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A Conversation with Persi Diaconis

Morris H. DeGroot

Persi Diaconis was born in New York on January 31, 1945. He received a B.S. in mathematics from the College of the City of New York in 1971, and an M.S. and a Ph.D. in mathematical statistics from Harvard University in 1972 and 1974, respectively. He was appointed an Assistant Professor in the Department of Statistics at Stanford University in 1974 and has been a member of the faculty at Stanford since then. His title is presently Professor of Statistics.

The following conversation took place in his home in San Francisco one morning in October 1984.

"I RAN AWAY FROM HOME AT 14 TO GO ON THE ROAD DOING MAGIC"

DeGroot: Tell me how you got interested in statistics and probability.

Diaconis: It's always hard to say. One way was through my interaction with Martin Gardner. When I was a kid I used to invent card tricks, and somehow I met Martin Gardner when I was 13. He was a fascinating character and I started to read stuff he wrote.

DeGroot: He is the fellow who wrote the column in the *Scientific American* on mathematical puzzles and games?

Diaconis: Right.

DeGroot: Was he doing that at the time you met him?

Diaconis: Sure, since well before then. He wrote that column for 25 years, and once a year or twice a year there would be a column on probability and randomness. That helped get me hooked. There were other links through magic. Magicians revere gamblers—especially crooked gamblers—because crooked gamblers have ways of doing sleight of hand that has to be good. Otherwise they get their arms and legs chopped off. And so one is always trying to figure out about gamblers and trying to get to meet them. I got fascinated by gambling. Also, when I was 14, I bought a copy of Feller, Volume 1, because somebody told me that this is the best book there is. I thought I could do anything. By then I had left school.

DeGroot: At the age of 14 you left school?

Diaconis: Right. I ran away from home at 14 to go on the road doing magic, and I never went back. So I bought Feller and I thought, "Well, I'll just read this book." And I couldn't read it. I didn't know calculus, or at least not enough. Still I kept trying to read Feller, and I finally did. So those are sort of early influences that got me interested in probability through gambling and through magic tricks. There are magic tricks that work on chances. Things like the matching problem are the basis of magic tricks. That would make a good article—magic tricks that work by probabilistic methods.

DeGroot: Well, that means they only work some of the time.

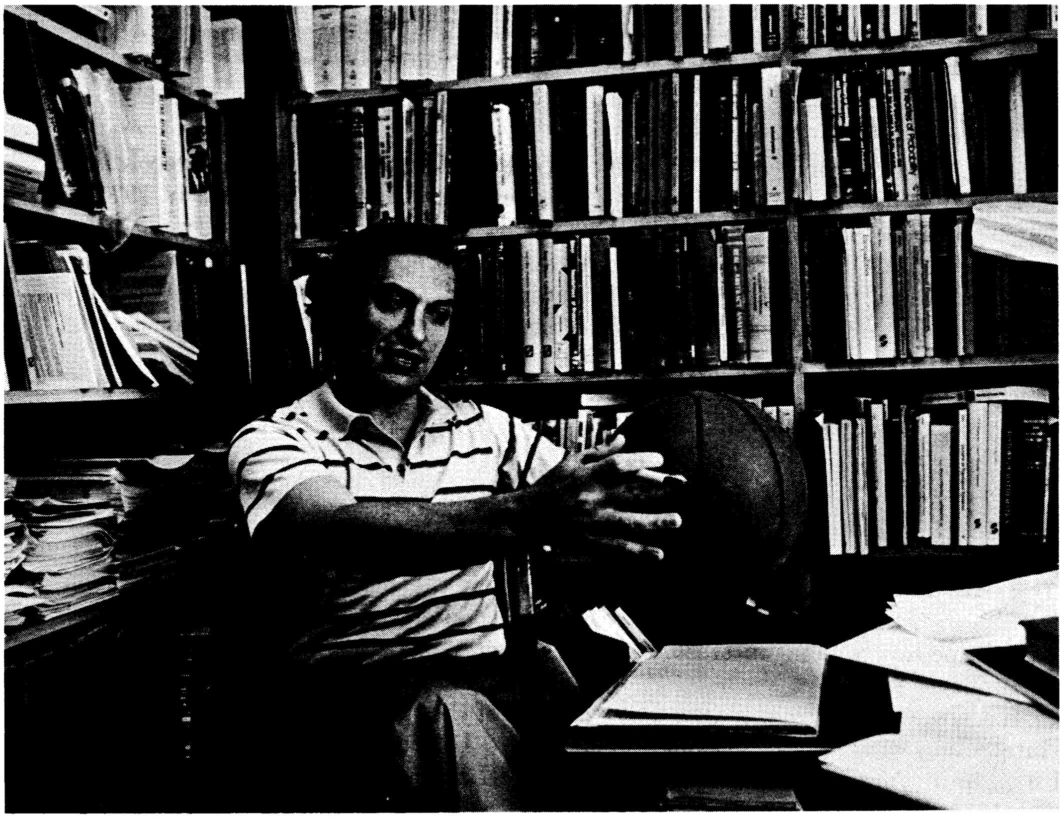
Diaconis: Right. I only claim to be 90% correct. [Laughs] Certain psychic tricks actually look more believable if they only work some of the time. Anyway, when I went back to City College, which was a long time later, there was a guy there named Leonard Cohen who had been a student of Howard Raiffa's at Columbia and taught beautiful courses in probability and statistics. I think he played a big influence in making me think that probability and statistics were interesting subjects.

DeGroot: When did you go back to City College?

Diaconis: When I was 24, I started City College at night. They wouldn't let me in during the day.

DeGroot: You had no high school diploma?

Diaconis: I did get a high school diploma, through some strange quirk of fate which I still don't understand. I left to go on the road, although I was going to graduate anyway at 15. I had been pushed through the New York City school system rapidly and before I left I took lots of scholarship exams and stuff. I came back to New York, and kept getting mail as if I had graduated. Letters from the Army saying "Dear Graduate, perhaps you would be interested . . . " And I had won some scholarships—a Merit Scholarship and some others. And I thought, "Gee, this is funny. I didn't even graduate from high school and there are all of these opportunities that I can't take." But I kept getting these letters, so I then went into school and I said, "Did I somehow graduate?" This is a giant New York City high school, George Washington, and the assistant principal said, "Oh, Diaconis. Yeah, the teachers got together and decided it would not do you any good to cause you trouble, and they just decided to give you grades and graduate you." He said that



Persi Diaconis.

some wouldn't go along with it and others gave me 90's so that my overall average would be high enough. And I had a diploma. I took it and ran. It shows that somehow the system can adjust—there was some human element in it.

But, anyway, I was on the road for a long time, and I decided to go back to school largely because I wanted to learn to read Feller. I decided it was time, and I needed to take this calculus and other stuff. Actually, I was interested mostly in number theory when I was an undergraduate. I graduated six months early, in January, and a lot of places wouldn't let you apply except in September.

DeGroot: What year was this?

Diaconis: 1971. I applied to only one statistics department, Harvard's, because nobody from City College had gotten into Harvard in math. I got into the Harvard statistics department, and I really didn't know if I had any interest at all in statistics. But I thought it would be great to go to Harvard, and so I went to Harvard. I said, "I'll try it for six months."

DeGroot: You had applied mostly to math departments?

Diaconis: Right. And I had gotten into lots of math departments. But I decided to try statistics because I wanted to go to Harvard. And I thought, "Well, I'll transfer to math or something. They'll learn that I'm terrific." And then I got there and I liked it. I mean, after about a year I decided I was interested in statistics. I'm sure you can remember that, too. I remember being in graduate school and thinking, "When am I going to get interested in this stuff?" You know, you go through the courses and churn along. It took about a year or a year and a half, and then I did my first little thing. Some tiny little math problem that I solved on my own and I thought, "Gee, this stuff is pretty good." Anyway, somehow it was a series of accidents that got me into statistics proper, through probability. And now I do a lot of different things, and people say what am I? I am a statistician. That's my training and my interest, and that's the language that I speak and the way I think. And it is because of Harvard and this accident of my getting in.

"I CAME TO STANFORD TO FIGURE OUT IF THERE WAS ANYTHING IN-BETWEEN DATA ANALYSIS AND MATHEMATICS"

DeGroot: In what area did you write your dissertation?

Diaconis: Sort of number theory, in the statistics department with advisors from math—probabilistic number theory, Erdös-style. If you pick an integer at random, how many prime divisors does it have? You know—the number of prime divisors has an approximate Poisson distribution, and stuff like that.

