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A Conversation with Geoff Watson

R. J. Beran and N. I. Fisher

Abstract. Geoffrey Stuart Watson, Professor Emeritus at Princeton University, celebrated his 75th birthday on December 3, 1996. A native Australian, his early education included Bendigo High School and Scotch College in Melbourne. After graduating with a B.A. (Hons.) from Melbourne University in December 1942, he spent the next few years, during and after World War II, doing research and teaching on applied mathematical topics. His wandering as a scholar began in 1947, when he became a graduate student in the Institute of Statistics in Raleigh, North Carolina. Leaving Raleigh after two years, he wrote his thesis while visiting the Department of Applied Economics in Cambridge University. Raleigh awarded him the Ph.D. degree in 1951.

That same year, he returned to Australia, to a Senior Lectureship in Statistics at Melbourne University. He moved in 1954 to a Senior Fellowship at the Australian National University. Three years later, he left for England and North America. In 1959, he become Associate Professor of Mathematics at the University of Toronto. In 1962, he became Professor of Statistics at The Johns Hopkins University in Baltimore. Soon thereafter he was appointed department chairman. In 1970, he moved to Princeton University as Professor and Chairman of Statistics. He became Professor Emeritus at Princeton in 1992.

He has published numerous research papers on a broad range of topics in statistics and applied probability. [A curriculum vitae is given in Mardia (1992).] His best known contributions are the Durbin–Watson test for serial correlation [see Kotz and Johnson (1992), pages 229–266], the Nadaraya-Watson estimator in nonparametric regression (Watson, 1964) and fundamental methods for analyzing directional or axial data. He is the author of an important monograph, *Statistics on Spheres*. His professional honors include Membership in the International Statistical Institute and Fellowships of the Institute of Mathematical Statistics and of the American Association for the Advancement of Science. In private life, he is an accomplished painter of watercolors, a few of which may be seen on his website (http://www.princeton.edu/˜gsw/) at Princeton University. He married Shirley Elwyn Jennings in 1952. Their four children, one son and three daughters, pursue careers in Japanese literature, health care in Uganda, singing opera, and administering opera and ballet.

We interviewed Geoff Watson in his office at Princeton University on the occasion of his 75th birthday. The walls of the room were covered with a selection of his watercolor landscapes. Early December sunshine, slanting through the large windows, lit up his paintings and our conversational circle. Because Geoff has given a highly readable account of his boyhood and career beginnings in the article "A Boy from the Bush" [in Gani (1986); reprinted with additional material in Mardia (1992)], we directed our questions to his subsequent professional career.

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Beran/Fisher: Perhaps we could start with your life outside statistics, particularly your family and your hobbies? Your wife has a very interesting family background.

Watson: Her father was a very famous academic constitutional lawyer with a gift for administration. He wrote fundamental books about the British constitution and then took a job to reorganize the University of Ceylon. It wasn't a university then, it was a college examined by the University of London. He went out there and did that, so Shirley went to school in southern India. She's the really interesting member of the family. During World War II, when her ship was attacked and sunk, she was injured. Our children often asked to hear the story of the German plane strafing this half-sinking boat, and Shirley jumping overboard and missing the water and hitting the lifeboat and smashing a leg. The anesthetic was Scotch, so she can't bear the stuff!

Beran/Fisher: They didn't want to hear about your life in Bendigo?

Watson: No, it's pretty boring. I didn't hear a shot fired while she went through the Blitz.

Beran/Fisher: Where did you meet her?

Watson: She was in Women's College at University of Melbourne when I came back from the States in 1951, so soon after term began, actually on Anzac Day, I met the head of the college at a cocktail party, who said: "You must come to dinner and meet the girls. They like to have a faculty member at High Table." It so happened that one of the math girls had a date, so she swapped with Shirley and Shirley told a set of very funny stories. I thought I could do with a little more of this humor, so I asked her out. I took her to the ballet. She had said she was interested in the ballet. I had a friend who knew one of the ballerinas and I thought, "I've never given flowers to a ballerina; it would be rather fun to do that." I didn't have a car so we took a taxi. We went downtown and I bought a little gardenia for Shirley and I bought this great bouquet of flowers. Shirley didn't know what was going on. I went to the stage door attendant and said: "Will you give this to Mademoiselle Slobodova?" or something like that. Which he did. It never occurred to me what sort of impression this would have on Shirley. I'd never done it before.

I've never forgotten that first meeting. That's how we met. So we wandered around and had all these kids. They've been as restless as we have. None of them lives in the U.S. any more.

Beran/Fisher: A lot of Australians are strongly peripatetic.

Watson: They've caught the virus. Our son does medieval Japanese in Japan; he has an academic job there. The eldest daughter has a Princeton biology degree. Now she writes and broadcasts in Uganda on social and medical problems. She lives in Kampala. The next one, Beccy, is an opera singer based in London. The youngest daughter works for The Royal Opera House and is about to leave to go to Frankfurt. Fortunately they have much higher standards of living than we had at their ages and are passionate about their vocations. We have a place in the Adirondacks that they like, and I spend most of my life maintaining it.

Beran/Fisher: None of them showed the slightest interest in following in Dad's footsteps?

Watson: I didn't push it. Michael could have. But he had a real flair for historical and literary things. I hoped for art history. I thought I could really empathize with all that. He decided his visual memory wasn't that great. So he went into medieval European literature at Cambridge University and then the Japanese version. It's all beyond me. He decided he'd better go to Japan and do it. Beccy gives us a reason to travel to hear her sing.

NORTH CAROLINA

Beran/Fisher: Turning to your professional life, you've written many papers since your early work on serial correlation with Jim Durbin. What stimulated your interest in that area?

Watson: Well, the training in Raleigh, in North Carolina, was exclusively analysis of variance. It was a bit different in Chapel Hill, of course, but still there was a great emphasis on regression and ANOVA—I rather liked the algebraic side of it. But I knew little about matrices then, so I had to learn matrix algebra from lecture notes of P. L. Hsu, who had given a course, unfortunately a year before I got there. At some stage I had to think about a thesis and also where to be. I was a little bored. There didn't seem to be anybody quite right for me in Raleigh or Chapel Hill, and Cochran was going off to Johns Hopkins.

I got interested in time series analysis, thinking there must be some estimation problems in stochastic processes with which I could combine my interests. Wishart happened to pop into Raleigh and I looked after him and I told him about my problems. He said, "I can get you a job in the Department of Applied Economics at Cambridge." Regression with stationary errors was of interest to them. Dick Anderson's thesis dealt with circular serial correlation matrices. Hotelling had pointed out their nice properties. Ted Anderson had discovered when there are optimal tests for detecting serial correlation in the error term and when the least squares estimator was also the BLUE. Dick and Ted had just written a joint paper applying their methods to the case of regression on trigonometric functions, that are eigenfunctions of a circular stationary matrix—that's a case where the LSE coincides with BLUE. So Dick Anderson said: "Well, here's a thesis problem. Why don't you do the general case?" That was good enough to get me a job at Cambridge. So as soon as I finished my general exams after two years I went to Cambridge.

I mentioned Bill Cochran earlier. He was really marvelous with graduate students. I went to NC to be his research assistant. He gave me job after job—never the same one twice. He would call me and say "This is what it's about, check it out." I learnt a tremendous amount in that first year just doing little jobs for him, for example, developing missing plot formulae for some crazy designs. He used to joke that my formulae made an experiment unnecessary. He was very, very good in that sense—a model guide. I greatly admired R. C. Bose. He taught me multivariate analysis, and design and analysis of experiments. Every lecture was a work of art. He used modern algebra, well ahead of everyone else, so much so that I did not quite have the courage to think seriously along those lines for several years. He was very modest and, sadly, his mimeographed notes were not published.

Pitman visited and gave three truly marvelous and madly original courses—nonparametrics, inference, and applied probability [Department of Statistics, University of North Carolina at Chapel Hill; available in the department's technical report series.]. I had lunch with him after lectures and became a close friend. I have continually revisited these lecture notes—based courses on them, used certain tricks I learnt there. I got nothing much out of any other courses except Cochran's "messy data" course.

CAMBRIDGE

Beran/Fisher: Who was at Cambridge at the time you went there? You mentioned Wishart.

