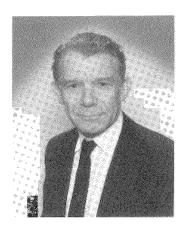# THE ET INTERVIEW: PROFESSOR ALBERT REX BERGSTROM

Interviewed by Peter C. B. Phillips

In the previous interviews in this series, we have learnt from Denis Sargan and Jim Durbin about the role that was played by the London School of Economics (LSE) in shaping the development of econometrics during the 1960s. Two other figures who were central to the initiatives that were taken in this transitional phase at the LSE were the New Zealand born economists Bill Phillips and Rex Bergstrom. Unlike Bill Phillips, who was schooled and apprenticed at the LSE, Rex Bergstrom was trained as an economist in New Zealand and worked there as an academic throughout the 1950s before joining the LSE in 1962. Rex was already an accomplished econometrician when he came to LSE. His econometric study of supply and demand for New Zealand's exports had attracted international attention in 1955 when it appeared in *Econometrica*. It was the first large-scale macroeconometric model to be estimated by the new Cowles Commission methods and it predated subsequent work on international macroeconometric models by some fifteen years.

Since 1966 Rex has been the world's leading proponent of continuous-time econometric modeling. His contributions cover the entire field of research encompassing theoretical work on cyclical growth models, econometric methods of estimation, and a major empirical implementation of the methodology to the U.K. economy. The latter, which is now popularly known as the Bergstrom–Wymer model, has become the prototype for

many subsequent models which have been developed by research teams in the central banks of industralized countries.

Rex Bergstrom's research bears the hallmark of exemplary scholarship. A deep concern for fundamental issues is always evident in his writing; he is a lucid thinker and a fine expositor. There is real follow through in his work, reflecting a desire that is present in all good scholarship to report only what has been properly researched and fully thought out. Above all, Rex is an economist's econometrician with a fervent interest in economics as well as deep respect for mathematics and mathematical statistics.

Only part of Rex's career has been spent in New Zealand but his impact on the professional community of economists in New Zealand has been enormous. Many New Zealand economists themselves carry the Bergstrom pedigree, as do many expatriots who live overseas in Australia, England, and North America: some are now in business, some in academe. Many others have been influenced by his work and his style of research through the teachings of his students. All of us who have been fortunate to have had Rex as a teacher know that we have studied under a powerful and intense mind whose lessons and inspirations have the special quality of enduring relevance.

In August 1987, the recently formed Australasian Chapter of the Econometric Society held its annual meeting in New Zealand. These were the first meetings of the Econometric Society to be held in New Zealand and they took place at the University of Canterbury, where Rex had himself studied when it was a College of the University of New Zealand. Before the conference on August 22, Rex and I met in the Economics Department of the University of Canterbury and recorded the following conversation.

Would you like to start by telling us about your schooling in New Zealand? Did you develop academic interests early in life? Were you attracted to the sciences, the humanities, or both?

I attended the Christchurch Boys High for four years, 1938–1941. Christchurch Boys High was the leading academic state school then in Christchurch and probably still is. In those days the top form had to do six compulsory subjects for three years. Then we sat the University Entrance examination and later there were opportunities for specialization in the sixth form. The six compulsory subjects were English, Mathematics, Chemistry, Latin, French, and History. Mathematics was my first love, intellectually, and has remained so throughout my life. Mathematics and Chemistry were my best subjects at school and I was at the top all the way through in those subjects. But I dropped chemistry in the fourth year and substituted a course in economics which was being given by one of the teachers on an experimental basis. The reason was that by this stage I had decided to do a commerce degree rather than a science degree, which would have been the obvious thing

for me to do on academic grounds. The economics course was fairly general and elementary, but it did stimulate my interest in economics and I looked forward to doing some economics as part of my B.Com degree.

At that time was applied mathematics also a subject in school?

It was. But we didn't take it as a separate subject. It was probably an option for the scholarship examination. Mathematics was all one subject in the mainstream course.

Then you went off to study at Canterbury University College. That must have been during the early war years.

Yes. I left school at the age of sixteen and took a full-time job. That was in 1942. At the same time I enrolled as a part-time student for a B.Com at Canterbury University College and commenced studying for the Accountants' Professional Examinations. In those days Canterbury University College was one of the colleges of the University of New Zealand. The University of New Zealand was made up of four main colleges: one in each of the main cities plus two agricultural colleges. The B.Com degree was a very convenient degree to do part time. Normal office hours were from 8:00 A.M.–5:00 P.M., and formal lectures for the B.Com were after 5:00 P.M. It was a fairly hard life. In addition to working in the evenings and weekends, I often used to get up at 5:00 A.M. in the morning and do an hour or two of work before going to the office. That went on for five years. After five years I had completed the B.Com and the Accountants' Professional Examinations. Also in the same period I had a year off in the Air Force.

Was your main objective upon entering University to take up a career in accountancy or did you have other possibilities in mind as well?

I did initially plan to qualify both as an actuary and as an accountant. I thought that actuarial work would give me an opportunity for application in mathematics. In addition, both the actuarial and accountancy professions were quite well paid. I thought it would be useful to have qualifications in both of them. But, in fact, I never started on the actuarial examinations because I soon became very interested in economics and decided to become an economist.

How did your interests in economics develop? Were there teachers that you found particularly stimulating, or was it simply the subject itself?

I think it was the subject itself, although Alan Danks and Wolfgang Rosenberg were my main teachers and they certainly stimulated me to read all the right books. That was probably later on when I took graduate work.

Did you get to read Keynes' General Theory as an undergraduate?

This, in fact, was the thing which finally made me make up my mind to become an economist. During the last year of my B.Com I was seriously considering becoming an economist and during the Christmas vacation, following completion of my B.Com, I read the whole of Keynes' *General Theory*. I found it quite exciting to have a complete system of equations determining all the main macroeconomic variables and synthesizing real and monetary phenomena. I decided then definitely to become an economist. I enrolled for the M.Com degree.

How far did you continue with mathematics at university?

When I did the B.Com, mathematics was not on the list of optional subjects, unfortunately. But after completing my B.Com and M.Com degrees and before going to Cambridge, I did do the first two years in pure mathematics for the BA in mathematics. That is Stage 1 and Stage 2 mathematics, as they were called in New Zealand. I attended the lectures and sat the examination in Stage 2 mathematics while I was a junior lecturer in economics at Auckland University. That was a year before I went to Cambridge. During that period I also spent a lot of time reading books on mathematics outside of a formal course work. I think that the most important book on mathematics I read at that stage was Hardy's *Pure Mathematics*. Although it is only an elementary book, it is beautiful and rigorous and it has had a permanent influence on my approach to mathematics. I also spent a lot of time reading mathematics when I was at Cambridge and I have continued to read many books on mathematics throughout my life. Some of the books which I found most useful were not published until long after I had finished my Ph.D. Examples are Halmos's *Finite Dimensional Vector Spaces* and Kolmogorov and Fomin's *Functional Analysis*, both of which I read completely after I finished my Ph.D.

