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THE ET INTERVIEW: PROFESSOR E. J. HANNAN

Interviewed by Adrian Pagan

EDWARD JAMES HANNAN

It is a rare event that a discipline is greatly influenced by contributions made by individuals away from the great centers of learning in Europe and America. Yet this is the case for mathematical statistics, where Australians have made an impact far exceeding that expected on a comparison of population sizes. A glance at names such as Moran and Pitman, who remained in Australia throughout their academic careers, along with those of expatriates such as Watson and Gani, emphasizes this fact. But of all these, perhaps the person in this school best known to econometricians is Ted Hannan. There can be few students exposed to modern macroeconomics, with its heavy emphasis on time series methods, who have not encountered at least one of the important papers Ted has contributed to the econometric literature on the analysis of time series.

All those who know Ted have always stood in awe of his catholicity of interests and knowledge. Any discussion is generally peppered with observations about interesting problems arising in physics, geophysics, astronomy, meteorology, mathematics, ... This amazing versatility was once underscored for me when a distinguished econometrician gave as one of his reasons for visiting the Australian National University (ANU) the hope that he might get Ted interested in his problem for an hour or two!

Unlike American graduate schools where there are large numbers of students, the number pursuing a doctorate in Australian universities is always small. Nevertheless, Ted has supervised students who have made important contributions to econometrics and statistics—Des Nicholls and Deane Terrell at the ANU, Peter Robinson at the London School of Economics, and Victor Solo at Harvard. But concentrating upon such direct connections would be misleading; as many graduate students at the ANU can testify, including myself, Ted's influence was felt even if he was not formally a supervisor of their dissertations. It was his presence, allied for a time with that of A. W. Phillips, that did much to make the ANU such an exciting environment in econometrics during the 1960s and 1970s.

Apart from overseas visiting appointments, Ted Hannan has spent his entire academic career at the ANU, beginning as a Research Fellow in 1953 and becoming a Professor of Statistics in 1959. In 1986 he retires from the Research Professorship he has held since 1971. To mark the occasion *Econometric Theory* felt it appropriate to record the influences that have shaped Ted's academic life and work, and to seek his opinions and thoughts on current and future trends in econometrics. On November 7, 1985, I met with him at his office at the ANU. The interview published here is a transcript of that conversation. We hope that the flavor of the interview conveys Ted's personality and insights, faithfully to those who know him well, and forcefully to those who have not had that privilege.

I'd like to begin by noting that you did not receive your Bachelor's degree until you were 28 and the doctorate until you were 35. Such a late beginning to a career is rather extraordinary now, and I wonder if you might outline the reasons for this state of affairs, and what made you eventually begin the B. Com degree at the University of Melbourne?

I left school when I was only 15. I'd been to a Catholic school, a Jesuit school, a very good one in Melbourne, and I had done very well at that school. My father was a rather impractical man. He was also very irreverent in every way, including religion, and I think I went to a Catholic school because he would have been ashamed, in the sight of his family, all of whom were devout, to send me to a school that was not Catholic. But I went to this Catholic school which was a good one, and then my father at the end of the Depression thought that the best thing was to get me a safe job. He'd been a freelance commercial artist during the Depression, and he probably thought the best thing was to get into a secure job. He probably overrated security in the light of the experience of the Depression. So I went to work in a bank. It was the Commonwealth Bank of Australia, which was at once the Reserve Bank and a commercial bank, and I went into the worst part of the commercial bank, which was the Savings Bank. I worked there until the war and did some accountancy. Then I went to the war as an infantry soldier. Coming back I had the opportunity to go to the university in 1946 as an ex-serviceman under Australia's version of the GI Bill of Rights. I went to Melbourne University one day, just having decided that day to go to university, and I went up to the people who were running this education scheme and I said I wanted to do a degree. They asked what I wanted to do, but I didn't know. They handed me the Melbourne University handbook and I looked at the offerings. Now, I had no noble motive, as all I wanted to do was to get qualifications to earn a living. So I looked, needless to say, first at medicine. But you had to have had Latin to do medicine at Melbourne University and, as I had not done Latin in my Leaving Certificate, I couldn't do medicine. Then I looked at law, and you had to have Latin to do law. I remember years later Dudley Moore saying that "you had to have the latin for the judgin." So Melbourne anyhow was spared me as a lawyer or doctor, which was probably a good thing. Finally, I then looked at economics and commerce, which seemed to be the next best thing from the point of view of making a living, and I therefore started to do a commerce degree.

What types of subjects did you study in the B.Com and were there any that you became particularly interested in?

