

A Conversation with Norman L. Johnson

Campbell B. Read

Abstract. Norman Lloyd Johnson was born on January 9, 1917, in Ilford, Essex, just east of London, England. He received degrees at University College London in 1936 (B.Sc. in Mathematics) and 1937/38 (B.Sc. and M.Sc. in Statistics). In 1938, at age 21, he was appointed Assistant Lecturer in the Department of Statistics at UCL. During World War II, he served under Egon Pearson as Experimental Officer with the Ordnance Board.

He returned in 1945 to the Statistics Department at University College London and remained there until 1962, as Assistant Lecturer, Lecturer and then Reader. In 1948 he obtained his doctorate in Statistics at UCL, based on his work on the Johnson system of frequency curves. In the following year he became a Fellow of the Institute of Actuaries in London. Two visiting appointments in the United States, at the University of North Carolina (UNC) at Chapel Hill in 1952–1953 and at Case Institute of Technology in Cleveland, Ohio, in 1960–1961, led to his permanent appointment as Professor in the Department of Statistics at UNC in the Fall of 1962. He served as Chairman 1971–1976 and officially retired in 1982, but has continued to be active in scholarship and research as Professor Emeritus.

Norman Johnson is the author or co-author of 17 books, including two textbooks, each in two volumes, and the *Distributions in Statistics* series initiated with Sam Kotz, 1969–1972, which in its second edition will have run to six volumes. Along with Sam Kotz he has been Editor of 14 books, including the 10-volume *Encyclopedia of Statistical Sciences* and the three-volume *Breakthroughs in Statistics* series, all of which are invaluable sources of information for students, teachers and researchers. He is author of more than 180 papers, monographs and tables, many appearing in journals such as *Biometrika*, *The Annals of Mathematical Statistics* and *Journal of the American Statistical Association*. His co-authors include B. L. Welch, Egon Pearson, Florence David, Fred Leone, Harry Smith, Jr., I. W. Burr, James Grizzle, A. W. Kemp, N. Balakrishnan and his wife Regina Elandt-Johnson. He is a Fellow and recipient of the Wilks Award of the American Statistical Association (ASA) and recipient of the Shewhart Medal, American Society for Quality Control. He is featured in the 1995 ASA videotape “An Interview with Norman L. Johnson.”

Norman L. Johnson died in Chapel Hill November 18, 2004.

This conversation took place in Peabody Hall, University of North Carolina at Chapel Hill, in May 2002.

Campbell B. Read is Professor Emeritus, Department of Statistical Science, Southern Methodist University, P.O. Box 750332, Dallas, Texas 75275-0332, USA (e-mail: cread@smu.edu).

Read: Well, Norman, it is a delight for me to be back in Chapel Hill after so many years and to have this conversation with you.

Johnson: It is a delight for us to see you again, Campbell.

Read: Thank you, Norman. Let's go back to your very early days. I notice that you were born and grew up in Ilford, Essex, in England.

Johnson: Yes, 10 miles east of central London.

Read: And what was your father's occupation?

Johnson: My father worked for a paint and glass firm; he was an export manager. As a result, I got a lot of foreign stamps from places all over the world, like Sierra Leone and the United States, of course. They supplied some of the stained glass windows in Duke University Chapel. The company was George Farmiloe and Sons. I can't say that he directly employed mathematics at all, except that he had to do a lot of numerical work in his occupation. My mother was from a Welsh family; her grandparents had come looking for work in the middle 1800s and her name was Lloyd, so I am half Welsh. I attended a county high school in Ilford from 1927–1934. In 1932 I matriculated and in 1934 I took the Higher Schools Examination, on the basis of which I got a state scholarship to go to University College in London.

Read: So, what was the year that you went as a freshman to University College?

Johnson: I went in 1934 as an undergraduate in mathematics. As I had already taken the intermediate examination in 1933, I only needed two years to get to the final special B.Sc. degree in mathematics in 1936, when I was 19. To complete residency requirements, I had to stay one more year, studying "something." Then I had a conversation with a fellow student, David Bishop. Because an earlier student from his school in Kingston-on-Thames had gone into statistics, he was going into statistics and I sort of said to him, "What's that?" and he showed me the department he was going to. I thought that would be a nice change from mathematics, so I went to the Statistics Department in 1936.

Going back a little bit, I may have been encouraged (as a child) and shown some tendency in that direction, because we used to visit my grandmother, my mother's mother, where they lived on the main road in Ilford. From their house, one could see the traffic on the roadway, and to keep me quiet when I was a small boy, they would put me in the window with a large sheet of paper and I would take a count of all the vehicles, trams, cars, buses, bicycles, going in each direction, with times, and I would draw pictures showing when there were the biggest densities of travel. Now, I don't know if that encouraged me to be a statistician or if it was something that was there already.

Read: So you were an early collector of data for some kind of Poisson or semi-Poisson process.

Johnson: I didn't know that then.

Read: I imagine you specialized in mathematics in high school.

Johnson: For mathematics, I took group D, which at that time in the high schools' University of London examinations meant science. There was a group C called "mathematics" which was all mathematics, but the school did not encourage us to do that. I think the mathematics masters didn't like the amount of work it would involve. The material was much more advanced. I took four subjects: pure mathematics, applied mathematics, physics and chemistry. I got a state scholarship because I got "Distinction" in all four of these. (It took me a lot of hard work to get "Distinction" in chemistry! Not my favorite.)

Anyway, in University College London at that time you didn't have to take subsidiary subjects, which we call "minors" in the United States, with a mathematics major. I guess this was because there was a Department of Pure Mathematics and a Department of Applied Mathematics and they decided that that was enough. So I just had pure and applied mathematics for two years.

Read: With no courses in probability or statistics?

Johnson: No, not at that time. Statistics wasn't a well-accepted subject in graduate mathematics courses. I didn't know much about probability or statistics except that it was different from mathematics.

Read: So when you went over in 1936 to study statistics, that was your first contact with the faculty and the Department.

Johnson: Yes, they were in a separate building close to Gower Street, while Mathematics were in the main building, which was set well back from there. Karl Pearson had died that previous spring, having retired in 1933. I had seen him around in the University, people had pointed him out, but I never conversed with him. Egon Pearson had just taken over from him and so at that time all potential new students had an interview with Egon Pearson. The Department was not big enough to have a special person to interview new students. So that was who I saw first. The other people on the faculty were Florence David, who was quite young, with two years as an assistant professor, Bernard Welch and P. L. Hsu, who was a visitor from China. They hoped he would be permanent; he wasn't in fact. They were the whole staff.

In statistics you had papers on finite differences and interpolation, which formed a kind of mathematics "minor." But in a mathematics course you didn't learn much about finite differences or interpolation. Those subjects were not very respectable.