You mentioned that you served with the Air Force during the War. What was the nature of your work for them?

I went into the Air Force in early 1945 when I was 19. At that stage I had my name down to be a navigator. I had been in the air training corp at school and I knew a bit of navigation, which was just the application of elementary trigonometry. Two months after I went into the Air Force the War ended in Europe and at that stage they decided that it wasn't worthwhile training any more air crew, so I transferred over to the accounts section. The War ended completely when Japan surrendered a few months later and after that there was still a lot of work to do winding down the whole operation. I was in the Air Force for about six months after the War ended. So it occupied about a year all together.

You went on to do your M.Com in economics and got First Class Honors. Did your degree entail course work and a thesis?

It entailed course work and the choice of a thesis or writing an essay, which was on a list of optional topics which one was presented with at the examination. I chose to do the essay. I got quite a good mark for it. But it was during the M.Com work year that I did most of my reading. As I mentioned earlier, my teachers Alan Danks and Wolfgang Rosenberg stimulated me into reading all the right books. The most important book which I read during my M.Com year was Hick's *Value and Capital*. At the time when I read this, it was the most exciting book, intellectually, which I had ever read. More exciting than Keynes' *General Theory*, and I read the complete book, not just the first part, as many people read. For years afterwards I thought that it contained the answer to every economic question that I could think of. I still think that many of the ideas which have subsequently been attributed to other people can be found in *Value and Capital*. Other books which I read during that year were Wicksell's *Lectures in Political Economy*, Marshall's *Principles of Economics*, Pigou's *Economics of Welfare*, Chamberlain's *Monopolistic Competition*, Joan Robinson's *Economics of Imperfect Competition*, Haberler's *Prosperity in Depression*. Ohlin's *Interregional and International Trade*, and there were several others. At that time Samuelson's *Foundations of Economic Analysis* had still not been published. But I did read that several years later, after it was published.

That was a busy year's reading in economics. Did you do any statistics as part of your degree?

I did a statistics course, but it was very elementary. I could do a regression, but I didn't know much about statistical inference at that stage.

After your M.Com degree, you won a scholarship to travel overseas, but before doing so you took up teaching positions at Massey and at Auckland. Can you tell us about this part of your career?

The job at Massey was an assistant lectureship in economics. This was my first academic post. Massey College was an agricultural college and also part of the University of New Zealand. There were only two of us in the department: Brian Low was the head of the department and my job was to give the lectures in pure economics for the students doing the bachelor of agricultural science degree. It was at Massey that I started to be seriously interested in statistics. This was partly because I was surrounded by agricultural scientists who were all working in statistics. It was at Massey, also, that I wrote and published my first paper, which was just a little paper on the application of price discrimination theory to the pricing of dairy products. After two years at Massey, I moved to Auckland University College. It was only a small

department. There were four of us. But it was the best economics department in New Zealand and it was certainly the strongest department in econometrics. The head of the department was Colin Simkin. Although he was not himself an econometrician, Colin was one of the first people in New Zealand to appreciate the importance of econometrics. He had acquired a complete set of back issues of *Econometrica*, as well as the Cowles Commission papers, so there were plenty of things for me to read there. My colleague, Malcolm Fisher, who was a lecturer in the department, was also working in econometrics at that stage. It was at Auckland that I started to work seriously in the field of applied econometrics and it was there that I published my first econometrics paper. This was a paper on New Zealand's export supply function published in the *Economic Record* in 1951. I also had a wide teaching experience at Auckland, as one has to in a small department. I taught courses in money and banking, public finance, and all the third-year economic theory courses. As I have already mentioned, I also spent a lot of time studying mathematics during those years.

In 1952, you traveled to Cambridge in England to do your Ph.D. What made you select Cambridge University?

The immediate reason was that Richard Stone was there. I had read his papers on demand analysis and other aspects of econometrics and I hoped that he would agree to be my supervisor, which he did. But there were more general reasons for going to Cambridge. In addition to being a famous center of economics, Cambridge was one of the world's leading centers of science and mathematics. Even when I was at school and still thinking of doing a degree in science, I had aspirations of going to Cambridge. Then, after I had finished my degrees in economics and became intensely absorbed in the study of mathematics, I seriously considered going to Cambridge to do the mathematics tripos. But I decided that in any case I could spend a lot of time reading mathematics while I was at Cambridge doing my Ph.D, and I did. Although I didn't do any formal course work there, a lot of my friends and associates at Cambridge were scientists and mathematicians.

When you went to Cambridge did you already have a topic for doctoral dissertation research in mind?

Yes, I had actually planned and formulated a model which I proposed to estimate. It arose out of my early work on the export supply function. I decided to disaggregate this and have a separate demand and supply equation for various products. I had formulated a complete model before I went to Cambridge and, indeed, had already collected some of the data for the New Zealand side of the model.

In *Econometrica* in 1955 you published your article on the market for New Zealand exports and this was one of the first, if not the first, empir-

ical implementations of the LIML estimator. Can you tell us about that research?

It arose out of my doctoral dissertation. The model was quite a large simultaneous equations model involving U.K. demand equations and New Zealand supply equations. In those days the New Zealand economy was just a subbranch of the U.K. economy. Almost the entire exports of lamb, mutton, and dairy products went to the United Kingdom and these provided most of the export receipts. They were a very important part of the New Zealand economy. The supply side of the model was quite elaborate, with interrelated equations determining output and livestock numbers and based on a dynamic model of the firm. Also, it was necessary to treat all the main New Zealand macroeconomic variables as jointly dependent, endogenous variables because of the important effect that the export market had on the New Zealand economy. The complete model had 27 equations and I estimated 55 parameters by both ordinary least squares and limited information maximum likelihood. I did most of the calculations on an ordinary electric desk calculator, a MADAS. But I did invert some of the large matrices on the old EDSAC computer, that is, electric delay storage automatic computer, which was one of the first two electronic computers in the U.K. But often this machine was broken down just when I wanted to use it and I certainly did invert some 10 × 10 matrices on the desk calculator. There were no econometric textbooks in those days dealing with LIML. Klein's book had not yet been published, so one had to learn LIML by reading the Anderson and Rubin articles. At the time when my paper was published, I think it was possibly the first paper to give strong empirical support to the new method. I estimated the model from prewar data and then tested the post-sample predictive performance against postwar data. The predictions obtained from the LIML estimator were better not only than those from the least squares estimator, but also than those from the two commonly used naive models in those days. One of these was just that there would be no change between years t and $t+1$ and the other was that the change between years t and $t+1$ would be the same as between years $t-t$ and t. It was considered to be quite an achievement just to get a model which just predicted better than either of these two. In the years following the publication of the article in *Econometrica*, 1955, it got quite a bit of publicity and was referred to by Klein, Koopmans, Wold, and several other leading econometricians.

