Statistical Science
1986, Vol. 1, No. 1, 97–105

A Conversation with T. W. Anderson

Morris H. DeGroot

Ted Anderson was born on June 5, 1918, in Minneapolis, Minnesota. He received an A.A. degree from North Park College in Chicago in 1937, a B.S. in Mathematics from Northwestern University in 1939, and an M.A. and a Ph.D. in Mathematics from Princeton University in 1942 and 1945, respectively. In 1945–1946, he was a Research Associate in the Cowles Commission for Research in Economics at the University of Chicago. From 1946 to 1967 he was a faculty member of the Department of Mathematical Statistics at Columbia University, starting as an Instructor and, in 1956, becoming a Professor. He served as Chairman of the Department in 1956–1960 and 1964–1965, and as Acting Chairman in 1950–1951 and 1963. In 1967, he accepted his present position as Professor of Statistics and Economics at Stanford University. He was a Guggenheim Fellow in 1947–1948, Editor of the *Annals of Mathematical Statistics* in 1950–1952, President of the Institute of Mathematical Statistics in 1963, and Vice President of the American Statistical Association in 1971–1973. He is a Fellow of the American Academy of Arts and Sciences and a member of the National Academy of Sciences. The following conversation took place in his office at Stanford one morning in late February 1985.

"I WAS TERRIBLE IN THE LABORATORY"

DeGroot: How did you originally get interested in statistics?

Anderson: As an undergraduate I was a student in chemistry, and that came about because I had a high school teacher, Henry Schoultz, who made chemistry seem very interesting and fascinating. I went to a junior college in Chicago, called North Park College, for two years and I took all the chemistry that I could there. Then I went on to Northwestern in my junior year and I took physical chemistry and quantitative analysis. There was a lot of laboratory work, and I was terrible in the laboratory. I could hardly come within 50% of the right answer. One of my professors still delights in talking of the miserable results of this student who went on to become a well-known mathematical statistician. After that year I got so discouraged about the laboratory work (I didn't realize that you could be a theoretical chemist) that I gave up chemistry. At that point I debated whether to go into psychology or into mathematics and statistics. I had an advisor, Angus Campbell, a psychologist who has since died, who thought it was just crazy of me as a senior to think about changing into a new major. But I was sure that I wanted to get out of chemistry. (Laughs) So I insisted, and mathematics seemed to be the more suitable field for me. Certainly it turned out to be correct that mathematics is what turned me on and what I was good at. The trouble was that in spite of having been a chemistry major I took no mathematics in my first two years, so as I went into my senior year I had only gone through differential calculus. It was kind of late in the game.

DeGroot: You had to make everything up in your senior year?

Anderson: Yes, I made everything up in my senior year. In fact, I took two required courses at the same time; I would drop off my homework in one course, walk across the hall to the lecture in the other course, and at the end of the hour get the assignment in the first course. I was also interested in economics and social science generally, partly out of interest in the subject and partly because of some vague idea of doing some good. So I followed my interest in economics along, and statistics seemed to be a natural link between economics and mathematics. At Northwestern there was a very lively and interesting man by the name of Harold T. Davis. He had the nickname of Little Caesar.

DeGroot: Because of his personality?

Anderson: Yes. [Laughs] He taught econometrics and statistics, and some time series analysis.

DeGroot: Was he in the math department?

Anderson: He was in the math department, yes. He got me going along that track, and I think my interest in time series analysis really started with him. Then I spent one year as a graduate student at Northwestern because I had been so far behind in my mathematical preparation. I took some courses in mathematical economics with a fellow by the name of Bill Jaffé, who made a lifetime career out of translating Walras and editing his letters. He was very enthusiastic about the use of mathematics in economics, and he also stimulated me in that direction. So I got into statistics in part because it's a branch of mathematics and in part because I thought it would be useful in the social sciences.

DeGroot: So your interest in social science and economics really goes way back to your undergraduate days.

Anderson: Yes, it really does.

"I TALKED MARSCHAK INTO TAKING ME ON"

DeGroot: When you moved from Columbia to Stanford in 1967 your title changed from Professor of Mathematical Statistics to Professor of Statistics and

Economics. Did that represent a real change in your activities?

Anderson: Well, here's the background on that. At the end of the war, I was interested in doing research and I continued my interest in economics. Oskar Morgenstern suggested that I go to Arthur Burns, who was head of the National Bureau of Economic Research, and see about getting a position there. That interested me because I thought it would be fun to live in New York City.

