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THE ET INTERVIEW: PROFESSOR JAMES DURBIN

Interviewed by Peter C. B. Phillips

Few institutions can be as proud of their tradition in quantitative economics as the London School of Economics and Political Science (LSE). This tradition has a long history but, to most of us, its recent cycle of development begins in the postwar period. In 1950, Jim Durbin joined a newly established statistical research unit at LSE and, in retrospect, it is clear that his appointment broke a new dawn for the LSE. With a wide background of interests in statistics and economics and an unusually versatile intellectual talent, Jim Durbin helped to move the LSE forward into what was undoubtedly a new era of quantitative research in the social sciences. By the 1960s it was apparent to many that the LSE was the place where it was all happening in econometrics, not only in research but also in teaching programs. Indeed, successive waves of students graduated with a special LSE pedigree that stood for the best in econometric training combined with a special interest and understanding of statistical time series. This combination has endured to the present and one of Jim's distinct legacies to the LSE has been the establishment and continuity of this intellectual tradition.

Jim Durbin's research has had an extraordinary impact on the applica-

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tion of statistics. All of his work has been guided by an unswerving principle of relevance, commencing with his famous collaboration with Geof Watson; and it has found empirical application throughout the social sciences. His contributions, more than any others, have helped to turn professional attention to the importance of diagnostic testing in regression. His early work on testing for serial correlation stands as an established landmark and his later work on the same subject has opened up fresh fields of research for econometricians on LM diagnostics and for statisticians on the use of empirical measures. To probabilists, statisticians, and econometricians alike, his work is inventive, stimulating and always fruitful for others.

In the English and in the international communities of statisticians, Jim Durbin is a distinguished and honored scientist. He is the recipient of the Bronze and Silver Guy Medals of the Royal Statistical Society. He served as President of the Royal Statistical Society in 1986–1987 and as President of the International Statistical Institute in 1983–1985. He has visited and lectured at universities on every continent. He has the honor of being a Fellow of the Econometric Society, the American Statistical Association, and the Institute of Mathematical Statistics.

On July 14, 1986, I spent a memorable day with Jim Durbin at the LSE. We recorded the interview that follows in two sessions that occupied the morning and most of the afternoon. Our conversation covered a vast range of topics and it is a testimony to Jim's eloquence and his diversity of intellectual strengths and interests. I hope that through this interview we can share Jim Durbin's presence with a wide community of scholars from different disciplines and, of course, with those of us who are especially interested in time series and econometrics.

You seem to have always had a wide diversity of intellectual and research interests. Does this go back to your early education? What were your main interests and intellectual strengths at school? Did you ever expect to become an academic or did you have thoughts of other possible careers?

I was something of an all-rounder at school, although one subject that I disliked was physics. I really didn't see myself as a potential scientist or engineer. But I was always good at mathematics and in the last couple of years at school I got much more interested in mathematics. So I got the idea that I would like to go to university and take a degree in mathematics.

You entered Cambridge University during the war years. This must have been a very difficult time to pursue a university degree. Would you tell us about your studies: the courses that you followed; the people that influenced you most; and what it was like to do a war-time degree?

Part of the reason for my lack of interest in physics was that I never, in fact, studied applied mathematics at school. In British schools, mathematics was divided into pure mathematics and applied mathematics. Applied mathematics was largely mathematical physics and I did not take any examinations in that subject in my last three years at school. Also, I missed out on the first term at Cambridge and so I only had five terms at Cambridge University in the mathematics courses—that's a year and a half. That was the extent of my undergraduate education. Cambridge University had a system of war-time degrees in which if one did five terms of study, completed the examinations that went with it, and then did four terms of national service, one qualified for what was called a war-time degree. I got a war-time BA on that basis. I had an interesting time as an undergraduate there because I was a member of St. John's College. My contemporaries there included both Denis Sargan and David Cox, who were both mathematical undergraduates at the same time. I performed relatively poorly in what was called applied mathematics, which is not surprising since I had not studied it at school. The subjects I was interested in, and did well in, were analysis and algebra, particularly matrix algebra, which was only just coming into undergraduate mathematics at about that time, that is, the 1940s. All mathematics students at Cambridge at that time had to go to a compulsory statistics course, but I regarded this as rather a chore and I took very little interest in it. It was mainly concerned with topics like moments, cumulants, and descriptive measures of frequency distributions, which didn't seem to me to be intellectually very exciting. Harold Jeffreys was there at the time, and I went to his lectures on probability theory. I had read his book on scientific inference and I also made an attempt at his book on the theory of probability. But he was rather a poor lecturer and so, although it was very stimulating to have been in the presence of greatness, in fact, I didn't learn very much from Jeffreys.

You did some work in descriptive statistics as an undergraduate. Did you begin to think that you might develop an interest in mathematical statistics at this stage?

No. My intention was to go back to Cambridge after the war and switch to economics. Wartime in Britain was really rather inspiring from some points of view. It was a time of great idealism and many people of my generation were starry-eyed about the opportunity to build a more just society after the war. We thought that part of this would be expressed as a shift of emphasis from the natural sciences to the social sciences and we believed that mathematically trained people could make a useful contribution towards statistical aspects of these activities. I thought I would take Part II of the Economics Tripos, which would take two years, and then become an economic statistician. So I was not thinking of mathematical statistics at all.

How did the transition to mathematical statistics occur?

The senior tutor at my college, a man called Guillebaud, was a lecturer in economics at Cambridge and he said to me that since I was a mathematician I shouldn't waste two years studying economics. He told me that Cambridge University was about to start a one-year postgraduate diploma in mathematical statistics and it was possible to specialize in economics within this diploma as an applied field. He thought that taking that would save me wasting too much time on economics! It's rather interesting that Denis Sargan was a fellow mathematics undergraduate in my own college and he had developed the same intention of going back to take Part II of the Economics Tripos and in fact did so. I don't know whether he missed out consulting with Guillebaud or decided to ignore his advice. I have occasionally regretted that I didn't do this.

Were you involved in any scientific or other war work during the war?

We had a committee on scientific manpower during the war and the chairman of that committee was the novelist C. P. Snow, later Lord Snow. The secretary of that committee was a man called Harry Hoff, who wrote a rather brilliant novel under the name of William Cooper. So there were two famous novelists running this committee. I was interviewed by them and they suggested to me that I should go into statistical work. I asked to go into the services instead, because I didn't want to go into statistical work, which I thought would be rather unexciting. In fact, they put me into a unit called the Army Operational Research Group, where I worked on anti-aircraft and, to a lesser extent, coastal artillery during the war.

After the war you went back and did the diploma in mathematical statistics. Would you like to tell us about that?

I was allocated to Dick Stone as my supervisor in the economics part of the diploma. I went to see him before the course began and he had just returned from the United States with a pile of offprints on sampling. He was rather excited at that time with the idea of using sample surveys for collecting economic data. He suggested that, as part of the project that I had to do in my applied field for the diploma, I might do some work on sampling business enterprises. That was the origin of my interest in sample surveys.

What about other people who were involved as instructors in the diploma. Do you remember anyone in particular?