I started off thinking that it was going to be difficult for me to get a degree. I'd left school 10 or 11 years earlier and had no idea how difficult it would be to get a degree. So I started off in what in Melbourne University was called a pass stream. That is, I did not do honors level subjects. About halfway through the year I became aware of the fact that I could do better, but there were only two subjects that did not have a special honors stream, and they were accountancy and commercial law, so I put a lot of effort into those. I passed all the subjects that I did, winning an exhibition for commercial law and getting the second best first-class honors in accountancy. The one who got the first was Russell Mathews.[1] Then I realized that I could do better. By this time I'd also realized that I was more interested in theoretical ideas. At least whether I realized it or not I don't know, but I was. I heard a man called Sam Soper, who became an economics professor in Melbourne, say he was going to do some mathematics and statistics. So I thought, if he was going to do it I'd better do it too. Thereupon, in second year I did some mathematics, and in the third year some statistics and a bit more mathematics. In second year the course that attracted me most was the course which was entitled Money and Banking but which included a general equilibrium analysis; Keynesian theory I guess you would say now. It was started off by Isaacs, and then it was taught by a man, whose name I now see a lot, called Haddon-Cave; C. P. Haddon-Cave, who is now a very senior civil servant in England. The courses that attracted me most were the money and banking course on Keynesian theory, public finance (the operation of the Treasury, etc.), but I was also attracted very much by the mathematics and statistics courses, which were all very well taught by first-rate people. The statistics department course was taught by a man called Belz who was later professor of statistics, and he ran a course that was a very good one for its time. The professor of mathematics in Melbourne was T. M. Cherry, who was a fine mathematician and his department was very good, including a lot of clever mathematicians; particularly a man called Hans Schwerdtfeger, who was recently at McGill. Altogether I did 13 subjects, of which only three were statistics and mathematics and the other ten were economics. Two of those were accounting and commercial law. Although I did very well in those I did not go on with them because I was more interested in the more exciting ideas of economic theory. Of the economic subjects there were only about two or three that were on macroeconomics as you would call it today. One was the course on money and banking, half of which was on the operation of the London money market actually, and a very old-fashioned course. The other was public finance; half was on the action of the Treasury and half was on things like the Grants Commission and detail about the structure of the Australian financial system. And then there were courses on microeconomics of course, the theory of the firm, a bit on the theory of value (not very much though), economic history, economic geography, and some economic statistics. That was about the whole gamut. I did not start a final honors year although I could've as I'd done very well. But I wanted to get married and I did not have enough money to get married if I stayed a student.

After you took your B.Com degree you were a research officer in a bank for some years before coming to ANU as a Research Fellow in the Statistics Department. What was the nature of the work you did there?

After taking the degree I looked for a job, and I was interviewed and offered a lot of jobs. One of my interviews was with a man called Horrie Brown, later at ANU, and Horrie told the people in the bank that there was a fellow who'd done well and, since they were looking for people, why didn't they transfer me to the economic advisor's department in Sydney. After that I was transferred very quickly. I also went to see Dick Downing[2] about which place to go to, and he said that one of the two best places was the Reserve Bank, the other being the Treasury. So I went to the Reserve Bank, and it was a good place to go. The economic advisor at the time I went there was a man for whom I have the highest respect—Sir Lesley Melvile—but he went very soon after that to the International Monetary Fund. In the bank I worked mainly as a statistician, but not entirely. They needed somebody to do their statistical work, so I reorganized an index of import prices and did quite a lot of seasonal adjustment and other work connected with the production of the statistical bulletin; for example, calculating average interest rates and a certain amount of straight statistical investigation. I became familiar with the Cowles Commission Vol. 10 and I tried to estimate a small model of the Australian economy by LIML to find a consumption function. In a way it is harder to do it if you are working in an institution like that, because you know the inadequacies of the data and you are familiar with the realities of the world. Empirical work becomes more difficult to do because you are familiar with the complexities of things.

What prompted you to come to ANU?

What happened was that Coombs[3] was one of the founding fathers of the ANU and he decided that one young man a year should be sent to Canberra to study, really under Trevor Swan. I was chosen, because I was probably the most research-minded fellow in the economic advisor's department, from which the person would naturally have come. So I was sent for a year. Trevor did not take a great deal of notice of me, but he did mention me to Pat Moran, and Pat was impressed with the amount of mathematics I'd taught myself in the four or five years I was working in the bank. He said he had never known anybody to teach themselves as much mathematics, and he went to see Coombs and got Coombs to give me two years' leave without pay. He then got me a research fellowship at the ANU at almost the same pay. Since I'd only been in Canberra for about three or four months before Pat organized this, at the end of the two years I didn't owe the Bank anything and I just resigned. By that time I had got a Ph.D at ANU and I have stayed on at ANU from that time to this. I owe Pat a great deal as a result of this.

One of the papers written during those years was the well-known one on serial correlation in regression models with Geoff Watson. Could you describe how you came to be associated with him in this paper? Were there any other (now) prominent statisticians at ANU at that time?

Pat Moran's department was a very good department. I was the first appointment, being a Research Fellow, although actually doing a Ph.D. The next appointment as a Senior Fellow was Geoff Watson; G. S. Watson, who is now Chairman at Princeton. He is a very clever man and he had a lot of influence on me, particularly as at that time he was interested in time series and wrote a number of papers about time series. I can tell you how I became associated with him in the paper you mention, because it is certainly an amusing story. Geoff had done his Ph.D. at the University of North Carolina and he'd done it on the efficiency of regression when the residuals are autocorrelated. What he'd done was to postulate a true model for the autocorrelation structure, assumed another model, made an autoregressive transformation, and then determined what the efficiency was. Of course the efficiency will depend on the vector on which you regressed, but you can get a lower bound to the efficiency by taking the worst vector. Now he'd prepared a whole series of diagrams, each diagram consisting of a true correlogram, the assumed correlogram, and the lower bound to the efficiency. He was in absolute terror when he went to be given an oral for his Ph.D., because he couldn't explain the diagrams. One of the diagrams would show two correlograms with the assumed and true ones almost identical and an efficiency of zero, while another would show two correlograms terribly different with efficiency 0.87. Of course, he'd guessed it had something to do with spectra—the lower bound will come if the true spectrum has a zero—and because I was concerned with spectra at the time we wrote that paper together. As well as Geoff there was a man called Joe Moyal. He is a very clever man and a fine mathematician too. He was a pure probabilist who is one of the founding fathers in his way of the theory of point processes, through an important paper written in Acta Mathematica round about the late fifties or early sixties. So at that time the department consisted of Pat as Professor, Moyal as a Reader, Geoff Watson as Senior Fellow, and myself as a Research Fellow and, later, Fellow. There were a number of students. Amongst the first students would have been Joe Gani, Chip Heathcote, Chris Heyde, and Eugene Seneta.