DeGroot: When was this?

Anderson: This was in October 1945. I went in there and Burns said to me, "Well, we have no use for anybody who is as well trained in statistics as you are."

DeGroot: What did he mean by that?

Anderson: I was over-trained. He had no use for more than data collection and very simple statistical procedures. He was afraid that I would try to do something too sophisticated. So then I got in touch with Jacob Marschak, who was head of the Cowles Commission for Research in Economics at Chicago. It happened that he was out at Rye, New York, for a conference on atomic energy just at the end of October. I had an interview with him on a Sunday, and on the following Monday I was supposed to start teaching at Princeton for the fall term. I had said I would teach, but I really didn't want to stay on at Princeton for another year. The Sunday that I had the interview with Marschak was the cut-off point. He wanted to go back and talk to Tjalling Koopmans about appointing me but I talked him into taking me on. There was a price for the quick decision; my salary was set at a fellowship level, $2500, for a year. So I spent a year at the Cowles Commission, and that's when I got very involved in econometrics. We were developing statistical procedures for estimating coefficients of simultaneous equations, and there were new problems. It fitted in very well with my interests because in my dissertation I had followed up on Fisher's work on discriminant functions. I had considered problems of estimating several mean vectors when the rank of the matrix of mean vectors could be less than the maximum. It turned out that in the simultaneous-equations problem, estimating a single equation involved essentially the same kind of mathematics and the same approach. So it just worked out well; I was prepared to handle some of their problems.

I was at the Cowles Commission for a year, and during that year Abraham Wald invited me to join him in the new Department of Mathematical Statistics at Columbia. He said that he would like to work with me on some of these problems in econometrics and that sounded fine. But when I got to Columbia I found that he was interested in statistical decision theory, not in econometrics anymore. [Laughs] So that cooperation didn't work out the way I had expected. At Columbia, there wasn't anybody interested in the kind of econometrics that we had been doing, econometrics based on probability models and modern statistical inference. So I finished up a couple of things I had been working on; in some cases it took quite a long time, like 10 years. But I didn't really keep up activity in those topics. I got into latent structure analysis and panel studies after being stimulated by Paul Lazarsfeld. Although I didn't really keep developing econometric procedures for a good part of the time that I was at Columbia, I still had the interest. So in coming to Stanford, I took a joint appointment which is half in economics and half in statistics. I now teach econometrics in the Economics Department, which renews an interest that I had for a long time.

"THE COLUMBIA DEPARTMENT HAD A SERIES OF CRISES"

DeGroot: We sort of skipped over your graduate-student days at Princeton. With whom did you work there mainly?

Anderson: At the time that I was going to leave Northwestern to study for my Ph.D. in a more developed mathematics department, the two most appealing possibilities were to go to Columbia where Harold Hotelling was or to go to Princeton where Sam Wilks was. It turned out that Al Tucker at Princeton had married a daughter of D. R. Curtis at Northwestern, and so that system worked out to get me to Princeton rather than to Columbia. [Laughs] Actually, it worked out very well because at Princeton I got good training in mathematics and, since Wilks was very knowledgable in mathematical statistics, I also got training in statistics. Then the United States got into the war and, of course, war research work developed. So in early 1943 I got into defense work. I would still work on my dissertation in the evenings, but I also did various other projects. The first was the evaluation of long-range weather forecasting. Then we got into a project in which we were going to advise the Navy how gunfire battles involving battleships should be fought. A kind of operations research.