Your paper must have appeared around the same time as the Klein—Goldberger model?

It was before the Klein–Goldberger model. Klein's first simultaneous equations model was published in 1950 and the Klein–Goldberger model, which was really a sort of Mark II Klein model, was published later. My article was published in between.

Your model also had more endogenous variables and equations than the Klein-Goldberger model.

Yes, it did.

What would you say was the main stimulus to you to work in econometrics?

I think Haavelmo's papers were the main stimulus and then the Anderson and Rubin papers and other Cowles Commission papers. I think another stimulus was Klein's first econometric model, his 1950 econometric model of the United States.

Much of your research in the 1950s was concerned with the New Zealand economy and dealt with various applied macro and international aspects of small open economies. Were there other economists in New Zealand doing empirical research on these issues at that time?

No, I was certainly the first person to apply econometrics to these issues. People were interested in what I was doing.

Did you feel isolated at that time from the growing activity in econometric research overseas, particularly in the United Kingdom and in North America?

I suppose I felt a bit isolated. But, as I said, my colleagues in the economics department at Auckland were interested in what I was doing. So also were the mathematicians in New Zealand. I was once invited down to Wellington to talk to the staff seminar of the applied mathematics section of the Department of Scientific and Industrial Research. This was reputed at that stage to be the best mathematics department in New Zealand, better than any of the University departments. One of the participants in the seminar was Peter Whittle, who had recently completed his Ph.D under Herman Wold in Sweden and had returned to New Zealand for a few years before finally settling in England.

In 1962, *Econometrica* published your article on the finite sample distributions of least squares and maximum likelihood estimators of the marginal propensity to consume. Your paper and Basmann's related research marked a watershed in econometric research, opening up a new research program on finite sample theory. How was it that you came to work on this problem?

I first started to think about working on this problem when I was at Cambridge, because in those days there was a lot of opposition to the new methods. Whenever I gave seminars, I was in the position of defending the new simultaneous equations methods against least squares. I decided that the most effective way to answer some of the criticisms would be to find the

exact distribution of some of these estimators. But I didn't start doing that until several years later when the stimulus was the publication of Nagar's article on approximations to the moment matrix of the k-class estimators. Shortly after that I decided to try to find the exact distribution. I thought that the marginal propensity to consume in the basic stochastic Keynesian model would be a very good point at which to start, because this model had gained a lot of publicity through Haavelmo's famous paper in *JASA* in 1949. I think Theil had also found an approximation to the distribution. So quite a lot of people were interested in the problem.

Did the analytical results come easily?

The maximum likelihood estimator was, of course, quite a straightforward exercise, but finding the distribution of the least squares estimator was much more difficult, because there was no obvious way of proceeding. The key simplifying step was an orthogonal transformation which I made at an early stage of the proof. This is the sort of idea which somehow just comes, if one thinks about a problem for a while. In fact, I think I spent only a few weeks producing the paper and some of this time was spent doing the numerical calculations for the graphs and tables. I first met Haavelmo in 1962 at the LSE, just about the time that the paper was published. When I told him that I had found the exact distribution of the least squares estimator, he said that he didn't think anyone would ever find that in the closed form, in terms of known functions. In fact, it is interesting that it comes out as just a finite number of terms, in terms of just products of rational functions and exponentials of rational functions.

There are few finite sample distributions which can be expressed so simply and without relying on the special functions of applied mathematics. Have you ever thought of returning to that topic?

I did for many years think of returning to the topic, because to me it is a very attractive topic, mathematically. But I am sure that I will never return to it now because you, in particular, have done so much and pushed it so far forward, that one feels that the field is almost cleaned up.

After working at Auckland University in New Zealand for more than 10 years, you moved to the London School of Economics in 1962. How did this come about?

I was on leave at the LSE from the University of Auckland in 1962. At that stage, I had either just completed or was working on several topics which were of interest to people at the LSE. I had just published my paper on finite sample distributions. While I was there, I produced my first paper on cyclical growth models and presented it at the famous Robbins seminar. I was also, in that year, starting to work on my continuous-time estimation problems. They knew that I would be interested in a post if one came up and, in fact,

as the result of a resignation, a readership did become vacant. They offered it to me and I accepted. I am sure, that even if the job had not come up at the LSE, I would have stayed in London. London was the center of the world for me and I had never really regarded myself as permanently settled in New Zealand. At that stage, I was seriously considering leaving academic life and taking a job in the city. I did, in fact, have an offer of a job in the city and probably would have accepted it if the job at the LSE had not come up.

What were your impressions of the LSE in the early 1960s? Was there any sense that this was an important period of transition and development for the LSE in econometrics?

Yes, there was. I think that, indeed, it was a very important period of transition. It was during this period that the LSE replaced Cambridge as the leading center of econometrics in the United Kingdom. After Denis Sargan arrived in 1963, there were four of us working in the field of econometric theory and econometric methodology: Jim Durbin, Bill Phillips, Denis Sargan, and myself. Then, in addition, there was another larger group which was very interested in applied econometrics and there was a regular weekly seminar devoted to applied econometrics. Some of the people who used to come to that seminar were: Dick Lipsey, Max Steuer, Chris Archibald, Morris Peston, and others. In those years, also, we were planning to introduce the first taught Masters course, although that didn't get going until shortly after I left the LSE.

Do you have any special memories of that time; seminars, courses or incidents that seem significant to you now?

I remember Jim Durbin producing his algorithm for obtaining full information maximum likelihood estimates, which somehow got lost for a number of years, until it was resurrected many years later by other people. He did present it at Copenhagen at the conference and then he got a computer program written to implement it. He asked me to design an econometric model to apply it. We did do this and completed a model but it was never published because after that I returned to New Zealand and I had decided that the best way to do dynamic econometrics was in continuous time.

It was during this period that your research developed two major themes. One was cyclical growth models and the other was continuous-time estimation. Would you like to talk to us about the source of your interest in these fields and the main objectives of your research?