My thesis actually was done because of Fred Mosteller who has always been a closet number-theorist and is fascinated by the integers as data. He does a lot of snooping around. I remember the first day that I came to Harvard to interview. I had been accepted and I thought, "Am I going to go there?" Mosteller looked at my record and said, "I see you're interested in number theory. I'm interested in number theory. Could you help me do this problem?" It was a problem in analytic number theory about getting a correction term to some result about the mean number of prime divisors of a random integer: You pick an integer at random between 1 and x. The number of prime divisors is about log log x, and then there's a correction term. He wanted to know what it was, and I figured it out that summer, a couple of weeks later. My first paper is with Fred Mosteller on that problem. He really computed a lot and formed very interesting conjectures, many of which can't be proved but are undoubtedly true.

DeGroot: What do you mean "Can't be proved?"

Diaconis: *Haven't* been proved. For example, Fred looked at the number of primes in intervals that are pretty long. There should be about a Poisson number of primes in disjoint intervals, but Fred noticed that the variance is too small. Now that's strange because primes are ordinarily hard to do things with, and why should the variance be smaller than Poisson variability. The means are right; they are what they should be. If you assume strong forms of the twin-prime conjecture, you can prove Fred's empirical finding. Charles Stein and I actually did some of that, but that's assuming a very strong unproven number-theoretic hypothesis. There's correlation, because primes do or don't lie in certain arithmetic sequences. If you assume these strong number-theoretic hypotheses, you can prove that this correlation would make the variance smaller. But it's Fred's discovery. He wrote a fascinating article on that in the Snedecor volume. [Mosteller, F., "An empirical study of the distribution of primes and litters of primes," *Statistical Papers in Honor of George W. Snedecor* (T. A. Bancroft, ed.), Iowa State University Press (1972), 245–257.] Just a beautiful piece of data analysis, guessing the form of the correlation. And if you assume all of this stuff that is a thousand years away from being provable, you can actually prove his conjectures. So Fred really does look at the integers as data. And I thought, "Ah ha, I could do number theory *and* statistics." And that's what I wound up doing. People that I worked with were mostly in the math department: Andy Gleason was a reader of my thesis; and Dennis Hejhal, who was a young mathematician in the math department; and Steve Portnoy, who was a Stanford Ph.D. and a very mathematical statistician. Mostly, I wrote a thesis and had to find somebody to read it. I can remember going in to Fred and saying, "Gee, I think I could write a thesis about this sort of number theory." And he said, "Great, go ahead." He just was very encouraging throughout.

DeGroot: Do you still retain this interest in number theory in probability and statistics?

Diaconis: Yes. This year, next term, I'm teaching a course which is about half—well, maybe a third—number theory and probability. But my interest is less than it was, now that I'm at Stanford. Harvard's department is very applied and I think I was hired at Stanford as a Tukey-style data analyst. I was trained in a sort of schizophrenic way. I had this very strong math component and a very strong data-analytic component. I didn't know much mathematical statistics. Almost none. I came to Stanford to figure out if there was anything in-between data analysis and mathematics. [Laughs] I suppose there is. That's one thing I've learned. I decided I had better start to do some mathematical statistics since that was the strength of Stanford's department, and I got away from number theory somewhat. I am influenced by my environment. If I was in an environment where people were doing lots of pure math, I probably would do more of that. But if you're in an environment where people are really looking at data and thinking about inference, well, it's a little funny to be thinking about the zeros of the zeta function.

"I WANTED TO TALK TO SOMEBODY WHO WAS REALLY GOOD IN MATHEMATICAL STATISTICS"

DeGroot: Do you interact with the math department at Stanford?

Diaconis: I do to some extent, and I still follow math a little bit. Charles Stein is also a very active closet number-theorist, and we have written number-theoretic papers and do work on such things. There is some interaction between statistics and this work. For example, the running time of the fast Fourier transform depends on how many prime divisors n has, because you split it up that way. I wrote a paper on the average running time of the fast Fourier transform that really does blend together probability and number-theory. It seems useful and perhaps interesting. Right at the moment I'm working on how to draw pictures on a computer; something that has to do with iterated affine maps. Analysis of those algorithms is really getting me involved in algebraic number theory. Having studied three years of algebraic and analytic number theory certainly doesn't hurt. So I keep up an interest, but mostly I'm doing other things.

DeGroot: From what you were saying, Mosteller was an important influence on your career. Are there other people who have been influential?

Diaconis: The whole Harvard department when I was there. Art Dempster is the guy who got me interested in Bayesian statistics and explained to me that it could make a difference in what you did. And then when Mosteller turned out to be a Bayesian too, well, I thought, "Ah ha, what is this stuff?" And I'm still trying to understand it. Steve Portnoy taught me that mathematics could be useful and interesting in statistics. And Bill Cochran was Bill Cochran, who just said that statistics can be interesting in the world. Having a chance to take courses from him in design and stuff like that gave me a feeling for real statistics. Going through training like that is just different from reading it in books. So certainly the whole Harvard faculty was important. I can remember when it came to be time to take a job. I had a chance to go to Stanford for a year or two with no promise beyond that, not tenure track or anything. Paul Holland said, "You should just be around Brad Efron and Charles Stein." That's why I went to Stanford; I wanted to talk to somebody who was really good in mathematical statistics. And then things worked out well. Certainly Stein has been an enormous influence, not only on me, but on his entire environment. He keeps us all honest and keeps us all from taking ourselves too seriously. Anytime anybody gets a slightly overblown notion of himself, there's Charles working away and you know what depth and quality means.

DeGroot: Do you mean that he keeps you honest more by his example than by any criticism of your work?

Diaconis: Well, Charles is also critical. He has a very broad view of statistics, and he doesn't compare what he or you or anybody else is doing with what was done this week. He compares it with what's been done in the history of statistics, and you know he himself suffers the most from that. We have written papers together which he'll occasionally refer to as "that stupid little paper." He's quite critical. Fortunately, every once in a while he likes something I've done.

"IF THERE WERE SUCH A THING AS SORT OF ACADEMIC MAGIC, I WOULD LIKE TO DO IT"

DeGroot: Let me move backward in time and ask about how you started your career as a magician. You said you ran away from home at the age of 14 and went back to college at the age of 24. There's a ten-year period in there in which you were a magician, I take it.

Diaconis: Right. I started doing magic tricks when I was five, and it fascinated me then and it still does now. If I could have made a living doing magic in a high-class way, I probably would still be doing it. But, alas, to make a living in magic is to be in show business, and it means changing in bathrooms, etc. And doing the same 17-minute act again and again. You try to change a trick and the agent rushes backstage and says, "You can't take out the butterfly trick. That's what I book you on." And you think, "Oh God, am I going to be stuck doing this routine for the rest of my life?" If there were such a thing as sort of academic magic, I would love to do it.

DeGroot: There is no department of magic here at Stanford?

Diaconis: Not a hope. I did magic with all my energy. I never did homework. Somehow I kept getting promoted because I was a bright kid, but without ever doing anything. When I was 14, America's greatest magician was a man named Dai Vernon. We met at a magicians' cafeteria and he invited me to go on the road with him as sort of an assistant, and I jumped at the chance. I just went off. I didn't tell my parents; I just left. And we traveled around, sort of following the wind as I say all the time. He really is a very charismatic, interesting character—a great inventor of magic tricks. Still alive, 90 now. When we were on the road he was 65 or so. We would go to little towns and do shows and lecture for magicians. If we heard an Eskimo had a new way of dealing the second card with snow shoes, we would be off to Alaska. Just going, no itinerary. We did that for two years or so. Then he went off to the west coast to found what's called the Magic Castle, which is a very lovely establishment in Los Angeles—sort of the magic center of the United States now. I went back to New York and worked as a professional magician and just traveled all around doing the best I could. I never was a star in the sense of making a great living, but I lived OK.

DeGroot: Did you do this under your own name?

Diaconis: No. Nobody could pronounce Diaconis and so I worked under my middle name, Persi Warren. I can show you a business card (Figure 1). That's not a great life, but it's OK. Working in the Catskills. What happens is that you get somebody who sees you and likes you and says, "Gee, would you ever think of coming to Boston?" or "Ever think of coming to Chicago to do a birthday party? I'll pay your fare and $200." And you think, "Great," and you go to Boston or Chicago, and you check into a show-business rooming house. You do that date and maybe an agent will get you another job while you're there, and so forth. You stay a couple of months or a couple of weeks, and then move back to New York. I have a lot of friends still in that world and I still do some work with them. It interacts with statistics a little bit through various cute little math questions.

I'm writing a book now with Ron Graham that tries to teach mathematics by magic tricks. That is, I find that simple tricks can lead to hard interesting math projects. I thought that would be an interesting way to explain to the outside world what it is we do, and

FIG. 1. *The business card of Persi Warren.*

how you could get hooked on a math problem through a real world problem. Also, I still invent tricks for people in the business. I love to invent tricks and brainstorm. I'm working with a guy in Los Angeles right now, trying to invent tricks for a show he's doing on Halloween. Tonight, for example, a brilliant young magician, Michael Weber, is coming over. Here's a crazy thing Michael Weber can do. He can take an ordinary rubber band, hold it between his fingers, and go like that, and the thing shoots out 15 feet and then rolls back to him. [Laughs] Well, he's in town and he's doing a show and then he's coming over here. We'll probably stay up until 2:00 in the morning doing card tricks.