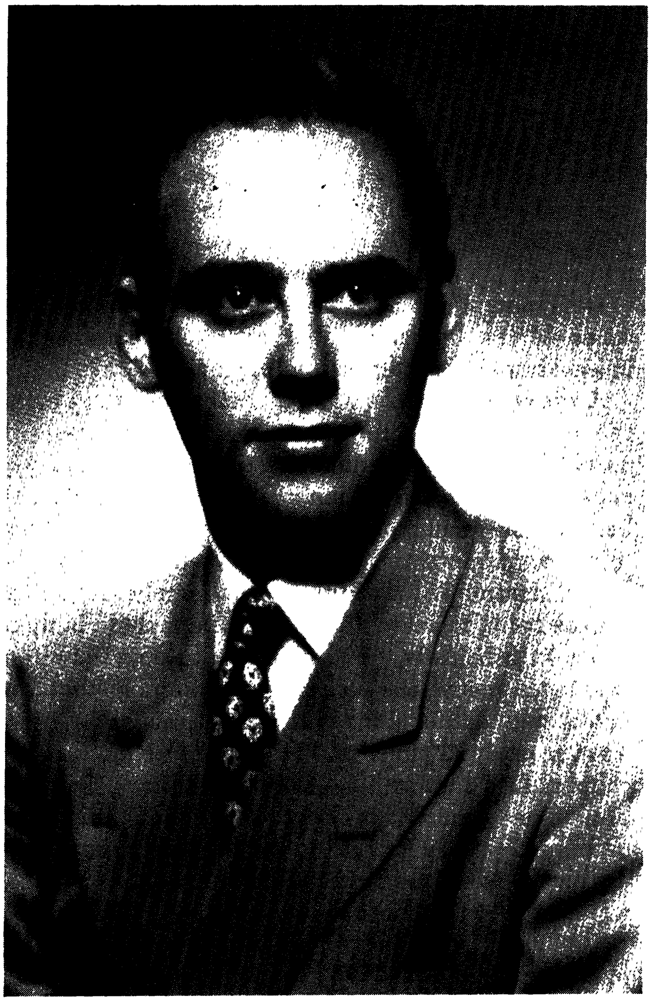
DeGroot: All this was done at Princeton?

Anderson: This was all done at Princeton, yes.

DeGroot: And was Wilks involved?

Anderson: Wilks was directing these projects. He was on the Applied Mathematics Panel of the National Defense Research Committee. So he was very much in on the statistical research and operations research that went on. There was a group at Princeton called the Princeton SRG (Statistical Research Group) that included Bill Cochran, R. L. Anderson, Alex Mood, Will Dixon, and others.

DeGroot: A high-powered group.



T. W. Anderson, 1940

Anderson: Yes. There was also another group at Princeton with John Tukey, Charley Winsor, George Brown, Merrill Flood, and some others. So there was a lot of activity in statistics, and I got a good education out of that. The training I had had before was strictly mathematical statistics and here, in many cases, I had to come up with some actual useful results.

DeGroot: You've been pursuing that vein of solid theory with an eye toward applications ever since.

Anderson: Yes, I think that experience helped me keep the purpose of applications in mind—that and an interest in economics. Also during that period I participated in seminars of Oskar Morgenstern in mathematical economics. At that time John von Neumann and Morgenstern were writing their book on the theory of games and economic behavior, so I still kept up a little tangential interest in mathematical economics.

DeGroot: You never got caught up in the decision theory movement that grew out of the theory of games?

Anderson: Not very much. That comes in in the classification problem in multivariate statistics and, of course, in general statistical theory, but I didn't follow that up. I guess I could have been in on the theory of games from the outset, since it was being developed right there.

DeGroot: And decision theory, too. [Laughs]

Anderson: And decision theory at Columbia, that's right.

DeGroot: Who was in the new department at Columbia when you arrived?

Anderson: The new department consisted of Wald and Jack Wolfowitz, and I was the junior member. The first year, 1946–1947, Jerzy Neyman visited for the fall semester and Joe Doob for the spring semester. Wald invited both of them to take permanent appointments at Columbia, but Neyman wanted to go back to Berkeley and Doob wanted to go back to Champaign-Urbana. I had been awarded a Guggenheim Fellowship and took leave for the year 1947–1948 in Stockholm and Cambridge, the second year of the department. Some of my teaching was taken over by Howard Levene, who continued as a department member. Then there were other visitors: Loève, Bose, Pitman, and Roy, I think. Henry Scheffé was added to the department in 1948. But the department didn't keep that structure very long because Wald was killed in a plane crash in December 1950.

DeGroot: Were you there at that time?

Anderson: Yes, I was there. In fact, I was the acting chairman of the department. It was really tragic because both Wald and his wife were killed, and they had two small children. We were very concerned about the children, as well as about the department. Then Wolfowitz decided to go to Cornell in the spring of that year, so that made a crisis in the department—well, aggravated it. The Columbia department had a series of crises.

DeGroot: I'd be interested in hearing about them.