Much of your early work was concerned with the detection of serial correlation, and you mentioned in a paper in the Economic Record in 1958 that the problem was "not completely resolved for autoregressive models." At least one of your papers—that in Biometrika in 1957—seems to me to have come close to developing the h-statistic that Durbin developed in 1970. Did you actually spend much time thinking about this problem when you were writing the various papers on testing for serial correlation?

No. I think you are giving me credit that I am not due for. Because I really missed the point a bit. That paper in Biometrika was motivated by Fourier considerations. In the back of my mind was the situation where the vector you were regressing on had simple structure, such as a simple Fourier series expansion. And I think I really missed the point then and later about the simplicity of the notion of how to test for serial correlation when you have got lagged dependent variables in the regression. It might have been a little bit in the right direction but I think you are giving me too much credit. What misled me was the excessive concern at that time with exact distributions, and there was really no hope of getting the exact distribution. It was hard enough to get the exact distribution of an autocorrelation coefficient even when the series was Gaussian white noise. So I was probably obsessed with exact distributions, and I missed the sort of point involved in Durbin's work.

After you finished your time as a Fellow in the Institute of Advanced Studies at ANU, you became a Professor in the teaching section of the University. At that time your interests seem to switch from analysis in the time to the frequency domain. Can you indicate what factors led you to this change of emphasis?

I think that's right. I did change a bit. I think the reason was that I saw a prepublication copy of Grenander and Rosenblatt,[4] which I still think is in its way probably the best book ever written on time series, certainly having in mind the time it was written. They really are both very fine scholars and it was a very illuminating book, and that book influenced me greatly. It introduced me to Fourier methods, which attracted me because I'm mathematically inclined. Moreover, there was a general movement at that time throughout the world toward the Fourier methods, and I think this is undoubtedly due to the fact that computers were becoming available, so that you could do the computations. I think it was just the normal thing to do at that time, particularly for somebody who was mathematically inclined, to get involved with Fourier methods.

During this period your famous paper on spectral regression appeared. What led you to formulate regression in this way and were there any antecedents to this work?

To say it's a famous paper is a bit generous. Anyhow, it came out of Bruce Hamon, the Senior Principal Research Scientist at the Division of Fisheries and Oceanography of the CSIRO in Sydney. What actually happened was this: Bruce came to see me about some data on a strange phenomenon he had observed. It is a wave that passes up the East Australian coast from bottom to top. It's called the Continental Shelf Wave. The Continental Shelf acts as a wave guide and I think the wind provides the driving force. You get this wave, which is only a few centimeters high but a few hundred kilometers long, going up the coast and you can discern it just as you can discern any signal of that type by measurement at two places apart, as all the noise is incoherent between the two places. He had discerned this partly by looking at the relation between atmospheric pressure and sea level, but there were anomalies there which, I believe, led him to find the Continental Shelf Wave. When he came to me with this sort of data he was basically doing a regression by frequency band, and he said to me that he would like to join them together and get some overall measure of the regression coefficients. I remember him saying to me that he supposed it was necessary to weight the different frequencies, and then I realized that he had an idea and we wrote a paper together about it. Now actually I could've seen the same thing by looking at Peter Whittle's work in his book *Statistical Hypothesis Testing and Time Series Analysis.* In that book he emphasized representing the likelihood in terms of the Fourier coefficients. In other words, take the Fourier coefficients, regard them as independent Gaussian with a variance given by the spectrum, and write the likelihood down. It is a spurious likelihood because they are not really independent, but if you started off from that and then took the spectrum as constant over bands, you would be led effectively to the idea that Bruce Hamon and I had. But I did not think of it in that way.

Around 1965 you published an excellent book on group presentation theory in applied probability. This seems something of a departure from your main line of research at that time. What were the origins of it and is it a subject that still interests you?

I became interested in group representations and their use in probability, I think, for two reasons. The first one being that the general notion of Fourier analysis is intimately connected with the notion of a group; in the case of classical Fourier analysis it is either the group of translations of the real line for Fourier integrals, or the additive group of integers for discrete-time Fourier series. I had already read a book by Loomis[5] on abstract harmonic analysis that is concerned with locally compact groups—natural extensions of the real line—but then I realized also that there was a further extension to noncommutative groups and the unitary representations of those groups. That is, to each group element you associate a unitary matrix operating in Hilbert space. The other thing that connected all this up for me was contact with Alan James in Adelaide. Alan James was one of the creators of some of the modern theory of multivariate analysis, as he'd recognized the importance of groups and their representations in connection with the multivariate normal distribution. If you think of the multivariate normal distribution the exponent is the trace of the product of the inverse of the true covariance matrix and the matrix of sums of squares and cross products. It's therefore invariant when you subject both of those matrices to the same similarity transformation of the form $\Omega \rightarrow P\Omega P^{-1}$, and therefore its like an autocovariance function. Just as a covariance function for a stationary process is invariant when you subject both T_1 and T_2 to the same translation, so this is *invariant* when you *subject both points* to the same similarity transformation. Alan had recognized the importance of this and these ideas attracted me. So I started to learn a certain amount about group representations and I wrote that book. Of course, I did not go on from that. Now it has become a popular subject again and there has recently been a book about it written by Percy Diaconis which will soon be published by the Institute of Mathematics and Statistics.