DeGroot: It strikes me that magic is a good combination of theory and applications in that you have to have some actual physical dexterity as well as have the underlying theory.

Diaconis: Absolutely.

DeGroot: There are few fields that combine those two.

Diaconis: It's close in a way to music or being a pianist, except that the public doesn't recognize it as such. I mean the public sees magic in the same light as it sees a trained-dog act or something like that, and not the same way as it sees a concert pianist. Well, perhaps it's good, perhaps it's bad .... I finished a book on card magic with Vernon just last month. Let me tell you about it: Modern sleight of hand began about the turn of the century. Before 1900, books on magic would say, "The performer deals the second card by dexterously pulling back the top card slightly and dealing out the second card." In 1902, a book appeared which has very careful technical descriptions. It's called *The Expert at the Card Table* and it was written by a "reformed" gambler. It has five-page technical descriptions: "The left little finger pushes lightly on the outer corner," and so forth. It's still the best book ever written on sleight of hand. Vernon wrote a commentary on it. When we were on the road, we gave that commentary to a publisher who held it for ten years. Then I sued the publisher and finally got the manuscript back about ten years ago. I added a long historical preface. So I am still quite involved with magic. I like it and enjoy it.

"THE COMBINATION OF KNOWING ABOUT DECEPTION AND KNOWING ABOUT STATISTICS MAKES ME UNUSUALLY WELL-QUALIFIED TO TAKE A HARD LOOK AT PARAPSYCHOLOGY"

DeGroot: There are two areas in which I know you've been involved to some extent, I guess through your interest in magic. One is ESP and the other is gambling. Can you talk a little bit about how you've been involved?

Diaconis: Sure, The parapsychology stuff started in the following way. Martin Gardner was involved with *Scientific American*, of course. They got a book to review which was a report by a psychologist—a book-length report on a man who could make psychic photographs with his mind. Martin asked me to go take a look at the guy and try to do a really sexy book review. I flew to Denver and watched this guy cheat and swindle and double deal a whole room full of Ph.Ds in chemistry and physics and psychology. I've written about that several places. ["Statistical problems in ESP research," *Science* **201** (1978), 131–136.] I came back and reported. The topic was too hot for *Scientific American* to handle. They weren't interested. The guy was really phony, and they decided not to cover it. But that got me interested in parapsychology, and I started to do other investigations. Mostly reporting on outstanding psychics, and catching them cheating using magic expertise. But then when I got to Stanford I found out that there was another way that people cheated, and that was by using lousy statistics. And that's actually far more prevalent than sleight of hand and probably far more dangerous. I do have a peculiar combination of abilities for investigating these things. That is, the combination of knowing about deception and knowing about statistics makes me unusually well-qualified to take a hard look at parapsychology.

What happens is that you get involved and then, as with any applied area, interesting math problems come up. A typical example is this: In the classical parapsychology experiment, the one that Rhine did for years and years, you have a deck of 25 cards with five different symbols each repeated five times. The deck is shuffled and then I look at the cards and concentrate, and you try to guess what they are. Well, of course, if you had no ESP and you just tried to guess, your chance of getting the top card right is 1 in 5. Each card is like every other so you expect to get about five right. The distribution of the number of correct guesses of course depends on what sequence you guess. If you always guess the same symbol, you'll get exactly 5 right with no variability; and the variability is greatest if you guess a permutation. Well, the way they actually do ESP experiments often involves feedback. This feedback might come about consciously by my showing you—that is, you guess the top card as a "star" and I say no it was a "plus"—I give you feedback as I go through the deck without replacement. Or it might come about unconsciously—that is, I smile when you get it right and I frown when you get it wrong. You know, body English kind of feedback. I got interested in how feedback changes the odds, and that turns out to be a challenging, interesting math problem that has ramifications in sequential trials and all kinds of other things. Some of the hard math problems are wide open to this day. I've written a bunch of papers on math problems that came up in that way. [Diaconis, P., and Graham, R. L., "The analysis of sequential experiments with feedback to subjects," *Ann. Statist.* **9** (1981), 3–23; Diaconis, P., Chung, F. R. K., Graham, R. L., and Mallows, C. L., "On the permanents of complements of the direct sum of identity matrices," *Adv. Appl. Math.* **2** (1981), 121–137.]

So this work feeds on parapsychology. It's funny; I got tired of that finally. I mean, you get tired of proselytizing for science. When you go to several parapsychology experiments—I've been to dozens and dozens by now—and you see what actually goes on as opposed to what's reported in published papers, it's just bad news. They're sloppy and they're very often uncontrolled. People just don't understand what it takes to run a careful experiment. You just write off the field, as I have to some extent. And then you get tired of telling the same stories over and over again, saying "Listen, you shouldn't take it seriously either." But people still do take it seriously.

Recently, I had a funny opportunity. Stanford asked me to address their incoming freshman class about anything I wanted. I had to think hard, "What can I say to kids in an hour that would be important to them?" I decided that probably the most useful thing I could tell them is that parapsychology isn't a science and there are no replicable experiments, and to try to explain that. And then I could try to explain why probability and statistics is interesting in that context. When I began my talk, I explained what parapsychology is and said, "How many of you take this stuff seriously?" And about 80 to 90% raised their hand, thinking that it was a demonstrated part of science. So it probably is a useful service to go and say "Gee, there is no hard science there." Actually there is no hard science there by the parapsychologists own admission, but somehow people get fooled. You know, the field is 100 years old or more, and there are journals just like any other field. You get to thinking that there must be something there. Well, you look and look and look. I think that there's a lot to be learned for the application of ordinary statistics; that is, for me parapsychology is a field with no signal. Yet the fact that there could be all of this published literature . . . .

Think about other soft-hard fields—some of the social sciences. If there's that much leeway in multiple testing and throwing out data and sloppy experimental conditions to make a field out of parapsychology, there's also room and something to learn about other uses of statistics. That I think is interesting and feeds back into our field. A friend of mine, Ray Hyman, who is also a magician, and a psychologist and a good statistician too, went to the parapsychologists last year and said, "Give me your best paradigm. Give me the experiment that's been most thoroughly replicated." It's an experiment called the "Ganzfeld experiment." There were about 50 studies and he looked at all of them. You ask yourself, "Well, what are people actually doing?" They say they're going to do this test, but in fact they do six different tests and then take the one that works. Or you find lots of unpublished studies. By the time he put it all together, he took a seemingly very successful paradigm and showed that it just reduced to chance. [Hyman, R., "Does the Ganzfeld experiment answer the critics' objection?", *J. Parapsychology*, to appear.] It's a beautiful study. And there is something in it for us to learn that applies to other areas. Especially that there is a difference between data analysis and actually testing a well-formulated hypothesis. Those shouldn't be confused.

"I'M STILL FASCINATED BY GAMBLING"

DeGroot: Talk a bit about gambling.

Diaconis: I'm doing something right now that I'm very excited about. Let me lead into it through Bayesian statistics. It seems to me that the old fights in Bayesian statistics have gone away. That is, the fight between the frequentist and the subjectivist just doesn't have any electricity for anybody anymore. I mean, we all have coherent stories. We know how mathematics and experience link together, and a lot of theory has just diffused that debate. One change is that even for a subjectivist it's no longer enough to say. "I've got my prior." A thing I hear a lot is, "What information are you basing your prior on, and let's spell it out so that all of us can know it and know what you're saying." I heard that at the conference in Valencia that we were at. [Second International Meeting on Bayesian Statistics, Valencia, Spain, September 1983]. Well, it got me thinking about the basic paradigms of chance for me as a subjectivist. Things like flipping coins, drawing from an urn, shuffling cards, and rolling roulette wheels. Why do I think they're random? What is the basis of that? These gambling models are the very essence of probability, and also the basic models that we have in our heads when we think about chance. I've been carefully investigating those models. I'll give you one very cute example that I'm excited about now. Joe Keller and I began to think about the mathematics of flipping a coin. Why is a coin random? Of course, a coin isn't random. Let me explain a little bit. Suppose I flip a coin about a foot up into the air and let it land in my hand. Well, when it leaves my hand it has a certain upward velocity and rate of spinning, and then Newton determines how it's going to land. There's nothing random about it at all. Well, with Joe's help we did the physics and what happens is that the velocity-spin plane is partitioned into regions. In one region it comes up the same as it started, and in the next region the opposite. Same, opposite, same, opposite.

DeGroot: This is the no-bounce model.

Diaconis: Right. Bouncing is much too complicated. But we actually do flip coins and catch them, right?

DeGroot: Sure.