Anderson: Well, after Wald's death there was a question of what the university would do about the department. I met with the Vice President and Provost, Grayson Kirk, and John Krout, Dean of the Graduate Faculties. Eisenhower was the President of Columbia then, but he didn't concern himself with such mundane questions as departments. The university decided that it would continue the department and get some new personnel, but in the spring of 1950, Wolfowitz decided to go to Cornell. So that made for more difficulties. But somehow we struggled through and persuaded Howard Raiffa to come to Columbia. Then, a couple of years later, Henry Scheffé got an offer from Berkeley, and when he decided to go we were fractionated again. The outcome of that crisis was to bring Herb Robbins to Columbia, although that was kind of nip-and-tuck.

DeGroot: In what way?

Anderson: Well, Robbins was at the Institute for Advanced Study that year, in 1952–1953. We brought him to Columbia, and he met with some of the administrators. Late in the spring when we pressed him to commit himself, he said "I can't decide now, I have a ticket on such and such a ship to England, leaving next week. If I don't go I'll lose my payment." The upshot was that Herb Solomon, who had come to Teachers College, went to the travel agent and cancelled the ticket and brought back the deposit. So Robbins went to Columbia instead of to Europe. [Laughs] With Robbins we did manage to rebuild the department.

Things went along until in the 1960s, when Herb Robbins spent time on leave at Minnesota and Purdue. I think he had offers from both of those places and, in addition, the University of Michigan. When he resigned at that point, I was pretty discouraged about the future of statistics at Columbia. Then I accepted the offer at Stanford and left Columbia in 1967. Fortunately for Columbia, Robbins came back and built up the department again. You know, we had these crises so often. I remember going in to see the Provost, Jacques Barzun, once with one of these crises—I was chairman or acting chairman off and on during all of this period—and at the end of the conversation he said to me, "Well, Ted, you'll be glad to know that we are not thinking of abolishing the department." That was the nicest thing he could say to me.

"IT WAS AN EXCITING TIME, BUT I'D RATHER HAVE IT A LITTLE CALMER"

DeGroot: Were you at Columbia during the student turmoil in the 1960s?

Anderson: No. I left in 1967 and the turmoil was in 1967–1968. The students occupied five buildings, including Fayerweather Hall where Mathematical Statistics was located. I had left the manuscript for my time series book with my secretary there in an office on the second floor, which is actually underground, and she told me later that the students used that as one of their rooms for headquarters. If I had known that at the time, I would have been pretty anxious about that manuscript.

DeGroot: The manuscript wasn't harmed though, I take it.

Anderson: No. I went to England for the year, and I spent one month in Paris. I was invited to be a Professor of Statistics at the Sorbonne. I thought that May in Paris would be beautiful. But that turned out to be just the time that the students were rioting in Paris. So I missed the one at Columbia, but I got into it in Paris. And then when I came to Stanford, I got into it at Stanford. Somehow or other the students were a little bit later here, so in 1968–1969 and also in 1969–1970 there was a lot of student unrest. I guess a lot of us got into it. But Columbia must have been more severe than most. The faculty were split and a lot of hostility grew up. So I was lucky to avoid that. I must say that before I left, I sensed some unrest and animosity among the students. In 1965–1966 and 1966–1967, the students were expressing their dissatisfaction a lot more than they had been.

DeGroot: Perhaps that wasn't particularly because of the university, but rather because of the international and national political situation?

Anderson: Well, it was partly that but it was also the university. There was a lot of dissatisfaction, particularly with the administration. There was a gap between the administration and the faculty, and a gap between the faculty and the students. And it was hard to get complaints and proposals taken care of by the administration. So I think it was largely Columbia. Maybe being in New York City, where there is more going on politically, had something to do with it too. But there wasn't the cohesion at Columbia that you had at many other universities. So it was different. It was an exciting time, but I'd rather have it a little calmer. At Stanford, the window in my office was broken one time. I think that the students must have been on their way back from one of the buildings with big picture windows, and must have had a few stones left over that they didn't want to waste.

DeGroot: You don't think it was anything against multivariate analysis?

Anderson: I don't think so, no.

"FROM TIME TO TIME PEOPLE DISCOVER THAT PAPER"

DeGroot: Tell me a little about your boyhood and your family. Are there other mathematicians or economists or statisticians?

Anderson: No, my father was a minister, an educator, and my mother was interested in music and art. My sister, Jane, who is five years younger, majored in mathematics at Northwestern and worked for a while at an advertising agency; she married and raised a family and developed other interests. My brother, Dan, who is eight years younger than I, was not academically inclined. He was in the Navy for a while during the war, and then he finished up at Northwestern and went into business, actually selling. My children have been more intellectually oriented; my son is into computer software and my two daughters study psychology and medicine.