My supervisor for mathematical statistics was Henry Daniels, who is an inspiring teacher and has been, of course, a creative influence in mathematical statistics in Britain every since. Dennis Lindley had just arrived as a demonstrator. Wishart was the head of the statistics group there. Frank Anscombe was a lecturer. Bartlett had just left to go to Manchester immediately before I arrived. David Cox arrived there as an assistant lecturer the year after I finished the diploma. A constellation of stars.

After the war, in 1946, you were employed by the British Boot, Shoe and Allied Trades Research Association. What did your work with this organization entail?

What happened there was that after I had finished this work on anti-aircraft and coastal artillery about the time of the end of the Japanese war in 1945, I was not going to be eligible for release from national service for more than another year under the regulations prevailing at that time. So I went to the registry of scientific manpower and explained to them that my job was folding up in army operational research, and asked whether there were some other job that they could offer me. They had a job as a statistician in the British Boot, Shoe and Allied Trades Research Association. I really had not been working on statistics during the war and I had forgotten what I learnt at Cambridge, but nevertheless I took the job and I did some statistical work for them. When the suggestion was made that I should go back to doing mathematical statistics at Cambridge, it wasn't such a big culture shock to me because I had done a moderate amount of statistical work, although not very high powered, for the Boot and Shoe Trade. That was applied work. Frankly, I was just killing time to some extent. I didn't do any serious academic work during that year and a half, I was just waiting to go back to Cambridge.

Then, in 1948, you joined the new Department of Applied Economics (DAE) at Cambridge. Would you like to tell us about your time there and how your appointment came about?

Richard Stone was a very inspiring individual. He was quite a young man, only about thirty-six at the time. The creation of the Department of Applied Economics was due to Keynes. Keynes felt that there should be a department of quantitative economics at Cambridge and he met Stone during the course of Stone's war work. Stone worked with Meade, doing the first national accounts of Britain under Keynes' general guidance. Keynes had a great regard for Stone and he was very keen that the department should be set up and Stone should be the director of it. When I finished the diploma in mathematical statistics with Stone as my supervisor in economics, he offered me a job at the DAE which I accepted.

This must have been a very exciting period at the DAE. I expect you have lots of personal memories of the research environment there during those formative years, the people you met and worked with and the work that was being done. Would you like to share some of those memories with us now?

Young people, as we all know, tend to take whatever is happening to them very much for granted, as though it is the most natural thing in the world. But looking back on it, it was really a most remarkable, exciting, and creative period. When I went there, Guy Orcutt was there, for two years I think, working on time series problems. Don Cochrane was doing a Ph.D in econometrics and he was working with Guy on problems of the effect of autocorrelation on regression analysis. Although he was not a member of the department's staff, he was very often in there working with Orcutt, using the library, turning up at seminars and so on. So he was almost a part of the team. They were interested in these questions of regression analysis of time series data and so, of course, was Dick Stone himself. I had been to a course called "stochastic processes" as part of my diploma, which was given by Daniels, but was related to the lecture notes on stochastic processes that Maurice Bartlett had written while he was at North Carolina. There was no book on time series analysis at that time, but there were these lecture notes that Bartlett had prepared. They had a great deal about autoregressions, moving averages, autocorrelations, and all that. To some extent Daniels' lectures were based on these lecture notes of Bartlett. That was a very good and inspiring course. So I came into the DAE group as someone with a knowledge of mathematical statistics and a special knowledge of time series analysis.

Can you tell us about the people who passed through the DAE during the period you were there and the genesis of your joint work with Geoffrey Watson?

During that time on the permanent staff, Hank Houthakker arrived shortly after me from Holland, Mike Farrell arrived from Oxford, and Gerhard Tintner was there for a year. Larry Klein came for a short period and gave some seminars at the DAE. Ted Anderson was a visitor there for a six-month period, around 1948, and of course he was also a time series specialist, and contributed to the general intellectual climate there. I have mentioned Cochrane and Orcutt. Then in the summer of 1949, Geof Watson turned up from the University of North Carolina. Watson was an Australian from Melbourne, where he had already held a job as a very junior lecturer. Maurice Belz had started up a statistics group in Melbourne. He had recruited Watson and he wanted him to go to North America and get a Ph.D. So Geof went to what then was North Carolina State College and his supervisor there was R. L. Anderson. Now, R. L. Anderson had done a Ph.D. thesis on serial correlation and just at the time that Watson was doing his course work, R. L. Anderson and T. W Anderson had collaborated on a paper where they had worked out the distribution of circular serial correlation coefficients, calculated from the residuals of regressions with Fourier regressors. R. L. Anderson had suggested to Geof that he might consider doing something for general regressors as a topic for a Ph.D. thesis. Watson, with his Australian background, was quite keen to come to Cambridge, where a number of his fellow students from Melbourne had gone for highest degrees. At that time it was a tradition for Australian science students to come to Britain for postgraduate work. Dick Anderson had met Dick Stone in the United States and he had asked whether Watson could come and work at the Department of Applied Economics. All of this illustrates the crucial influence that chance personal encounters have on the development of academic research.

Geof turned up some time around the summer of 1949. I had already been interested in the testing of serial correlation because of the work that Stone, Cochrane, and Orcutt were already doing. They were using the von Neumann statistic and we knew the procedure was not exact for regression residuals. With this background it was natural for Geof to talk to me about these matters. We had some preliminary discussions and decided to work together on the problem. In fact, we got the basic idea of the bounds test really quite quickly. We used to work together in the same room by using the blackboard and talking. Then we would go away and think and come back and talk again. It must have been in something like three weeks that the ideas came together—that's my recollection. Although we saw early on that we couldn't get the exact distribution, as we understood it at the time, of a serial correlation coefficient calculated from regression residuals, we thought we might be able to get bounds. That was the basic idea and we soon began to see how it could be done.

So the concept behind the bounds test came quite easily?

It came fairly quickly, yes.

The mathematical details, I suppose, took a little longer?

A few months. In fact, we only worked as colleagues in the same department for about six months. Then I went on to the LSE. But, after that, we did meet from time to time, either in Cambridge or in London and that was when we wrote the papers.

In the second paper, you reported tables which people could use in their empirical work. This is what ultimately helped applied researchers. Was it very difficult at that time to do these numerical computations?

We thought at the time that they were horrendously difficult calculations, but one of the assets of the Department of Applied Economics was that we had a room there with perhaps eight or ten young ladies operating desk calculators, supervised by an older lady of forbidding demeanor. They did the computing. Geof and I had to set up the calculations and decide what should be done. But then, of course, I moved to London and so he had the job of the day-to-day supervision. We were very concerned about the accuracy of these tables, as everybody was, doing computing in those days. What we tried to insist on was for the girls to do all calculations twice. But, of course, this was rather boring from their point of view. To some extent you had to play a game when you were organizing this type of computing, in getting the right amount of checking done and getting it done properly. We ourselves, although neither one of us liked computing, had to do some checking of those tables. There was always a doubt in our minds whether the tables really were accurate to the order that we claimed they were. Years later when electronic computers became available, we thought we really ought to recalculate them. When Geof came to spend a year in Italy in 1967–1968, we had an initial discussion in London about cooperating on this task. However, on returning to Italy he found waiting in the mail for him a set of tables recalculated by Koerts and Abrahamse, in the Netherlands, in which they had calculated the tables to three decimal places. We had only calculated to two decimal places but fortunately our tables were accurate to the order that we claimed. So we were very relieved about that.