Yes, let me talk first about the cyclical growth model. In 1962, I had been thinking for some time about doing some work on cyclical growth models, with the particular object of introducing more economic theory into the models and introducing a price mechanism. In those days, cyclical growth

models were very mechanical. The cycles were produced just by the interaction of the multiplier and accelerator and the growth was produced just by exogenous trends. There was nothing in the models to ensure that over a long period aggregate demand would follow supply. When I had arrived at the LSE in early 1962, I was quite excited to find that Bill Phillips had just published a paper on cyclical growth introducing Keynesian-type feedback mechanisms and synthesizing real and monetary phenomena. This was very much the sort of thing that I had had in mind. Also, I was not surprised to learn that the main stimulus for his work had come from James Meade. James Meade had himself been working on this problem and it was out of his discussions with James Meade that he ended up writing his own paper. There were some aspects of his paper which I found rather unsatisfactory. My first cyclical growth paper, the one published in *Economica* in 1962, was largely an attempt to deal with these, while at the same time keeping as close as possible to Bill Phillips' basic framework. In particular, I retained his rather extreme assumption of a single technology, but I introduced the effect of the price mechanism by developing a pseudo production function which worked through variations in the age distribution of the stock of capital. At that stage, I was also starting to think about the estimation of continuous-time models. This is really another branch of research which was stimulated by another article by Bill Phillips, his 1959 *Biometrika* paper.

Your monograph on *The Construction and Use of Economic Models* brought together many of these developments and is a masterpiece of exposition. Students at Auckland University in my generation used to have competitions among themselves to find even a single typographical error in your book. What inspired you to write it?

I was invited to write the book by English University Press for their applied mathematics series. The most famous book published in that series was Peter Whittle's *Prediction and Regulation*. The Editor at English University Press who was handling the series told me that Peter Whittle had suggested to him that he invite me to write a book on economic models for the series. I was pleased to do this. My main object in the book was to develop what I called "prototype continuous-time econometric models." Indeed, I think, from this point of view, the book was quite successful because the cyclical growth model developed in the central chapters of the book did, in fact, become the prototype for the first continuous-time econometric model of the United Kingdom. The general methodology developed in the central chapters of the book that deal with formulations and stability analysis have since been widely used in continuous-time econometric models.

In 1964, you returned to the University of Auckland. Did you find it difficult to maintain your ongoing research agenda there, after the lively research activity of the LSE?

I was very busy during my first year writing my book *The Construction and Use of Economic Models*. I commenced this at the LSE, but I finished it during my first year at Auckland. After that I had a number of other things to do. I had several new books which I wanted to read, including Malinvaud's *Statistical Methods of Econometrics* and a number of new graduate courses to plan. During one long vacation, I also went up to Bankok and spent nine weeks working as a consultant for the Economic Commission for Asia and the Far East. There was a bit of a lull in my research then as I began to look forward to my next major research project, which would be a new continuous-time model of the United Kingdom. I hoped to return to England in a few years and decided that I would start work on that.

During this period at Auckland, you developed a graduate econometrics course based on the most rigorous and general parts of Malinvaud's *Statistical Methods of Econometrics*. This course must have been very different in character from typical econometrics courses in other parts of the world. What made you choose to design your graduate econometrics course this way?

It was really Malinvaud's book itself. I had read enough of the French edition to realize that the book was in a completely different class from any other econometrics book which had been published. I decided then that, as soon as the first English edition came out, I would base a graduate course on it — which I did. The central chapters of that book were very much to my taste. Although general and rigorous, there was a continuous logical development running from the basic linear model in Chapter 5 through to the nonlinear model in Chapter 9, and beyond that into simultaneous equations models. This appealed to me very much as the basis for a course, and I continued to give that course after I moved to the University of Essex. I had a remarkable group of students during that period at Auckland and many of them now have worldwide reputations in econometrics. Apart from yourself, my students at Auckland during that period included Cliff Wymer, who was just finishing off his masters thesis, in 1965. There was also Roger Bowden and Viv Hall. In addition to these students who have made careers in econometrics, I had some brilliant students who did my advanced econometrics course and then moved into completely different fields. Of these the most notable is Hugh Fletcher, who is now the chief executive of the biggest company in New Zealand.

What do you think caused this surge of good students at Auckland during the 1960s?

I don't know. Quite a few of them came over from the mathematics department and had done complete degrees in mathematics or the equivalent. You, yourself, virtually did two degrees simultaneously in mathematics and economics. And Roger Bowden had done a degree in mathematics.

Many of these students not only followed your graduate economet-
rics course but did research for their masters theses under your super-
vision.

That's right. I helped Cliff Wymer towards the end of his master's thesis,
then I supervised Viv Hall's Ph.D. thesis and your master's thesis.

Looking back on your graduate econometrics course at Auckland
now, do you feel that the level of the course was comparable or higher
than that of other courses overseas with which you were familiar, like
those at the LSE, for example?

I felt that it was at least as high, probably a bit higher. I have subsequently
talked to students who have come back from places like Stanford and said
that many faculty there were surprised at the level of the courses they had
already done — people like Hugh Fletcher and George Wheeler, for example,
who subsequently went to Stanford.

A second theme to emerge from your research in the 1960s was
econometric modeling in continuous time. This involved both theoreti-
cal and empirical research. Can you tell us about this general research
program which you initiated in the 1960s?

Yes, the theoretical work was stimulated by reading Bill Phillips' 1959
Biometrika paper. I had, several years earlier, read Bartlett's paper in *JRSS*
in 1946, but it was not until I read Bill Phillips' paper that I became con-
vinced that continuous-time econometric modeling was really feasible. It was
at that stage that I decided to do some work in the field. In fact, I was work-
ing on finite sample distributions at the time, but I decided that as soon as
I finished that I would move on to do some work on continuous-time esti-
mation. I had already planned my approach when I arrived at the LSE in
1962. Although Bill Phillips' article was the immediate stimulus to my work,
my approach was completely different from his. My approach was to use the
exact discrete analog in order to investigate the sampling properties of esti-
mates obtained from an approximate simultaneous equations model. I also
aimed to link up this research with the debate on independent versus causal
systems, which had been initiated by Herman Wold, a debate which was still
quite active at that stage. When I arrived at the LSE and told Bill Phillips
what I was planning to do, he was very interested, even though my approach
was different from his. He made available computing facilities for me, and
I commenced work. Bill Phillips was away himself, on leave at MIT, while
the rest of the work was going on, but he arrived back in time to read my
paper before I submitted it to *Econometrica* in 1964. Denis Sargan, at that
stage, had just arrived at the LSE and he also read the paper. He himself
started to work in the field shortly after I left the LSE. I discussed these
methodological problems a bit more in my book on *The Construction and*