DeGroot: Who do you feel were the major professional influences on your career and on your work?

Anderson: I'd say that at the outset Harold T. Davis was an important influence. Then Wilks had a

W. G. Cochran, T. W. Anderson, S. S. Wilks, and J. W. Tukey at a panel discussion on television in 1963

big effect on my directions; I think I got interested in multivariate statistics because he had done a lot in that field. And Wilks' point of view was to formulate a statistical problem in a pretty rigorous mathematical fashion, solve the mathematical problem, and then use that for applications. I think that my own emphasis on solving mathematical problems in statistics probably stems from his influence. I went from there to the Cowles Commission, and I would say that the point of view there of Koopmans and Marschak was rather similar. The problems came from economics but we did try to get rigorous mathematical solutions after the problems had been stated properly. And, of course, Wald had that point of view. I always thought that Wolfowitz went too far in the direction of making the mathematics in the problem perhaps more important than the statistical objective. Wald had also made contributions to multivariate statistics, so it was easy to discuss multivariate problems with him.

DeGroot: What were some of those contributions?

Anderson: Well, he had a paper on a classification problem which kind of got bogged down in a difficult integration (*Ann. Math. Statist.* **15** 145–162, 1944). Then his paper on maximum likelihood in the *Transactions of the American Mathematical Society* (**54** 426–482, 1943) is very general, but it formed the basis of a lot of the asymptotic theory and that was useful in multivariate analysis.

DeGroot: Let's talk about your papers. Are there papers of yours that you particularly like or feel were particularly influential or particularly enjoyed doing?

Anderson: Those are somewhat different aspects.

DeGroot: Right. OK, let's start with papers that you feel were particularly influential.

Anderson: Well, the area that I mentioned before of discriminant functions and tests of rank, and the estimation of matrices of means or regression matrices of specified rank, involves a number of papers. I think that's an area where I have made important contributions and had an effect. ("The noncentral Wishart distribution and certain problems of multivariate statistics," *Ann. Math. Statist.* **17** 409–431, 1946; "Estimating linear restrictions on regression coefficients for multivariate normal distributions," *Ann. Math. Statist.* **22** 327–351, 1951).

DeGroot: This is the area that grew out of your dissertation?

Anderson: Yes. Then at the Cowles Commission, Herman Rubin and I developed the limited information maximum likelihood method for estimating coefficients of a single equation. That led subsequently to the development of the two-stage least squares procedure which has been used a great deal in econometrics ("Estimation of the parameters of a single equation in a complete system of stochastic equations," *Ann. Math. Statist.* **20** 43–63, 1949). Another aspect of that work was the multivariate components of variance model which I had worked on early in the game, put aside, and then came back to. I reviewed a good deal of that material in the 1982 Wald lectures ("Estimating linear statistical relationships," *Ann. Statist.* **12** 1–45, 1984). It's also related to factor analysis, to which Herman Rubin and I made contributions ("Statistical inference in factor analysis," *Proc. Third Berkeley Symp. Math. Statist. Prob.* **5** 111–150, 1956). I consider that to be all one area where I've put in a lot of my time and effort, and I think it's had some effect. Related to that, which also is part of my dissertation, is the noncentral Wishart distribution. Abe Girshick had independently arrived at some of the same results, which we then published in a joint paper ("Some extensions of the Wishart distribution," *Ann. Math. Statist.* **15** 345–357, 1944). I think that the noncentral Wishart distribution was a forerunner of work by Alan James and many others in obtaining noncentral distributions. The later development depended on deriving zonal polynomials or some expansion of symmetric functions of that kind.

DeGroot: The development of zonal polynomials flowed from your work?

Anderson: Yes, I think it did in a sense. A few years after I left, Alan James wrote a dissertation at Princeton on group methods in multivariate analysis, extending our results on the noncentral Wishart distribution. Closely related was the dissertation by Carl Hertz on matrix hypergeometric functions; his adviser was Salomon Bochner with whom I had discussed my work. A little later James developed zonal polynomials. It's used in getting more general noncentral distributions, such as the Wishart distribution, the distribution of roots of determinantal equations, and so on.