During your career you returned several times to the question of seasonal adjustment—the first paper appearing in 1960 and the last (in English) in 1973. Were these papers motivated by a feeling that existing seasonal adjustment procedures might be improved by formulating models of the underlying components? This approach seems to be a fairly popular one now, at least when expressed in the time domain, and I wonder if you think the approach was (and is) a good one, or do you feel that the "data smoothing" methods employed in programs such as X-11 are better?

First of all it was natural for me as a time series man to think in terms of filters. Moreover I thought I'd recognized something that other people had not recognized: it was not necessary to use filters which did not alter the seasonal component. Since you knew what the effect of the filter was you could always reverse that. I had noticed in the X-11 method a tendency to try and use elaborate sequences of filters in order not to alter the seasonal component. But if you apply a filter so as to make the data more amenable by eliminating nonseasonal components, then as long as it does not eliminate the seasonal component you can reverse the effects, since you know the response of the filter. That was the idea that motivated me. I knew a bit about seasonal adjustment because I'd done a lot of it in the Bank, but I did not go on with it very much. I am inclined to think that the best approach will be one that involves some sort of mixture of modeling and ad hoc methods of the type in X-11. The trouble with models are that you really can't ever quite believe them, so that you don't want to become hidebound in the use of them. Nevertheless, I think modeling, particularly modeling as a signal plus noise, rather than as an ARMA model, is probably the best idea, but I suppose it will have to be used sensibly. My overall impression of seasonal adjustment is that in practice there are a lot of methods that work well almost all the time. But at the moment when you really want them to work well, when there is some change in the state of things, they all work badly. So none of them gives you a good picture of what's happening with the unemployment series, because everybody's behavior changes suddenly when unemployment becomes marked.

Very few applied papers appear in your vitae. I know that you have always been interested in applied problems and one might have expected, particularly in the light of your early career, a much greater number. Why wasn't this so?

I think it's a lack of good judgement by me. It's important to maintain an interest in applications. I think I became so involved in theory that, when people came to consult me, unless they were consulting me about something that immediately took my attention or were somebody with whom I felt a natural affinity, as with Bruce Hamon, I tended not to give them enough help, although not through a selfish desire not to help them. Just because I did not show enough interest I became separated from applied problems and I think that's a mistake. A wise scholar working in a field such as my own would have maintained contact with applications.

Your work on spectral regression has been very influential in econometrics, particularly in applied macroeconomic research. An equivalent impact was made with the work on maximum likelihood estimation and identification of the parameters of ARMA and ARMAX models. I know that Bill Phillips had written his paper on these topics in 1966 and had come to ANU shortly afterward. To what extent did his presence here have an influence on the direction of your work, or were these long-standing problems on your research agenda?

I must say that I had great admiration for Bill. He was one of the nicest men I've ever met. He was completely free of jealousy or intrigue or anything like that. He was a very fine man and we got on very well. He was a marvelous person and I think his paper did influence me a bit, because it made me aware of problems, but I can't say that it really directly led me into anything. I started to work on problems of ARMA estimation for vector models, and being the sort of person I am, I felt I couldn't go ahead until I understood how to parameterize the problem, and that led to the work I did in connection with the identification of ARMA models.

Had you seen Box and Jenkins's work at that stage?

I would not have seen Box and Jenkins's book at that stage because I did the work about 1968 when I was over in the Faculties, and I saw the book when I reviewed it for Mathematical Reviews and that was when Jim Durbin was visiting here in 1971. However, Box and Jenkins produced a whole series of papers which weren't even published but which were in preprint style, and I would have seen those. I suppose what they were doing did draw my attention to the problem, but I didn't read their work that well until I read their book, and that was almost entirely concerned with the univariate case, not multivariate models. I had been influenced a bit by Quenouille's book,[6] which in a way was a failure but still drew my attention to the problem. I spent quite a lot of time trying to understand that book. The reason why it's a failure is that he did not get hold of the right mathematics. He was trying to study multivariate ARMA-type models with the classical theory of matrices whose elements are real numbers, whereas he needed the theory of matrices whose elements are polynomials.

Whilst the estimation papers were essentially constructed as efficient two-step MLEs, and hence might be viewed as located in the mainstream of statistical theory, I have always felt that the paper on identification in

ARMAX systems was radically different, particularly for econometricians brought up in the Cowles Commission tradition. Could you therefore give us some insight into the way you work by outlining how you managed to solve this problem? For example, were you familiar with the theorems from MacDuffee's book used in the solution before beginning work on the issues? How long and how intensively did you work on the problem before solving it?

That is a very good question, I think, because it relates to how you do research. Now, I did know the mathematics first. I had seen a book on matrices by Schwerdtfeger—who had taught me some mathematics at Melbourne—and it referred to an "encyclopedic treatise" on matrices by MacDuffee,[7] so I got MacDuffee and read it. It's a very brief book, but it's a marvelous book to read, because it has all the proofs of the things you don't know about already, and it doesn't prove the things that you do know about. It's a very concise but very good book. The algebra appealed to me and I knew about that algebra of matrices whose elements belong to a principal ideal ring, such as a ring of polynomials over a field, and then I started to think about ARMA systems and vector ARMA systems and their estimation. Your point is quite correct of course; once you solved the identification problem, in a sense the estimation problem is not hard. It is maximum likelihood. There are some hard problems with proving the asymptotics, but the idea is direct. And in thinking about that I saw roughly what the problem was and I wrote a paper in Biometrika. Actually, that paper was coincident in time with another paper by a man called Popov who went much further than me. But they were, I think, the first two papers on the structure of ARMA systems recognizing what the right mathematics was. Popov went further as he knew of the work of Kronecker and others, but I am quite pleased with the work as some of the best I've done. It was really quite simple once you recognize what the underlying mathematical technique is. It's really relatively simple to describe the structure of ARMA systems. Relatively simple of course as there was a lot more to it than was in my paper, much more. That's how it came about, and I think the point you make is a good one—it is important to have in command the mathematics so you can solve the problem. Of course, the 64 dollar question is which mathematics to learn, because you can't learn all of it.