Read: No, but I remember the courses I had from you here at Chapel Hill in the 1960s in which you used

finite differences quite frequently with discrete probabilities.

Johnson: Oh yes, that was a result of what you will see in a moment. Being very new, in the first courses I gave in “statistics,” I had to teach just interpolation and finite differences.

In 1937 I took the B.Sc. in Statistics and in 1937–1938 I was working on a Master’s degree with a thesis, under the direction of Professor Jerzy Neyman. He came to the University in 1936–1937. He was Polish, and very fortunately, I think, Professor Pearson assigned me to him.

Read: That must have been quite an experience.

Johnson: Yes, working under his direction resulted in a paper “Parabolic Test for Linkage” (Johnson, 1940). I must have impressed him in 1937–1938, because half-way through the year, when they realized that P. L. Hsu was going, Professor Pearson asked me if I would like to join the faculty as an assistant lecturer. Seeing as I was only 21, I thought I was very lucky to get something like a tenure track offer at such a young age.

Read: So you had one year as an assistant lecturer before the war broke out.

Johnson: Yes. I had not got beyond thinking of what I was going to do for my Ph.D., but I did write with Dr. Welch our paper “Applications of the Non-Central t -Distribution” (Johnson and Welch, 1940).

Read: I notice that your first two publications were with Dr. Welch.

Johnson: The other was a very simple one on calculating the cumulants of the distribution of chi, at that time something new (Johnson and Welch, 1939). The work we did on noncentral t was remarkable in that the calculations were all done on hand-operated Brunsviga calculators; there were not even electric ones.

Read: I remember those. Did you take courses from Florence David before the war?

Johnson: Yes, I took a probability course and she covered some elementary genetics as well. Egon Pearson taught mostly statistical inference, and, of course, there was a lot of emphasis at that time on power functions and confidence intervals. Professor Neyman’s lectures were rather more advanced; I got more of those in the second year when I was working toward the Master’s degree under his direction, but his lectures on confidence intervals were more advanced. He was particularly interested in what we now call survival functions. I also went to courses in the London School of Tropical Medicine and Hygiene. These were on life tables and things like that, taught by Bradford Hill. These courses



FIG. 1. D. Newman, B. L. Welch, Miss Francis, David Bishop, Jerzy Neyman, Egon Pearson, Norman Johnson and an unidentified individual, at University College circa 1938. By kind permission of University College London.

were part of our studies. I wasn’t being overly energetic; you just had to walk 200 yards down the road.

Read: I noticed that around 1936 Pearson published a monograph on industrial standardization and quality control. I wondered if your interest in quality control was connected with him at all. Do you get started on this before the war?

Johnson: Oh yes. Pearson was working with B. P. Dudding at General Electric. Because of that I got some Russian publications on quality control, later on, that Professor Pearson wanted translated into English, and I did that too. I don’t know if it was a very good translation.

Read: You could read Russian then.

Johnson: I did learn some Russian during the war. There was nobody else around who both knew some Russian and whom he could tell that he wanted it done. I did develop an interest in quality control at the time, and Dr. Welch was interested in it also.

Read: You mentioned coursework on life tables also before the war. Were you developing an interest in actuarial statistics also?

Johnson: Yes, I started to take the earlier examinations of the actuaries before the war started, because I did not yet know what my career would be. Some friends of ours were related to actuaries and knew the sort of things they did. I went to the Actuarial Tuition Service toward the end of the war, but I had already taken some of the examinations. The early examinations were really on mathematics and finite differences.

Read: Did you ever have occasion to meet R. A. Fisher?

Johnson: I met him once. Of course, I went to his lectures after the war. As the youngest member of the faculty in 1938–1939, I didn't get a room to myself. Being most junior, I got a large corner of the laboratory on the top floor which was mostly Dr. Fisher's Genetics Department. I was also talking to Welch at this time. The Behrens–Fisher test tends to use fiducial probability, not real probability, and I noticed and talked about this with Dr. Welch. You didn't get the right frequency of decisions that you would think from probabilities. Thinking in a way that I was young and brash (as indeed I was), he said, "Why don't you go and tell Fisher that he's got it wrong?"

Read: Was he setting you up?

Johnson: Well, I only had to cross the corridor to see Fisher, so I thought I would go and tell him. Being young as I was at the time, I was unaware that he had a reputation for abruptness and dislike of criticism.

I said, "Hello, Professor Fisher, may I speak to you for a minute?" He said, "Oh yes. Oh yes." I showed him the paper and said, "You know, these tests don't have the right frequencies of decisions, or in the case of confidence intervals, a conclusion, because these fiducial probabilities are not really the same, always, as regular probabilities." And he said to me, "I know that." That was my total conversation with Dr. Fisher. I thought afterwards that I was lucky to get away with my life, really.

Read: And he didn't say anymore than that.

Johnson: No, just, "Yes, I know that. Goodbye."

WAR YEARS (1939–1945)

Read: Let's talk about the war then. I know that Egon Pearson was the head of a group of statisticians working on weapons assessment with the Ordnance Board. Were you a member of that team?

Johnson: Oh yes, right from the start. We all went, in fact, Professor Pearson, of course, initially Drs. David and Welch and myself and David Bishop who had first tempted me into statistics. He wasn't interested in

an academic career and eventually he went into business. Later in the war we had an additional person and Dr. David went to the Ministry of Home Security to do air raid analysis and so on. Donald Mills, who had just taken his degree during the war in 1941–1942, came to join us. He was my assistant during the rest of the war.

Read: Did the group under Professor Pearson during the war work on any interesting statistical problems?

Johnson: "Weapons assessment" was a broad title, but there are some limitations on what I can say in this area because of the Official Secrets Act. But I can speak in broad terms. We were specifically working on terminal ballistics, which includes, for example: What happens when an antiaircraft shell explodes near an aircraft? How does it do damage, and how can you tell beforehand how much damage it is going to do from the results of trials performed on the ground? They would explode a shell within a circle of boards on the ground and observe the damage done to the boards. We could get distributions on those by doing some three-dimensional geometry and with some trivariate distributions which we supposed and hoped were normal with no bias errors. We got measurement of error from cameras within planes in raids over Germany, for example. We could work out from that what would be the probability of what would be considered a lethal strike on the airplane. At that time, as I said before, we had no electric machines; we didn't have anything much better than Brunsviga calculators. That sort of problem occupied a good deal of our time. There were related problems on when was the best time to start firing with quadruple machine guns, particularly if you were in the desert. If you saw a plane from a long way off, the tendency was: start firing as soon as you see it. By the time it gets near enough to hit, you have exhausted the ammunition in the gun. You have to plan the correct time to start, otherwise you miss it altogether. That involved some very interesting probabilities.

Read: When did the new discipline of operations research begin?

