals," Proc. Am. Math. Soc., Vol. 2, No. 3 (1951), pp. 390-395.

13. "On a theorem of Lyapunov," Ann. Math. Stat., Vol. 22, No. 1, (1951), pp. 112-114.

14. "On moment spaces," Ann. of Math., Vol. 54, No. 2 (1951), pp. 272-274. (with R. Bellman).

15. "On the translation parameter problem for discrete variables," Ann. Math. Stat., Vol. 22, No. 3 (1951), pp. 393-399.

16. "Admissible points of convex sets," Contributions to the Theory of Games, Vol. 2, Princeton University Press, 1953, pp. 87-91 (with K.J. Arrow and E.W. Barankin).

17. "On randomization in statistical games with k terminal action," Contributions to the Theory of Games, Vol. 2, Princeton University Press, 1953, pp. 183-187.

18. "Extension of a renewal theorem," Pacific Jour. of Math., Vol. 3, No. 2 (1953), pp. 315-320.

19. "Equivalent comparisons of experiments," Ann. Math. Stat., Vol. 24, No. 2 (1953), pp. 265-272.

20. "On optimal systems," Ann. Math. Stat., Vol. 25, No. 2 (1954), pp. 394-397.

21. "A representation problem," Proc. Amer. Math. Soc., Vol. 5, No. 2 (1954), pp. 283-287.

22. Theory of Games and Statistical Decisions, Wiley and Sons, 1954, xi+355 pp. (with M.A. Girshick).

23. "On multi-component attrition games," Naval Research Logistics Quarterly, Vol. 1 (1954), pp. 210-216.

24. "On transient Markov processes with a countable number of states and stationary transition probabilities," Ann. Math. Stat., Vol. 26, No. 4 (1955), pp. 654-658.

25. "An analog of the minimax theorem for vector payoffs," Pacific Jour. of Math., Vol. 6, No. 1 (1956), pp. 1-8.

26. "On a class of probablity spaces," Proc. Third Berkeley Symposium on Math. Stat. and Prob., University of California Press, Vol. 2 (1956), pp. 1-6.

27. "Controlled random walks," Proc. of the Internat. Congress of Mathematicians, Amsterdam 1954; Vol. 3 (1956), pp. 336-338.

28. "On discrete variables whose sum is absolutely continuous," Ann. Math. Stat., Vol. 28, No. 2 (1957), pp. 520-521.

29. "Design for the control of selection bias," Ann. Math. Stat., Vol. 28, No. 2 (1957), pp. 449-460 (with J.L. Hodges, Jr.).

30. "On the identifiability problem for function of finite Markov chains," Ann. Math. Stat., Vol. 28, No. 4 (1957), pp. 1011-1015 (with L. Koopmans).

31. "The entropy of functions of finite-state Markov chains," Trans. First Prague Conference on Information Theory, Decision Functions, and Random Processes, Prague, 1957, pp. 13-20.

32. "Another countable Markov Process with only instantaneous states," Ann. Math. Stat., Vol. 29, No. 1 (1958), pp. 313-316.

33. "Proof of Shannon's transmission theorem for finite-state indecomposable channels," Ann. Math. Stat., Vol. 29, No. 4 (1958), pp. 1209-1220 (with L. Breiman and A.J. Thomasian).

34. "The probability in the extreme tail of a convolution," Ann. Math. Stat., Vol. 30, No. 4 (1959), pp. 1113-1120 (with J.L. Hodges, Jr.).

35. "The capacity of a class of channels," Ann. Math. Stat., Vol. 30, No. 4, (1959), pp. 1229-1241 (with L. Breiman and A.J. Thomasian).

36. "Infinite codes for memoryless channels," Ann. Math. Stat., Vol. 30, No. 4, (1959), pp. 1242-1244.

37. "The capacities of certain channel classes under random coding," Ann. Math. Stat., Vol. 31, No. 3, (1960), pp. 558-567 (with L. Breiman and A.J. Thomasian).

38. "On the completeness of order statistics," Ann. Math. Stat., Vol. 31, No. 3, (1960), pp. 794-797 (with C.B. Bell and L. Breiman).

39. "On the functional equation of dynamic programming," Jour. Math. Analysis and Applications, Vol. 2, No. 2 (1961), pp. 273-276.

40. "Minimax and irreducible matrices," Jour. Math. Analysis and Applications, Vol. 3, No. 1 (1961), pp. 37-39.

41. "Exponential error bounds for finite-state channels," Proc. Fourth Berkeley Symposium on Math. Stat. and Prob., University of California Press, Vol. 1, (1961), pp. 57-63.

42. "Discrete dynamic programming," Ann. Math. Stat., Vol. 33, No. 2 (1962), pp. 719-726.

43. "Merging of opinions with increasing information," Ann. Math. Stat., Vol. 33, No. 3 (1962), pp. 882-886 (with L.E. Dubins).

44. "A converse to the dominated-convergence theorem," Ill. Jour. of Math., Vol. 7, No. 3 (1963), pp. 508-514 (with L.E. Dubins).

45. "Non-existence of everywhere proper conditional distributions," Ann. Math. Stat., Vol. 34, No. 1 (1963), pp. 223-225 (with C. Ryll-Nardzewski).