Watson: Wishart was a reader in agriculture and head of the Statistical Laboratory at Cambridge. I was in the Department of Applied Economics, just 200 yards away, and that was run by Richard Stone, who subsequently won a Nobel Prize for economics. Jim Durbin was already there doing research. He'd just graduated with a diploma in statistics from Cambridge. Frank Anscombe and Henry Daniels were lecturers in the mathematics faculty and Dennis Lindley was an assistant lecturer. Lindley was very silent in those days, just getting himself organized. (I've just read his *Statistical Science* interview with Adrian Smith (Smith, 1995) that nicely describes him.) He was giving a course in Kolmogorov-based probability, being critical of Fisher, and so on. I talked to Henry Daniels a lot. I was treated like an additional faculty member. They were very nice to me and we lunched together. In my usual way I would enthusiastically describe my latest results. Henry would often say the next day: "That was obvious." No doubt it was.

Beran/Fisher: It sounds as if your colleagues didn't worry much about departmental boundaries.

Watson: There were no departmental boundaries where I was concerned, but the joke used to be that this was an applied economics group. We weren't allowed in the door of Economics. All the economists were anti-mathematical. They believed you had to do it with words, which was bloody hard. You have to be very clever to say all these things, for example marginal utilities—quite hard to define in words but mathematically trivial. In fact, the economists thought that Richard Stone was so subversive they made this little extra department to keep him out of theirs. There were two or three people in Stone's group who subsequently got Nobel Prizes and every one of the first 15 Nobel Prize winners in economics visited Applied Economics. By the way, I dislike the social sciences!

Beran/Fisher: Did you ever feel that you wanted to be an academic in Cambridge after you finished your Ph.D. studies?

Watson: Never occurred to me. I never found anything that made me want to be other than an academic. I didn't think that way as a child. My father wanted me to be a dentist. Once I got to university I was so excited by the whole cultural and intellectual atmosphere—such a change from a country town—that nothing else but academic life even vaguely entered my mind. When I finished mathematics in Melbourne and got the prizes, my father took us all out to dinner, which was a very rare thing, and said, "Well Geoff you've had your fun, would you like to start Medicine next year?" He hadn't quite given up. He actually said that at the celebration! Medicine was an over-respected career then. I used to say how boring it would be to be a dentist. I was saved by a doctor my father greatly admired, who assured him that I could have a good career in research, which was so alien to a country town.

DOING RESEARCH

Beran/Fisher: How did you find research problems after you finished your thesis? What kept you going?

Watson: One accrues problems. You get some insight: like a bad dream it keeps coming back. You hear of related problems and at least you know what they are talking about. Sometimes you've got enough insight to make a contribution.

Beran/Fisher: Did you have some specific problems in mind?

Watson: No. I always tried to think of an economical use of mathematics to get right to the heart of whatever the question was. I really set out to emulate Cochran. I never regarded myself as a mathematician going back to first principles, never ever, for obvious reasons. I wanted to be motivated by practical problems and then do the theory. That's always been the thing that I really value. You've got to do a lot of talking to people to get the background science. It happens naturally when you're young, because you are talking to other young people and they've got time to chat. But as you get older it's rather harder to do that, so I must say that in recent years I just do the things that I've thought about for fifty years.

Beran/Fisher: Your earliest university studies were quite catholic in subject range. These days, it seems that students don't receive a general scientific education and so have difficulty communicating with people outside their own area.

Watson: Yes, it's part of the price of being good at mathematics nowadays—it's become very abstract. I used to read *Scientific American* every month almost cover to cover. Now I can't get through one article: either it's got too technical or I've become too stupid or something. I think it's a bit of both. I had this great dream at Hopkins (because there really were no departmental boundaries there and I was basically ruling the roost) that I could get each student involved in some scientific problem. But no graduate student wants to take the gamble of putting real time into learning some other subject because once you get a degree, you're off to get a job. Maybe they didn't have the basic science background to build on.

Beran/Fisher: The closest thing in some U.S. universities may be the Minor system?

Watson: I was speaking of graduate students. I always think statisticians get into a subject after the horse has bolted. All educational systems seem to stifle the openness of minds.

Beran/Fisher: Is there any instance where statisticians have not gotten into a subject after the horse had bolted?

Watson: Yes. Paleomagnetism is a case, one of very few.

Beran/Fisher: What got you into that field? You were fortunate enough to be involved in this.

Watson: No smartness in my choices, always just luck. I hope Pasteur's remark applies: "Chance favors the prepared mind." R. A. Fisher had been visiting Melbourne. I was in charge: Maurice Belz was off on his fifteen months of study leave, so I looked after Fisher. We took Fisher on a picnic, "we" being Evan Williams, my wife, Rupert Leslie and his wife, Bruce Hall and Fisher. [In a letter to RJB, Shirley Watson remarked that Geoff's photograph of R. A. Fisher was taken at this picnic near Melbourne.] We ended up at the camp fire, boiling up the billy and all that stuff. Either Rupert or I spotted a snake. My eyes were always peeled for snakes: I'm very scared of them. Anyhow we spotted this snake and chased it with a stick as any Australian would have, and broke its back—now of course it's illegal—and then Rupert had it at the end of the stick, dangling. The head of the snake was looking alive. Rupert took it proudly back to Fisher to show him what intrepid Australians we were. He went absolutely through the roof and gave me my first lesson in ecology. The idea of killing a living creature who was not trying to do anything else except to get away from us shocked him to the core. Anyway, he must have forgiven me. We sent a food parcel to his daughter, Joan, in England, at his request. As a "thank you" note he sent me a reprint of his 1953 paper "Dispersion on a Sphere" (Fisher, 1953) which is not easy reading, as you would know, Nick. Anyhow I had a look at it and suddenly saw that I could clarify things.

I was still in Melbourne but about to leave for a new position in Canberra. A Melbourne friend who was at the Australian National University (ANU) said: "We've got a chap, Ted Irving, who's coming to do paleomagnetic research so you'll have a great time when you see him." So I put off doing these things until I got settled at the ANU and could find out what the real problems were. Ted and I got on like a house on fire. So that's how my involvement in paleomagnetism started. And of course once you get into a subject it's hard to drop it.

Beran/Fisher: In this case there weren't many people in the field.

Watson: No, there was nobody, I had no smart blokes to compete with, which was very nice at that time.

Beran/Fisher: You are now regarded as the founding father in directional statistics. You've kept on coming back to it. Did you actively seek new problems apart from applied problems?

Watson: I did actually let the area lie fallow for quite a long time, because there was nobody around me who was doing the experimental work. So for quite a few years I pursued obvious mathematical leads.

Beran/Fisher: Such as?

Watson: I always had this feeling that you really understand something if you can see it in a more general setting. Fisher said, "You can only understand two sexes by studying three or more." In the same way that I think, for the most part, one of the reasons I love linear algebra is that it all looks to me so much simpler in the abstract in any number of dimensions than it does when I am fiddling around with two or three dimensions.

Beran/Fisher: There's a tremendous paper that you wrote which is not well known to statisticians, on "Orientation Statistics in the Earth Sciences" (Watson, 1970). When did you begin your interactions with geologists?

Watson: There was a very clever young structural geologist at Hopkins when I arrived there in 1962 and we started talking. He'd obviously had a good mathematical training, especially for a geologist. The head of Earth and Planetary Sciences was Francis Pettijohn, who's a famous sedimentologist and quite interested in statistical things. So my collaborations with earth scientists started again at Hopkins. But I took too big a bite. I thought I'd write a book about orientation stuff for the whole of the earth sciences. So of course I just got lost learning earth science.

This project also had to compete with my other passion, molecular biology, which is where I really put most of my time. This interest began in Canberra, working with John Cairns and Stephen Fazekas de St. Groth in microbiology and immunology. Various people came to Canberra, for example Joshua Lederberg. He won a Nobel Prize for work on sex in bacteria. I was really deeply involved in these topics and in fact wrote several papers. So with Lederberg's support, I got a fellowship to Cold Spring Harbor on Long Island, in 1958. I did the lab courses, tissue culture, phages and bacteriological genetics. I was really hot! Jim Watson came there and lectured on DNA structure. (He and Crick were actually doing it when I was in Cambridge and I lived right next door to the pub where they used to go. I didn't know them then.) I really invested a lot of time in molecular biology and I'm not sorry. I never got a damn thing out of it but it was fabulously exciting science. At that stage no mathematical people got anything out of it, not until later.