Over the years I've heard various stories about how you solved different problems. As a student I remember hearing you went down to the coast one weekend and came back with your solution to the MLE for ARMA models. Is your normal style of work to see the result very quickly or is there a fairly long gestation period during which the results accumulate?

I don't know I've solved that many problems which really matter for me to judge it too well, but in the case of that identification problem, once you recognize the issue as having to get rid of these indeterminacies, then it can be done very quickly. In things like the maximum likelihood business, some of which I wasn't very perceptive about incidentally, it is fairly straightforward. The real difficulty is trying to do the proof under very general conditions and recognizing what techniques of proof have to be used. That can take you a long while because there are all sorts of little difficulties that emerge and you have to solve them as you go along.

Do you have many false starts when you do work?

Well, I think all of us do that. I could say I've spent years out of my life doing things which have led me nowhere, and I've filled up pages of notebooks and got pages of stuff all of which has led me nowhere. I think all of us do that. It makes you very depressed when you think about it.

I've just mentioned the Cowles Commission tradition. One of the striking features of econometrics during the 1950s and 1960s was just how many econometricians had worked at or were greatly influenced by what was done at the Cowles Commission. Was this true for yourself, or do you think that the geographical separation diminished the impact of that institution?

I was greatly influenced by it. I was working at the Bank when I read it, and it greatly influenced me because I was an economist at that time and I was definitely thinking of myself as an economist and econometrician. Haavelmo's work also influenced me a lot. Because it was so neat and clear this material seemed to point the way toward a bright new future. Now, I was never as naive as some people might have been, feeling this was going to solve all economic problems, which it certainly has not done. In fact, it has been a little bit of a failure I'm afraid. But it certainly attracted me very greatly and the book, because it was a good book, well written by high-class people—Koopmans, Anderson, Rubin, etc., were really very clever men—it made a great impression on me. Even though I was at the Bank, in 1950 or 1951, I knew enough about it to apply the methods and actually build a small model of the Australian economy, estimating a consumption function by LIML.

At the beginning of the 1970s you took up your current position as a full-time Research Professor in the Institute of Advanced Studies at ANU. Why did you make this change?Many scholars seem to feel that teaching is a powerful and important stimulus to them, both in generating new ideas and refining old ones. Do you feel this, or do you prefer to be solely engaged in research?

I think the point is a really good one. I think that teaching does help you a lot. For example, teaching played a part in that work associated with group representations, because I had to teach experimental design and analysis of variance. In learning about that I recognized the analogy with Fourier analysis of data, except that in the general experimental design situation you have got noncommutative groups involved. So that informed me and helped me. I think you are absolutely correct and teaching, particularly at advanced undergraduate or graduate level, is a big help. Why did I switch? There were two or three reasons. The first one was that I was in charge of a very large department and I really was getting to the stage where I had no time left, because I was the only full professor in a department that taught economic statistics, theoretical statistics, econometrics, and I was also in charge of the small computer science group. Because I'd been the one to press for the starting of a computer science group, when they actually appointed three people they said you have got to take charge of it as we haven't got money to appoint a Professor. So I finished up with 14 academics under me, and I already was just rushing around like mad. That was the first thing; if I had not come here I would have gone somewhere else overseas. The second thing was that it was a matter of pride, the first of the seven deadly sins, because a Professorship with the Institute has a certain standing. And thirdly, it was because it gave me a great opportunity to do research. Having the time to do research does help. There is no doubt about it; somebody doing full-time research has an advantage over somebody who doesn't. Reasonable time to do research may not be full time. If you did two or three hours of teaching in a week, I would not care about that, but if you have to run a department and do six hours teaching as well, that's a different story.

Your 1970 book on vector time series seemed to represent a distillation of the previous decade's research. There were a number of cryptic comments in it. One was to the idea of developing asymptotic expansions for the main central limit theorem of the book. As far as I know you have not generally attempted to refine asymptotic approximations. What are your thoughts now on the need for such adjustments and, in particular, do you feel it to be an important area of research?

First of all I haven't been associated much with asymptotic expansions. The only way it occurs in that book is through asymptotic expansions for distribution of autocorrelations and partial autocorrelations, which followed on from ideas of Henry Daniels using the method of steepest descents or stationary phase. Because I had read all that I put it in the book. But I have not made use of those ideas even though I know about them. I am not too good a person to ask for a judgement on this area, but there is a debate going on at the moment in statistics relating to asymptotic expansion versus new ideas such as that of the bootstrap. That is, instead of trying to find an asymptotic expansion for the distribution of a complicated statistic, you try to find its distribution by repeatedly sampling from the empirical distribution function. It's computationally expensive, and it conceptually solves all problems, but for the multivariate case it's probably impossible to do. The bootstrap also covers non-normality whereas the asymptotic expansion doesn't; you generally have to start out with some distribution for the initial data. I think both of them are important. I believe the asymptotic expansions have played a part, in that they have suggested that some of the techniques used in econometrics were actually working better than people thought they were. They were misjudging them using inadequate approximations for the distributions of the statistics they were computing. Nonetheless, it seems a bit strange to be going back to old things, and it seems a bit strange that you are usually starting off from assumptions of normality. But it certainly seems to be of some importance.