Johnson: It began during the war, because Professor Pearson got involved with it and he would go to meetings of the Operational Research Society. Occasionally, he would drop problems on our laps, I suppose, because they involved some mathematical probability problem. That is all I can say. It was not highly prominent in our lives. The other thing I remember about the war years is that for the first three months, until the end of 1939, we were in Oxford (though not in the university) in various accommodations there, then we moved

to Sidcup in Kent until 1942, and then to Kensington, near the palace where Princess Diana lived much later. For a short time in Sidcup, I was in the local fire brigade; among various useful things, I learned how to climb up a ladder and not fall off.

Read: Were you ever called out on duty there?

Johnson: No, later on, after it became apparent we might get an invasion in 1940, we formed our own Home Guard unit; then when we moved into London in 1942, they decided they didn't want a Home Guard any more and we went back to fire watching. We did have one occasion when an incendiary bomb fell into an office building nearby and we all raced over with our hoses, etc. ready and were quite disappointed when the office girls had already put it out with their stirrup pumps. It was very amusing. They said, "Never mind, ducks, have a cup of tea!" We thought that was very generous, since tea was rationed. So that was the sum total of my fire-fighting experience.

Read: The Department of Statistics in University College geared up again in 1945?

Johnson: Yes, we had a good long notice, since the war was ending in August against Japan, so we had time to get ready for the session, which in Britain didn't begin until October. We were all back together again

for a year or two except that Bishop went into business and Donald Mills went for a short period to the National Coal Board. Later he converted and became a Roman Catholic priest; I am still in contact with him two to three times a year. When I am in England, I go to visit him, now retired, in an "almshouse" where a group of retired Catholic people of various kinds lives, in Ingatestone, Essex. When we started again, of course, I got going on the Ph.D., and I was also allowed to give lectures on statistics and probability.

Read: You worked for your Ph.D. dissertation on the Johnson system of curves?

Johnson: My advisor was Dr. Pearson, but as a benefit from a very unfortunate situation, in a way, I had had all the wartime, when I wasn't doing anything else, to think about what I might write for my thesis. Whereas nowadays people have to think about that in a month, say, I was pretty clear on what I was going to do. I hadn't done much about it, but I could see the possibility of a system of curves like the Pearson ones having some advantages as well as some disabilities, and I could get going on that. It took me three years anyway, because I was also concerned about establishing myself. It's a long story but perhaps not too long to talk about.

My actuarial interests came up again and I thought I might as well take those exams, which were suspended during the war, and finish them off. Also, in the last year of the war, 1944–1945, Regent Street Polytechnic (which is now a university) decided they wanted someone to teach statistics. They asked Professor Pearson, and I went to do it. I taught evening classes in 1944–1945 and 1945–1946. Among my students there was H. W. Haycocks, who was the secretary of the Actuarial Tuition Service, and I was taking courses from him as well. He wrote to me saying that we were getting a rather odd case of teaching each other things at the same time. So I got quite friendly with him. Soon after 1946 the Institute of Actuaries decided that they wanted a statistics textbook. After some discussion they decided that Herbert Tetley, a real actuary, was going to do it, but they wanted someone who knew some statistics. After some further discussion, and at Haycocks' urging, they chose me and we wrote a textbook.

Read: *Statistics: An Intermediate Textbook* (Johnson and Tetley, 1949, 1950).

Johnson: Two volumes. They wanted the second to be more advanced; they didn't think the first one was enough.



FIG. 2. Norman Johnson in his office at University College. By kind permission of University College London.

Read: First edition, 1949, and the second edition in 1962.

Johnson: Let me jump forward. This textbook was seen by Professor Nicholson who was the Head of the Department here at the University of North Carolina in Chapel Hill.

Read: George Nicholson.

Johnson: Yes, he thought it would be nice to get me to come and visit. At that time, this was the first textbook that had some simple, but relatively advanced statistics set out in an organized fashion and he knew I was the statistical part. So that is how I got invited here for a year in 1952–1953, which ultimately led to me coming here permanently in 1962. That is jumping a long time ahead but this all started with my giving those evening classes in 1944. It wasn't a plan of mine, but it led me to the United States.

Read: It is interesting how things link together. In 1949 you were elected as a Fellow of the Institute of Actuaries. Did you have to pass an examination?

Johnson: There was a series of examinations, some rather difficult. It was not like the Royal Statistical Society (RSS) where you were elected as a Fellow. Recently, the RSS have decided not to do that any more. It has been proposed that members will become Fellows only by examination or by being named honorary Fellows. The members who are already Fellows are going to be allowed to remain as Fellows. I always thought it a strange thing to become a Fellow without any sort of exam. In 1948 I took the Ph.D., finally.

Read: Yes, I have 1948 as the date you got your Ph.D. in statistics. About that time also appeared the

first of a series of about 10 papers you published jointly with Florence David between then and about 1956.

Johnson: Yes, that's right. Among a variety of topics, the most important one was the paper presented at the Royal Statistical Society, really on using "dirty tests" with order statistics (David and Johnson, 1956). Also, there were a number of special topics papers.

Read: Yes, there seem to be four areas, probabilistic integral transformations, approximations to distributions (when you got started on your interest in that area), some specific discrete distributions and then order statistics. This continued to about 1960 when E. S. Pearson retired and Maurice Bartlett became the Department Head, and Florence David was offered a professorship at Riverside, California.

I notice that you are listed as having been a vacation consultant to the Road Research Laboratory in 1956–1958. What kind of work was that?

Johnson: Oh, that was quite nice. There was an ex-student of the department in charge of statistical work there, Frank Garwood. They wanted a person to work there for a month; we dealt with road accidents with a lot of data coming in. We were concerned with such things as the age of the car, when there was an accident, the density of the traffic on the roads, things of that kind. I remember writing on road accidents in Europe. It was interesting to see whether different cultures had their highest intensity of accidents at different times of day, on different days of the week, things of that kind. I went there for three summers, 1956, 1957 and 1958.

Read: Were you able to publish anything arising from your work there?

Johnson: Yes, there was a paper published about 1959 I suppose.

Read: "Road Traffic Accidents in Europe, 1955–1957" (Johnson, 1958).

Johnson: Yes, that one. There is another one in the *Journal of the Institute of Actuaries* with Frank Garwood, who is no longer alive.

Read: "An Analysis of the Claims Experience of a Motor Insurance Company" (Johnson and Garwood, 1957).

Read: Did you do more work in quality control in England before you came to Chapel Hill?

Johnson: Well, yes. I was (it was a glorified name) a consultant for Carreras, the cigarette manufacturer. I have to drop my head in shame, knowing that I worked for a tobacco company as a consultant. This arose from contact with another student in my evening classes, F. D. Neech, who worked at Carreras. They wanted to do something about sampling.



FIG. 3. Norman Johnson, R. C. Bose, S. N. Roy and Wassily Hoeffding in Chapel Hill, 1953.