Beran/Fisher: To continue with directional statistics for a bit, your book *Statistics on Spheres* went through several drafts. When did your work on the book begin? In your Johns Hopkins days?

Watson: Oh yes. I wanted to set out some basic directional stuff and then apply it to all the earth sciences. I never set out to write a theoretical book about directional data, mainly because I hate reading what other people write. I find it so hard. I have to see things my way. So that's why I could never be a real author. My cupboards are full of book drafts!

Beran/Fisher: Are you currently interested in spherical statistics?

Watson: I do feel some responsibility for spherical statistics. If I see anything that's wrong, especially in my own papers, then I'd like to fix it while I'm still able to. It's a loyalty to something I've grown up with. In a 1983 paper on the theoretical aspects of estimation on spheres (Pitman and Bayesian estimators, decision theoretic things I usually avoid), I mentioned that I would give the analogs of the Cramér–Rao lower bound to the efficacy of the estimators of unit vectors in another paper. I gave these results in lectures but never wrote the papers. Mysteriously, I didn't put them in *Statistics on Spheres* either. Producing that volume was a precondition set by the University of Arkansas when they asked me to give four lectures in a series they have. The book was typed as I went around the world on a sabbatical, which partly accounts for the awful state it's in.

Beran/Fisher: What interactions have you had with some of the British researchers who have worked on directional statistics?

Watson: I've visited Kanti Mardia and John Kent in Leeds several times and they've often come to Princeton.

Beran/Fisher: Have you kept in touch with Mardia?

Watson: Yes. Kanti loves to come here in the spring because there's a big secondhand book sale that Shirley takes him to. The top floor of his house is basically full of books. I don't know why it doesn't collapse. He has about 900 editions of *The Rubiyat of Omar Khyham*!

John I first knew by reputation because of his work. His Ph.D. thesis was motivated by my paper with Hartman (Hartman and Watson, 1974) showing that with diffusion with a random stopping time you could get the Fisher–von Mises distribution. He was born in New Jersey so I tried to get him to come to Princeton. But he decided to go to Leeds. We have one joint paper. One time he was here I was organizing a seminar in geology. Somebody spoke every week. I picked people who had some vague statistical difficulties and then I would try to dream up how to solve them on the spot. Somebody started talking about radioactive dating. The methods used were rather awful and I thought they could be improved—errors-in-variables problems. So John and I and the chap who gave the lecture and provided the data worked on it. Another chap gave a talk about reversals of the earth's magnetic field and we were led to make a little model for the times between reversals, a nonhomogeneous Poisson process. So it led to at least two papers. I became a little bit more interested in rotations because one of the chief plate tectonic guys took part. Plate tectonics was started at Princeton by a man called Harry Hess. The tradition's been kept on.

Beran/Fisher: You worked on an estimator of rotation?

Watson: Yes, and describing rotations in the various ways. Part of the reason that I got involved was that an undergraduate friend of mine, Jock Mackenzie, first solved the rotation problem in crystallography. (Mackenzie, 1957). Apart from moving continents, people asked me how to align large molecules and to see how ice flows move. Ted Chang does all this beautifully, now.

Beran/Fisher: To return to England for a moment, you mentioned in "A Boy from the Bush" (Watson, 1986). that you had some interesting interactions with Harold Jeffreys.

Watson: Yes. I knew who Jeffreys was because I'd seen him on his bike, riding around Cambridge. He never came to the Stats talks. But when I walked in to give my Durbin–Watson talk to the Stat Lab in Cambridge, who should be sitting in the front row but Harold Jeffreys. I was absolutely petrified. He had a very vigorous pen. When I finished Wishart made a joke. I had two matrices **M** and **A** and he said I was transforming them to **Ph.D.** That was Wishart's contribution. But Jeffreys said, "That was a very nice talk, Watson, I enjoyed it. Er, that inequality you mentioned, I think I've seen it before. Maybe Courant and Hilbert?" [Specifically, Courant and Hilbert (1931, 1937).] I thought it was very kind of him to let me off the hook. Years later I looked at the English translation of Courant and Hilbert (Courant and Hilbert, 1953, 1962). and there it was! When later I sent Fisher the first papers I wrote about statistics on the sphere, he gave them to Jeffreys to submit to the Royal Astronomical Society, which he did.

Jeffreys did these two nice things for me. So, when I make a joke about Jeffreys getting the wrong end of the stick with continental drift (which means that Bayesians should be aware of believing that theory dominates sense!), don't get me wrong: I do admire the man immensely. As did Fisher. They were friendly rivals. Ted Irving thinks that one of the joys that Fisher got out of the work on continental drift was that he knew Jeffreys was a fixist while he believed, with young people like Keith Runcorn, that continents moved. If he could help in any way to defeat Harold Jeffreys, he'd like to do so. Fisher was a mischievous sort of chap so I think there's something in that! [In a personal communication to NIF, the paleomagnetist B. J. J. Embleton commented: "Jeffreys was regarded as a fixist—he believed in the finite elastic strength of the Earth's mantle. Fisher's involvement was through partnership with palaeomagnetists who were actually developing the tools in the 50's and early 60's to investigate the magnitude of the continental displacements that Wegener had postulated in 1912."]

R. A. FISHER

Beran/Fisher: Can you expand a bit on your interactions with Fisher?

Watson: My first meeting was the one I described when he visited Melbourne. Later we went on leave from Canberra and Shirley went ahead to Cambridge. She saw Fisher walking down the road and she knew he was infirm in some way. She decided he was deaf. So she stood just out of his visual range, and shouted! They got that sorted out. Then I turned up. I had been reading his book *Statistical Methods and Scientific Inference*, and trying desperately to understand it. I knew the man was a genius and he sounded so plausible. But when you say, "What the hell does it mean?" it's not so good. His 1953 sphere paper is really dedicated to fiducial inference—that's what it's about. "Once again I'll tell you clods how to do it." My paper with Evan Williams (Watson and Williams, 1956) annoyed him because we were explicitly using the Neyman–Pearson approach, whereas earlier papers of mine were just analysis of variance stuff. So I met him at Cambridge and he invited me to dinner at his college. I told him of my dilemma understanding his paper. His study had a hot gas fire and he said, "Sit right here in front of the fire" and there was just enough room for him to walk up and down in front of me, which he did. I was hot and dizzy. He really blew me away. I couldn't understand what he was saying.

Beran/Fisher: In a schoolmasterly kind of way?

Watson: He was very schoolmasterly. On other occasions I used to go out to his lab and sit with him and have a cup of tea. Graduate students would arrive with some biological problem. He was absolute charm itself, he would talk away and try to sort it out. But with people who were statisticians and trying to do something he had a much tougher line.

Beran/Fisher: Did you talk to Fisher about science?

Watson: He was better at describing the state of the earth and paleomagnetism than Ted Irving.

Beran/Fisher: He was a scientist first?

Watson: He was a really great scientist—a genius. He was clear as crystal. His lectures were very clear for the first five minutes. I don't blame him for having no patience with ordinary mortals.

Beran/Fisher: In Fisher's early work you see his tremendous geometrical intuition. Did this play a role in his scientific perceptions?

Watson: I think so. That was partly because of his poor eyesight. Certainly when we were discussing the earth and the internal fluid motions. They are hard to "see." He had so many things right. When I was a graduate student anything geometrical was not considered a proof: you had to use characteristic functions or some other boring technology.

Beran/Fisher: Counter intuitive. I suppose most people didn't have a command of n-dimensional geometry.

Watson: No. That's a relatively modern thing, unfortunately. I was slow to grasp it. Although Bose talked in these modern terms. I loved his lectures. He was really a very fine mathematician, but I realized that I couldn't be a student of his: I did not want to create designs or do combinatorics.

Fisher was at least as good at algebra as he was at geometry. In 1923 he wrote a paper on the effect of manure on potatoes with W. A. Mackenzie (Fisher and Mackenzie, 1923). In it he found the best l_2 rank 1 approximation to a matrix. The algebra leading up to the singular value decomposition is all there but he didn't go on because the first term was such a good fit. He certainly could have—maybe he originated this approximation problem.

A little later, he introduced group theory into design of experiments. Just before World War II he showed how to generate "scores" [see e.g., Example 46.2 of Fisher (1941) and later editions]. This involved eigenvalues and eigenvectors. The background paper was apparently never published. In a regression context, I used these ideas for inspiration in a 1952 medical consultation to get scores for the visual state of the tongue, where the regression variables were nutritional. One chose these scores to maximize the linear regression SS to error SS ratio. For contingency tables, this has been rediscovered by Benzecri, Goodman and others.