Twenty years later you came to collaborate again with Geof Watson to write the third paper on the subject for *Biometrika*. How did that come about?

Well, that was a fun thing. Originally, we had this idea that we would get together and recalculate the tables during the year that he was in Europe. The intention was that as well as recomputing the tables, we could do some checks on the accuracy of the approximations like the beta approximation. But when it turned out we didn't need to recompute the tables, we still thought we would like to write a joint paper on the other topics. We thought that, having written Parts I and II in 1950, it would be fun to write Part III in 1970 and maybe a Part IV at some other distant time in the future. But we never got around to Part IV.

You did use a somewhat different approach to the subject in Part III, relying on the theory of invariance. Was this something of a departure from Parts I and II?

Yes. We had convinced ourselves that the choice of test statistic that we had made in 1950 was correct, that among the lag one coefficients, the d statistic was a good statistic to use. Geof was particularly interested in invariance and it was a rather fashionable thing in the 1960s, I think mainly because of the influence of Eric Lehmann. One doesn't hear so much about it today. So we thought it was quite important to check out whether this was a good statistic from the standpoint of invariance. I think we thought it was an interesting academic exercise, but we didn't really attribute all that much practical importance to the invariance aspect. It was power and other aspects of the performance which were more important to us.

Looking back now the Durbin–Watson test has had an extraordinary impact on the profession, particularly in applied econometric work. It also seems to have undergone a recent revival as an exact diagnostic test (i.e., with exact critical values computed by numerical integration); and it seems to have power properties that are difficult to beat, even for alternatives other than AR(1) errors. Has the continuing interest over 35 years in tests of serial correlation been a surprise to you?

No, it has not been a surprise, because I've always thought it was really quite important to carry out diagnostic tests. Certainly in econometric applications and other applications of regression analysis to time series data, I think it is important to check out whether the assumptions on which inference is based are satisfied. In fact, both Geof Watson and I always intended to develop these ideas further. In 1950, we considered the question as to whether one could get out good procedures for higher-order lags and we concluded at the time that the mathematics was too intractable.

In 1950, you moved to the London School of Economics and Political Science. What attracted you to LSE?

The job I had at Cambridge was only a short-term one, financed by short-term money, and by that time I had already got the idea that I thought I would like to be a university teacher of statistics, mainly because I thought that the long holidays would give me plenty of time for mountaineering which at that time interested me more than academic research. Maurice Kendall had just been appointed as Professor of Statistics at the LSE. There was a long statistical tradition at the LSE—in fact Bowley started teaching statistics there in 1895. LSE already had Roy Allen who was a Professor of Statistics interested in applications. Kendall was brought in as a theoretical statistician. As part of the deal he obtained an extra post for a lecturer. So he wrote to Daniels at Cambridge and asked whether there was anyone there he thought might be suitable for this lectureship. Daniels suggested me and I got the job without any formalities whatsoever. At that time I didn't see any long-term future for me at Cambridge which already had a statistical group staffed by people who were already world famous statisticians. I didn't see any opportunities there. Here was an opportunity in London. I had worked for a time in the Army Operational Research Group in London and the prospect of working in London again for a few years rather appealed to me. Of course, I had no intention of staying permanently, I thought that I would stay here a few years and then move on. But I have remained here ever since. Whether that was just inertia or because no better job was available I will never know.

From what you say, there was already a small group of statisticians at LSE when you joined the department.

Yes. Kendall also got some money from the Leverhulme Foundation to start up a small research unit, which he called the Division of Research Techniques, and in this he had two research assistantships available. One of those he filled with a young chap who had just graduated as a statistics specialist at LSE. His name was Alan Stuart. So Kendall offered Stuart a job as a research assistant at about the same time he offered me the job as assistant lecturer. Alan has, of course, done prodigious work in collaboration with Maurice, and subsequently with Keith Ord, also an LSE graduate, on the advanced theory of statistics. It has always intrigued me a little that Kendall offered these jobs to Stuart and me because we were the first two people to turn up, not because of any discriminating search or selection procedure.

In your early years at LSE you started to develop interests in a wide number of fields, including estimation theory, rank correlation, and sample surveys as well as time series and econometrics. Was this a conscious choice to develop a diversity of interests or were other factors at work in determining your interests?

I was in a department of statistics and we thought that we had to teach statistics over a wide field. I knew that sample surveys were going to be increasingly important in the applications of statistics in the social sciences. I had become interested in sample surveys because of the influence of Dick Stone and so one of the things that I offered to do when I arrived at LSE was to teach a course on sample survey theory. In fact, that course, together with elementary statistical methods, were the first courses that I gave here. I continued lecturing on sample surveys for a number of years before the course was taken over by Alan Stuart. He and I shared it for a time. We would alternate every few years and other people took it over subsequently. Then I thought it was my job to cover other topics here as our teaching developed like the analysis of variance and linear models and so on. So, whereas previously in the Department of Applied Economics I had felt myself to have a special loyalty to economics and econometrics, here I felt I had a more general responsibility to statistical theory.

So in your early days as a teacher at LSE you found yourself teaching and doing research in a variety of areas?

Yes. And the other thing was that I wasn't teaching time series in those early years because this was a course that was taught by Maurice Kendall. He was an international authority in the field; he wanted to teach it and he was the boss!

In the early fifties you wrote a paper on errors in variables and instrumental variables estimation. In its own way this has been rather an important paper. Do you remember writing the paper and the circumstances that led to it?

There were really three circumstances. First, Dick Stone had been interested in Frisch's work and through him I had some knowledge of Frisch's work on confluence analysis. Secondly, Maurice Kendall in 1950 or 1951 wrote a paper on aspects of regression and he discussed some of the underlying problems with me. During those discussions I got some ideas that I wanted to develop. Thirdly, I was asked to give a talk at the European meeting of the Econometric Society in Innsbruck in 1953, so I decided to give it on these topics. Present at the talk was Gil Goodswaard who at that time was editor of the *International Statistical Review*. He asked me to write up the talk and publish it in the *Review*. I polished it a bit more and put in a few more things. It was really accidental that I happened to publish that paper. It was essentially the write-up of the talk I gave at the conference.

Historically, that paper has turned out to be important because it contains a test which was rediscovered about 25 years later by Jerry Hausman in 1978 and which is now known as the Hausman test for exogeneity. Have you followed that literature?

Only vaguely. I've not had a research interest in following it up. But people from time to time have talked to me about it. And, of course, I was interested in Denis Sargan's work on errors in variables. I felt I really ought to do a bit more work in that field, but it fell by the wayside.

Perhaps you would like to tell us about your early years at LSE in the 1950s and the tradition in time series and econometrics that developed during that period?