Read: Were there other things of interest before you came to Chapel Hill?

Johnson: I went to the Case Institute of Technology in Cleveland in 1960–1961, where I worked with Professor Leone.

Read: That's where you met Fred Leone then.

Johnson: Yes, that's right. He suggested that we should write textbooks on experimental design in physics and engineering.

Read: *Statistics and Experimental Design in Engineering and the Physical Sciences* (Johnson and Leone, 1964).

Johnson: They were published after I had gone back to England. In 1961–1962 Professor Leone came over for a while, we collaborated then, and the book came out in 1964. I didn't know I was coming back to America then, or perhaps we would have waited.

Read: Those books appeared in a second edition in 1977, at which time Leone was Executive Director of the American Statistical Association.

Johnson: Yes, that's right. By then he had left Case; he went for a while to Iowa and then went on to become the Executive Director of the ASA.

I should point out one interesting thing that happened during that period. It is not entirely nonstatistical. I met my future wife. In late 1958 she came as a postdoctoral student to University College. She had taken her degrees already in Poland, in Poznan. Professor Pearson said to me, "I want you to meet Dr. Regina Elandt, who

is a postdoctoral visitor from Poland, and I want you to look after her while she is here." I always obeyed the Professor, so I looked after her and married her.

UNIVERSITY OF NORTH CAROLINA AT CHAPEL HILL (1962–1982)

Johnson: We didn't actually get around to getting married until 1964 because she had obligations in Poland. She was the head of the new Department of Statistics in Poznan Agricultural University. In January of 1964 she came over to England and I went back to England from Chapel Hill, so we got married in England. We had a 10-day honeymoon; she went back to Poland because she had a commitment in Poznan until the end of the academic year, and I returned to my work in Chapel Hill. We had a couple more honeymoons during the summer, I went over to Poland, we had a honeymoon in Zakopane, then we went back to England together and had a honeymoon on some bus trips in Wales and Cornwall. We were both here at UNC from 1964 onwards. She was in the Biostatistics Department here and I in the Statistics Department. As you may know, at that time, you could not have husband and wife in the same department.

Read: It was fortunate that Chapel Hill had two separate departments.

Johnson: It was very fortunate, yes. The work she had been doing in Poznan was on agricultural research on wheat and plant genetics. When she came to Chapel Hill she shifted to human genetics. I mention that it had statistical applications, because we wrote a book together on models (Elandt-Johnson and Johnson, 1980).

Read: I noticed in going through your publications that in 1967 the first joint paper between you and Sam Kotz appeared; D. W. Boyd was a third author.

Johnson: That was the first one together.

Read: "Series Representations of Distributions of Quadratic Forms in Normal Variables" (Kotz, Johnson and Boyd, 1967a, b). Where did you first meet Sam?

Johnson: I can tell you that, very precisely. When I came to take up my permanent appointment here in 1962, it was in September and the semester had not started. I had to go for lunch to the hospital cafeteria, which we were allowed to use then, because our own cafeterias on the University campus were not open. I met Sam Kotz as we were both walking down to the cafeteria; we were both on our first day here, he as a postdoctoral fellow and I as a Professor in the Department of Statistics. We had our lunch together, talked about various things, what he was going to do



FIG. 4. At University College, 1959.



FIG. 5. In the U.S., 1961.

and what I was going to do as far as I knew. We have been friends for 40 years, from then until now. At that time, he was living in an apartment on Columbia very close to campus here. We would walk over there sometimes for lunch, and he would talk about his early life in Manchuria, then subsequently in Israel, and about how he met his wife. That is when I met him then. Sometime after that, he went up to Toronto, Canada, but we were in contact; we would talk on the phone. He suggested that we write some books on distributions sometime in the latter part of the 1960s. The publication dates were 1969, 1970, 1972, for three volumes, which became four and developed from there (Johnson and Kotz, 1969, 1970, 1972).

Read: It has been a very productive relationship.

Johnson: Yes, we've written about all sorts of topics and models. They were the most academic of our books. Sam has a great ability to formulate interesting projects.

Read: We'll come back to that in a moment. You had some work that you had been doing in University College that you essentially continued here for a few years, for example, on the Johnson system of curves.

Johnson: There were some details and some tables for fitting S_B distributions (Johnson and Kitchen, 1971a, b). These were really what I thought were odds and ends. There was a paper where instead of starting from normal distributions, as the Johnson system does, you start from the Laplace double exponential. With Sam we did some work on power transformations of gamma distributions and there it is not quite so tidy; not only do you not cover the whole plane, you cover

part of the plane twice. In my Ph.D. work the most difficult task I had was in establishing that the (β_1, β_2) mapping is one-to-one; that is, there is only one curve that has a given β_1 and β_2 .

Read: Some of the computer work for fitting S_B curves was done by Jim Kitchen, who was my classmate here at Chapel Hill, at least during my first year.

Johnson: I was his advisor for his Master's degree, which he did on zonal polynomials. Whenever I was working with Jim, whatever sort of research I suggested would turn into computations. He computed some things for the zonal polynomials. I always wanted to see what all they were but I knew I could not do the programming. His interests turned away from statistics into computing. I explained all the statistical background but he only perked up when I said, "Then we need to calculate this."

Read: Yes, and he is still working here at UNC in computer programming. I remember that when I was a student here the mainframe was in Raleigh and we used to have to dial through on the phone. Jim knew how to do all that.

Do you think that your work on the Johnson system of curves was the main piece of work that you have done, looking back?

Johnson: I think it is the main piece of work I did on my own. I have done a lot of work jointly with other people, and not just with Kotz. The Ph.D., as it should be, was done by me alone. Professor Egon Pearson was my academic advisor, and we agreed that this was the best way to do it. I told him what I was going to do and he said, "Alright, I don't mind advising you on that." He did advise me, but mostly on how to present the paper. That I think was the most useful research, in terms of papers. Some books have been used more than others, such as *Survival Models and Data Analysis* that I wrote with my wife (Elandt-Johnson and Johnson, 1980), and which was adopted by the Society of Actuaries here as their official textbook for a while. That did wonders for the sales, of course. That was more generally useful, as far as people using it. For my own work, you are right, I think the Johnson curves was the most important.

Read: Of course, you linked up with the American Society of Quality Control (ASQC).

Johnson: That was through Leone. He was very active in that and pushed me into it. I did some work there on acceptance sampling and sat on various international committees as one of the members representing the ASQC. I felt very strange when we had one in London and I was a member of a foreign delegation.



FIG. 6. With Regina at their home in Chapel Hill.

SOME PERSONALITIES

Read: Did you have any contact with Deming?