Diaconis: The regions have hyperbolas as boundaries—I want to show you a picture (Figure 2). As you go further and further out on the velocity-spin plane, the regions get closer and closer together, so that tiny little changes in the initial conditions determine whether you come up heads or tails. So that's a very beautiful analysis of why a coin is random. That is, from a subjectivist perspective it says that no matter how sharply you know things—as long as you don't know them too sharply—it's going to be random for you. OK. Well, I got interested in just where we are in this picture for actual coin flips. What you have to know when I flip a coin is how fast it is going upward and how fast it is spinning. Well, it's not too hard to get the velocity. You just see how high it goes. But the rate of spin is interesting. How do you measure that? Well, I spent yesterday with a stroboscope actually counting how many times a coin spins. The answer is about 10 to 20 if you flip it about a foot. It's quite sharply peaked at about 15. The point is that 15 is not a lot of times. I had four or five ways of measuring it actually, and I cross checked. One cute way is that I took a thin ribbon and I let the coin come to rest at the end of the ribbon. Then I spun it and I carefully unwrapped it. The result was consistent with the stroboscope. Now here is the interesting thing: If it is only about 15 spins, then the outcomes of successive tosses are correlated.

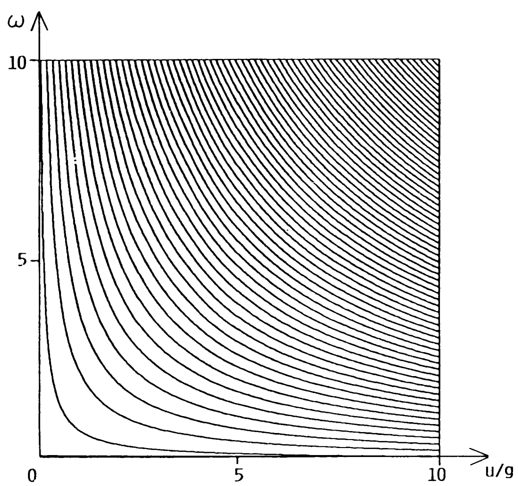


FIG. 2. *Hyperbolic boundaries for obtaining heads or tails. Here, u is the initial velocity in ft/sec; g is the gravitational constant, 32 ft/(sec)*^{2}*; and ω is the number of revolutions/sec. (Joseph B. Keller, The probability of heads, unpublished technical report, Department of Mathematics, Stanford Univ.).*

When I gave a talk at Valencia about spinning coins, the commentator on my talk said he'd asked a class to spin coins 500 times and he'd noticed correlations. Well, it was a class of kids, and 500 times is a lot of times to spin a coin. It takes an hour or maybe longer. The kids got tired and were only spinning it four or five times, just from hand to hand and just a couple of inches. In that case it *is* strongly correlated from one toss to the next, and it's mildly correlated if it only spins 15 times too. I think the correlation could be about .1 or something like that. Today I'm going to spin a coin a thousand times trying to keep the number of spins about the same and I'm going to measure it.

DeGroot: Do you always start the coin with the same initial conditions? Always heads up, or whatever?

Diaconis: No, I'm doing a repetitive thing. I'm going to just take the coin wherever it lands, pick it up, and flip it from that position.

DeGroot: But then obviously the tosses are going to be correlated.

Diaconis: OK, they are going to be correlated to *some* extent, but there's a question of whether it is .01 or .001 or .1. I actually think it's about .1, which I think is interesting because that is large enough so that it matters. I've done a similar analysis for dice, which is a much more complicated problem, and for roulette wheels and shuffling cards and drawing from urns. I'm actually interested in trying to quantify, when you turn potatoes in a pan, just how much you have to mix them up and how mixed up they get. All of that is the mathematics of gambling. In the roulette case it's not random and you can go make money, in the sense that you can do the physics of the wheel; they let you bet late enough so that you can get enough input to predict where the ball will come within half a wheel. People have known that for a while and have built gadgets to make that prediction. So certainly right at the moment, I'm doing math and probability that links into gambling. One project I've been fascinated by is the number of times you have to shuffle a deck of cards until it's close to random. Those are beautiful math problems. And because of that project I've had to do all of this noncommutative Fourier analysis I'm doing now, treating shuffling cards as a random walk on the symmetric group and repeatedly convolving a probability with itself. You can actually use that machinery and get useful answers; for real riffle shuffling the answer is seven [laughs] and I could explain why. Anyway, I'm still fascinated by gambling. I read about it and think about it. You know, the roots of probability go back there, and I think that the interest for us hasn't stopped. There's still a lot to do, lot's of interesting questions.

Here's a question I can't do: One way casinos cheat, when they cheat, is to shave off a little of a die. Suppose I tell you the measurements of the die, what are the probabilities? Nobody can touch that problem. I could go on for an hour about it. An ordinary casino die is .75 on a side—¾ of an inch. And they shave off about 1/100 of an inch, so that means the top and bottom are each squares which are .75 on a side, and the other four sides are each .74. So what is the chance of a one coming up? Is it going to be .169 instead of .1666? You'd have to roll the die a couple of hundred thousand times to detect that. Dice get round if you roll them a couple of hundred thousand times. You're not going to do it without thinking about it, and how do you think about it? Well, I could do it letting the die land in my hand, but that isn't so interesting. It's a bouncing, rolling problem. It's not a conservative dynamical system.

I have many funny stories about that. I actually did an experiment. I went to a crooked gambling house and got them to make, very carefully, special dice for me. Clearly a big source of sampling error was going to be counting; that is, we were going to do an experiment which was rolling ten dice at once and counting the number of big faces that came up. Well, if you had ordinary dice, there just would be some error from counting and that would probably swamp everything. So I got dice carefully made to be all blank and having the two big sides with giant spots on them. And the dice maker said to me, "Gee, I never saw anything like that. What do you want it for?" And I said, "I'm a professor of statistics and I'm doing an experiment." And he said, "Sure, buddy, sure." [Laughs]

This is also work with Joe Keller, and we each had a model. Joe modeled how much energy the die loses each time it hits, and my model was very different. But our models gave the same answers to three decimal places, .169 we thought. We knew that even rolling the dice 10,000 times wouldn't be enough to distinguish our models, but we did enough of the experiment to show that both of our models were wrong. [Laughs] Nobody has a clue as to how to do that problem.

"IF YOU'D ASKED ME TWO YEARS AGO WHETHER THIS STUFF WOULD EVER BE OF ANY INTEREST IN A REAL-WORLD PROBLEM, I WOULD HAVE SWORN NO"

DeGroot: I notice that you have some 45 papers on your list of publications. Do you have favorites among those that you particularly like or that you think are particularly important?

Diaconis: Well, one always likes what one is doing at the moment. Right now I'm hard at work on two big projects. One is a way of drawing pictures on a computer-graphics terminal. I have a friend, Mehrdad Shahshahani, who works at Boeing Aerospace in a flight-simulator wing. He is designing methods of making background pictures. When a pilot flies in a flight simulator, he wants to be able to look out the window and see pictures of leaves and mountains and trees and things that look realistic (Figure 3). If you think about flying 600 miles an hour, that's an awful lot of scenery to store. In fact, it's much too much to store, and you get the idea that you want to create it in real time as the thing goes on. Even that's not so easy to do because you want to have it look sort of realistic. Mehrdad came up with an algorithm to make very realistic pictures with very little memory. The algorithm uses the fixed points of certain affine transformations. He developed it heuristically and could make very realistic pictures. Let's see if I can find any of them. [Getting out some pictures] That leaf requires about 24 numbers to store, but it has a level of complexity about it that just doesn't look like 24 numbers, right?