Econometrics had been taught as an undergraduate course at LSE since the early 1950s, I think, in the first instance by people like George Morton and Wilfred Corlett. Roy Allen also had an interest in econometrics. Bill Phillips came on to the academic staff of the school at about the same time in the economics department. With his background in electrical engineering, he was very interested in the idea of applying control theory and quantitative techniques for economic policy analysis and so he also became interested in econometrics. At some time in the mid-1950s, Kendall and Phillips got together and put up a proposal to the Ford Foundation for a research project in econometrics which was accepted and in which I became involved. We needed somebody to come as a full-time research worker in time series analysis and we succeeded in bringing Maurice Quenouille to the school. He was very active in those early years and I think he wrote the book on multiple time series, which has become historically an important book, during that period. Phillips and I both began teaching econometrics, he in the economics department and I in the statistics department. I began a graduate course, mainly for graduate students in statistics, called Advanced Statistical Methods for Econometrics. This was in the early 1960s. This was a time when the LSE was expanding and so there was a possibility of getting new posts. Bill Phillips and I cooperated in getting two new posts at the readership level at the school: one in the economics department and one in the statistics department, both in econometrics. Rex Bergstrom took the post in the economics department for a time and we persuaded Denis Sargan to come from Leeds to the post in the statistics department. Soon afterwards Bergstrom left and Denis migrated to the economics department as a Professor of Econometrics. So since the early 1950s, we have had a tradition of econometrics teaching and research in time series oriented towards applications in econometrics that has continued to this day. And this is part of the explanation why research students in econometrics that have passed through the school have inherited a strong time series tradition.

At what stage did you become actively involved in teaching time series at LSE?

I think this must have been some time in the mid 1950s. Although I already had this interest at Cambridge, Maurice Kendall taught the subject for the first few years I was here. When he wanted to move on to something else he handed over the time series teaching to me. Maurice Quenouille was in theory employed as a full-time research worker but he did some teaching and he and I cooperated in teaching time series analysis for a time. Later, David Brillinger joined the department for five years or so and he and I shared the time series teaching.

Did you have a graduate time series course at that stage? When did the M.Sc program begin?

We didn't start the M.Sc program until the 1960s, but before that we used to give what we called *joie de vivre* courses at the graduate level. These were voluntary courses given by individuals on topics that interested them and we used to put pressure on all of our graduate students to come. We also began to get students in the economics department, stimulated by Phillips in the first instance, coming to some of these statistical courses.

Quenouille's period at the LSE seems to have been rather productive. He wrote the multiple time series monograph and developed the theory of the jackknife, which is a topic you yourself became interested in towards the end of the 1950s.

Yes, I wrote one paper on the jackknife which appeared in *Biometrika*. I wrote that specifically arising from an earlier paper by Quenouille. Quenouille showed that in general the asymptotic variance of a jackknife estimator is not larger than that of the standard estimator. So one did not pay a price asymptotically in increasing variance by using the jackknife procedure. I wondered whether one could find a finite sample case where the variance was actually reduced. More generally, I wondered whether if one carried out an asymptotic expansion one might find out that higher-order terms could indicate a variance reduction. Because of my interest in sample surveys, I was very familiar with the theory of ratio estimation in sample survey theory, so I thought that I would look at the jackknifing of a ratio estimate and that's what I did in that paper. The results were interesting to me because they convinced me and also Quenouille that one need not pay a heavy price for the reduction of bias by jackknifing. This did not seem to me an intuitively obvious result.

There has been new interest recently in the systematic development of statistical procedures of this type, jackknifing and bootstrapping. Is this an area in which you have done any recent thinking yourself?

No. I did something somewhat similar to bootstrapping after the *Biometrika* paper, but it turned out that in those days the computing problem was really rather severe. I had a research assistant who did some work for me, but I never published it. The idea was that if you wanted to get an improvement on the asymptotic behavior of a maximum likelihood estimator, one way of doing so would be to simulate the estimation procedure, assuming that the true values were replaced by their maximum likelihood estimates. Then one would study, by simulation, the behavior of the maximum likelihood estimates, and this would lead to a correction to maximum likelihood to remove bias, for example, or to improve knowledge of the distribution of the estimator. In this way one could approximate the exact behavior of the maximum likelihood estimator. I had quite a lot of calculations done on this and the results looked promising, but the computing was so hard, that I got discouraged. There were some interesting problems in the design of the simulations on which I was able to make some progress, but I never published the work because I thought that the computing wasn't really practical for applied work.

It is a shame in a way that this was never pursued, although people seem to have been using simulation to do similar things now that computers are so accessible and so cheap for large scale computations.

I think that many younger people don't realize the influence of computing on theoretical work in statistics. When I started my original work with Geof Watson, for example, we knew that if our work was going to be used by practical people, it had to be computationally very simple. And from time to time one would develop a line of work that started to become rather complicated, and so one knew that it would never be used in practice and so one used to give it up. I think that is what really happened to me with this paper on local Monte Carlo, as I called it. With the technological constraints on computing capability at the time it could never have been implemented in practice by applied workers. I lost interest after that and I have taken no real interest in jackknifing or bootstrapping since those days.

In 1963 you presented a major paper on econometric estimation to the Copenhagen Meetings of the Econometric Society. This paper gave the estimating equations for the FIML estimators in a new and revealing form which facilitated links with other estimation procedures like instrumental variables. Most econometricians hear about this paper by word of mouth and its results are often taught in econometrics courses. How did you come to write this paper? Like many people I often wonder how it was that it was never published. [Editor's Note: this paper is now published in the present issue of *ET*.]

The starting point of that paper was the course that I was teaching, which I have already mentioned, called Advanced Statistical Methods for Econometrics. I had started teaching the course some time around 1960 or so. Immediately after the Zellner–Theil paper on three-stage least squares was published I included it in the lectures. I thought that there ought to be a way of getting full information maximum likelihood out of that approach. I wanted to teach full information maximum likelihood in the course, but it seemed to me that the basic Cowles Commission presentation of this material was too complicated for a teaching purpose. So I developed the technique described in the Copenhagen paper from Zellner and Theil's approach really for teaching the subject. In those days I still had the feeling that in writing a paper about methodology, if you wanted to convince people to use it, you had to give a numerical example. We just about had the computing capability to implement the technique at that time and I had a research assistant working on it. In the version that I originally presented at Copenhagen there were some calculations, but I discovered after the meeting that they were wrong. This chap was supposed to give me a correct set of calculations. There was a period of some weeks, perhaps even months, when he was trying to get these calculations done for me and the only computer that we had was the university computer that relied on paper tape. This kept breaking down, and there was also tremendous competition for using it. He needed quite a lot of time on the computer to implement this program he had written for me, and the only time he could get was late at night. So when I was coming back from the theater or from Covent Garden or something like that with my wife in the evening, we used to call in at the University of London computer center to see how he was getting on. There we would find him with these streams of paper tape from which he would read off the results to tell me how he was getting on. But, in fact, before he actually finished this, he got a job somewhere else. I still had the idea that one shouldn't publish a piece of methodology without showing it could be implemented and I always intended to have the calculations redone, but the programming was so difficult under the conditions at that time and using these machines that were always breaking down became so difficult that I never succeeded in actually getting the calculations completed. Eventually, I was working on other topics and it became one of those things that I put aside and never got back to.