Beran/Fisher: When did you last see Fisher? You left Cambridge and . . .?

Watson: I left Cambridge to come over here and I didn't ever see him again because I didn't go back to Australia for ages.

NONPARAMETRIC REGRESSION

Beran/Fisher: We'd like to ask you a bit about your work on nonparametric estimation and regression. You obviously entered the field early.

Watson: Yes, unfortunately before I could do any computing or anybody could do it for me.

Beran/Fisher: There were some graphics in your 1964 paper (Watson, 1964) on smooth regression analysis.

Watson: I wrote that at Johns Hopkins. I think I got Dick Jones to help with a little computing. It was pretty difficult. There was no real graphical output in those early days.

Beran/Fisher: How did you get interested in nonparametric regression?

Watson: I don't really remember, probably through the hazard curve. From linear models you get the idea of drawing lines through points and this is one of the fundamental problems of the subject, putting lines through points in one way or another. I think the penny dropped when I read a very strange mimeographed paper of John Tukey's (Tukey, 1961) called "Curve Estimation" I think. Everything had a funny name. I already knew of the kernel method of density estimation.

Beran/Fisher: How did you meet kernel density estimators?

Watson: Earlier I had worked at the Research Triangle Institute, when I was in Toronto. We were very much interested in hazard curves because we could get lots of money for contracts on reliability. People were always drawing these things. The only paper on density estimation at that stage was by Murray Rosenblatt and he put the essentials in two pages: kernel estimation—very neatly, no nonsense. Manny Parzen had done things with spectral estimation so it seemed to be the obvious thing to do these tricks with density estimation. In a way, density estimation should have come first because it is the more basic part, but in fact, historically, it was the other way around. We got Ross Leadbetter from New Zealand to come to North Carolina, and he and I wrote a density estimation paper (Watson and Leadbetter, 1963). That's how I got into density estimation.

Beran/Fisher: You and Ross also wrote two hazard curve papers and then you moved out of the area.

Watson: Well, I couldn't compute. The younger statistician just doesn't understand what it was like. I've gone through the slide rule, punch cards, tape, and girls on the calculators and all these things and now to have this thing here [pointing at a Power Mac on his desk]—it's better than the best computer in the world until around 1970. It's hard to believe, and graphical output just changed our lives totally.

TORONTO

Beran/Fisher: How did you come to take up the appointment at Toronto?

Watson: I came to the U.S. and spent the 1958–59 academic year at Princeton. The preceding summer I had been at Cold Spring Harbor on leave from the ANU and the next summer I was to visit North Carolina at Chapel Hill. I came to the U.S. on an Exchange Visitor's visa and of course I couldn't stay on in the U.S. So I had to go somewhere and we decided not to go back to Australia. Toronto was selected.

Beran/Fisher: Who was your contact in Toronto at that time?

Watson: Well, Don Fraser had been in Princeton as a Ph.D. student many years before and a Canadian, Al Tucker, was head of the Princeton Maths Department at that time. Statistics at Toronto was then within Mathematics. It was a pretty natural choice. Personally, Toronto was very nice indeed. We're still friendly with all our neighbors there. However, I found the climate very tough and the teaching schedule was in a very short (24-week) academic year, totally intense: I couldn't go away on holidays.

Beran/Fisher: How many courses were you doing in those days?

Watson: A lot, it seemed to me. I suppose it was only two courses a semester or whatever was the unit. I had a consulting job with Ontario Research Foundation. I'd go anywhere to earn a buck. I had no money at all. I had an ONR contract in North Carolina. I used to go down there. So I was really rather busy. Big stress.

Beran/Fisher: And you'd started directing Ph.D.'s.

Watson: That's right.

Beran/Fisher: What were your contacts with Dan Delury?

Watson: Well, Dan was actually Chairman of Mathematics in Toronto for a long time. I believe he was chairman when I arrived.

Beran/Fisher: Didn't he have something to do with the Ontario Research Foundation?

Watson: He found that job for me because he'd worked there previously. I didn't have really much time to interact with people in other university departments. I knew a couple of people in geophysics and had endless fights with the biologists because I used to give a course called "Mathematics for Biologists." I introduced the square root of minus 1 for mathematical reasons and the Chairman of Biology was onto Dan Delury saying "What's this young idiot doing? No one in biology needs to know this." The Biology Department was into 19th century stuff. There was nobody doing molecular biology. I had all these spherical problems in my baggage. I never bothered with the circle because no one asked me for methods. Michael Stephens was then a graduate student wondering about what to do and he could also compute. So I nabbed him. They had some IBM machine.

Beran/Fisher: An IBM 650 or something like that?

Watson: Yes, something like that. That's how Michael's thesis got started. At the same time I was doing work on density estimation with Ross Leadbetter by telephone and mail—unfortunately there were no faxes. That led to Ross's Ph.D. thesis.

Beran/Fisher: There is a rumor that Michael Stephens once helped paint your house in Toronto. Any comment on that?

Watson: I'd have to check with Shirley, but I think it's extremely unlikely. [In a personal communication to RJB, Shirley Watson commented: "Not the whole house! I would say a work session, lots of statistical chat while painting!"] I remember having you, Rudy, and Ed Rothman and others helping pick up some railroad ties in Baltimore.

Beran/Fisher: Jim Durbin was there too.

Watson: That's major exploitation.

Beran/Fisher: Michael apparently got to know your family pretty well.

Watson: That's right. His daughter is named after our Madeleine. He was at the house a lot.

Beran/Fisher: Was he your first graduate student, your first Ph.D. supervision?

Watson: It was my first opportunity. Nobody had any graduate students in Melbourne. There were graduate students in Canberra but I wasn't there very long. Everybody worked with Pat Moran. I remember helping quite a lot with an Indian chap in Canberra, but I left there too soon. By the way, one of the things I did when visiting Princeton in 1959 was the circular analog of the Cramér–von Mises goodness-of-fitness tests—the Fourier series stuff, U^2.

Beran/Fisher: Is this what's now called the l_2 approach?

Watson: Fourier series was my secret weapon for a while. Irwin Guttman was visiting Princeton when I was there. I'd done a lot of work in Australia just before I left on chi-squared goodness-of-fit tests, fiddling around with degrees of freedom and methods of fitting and so on. So Guttman asked how would it be with the Cramér–von Mises test. Being an old linear models guy, I turned everything into infinite sums of squares, which seemed a natural thing to do, and then I put it in a drawer because I didn't know what to do with it. Anderson and Darling had computed the W^2 percentage points. Then that summer in North Carolina when I was working with Ross, a German pigeon man came and said, “Look, I disorient my pigeons and I think they go off uniformly at random.”

Beran/Fisher: This was Klaus Schmidt-König from Tübingen?

Watson: Yes, Schmidt-König came in and said, “I want a test of this” and I said, “I just happen to have the exact thing you need”; same as the story of Fisher and the Fisher distribution. [See George Barnard's story about R. A. Fisher's introduction of the Fisher distribution, in Fisher, Lewis and Embleton (1993), pages 12–13.] I knocked it (Watson, 1961) off very fast. You know the Ajne paper. The report came in one Friday and the paper (Watson, 1967) was off on the following Monday, and then you, Rudy, were off generalizing it.

Beran/Fisher: Why did you move on from Toronto? Was this a stage in your life when you wanted to keep moving?

Watson: I was only an associate professor at the time and I was a little bored. I had no money. We did buy a house and did get started in life. Shirley loved it there. But I got lots of offers from the States.

Beran/Fisher: This was a time when many Canadian statisticians went to the States.

Watson: Yes and George Box wanted me to go to Wisconsin, and I was forever in the States giving talks. Then came the Hopkins offer.

JOHNS HOPKINS

Beran/Fisher: Why did you move to Johns Hopkins?

Watson: I rather liked the smallness of Hopkins and the fact that when I went down to see them I met everybody in every department. The chairman and staff were people at least of my age then. I thought this was going to be great science. I would not be bothered with serious undergraduate teaching. My role was to build up the graduate side and help down in biostatistics.

Beran/Fisher: I guess you knew Allyn Kimball already.

Watson: Allyn and I had been graduate students together in North Carolina.