Another area that I have worked on quite a bit in time series analysis has to do with autoregressive models. When R. L. Anderson was at Princeton during the war we talked about serial correlation, which was the subject of his dissertation, published in 1942, and we wrote a paper together in which we considered the circular serial correlation using residuals from fitted trigonometric functions (*Ann. Math. Statist.* **21** 59–81, 1950). That led to a paper on the theory of testing serial correlation. I was in Sweden at the time, and I remember having this idea of applying Neyman-Pearson theory and some algebra to put the concept of testing dependence in time series on a rigorous basis. I developed that while I was at Cramér's Institute in Stockholm. I enjoyed doing that paper because it just moved along very smoothly. Within a month I had proved the theorems and written the paper, and made a nice package. I published it, because I was in Sweden, in *Skandinavisk Aktuarietidskrift.* (**31** 88–116, 1948). As a result it didn't get a great deal of attention. [Laughs] So from time to time people discover that paper. The paper showed that if you had errors which were not independently distributed but you had a regression situation, and if the regressor variables are characteristic vectors of the covariance matrix or linear combinations of them, then least squares was the same as weighted least squares. And that has been picked up and written on by a lot of people since then.

More recently, in econometrics, I have been writing on distributions of estimators in simultaneous-equation models and asymptotic expansions of them. That's been a series of quite a few papers (ANDERSON, T. W., KUNITOMO, N. and SAWA, T., "Evaluation of the distribution function of the limited information maximum likelihood estimator," *Econometrica* **50** 1009–1027, 1982; ANDERSON, T. W., "Some recent developments on the distributions of single-equation estimators," *Adv. Econometrics* 109–122, 1982; ANDERSON, T. W., MORIMUNE, K. and SAWA, T., "The numerical values of some key parameters in econometric models," *J. Econometrics* **21** 229–243, 1983). Yesterday, I talked about another one at the econometric seminar here at Stanford.

"I WISH THE INTERVAL HAD BEEN SMALLER THAN 26 YEARS"

DeGroot: Let's talk about the new edition of your multivariate book which was recently published about 25 years after the first edition (*An Introduction to Multivariate Statistical Analysis*, 2nd ed. Wiley, New York, 1984).

Anderson: That's right—26 years. From 1958 to 1984. I wish that interval had been smaller because, for one thing, the book did call for revision for quite a long time and, secondly, it's a lot of work to fill in gaps over a period of 26 years. I think the first edition summarized what was known and generally accepted in the area of classical multivariate statistical analysis based on the normal distribution, and as a result of having the exposition available many statisticians worked on problems that were pointed out by the book: problems of developing other tests, particularly other invariant tests; problems of distributions of test statistics; asymptotic distributions; asymptotic expansions; comparison of powers. So I think that there have been hundreds and maybe thousands of papers written on subjects which were related to the first edition. When it came to the second edition, it was a matter of selecting what material to put in. I couldn't add everything that was new on the topics that came into the first edition. So I have kept the same list of chapters, the same organization, and the same point of view, but I've brought it up to date to the extent I could within the limitation of 700 pages. One new approach that was not available at the time of the first edition was the shrinkage or Stein estimation procedure, and other developments stimulated by that new point of view. That is closely related to the Bayesian approach and to empiricial Bayes theory. So I've now included Stein estimation of the multivariate mean, and Bayesian estimation of the mean and also of the covariance matrix. That's a little different direction from just straightforward developments from the first edition.

DeGroot: You haven't moved into the discrete multivariate area?

T. W. Anderson, 1983

Anderson: No, I think that discrete multivariate is a whole new ballgame. There have been a lot of developments in that area, but it seems to me that the mathematical techniques and some of the statistical problems are rather different from that of continuous variables. So I continued essentially to base the mathematical problems on the assumption of a multivariate normal distribution. There is another new direction that I didn't include, and that is to consider elliptically contoured distributions. Those are similar to the multivariate normal distribution, but instead of having a quadratic form in the exponent, one has some other function of the quadratic form. That is a new area of multivariate statistics which I think is an interesting development, although I don't know if it has had very much impact on applications yet.

DeGroot: Might we expect a new edition of the time series book? (*The Statistical Analysis of Time Series.* Wiley, New York, 1971.)

Anderson: Yes, I'm working on that. The time series book was different in that at the time I wrote the multivariate book, there was a body of theory and techniques that was pretty well accepted and was ready to be used; in the time series area that wasn't the case when my book came out in 1971. There's been big development in modeling with autoregressive moving-average processes and statistical techniques such as forecasting that go along with it. And I think that even now the modeling and the methods in time series are not as unanimously accepted as in the multivariate field. So I think that in time series you can't write in a definitive fashion. The field is still going to have a lot of changes in it.