That's an amazing story. The paper is referenced in Malinvaud's textbook, even in the earliest editions. So, no doubt, soon after you gave the paper a lot of people were familiar with it.

David Hendry was a student here, soon after that time, and he made some use of it, I think. Perhaps, David Hendry as well as Malinvaud helped to make the paper known.

Later on during the 1960s you went back to serial correlation and wrote several new papers developing the theory of the cumulative periodogram test, the *h* test, the *t* test, and so on. Perhaps you could tell us what brought about your renewed research interest in this topic.

After Geof Watson and I had completed the theory for the lag one serial correlation coefficient, we looked at the question of tests of higher-order serial correlation. And at that time we found that the mathematical theory was intractable, at least under the computation conditions at the time. But I had always had it in mind that this is something I would like to work on. About 1955, Maurice Bartlett's book on stochastic processes appeared, and in it he showed how one could use the Kolmogorov–Smirnov distribution theory for the cumulative periodogram as a general test of serial correlation in the nonregression case. I was rather interested in this. I thought that it gave quite a good way of looking at the problem, especially when you think of the graphical presentation of the cumulative periodogram. It was a good way to embody a total knowledge of the serial correlation properties of the series. I thought the cumulative periodogram might appeal to applied workers from that point of view. In 1965 I went to spend a year with Geof Watson at Johns Hopkins University, and I suggested to him that we might work together on extending the Bartlett work to the regression case and on trying to solve the finite sample theory. Bartlett's treatment used asymptotic theory. It turned out that Watson was involved in other work and was not available for collaboration, so I went ahead alone. I was very pleased from the theoretical point of view with the results and I did include numerical examples in the paper. I hoped that it would provide applied workers in econometrics with a practical way of looking at the higher autocorrelation properties of a series. Perhaps because of the low interest in graphics at the time, it did not catch on. I did meet a number of people who had got it programmed as part of regression packages, but they had not gotten the graphics in. They only had the result of the formal test. And to me the important thing about the approach was to look at and interpret the picture of the cumulative periodogram. So the outcome was a little disappointing in terms of practical take-up. But it did lead me to get interested in the Kolmogorov–Smirnov statistical literature and in boundary-crossing problems. I could see a number of possibilities for developments in this area and I worked on them extensively over the next twenty years.

So this was also the origin of your interest in boundary-crossing problems?

Yes. The origin of my interest in Kolmogorov–Smirnov test and in boundary-crossing problems was testing for serial correlation in time series analysis using Bartlett's idea of the cumulative periodogram.

Now your paper on the *h* statistic and *t* statistic came out a little later in *Econometrica*. What was the background to that paper and your work on the problem of testing for serial correlation with lagged dependent variables in the regressor set?

The origin of that paper was that Watson and I had considered the question as to whether our test procedure applied when there were lagged dependent variables present, and we were quite clear that the procedure did not apply. But, later on, I wrote a paper for *Biometrika* on applying the serial correlation test to simultaneous equations and I made a remark in that paper that perhaps was unguarded and could be misinterpreted as suggesting that I thought that the test applied where there were lagged dependent variables present. What happened was that I had written the paper first and I wrote the introduction afterwards as I usually do. The phrase concerned occurs in the introductory section. I think it is quite clear from the body of the paper that I am not claiming that the results apply in the lagged dependent variable case. Marc Nerlove and Ken Wallis wrote a paper in *Econometrica* in 1968, in which they were commenting on the fact that the Durbin–Watson test had been used where there were lagged dependent variables and they suggested that this was possibly because of a misinterpretation of my remark. I felt that in some sense they had succeeded in pinning on me the responsibility for this misuse and so I wondered whether I could do anything to correct that. I thought that I would go back and have another look at the problem. I did so and developed my own general theoretical approach which, in fact, I hoped would have wider applications later to other problems. It turned out that other people recognized that the approach was very close to the Lagrange multiplier test, which I did not realize at the time. In fact, it's the Lagrange multiplier procedure rather than the one that I developed which has been widely used subsequently for other problems. However, I was pleased that the *h* test came out in a neat form that could be implemented from an ordinary regression printout.

Another one of your major interests over the years has been seasonal adjustment procedures. Perhaps you could tell us about the origins of your interest in this field.

My colleague, Claus Moser, who was a Professor of Social Statistics at LSE, was appointed in the mid 1960s to be head of the Government Statistical Service by Prime Minister Harold Wilson. In 1968 he set up a research section in the Central Statistical Office (CSO) to investigate methodological problems. I was asked to act as an academic consultant on some of their time series problems. One of the first problems that we were asked to look at was seasonal adjustment of the unemployment series. Harold Wilson had worked as an economic statistician in the government service during the war, and he was really rather good at interpretation of numerical data. The government of the day had become increasingly worried about the rising unemployment and Wilson was very interested in looking at the figures himself. He got the idea, as Prime Minister, that maybe the reason why the unemployment series appeared to be behaving in a somewhat strange way was due to the seasonal adjustment procedure that was being used. So he called in Moser and asked the CSO to look into this. I was brought in as a consultant. I worked with Robert Brown on those problems for the next year or two. It turned out that Wilson was right and there was something wrong with the seasonal adjustment. I think it is remarkable that a point like this should be spotted by a prime minister.

So it was this practical problem that initiated your interest in seasonal adjustment?

Yes. I think that seasonal adjustment is not a subject that attracted academic statisticians very much. It was regarded as being a rather messy practical problem that only applied economic statisticians were concerned about. It was also regarded as being not really very respectable theoretically. It is not a problem that one can formulate in terms of precise modeling in the mathematical sense. But, nevertheless, I recognized it as an important practical problem and I continued to work with a variety of people in trying to make some contribution to dealing with it.

In 1973 your paper on weak convergence of the empirical distribution function appeared in the *Annals of Statistics* and your book on the distribution theory of tests based on the empirical distribution function was published. In these contributions you were one of the first people to mobilize the theory of weak convergence of stochastic processes in a major statistical application. How did your research in this area get started?

The monograph really originated from the work I did on the cumulative periodogram. I wanted to get an exact theory of the Bartlett type of statistic. I talked to Ronald Pyke about this, who had an expert knowledge of the empirical distribution function and, in fact, had worked on this with Bill Birnbaum. I had done the basic work on the paper on the cumulative periodogram at Johns Hopkins University in the academic year 1965–66 and Pyke invited me to the University of Washington for the following summer, 1966, where I worked on the finite sample distributions of these statistics. I got deeply interested in this work and, of course, Pyke was there as well as Birnbaum and Galen Shorack, all three of whom had a deep knowledge of this field. I found this a very stimulating experience and it led me to explore a wide range of problems in this area that led to the monograph that you have described.