Well, what I realized was that you can associate to his algorithm a Markov chain. The stationary distribution of the Markov chain becomes the plotted picture. One can prove a theorem like that, and it turns out that the stationary distributions that come up are very often singular continuous, which is what makes those things look so leafy and complex. So I'm very involved in studying the stationary distributions of these Markov chains. That gets into the fascinating world of the mathematics of iterated maps and chaos and dynamic systems. Physicists are very busy doing the same thing. They have a lot of their own machinery and, in fact, they sort of developed their own world

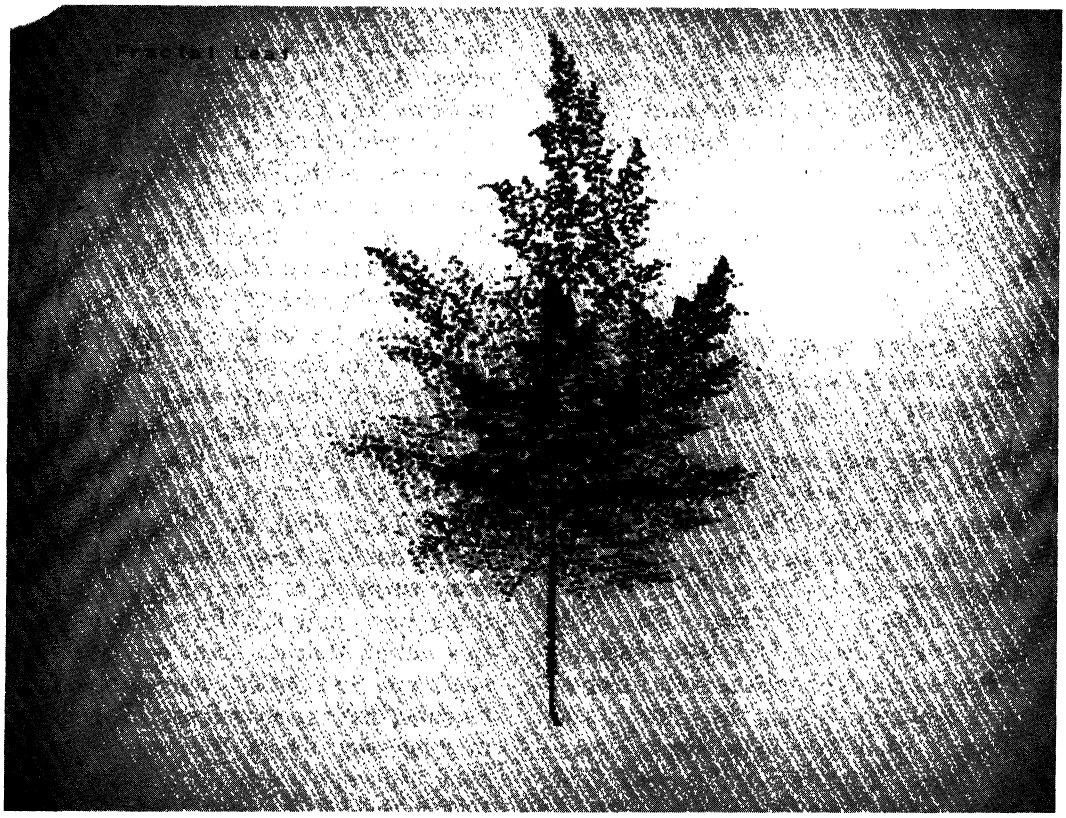


FIG. 3. *Fractal leaf.*

of randomness disjoint from ours. I'm quite excited by that work. One nice thing is that the picture problem uses theorems that were developed by Paul Erdös in the late 30's. If you'd asked me two years ago whether this stuff would ever be of any interest in a real-world problem, I would have sworn no and given 100 to 1 odds that no one in our lifetime would be interested in it for a real problem. Erdös' tools are the only tools we have to actually get our hands on these stationary distributions. They're very beautiful Fourier-analytic arguments.

DeGroot: Those are really fascinating pictures. But if you talk about flight simulators, don't you need a continually changing picture?

Diaconis: Yes, and actually at Boeing I believe they are thinking of making movies. These algorithms work fast enough so that they can be done in real time. They won't generate a whole scene; they generate pieces of a scene and you need something that keeps track of the pieces and decides whether it's a mountain with trees on it or just what.

DeGroot: And that changes smoothly, too?

Diaconis: Right. But having ways of drawing the ingredients is important. I'm not so involved in that, although I think that some of what we've learned from mathematics has actually helped explain some oddities in the algorithm. You can understand where certain bands that appear come from and how to get rid of them, and stuff like that. That's one thing I'm hard at work on.

I'm also hard at work on problems of exchangeability. That's been one of my major focusses. I never understood where models come from in statistics, and it seems to me to be an important problem. When is a model right, and what's the use of a model, and what do parameters mean, and what are we doing? I don't mean that in a critical way. I think that what I call English-style statistics is very useful and interesting, but how does one think about it and understand when it is useful and when not. For me, a lot of insight comes about through de Finetti's theorem, which takes simple notions of symmetry and lets them build a model for you.

So you believe the model if and only if the symmetry is plausible to you. And what kinds of symmetry give rise to which models? Almost all of that work has been done jointly with David Freedman. One of our main focusses has been on the fact that de Finetti's theorem is always set at infinity: If you have an infinite exchangeable sequence, then something holds. One wants to know if infinity is a tail wagging a dog or if, in fact, a finite sequence is well approximated in some sense. We have just cracked a problem that's been plaguing us for a long time, plaguing everybody who works in the field. We have a quantitative way to describe notions of partial exchangeability; if you have a finite exchangeable sequence, then it's almost a mixture of i.i.d. things, with sharp bounds. It's hard nuts-and-bolts calculus, but with this funny mixture of philosophy. You know, it really does start as a philosophy problem, trying to think of where models come from, and you're led to these very tangible computations.

"I PROBABLY KNOW MORE ABOUT THE BIRTHDAY PROBLEM THAN ANY LIVING HUMAN AT THE MOMENT"

DeGroot: And you're working on several books?

Diaconis: I'm trying to write about four books. One is based on a series of lectures I gave this summer on partial exchangeability, and in order to get paid I have to deliver a manuscript. So I'm working hard on that. I am writing a book with Charles Stein on graduate topics in decision theory. Charles is teaching the course again this year, and I hope that gets finished soon. I have sworn to Shanti Gupta, the editor of the IMS monograph series, that I would deliver my manuscript on group theory in statistics this year. And then there's a book with Fred Mosteller on coincidences. That's four. And then there's the book with Ron Graham on teaching mathematics through magic tricks that we've just signed a contract for.

DeGroot: Teaching mathematics to whom through magic tricks?

Diaconis: I guess we have in mind the bright high school student or Martin Gardner's audience. The thing that is wonderful about Martin Gardner is that no matter how much mathematics you know, you can pick up his column and be fascinated anyway. I hope our book has such broad appeal. We plan to start with very simple things like, "Here's a magic trick that you can do. It's a good trick and you can fool people with it. Now how does it work?" Well, in order to understand how it works you have to understand some group-theoretic concept or some notion of probability or something. And then we might talk about possible variations of the trick. Well, that leads to all kinds of math problems, and often some of them are solved by beautiful classical mathematics. The purpose is just to try to explain to people how it is that grown-ups can be so fascinated by mathematics.

DeGroot: It's a wonderful idea.

Diaconis: I think it's nice. As I learned from Fred Mosteller, there are problems with magic in teaching. I sometimes used to mix a little magic into a lecture. Like, I wrote a paper about how many times you have to perfectly shuffle a deck of cards to bring them back to order. The answer is eight, and sometimes when I give that talk I actually shuffle a deck of cards perfectly eight times and show that it comes back to order. And I notice that the hostility level of the audience triples. That is, as you know, you never explain everything in a talk, so people have to take things on faith. Well, people start asking you the simplest questions after you've done it. They're sure that nobody who could shuffle a deck of cards eight times perfectly could also prove this theorem. "What do you really mean?" and "Is it $2n + 1$ or $2n - 1$?" They really try to give you a hard time. That lasts about 15 minutes, and then they forget that you could shuffle cards perfectly and they believe you again.

The same thing happens with students if you do some trick. You know, put the piece of chalk down and talk, and then start writing again and you've got chalk again. Well, then if you do a demonstration, people don't believe the demonstration. If you do a demonstration of drawing balls from a box or anything, they think it's all hoaxed up. Somehow there is this notion that an actor couldn't really also be an academic. So I noticed that for me, mathematics and teaching don't go so well with magic. People lose belief in some way. I hope that we can carry it off in the book project. To actually convince people that there is this bridge between very tangible entertaining things and mathematics, and that actually it's all the same. I mean mathematics is just very entertaining, too, and surprising. That's what we're going to try to do. Ron Graham, my co-author—and my co-author in many other things—is an expert juggler and has written very interesting papers on the mathematics of juggling. You know, how many balls could you juggle on the moon, or if you were in a phone booth ... And there's a beautiful theorem due to Shannon which relates the parameters of gravity and the number of balls and the number of hands, and so forth.

DeGroot: Tell me about the book with Fred Mosteller on coincidences.

Diaconis: Well, what we are trying to think hard about is how people who have numeracy—that is, who know about numbers—relate to the plethora of coincidences that abound in life. All of us have had really surprising things happen to us. How do we think about them? It's not going to be a math book. It's a project to try to understand how to think about surprising occurrences. Of course, it's got some math in it. I probably know more about the birthday problem than any living human at the moment. You know, triple birthdays, nonuniform probabilities, dependent birthday problems; I could go on and on and on, and I have. The matching problem is a classical example of how math can trick you into being surprised at coincidences, but you can learn to understand how it's not so surprising. But we also talk about things like coincidences in literature or coincidence as a literary device. It's always hokey when somebody resolves his plot by having two characters meet in a bus. Well, Fred has just finished a chapter on that. A coincidence could be the *focus* of a story, too.