Johnson: Yes, we used to write to each other. Mostly I wrote to him about things I should do with certain control charts and he would write to me at great length about what I should do and wrote of things I didn't ask him about, as well. He was very generous with his time, and I did visit him once in his home in Washington, where he showed me some of his work.

Read: Another area where you did considerable work was in sequential analysis. In 1961 you wrote a survey of sequential analysis at that time (Johnson, 1961).

Johnson: I did present part of a course on sequential analysis in London, not long before Professor Pearson retired. Professor Pearson suggested that I might write it up because he knew I had done a lot of work, assembling what was known about sequential analysis in 1960. He thought this would be useful to go to the Royal Statistical Society and he more or less promoted it through the Society. I was more or less asked to do

it. That is how it got written. It was helpful for me getting the lectures together, of course, because I put even more effort into crystallizing different sections of the subject.

Read: Let's talk about some of your colleagues here at Chapel Hill, such as Harold Hotelling and Wassily Hoeffding.

Johnson: Hotelling was still the Chairman of the Department when I came in 1952–1953. I believe that Nicholson had succeeded him by the time I came in 1962. I was quite friendly with Hoeffding, because when I came in 1962 I was still single and he was also single. He worked in sequential analysis and decision theory, and we did a lot of work together. He was interested in a simple proof I gave of a theorem by Wald (Johnson, 1959).

When I came in 1952–1953, I took the slot that (Herbert) Robbins had, since he was on sabbatical. By 1962 I think he had left the department, but I felt honored to have had the same numbered slot in the University that once belonged to Herbert!

Read: When you were over here in 1952 did you meet Gertrude Cox?

Johnson: Oh yes, and in 1962 as well. She was a lot more active and there was a lot more going to and from Raleigh at that time. I used to see her there and she used to come over here. She was on several doctoral committees and was very pleasant to talk to. She spoke often of her orchid farm and sometimes about the history of statistics.

Read: Was Professor Greenberg in the School of Public Health at that time?

Johnson: Yes, and, of course, when my wife came in 1964, I got to know him better. He wrote recommending that she should be offered a position in the University here. We had a lot of contact with people in Biostatistics; I still see them. Having my wife as a colleague there was helpful to me in getting to know the people.

Read: There was Herbert David, of course.

Johnson: He was in our Department as well for a while. He was a student in London before I came here; H. O. Hartley was his advisor. I was invited to attend his retirement meeting at Iowa State in 1996. He made some very gracious statements on that occasion, for example, that I gave some "superb" lectures. It was nice to hear someone say that; not everybody does. I gave a paper there on the history of estimation with order statistics. It was a very pleasant occasion.

Read: I think that most of your students would attest to the high quality of your lectures; they were both

lucid and thought-provoking. One of your books, published in 1969, goes back to your work in University College, *Systems of Frequency Curves* (Elderton and Johnson, 1969), a revision of W. P. Elderton's *Frequency Curves and Correlation*.

Johnson: That was really through Professor Pearson. He wanted someone to help with it, and he thought that among the people working in the Department I was the one most suitable to do it. This was actually completed while I was here at UNC.

Read: Did you know Elderton?

Johnson: No, but I had correspondence with his son, H. P. Elderton. He was in London once and I met him there, which was very useful. I had always liked that book; of course, it was on distributions, and it was nice to have an opportunity to work on it thoroughly. I slipped in a little about Johnson curves as well.

Read: Would you like to reminisce about your year at the University of New South Wales in Sydney, Australia, in 1969?

Johnson: Yes, it was quite novel for my wife and myself. I had a Visiting Professor appointment in the University of New South Wales. My wife had a similar research appointment in what was equivalent to a Department of Biostatistics there. I was made a regular member of the faculty, I had courses with lectures to give, I had a temporary seat on the University council as a full professor and I had a lot of discussion with people there.

Read: Who was there at that time?

Johnson: There were John B. Douglas and Clyde ("Charles") McGilchrist. Chris Heyde was at Canberra; I spoke with him a great deal. I remember that I was on the Ph.D. examination committee for Lynne Billard.

Read: I recall you telling me about her at the time.

Johnson: I was not her advisor, but I was on her committee. Also, I was quite impressed with her thesis. When the University of Georgia was looking for new faculty, I was on a committee to suggest new people; I suggested Lynne Billard, and she was the one they finally selected. Since then she has become quite prominent here.

I visited Fisher's grave. He retired to the University of Adelaide in South Australia. When he died, his funeral service was at Adelaide Cathedral and he is buried there. In fact, the University of Adelaide published a set of his collected papers.

I enjoyed the working conditions there; there was a very lively atmosphere, and we got to see organizations like the National Institute of Statistics, the government statistical office. When I returned to Australia in 1980,

it created a nice basis for me to go around and meet people.

JOINT WORK WITH SAM KOTZ (1969–2000)

Read: Let's talk about the series of books on distributions with Sam Kotz. It was a staggering, enormous amount of work for both of you (*Distributions in Statistics* series; Johnson and Kotz, 1969, 1970, 1972; Johnson, Kotz and Balakrishnan, 1994, 1995, 1997; Johnson, Kotz and Kemp, 1992; Kotz, Balakrishnan and Johnson, 2000, 2004).

Johnson: It was. Initially, we weren't sure of the dimensions of it. Sam, as you probably know, is quite good at bibliographical work, getting references from all over the world. I contributed a few, since we had a bigger library here than where he was. We worked out a scheme. To start with, we sketched out the chapters, then we assigned one of us as the chief writer of that chapter. We would write everything we could for the chapter, send it to each other, then tell each other we were wrong and so would have to rewrite it! We also had to keep track of how long chapters were altogether. They tended to be rather long, and Wiley didn't want to publish such long books.

Read: Houghton Mifflin had the first run on them.

Johnson: Houghton Mifflin were the first ones, but they got cold feet. They were not really into selling this sort of a book, preferring more general textbooks. Finally, Wiley agreed to take it over. At a later date, Wiley said this was one of their better deals. By the time we moved forward with it, they were selling very well. I think what happened was this. *Discrete Distributions* was entirely a Mifflin production; it was only after we got started on *Continuous Distributions* that they didn't want to go on with it. Maybe the sales weren't as big as they expected. The customers to whom they sent announcements were not the sort of customers who would buy this, perhaps. They published more for general undergraduates, and they were a bit appalled at the number of formulas in these books. Even for statistics books they have a lot of mathematical formulae. I think that is what happened.

Read: They have been extraordinarily useful. They certainly were to me, as a source of information about moments and properties of distributions. I particularly liked the general introduction to the series in the first volume (*Discrete Distributions*). I think you had a section on finite differences in that, which is very useful.

Johnson: That was provided by me. We were going to use it a lot, later on. Some of the formulae for



FIG. 7. Johnson in 1975.

moments involve finite differences. Sam contributed a great deal, and I used to say that sometimes I felt my only contribution was putting things in English. At that time, Sam's written English was a bit fractured. Between us, we each contributed what we did best. The series did come out quite well and had a good reception.

