# UNIT ROOTS IN LIFE — A GRADUATE STUDENT STORY

By

**Peter C. B. Phillips**

**COWLES FOUNDATION PAPER NO. 1429**

**COWLES FOUNDATION FOR RESEARCH IN ECONOMICS**
**YALE UNIVERSITY**
**Box 208281**
**New Haven, Connecticut 06520-8281**

**2014**

**http://cowles.econ.yale.edu/**

*Econometric Theory*, **30**, 2014, 719–736.
doi:10.1017/S0266466613000455

# UNIT ROOTS IN LIFE—A GRADUATE STUDENT STORY

Peter C. B. Phillips
*Yale University, University of Auckland, Singapore Management University, and University of Southampton*

On the evening of March 7, 2008, the New Zealand Econometric Study Group Meeting held its Conference Dinner. The venue was the Owen Glenn Building, the spectacular new home of the Auckland Business School and the Department of Economics at the University of Auckland. The meeting was organized by my colleagues, co-authors, and close companions Donggyu Sul and Chirok Han. Chirok did double duty by videotaping the evening, Donggyu coordinated festivities with consummate skill, and we settled in to a memorable evening.

Econometricians, old friends, former students, two of my former teachers, faculty, and senior administrators were gathered together to celebrate my 60th birthday. Many had traveled long distances from overseas and navigated busy schedules to come to this event. It was a singular honor. My wife and daughter were with me. Opening speeches from Bas Sharp and John McDermott broke the ice with endearing tales from the past and jokes about some mysterious hole in my vita. I stood at the front table, looked out, and felt a glow of fellowship envelop me. I was fortunate indeed. Life had bestowed many gifts. The warmth of family, friends, and collegiality were at the top of the list. My education and early training in New Zealand were a clear second.

What follows is a graduate student story. It draws on the first part of the speech I gave that evening at the NZESG conference dinner. It mixes personal reflections with recollections of the extraordinary New Zealanders who shaped my thinking as a graduate student and beginning researcher—people who have had an enduring impact on my work and career as an econometrician. The story traces out these human initial conditions and unit roots that figure in my early life of teaching and research.

Special thanks go to Donggyu Sul and Chirok Han, who put enormous effort into organizing the 18th New Zealand Econometric Study Group Meetings held over March 7–9, 2008. Warm thanks also to Bas Sharp and John McDermott for their opening speeches at the conference dinner, to Les Oxley for his conference closer, to the many friends, colleagues, co-authors, and former students who came and supported this event, and to Viv Hall for the photographs of #2 and #4 Alfred Street. Address correspondence to Peter C. B. Phillips, Cowles Foundation for Research in Economics, 30 Hillhouse Avenue, New Haven, CT 06511, U.S.A; e-mail: peter.phillips@yale.edu

Dinner speech at the NZESG Meetings March 7, 2008.

## 1. ORIGINS

Many years ago, as a graduate student at the University of Auckland, I fell in love with econometrics. It was March 1969, the beginning of the Southern Hemisphere academic year, and I was the only graduate student in economics. There were no lectures. My weekly meetings were held in faculty offices at #4 Alfred Street, which adjoins Auckland's stunningly beautiful and historic Albert Park, an inner-city parkland sited alongside an ancient volcanic cone above the central business district of the city. The main economics building at #4 is still extant but now houses the Students Association and finds itself squeezed on either side by modern multistoried structures of steel, glass, and concrete, a quiet memorial to an increasingly distant university past. A second economics building originally stood beside it at #2 Alfred Street, housing lecturers, visitors, tutors (myself included), and a small classroom. That building has given way to a large new complex for student life and study—the Kate Edger Commons—sited on the corner of Alfred and Symonds street.

My graduate econometrics class was with Rex Bergstrom, one of New Zealand's great pioneers in econometrics. It was my first exposure to advanced econometrics. My undergraduate courses were in mathematics, applied mathematics, statistics, and economics all taken to third year level. These majors were coupled with classics where one of my teachers was the eminent classicist and accomplished lecturer E. M. Blaiklock. Blaiklock spent 45 minutes of each 50 minute lecture inspiring us with stories from classical literature that had nothing to do with the curriculum but gripped young minds with the energies of classical Greece and Rome, the excitement of modern excavations of Troy, and the elegance of latin prose composition. Like a lightning strike in the final 5 minutes of the lecture Blaiklock would then produce an astonishing extempore translation of 80 lines of the Aeneid, which we urgently scratched down for later study. Blaiklock was Chair of Classics, University Orator, Classicus columnist in the national New Zealand Herald newspaper, and the epitome of the erudite scholar—a passionate communicator with an immense command of the English language that humbled a student audience into adoring silence.