Use of Economic Models and tried there to link it up with my theoretical work on cyclical growth models. I decided that the next major step, having worked on both the econometric methodology and the economic theory, was to combine the two by estimating a continuous-time empirical econometric model. I was thinking about this during my period at Auckland and commenced planning the model shortly before I returned to England in 1970. When I reached Essex in 1970, I had already planned out my model of the United Kingdom, analyzed its mathematical properties, collected the data, and done a transformation to approximately eliminate the autocorrelation resulting from flow data. My former student and colleague, Cliff Wymer was at that stage a lecturer at LSE. When I told him about the project, he was very interested and said that he would like to develop the computer program and do the computing. This was a great help to me. Cliff had already written a computer program for estimating the approximate simultaneous equations model as part of his Ph.D. work and applied it to a model of financial markets. He developed it a lot further in the process of estimating my model. The model was modified a bit during these years, but it's remarkable how close the final estimated model was to the one which I had originally formulated and analyzed. We had to fix a few parameters for which the estimates were too unreliable, but I think that's about all. We estimated the model in two stages. We first estimated it by just estimating the approximate simultaneous equations model but using the transformed data to approximately take account of the flow data and the autocorrelation in the residuals. Then we used these estimates to initiate a second stage estimation which took account of the restrictions on the matrix of coefficients of the differential equation system through the matrix of coefficients of the discrete model. But we still used only the approximate moving average for the disturbances, we didn't get exact maximum likelihood estimates.

The paper was first presented at the European meeting of the Econometric Society in 1974 and then published in the book that I edited on *Statistical Inferences in Continuous-Time Economic Models* in 1976. I think that the model, in addition to being the first continuous-time macroeconomic model, had a number of other innovative features. In particular, it made much more intensive use of economic theory than previous macroeconomic models. It was the intensive use of economic theory that placed across equation restrictions on the equations of the model. It also was formulated, following the methodology used in my book *The Construction and Use of Economic Models*, in such a way as to enable us to do a rigorous analysis of the model's asymptotic stability properties. Prior to this, econometricians had tended to use linear approximations about sample means or arbitrary values of the variables in such analyses, for example, in much of the work on the analysis of the Klein–Goldberger model. But nothing can be rigorously deduced from such analyses about the asymptotic stability properties of the underlying nonlinear model. Ours was amenable to rigorous analysis. The model has, of

course, become a prototype for many other continuous-time models over the last decade.

Many researchers have pondered the merits of modeling in discrete time and in continuous time. And you have spoken and written about this yourself. Do you have any general thoughts on the subject that you would like to mention now?

I think that it is a fact that the economy does not cease to exist in between observations. This was pointed out by Bartlett in his 1946 article. Nor does the economy move in regular discrete jumps, at quarterly or annual intervals corresponding to the observations. The economy is adjusting in between the observations and it can change at any point of time. This is a fact and I think, therefore, that it must be possible to obtain some gain by taking account of this fact in our empirical work. Another point that I would like to make is that continuous-time modeling is not the same as assuming that the sample paths are continuous functions of time. This is a point which I have stressed in much of my recent work on the mathematical foundations. Many economic variables, particularly financial variables, do make discrete jumps and those jumps can occur at any point of time. I think, therefore, that it is important to formulate our models in continuous time, even though the sample paths may not be continuous functions of time.

Tjalling Koopmans also wrote about this topic in an article in Cowles Commission Volume 10.

I had certainly read the Koopmans article. I remember once discussing it with Bill Phillips. Koopmans, of course, did not get very far, but he did point out that, in principle, it should be possible to derive the exact discrete analogue for a continuous-time model but at that stage it would not have been practicable. It looked rather a daunting task and Koopmans said it would be mathematically very difficult. But in the last few years we have, in fact, done this.

Do you find it of interest that leaving aside Koopmans' article on the topic, for many years the entire research program in this field was conducted outside of North America?

It is interesting. It's just hard to know why that is the case. I think it's partly because Bill Phillips was himself at the LSE. I started to do my first work on the topic at the LSE and a nucleus of people working in the field started there. Denis Sargan then came and started to work there. I returned to New Zealand and you and other people like Cliff Wymer, became interested in the field and you both went to the LSE and became Denis Sargan's research students. So there was a nucleus from which the field could develop.

On the other hand, there was a lot of intellectual capital invested in the existing methods. This would tend to perpetuate them unless there was a

fairly strong head of steam to push the research in another direction. I think Bill Phillips was never very happy with the simultaneous equations methodology, because he had been trained as an electrical engineer. I think that he was very receptive to the ideas of Herman Wold and the idea of trying to model the economy as a system of causal relations. He had also been influenced by the work of Arnold Tustin, who was also an electrical engineer. I think the first work after Koopmans in the United States was probably Christopher Sims's 1972 paper in *Econometrica*. What stimulated him to think about the problems, I don't know. Telser had also done a little bit of work on the identification problem. After Koopmans, I think that these were the first two on the U.S. scene.

There has recently been a strong resurgence of interest in the field, covering a broad spectrum of research that includes both theoretical and empirical work. This has included a number of researchers in the North American continent, as well as yourself and others in the United Kingdom. What factors do you think have led to this revival and what developments do you see on the horizon now?

I think there are three major factors. I think the first one is the enormous development in computing technology which has taken place during the last 10 or 15 years. It would now be feasible to estimate a second-order differential equation, similar to the one which Cliff Wymer and I estimated for the United Kingdom, using stock and flow data and taking account of the exact restrictions on the discrete-time analogue that are implied by the continuous-time model. Now this would not have been feasible 15 years ago, when we first started working on that model. So I think that the realization that it is now feasible to estimate a model by these exact methods has encouraged a number of others to undertake the rather daunting task of working out the mathematical algorithms. I think the latest phase of this started with my 1983 *Econometrica* article. But this stimulated Harvey and Stock to produce their Kalman filter algorithm and that, in turn, stimulated me to do further work on computational algorithms. There are now a number of other people in the United States working on these problems like Christiano and Zadrozny. Somewhat earlier, Hansen and Sargent had been working on another method which was close to Bill Phillips' procedure and in the context of rational expectations models. There is now quite a lot of trans-Atlantic research. I think that this is the first factor.

The second factor is the effect of the empirical work which has been going on. I think that since we produced our first continuous-time macroeconometric model of the United Kingdom, there has been a steady acceleration in the output of papers on the construction and application of continuous-time models. We now have four or five research teams around the world who are very active in continuous-time macroeconometric modeling. One of the first was Jonson and his team at the Reserve Bank of Australia. Another is Gan-

dolfo and his team in Italy who have done a lot of work. There is Kirk-patrick and his team in Germany. There is another team at the World Bank. I think this activity has also stimulated people to do more work on the theoretical and methodological issues.

The third factor is the growing interest among economic theorists in continuous-time stochastic models. I am thinking particularly of the work on modeling of asset prices and related issues by Merton and others. There have also been some empirical continuous-time econometric models based on this work.

Many time series from financial and commodity markets are now near continuously observable and one finds, of course, that the variables are not continuous functions of time, as you earlier observed. In fact, they involve jump discontinuities.