Read: Well, they broke new ground. I noticed that Wiley had taken over by the time the fourth volume, on continuous multivariate distributions, came out.

Johnson: They were also producing the reprinting of the earlier volumes as well. You have done plenty of writing on distributions too.

Read: Yes, but mainly on normal distributions. Around the time the fourth volume appeared, multivariate distributions were taking off in the world of research. I don't know how you feel about the second edition because you both brought in Adrienne Kemp for discrete distributions and Balakrishnan for the others.

Johnson: I think there is a great deal more material in them than in the first edition; the books are twice as big as they were before, and there is yet to come out a *Continuous Multivariate Distributions*, Volume 2. Volume 1 is titled *Models*; the second volume is titled

Sampling Distributions. You may notice the new edition doesn't have Wishart distributions in Volume 1, because they are sampling distributions, and you don't often use a Wishart distribution as a model.

Continuous Multivariate Distributions 1—Models is mostly modeling distributions. There I suggested demoting myself from Johnson, Kotz and Balakrishnan to Kotz, Balakrishnan and Johnson, because I did not take such an active part in it, and I'm getting older. I also don't keep up to date with publications. Incidentally, there is a third edition of *Univariate Discrete Distributions* planned, which will probably be by Johnson and Kemp, but should be Kemp and Johnson, because I really am only carrying out a minor role, though I expect to have to spend some time checking for typos, because it is being reset.

Read: In 1977 you and Sam produced *Urn Models and Their Applications* (Johnson and Kotz, 1977).

Johnson: This was one we felt the proudest of, in an academic sense, but it did not produce many sales because it was too academic, full of details and special cases, and it did not appeal to the general populace. Sam is also very proud of this book. It looks very impressive and has very interesting results in it. It doesn't have many misprints, which is unusual.

Read: In 1991 Chapman and Hall published *Inspection Errors for Attributes in Quality Control* (Johnson, Kotz and Wu, 1991).

Johnson: Yes, that was in the quality control area. Professor Wu was a visiting professor at UNC, two or three years before we wrote it. He was interested in quality control, and when Professor Kotz came to visit, I talked things over with him and he suggested that perhaps we could ask Wu to join us, which he did. The book was completed after Wu went back to China, although he is back in Carolina now. It is a little book, codifying a lot of things that were in separate papers by other authors as well as ourselves, so that you can see the effect of errors and you could see when they were important and when they weren't.

Read: Well, that brings us to the *Encyclopedia of Statistical Sciences* (Kotz, Johnson and Read, 1982–1989). We'll talk about that, although you and I are very familiar with the details. But I was not in on its founding. What was the germ of the idea?

Johnson: As with many of our ventures, the real founder was Sam Kotz. The *Distributions in Statistics* series was his idea. The book with Wu that we just mentioned was his idea. He thought we could have an encyclopedia in statistics. We went through a long period of planning, imagining just what it would look

like when we finished. (Although it was actually much larger, these things grow. We sketched out 24 subgroups, not from the point of a published book but from the point of listing entries and then correlating and collecting them.)

We spelled these out and when we got all of them together, we forwarded them to Wiley to see what they thought of them, because Wiley published this sort of thing. I don't know exactly how long it was but what we sent them was like a paper. They were more or less favorable, but they decided to have a meeting in New York sometime before we started work on it in 1980. I was quite happy because I was going to retire in 1982 and I was going to have time for it. Sam and I were to be the Editors-in-Chief, as they put it. Eventually, there was a lot more work than I expected. After a while Wiley said okay, we started work and sent out invitations for contributions. We set up an Advisory Board, a real advisory board, with ideas, but mostly we wrote to them for advice if we needed it. Things sort of grew from there. It took up a lot of space in my office, as well as in Sam's. We had a folder for each entry. We discussed format with Wiley, how big each volume was going to be, etc. The *Encyclopedia* was not published all at once, but one volume at a time. This was more or less forced on us because it would have taken years, if we had waited until every volume was ready. The first volume was slimmer than the others, but we built up as we went along.

Read: When we did the update volumes, Sam said that we should give a lot of attention to the early letters of the alphabet because they tended to be left behind in the first run through.

Johnson: And at some point, we decided we needed someone else to do some editing. That is how you came in.

Read: Yes, that was shortly before the first volume went into production. I said, "Yes, I'll do it provided I can participate in the invitations, the editing and reviewing and in writing some of the entries."

For all I hear about it from students at SMU, the *Encyclopedia* has been invaluable to students. Students will come into my office and ask me questions about this or that and I send them to look it up in the *Encyclopedia*. I might tell them the name of the entry, and that is all the connection they need. Certainly, there were some problems with certain topics. I remember how long it took to find a contributor to write on the subject "Statistics in Astronomy." By the time we found someone, we had not only passed the letter A but we

had passed the letter S and it had to go into the supplement volume. Of course, the people at Wiley are used to encyclopedias.

Johnson: Of course, you know, the *Encyclopedia of Biostatistics* grew from that. Wiley published it in several volumes.

Read: They published it all at once; they had a lot of associate editors.

Johnson: That, of course, is a consequence of the *Encyclopedia of Statistical Sciences* being successful. The biostatisticians thought, "We could do one on our own."

Read: All that we had was an entry in Volume 1 on biostatistics that Bernie Greenberg wrote.

Johnson: We had other entries such as those on life tables and clinical trials that biostatisticians might need.

Read: One of the subject areas in the *Encyclopedia* is biographies of leading personalities, which brings us to another book that you and Sam edited, essentially, the biographies from the *Encyclopedia*. However, there are several biographies in that book that are not in the *Encyclopedia* (Johnson and Kotz, 1997).

Johnson: Yes, that's right. I'm not sure how the decision was made whether to include them in the *Encyclopedia* or not.

Read: It could be that they were submitted later. I think *Leading Personalities* came out in 1997, many years after the *Encyclopedia*.

Johnson: Yes, consequent to our work on the *Encyclopedia*, Sam had several bright ideas for us to produce work on various aspects of the history of statistics. Not all came to fruition, but *Leading Personalities* did, and so also did three volumes on *Breakthroughs in Statistics* (Kotz and Johnson, 1992, 1997). The first two volumes appeared in 1992, well before *Leading Personalities*. The preface opens with an acknowledgment of its conception in our work on the *Encyclopedia*. Each volume contained reprints of about 20 papers that, in our opinion—based, in part, on advice from many eminent statisticians—contained seminal contributions to the development of statistical theory (Volume 1) or applied methodology (Volume 2) in the period 1890–1989. Each reprint is introduced by a commentary from a contemporary authority. Volume 3 came later (in 1997) in response to many suggestions from colleagues. (The desirability, or at least likelihood, of such a development was foreshadowed at the end of the preface to Volumes 1 and 2.) This time, again, we had about 20 reprints, but gave up trying to segregate them into "theory" and "methodology."