In my first year at Auckland in 1966, I had started off doing accounting and law as well, but I left these subjects behind in the second year and never returned. In mathematics I was fortunate to enter directly into the second year level based on national entrance scholarship results. My mathematics cohort was enormously stimulating and there was great camaraderie. The class was sprinkled with extraordinary talent and bristled with friendly competition spurred on by the seasonal cycle of examinations. In those days, all test results were posted on departmental noticeboards and final exam results appeared in the university cloisters and were published in the newspapers, giving full public exposure of individual performance. It was impossible to hide. A different world from today.

My third year mathematics classes were at graduate level. We had courses in measure theory and integration, numerical analysis, differential equations, Bessel functions, and statistics. But no probability. There was no advanced probability course at Auckland in the 1960s. We were blessed with some outstanding young teachers. John Butcher,[1] who became a world authority on Runge Kutta methods, taught us topology. A bright Chinese lecturer named Chang gave an advanced course on complex analysis. George Seber covered regression and the linear model based on his brilliant monograph *The Linear Hypothesis,* a book that was soon to become a classic. Seber had just arrived back in New Zealand from the UK, fresh from his Ph.D at Manchester under David Silvey,[2] and an assistant lectureship at the LSE. He was the only full-time statistician in the university and taught all of the statistics courses in the university with the exception of the main undergraduate statistics/econometrics course in the Faculty of Commerce, a course that had been taught by lecturers in economics since it was established soon after the turn of the century in 1906.[3] Seber was unique—he came into lectures without a scrap of paper and gave perfect 50 minute lectures written out in full on the blackboard without notes. It was an inspiring sight to watch his effortless, unassisted derivations on the board of the densities of the noncentral chi-squared and F distributions. Peter Lorimer taught us algebra and group theory, working through an entire year's lectures numbering his theorems sequentially as he went along until the students exploded in uproar at theorem 100. The glorious freedom of the 1960s!

Back at #4 Alfred Street, I began my graduate econometrics classes with Rex Bergstrom. We met in Rex's professorial office—spacious and scrupulously well organized with no clutter and no papers on any surfaces. The bookshelves around the office had an unusual look—Rex had removed all the covers of his books giving a clean uniformity, a scientific precision, and formality to the shelves. A single book stood in a cradle on his desk. It was the newly translated edition of Malinvaud's *Statistical Methods of Econometrics.* Rex told me that we would start by reading Malinvaud line by line and cover to cover. Like Cramér's similarly named classic *Mathematical Methods of Statistics,* Malinvaud's treatise completely outshone its contemporaries (and all of its successors for decades) in terms of its innovation, comprehensiveness, rigor, and strong links to economics. This book, Rex's grasp of the subject, and his passion for excellence gripped my imagination and I soon found myself in a new intellectual world. What I noticed about econometrics first was its freshness, vitality, mathematical precision, and its connection with empirical research and economic modeling. There was no other subject at university remotely like it.

Econometrics is the tool that forces economic ideas to face the reality of observation. The subject is distinguished by the unifying power of economic theory, mathematical technique, and statistical method in the empirical search for economic laws. This mantra inspired Ragnar Frisch, Irving Fisher, Jan Tinbergen, Tjalling Koopmans, and the first generation of econometricians. It makes its presence felt in the early chapters of Malinvaud's book and persists to its closing pages. Reading Malinvaud and studying with Rex reinforced for me this powerful perspective on econometrics. It was a truly fortunate beginning, built on the guidance of a great teacher and an inspiring monograph that pointed many ways forward in a vibrant young subject.

#4 Alfred Street, 1969: The Department of Economics, University of Auckland.