A lot of work by us and others has gone into understanding the psychology of coincidences. That is, what do you notice and remember. I just bought a slightly fancy car, and I notice a lot of them on the road now. In fact, perceptually I think one out of ten cars is a BMW. Of course that's crazy, and the problem is that I have a tough time keeping track of the denominator. Well, that's a phenomenon that psychologists like Ruma Falk have in fact investigated pretty carefully, and we will report some of that work. If I tell you a story about a coincidence happening in the future, it seems more surprising if it happens than if I tell you it happened in the past. People find that less surprising, the exact same story, because they understand that you can hunt around in the past and find things. But if I'm making a prediction about the future, that's different. So we're investigating pretty broadly the phenomena of coincidences. Right at the moment I'm reading quite seriously the work of Jung and Koestler on coincidences.

DeGroot: Arthur Koestler?

Diaconis: Arthur Koestler wrote a couple of books on coincidences and did some experiments and stuff. You know he's a great writer, and nothing of a scientist. I hope to make that clear in my treatment. But he does write beautifully. Jung said that there are too many coincidences to explain by simple chance mechanisms, and therefore there must be a hidden synchronous force. And Koestler picked it up. I want to try to think hard about that. In particular, Jung did some experiments and made a lot of interesting statistical errors.

DeGroot: That aspect sounds like it's related to the parapsychology stuff.

Diaconis: Right. It is related, and yet it's not a negative treatment. I've had amazing coincidences—walking in the library, picking a book off the shelf, opening it up at random, and seeing a formula that I was working on right then and there. That happened to me at a very crucial time in this perfect-shuffling stuff. I picked up a volume of Paul Lévy's collected works, in French, and I thought, "Well, I wonder what Lévy wrote about." I opened it at random and there was the equation for perfect shuffles. And I just let out a whoop because I realized Lévy had written about perfect shuffles and there was going to be some amazing connection. Now, how do you think about that? Well, the first thing is that you *can* think about it. I mean, it's not that miracles occur. I have very broad interests and I browse a lot, so if I pick up almost anything, the chance that I'll find something that connects to something that I'm interested in is actually pretty high. Well, I've used some of that to build a theory of browsing.

I find browsing very useful as a positive use of coincidences. When we see a coincidence, somehow it delights us and we learn things. Well, you can study how much people browse. And you can think about how you should browse. I built a little theory and have some examples and some data. Fred has been collecting coincidences and actually trying to document them. You know, there was a hurricane on May 3rd three years in a row in a town that never had any hurricanes for 50 years; how do you account for that, Mr. Wiseguy? Well, maybe we *can* account for it, although certainly strange things happen that nobody can account for. It's not finished but I think that the bottom line will be a rational theory for examining coincidences. My feeling is that miracles don't occur, but I'm not going to stick to that yet. [Laughs] Does that give a fair picture of what we're about? If I spend next year at Harvard we might finish a solid first draft of it. We've got about 400 pages of typed manuscript.

"I FEEL A LOT OF TENSION FROM A YOUNGER GENERATION COMING UP NOW WHICH FEELS IT CAN DO ON THE COMPUTER AN AWFUL LOT OF WHAT WAS PREVIOUSLY DONE BY MATHEMATICS"

DeGroot: I'd like to hear your views about the field of statistics in general. Let's talk about the accomplishments that you've admired most in the field.

Diaconis: Well, for me statistics starts out with real science. Everybody takes averages and tries to put a plus or minus number on them. You know, tries to give a feeling for the uncertainty. What happened was that there was a plethora of different sorts of techniques, and then around the turn of the century people started to think hard about that and introduced more careful calculations, and tried to build mathematical models that would make this or that procedure reasonable. That was when mathematical statistics really started. Of course it started with Gauss, but it became an industry about the turn of the century. And then it got very mathematical and made giant strides. Having a genius of the power of Fisher certainly helped. He's been our dominant intellectual figure, in my view. Even though he wasn't a Bayesian, he was a great scientist. But then statistics got very mathematical and inbred. It became a field of its own and almost an area of mathematics, at least mathematical statistics did. And of course I'm always amazed and delighted when I go into the real world and see how much of standard statistics is used there. People really do a lot of interesting stuff working in the field. But I think that we also started to look at our own navels pretty sharply.

I feel a lot of tension from a younger generation coming up now which feels it can do on the computer an awful lot of what was previously done by mathematics, and therefore it doesn't have to know mathematics and is even hostile to mathematics. If I give a course on new developments in computer simulation or data analytic procedures, I'll get 50 people in the course, and if it's on some beautiful topic in classical mathematical statistics, I'll get a much smaller group. And that's at Stanford, which is very strong in mathematics. I really do feel a tension. People say, "Why do I need to know mathematics? I don't know that I can trust the asymptotics anyway, and I can just try it out and I'll know the answer."

I think the tension will be resolved in the following way. There is a plethora of data-analytic techniques. Tukey must have invented 10,000 things, and one out of a hundred is actually interesting and 99 are crazy. I think he admits that. Now as soon as he comes up with one that actually has some interest and is worth thinking hard about, we can do the mathematics of it or make a good stab at it. One of the big things has been robust statistics, and mathematics has contributed. The influence curve, which is a function–space derivative, and many other notions have really contributed substantially to robust statistics. I think mathematics has contributed a lot to the bootstrap and projection pursuit. These are all things which started out totally ad hoc. "Well, we tried this, we tried this, we tried this. Ah! Here's something that looks interesting." As soon as you get something that looks interesting, mathematics has contributed. And I think the young people will see that mathematics has the language and machinery for clarifying and unifying. So I think mathematics will prove it's utility in that way. It's not even such different mathematics from the mathematics that was developed for other things—the invariance principle and, of course, a lot of combinatorics. You know, the central limit theorem is just here forever and you've got to learn about it. [Laughs]

So, some of the hot topics that I see are problems that involve the computer, certainly. But I like to point out that there are also problems that are very simple to state that the computer can't touch. They involve questions like how many times you have to shuffle a deck of cards until it's close to random. Well, 52! is just a *big* number; and if I give you a specific way of shuffling, a mathematical model, and you start to try to similate it, you just can't touch it. You know, it's 10^{58} or 10^{63} or whatever. You just can't do that problem without thinking hard about it and doing some mathematics. The same is true for lots of kinds of combinatorial problems that are coming up now in statistics. You can't do them on the computer without *some* mathematics. A blend of mathematics and the computer is developing. I hope that after 10 or 15 years, the tension will go out of that debate in the same way that the tension has gone out of the frequentist-subjectivist debate for me. People will see that the computer is important, mathematics is important, and you can't get along without both of them and be a professional. But at the moment I sense that there really is a feeling of hostility against classical mathematical statistics.

DeGroot: I've heard some dire predictions for the future of statistics as a field because of the dominance of the computer and computer science. Do you think the field is healthy and thriving?

Diaconis: Sure. Statistics is just starting. The problems of how to make inferences from big messy data sets can't be done by brute force, and the tools and approaches we have are sensible things. I see the same ideas that we've kind of codified in statistics coming up in area after area. When I say it's just starting, I mean that modern statistics is only 80 years old. Compare it to physics. It's a field that's going to grow and prosper, as long as it admits, which I think it does, that there is a real world and that one has to continuously see what real scientists are doing and need. It cannot just take the old paradigm and say "This is what statistics is." But we're all moving in the right direction; anybody serious does look at what people do. I think that our tools are the right tools, and I see them being applied again and again in strange new territories. I don't think that statistics is out of business at all.

"NOTHING DELIGHTS ME MORE THAN FINDING A REAL PROBLEM WHERE, TO ACTUALLY UNDERSTAND IT, I'M GOING TO HAVE TO LEARN A LITTLE ALGEBRAIC GEOMETRY OR ANALYTIC NUMBER THEORY"

DeGroot: Some of the problems that you work on seem to be chosen because they are beautiful problems, and others because they arise from real problems. Is that right?

Diaconis: It's exactly right, and it's a good way to understand what I do. There are two threads to it. Part of my work is that I'm paid by the Stanford linear accelerator to work with Jerry Friedman's group. In a sense I'm hired to keep Jerry honest. That is, Jerry is one of our most creative statistical scientists, but he never took a statistics course. And so every once in a while he invents something wonderful and amazing, and sometimes he reinvents the wheel. When Jerry invents something wonderful, I try to link it into mathematics and statistical inference. Maybe 20% of my work has been taking something like projection pursuit and making mathematics out of it.

I can't understand mathematics without a real problem. I've learned more mathematics than is good for any one person in a lifetime and if I don't have a real problem that it links to, I just forget about it. So nothing delights me more than finding a real problem where, to actually understand it, I'm going to really have to learn a little algebraic geometry or analytic number theory or whatever. I just couldn't be happier. That could be a fatal character flaw, I don't know. But it's certainly true that I love mathematics and I use working on applied problems as a way to help me understand mathematics.

DeGroot: What kind of courses do you particularly like to teach?