I do remember we had one principle in the *Encyclopedia*, you didn't get in there until you were dead. We made one slip, in that we put E. J. G. Pitman in, although he did die afterwards.

Read: I remember that. It was unfortunate.

You and Sam wrote a book *Process Capability Indices* (Kotz and Johnson, 1993).

Johnson: It is very similar to the one on inspection errors. It is our most recent focus of work. Process capability indicators have become widespread in use in industry. We decided that it would be useful to have a book setting out what a process capability indicator is, what ideas went behind having such things and how far they do what they are supposed to.

There is another book by Sam Kotz with Cynthia Lovelace which is more on practical applications of process capability indices. Ms. Lovelace runs a sort of quality control advisory service. In the *Journal of Quality Technology* in January of this year (2002), there is a 19-page article by Sam and myself, giving a review of process capability indices from 1992 to 2000 (Kotz and Johnson, 2002). It is an historical look, followed by eight people commenting on it, some favorably, some adversely, and then a three-page reply by Sam and myself.

Read: That was a substantial piece of work. Are you continuing work on that?

Johnson: Right at this moment I am not. I don't know whether Sam is; I think he has gone back to work on distributions.

OTHER ACTIVITIES

Read: You have received various honors. You received the Wolfowitz Prize from the *American Journal of Mathematical and Management Sciences* in 1982, jointly with Sam Kotz, and the Wilks Medal from the American Statistical Association in 1993. When you received the Shewhart Medal from the ASQC, you addressed the ASQC with the topic "A Statistician's Apology." What were some of the things you said to them?

Johnson: This was a speech I made when I received the Shewhart Medal in 1985; it was published in *Quality Progress*, 1985. In that I wrote about mathematical statisticians and quality control workers, and I tried to explain the relationships between them and how I felt that, although I was a mathematical statistician, I was qualified to receive this honor from them.

Quoting from the article: "What good advice can I give? I certainly cannot say much about the best way

to organize a quality control department (or 'quality function,' perhaps), though the effort to do so might provide a much-needed element of humor. Well, I am a statistician, and applications of statistics form a unifying feature of quality control techniques in many fields. However, I do not have any brand-new innovative techniques to uncover, and even if I did, this is hardly a suitable occasion. Furthermore, I have to admit that I am a theoretical, even—horror of horrors—*mathematical* statistician. So I cannot be expected to provide expert insights into QC applications of statistics."

I proceeded to explain why I should say something useful.

"Unlike many of the early recipients, I cannot claim the honor of personal acquaintance with Dr. Shewhart—but I worked for 25 years with Egon Pearson, whose interest in QC was fired by correspondence with and ultimately by meeting with Shewhart on a visit to the U.S.A. in 1931.

"My interest in QC matters received a considerable boost from meeting in 1960 with Fred Leone when he invited me to visit Case Institute in Cleveland." The first direct consequence was the book on design (*Statistics and Experimental Design in Engineering and the Physical Sciences*), with Fred Leone. "It was through Fred's influence—not to say insistence—that I joined ASQC, a move not yet regretted, which led me to enjoyable participation in many of its activities."

The original inspiration for the other book in the citation, *Encyclopedia of Statistical Sciences*, came from Sam Kotz. Then I go on to say,

"A legitimate concern for a mathematical statistician is the derivation of new results (and, of course, sometimes even consideration of how they might be used). Over the last few years I have been involved in collaboration with Professor Kotz and lately with Bob Rodriguez in studies of the consequences of errors of inspection on the results of such inspection. (We use *inspection* in a broad sense, not limited to traditional acceptance sampling, but including some operations in process control.) I must confess that my interest in this topic has been fueled by the remarkable variety of theoretical distributions that can be encountered, rather than by considerations of immediate utility. But there have to be persons who do this sort of thing, and I feel I am lucky to be one of them."

"I also have a possibly more respectable interest in trying to recognize potential applicability in QC (in other fields also, but especially in QC) of new statistical techniques as they pour off the presses. It is rather like panning for gold—or some even rarer

element—to identify the 2 to 4% of publications with new techniques promising eventual wide application, and then to decide whether they belong to the small proportion—shall I say 10%?—of these which can be used in current works or likely immediate developments. This is, I believe, a very necessary function.”

Then I refer to the first Shewhart medalist, General L. E. Simon, who said:

“In only a quarter of a century QC has changed so much that the term has lost some of its descriptive values.” I commented: “Quite possibly things are now even moving a bit faster.”

Of course, now I can say even faster still. Then I make a favorable comment on the *ASQC Basic References in Quality Control: Statistical Techniques* (also known as the “How To” Series) under the editorship of John Cornell and Sam Shapiro. I explain how they are very useful and recommend them. Later on I go on to say:

“W. H. Auden once wrote: ‘Thou shalt not sit with statisticians, nor commit a social science.’ It is pretty clear that the Society has broken the first of these two commandments; I think we should realize that we have in effect also broken the second. Although not generally regarded as such, I believe that QC is a ‘social science,’ though perhaps not in the generally understood sense of the word. (I may mention that a few years ago there was a study in Britain inquiring into the esteem with which members of different professions regarded each other; statisticians and sociologists had the lowest recorded mutual esteem for each other, but this, in my opinion, is a matter of semantics so far as statisticians are concerned—some of my best friends are sociologists.) Returning to the topic of QC as a social science, this is at least arguable.”

Then I say that QC is concerned with quality of life and so forth.

Read: I wonder if there are any other topics that you would like to talk about that we have not brought up yet.

Johnson: I would like to mention the influence that someone had on my life to some extent. In my first, prewar, year on staff at University College, we had a visitor from Poland, Jozef Marcinkiewicz. He was only over for a month or so because he was also visiting Paris. He was a very good theoretical probabilist, he was interested in statistics and he was very remarkable. He was only 28 years old and already a Professor in Poland. We had a lot of talks. I was flattered that he took notice of such a junior member of the staff as

I was, in my corner of the laboratory where I felt protected against Fisher. We talked a lot about that, and he came to me for what he called a good practical outlook, thinking that his mathematical statistics ought to be more applicable than it was in the way he had learned it. I also thought I was learning a lot more about mathematics than I had ever learned as an undergraduate in University College. We had a lot of conversations then. When he left in the spring of 1939, it was pretty clear there was going to be a war. He already had been offered a post in the United States, and I said (which almost destroyed our friendship), “Won’t you perhaps accept this and be out of the way if the Germans invade Poland?” He was extremely indignant. He said, “My duty is to go back and defend my country, I am a reserve officer and I am surprised you would think of something as bad as that. Why don’t you go off to the United States?” I was able to calm him down and sort that out. He did go, and he was taken prisoner by the Russians and ultimately murdered in Katyn Forest near Smolensk. I always felt that I would like to take an opportunity of saying how highly I thought of him as a person and as a probabilist who was appreciative of statistics and that somehow or other thought I could do something useful. At that time when I was just starting, as you know, you are not very sure of anything. I would like to take the opportunity of mentioning that. In fact, in the *Encyclopedia*, there is an entry “Marcinkiewicz’s Theorem.” He wrote a book, but I don’t know the title.