As far as weak convergence is concerned, that arose in the following way. I saw the possibility of doing some work on Cramér-von Mises statistics, as well as Kolmogorov–Smirnov statistics. In particular, I saw how one could obtain the limiting distribution of Cramér-von Mises statistics for the case where parameters had been estimated. I wrote two papers on that with my colleague Martin Knott. The question was, how was one going to be able to prove that the results that we were presenting were correct from the standpoint of probability theory? For that we needed a weak convergence theory. There was a famous paper by D. A. Darling on the Cramér-von Mises statistic when parameters had been estimated, but the weak convergence arguments used by Darling were very rudimentary and, in fact, were incorrect from the strict standpoint of mathematical probability theory. I felt that Knott and I should have in our paper a proper proof that our statements were correct. So I started work on this but it became clear that the work was becoming too big to be incorporated in those papers. Already the one paper that we initially intended to write had been divided into two papers and they became rather long and contained a lot of detailed results. So I decided to split off the weak convergence paper, which I thought would have more general applications anyway, into a separate paper. In other words, this work also arose because of the desire to solve a very specific problem. But I could see the possibility that it would have applications elsewhere. That led me to get very interested in the theory of weak convergence.

It must have been very satisfying to have made so much progress on these statistical problems using mathematical theory that was still relatively new to probabilists.

It seemed to me to be the right way to do it. Later, of course, other people in Hungary discovered alternative ways in which one could prove weak convergence. In the book by Csörgo and Revesz, for example, you have the main emphasis being strong convergence. That didn't exist at the time I was doing this work. And to me weak convergence was the right way to approach these asymptotic convergence problems. And having missed out to some extent on my mathematical education because of the war, I quite enjoyed learning the mathematics needed.

In fact, the almost sure convergence results, while stronger, don't affect anything that one does from a practical perspective, from the point of view of statistical theory, because it is distributional results that ultimately one wants to use.

That's right. Convergence and distribution theory is what you want. And, in fact, the strong convergence results are usually used in practice to validate the asymptotic distributions.

Your paper on the distribution of sufficient statistics in *Biometrika*, 1980, broke new ground for the saddlepoint approximation. Can you tell us how you came to work on this problem and the origins of your rather novel approach to the saddlepoint method?

That arose because, as I explained earlier, I knew that the cumulative periodogram had not been accepted by applied econometricians as the way to look at higher-order autocorrelations and so I thought that I would like to go back to that problem and consider the appropriate statistics in the time domain. I already knew that the correct statistics for the purpose would be the partial autocorrelation coefficients. They would have to be noncircular statistics because circular statistics are not useful from a practical point of view. Ideally, what was needed was the joint distribution of these statistics for a general set of regressors. I was aware that Daniels had written a paper in 1956 for the circular serial correlation coefficients, but not for regression residuals. My first thought was to extend Daniels' work using Daniels' saddlepoint technique based on integration on the complex plane. Since I needed a set of noncircular statistics that were mathematically tractable, I based them on the d statistic that Watson and I had used for the 1950–1951 papers. I considered a set of statistics whose numerator quadratic forms had the same eigenvectors. I then went ahead and used Daniels' complex integration technique to get their joint distribution. About the time that I had completed this work, Cox and Barndorff-Neilson produced their paper on saddlepoint approximations for exponential families. However, all the problems they considered were for independent and identically distributed random variables. So I knew that I would have to modify their theory in order to use it. After some preliminary work, I realized that this approach was mathematically simpler than integration on the complex plane. That led me to write a general paper on getting saddlepoint approximations by a rather simple technique based on the properties of sufficient estimators. The simplification was the first thing. The other thing was that I showed that these problems could be treated for the case of dependent random variables. When I came to write up the work, I needed a theorem for Edgeworth expansions for dependent variables and, to my surprise, I found that such a theorem did not exist. There was the paper by John Chambers, but as you had pointed out in a paper in *Econometrica*, there was actually some kind of gap in Chambers' argument. It is simply not the case as Chambers had assumed that moment conditions are sufficient for the validity of the Edgeworth expansion for dependent variables. I knew that Sargan and Satchell were also working on this problem. Their work was presented in a form that I couldn't use. And, looking into the literature in probability, it seemed to me that a much simpler argument than theirs could be developed using Feller's treatment of the ordinary Edgeworth expansion. Feller has an extremely clear treatment of the validity of the Edgeworth expansion for independent random variables in his Volume II. It seemed to me that by extending this one could prove a theorem for dependent variables. So I went ahead and did that.

In your recent paper on the "Evolutionary Origins of Statisticians and Statistics" you develop a fascinating thesis about the capacity of the

Jim Durbin celebrated his sixtieth birthday by climbing Mount Blanc, the highest mountain in Western Europe, in July 1983. Here he is on the summit with his climbing companion, the renowned Chamonix guide Renato Ghilini.

human species to do mathematics and the applicability of statistical theory in the real world. I suspect that these are issues that you have thought about for a long time. Do you think that your conclusions about the applicability of statistical theory hold more for the natural sciences than for social sciences?

I have been interested in the question for at least 30 years as to why the human species can do mathematics as well as it can and also why mathematics works as well as it does, when you apply it in the real world. It has always been a bit of a surprise to me that there isn't a wider interest in these questions among people who earn their living using mathematics. I've always treated these questions as suitable material for party conversation but had not thought of writing up a discussion of these questions for publication.

Then I had the invitation to write this paper for the ISI centennial volume and the editors specifically asked me not to write a paper about anything technical in statistics, because they said they already had too many technical articles. They wanted something of a general nature that had more of a human element. So I thought I would work up some of these ideas on the evolutionary origins of the human intellect that I had always held in an informal way into a paper with documentation that would help justify what I said. I took the writing of the paper fairly seriously. I knew that a lot of people in the statistical world who might possibly read the paper would not be too well informed about evolutionary biology and would regard what I wrote about it with some skepticism, so I took the trouble to read up on some work in the area to try to get those aspects of the paper adequately documented.

The interest in this paper has been very limited among mathematicians. I found almost nobody in mathematics who was willing to take any interest in these ideas. Among philosophers there is a similar resistance. People in other sciences seem more amenable. One thing that I did before publishing the paper was to check through the LSE library on the philosophy of mathematics. LSE has been a center for the philosophy of science and mathematics because of the work of Lakatos and Popper. But there seems to be no mention of the word evolution. This seems extraordinary. It appears self-evident to me that if you wish to understand the philosophical foundations of mathematics you have to start with the evolutionary origins of human reasoning power. Although at present philosophers don't seem very interested, my belief is that in 25 years' time all philosophers will recognize that to understand how the human intellect works you must begin with the question of where the human intellect came from, why it has the form that it has, and why the nature of thought is the way it is. The only way you can get an answer to those questions is by studying the evolution of living organisms. First you study the evolution of primitive organisms in response to their environment and then you work your way up until you eventually get to man and ask how man came to develop his intellectual equipment in order to survive and reproduce the species.