46. "Sharp bounds on the distribution of the Hardy-Littlewood maximal function," Proc. Amer. Math. Soc., Vol. 14, No. 3 (1963) pp. 450-453 (with L.E. Dubins).

47. "Memoryless strategies in finite-state dynamic programming," Ann. Math. Stat., Vol. 35, No. 2 (1964), pp. 863-865.

48. "The tail σ-field of a Markov chain and a theorem of Orey," Ann. Math. Stat., Vol. 35, No. 3 (1964), p. 1344 (with D. Freedman).

49. "The last return to equilibrium in a coin tossing game," Ann. Math. Stat., Vol. 35, No. 3 (1964), p. 1344 (with P. Deuel and D. Freedman).

50. "A remark on the coin tossing game," Ann. Math. Stat., Vol. 35, No. 3 (1964), pp. 1345-1347 (with D. Freedman).

51. "Probability bounds via dynamic programming, Stochastic processes in Mathematical Physics and Engineering," Proc. of Symposia in Applied Math., Vol. 16, (1964), pp. 277-280.

52. "The Martin boundary for Polya's urn scheme," Jour. of Applied Prob., Vol. 1, Nos. 1 and 2 (1964), pp. 284-296 (with D.G. Kendall).

53. "Discounted dynamic programming, "Ann. Math. Stat., Vol. 36, No. 1 (1965), pp. 226-235.

54. "Positive dynamic programming," Proc. of the Fifth Berkeley Symposium on Math. Stat. and Prob., Vol. 1, Los Angeles and Berkeley, University of California Press, 1966, pp. 415-418.

55. "A note on Bayes estimates," Ann. Math. Stat., Vol. 38 (1967), pp. 1907-1912 (with P. Bickel).

56. "Infinite games and analytic sets," Proc. Nat. Acad. Sci., Vol. 58, No. 5 (1967), pp. 1836-1837.

57. "Elementary path counts," Amer. Math. Monthly, Vol. 74, No. 7 (1967), pp. 801-804 (with J.L. Hodges, Jr.).

58. "An elementary proof of an identity of Gould's," Reimpreso de Boletin de la Sociedad Matematica Mexicana, Vol. 11, No. 2 (1968), pp. 108-110 (with L. Dubins).

59. "The big match," Ann. Math. Stat., Vol. 39, No. 1 (1968), pp. 159-163 (with T.S. Ferguson).

60. "A Borel set not containing a graph," Ann. Math. Stat., Vol. 39, No. 4 (1968), pp. 1345-1347.

61. "On the local behavior of Markov transition probabilities," Ann. Math. Stat., Vol. 39, No. 6 (1968), pp. 2123-2127, (with D. Freedman).

62. "Infinite G games with imperfect information," Zastosowania Matematyki Aplicationes Matematicae, Vol. 10, Hugo Steinhaus Jubilee Volume (1969), pp. 99-101.

63. "On stationary policies," J. Royal Stat. Soc., Vol. 133, Part 1 (1970), pp. 33-37.

64. Basic Statistics, McGraw-Hill Publishers, New York, 1970, 143pp. (Book).

65. "Ferguson distributions via Polya urn schemes," Ann. Statist., Vol. 1 (1973), pp. 353-355 (with J.B. MacQueen).

66. "Discreteness of Ferguson selections," Ann. Statist., Vol. 1 (1973), pp. 356-358.

67. "On the amount of variance needed to escape from a strip," Ann. Prob., Vol. 1 (1973), pp. 772-787 (with D. Freedman).

68. "The optimal reward operator in dynamic programming," Ann. Prob., Vol. 2 (1974), pp. 926-941 (with D. Freedman and M. Orkin).

69. "On existence and non-existence of proper, regular conditional distributions," Ann. Prob., Vol. 3 (1975), pp. 741-752 (with L. Dubins).

70. "The stochastic processes of Borel gambling and dynamic programming," Ann. Statist., Vol. 4 (1976), pp. 370-374.

71. "Borel-programmable functions," Ann. Prob., Vol. 6 (1978), pp. 321-324.

72. There are no Borel SPLIFs. Ann. Prob., Vol. 8 (1980), pp. 1189-1190.

73. Borel sets via games. Ann. Prob., Vol. 9 (1981), pp. 321-322.

74. A Bayes but not classically sufficient statistics. Ann. Statist., Vol. 10 (1982), pp. 1025-1026 (with R.V. Ramamoorthi).

75. A hypothesis-testing game without a value. Festschrift for Erich L. Lehmann, Wadsworth, 1982.

76. An extension of Skorohod's almost sure representation theorem. Proc. Amer. Math. Soc., Vol. 89 (1983), pp. 691-692, (with Lester Dubins).

77. Factorization of probability measures and absolutely measureable sets. Proc. Amer. Math. Soc., Vol. 92, (1984), pp. 251-254 (with Ashok Maitra).

78. Ulam's redistribution of energy problem: collision transformations. Letters in Mathematical Physics, Vol. 10 (1985), pp. 149-153, (with R. Daniel Mauldin).

79. Stationary plans need not be uniformly adequate for leavable, Borel gambling problems. Proc. Amer. Math. Soc., Vol. 102, (1988), pp. 1024-1027 (with S. Ramakrishnan).

80. Operator solution of infinite G_δ games of imperfect information. Probability, Statistics and Mathematics. Papers in Honor of Samuel Karlin, pp. 83-87. Academic Press.

8/90