A large part of your output after 1970 was concerned with the estimation of systems of ARMA and ARMAX equations. Now my impression is that most applied workers have tended to opt for vector ARs rather than ARMA processes, primarily because of specification and computational difficulties. What is your opinion on this choice? Do you feel that it is worthwhile estimating ARMA models? Do you see any likelihood that the computational issues will be resolved in the future?

That is a very good question. In fact, I think there are three things you might say. First of all, you are quite right that generally people have used autoregressions because they know how to fit them. It can be done recursively on the order and the fitting process is an easy one. Hence they've used those. There has been some use of general ARMA models, through the state space form, and that's been used quite a lot by engineers. Fred Schweppe at MIT for example has done a certain amount of it. That is, they've specified the system in a state space form, usually with the state transition matrix being highly constrained, most of its elements usually being set to zero, and those not zero being nonlinear functions of a relatively small number of parameters. Then they've estimated that directly by maximum likelihood. These are general ARMA models, not just an autoregression, but they are very highly prescribed and all the structure theory is irrelevant to them because there are so many constraints on the matrices of the system. It would be impossible to work through canonical forms, because when you transform to canonical forms you'd lose track of constraints. Although it's technically possible, in practice it would be impossible. So ARMA systems have been fitted that way. But fitting of ARMA systems in what I regard as a more ad hoc way, that is, you're just trying to describe the data or fit a model to the data without any major prior constraints other than stationarity or reasonable smoothness of spectra, is another matter. Very little of that has been done and for the reasons you've stated. It is a rather complicated system of computation, and people, I think, just don't feel its been worthwhile doing with the data they have to hand. Now, what will happen in the future? I think, possibly it will come in eventually, for the following reason. The trouble with a high-dimensional vector of observations is that, as you increase the order of the autoregression, you add, if the vector has s components, s^2 parameters. You would like a method that doesn't do that, that economizes on parameters more, and working through these other highly structured canonical forms described via Kronecker invariants, you can of course increase the number of parameters by one at each occasion, and you can try to economize on parameters. So it may work out, and I think it probably will work out, that eventually people will do that. But somebody has got to produce programs that will work and an explanation that people understand. It's got to be made accessible to a range of people so they can actually use it and understand it. Maybe it will never happen, and it certainly isn't happening at the present moment.

One of the characteristics of your work has been its rigor, especially when compared with the standard econometric theory in the 1960s. In fact, Malinvaud, in a letter to the editor of the International Economic Review in 1971, had this to say about the standards of that time: "For some time already I have been thinking that our profession ought to raise its standard of rigor in one field, namely the asymptotic theory of mathematical statistics." Do you think that this challenge has been answered in econometrics? What do you feel about the current trend to put mixing conditions to the forefront in asymptotic theory, and do you feel that there are some emerging areas in probability theory that should be of interest to econometricians?

First of all I think that you are quite right that the situation has changed altogether in econometrics. You used to get people coming in from other subjects with a fairly good mathematical equipment thinking they can go into econometrics and make a name for themselves. But they can't do that any longer. I think the situation has changed radically in the time I've been watching it. There are now plenty of people in econometrics quite well equipped mathematically and in touch with probability and statistics. It's become a subject in its own right. Regarding the second part of the question, what happens sometimes in science is that people get hold of a certain equipment and they use it a bit uncritically. That's likely to happen in this area, and I think you have always got to be a bit careful about that. It still does happen that theorists, people who do theoretical work, get undue credit for what they are doing. I think, sometimes you also get people doing things that are not really econometrics and could be left, as it were, to the probabilists to do. Really, the task of the econometrician is to think about what's the important economic problem, and what are the worthwhile econometric techniques. This may require a certain amount of statistics and statistical theory, but I think it is probably unprofitable for the econometrician to be too concerned with such issues. When I think of probability theory I think of the sort of thing that I've heard people talk of recently, and there would be two big movements. One of them has been what would be called stochastic analysis; stochastic differential equations, etc. Now I can't see it really affecting economics very much. It has not affected any other part of applied probability very much at the present moment. It will eventually; it's deep stuff and it's very beautiful and very important, but I can't see it really affecting economics much. The other movement I can think of is that which started off with weak convergence theory and strong convergence theory: Billingsley's book[8] and then Cszörgö and Révész.[9] This has all been a movement to coordinate and organize the whole business of the central limit theorem and associated limit theorems such as the laws of the iterated logarithm and so on. And there's been an enormous development there; an enormous simplification and, as is typical of mathematics, enormous organization and getting down to basic ideas. But I can't see that as really being important in econometrics, and I think I'd say what I said before again, that the task of the econometrician is to really consider economic problems, economic modeling, and what are the useful statistical techniques. They may have to solve problems of statistical theory in doing that, but I don't think they are going to do it that often. Knowing some probability will help you because you'll know what you could do and could not do; it couldn't hinder you, but you've got to make a judgement as to what you are going to spend your time doing.

Although most of your articles have been concerned with estimation, you have done a few papers on the question of model selection. How do you rate the relative importance of the estimation and selection problems?