Diaconis: A good or bad thing about Stanford is that we are mainly a graduate program. So (a) I have almost never taught undergraduates, and (b) I have almost never taught the same course twice. I've been here 11 years and the one course I've taught twice or maybe three times now is data analysis—sort of Tukey-style data analysis. That's interesting and keeps me in touch with what's going on there. But next term I'm teaching a course called analytical combinatorics, which is how to get information from generating functions. It will be about complex variables. And I'll prove the prime number theorem and talk about a beautiful new technique using Riemann surfaces for solving queueing-theory problems that I'm trying to understand. I'll also be teaching a course in Markov chain theory. I've never taught either course before. One I made up and the other is our standard masters' level course in Markov chain theory. I'm looking forward to both of them. Teaching is terrific in that again it forces you to actually figure it out. It's nice that I've had a chance to just teach all kinds of different courses. I'm actually well known for it. I'll teach anything as long as I haven't taught it before.

DeGroot: How long are you going to keep that up?

Diaconis: As long as I can, Morrie. [Laughs] I've started to teach courses in the math department a little bit. We trade. I'd like to teach calculus. I taught group theory and it was a lot of fun. I learned a lot and I hope the kids learned something, because I do really use group theory in very tangible ways in real problems and it's real for me. It's not symbols on a page; it's ways of shuffling a deck of cards or rotations of a die or something. I hope that came across. Students keep you alive in important ways. That's a terrific thing about Stanford. If I put up a sign saying I want to teach a course about any crazy subject, or give a seminar, I'll get 20 smart people there. It could be infinite-dimensional convex sets. This year it's the other extreme; I'm giving a seminar on correspondence analysis. I was embarressed about not knowing what European contingency-table analysis is, and Jerry Friedman and I decided to figure it out. We put up a sign, and there are 30 people sitting in. Next quarter I'll do a seminar on foundations of Bayesian statistics—half in the philosophy department and half in statistics. Dick Jeffrey will be visiting Stanford and we hope to make some progress on connections between philosophy and statistics.

DeGroot: That's good stuff.

Diaconis: It's terrific. I mean, I use teaching as an excuse for learning stuff. Both of my seminars are free; I don't have to do them. I'm just doing them because I trick myself into learning. I still like it.

"THE MAIN IMPACT HAS BEEN TO MAKE ME TAKE MY OWN IDEAS MORE SERIOUSLY"

DeGroot: A couple of years ago you got this MacArthur Foundation Fellowship commonly called the genius award.

Diaconis: Oh God.

DeGroot: Tell me about that award. What does it involve?

Diaconis: It involves a complete surprise and close to $200,000, tax free, over a period of five years. Every month I get a check. It's ceased to become a terribly visible part of my life. It's going to be visible when the checks stop, but I don't notice it so much now. The main impact has been to make me take my own ideas more seriously. Before I got that award, I was thinking about what I should do in the next four or five years. Well, Charles Stein is retiring. I have a very good relationship with Charles, and I should really try to write down some of what Charles knows. I have done some of that. We have written joint papers and I've done a book based on his course notes, but I was really going to devote a sizable amount of time to that. I decided not to do that full scale, but instead to say, "Gee, the world is saying that you should follow your nose more, and the things that you think are interesting maybe actually *are* interesting." I'm working on pretty nonstandard things, these funny dice things, and I really am trusting my subconscious more. If something smells interesting and I don't know why it's interesting, and everybody else thinks it's crazy—well, I'm going to do it anyway. I haven't used the money to free myself from teaching because I find teaching very healthy for me. It keeps me working hard and learning stuff. I'm not thinking of the award as a five-year thing. I'm thinking of it as, "I have this money and I should salt it away, and then when I want to go away for a year or a quarter I can do it."

But the main thing that it has done is to make me have a kind of healthy respect for my daydreaming, healthier than I had before. I don't know if that's good or bad, but I think that's been the main impact of it. I don't worry so much about whether something is actually going to pay off or be interesting. I just say, "Well, I'll try it." I think I probably always did that. That is, my first bunch of papers were all on number theory, and trying to get tenure at Stanford as a number theorist doesn't sound so sensible. Except that people were very encouraging and said, "Well, if you do serious stuff, we'll take it seriously too."

DeGroot: I would imagine that your being given this award was a result of your having had that attitude in the first place.

Diaconis: It must be something like that. Anyway, it's neater than anything. The money isn't bad either. [Laughs]

DeGroot: The hours are good and the pay . . .

Diaconis: The pay is right. It's terrific. They don't care what you do with the money. You can give it to the Communist Party or buy a swimming pool or go around the world.

DeGroot: What is the Rollo Davidson prize that you were awarded by Cambridge University in 1981?

Diaconis: I was as thrilled with getting that as I was with the MacArthur, and it's 100 pounds. Rollo Davidson was a brilliant young English probabilist who died very young, at 28 or so. He did geometric probability, and David Kendall and people at Cambridge University built up a trust fund. Every year they give it for innovative probability or something like that. In my case it was to recognize work in data analysis and exchangeability. I was surprised and thrilled to get it. Let me tell you why. Much of my recognition has come from "Prof does card tricks." You know, I'm this colorful character who was a magician and stuff, and a lot of the public recognition came from that. But this was recognition for more serious work that I had done, and it just pleased me a lot that somebody had noticed.

DeGroot: Did it involve going over and giving a talk?

Diaconis: No, it didn't. But I think I'm spending this spring in Paris, and I'd like to go over and give a talk there at Cambridge. I hope they remember me.

DeGroot: How long are you planning on staying in Paris?

Diaconis: The whole term, spring quarter; teaching a course on what I call new-wave multivariate. It's sort of funny. They asked me what I wanted to give a course on. I said, "Well, all of this new-wave multivariate." I think that's a very exciting area of statistics: projection pursuit, recursive partitioning, some of the work of the Danish school in multivariate. Things are actually happening. The old normal-theory models are going by the wayside, thank God. People have very innovative ways of looking at data. I call it new-wave multivariate. So I wrote that to them, and I got back a very concerned letter asking whether this is some kind of Fourier analysis. [Laughs] They hadn't heard the "new-wave" expression before.

DeGroot: What kind of group will you be working with?

Diaconis: It's a group of what we in America would call very mathematical statisticians, but they are in an economics department. France has very applied statistics—this correspondence analysis around Benzecri and his group, which shuns any probabilitistic calculations and is very close to Tukey. And it has wonderful theoretical probabilists. But it doesn't do much mathematical statistics the way we do it, and what there is is done in economics departments.

DeGroot: I know you've also been involved in research on the psychology of vision.

Diaconis: Right. I've written a couple of papers on that and I'm doing work on it now. One avenue comes from data analysis. Tukey is always saying that this is a good way to draw a graph and this is a bad way to draw a graph. You know, hanging rootagrams. Well, how does he know, and is he right? I don't know how to think about that other than by taking different ways of drawing graphs and trying them out on people; asking them to make inferences and seeing which ways are insightful and interesting. Bill Cleveland and I did a project in which we took different ways of scaling scatter plots and asked people to make inferences, such as assessing association. [Diaconis, P., Cleveland, W. S., and McGill, R., "Variables on scatterplots look more highly correlated when the scales are increased," *Science* **216** (1982), 1138–1141.] We found that there are some funny artifacts. If you make a scatter plot little in a surrounding box, people think the data are correlated; whereas if you blow it up in the box, people think it looks more like noise. Of course, rescaling a scatter plot doesn't change the association at all. You can think about that and try to choose a scaling which will not distort pictures too much. The problem is how to automatically get a computer to scale scatter plots. That was an experiment with real people, going and asking them questions.

I'm also involved in another very interesting class of problems. There's a perception psychologist at Bell Labs named Bela Julesz, who wonders about how people see foreground and background. That is, if I look at you, I see you, and other things blend into the background. Well, what features of a pattern make the eye see part of it as foreground and part of it as background? Julesz does experiments in which he shows people lots of pictures and asks them questions. It turned out that some very esoteric math that I was doing in the language of partial exchangeability was able to demolish a 20-year-old conjecture of Juelsz's. Julesz thought that all the eye could see was density and correlation. He conjectured that two pictures that had the same amount of black and white in them and the same correlations—first and second order statistics—would be visually indistinguishable. But using mathematics from partial exchangeability one could very easily build striking counterexamples to that conjecture and, by smoothly interpolating, suggest further directions and experiments. That was a long paper that David Freedman and I did and published in a psychology journal. ["On the statistics of vision: the Julesz conjecture," *J. Math Psychology* **24** (1981), 112–138]. It's been thought to be useful and people are doing experiments based on our models. I did a lot of looking at pictures during that work.