DeGroot: So in the new edition there is still a question of selecting which topics and approaches to include.

Anderson: Well, I expect to develop the moving-average and autoregressive moving-average model and techniques more. Time series has the time-domain approach and the frequency-domain approach. The time series book includes both of them, and I expect to revise it with a balanced emphasis on the two approaches. I think that now it is possible to tie them together more than I could in 1971. But I do not expect it to take 26 years for the second edition of *Time Series.*

DeGroot: That's good news. Do you have other books or major projects in the works?

Anderson: I think that's enough. [Laughs] But I also have an elementary book, *An Introduction to the Statistical Analysis of Data* (Houghton Mifflin, Boston, 1978), which I am revising. Houghton Mifflin has let the book go out of print and Scientific Press is taking over, so this is an opportune time to make some revisions. I hope to have a new edition of that out next year.

DeGroot: Good. That's the book with Stan Sclove?

Anderson: Yes. And I have a number of research papers to finish up.

DeGroot: I'm sure you do. I was going to ask you about your current research.

Anderson: Well, I've written a paper with Akimichi Takemura on maximum likelihood estimates in moving-average models. There is a positive probability that the estimate comes out to be exactly a noninvertible value. At first glance it seems a little surprising that you have a positive probability of a single value when the parameter can take on a continuum of values, and secondly, that it's a value that in some sense you don't really like to have. We've been investigating that and we haven't got all the answers, but it's been an interesting problem. Another area that I've continued to work on is the linear structural relations model and multivariate components of variance. Ingram Olkin and I have done some work together on that. I'm also continuing to be interested in the techniques of estimating the coefficients in moving-average and autoregressive moving-average models. I don't think the last word has been said there. I'm doing some work with Raul Mentz on that.

"A FINAL OBJECTIVE IS TO HAVE AN EFFECT ON ANALYZING DATA AND DECISION MAKING UNDER UNCERTAINTY"

DeGroot: What is your assessment of the present state of health of the field of statistics? Where do you see the field heading, and where do you think it should be heading? I hear a lot of people comment that computer science is sweeping up everything in sight and that statistics is in a decline as a field.

Anderson: Well, my impression is that computer science does compete for personnel with statistics, particularly with mathematical statistics. And it competes also in problems that are tackled. I keep hearing that artificial intelligence is going to solve lots of problems that I would expect statisticians to be solving. But I think that statistics has a firm base. You know there was a time when operations research seemed to be a pretty serious competitor. But I think that operations research has found its problems—queueing, linear programming, and so on—and statistics still has the area of analysis of data and model building where there is randomness or statistical errors. Statistics is making use of new computer facilities both to put into practice the statistical methods that have been developed and to point the way toward new statistical methods. Monte Carlo techniques have been used a lot in studying statistical procedures, but I think computers are going to make available new kinds of statistical techniques such as the jackknife and bootstrap and other techniques requiring a lot of computation.

It is important for the mathematical or theoretical side of statistics to be tied to, or at least stimulated by, the applications. I have been concerned about the Institute of Mathematical Statistics; it has not been growing in membership. I think that we who consider ourselves mathematical statisticians do need to pick up the possibilities of computers and to keep in mind the applications which are really the end objective of research in statistics. While I find intellectual interest in mathematical statistical problems, I think a final objective is to have an effect on analyzing data and decision making under uncertainty. I think that statistics is here to stay, but we have to adapt our problems and directions of research as circumstances change.

Another stimulus to statistical methodology and theory is the huge increase in collection and storage of data. There are just more numbers to analyze, and the scope and complexity of questions asked of the data grow. Among other things, our customers are getting more sophisticated.

DeGroot: Do you still see as much of a division between applied and theoretical statistics as perhaps there was at one time?

Anderson: I think there has always been that kind of division. When I was a graduate student, a lot of readers were complaining that the *Annals of Mathematical Statistics* was too hard to read because it was too mathematical and too theoretical. Wilks was editor of the *Annals* for 12 years and he mentioned these complaints. At that time the kind of students that Wilks had and the kind of training he gave them were more on the mathematical side, and there were objections from the other camp. This division occurred also in econometrics. The Cowles Commission was using probability models and rigorous statistical inference; and the National Bureau of Economic Research represented the other side, where you collect data and graph it and so on, but you don't have a model in mind. There was a volume written by Wesley C. Mitchell and Arthur F. Burns called *Measuring Business Cycles.* Tjalling Koopmans wrote a blistering review of it called "Measurement without theory," and that was a very pointed version of this conflict between econometricians who wanted to have a mathematical basis for procedures and those who used methods that didn't have a theoretical basis at all.