Well, there are a number of things to be said. First of all, I found when I worked in the Bank that hypothesis testing never arose. I only remember once ever a problem becoming an hypothesis testing problem, and it actually had nothing to do with economics. Secondly, I was a bit influenced by a statement in Grenander and Rosenblatt; I can't remember the exact words, it was something like this: "It will generally be agreed that problems of estimation and allocating confidence intervals are of much more importance than problems of hypothesis testing." I was influenced by that sort of statement. Later I came back to what I would call model selection, because I'd been influenced by people such as Rissanen who view statistics as a process of approximation. In his approach you have an unknown random process generating the data, and you don't believe that you can model the random process exactly; but what you are trying to do is to find some approximation to that random process, for example by a rational transfer function process. There is an enormous literature about this associated with the work of Adamyan, Arov, and Krein,[10] for example. There you do come into model selection, in the sense that you don't know what the order of the process is so you've got to choose those integer parameters characterizing the order by reference to the data. You don't believe that you are selecting the right model, but you are trying to determine how many parameters are worth using. Of course, Akaike was probably the first to come to this idea, so I've been greatly influenced by people like Akaike, and particularly Rissanen. I think that's an important movement but it's not really hypothesis testing. Generally, I'm rather sold on the idea of statistics as summarization of data and Rissanen's position that you're concerned with a compact description of the data. It goes back to Fisher of course.

Apart from two papers on the periodogram, your work in the last 15 years seems largely to be in the time domain. I also detect a loss of interest in frequency domain methods in econometrics (at least as a tool rather than as a pedagogic device), partly because of the nature of the data and size of samples. What is your opinion of the use of spectral methods in econometrics? Do you see any advances on the horizon which could change this situation?

As far as the contribution of time series in general, Fourier methods remain terribly important. If you look at a journal like the Journal of Geophysical Research, you'll find that practically all the statistics used in the whole journal, and it's huge, is based on Fourier methods. Of course it's necessary in a field where wave motions and so forth are the nature of things. I still work on Fourier methods. I've just written a paper with Peter Thompson on a problem which is that of delay estimation. There you cannot use a rational transfer model because $\exp(irw)$, when r is not an integer, is not a rational function and cannot be approximated by one either. It could be approximated by one if you eliminate a couple of points, $\pm\pi$. So Fourier methods are still terribly important, but I don't think they ever have been of that much importance in economics, and I think they are not going to be in the near future. They may in some cases have importance in connection with, say, seasonal adjustment or something like that, but I don't really think so. It's because economic series are nonstationary series of relatively homogeneous data, are often short, and there is a dominance by low frequencies very often. For all these sorts of reasons I don't feel, if I was brought economic data to analyze, I would usually use Fourier analysis, although I think you might occasionally think of doing some in an investigative phase.

Over the years, I don't recall ever hearing you refer to or comment upon Bayesian methods. There is currently a feeling in econometrics that our need to use prior information to overcome "bad" data may make the use of these methods essential. What do you see as the disadvantage of such an adoption?

I have never used Bayesian methods or thought about them. I think probably because I am not the sort of person who would naturally want to be too subjective; I'm really very much the natural scientist mathematical type. Furthermore, I think Pat Moran would have influenced me a great deal, and he is anti-Bayesian on the whole. However, I am a person who would be eclectic about this, because I doubt if probability is any more than a device used by scientists to understand the real world, and if Bayesian methods work in a particular context I'd use them, and I'm sure there are situations where you would be well advised to take account of subjective prior feelings in a Bayesian way. But, quite frankly, the notion that every problem must be formulated in the Bayesian way is, I think, silly. I don't believe it.

I wonder if you might point to what you regard as your most important articles, and why you regard them as such.

Among the early papers there is a paper [1956*] which is about the asymptotic powers of tests for multiple correlations. It has some significance and has often been referred to. It relates to the idea of asymptotic relative efficiency applied in situations where your statistic is a vector statistic, and you can handle it through the ideas introduced there. There was a series of papers which are first mentioned in [1960*]. This paper is concerned with the estimation of seasonal variation, and has been neglected to some extent because it was meant to be published in the first issue of the Australian Journal of Statistics, but it was not in time for that and appeared in the second one. It started off a bit of work in seasonal variation. There is a paper [1969*] which is the identification paper. That has had some influence and it goes with [1971*] which is the one in Econometrica on identification. There are two papers, [1972*] and [1976*]; [1972*] with Chris Heyde on limit theorems for quadratic functions of discrete time series, and [1976*] with Bill Dunsmuir. They were amongst the first papers to use theory based on Martingale differences for ARMAX and other models and they've had some influence. There is a whole series of papers, including [1982*], which is I think the best paper I ever wrote, and it appeared in the Festschrift for Pat Moran. They are about Akaike's ideas in connection with the estimation of the order of an ARMA system by Akaike's methods. There are two papers, one with Rissanen and one with Kavalieris, who is now at MIT, on recursive estimation of ARMA systems ([1982‡] and [1984*]). [1982‡‡] is a good paper on autocorrelation and autoregressive approximation. That has some significance, because once you adopt the idea that you are going to be concerned with finding some good approximations to the structure of the process using a class of models, such as rational transfer function models, you then recognize that, as the amount of data increases, the order will increase—as it always does in practice. That is, you will use more and more parameters as you've got more and more data, which is a sensible idea. Then, of course, since the number of parameters is going to increase with the order, uniformity problems arise. [1980*] is about recursive estimation. I've done quite a bit on recursion and I've had a very good student, Victor Solo, work on the area. Some of that work is quite difficult and that paper is not a bad one. Finally, I think [1979*], a paper on the central limit theorem for time series regression, is a good paper, and I personally found it useful. Whether anybody else has I don't know.

Just on that question of approximation, could you expand on the ideas underlying that approach?