That is right. This is one of the factors which has caused me to do more work on the mathematical foundations of continuous-time models and to use assumptions which will allow the innovation processes to include more general processes.

Much of your own work on disequilibrium econometric models has involved tightly specified systems with many prior restrictions delivered from an underlying economic theory. This methodology is very much in line with the original Cowles Commission approach to simultaneous equations. In contrast, alternative methodologies which rely on the use of unrestricted systems of equations have been strongly advocated by Christopher Sims, David Hendry, and others. What is your response to these different methodologies for applied econometric research? Do you see value in both approaches?

I think that given the small samples that we have to work with in econometrics, it is essential to find some way of restricting the number of parameters to be estimated, in addition to the restrictions implied by the assumed order of the system. Christopher Sims, in his important 1972 *Econometrica* article, said that econometric models should be formulated in continuous time. I fully agree with that view. I also think that in order to obtain sufficiently realistic dynamic specifications to make the best use of the data, we need to formulate the continuous-time model as, at least, a second-order differential equation system. The discrete analogue of a second-order differential equation system with white noise disturbance and mixed stock and flow data is an autoregressive moving average system that is of second order in both its autoregressive and moving average parts. So if, for example, one has ten variables, as we had in our model of the United Kingdom, then the discrete analogue of a second-order differential equation system will have 455 parameters. This is four 10 × 10 matrices of coefficients plus 55 parameters in the covariance matrix of the innovations. I don't believe it is possible, given the

sample sizes that we have in econometric work, to get reliable estimates of so many parameters. I believe that even if the Cowles Commission-type restrictions on the coefficients of a differential equation system are rather approximate, the reduction in variances obtained by taking account of these restrictions, in most cases, far outweigh the bias from imposing those restrictions. I think that we are now reaching a stage where these issues, which have been the subject of methodological debates over the last few years, can be tested empirically. I think that we have reached a stage where you can compare different types of models and their explanatory power by fitting them all to the same data set and computing the value of the likelihood function. Now that we have an algorithm for computing the exact Gaussian likelihood of a second-order differential equation system with mixed stock and flow data, one could fit a model of this sort with, say, ten variables to a set of data and compare the value with the likelihood function of various discrete-time models involving the same number of parameters. This is something which I intend to do.

I would also like to see a competition to find the model which gave the highest value of the likelihood function for a given number of parameters. One could have a competition for various numbers of parameters: for example, the best 100 parameter model, the best 120 parameter model, and so on. The data set might, for example, consist of 60 quarterly observations of the ten leading macroeconomic variables in the United Kingdom or the United States. All of the models, both the continuous time and discrete time, would be nested in the very general model which assumes only that the 600 dimensional vector of observations has a multivariate normal distribution. I think it might be necessary to require that the models made some economic sense, but I would interpret this fairly liberally. The criterion would be to find the model with the highest value of the likelihood function.

Apart from this and in a more general context, one criteria which I have emphasized in a number of papers over the last 15 years is the desirability of having a model which generates plausible long-run behavior. I should mention that David Hendry also has, in a number of recent papers, advocated this restriction in dynamic formulations. So, I think we are in agreement on this point. I am also in agreement with Christopher Sims, that there should be much more attention to modeling of complete systems rather than single equation methods.

Christopher Sims has suggested that one achieve parameter economy in vector autoregressive systems by the implementation of Bayesian priors. Do you have any thoughts about the use of Bayesian methodology in this context?

I think the introduction of Bayesian methods is one of the important developments in econometric theory. I hadn't myself thought about it very specifically, although I do to some extent regard just placing the Cowles

Commission-type restrictions on models as, to some extent, Bayesian. We know that they are approximate anyway. Ultimately, I suppose, one could do a more exact Bayesian estimation of such a model. I don't know if I will ever get around to doing it. But I think that certainly I would like to see it done.

Another important aspect of econometric modeling is specification testing and in the last decade there has been tremendous interest in this topic. In fact you could go so far as to say that it has become a veritable cottage industry. Do you feel that this is an area to which continuous-time modelers have not yet given enough attention, in contrast to discrete-time modelers?

It certainly has not been given as much attention yet. But I think that this is partly because there has been a lot of other work to do. But I see no reason, in principle, why these tests should not be applied to continuous-time models just as much as they are applied to discrete-time models. In fact, to some extent, by developing algorithms for computing the value of the likelihood we have provided the basic computational procedures for carrying out likelihood ratio tests. Then one can also develop non-nested tests, based on likelihood ratios. I have a preference for likelihood ratio tests as against, for example, Lagrange multiplier tests, because I think one can find cases in nonlinear models where the likelihood ratio tests are more powerful.

One of the main reasons why Lagrange multiplier tests have become so popular is that they are very easy to construct and interpret. One doesn't need to estimate the likelihood function twice. That economy is useful.

That's right. But it is becoming less important as computation technology develops.

Would you like to say more about your idea for a competition among modelers who advocate different methodologies and the criteria to be used in evaluating the competition?

The main criterion would be the value of the likelihood function. I did recently mention this idea to David Hendry. It seemed to appeal to him too. The main thing that one would have to be careful about, though, is to make sure that people didn't cheat by presenting a model which apparently had a small number of parameters but which was really a disguised version of a model with a much larger number of parameters. After preliminary exploration, for example, a simpler model could be formulated by introducing combinations of variables as a single variable. Now in order to avoid that, I think it would have to be necessary to have rules to ensure that the model made economic sense and to prevent people from artificially reducing the number of parameters.

Several other factors seem to be important here. For example, models do have different objectives. Your own models are as much designed to explain medium-term and long-term behavior as to deliver short-term forecasts. Other models are designed more as short-term forecasting instruments and may have poor long-run properties. Do you think it would be important to control these objectives in your competition?

I think we could have several competitions. It would be of interest just to see which sort of model had the greatest explanatory power for a given data set independent of these factors. But then we could also compare their long-term properties.

How would you control for the fact that some models have large numbers of exogenous variables and extract a good deal of leverage from these variables in terms of sample fit?

I think the exogenous variables would have to be specified and this would be part of the competition. We would specify the endogenous variables, say, the 10 variables we had in the model of the United Kingdom plus 3 or 4 exogenous variables to allow for such things as GDP and prices in the rest of the world.

In your teaching and in your research you have always been very much of an economist as well as an econometrician. I think that this has come over clearly in the conversation that we have already had. But one of the things that I have known about you for many years is that you have a very healthy respect for mathematics and for mathematical statistics. How do you feel about the possibility of training new generations of econometricians as the subject has become so much broader in scope and its mathematical requirements have continued to deepen?