Beran/Fisher: He was chair at the time you started at Hopkins?

Watson: That's right. That was part of the deal. He would be chair. So I had no administrative responsibility. All appointments, all academic choices and so on were mine. That worked out well of course, but when he became dean, my life wasn't so pleasant.

Beran/Fisher: Were there other considerations that prompted your move to Hopkins?

Watson: I've jumped over the fact that I wanted to be somewhere warmer. I also wanted to be on the East Coast so that I could go to Europe easily. I had a big thing about Europe in those days.

Beran/Fisher: Was closeness to New York and Washington relevant?

Watson: No. We didn't know how interesting Washington was until we got there. We'd often go down to the coast. We rather missed the sea. I think it was going south to get the warmer winters and staying on the East Coast. Hopkins is the classiest university as you go down the coast. Maybe Duke is classier but nobody offered me a job at Duke, and I have reservations about the South.

Beran/Fisher: One result of your Hopkins stay was that you suddenly found yourself with a group of Ph.D. theses to supervise.

Watson: Yes I did. I don't know whether I handled that well. You have to find out how to do these things. There was nobody to teach me how to do that.

Beran/Fisher: The success rate of your students was high.

Watson: I like to think that's true. When I look back it seems pretty weird. We are given teaching positions for life but with nobody teaching us how to teach looking after graduate students. I didn't have much in the way of models because Dick Anderson suggested a problem and then I left to go to England. I learnt my research tactics from the group at Cambridge. Nobody taught me how to cope. I had to do it intuitively.

Beran/Fisher: Only a tiny fraction of Ph.D.'s continue doing research. It's as if they never learned to do it properly in the first place. Somebody's pushed them through a Ph.D., and that's it. They haven't got the basic skills to identify a problem and set about solving it.

Watson: I thought I was able to teach by example, but I can't really discuss this! My courses were all based on my own work!

Beran/Fisher: The interesting thing was, you weren't writing the thesis for anybody. You were getting people moving by forcing them to think about the material.

Watson: That's certainly the way I like to operate. I did that in many ways with Michael Stephens. There I mapped it out in much more detail. We were systematically exploring things.

Beran/Fisher: You had many very interesting visitors at Hopkins in those years—people like Vidyadhar Godambe—because the funding was there.

Watson: Godambe was the result of an error I made in a paper on finite population estimation. I discovered his paper in the *JRSS* where he had got it straight, so I invited him to come from India. What he had done seemed to me a very fundamental thing. There was no existing theory for sampling from finite populations.

Beran/Fisher: Nothing sustainable.

Watson: No. And no optimality. So I got Godambe to come. He was a clever bloke.

Beran/Fisher: Although it was a small group, there were these tremendous visitors who stirred things up.

Watson: David Kendall, C. R. Rao, Vidyadhar Godambe, Jim Durbin, Ted Hannan, Rupert Miller, Edwin Pitman. With Rupert I was lucky because Rupert's mother lived in Pennsylvania, so he wanted to be in a pleasant atmosphere within driving distance from his mother. He was there the same year as Pitman.

Beran/Fisher: It seems that several people wrote their books there.

Watson: That's right. Rupert did *Multiple Comparisons* and I was able to help there because I'd worked through Tukey's enormous manuscript while in Melbourne! Rao was working on *Linear Statistical Inference*, and Ted Hannan wrote the *Multiple Time Series* book there.

Beran/Fisher: It must have been a good environment.

Watson: You, Rudy, saw it more objectively probably than I did. But I was very successful in getting these visitors because they were almost all from overseas. And it was fairly easy to raise the salary from the government.

Beran/Fisher: As I recall there was some grant that was starting up.

Watson: I can't remember all the grants, but money was not a serious problem. We were very small, so without classy visitors we were nothing.

Beran/Fisher: After this brilliant start, the department basically collapsed. What happened in the university?

Watson: I left, but I seem to be the kiss of death for statistics departments!

Beran/Fisher: It was probably more complicated.

Watson: It *was* more complicated. I went off on leave to Italy for a year and while I was there I had a bunch of job offers—because of all this success, everybody thought I was chairman of the year. One was from Princeton, so I flew back and checked it out. After trials and tribulations and changes of mind I decided to come to Princeton, but when I left, Hopkins decided they would roll Statistics into Operations research and call it Mathematical Sciences; and now that's also disappeared. But there weren't that many people involved. Dick Jones had already gone to Colorado. Leon Gleser and Joe Gastwirth were no doubt the only people there. There was not much interaction with Biostatistics as you recall. Unfortunately that stopped totally when I left. Biostatistics is now a very good department.

Beran/Fisher: I recall that you visited Stanford Linear Accelerator Center during this period.

Watson: They invited me out when money was no object! A car and a glorious house on the edge of the campus with a swimming pool. I would drive up to SLAC every day and sit in a horrid office in a trailer. I was taken to see this great accelerator and the only other thing I remember is that they had a computer terminal that showed two-dimensional histograms. It had a picture which you could rotate.

Beran/Fisher: They had a very advanced computer.

Watson: It was really very staggering. But I again made the great mistake that I felt I had to learn all about particle physics before I would know what to do with collision data. So I began to learn quantum mechanics and ignored all these research possibilities and marvelous databases. I gave some lectures in the Statistics Department.

Beran/Fisher: You made a tantalizing reference to Bayesians and Bayesian statistics a little while ago. Can you be drawn a little bit more?

Watson: That's right. I gave a course at Hopkins. I was writing Bayesian papers. Everybody was talking about it. It's fun doing the computations. It seemed to me, and still does, that there are a lot of problems that are insoluble with the standard frequentist formulation, but they would not be problems at all if one took the Bayesian point of view. And so it was. I think it was just a general interest. I can't stand theological Bayesians or theological anything. I'm a great admirer of Dennis Lindley, but he has this feeling that he has to have a complete logical structure—politically, socially, statistically—and I don't feel the need for that. I would rather trust my common sense.

Beran/Fisher: Are you saying you like treating a problem on its merits?

Watson: I suppose so. I mentioned the paper on optimal estimation I did in 1983 for the ISI. I was talking about Pitman estimators. They agree with Bayesian estimators. For anything I do I like to have the protection of it making sense no matter which theory you take. It's a bit like my confidence cone on a sphere. Mine comes from the first term of a series. So does Fisher's. Though the series are different, their first terms are the same. I find that very cheering. Scientists don't give a damn as long as it makes sense.

PRINCETON

Beran/Fisher: Let's talk about Princeton.

Watson: We had a lot of fun in the early days and a lot of visitors. It began with the Robustness Year: Huber, Hampel, Bickel, and others. The rise of computing was really quite exciting.

Beran/Fisher: Princeton was relatively well-equipped?

Watson: Yes, we were ahead of most statistics departments. We bought a PDP-11 in the 1974–75 academic year, largely due to pressure from Don McNeil. The money came mostly from grants and the Wilks Fund. The Mathematics Department opposed putting any machine in Fine Hall! But it was a math major, Jeff Rottman, who got the PDP going. Then Rottman and David Donoho developed ISP under an early version of UNIX. Finding money for maintenance and upgrades became a big burden. To save money, we had no service contract, so we were at the end of the queue which meant we wasted operating time. Don returned to Australia and Peter Bloomfield took over. Allan Wilks came as a graduate student in 1976 and soon took charge.

The PDP and later machines were only used for research and for generating and writing theses—and that was a real novelty in those days—and for some graduate classes. For undergraduate courses we had to use the Computing Center's IBM mainframe. The period 1975–85 was exciting, the coming of age of statistical computing, and the department was at the forefront of it.

People talk about undergraduates. We really had a crack undergraduate program. It turned out a lot of people who became statisticians. I was always very proud of that. Watson's Law says that we would get about six or so stat majors every year. If you forced it, you could get it up to ten but then there would be a few who weren't quite so dedicated. Everyone wants to make graduate students into academic statisticians. I think it's pretty good to turn undergraduates into graduate students who go on to become professors. There are quite a few of them. But we could never compete with the big departments when it came to graduate teaching of major courses. We could only do our specialties plus use visitors often for the standard fare, meat and vegetable courses. In that respect Tony Pakes was fantastic. He covered so much ground in his courses. He was absolutely limp at the end, but says it was the best year of his life!

Beran/Fisher: Why is it that the Department of Statistics no longer exists?