I think it is very well put in your article how men in hunter-gatherer societies needed to conceptualize their surroundings in the form of a stylized model and that mathematical models may be an entirely appropriate way of doing this because they fit the real world rather well. The issue that arose in my mind as I read this was the relevance of the same thesis to the socio-politico-economic world, which seems to be every bit as complicated in its own way as the physical world around us. Do you have any thoughts about the appropriateness of mathematical and statistical modeling as we currently know it in the context of that aspect of the real world?

Well, I suspect that we will get better models in the future which are not based on one-dimensional logical statements. A lot of people at present seem to be under the impression that thought and logic should be like a Turing machine, that is a one-dimensional sequence of symbols and it seems to me that a mathematical statement is of this kind. If you write a theorem, you write it in terms of logical steps, one after the other. I suppose that this has something to do with the evolution of language. As the evolution of the human species progressed, people became able to communicate with each other using one-dimensional streams of verbal symbols. But it was a sort of biological accident that we learned to communicate through speech, which is one-dimensional. But that isn't the way our minds work, when we are absorbing information about the complex nature of the world around us. We can close our eyes and visualize images that mimic events awfully well. We do that in a very multi-dimensional way. Think about how your intuition works when you are solving problems. Anybody who thinks he's going to do it by writing down one logical step after another is usually a very dull chap. The clever people think all the way around the problem, they have a hierarchy of thoughts, they get insights and then only at a later stage try to convert it into a logical stream. To answer your question very inadequately, I think that in the distant future mathematical models of human and social behavior will need to reflect the multi-dimensional complexity of the way the human mind works though I have no idea at present how this will be done.

The concept of a one-dimensional string of information that you have been talking about is fascinating. The real strength in conceptual reasoning comes first from lateral thinking in some measure and it's very hard to achieve this until you can conceptualize the whole problem. Later, you can find mathematical language which helps to analyze it and break it down into pieces. I suspect from what you are saying that you have an affinity for geometrical arguments rather than pure analytical reasoning. Is that correct?

I have found in my own work that if I can put a problem in geometrical form, it's a lot easier to understand the problem and thus intuit a solution.

There are so many different aspects to this question. One that occurs to me even now, is that there is an essential endogeneity in the human socio-politico-economic world. All of our institutions, be they political, economic or social, are constructs of human intelligence and evolution. So the form of our social, economic, and political world is endogenous in a very basic sense, endogenous to our own intelligences.

It's a map of some kind from the mind to the institutions. I think you have brought up an interesting point which may explain why the attempt to use pure techniques of natural science usually is such a failure. That is, we are using more complex aspects of human relationships in constructing our institutions which enable people to work together for various objectives. To think that you can take the thing that works so well in physics, this sort of logical one-dimensional stream, and construct a mathematical model from that to explain social behavior does not seem entirely appropriate.

I wonder if you would like to talk about the continuity of the intellectual tradition here and tell us more about the LSE from a modernist perspective?

The LSE to me has always been an interesting and rewarding place to be. In the fields of time series and econometrics we have had this continuous development since the early 1950s and it is still as strong, if not stronger, than ever. We have two professors of econometrics at LSE at the moment, both young men, one in the economics department, Peter Robinson, and one in this department, Andrew Harvey. Both of them are former students of the school and, in fact, it may perhaps be surprising to some of your readers, that they both graduated in statistics rather than econometrics.

In all the quantitative social sciences, I think the future is going to be more exciting than the past because of the far greater computing power that we are going to have available. I think that most young people here in economics and in other social sciences are interested in quantitative analysis and many of them are quite sophisticated in mathematics, statistics, and computing. We are now getting on-line, powerful computing facilities for every individual worker and we are having immense data bases that are going to be accessible through terminals on national and international networks. There is the possibility of electronic mail, electronic communication between two or more workers cooperating on the same subject. One can look forward to much more international cooperation between people on research projects, each working at his or her own terminal and communicating almost instantaneously with his or her collaborators in other institutions. Similarly, we shall be able to handle much more powerful models based on a greater perception of the real structure of the area of interest that we are working on than the more approximate structures that we had to use previously. One thing that springs to my mind here is time series analysis. Box and Jenkins made a very great contribution to developments in time series analysis, but their approach to the subject is going to be replaced by a more structural approach based on a deeper understanding of the processes underlying the data. What Box and Jenkins do is start with the concept of stationarity, which is a purely mathematical concept. In the social sciences we don't usually have stationary series. Then, by transformations such as differencing and Box–Cox transformations, one is supposed to transform series into stationary series. Then you analyze the correlogram to determine a model for the system. But the correlogram is not a very powerful discriminator because of its poor statistical properties and in a multiple series situation one has to use the cross-correlogram and that's even less precise as a discriminating statistic. It seems to me that in time series analysis, in relation to econometrics and other fields, one is going to look more to developing mathematical models that are more closely related to the perceived structure of the series. Of course, my colleague Andrew Harvey and other people at LSE have been working on this approach to time series analysis in the last few years.

You have travelled and lectured all over the world during your career. Did you ever think of leaving England or leaving the LSE at any stage?

Certainly in the early years I never expected to spend the whole of my career at the LSE, because I thought in principle that if one were alive intellectually one would want to move and change one's environment to provide intellectual stimulus. I think I have always been happy here and that's why I haven't moved. There are a number of things I like about the LSE. I suppose they reflect to some extent a maverick streak in my personality. One thing that has always appealed to me is the international character of the school. We have always had a high proportion of students from overseas. We receive a constant stream of visitors from all over the world and I think we have had many interesting individuals here. As students, they have sought out this institution because they felt that it has something rather special to offer. So as I travel about the world and meet some of our former graduates, it's always a very stimulating experience. I've always been interested in social and political questions and we have experts on tap here over the whole range of social and economic studies which is quite stimulating. As for emigration, I never thought much about it since as I am an Englishman who on balance likes living in England, there seemed no need for it as long as I could get a decent enough job here.

Of course, you have spent substantial periods of time away as a visiting professor at other institutions abroad. Has this been a stimulus to your research on a continuing basis?

Oh yes, particularly visits to the United States, where I have spent more than three years of my life there at various periods as a visiting professor at different universities. Certainly in earlier days I enjoyed the more competitive atmosphere in the United States because I am naturally a rather indolent person. In England there isn't the same pressure to work for the benefit of one's career or anything of that sort. In fact, on the whole, people don't seem to care very much whether you work or you don't work at research. It doesn't seem so important here. But as soon as I used to get off the plane in the U.S. I began to feel the adrenaline flow a bit more and I felt I wanted to compete with the Americans to some extent.

Did you ever consider writing a textbook in some branch of statistics such as time series analysis?

I did think about this once when Maurice Quenouille was at the school, when he suggested that he and I should write a joint book on time series. I started out to write a chapter for this book and I found that it led me onto some questions to which I did not know the answer. I started writing research papers on these and they led to more research problems and so on. A group of papers that I wrote around the late 1950s and early 1960s arose out of that original suggestion. The result was that I actually made no progress writing the chapter. Quenouille and I abandoned the project.

Did you think about writing a book in any other field? You did, of course, write the book on tests based on the empirical distribution function.