Our meetings started off with Rex asking me if I had any questions on the previously assigned pages of Malinvaud's book. Week by week we went through the book. Week by week we collected a card file of errors, typographical slips, and what we thought were better proofs and shorter derivations. Very soon we found ourselves in an implicit, friendly competition with teacher and student each trying to outdo the other by finding slips in the text and new derivations. In less than twenty weeks we had read the entire book equation by equation, including its standout chapters 5 and 9 on linear and nonlinear estimation.

Malinvaud's treatment of linear estimation is masterful in its elegance and generality, accommodating restrictions implied by deficient rank systems and introducing the reader to the linear space geometry of the Gauss Markov theorem via concentration ellipsoids and conjugate subspaces. Equally inspiring and novel is its rigorous derivation of the consistency and asymptotic distribution theory of nonlinear estimators, some years prior to Jennrich (1969) and Malinvaud's (1970) own paper on the subject in the *Annals of Mathematical Statistics*. One thing was clear. In the matter of a single course, Rex had brought me right to the research frontier in all these major areas as well as the subject that was the central edifice of econometrics in those days—the simultaneous equations model.

The course did not end there. Rex felt that the weakest part of Malinvaud's book were its chapters on time series. So, he recommended that I read Grenander and Rosenblatt's (1957) treatise *The Statistical Analysis of Stationary Time Series* - another classic work that is now seldom read or referenced. In the final weeks of the course, we went through the last chapter of Rex's own (1967) monograph *The Construction and Use of Economic Models,* which gave me my first introduction to continuous time models and Brownian motion. Remarkable for its wide coverage, its stylistic economy, and its mathematical precision, Rex's book rewarded repeat readings and it joined Malinvaud's text as among my long time favorites. It is an astonishing testimonial to Rex's work that, to my knowledge, no one has ever found a typographical error or slip in his book—a massive accomplishment, especially in an era before electronic typesetting.

The earlier chapters of Rex's book I read line by line in another graduate course—on economic growth theory—with a newly appointed lecturer in the economics department, Alastair MacCormick. Alastair joined a senior faculty member Harro Bernadelli in running this course. Harro was nearing retirement. He was a student of the famous Austrian economist Joseph Schumpeter and had catholic interests right across the discipline covering economic theory, business cycles, economic growth, econometrics, Marxist economics, and the history of economic thought. I picked up a smattering of everything in our weekly reading and discussions. Harro was enormously entertaining. He claimed he was one of the few people who had actually read Marx's Ph.D thesis. He told a fascinating story, whose validity I have not seen confirmed, of how the thesis was turned down by the University of Berlin and ultimately accepted by the University of Jena after Marx's father sent in a generous check to accompany it. Harro recommended I read one of his favorite books that had virtually nothing to do with the course—Turnbull and Aitken's (1932) luminary treatise on canonical matrices. We had used Aitken's (1939) famous monograph *Determinants and Matrices* in one of my early mathematics courses. These two books were my introduction to the work of New Zealand's most renowned mathematician, Alexander Aitken, who did his doctorate in Edinburgh under the great English mathematician Edmund Whittaker. A fact that should be but is not generally known in econometrics is that Aitken (1934) devised the matrix notation of the general linear model, and the projection matrix formulae for least squares and generalized least squares. Every econometrician is in his debt for that. He also worked with Whittaker in the 1920s on a technique for graduating data, a special case of which is now called the Hodrick–Prescott filter in economics (Aitken, 1925, 1926). Another of Aitken's lasting contributions that has an important bearing on econometrics is his early development of estimation efficiency that involves what is now universally known as the Cramér–Rao bound (Aitken and Silverstone, 1942).[4]

In contrast to Harro's discursive style and lengthy diversions, Alastair kept to the same formula as Rex with a tight reading program. We read Debreu's *Theory of Value* and Peter Whittle's *Prediction and Regulation,* both brilliantly written monographs that quickly attained classic status and have endured for decades. We also read some stochastic control theory, using Solodovnikov's (1965) book, which was the topic of Alastair's master's thesis that was supervised by Rex. Alastair became a good friend and went on to do his Ph.D in operations research at Yale. He returned to Auckland in the 1970s and moved on to become the founding Dean of the Auckland Business School and Pro Vice Chancellor of the University.