Watson: I think that one of the things that probably made it inevitable was just aging. The university could see John Tukey was coming up for retirement and that I'd be retiring not too many years afterwards. And we had all this space in Fine Hall, which was precious. I made it clear in my annual reports that there was no way Princeton could now have a good Statistics Department unless they had four well-known professors. The administration showed no interest in putting up more money.

Beran/Fisher: Can you isolate why they had this lack of interest?

Watson: They saw that computational and applied mathematics was hot. There were big pressures to build that group and they in fact inherited all of our resources. Another major reason for our demise was that the other departments would never give up courses because there's money to be had in the big introductory courses. Civil engineering, economics, sociology, psychology, and others all had established such courses when I arrived. When I first came, the president said, "Don't worry about those things. You just have a good undergraduate and major graduate program. That's all we want." But as financial pressures come along, that's not enough.

Beran/Fisher: There were some other statisticians around on the campus, Stu Hunter for example?

Watson: Stu was teaching in Civil Engineering. As soon as I came I invited him to come to stats to be head of undergraduate studies, but he refused. He was a very mesmerizing lecturer. I also think we cut our throats a bit with some of the courses because we tried to introduce computing in them. We got a chance to do that with Stu's course and chose Allan Wilks. The students hated this more demanding course.

Beran/Fisher: That's one of the big changes. Now it's students who demand that we provide a good computing environment! Getting back to the demise of the department, were external reviewers consulted?

Watson: I tried without any success to get help from external reviewers. Even though basically I agree with John—we have the same motivations—on many things he and I didn't agree. For example he is very anti-mathematical though clearly talented mathematically. That was difficult. It was just two people and you have to vote!

FIG. 1. *From left to right: P. A. P. Moran, E. J. Hannan, G. S. Watson and G. A. Watterson in 1957 outside University House, Australian National University.*

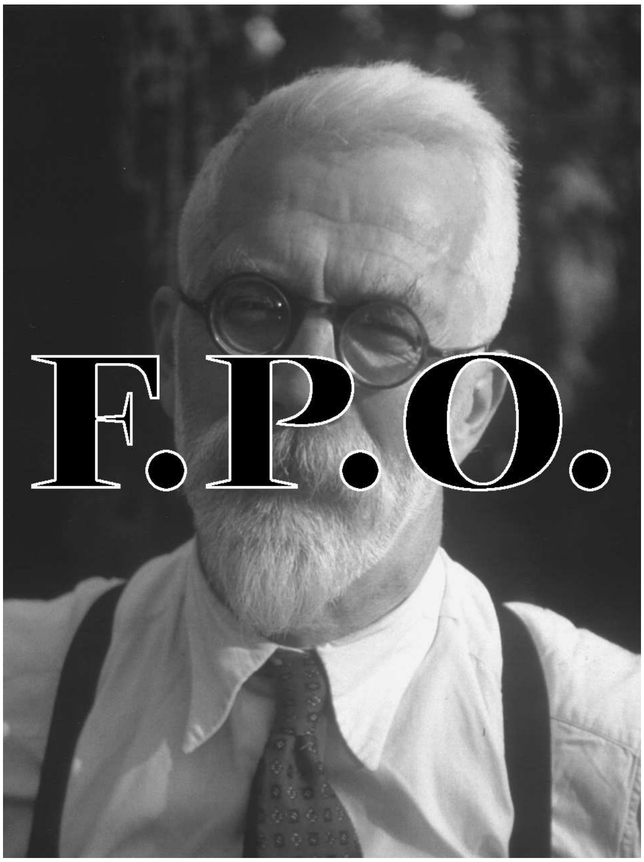

FIG. 2. *R. A. Fisher in 1953, photographed by G. S. Watson on a picnic near Melbourne.*

FIG. 3. *G. S. Watson lecturing in 1980 on "The Key to Kato" in Fine Hall, Princeton University.*

FIG. 4. *G. S. Watson, photographed by R. J. Beran during the interview on his 75th birthday in his Fine Hall office, Princeton University.*

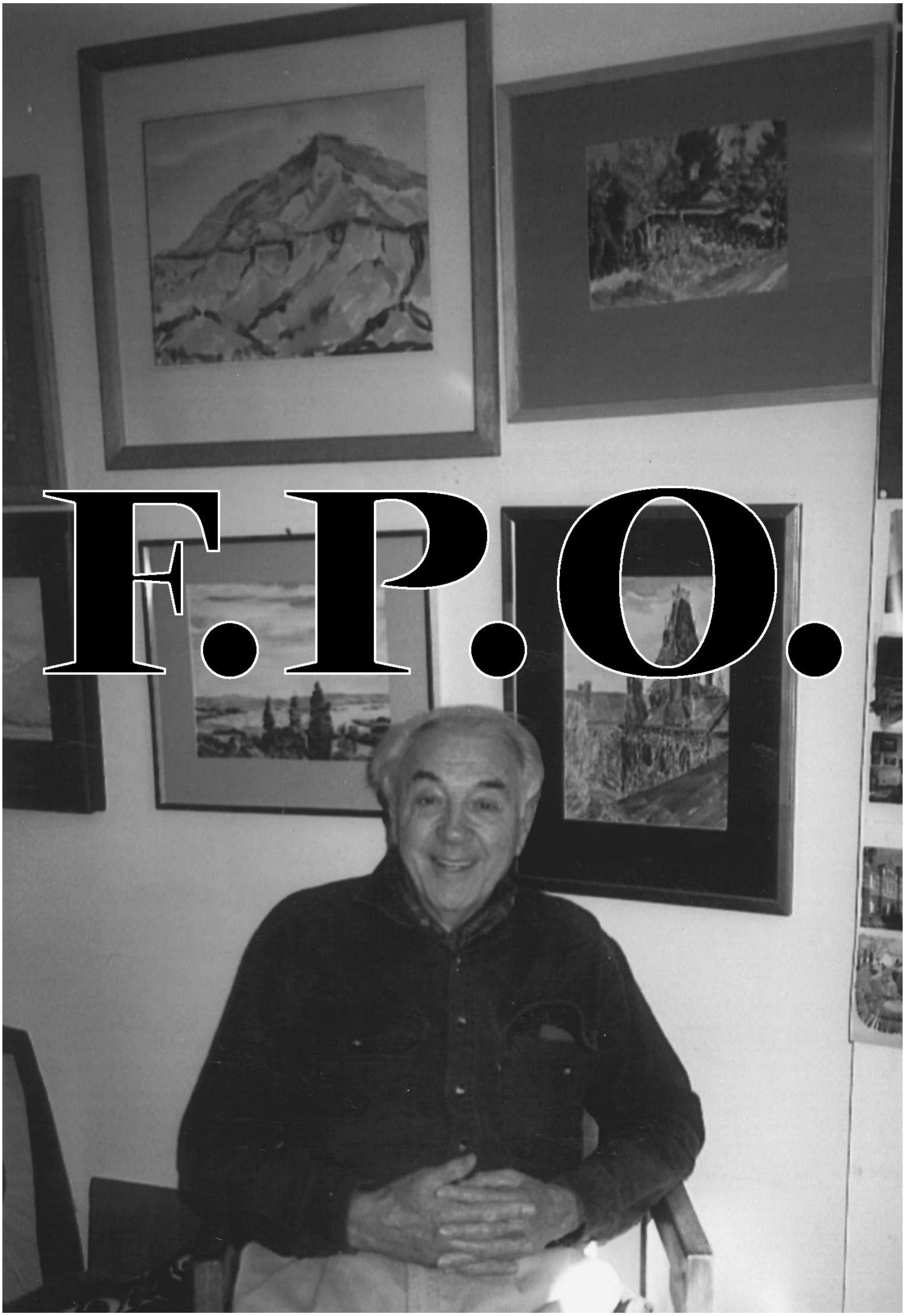

FIG. 5. *G. S. Watson, photographed by N. I. Fisher at home in his study after the interview.*

Beran/Fisher: Are there other Princeton memories?

Watson: I think we spent far too much time on the statistics of public issues. There's a moral in this that I won't try to spell out. The big contracts that we had on stratospheric ozone, U.S. oil and gas reserves and environmental issues took control of the department and deflected us away from our usual research. Also, I spent a lot of time away from Princeton on Washington committees dealing with these issues, to the detriment of my personal work. So did John, but he could handle it better, and he didn't have any administrative duties.