There was a special reason for that. I was invited to give this set of ten lectures by the Conference Board of Mathematical Sciences. Part of the deal was that one had to write the lectures up as a monograph. One did not get paid for the work until the monograph had been received. So I knew I had to write some sort of monograph. In fact I enjoyed it very much and filled in some of the gaps in the theory while doing so. I just sat down and did it. It was one of the things that I did enjoy and so I have always felt that if I did write a book I would have enjoyed it. How do you feel about that kind of thing?

I find that writing books can be exhausting. Small aspects of the task can ultimately wear you down. Also, I think you have to be a special type of person to keep a big concept under control and manageable. Sometimes a good editor of a publishing company can be helpful in that regard but, undoubtedly, some people are better than others at this type of work.

In some respects each individual finds his own way and spends his time on the things that he feels he is best at. There are a lot of people who, in some sense, are ideas people, who really do like to feel that they have done the best they can developing new ideas. If you have done anything original in terms of ideas, then just writing research papers is a hard business and requires a great personal commitment. I used to like to feel that every paper I wrote had at least one good idea in it. If you are writing a book there is a long stretch of time when you are working quite hard writing about other people's ideas.

In the economics and econometrics wing of the LSE they have published a lecture series. Did the idea ever occur to you to do something similar in statistics?

No. Maurice Kendall already had a connection with the publishing firm, Griffin, when he arrived at LSE and he started a series of statistics books for them. When he gave up the editorship, Alan Stuart took it over. It is possible that if I had been in another institution I might have done it, because I might have felt a responsibility for doing it, to promote the ideas of that institution.

One area of statistics and econometrics that has come under discussion in recent years is the foundation of statistical inference and more general methodological questions. Are these areas that you have also been thinking about and have anything specific to say on?

My feeling at present is that the debate about statistical inference is conducted on too narrow a base. I think the argument as to whether you should have a Bayesian approach or a classical approach seems to me to avoid the most interesting aspects of the use of statistics in applied work. I think there are many factors at the present time that are relevant to this. One is the development of statistical packages. I think it's wrong for a student in a particular department to be educated only in one particular system of statistical inference, because it means that the statistical packages that are based on the different approaches that he may find useful for later work, will not be accessible to him. If one goes to the foundation of probability theory I think that it was unfortunate for the development of the subject that de Finetti and Savage insisted that statistical inference should be based purely on a personal theory of probability, a subjective theory of probability. I think if you go back to the origins of probability theory in games of chance, in the 16th and 17th centuries, it seems to me quite clear that there were two aspects of these phenomena. One was the behavior of the objects, such as dice or playing cards which, being manipulated in the real world, were subject to variability according to the operation of the chance mechanism. The second was the assessment of uncertainty by the gamblers. I think, to suggest as some Bayesian statisticians do, that probability should be concerned with only uncertainty, is too one-sided. I believe that the profession as a whole should recognize that there are two aspects of probability: variability and uncertainty. Thus, we should have an integrated philosophy with the subject which embraces both.

Beyond that, I think our approach to the techniques that we use should be much more pragmatic. I think there are many practical problems where increasingly we will have powerful computing facilities accessible to us which will help us to look at likelihood functions, for example. Even if you are a statistician who basically likes to use what is called the classical approach to statistics, sometimes it's rather informative to see, for a particular parameter of interest, what the shape of the profile likelihood looks like. For example, what one can do is take the likelihood function where one has a substantial number of parameters under study and take the maximum likelihood estimates of all the parameters except the one of particular interest and then look at the shape of the likelihood function evaluated at the maximum likelihood estimates of these other parameters. Then you might find it interesting to look at the picture of the posterior distribution of that parameter after you have integrated out all the other parameters using some kind of arbitrary prior. Even if you are not a Bayesian you can, for the sake of the analysis, assume some arbitrary prior distribution that is convenient for the purpose. It seems to me that one can use these techniques without being personally too involved with the ideological basis on which the techniques have been developed.

A further aspect which I think is extremely important in a practical approach to statistical work is diagnostics and sensitivity tests. In the many fields of interest to me such as time series and applications in econometrics and the social sciences, one now has the possibility of calculating a large number of different diagnostic test statistics. Of course, I have a special interest in tests of autocorrelation, but one thinks of tests of normality, one thinks of tests of heteroskedasticity, and so on. There is no doubt that, together with the development of statistical packages, we are going to see in the future a large number of diagnostic procedures being developed which will enable us to examine the validity of the assumptions on which we intend to base our analysis. And if we find these assumptions are invalid we can make modifications and then do some more diagnostic tests. If one goes back to an earlier, more rigid approach to statistical inference, people used to worry about questions of the power of a test, and whether it was legitimate to carry out a number of independent tests of significance on the same set of data. I think these questions are too theoretical, using the word *theoretical* in a derogatory sense of the word. I think it's quite right and proper for an applied worker to look at a wide variety of diagnostic tests and, especially, I like the idea of graphical procedures. When I first got interested in Kolmogorov-Smirnov tests, for example, what I liked was the idea that you look at a graph of the sample distribution function. Then you could see at which part of the domain of variation anomalies appeared to occur. Similarly, in the time series case it is a good idea to have lots of graphs so you can see variances changing over time and so on.

I think a lot of the earlier ideas of statistical inference were based on small parametric models which were regarded as being true in some sense. The reason why this was done was because of the computational limitations of the time. In Fisher's day, one could only have a very simple model, one had to assume normality or some simple distribution and the range of possible ways of analyzing a particular data set was extremely limited. Nowadays, we have this great freedom, because of the computing power available to us, of looking at the data from many different standpoints. There is the whole question of robustness and dealing with outliers and modifying data so that we get a better analysis. Many new techniques are going to be developed and will be incorporated into packages. I think we should educate our future students to take a much more pragmatic view of the subject and to use various theories of statistical inference according to the practical value of the techniques that they lead to. I would like to think of this as being more a philosophy of statistics, a general approach to the subject, rather than the application of any specific scheme of inference or system of inference. And I would like to hope that the profession as a whole will move towards what I like to call a unified philosophy of statistics in future years.

The development of microcomputers and more extensive use of interactive statistical computing is going to be a vital element in the pursuit of that overall strategy.

Yes. Also I think that purely routine analysis of things like straightforward regression and analysis of variance is something that the professional statistician and econometrician will not be very much concerned with. That is something that will be available on a routine basis with some kind of expert system at the front end which will, in an interactive way, guide the applied person rather easily through some of the questions that he used to have to discuss with a consulting statistician. I think that the professional econometrician or econometric methodologist and the professional statistician will be needed to draw on a wider range of knowledge of techniques that one couldn't expect the nonstatistician to be aware of. He or she should be willing to do this over quite a wide spectrum, instead of insisting on a very rigid system of inference, whether it is on the so-called classical side or on a so-called Bayesian approach.

Do you have any specific plans for the future?

Retirement age is approaching really rather rapidly. I feel in no way ready for retirement in the conventional sense. I'd like to go on having an active life in the subject, but I don't have any specific plans at present.

THE PUBLICATIONS OF JAMES DURBIN

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