My other graduate course in economics was in macroeconomics and planning with Colin Simkin, who chaired the department. Colin was a deep thinker, one of Australasia's leading economists, and like Blaiklock an extraordinarily erudite man. He went to university to study literature, missed a morning final exam by turning up in the afternoon, and ended up in economics. Our classes involved wide-ranging discussions of economics. Colin was a senior member of Auckland's professoriate and had been chair of economics since 1946. He held a deep conviction about the importance of quantitative training and evidence-based research in economics. His initiatives secured for the departmental library a complete set of volumes of *Econometrica* as well as all of the major statistical journals, including the *Annals of Mathematical Statistics*, *Biometrika*, *Journal of the American Statistical Association*, and all series of the *Journal of the Royal Statistical Society*. Prior to George Seber's arrival in the mathematics department in 1965, economics was the central engine of teaching and research in statistical methods at Auckland. Colin's respect for the use of quantitative methods in economics had certainly fostered this outcome as had his early appointments of faculty with evident strengths in econometrics such as Malcolm Fisher (in 1948) and Rex Bergstrom (in 1950).[5]

I was intrigued to learn that Colin was a close and lifelong friend of Karl Popper from their days together at the University of Canterbury in Christchurch during the early 1940s. Colin was the sole lecturer in economics at Canterbury at the time and Popper was a lecturer in philosophy having arrived as a refugee from Austria, via England, in 1937. Popper approached Colin and asked him for assistance in his English and in understanding social science and economics. A strong and lasting friendship was born. Colin had enormous respect for Popper and felt that many of his ideas were improperly understood, in part because most people learnt about him second or third hand and not by reading him. To assist in rectifying some of these misunderstandings, in his retirement Colin wrote a masterful summary (1993) of Popper's philosophy. The introduction to the volume contains a brilliant synthesis of Popper's ideas into twelve central theses.

One morning during our sessions together Colin said you should read this. He passed over a long letter from the LSE in Popper's beautiful handwritten script describing the student riots there and reminiscing about the birth of the *Open Society and its Enemies*. Under Colin's direction I then read Popper (1945, 1959, 1961) we discussed the *Open Society* (which was written and published while Popper was still at Canterbury) and the *Poverty of Historicism,* which was also begun in New Zealand. I learnt some philosophy of science, radical empiricism, and the notion of empirical falsification—amidst a sea of national economic planning. I also learnt to take greater care over the written and spoken word. Colin's literary background lingered close to the surface. He had well thumbed copies of Fowler's Modern English Usage and the Concise Oxford Dictionary sitting prominently on his desk. He put these to good use, letting no opportunity slip by to comment on my written work.[6]

In the final couple of weeks in Rex's course, I studied two of his own papers. First up was his famous *Econometrica* 1962 paper on the finite sample distribution of the marginal propensity to consume. This paper pioneered exact distribution theory in econometrics, a new field that gripped me in a vice of fascination. I can still feel the adventure that ran through my veins when I read it. Second up was his *Econometrica* 1966 paper on nonrecursive approximations to continuous systems. That paper led us to discussions of the debate surrounding the Wold (1954) and Strotz and Wold (1960) papers on causality and recursive modeling in economics, work that has recently been revitalized by Pearl (2000, 2013). Rex told me he was working on a continuous time model of the UK economy which would use the methodological approach of his 1966 paper. This topic and the econometric theory that enveloped it were to consume Rex's intellectual energies for the next three decades.

Then, as quickly as the course began, it was over and I was ready to face the exam. Four hard technical and numerical questions in three hours. One on the efficient estimation of a multivariate linear model, another on nonlinear estimation asymptotics, the third on simultaneous equations, and the fourth on continuous systems. Exhilarating and inspirational. The foundation had been laid for a new trajectory. I sensed it but didn't grasp its import. A career in econometrics. The first unit root.

## 2. RESEARCH BECKONS

It was time to move on. Two weeks after the final exam, Rex called me in for a conversation in his office and told me I was being appointed to a junior lectureship and that next year I would be teaching the major undergraduate statistics/econometrics course—the course mentioned earlier which now had around 350 students enrolled. My salary, he informed me, would be $2,000 New Zealand dollars. He didn't ask me if I was interested or would accept. He simply told me I was being appointed and these were the terms. What an opportunity. Four years training at university, 21 years old, and I was to become a lecturer. Simply amazing. More so because I was an utter novice and knew nothing of the realities of the assignment at the time—facing the biomass of 350 students in a lecture hall, teaching a compulsory course that many feared, some hated, and others had failed several times, not to mention running a final exam that would take me three solid weeks to mark. A whirlwind of thoughts and emotion swirled around me.