Amos Tversky and I are currently doing psychology experiments to try to quantify how we look at graphs and how things are distorted. It's an area I find useful, especially in data analysis. Another problem of perception is that when you start to analyze a big body of data, your first impressions often can make you look in certain directions and forget to check out other directions. Psychologists have quantified how preconceptions affect inferences in very interesting ways. I've written about that too, in the language of data analysis, trying to bring to bear the hundreds and hundreds of studies in psychology to the psychology of data analysis. ["Theories of data analysis: from magical thinking through classical statistics," to appear in *Exploring Data Tables, Trends and Shapes* (D. Hoaglin, F. Mosteller, and J. Tukey, eds.)] I think that there are things to learn from psychologists. They've done a lot of interesting things that just need to be translated out of their language and into ours. So I hang around with psychologists and talk to them and read their literature.

"I THINK STATISTICS IS A BEAUTIFUL RICH FIELD"

DeGroot: What does the future hold for Persi Diaconis?

Diaconis: Just going crazy, working hard, learning more math.

DeGroot: You are already going in all directions.

Diaconis: You know, to prepare for this interview I got out my vita and looked at what I've been doing. I guess I feel happy about it. I think statistics is a beautiful rich field. How could you work in all the different areas that I've been allowed to work in, and still call yourself *one thing*—a statistician. The field is rich enough to allow that and even support it. You can do work in group theory and number theory, and do work in applied perception psychology, and it's all part of probability and statistics. I have no plans to do anything other than what I'm doing, which is going 20 hours a day and hoping to keep up with myself. [Laughs] I'm still fascinated by problems and trying to understand in my own language what the elders have been doing, hoping to relate that to what the youngsters are doing.

One thing I haven't mentioned is how much I've benefitted from work with co-authors. That is, even though I read a lot, there's an enormous difference between reading and having somebody say "Ehh, it's just so and so." You know, sitting you down. I've had the benefit of really great co-authors. David Freedman is my principal co-author. David is a marvelous skeptic about everything, and how he puts up with me is beyond me; but he patiently listens and helps.

DeGroot: Because you are a marvelous enthusiast about everything.

Diaconis: Right, and we complement each other very well. He helps to throw out the really crazy ideas, and when there *is* a germ of something he says. "Maybe what you were saying is so and so." And he'll say it back in terms of some beautiful math problem that we then charge at. He's been an enormous plus in my life. Ron Graham has taught me a lot of combinatorics and computer science. We've written half a dozen papers together. I've had the good fortune to work with Mehrdad Shahshahani, who is actually a group theorist and who taught me all this group theory that I'm doing, this noncommutative Fourier analysis. Just to have somebody who spent ten years learning it patiently sit you down and say, "Well, here's what it is in three dimensions." There's nothing better than that.

That's one way I think science is changing, and it's a thing I've learned from Erdös. Erdös and I had some hard topology problems when we were working together. He said, "Listen, we could do this, but it's crazy. I could just call somebody who knows the answer." Mathematical statistics has gotten broader, and it's hard to be an expert in all the different areas. By the end of my career, it really will be as split up as mathematics. I think that right now more or less everybody in my department knows what everybody else does. And in your department, too. You can still *just* keep track of everybody. But then if you think about the different areas—sequential analysis, design of experiments, graphics, data analysis, the computer interface—well, you realize it's all getting away from you. It's hard to keep track of. And I think that in 20 years it will have split up enough so that it will be hard to be a universal person. But now you can still hope to be that.

I would like to do a study about this notion of working with other people. How could we do a study like that? I think that in our field people have started to work with other people more and more. I mean, there are more and more double-authored and triple-authored papers. Why? Because there's just different

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expertise, and the field grows. But in all science I think there's more joint work because of communication. That might just be a funny impression and might not be true, but I think it is.

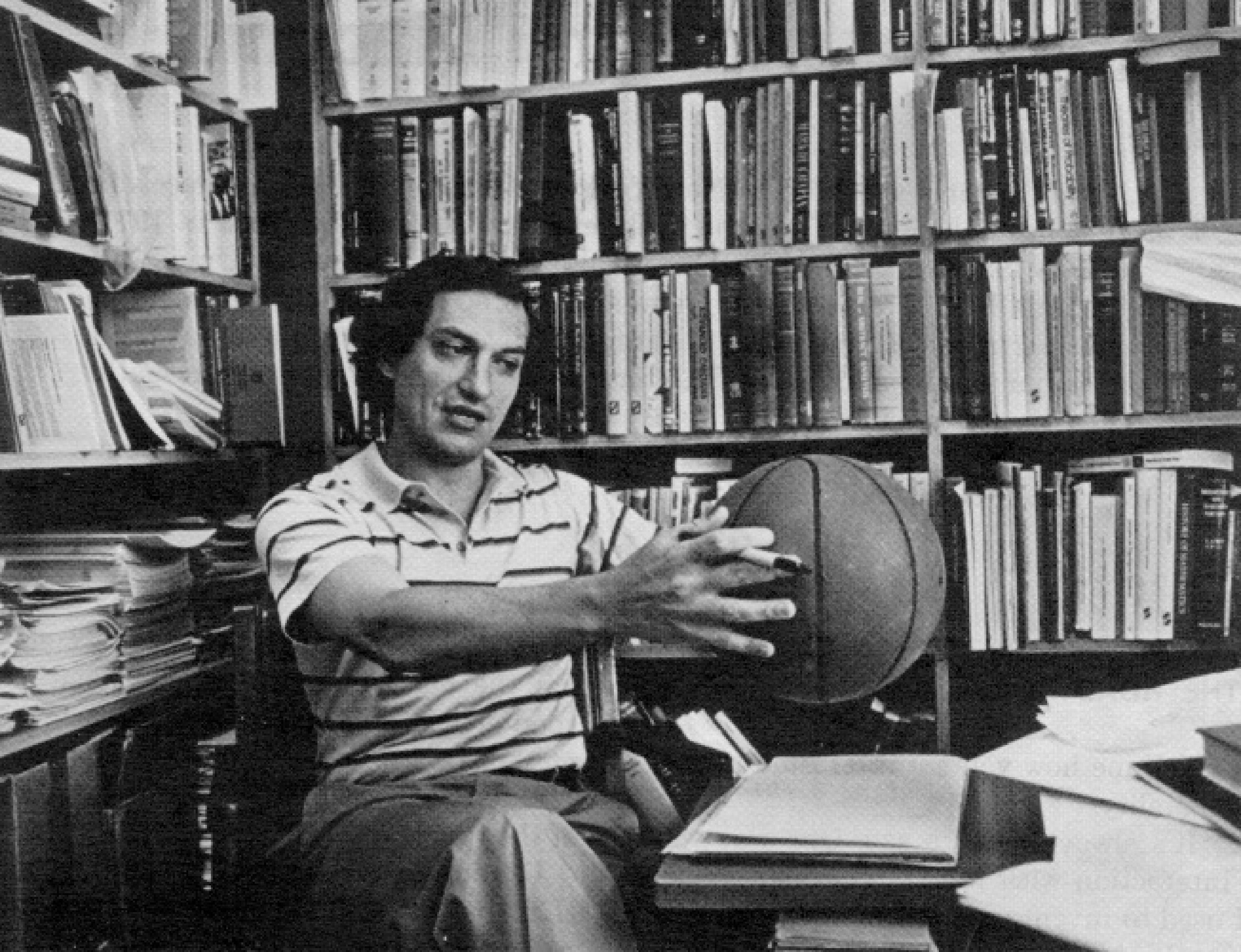
DeGroot: I always tend to think of mathematics as being a single-author field. Perhaps as statistics moves further away from mathematics into the main stream of science, and becomes more applied or just more interdisciplinary, we see more multiple-authored papers.

Diaconis: At the moment you may need co-authors because if you're doing a problem that actually requires heavy-duty clever computing, that's as much a contribution as the math. So you work with somebody who is good at that. But then as the field splits again and the areas become further apart, people will start working in their area and there will be more single-author papers. But then it will start to split further and at the start of the split you'll need other co-authors. I could see an oscillation going on.

DeGroot: That's very interesting. As one branch of statistics becomes a field itself, you'll get the single-authored papers again. But in statistics now, some skill is required in even identifying and formulating problems from substantive areas, and that's the kind of activity that lends itself to conversation with others out of which the joint papers grow. The problems are certainly not coming ready-made to work on.

Diaconis: They do a little bit. Every once in a while somebody walks through the door with a nice clean problem. But then it's already two authors. [Laughs] It's nice that our field is so noncompetitive in a way. That is, if you take many other fields, like biology, people just slice each other up. Our field doesn't have that. I notice it most with people like Charles Stein. Charles doesn't bother to write things down, but people will bend over backward to give him credit twenty years later. We all do. You know, you talk to somebody and they give you an idea. Well, you give them credit for it, and if it's substantial enough they become a co-author. Our field isn't in that kind of high-pressure state where you are publishing every two weeks. That's a very pleasant part of the field. If somebody borrows an idea, well more power to them. There's a lot to do.

DeGroot: Thank you, Persi.



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