DeGroot: It sounds analogous to the debate about data analysis versus models in statistics today.

Anderson: Yes, it is. Incidentally, last year I was the Wesley Clair Mitchell Visiting Professor of Economics at Columbia. That gave me great pleasure, to think that I had been turned down by the National Bureau of Economic Research at one time and now I had this named professorship. You know Mitchell had been long-time director of NBER. [Laughs]

DeGroot: But do you have the impression that these days we are training students more in both theory and applied statistics, or do you think the distinction still persists?

Anderson: I think the distinction still persists. At Columbia the program in statistics had been developed in the beginning by Hotelling and Wald, and it was required that the student have a minor, some field such as economics or engineering or zoology to which he could apply statistics. Well, we have given up on that. There is now so much statistics to learn that we can't ask our students to learn another field, even in a superficial fashion. But I think that a lot of students these days are getting some experience in consulting. We have a consulting program here at Stanford in which investigators around the university can bring in statistical problems, and we had that at Columbia too. The availability of computers means that students and faculty can run procedures more easily than they could before; it doesn't take as much work to do it. You have to learn a program, but you don't have to sit at a calculating machine for many hours. I think that makes a difference. There certainly is a lot of literature available—*The Journal of the American Statistical Association* and the *Journal of Econometrics* and many journals—where the theory and applications are combined. I find in econometrics, for example, that many econometricians are very well trained in statistics and mathematics. So they can hold their own in developing theory, but at the same time they are usually close to putting their methods to work in economics. I think that the same is probably true in other fields like biostatistics. You have more people who can communicate with mathematical statisticians as well as the practitioner in the field of application.

DeGroot: Did you get complaints about the *Annals* being too theoretical during your term as editor?

Anderson: Oh yes, I got that. I think everybody gets it. I also had complaints that we didn't have enough expository papers. We struggled for years with the problem of getting good expository papers.

"ONE ASPECT OF STATISTICS THAT I WOULD LIKE TO DEVELOP FURTHER IS THE USE OF COMPUTERS"

DeGroot: What do you do when you are not doing statistics?

Anderson: When I'm not doing statistics and the weather is right, I'm out playing tennis. When it gets a little bit warmer, I'll be swimming. In the other direction, I like to go skiing. The location here within driving distance of the Sierra Nevadas offers that opportunity. As a more passive activity, I enjoy going to the opera and the theater. Another outside interest is travel. This last August and September, my wife and I spent six weeks in New Zealand and Australia.

DeGroot: That sounds nice. Do you have future travel plans?

Anderson: Yes, I have plans now for going to India. I'll give the Mahalanobis Lectures at the Indian Statistical Institute in Calcutta in December. So we are planning on spending six weeks or so traveling around India. It has always fascinated me and I will make use of this opportunity.

DeGroot: Have you been to India before?

Anderson: No, I haven't. I missed the International Statistical Institute meetings which were held there a few years ago because they came during the academic term. Actually, I'm going to have to stop a little bit early in the fall quarter this year, but I decided that I wasn't going to pass up another chance to go to India.

DeGroot: Right. It sounds like it's worthwhile.

Anderson: I also think that within the next several years I'll go to South America and to Israel. There are a lot of places in the world that I haven't been and I want to catch up on some of them.

DeGroot: What else does the future hold for you?

Anderson: Well, as most of us do, I have lots of projects either under way or planned. I already mentioned the books that I am revising and the research papers that I am working on. One aspect of statistics that I would like to develop further is the use of computers. Computation facilities are so readily available now, and will do so much, that I think I want to exploit them in my research.

DeGroot: I don't hear any talk about retirement in there.

Anderson: Well, Stanford University will declare me Emeritus in 3½ years, so that's going to make a difference. It will give me the opportunity to visit other universities and try out some other environments. And it will give me more time for writing and research; since I have so many projects I really won't mind that. But I'll still expect to retain an office here and will have contact with my colleagues and students.

DeGroot: It sounds as though you regard reaching 70 as an opportunity to do more than ever before, over a broader area.

Anderson: Well, I've enjoyed doing mathematics and statistics and econometrics, so I certainly expect to continue. But it will also give me more time for some of my other interests.

DeGroot: Thank you, Ted.