But let me return to my 1958–59 visit to Princeton, when I gave a course on time series in Fine Hall. Guess who sat in the front row? Bochner.

Beran/Fisher: Bochner?

Watson: I nearly died. I barely understood what his theorem meant.

Beran/Fisher: Did you mention it in your course?

Watson: No. But the only person I talked to technically in that period was Bochner. He was immensely helpful. I was doing a paper on the joint distribution of serial correlation coefficients and so I had to do contour integration in many dimensions. I was getting a bit muddled. I remember going several times and chatting to him and he gradually clarified my thoughts. He was at loggerheads with some of the people in his department. To me he was charming.

EUROPE

Beran/Fisher: You slipped across to Europe for a while. How did you first come across the work that George Matheron was doing in France?

Watson: There was a meeting in Kansas at the Geological Survey on statistical topics and I was invited to go there and who was there but these two Frenchmen: Jean Serra, who could speak quite good English, and Matheron who could understand but couldn't speak. So I started chatting to them and found two exciting things: Serra's image analysis stuff, mathematical morphology, the mathematics of which was initially done by Matheron; and the geostatistical stuff. For a while I thought geostatistics was trivial and then I started thinking about it and I realized I was wrong. I thought I would like to find out more and they very kindly started inviting me to spend time with them, which I did. I got to know the Matheron family very well. I lived in Fontainebleau, went to the lab. But all I did really was write papers to get other people interested. Matheron and Serra had an army of devoted and very technically well-advanced helpers but for some reason or other none of them went on to advance the theory.

Beran/Fisher: You also went to Italy.

Watson: I spent a year in the Genetics Institute where the director was Luigi Cavalli-Sforza.

Beran/Fisher: Did you interact with him directly?

Watson: Only socially because he went on leave to Stanford and never came back. I had his office. He had spent time in Cambridge with Fisher working on genetical problems, but he had a basic feel for statistics. He was a very clever chap. You could claim that he should have shared the Nobel Prize. He was doing the same sort of work as Lederberg. The sexual stuff about bacteria. When I knew him, he loved going to Africa to study Pygmies, taking blood samples and tracing genes and so on. He did a lot of work on the historical records in Italy, which is one thing there that seems to be very well organized. Everybody's Catholic and they write down their names and so on in churches where they get married. It's quite easy to trace family trees and so trace genes.

When I was in Pavia, there was a conference. Ole Barndorff-Nielsen and several Scandinavians came and they obviously wanted to get something like this going in Aarhus. So they asked me to come at the end of my time in Italy, in the summer. I remember my family saying, "Denmark, how boring. Let's go to the Alps or some really exotic place." But I liked the Danes personally and wanted to give it a try and I said: "Look we're living in a medieval town in a medieval corner and so on. If we go to Denmark, it will be a sort of half-way house to getting used to going back to modern America." We got to Denmark and it was like going into the 21st century! Coming back here was like going to Sicily. It was a shock. I got to know them, particularly Ole Barndorff-Nielsen. He was a dominating figure, of course. Ole has a real feeling for science, a love of science. He has trained many good people. He had a lot of ongoing interests. I thought it was a beautiful blend of statistics, math, and general science. The last time I was there he had a used jet engine from the Danish Airforce: they probably only had two. He had set up a little wind tunnel in the basement and he was blowing sand around and looking at the ripples. Since Denmark is just a giant sandbar, it is very relevant work there. I went back many times.

Beran/Fisher: Is there any work of your own associated with Aarhus?

Watson: I think I learnt a bit there. I finally got rather intimidated, they were all very mathematical. I remember sitting in an office writing away but I can't remember what I was writing at the time.

> **Watson:** I gave a math course to the John Curtin School of Medical Research at the ANU. That's when somebody said: "Geoff may be a born teacher but he needs a born student."
> **Beran/Fisher:** Who said that?
> **Watson:** Cedric Mimms, a microbiologist.

I don't think anything I've done is closely related to what they were doing at that time. Later on I had a student, Javier Cabrera, write a thesis that involved hyperbolic distributions. That was much later. Maybe that came from Aarhus.

AUSTRALIAN STATISTICS

Beran/Fisher: You've interacted with some of the more remarkable Australian statisticians, such as Alan James. You've known Alan quite a while?

Watson: Yes. He's one of my great heroes. I didn't actually know him until I was in Canberra and we had him over. Ted Hannan probably urged us to invite him up to Canberra. Ted was very keen on group theory, algebraic things. I remember one day Ted was saying to Pat Moran: "It's impossible to understand this subject unless you understand the group structure" and Pat growled! He understood it very well without. Alan gave some very nice talks on group representations. I think Alan is one of the greats, a Pitman-like figure with a few beautiful papers. You couldn't alter a word and improve them. He's never been very ambitious—a very modest man.

Beran/Fisher: And you've kept in touch with him over the years?

Watson: Absolutely. I knew his wife Cynthia a long time before he did. She was a fellow student.

Beran/Fisher: Maybe we can track down some of the other Australian statisticians that you knew, such as Pat Moran?

Watson: Pat I knew very well. I first met him when he rang and asked me if I would be interested in coming up from Melbourne to the ANU. I answered with a resounding "Yes." We worked closely together for three or four fruitful years.

Beran/Fisher: Did you actually collaborate with him?

Watson: No, I never wrote a paper with him. We were slightly competitive. He had very much the same interests. He was very interested in science. He liked to find out things and put them together which he did again and again. He was very close to Stephen Fazekas and I was very close to John Cairns so we were slightly competitive in the microbiological area. I liked Pat very much and admired him very much. A really cunning guy. Some great two- or three-page papers—for example, one on estimation of birth–death processes.

Beran/Fisher: He was very encouraging with young people too.

Watson: He was *very* good with students and he had many very good ones. When I arrived at the ANU in 1954, Ted Hannan was a research fellow. Ted was actually writing his thesis with Pat. I think Joe Gani was finishing his thesis on Pat's theory of dams. One of the reasons Pat hired me was because I shared his interest in dams and water. I learned about it from an engineer called Jeff Alexander. It was Jeff Alexander who wrote to Feller about this phenomenon with the Nile flows. Feller wrote a paper in the *Annals* explaining the phenomenon. I gave a lecture at a conference on my theory of dams. I took the wrong strategy. Pat made it discrete. I tried to be continuous in time and got lost.

You asked me about various Australians. Evan Williams I knew very well. In fact while I was an undergraduate, I worked for him one summer. He was at the CSIRO Forest Products Laboratory. They were testing Australian timbers for building houses and so on, to find different kinds of breaking strengths, so we would do regressions. It was in the days of hand-cranked calculators. Evan said it took years to find all my arithmetical errors. It's a wonder Australian houses didn't fall down, using these strengths. I got to know him and through him, Pitman, because my parents were then in Hobart. Williams was a student of Pitman's. So when I went over to Hobart I looked up Pitman. In fact E. J. Williams is our son Michael's godfather.

Beran/Fisher: You kept in touch with Ted Hannan all his life?

Watson: Yes.

Beran/Fisher: Did you do any work with him?

Watson: Yes, we had a joint paper. In Part II of "Serial Correlation and Regression Analysis" (Watson and Hannan, 1956). I had all these results done in a discrete way. I knew that the smart thing would be to do the asymptotic stuff and so Ted did it. Ted's writings are very narrow. It was always time series. But he had a very wide mathematical and statistical knowledge. The most loved of Australian statisticians.

Beran/Fisher: Was he interested in the scientific side of things?

Watson: No interest whatsoever in natural science. He was a very literary man. He had quite an interest in economics. But I don't think anything he did was ever motivated by economics.

Beran/Fisher: Did Imre Binet have any influence on you? He seemed a rather tragic figure in later years in statistics when he turned up to meetings of the ISI and asked long, rambling questions, but he really was a brilliant and unusual man and in his early days you had a chance to work with him.

Watson: He was there when I came back from Cambridge and went to Melbourne. In the three years I was in Melbourne, I learnt more from Imre than anybody else there. He was a terrific intellectual, he seemed to know everything about everything and in particular he taught me the elements of genetics. He didn't have much mathematical knowledge, but he had a feel for it. He was very interested in medical problems—he'd done a lot of medical consulting, even medical training. Much of that was multivariate analysis and he brought things that he would like to do to my attention. In research I learnt much from him, so I'm really grateful. He was a tragic figure, tragedy after tragedy all his life.