I think it is really a very daunting task now for a new graduate student in econometrics. I don't think one could expect a graduate student to master the contents of the three volumes of the *Handbook of Econometrics*. I think that it is very important, therefore, for a student to concentrate on the fundamentals. I think it is particularly important for a graduate who wants to be an expert econometrician, rather than just a user of econometrics, to have a very good background in mathematics. Ideally, I think that he/she should do a degree in mathematics followed by a masters degree, including courses in economic theory, econometric theory, and applied econometrics. I think that the econometric theory course would be based mainly on the fundamentals and very much along the lines of the Malinvaud course I used to give and then perhaps with a choice of optional topics. After all, the student is going to learn a lot about some particular field or fields when he/she gets on to his/her Ph.D work.

So you don't believe that the gap between current econometric courses at the graduate level and research activity in econometrics is so wide that it's impossible for students to bridge the gap?

No, I don't think so. I think that once a student starts working on his/her Ph.D then he/she can be directed to reading the particular papers in that field.

I wonder if we can change our subject of conversation. For many years at Essex you have been involved with high-level administration for the University as well as the Department of Economics. Can you tell us about these administrative responsibilities? Have they placed a major burden on your time for reading and research?

I spent about 10 years altogether in major administrative roles at Essex. I was successively Dean of the School of Social Sciences, Chairman of the Department of Economics, and a Pro-Vice-Chancellor, with gaps of just a year or two in between these posts. They certainly took a lot of time away from my research. For example, when I was Pro-Vice-Chancellor I was on 15 different committees and chairman of five of them. I think that I was a very efficient administrator but, even so, it took a lot of time away from my research. I tried to keep up with my reading so that whenever I did have a few weeks to spare, I could get back to some research.

Have there been any positive externalities to you from those administrative responsibilities?

I think so. One of the positive externalities was meeting and working with men from outside academia on the council, and various committees of the council. Often they were men who had got first class degrees about 40 years ago and then got into management and were now chief executives of their companies. I think another externality was being involved in the central financial planning of the university during a very difficult period. I was on all the key committees and, in particular, the important finance committees which were involved with the planning of the university and university finances. And I am pleased to say that during the three years that I was the Pro-Vice-Chancellor chairing the main subcommittees of Finance Committee, the university accounts show a healthy financial surplus. This was during a period when many other universities in the country were close to insolvency.

During the course of your career you must have seen many phases in the development of econometrics as a discipline. Looking back, do any milestones seem particularly important to you now?

I think 1943 was certainly a milestone year. This was the year of Haavelmo's first paper on simultaneous equations models and also the Mann and Wald

paper which was the beginning of rigorous asymptotic theory. The next mile-stone was in 1949 with the Anderson and Rubin papers, which was the begin-ning of single equations methods and which made simultaneous equations models feasible in applied econometric work. After that I think the subject began to develop very rapidly and it's easier to pick out important develop-ments, rather than particular milestones. I have always tried, myself, to work in the fields which seemed to me to be the most important. So obviously I must regard both the finite sample theory and continuous-time modeling as two of the important developments. In finite sample theory, work on Edge-worth approximation theory is another development and here the important early papers are by Sargan and Mikhail and Anderson and Sawa in the early 1970s. Another important development during my period as an econometri-cian was the development of Fourier methods of time-series analysis, which began with the important papers of Peter Whittle in the 1950s. But then it was extensively developed by Ted Hannan. Although the methods have not been much used in econometrics, I think they have been important in devel-oping the asymptotic sampling properties of estimators. There has, of course, been a move back towards time-domain methods. I think another important development is the rigorous analysis of the asymptotic properties of estimation of nonlinear models in which the early papers by Malinvaud and Jennrich in the late 1960s and early 1970s were important. Although I have not myself done any work on Beyesian economics, I think that this has been another important development which I particularly associate with Dreze and Zellner.

Within each of these fields, one could pick out milestones. Certainly in the field of finite sample theory there are several of your own papers that I am sure will come to be regarded as milestones. Particularly important, I think, are your 1980 and 1985 *Econometrica* articles dealing with instrumental vari-ables estimation in the most general case, and then later the complete SUR regression model. These involved the introduction of your application of zonal polynomials and then the fractional matrix calculus.

Another major area of development has been microeconometrics.

I should have mentioned microeconometrics and I think I should have men-tioned some more applied work. All of the things I mentioned earlier were theoretical developments. In applied econometrics, the Klein models were certainly milestones: the first Klein model of the United States in 1950 and later in 1955, the Klein–Goldberger model. In applied microeconometric work, I think one of the important early milestones was another English con-tribution: Stone's original work on the linear expenditure system, which was one of the earliest papers on demand systems. Most recently, there has been rational expectations.

In your own research you have achieved a balance between empirical research and theoretical work in econometrics. Have you been happy with this balance?

I think I've personally been fairly happy with it. Although I sometimes wonder if I tried to spread the field too widely. But I think this reflects my general approach to econometrics and to some extent I think I was influenced in this way back in Cambridge by Stone. This was very much his approach: to formulate a 12-year project, develop a model, devise methods of estimating it, then set about estimating it and so on. I think that this is very much what I have done in terms of my continuous-time modeling work right from the theoretical model to the development of the estimation techniques and through to the application.

I have always found you to be a tremendously optimistic person about econometrics generally and econometric research in particular. Is there any particular reason for this? How useful do you now see econometrics to be in terms of understanding economic phenomena?

Given the limitations of the phenomena we are trying to model, I think econometrics certainly must be the basis for a scientific approach to economics. But I sometimes get a bit depressed about economics and think how nice it would be to be a physicist or a chemist where things remained unchanged. One feels that the structure of the economy is continually changing and as soon as one can get to the stage of explaining one period or another, the economy changes. You make some mention of this in one of your own finite sample papers: that one of the important reasons for finite sample theory is that the longer the period we estimate, the more parameters one must introduce and the more variables we have in order to allow for structural changes. I think there are some relations and some parameters which will remain fairly constant over a long period, but mixed up with these are other things which are changing all the time, this makes modeling the economy very difficult over a long period.

It is interesting that we are doing this interview now in New Zealand where there has recently been a most dramatic instance of institutional economic change through comprehensive financial deregulation. As someone who has seen both the European and American systems from the comparative isolation of New Zealand, do you perceive major differences in thinking about economics and econometrics between the two continents?

I think I am not really very qualified to answer that question because I have spent most of my active academic working life in England and I have never worked at all in an American university. Also, probably because I've been

so heavily involved with university administration and never even visited an American university for more than a few days. I do think that Europe and England, in particular, have fallen behind America in economic theory during the last 40 years. Forty years ago when I was a student, most of the most important recent developments in economic theory had been made in England by Keynes, Hicks, and others. But I think that most of the important theoretical developments in the last 20 years have been made in America. On the other hand, I do think that in England we are still quite strong in econometrics as compared to America, especially if one takes account of the differences between the populations of the two countries. So I think that there has been a shift in the relative balance between economic theory and econometrics between England and America. Now I know that that doesn't really answer your question, but I think that is about all I can say.