There is one thing of which I was very proud. In 1996 or so, for a short period, the Presidents of the Royal Statistical Society and the American Statistical Association were both people whose advisor I had been for their Ph.D.’s.

Read: Who were they?

Johnson: J. Kettenring here and D. J. Bartholomew in England. It just happened that their terms overlapped. Three years later, the President of the American Statistical Association was William O’Fallon, whose wife was a Ph.D. advisee of mine.

Read: Norman, you did some work with D. M. Ennis in the early 1990s. Would you like to talk about those papers?

Johnson: D. M. Ennis is a mathematical psychologist, mainly a psychologist. He had some problems in setting up models for certain psychological phenomena. I made a number of suggestions to him, and as a consequence we did write one or two papers together on the construction of the models and also on distributions that arose in the analysis. I still hear from him, and we correspond.

Read: One area that we have not touched on is censoring.

Johnson: The work I did on censoring is a topic that was interesting to me, one of the things that arose from my work during the war. One problem we often had to deal with was incomplete data, incomplete in certain peculiar ways. A number of the papers that I have written were about the estimation of sample size, where you are given the means, say, or the standard deviations in a series, as in a quality control sequence, but nobody tells you how many items were in the samples. You are trying to estimate how big a sample was being used. The same sort of thing, where you are lacking certain information, leads to a test of censoring. This is where you are given some data, you suspect it has been censored and you want to be able to test whether this is so. So this is called a test of censoring. There are various kinds of tests for various kinds of models, and they lead to some very interesting statistical theorems. Of course, you can let your imagination run wild over all the ways in which you may be lacking some important information about the data. It was natural for data to occur like that in wartime. Some of the information came from enemy sources where they didn't want to tell you everything.

Read: You had some publications on experimental design with J. W. Archbold and others (Archbold and Johnson, 1956).

Johnson: I never felt I was sufficiently well versed in experimental design to become a great authority on it, but there were certain points that came up that I thought needed to be cleared up. Sometimes I was able to clear them up on my own, sometimes with the help of Archbold. He was a lecturer in Mathematics at University College London. In fact, I had attended his lectures when I was an undergraduate. After I joined the faculty, I was friendly with him, and one day I was bewailing the fact that I could not see how to set up certain designs. He said, "Ah, this is all group theory." I said, "Do you know about that?" and he said, "Yes, I know a lot about that." So we wrote that jointly. The book with Leone was on experimental design, and I organized what I knew about it in contributing to that work as well. I never felt I was a great authority on that, however.

Read: You were very good at expository writing in that book.

Johnson: Yes, and I had to write it very carefully to understand it myself. Incidentally, Indra Chakravarti, who died recently, was an authority on experimental design. He provided a very great help in this part of my



FIG. 8. *Johnson in 2001.*

armament of statistics, where I knew the sort of thing to do but I often didn't know the details of how to do it.

Read: He was very helpful to me in his lectures, particularly on the distribution of quadratic forms in the general linear model.

Talk a little bit about Hartley.

Johnson: He was at University College when I was there as a lecturer. He left and went to the United States at Iowa State and finished at Texas A&M. I got to know him fairly well in London, and he had very good students so he must have been a very good advisor. Her-



FIG. 9. *Campbell Read and Norman Johnson outside New West at The University of North Carolina, May 2002.*

bert David was one of his students in University College.

Read: There was a paper that you wrote with Pearson and Burr (Pearson, Johnson and Burr, 1979).

Johnson: I think this was when Egon had retired and he wrote to his friends asking them to do certain calculations. We pooled our results together, but the paper was mostly written by him. That particular one Egon wrote himself with material provided by Burr and myself.

When I was in England, I used to visit Egon where he was living in retirement in the Sussex hills. He had a long, long room; at one end was his bed and the other half was his study with all his statistics works there. It was while I was there, I'm sure, that I was enlisted in that project.

FINAL THOUGHTS

Read: Finally, Norman, what advice do you have for young people considering or starting a career in statistics today?

Johnson: Well, since statistical methods can be applied in so many fields of endeavor, it seems desirable that young people should be made aware of the wide variety of forms under which numerical data can arise. It is then natural to consider how such data may be used effectively, to provide useful information on the relevant process(es) and sometimes to suggest possible practical action. Initially, the treatment should be very broad, and not highly technical, so as to be suitable for students in the last one or two years of high school. It should include some exposure to use of traditional graphical methods—bar charts, histograms, scatterplots, etc. Later, the need for a more disciplined approach—as opposed to anecdotal references—should become apparent.

An important further step is the introduction of the use of probability theory, and of the relationships between “probability” and “theory.” Of course, by the time that this stage is reached, the student should have an adequate (though not too highly abstract) background and command of basic mathematics—especially elementary algebra and calculus.

At this point, it should be possible to provide occasional elementary examples of applications of statistical techniques to deepen the students' awareness of circumstances under which such applications are possible and rewarding. Hopefully, this will provide some motivation for formal study of statistical methods and

principles and associated distribution theory, with special reference to regression, and, at a later stage, to multivariate techniques.

It should be borne in mind, of course, that relatively few young people decide, at an early age, to aim for a statistical career. In my own case, as I have already explained, it was only at age 19 that I came to study, in a fortuitous manner, “statistics” as a full-time subject, to complete a residency requirement for a first degree. In some ways, I regarded it, even so, as a welcome change after two years of formal mathematics, pure and “applied” (statics, dynamics, hydrodynamics and a little relativity). I was fortunate in getting a thorough training in statistical methods, with some exposure to applications. It was only later, while working at the Ordnance Board, that I encountered many varied statistical problems, including some of considerable interest (to me, at least).

At present (and, hopefully, even more so in the immediate future) students do have available more natural and gradual introductions to the theory and practice of statistics. I would like to note that the features I have sketched out are relevant to a person interested in being a sort of “general practitioner” of statistics. There will be specialists, for example, in design of experiments, survival analysis, analysis of (very) large data sets, modeling, etc., but *all* should at least understand the salient aims of such specialties and how they may be useful in various situations. What I hope will be avoided is the feeling that the ability to press the relevant buttons in computer programs is an adequate substitute for a clear appreciation of the logic and practical requirements for useful application of statistical techniques. (Of course, ability to press the correct buttons is also desirable!)

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