There is a paper by Rissanen.[11] His idea has been the following. You are trying to record the data in a computer in the minimum number of bits. Well, you could record the original data. Now, say somebody gave you the data and it was actually a series in arithmetical progression. I know this is a silly example, but just to illustrate. Well, all you'd have to record is the first number, how many numbers you had, and the difference. Three numbers would record all the data. Now, going beyond that, you've got data and a class of models you are going to use, say, ARMA or ARMAX models of any order. You are going to use those in order to reduce the amount of space in the computer taken up in recording the data. In deciding how many bits you are going to use you have got to have in mind the variability and the accuracy of the data. It is no use recording more bits than the inherent accuracy of the parameters. You put all that into the grinder and you come up with an answer which involves a modified maximum likelihood approach, Of course, it does still depend to some extent on distributional assumptions, but asymptotically it won't. So there is an approach which starts from this idea of minimal description length for all the data and which, in a sense, incorporates maximum likelihood. In fact Fisher described statistics in three ways. One of them was that it was concerned with reduction of data.

What areas do you see as good ones for research students now? For example, what do you think of the movement toward robust and adaptive estimators? Do you see important areas left to be explored in time series analysis?

There are plenty of areas left. The obvious ones being time varying parameter and nonlinear models. I think nonlinear models are the most important, outside of economics anyway. Most problems arise because of nonlinearity rather than because of nonstationarity, and a good approach to nonlinear modeling—and there has not been a really good one yet—would really take a lot of beating for a good bit of work. The trouble is that I think anything you are going to do about nonlinear modeling is going to have to be specialized. There are two approaches. One of them is to try to describe everything in terms of expansions and Hermite polynomials and so forth. I'm sure that won't work, because it is too general. You have got to get down to something special. Now Tong has tried that with these threshold autoregression models, and there has been some work by Ozaki which I think is very good, but I don't think anything has quite grabbed the field yet. Maybe it's impossible to do it. I think that adaptive estimation is very important, particularly in connection with real-time calculation. There is a good book written about it recently by Leonard Ljung and there is a lot of work to be done of a theoretical kind as well as a practical kind. That's a really good field I think, a really good field. The other one you mentioned was robust estimation. Well, that's going to be important too. But there are some problems. In connection with an autoregression, for example, the residuals are used in the first step to get the empirical distribution function, but at that stage they may be absolutely terrible, because the first estimate of the autoregressive parameter may be so awful. So it may not work at all.

Finally, some personal questions. You have visited and taught in American universities a number of times, but never seem to have visited the U.K. for an extended period. Could you tell us why this is so?

The first time I ever went away, apart from the war to New Guinea, was in 1959, and I really had to get some money to help me. I had a wife and three children and I sold everything I had. I did not own a house, but I sold typewriters and everything to raise the money, because I had no travel allowance. I had to get some money so I got it from the University of North Carolina. I could not have got enough money from England to pay for my costs. Having gone to America I established contacts and was invited back in 1964 by Geoff Watson, who was a great friend of mine, and went to Johns Hopkins. It was really because I was invited and money was offered to me each time that I went to the U.S. After 1965 I did go to England, and I have the greatest admiration for English statistics and English statisticians. My traditions are really there through Pat Moran, but it just is a hard place to go to unless you've got the funds. You see, the other thing was that I started academic life late, so by the time I started to go away I already had three children, and its a different matter from when you are a young unmarried chap—then you can go anywhere.

Just following up this theme, it has always seemed to me that unlike the case of economics, where Americans dominate, statistics has never been as strong there as in England.

It hasn't been true for statistics in the past but it's becoming true for statistics now as well. David Cox is possibly the greatest living statistician. He and John Tukey would be the names you would think of first. Statistics is still terribly strong in England, but I think now the center of statistics in a sense is in California in Stanford and Berkeley. If I were to name the two great places I would name Stanford and Berkeley.

Continuing on from the last theme, many of the statisticians at ANU during the 1950s went overseas, and this trend seems to have continued with Hartigan, Pollard, etc. What was it that made you stay?

I think staying is a disadvantage. Isolation in Australia is a disadvantage and I could give all sorts of examples of this. The reasons I stayed were threefold. First of all, by the time I had the opportunity to go, I had too many commitments in Australia. It wasn't until 1959 or 1960 that I did have a chance to go, and then I would have been 39. I had a wife and three children, a twin sister who is my only close relative, and my wife has a sister who is terribly close to her. So, we would have had to cut all those ties, and it's terribly hard to do that when you get to 40. Secondly, I am basically a terribly cautious person underneath. I've only ever really worked for two institutions—the Commonwealth Bank of Australia and the Australian National University, and I think I found it easiest to stay where I was. Finally, the other reason was that I got a Professorship in Australia fairly early, at 38 or 39, and so that meant I was well off in Australia. So, caution, the fact that I had a good job in Australia, the fact that I am Australian, very Australian in my way, and finally because its very difficult to move when you get to a certain stage because of your commitment to your family, were my reasons for staying.

Do you feel that this decision to stay hampered your research in any way?

Yes. I think it's both an advantage and a disadvantage. I talked to Peter Hall about this recently, who is a man for whom I have great admiration. Peter feels it's almost an advantage not to be caught up in the current trends, and there is an element of truth there. If you are in America there is a tremendous tendency to being pushed, by your own inclinations I know, by those around you into following the current fashion. Whereas if you are isolated you may be safe from doing that. But, I think the greater danger is that you lose contact. You know for example, I did not hear about the Kalman filter until I went to America in 1964/65, and it did not fully invade my thinking as it should have done. So in my book there is only a small section on it. This was a mistake and I think that it was partly due to isolation. I could give other examples. Isolation is a big disadvantage, and I think I would have benefited from going to the United States.

NOTES

1. Later, Professor of Accounting, Australian National University.
2. Later, Professor of Economics, University of Melbourne.
3. Then Governor, Reserve Bank of Australia.
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