Do you feel that you had any particular advantages coming from a small country like New Zealand or do you feel that it was a disadvantage?

I think it may have had the advantage that I've been less likely to be indoctrinated with a particular point of view. I read rather widely and I think that throughout my life books have probably had a more important influence on me than personal contacts with people. Of the important books I've read, and I've already mentioned Keynes' *General Theory*, Hicks' *Value and Capital*, and Hardy's *Pure Mathematics*, there is one which I haven't mentioned and this is Cramer's *Mathematical Methods of Statistics*. This was one of the most important books that I read when I was at Cambridge and this became the bible of statistics for me. Of all the books on my shelf, it is the book that I have used most.

What thoughts do you have about the future of econometrics in the course of the next ten years, let us say?

I think that among the most important developments will be those which take advantage of the enormous changes in computing technology that have taken place over the last few years. I have already said a lot about continuous-time modeling, and obviously this is one of the important developments which is going to continue but there are also others. The development of more exact methods of hypothesis testing and confidence intervals based, for example, on Edgeworth approximations, will be important and I think the development of nonparametric models will also be important. I think, generally, we are going to have also more emphasis on complete systems rather than single equation methods.

This question is a bit out of the blue. Did you ever think of writing a textbook of econometrics at any stage?

I once thought of writing an elementary, but at its level fairly rigorous, text-book on statistics for econometrics, very much based on the sort of under-graduate statistics course that I had given for many years. I have also thought of writing a book on continuous-time econometrics. I haven't ever thought of writing a 600 or 700 page treatise on econometrics.

A final question. What advice or encouragement would you offer young scholars who are thinking of doing research in econometrics?

I think that I have already mentioned some of them. I certainly think that they should get a very good basis in mathematics and also do some good graduate courses in economic theory. I think they should concentrate on the fundamentals and then branch off into specific areas of research. One other thing I should mention is that I think they should not be afraid of writing their own computer programs. I think that the proliferation of packaged programs that are available these days has a tendency to encourage students to just look around for packaged programs. But I think that if someone is ever going to become an econometric theorist, it's useful to write some computer programs at some stage. I think it is also useful to do some empirical work. A few random thoughts.

THE PUBLICATIONS OF ALBERT REX BERGSTROM

BOOKS

1. *The construction and use of economic models.* London: English Universities Press, 1967; also published as *Selected economic models and their analysis.* New York: American Elsevier, 1967; also translated into Japanese.
2. *Statistical inference in continuous-time economic models* (edited). Amsterdam: North-Holland Publishing Company, 1976.
3. *Stability and inflation: essays in memory of A. W. Phillips* (edited with A. J. L. Catt, M. H. Peston, B. D. J. Silverstone). New York: John Wiley & Sons, 1978.

ARTICLES

1949

1. The guaranteed price for dairy products. *Economic Record* 25 (1949): 91-97.

1951

2. New Zealand's export supply function. *Economic Record* 27 (1951): 21-29.

1952

3. Report of the taxation committee—New Zealand 1951. *Economic Record* 28 (1952): 88-90.

1955

4. An econometric study of supply and demand for New Zealand's exports. *Econometrica* 23 (1955): 258-276.

1956

5. The use of the index number in demand analysis. *Review of Economic Studies* 23 (1956): 17-26.

1957

6. A reply to Mr. Kemp. *Review of Economic Studies* 24 (1957): 215.

1958

7. The New Zealand economy, 1957-1958. *Economic Record,* 24 (1958): 306-316.

1960

8. Linear programming, import controls, and the exchange rate where the supply of exports is inelastic. *Economic Record* 26 (1960): 385-392.

1962

9. The exact sampling distributions of least squares and maximum likelihood estimators of the marginal propensity to consume. *Econometrica* (1962): 480-490.
10. A model of technical progress, the production function and cyclical growth. *Economica* 29 (1962): 357-370.

1965

11. An econometric model of the New Zealand economy (with A. D. Brownlie). *Economic Record* 41 (1965): 125-126.

1966

12. Nonrecursive models as discrete approximations to systems of stochastic differential equations. *Econometrica* 34 (1966): 173-182.
13. Monetary phenomena and economic growth: A synthesis of neoclassical and Keynesian theories. *Economic Studies Quarterly* 17 (1966): 1-8.
14. What is econometrics? *The University of Auckland Gazette* 8 (1966): 1-3.

1967

15. Mathematical restatement of the profit guidance problem (with A. Zauberman). In: A. Zauberman (ed.), *Aspects of Planometrics.* London: Athlone Press, 1967.
16. Monetary and fiscal policy in a growing economy. *New Zealand Economic Papers* 10 (1967): 31-39.
17. Forecasting the New Zealand economy (with A. D. Brownlie). *Economic Record* 43 (1967): 303-305.

1972

18. The covariance matrix of the limited information maximum likelihood estimator. *Econometrica* 40 (1972): 899-900.

1976

19. A model of disequilibrium neoclassical growth and its application to the United Kingdom (with C. R. Wymer). In: A. R. Bergstrom (ed.), *Statistical Inference in Continuous-Time Economic Models.* Amsterdam: North Holland Publishing Company, 1976.

1978

20. Monetary policy in a model of the United Kingdom. In: A. R. Bergstrom et al. (eds.), *Stability and Inflation.* New York: Wiley, 1978.

1983

21. Gaussian estimation of structural parameters in higher-order continuous-time dynamic models. *Econometrica* 51 (1983): 117-152.

1984

22. Continuous-time stochastic models and issues of aggregation over time. In: Z. Griliches & M. D. Intriligator (eds.), *Handbook of Econometrics,* Vol. 2. Amsterdam: North Holland Publishing Company, 1984.
23. Monetary fiscal and exchange rate policy in a continuous-time econometric model of the United Kingdom. In: P. Malgrange & P. Muet (eds.), *Contemporary Macroeconomic Modelling.* London: Blackwell, 1984.

1985

24. The estimation of nonparametric functions in a Hilbert space. *Econometric Theory* 1 (1985): 7-26.
25. The estimation of parameters in nonstationary higher-order continuous-time dynamic models. *Econometric Theory* 1 (1985): 369-386.

1986

26. The estimation of open higher-order continuous-time dynamic models with mixed stock and flow data. *Econometric Theory* 2 (1986): 350-373.

1987

27. Optimal control in wide-sense stationary continuous-time stochastic models. *Journal of Economic Dynamics and Control* 11 (1987): 425-443.