CATCHING THE WAVE

Beran/Fisher: Looking back, any general observations?

Watson: Looking forward, I have to think: how many years have I got left? How do I want to spend them? Do I want to paint? Do I want to do this?

Beran/Fisher: Is there inside Geoff Watson a different person trying to get out? Given your druthers, would you like to modify your mixture of talents? How would you modify them?

Watson: I've missed so many opportunities, done so many stupid things. Not written up things that I did. I think somehow or other I was destined to become an academic or research person in the general area I'm in: the center of applied mathematics. I wasn't very good at maths at school. Years later, I went to a high school reunion in Bendigo. My photo receiving a D.Sc. had been in a newspaper, and this angry woman came up to me and said: "I was better than you were at maths." She was right.

Beran/Fisher: So it's the interplay between maths and science?

Watson: I like all scientific things. I like painting and drawing. Statistics was a fluky sort of thing. The Math Department at Melbourne sent me away to learn it! I had a chance to go to into aerodynamics, fluid dynamics, and so on. I still love that field and sometimes regret I didn't go that way.

Beran/Fisher: Linear models have been a long-term interest. What do you see as your significant contribution in that area?

Watson: The thing that jumps to mind is: wherever I go, somebody says, "Ah, the Durbin–Watson statistic." My children's friends take an elementary course in economics or econometrics and they know this damn thing. While I think they're two very good papers, they didn't really solve a lot of problems. What do I do if I have a regression and find the errors don't survive the Durbin–Watson test? What do I actually do? There is no robust method. You'd like to use a procedure that would be robust against errors no matter what the covariance matrix is. Most robustness talk is really about outliers, long-tail robustness. Dependence robustness is largely untouched. That's the way it's been all my career. If you read the last chapter of my thesis you see how defeatist I was—"it's a mathematical exercise" conclusion.

Beran/Fisher: You did come back to the Durbin–Watson statistic during the Hopkins era. That's when your interest in time series analysis . . .

Watson: . . . came back a bit? Yes. Ted Hannan had an effect on me there. I didn't understand Hilbert spaces so I had to do everything in the finite-dimensional case. There are two kinds of approximations. There are my approximations in the finite-dimensional case and the asymptotic stuff, which is equally an approximation but a lot nicer and it's a clearer way to think. But I never made any contributions to spectrum analysis.

Beran/Fisher: If you were 45 now and not 75, knowing what you do know, which areas would you be looking to get into? Molecular biology still? Earth sciences still?

Watson: The thing is you've got to catch the wave. I was really lucky in catching the wave with paleomagnetism. I had the excitement of the DNA story but I just couldn't contribute. The computing business I guess I missed. When I was in applied economics in Cambridge, there was this little digital computer and the students' legs were sticking out the back—all vacuum tubes and wires and punched hole tape.

Beran/Fisher: Lots of vacuum tubes?

Watson: Yes, it's where the reliability problem first came up! I had to multiply something like 20 power series together. Another time Jim Durbin and I had 10 equations and 10 unknowns and the analog computer did it. There were flashing lights, and as the iterations converged, less and less flashing lights. I remember people were saying, "Now we'll be able to get the eigenvectors" or other things we'd been writing about, but they didn't talk about changing the way of thinking. That change didn't come for a long time. So all that's a great wave which it would have been nice to have ridden. I don't know where the next waves are going to come from.

Beran/Fisher: The computing wave probably isn't over.

Watson: That was a train of waves, really. I was looking for scientific waves. If you stay within statistics then you've got to be better theoretically than the next person, improving somebody's theory. You're just doctoring up a theory. That's where I think Neyman–Pearsonism drove itself into a sterile tunnel.

Beran/Fisher: It died in the 1960s, basically.

Watson: Yes. The main results had been around for so long. You've got to know when to get off, try another wave. It's timing. There was a period when I was young, in my 30's, when there was a kind of steady state. There was a continuous input from one external source through to another and theory was roughly keeping pace within the Fisher paradigm. Now everything happens faster.

Beran/Fisher: How do you view recent work on smoothing techniques?

Watson: It's still very limited.

Beran/Fisher: A lot of the work on classical multivariate analysis seemed to fall into disuse when modern smoothing techniques came along. Do you see the classical multivariate work suddenly becoming more relevant than it is at the moment?

Watson: By classical multivariate analysis you mean Ted Anderson first edition type of stuff? A lot of people did it and it provided a great deal of fun. But then Alan James came along and looked at the whole thing from a different point of view, a much more powerful and insightful point of view, where things just drop out. So you get those kind of advances. But it comes to an end after a while.

Beran/Fisher: Let me ask a very general question. Statistics is a very ancient subject. It goes back to the Babylonians and earlier and it's really only in the last century or two that people have decided to associate probability analysis with statistics—to evaluate statistical procedures under probability models. Do you see any other logical structures for evaluating statistical methods, not necessarily related to probability?

Watson: What I was just saying, it's still all in the mind. You're inventing other possible worlds to compare with the one you have and it's just a mental model. It's just a way of saying that things vary. Perhaps someone will think of another way of generating alternative worlds. Although I don't think much of the Benzecri school, they take an extreme view of this point. Quite interesting.

I don't know whether computer science has ever suggested any "class of worlds." They have always hammered on about speed and accuracy. Having no theology, they attack every problem by common sense. Either they look for new challenges or people now go to them instead of to us. Anyway they seem to have got into many things before statisticians. They don't fight among themselves like statisticians do, which must really put off potential users of statistics! If I haven't said so earlier, I think that too much statistical teaching is bad for the brain. Classic and modern computational methods are often linked with beautiful mathematics. They have always attracted great mathematicians such as Gauss. Of course the power of computers has allowed the collection and contemplation of big data sets so the special new computer science skills are essential. Now it's safe to say these obvious things at ASA/IMS meetings. In the old days it would have drawn scorn from the mathematical end of the trade. But has teaching changed to encompass these beliefs? At the top end I think it may have.

CODA

Beran/Fisher: If you had to save half a dozen of your reprints, which ones would you save? Not to take to a desert island, of course.

Watson: Actually, I'd save as many of my paintings as I could, not my reprints! I love them much more. I think the thing I get the most enjoyment in explaining is the geometry of the Fisher distribution on the sphere, the large κ approximations, dreaming up those analysis of variance analogs. I think that's the cleverest thing I've done. It's so painfully obvious when you think about it. It just falls out. I did actually write it all down before proving it. It is a basic way of thinking. You're talking about variability, within and between. That's what most statistics is about. You have to define what you mean by variability on a sphere, dispersion on a sphere, dispersion on some funny manifold. I would have taken it mathematically further but the modern literature was so hard to read in the abstract. Fortunately it's now been done, so I don't need to.

Beran/Fisher: The really serendipitous thing for your large κ approximations is that most of the spherical data sets tend to have κ bigger than 2, and the approximations work. On the circle, many data sets aren't like this.

Watson: If I'd started off on a circle my life might have been very different? I never quite thought of it in those lines, but you're absolutely right. Bird navigation studies have not caused a scientific revolution while paleomagnetism certainly did. Even so, my geometry makes sense almost regardless of κ.

EPILOGUE

With Geoff's consent, we submitted this Conversation to *Statistical Science* in October 1997. On January 4, 1998, we received the following message from his son, Michael:

"Geoffrey Watson died peacefully in his sleep on Saturday, January 3rd, four weeks to the day after suffering a heart attack. His wife Shirley was by his side. After a quadruple bypass operation on December 8th, he never recovered sufficiently to leave the intensive care unit of the hospital of the University of Pennsylvania. In the last two weeks he drifted in and out of consciousness, but there were many periods of clarity when he knew which of us was with him. Unable to speak because of the breathing apparatus, he communicated his feelings with a characteristic rolling of his eyes. Whatever else was failing, his sense of humor was unimpaired.

His four children were all able to fly to America and see him one last time, which was a great comfort to us all. The sudden deterioration in his condition in the last two days caught us off guard—his children had all flown back overseas when he underwent emergency surgery yet again. Madeleine bravely crossed the Atlantic for a second time in one week to be with him when he died. Rebecca is flying down today from Toronto. Although Cathy and Michael are unable to return from Africa and Japan, their thoughts are with the rest of the family.

There will be no funeral ceremony. Geoffrey's wish was always to be cremated simply and without fuss."

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