I came back to earth with a jolt when I realized that Rex had just asked me what topic I had in mind for my masters thesis. This 10 minute conversation was no walk in the park, it was serious stuff. A lectureship. Now a thesis! That was somewhere in the stratosphere. I was just recovering from my final exams and had only vaguely begun to think about it. A couple of ideas that had occurred to me during the course tumbled out in response.

The first was to develop Malinvaud's geometric linear space estimation theory into general conditions for the optimality of least squares. At this point, I was totally unaware of Kruskal's (1968) major paper in the *Annals of Statistics* on this topic - the geometry of the equivalence of GLS and OLS. In the 1960s, overseas journals arrived in New Zealand by sea-mail and often with a 6–12 month delay. In the following year, the Drygas (1970) monograph would appear, which contained some related work on the coordinate free approach to linear estimation. So the topic was in the air but I didn't know it. Of course, much earlier work had established the algebraic conditions for GLS/OLS equivalence (Anderson, 1948). It was the simple elegance of the geometry that fascinated me. When the conjugate subspace of the linear manifold containing the mean vector is spanned by a corresponding number of principal axes of the concentration ellipsoid, a linear projection along the conjugate subspace (GLS) trivially corresponds to OLS. Neat and powerful I thought.

The second idea I had in mind was to derive the exact distribution of the least squares estimator of the coefficient in a first order autoregression. I knew from Malinvaud's discussion, my reading of Cowles Commission Monograph 10 (Koopmans, 1950) and Grenander and Rosenblatt (1957) that this had not been found and seemed an important challenge. I had read some of the literature. Hurwitz (1950) had made progress on moments in special cases and Koopmans (1942) had produced some interesting approximate results. Even the great von Neumann (1942) had contributed. The problem had absorbed me some months earlier during the course.

Rex’ response to these ideas was devastating. The conversation echoes in my head like it was yesterday. The first idea, he said, would probably merit a footnote in Malinvaud’s next edition. As for the second idea, well ... the best mathematicians in the world had worked on it without success for 10 years. So what chance did I think I had to solve it? Back to square one with a thump!

After I got over the shock and thought about it later, I realized that Rex was right on the mark. Later still, I came to realize that his advice was spectacularly sound. In contrast to my ideas for a topic, a constructive thesis—one that builds a new technology of estimation and inference for instance—is much more likely to be important and influential in the long run than one that solves a mathematical problem, no matter how cute and appealing that problem may be. Mathematical problems in econometrics generally fall into the trivial category in comparison with the magnitude and importance of major mathematical puzzles.

I confess I haven’t always followed Rex’s advice on this research strategy over the last 40 years. Evolutionary instinct often drives us in divergent paths from our parents and mentors. Sometimes, too, we simply cannot resist the temptation of a fascinating problem. We sense a gap coming in the clouds and long to reveal what’s behind them. The possibility of new knowledge and discovery is often irresistible. But the passion for discovery needs control and direction to become

#2 Alfred Street. The second building of the Department of Economics. The second floor front offices were occupied by Alastair MacCormick and Viv Hall and a rear office by the author in 1969–1970.

productive over the long term. Constructive strategies that build new technologies of inference from small steps forward tend ultimately to pay off. Looking back now I know that's right. I also know that when I have followed Rex's advice I have never regretted it.

The conversation didn't end there. Having summarily dismissed my ideas, Rex pronounced this simple directive. Try estimating a continuous time system using the exact discrete model and do a simulation study with a simple trade cycle model. That was it. A single sentence from the master and I walked out the door with a thesis project under my arm. What a gift. A second unit root—a home run without lifting the bat.

## 3. MASTER'S THESIS

I was ready to go. It was the end of November 1969. I was upgraded to a new office on the second floor of #2 Alfred Street and the wheels started turning. The first step was to decide on the simulation model and write computer programs to generate data, while working on the estimation methodology and asymptotic theory. The choice of a simulation model was simple enough—a three equation stochastic differential equation trade cycle system for aggregate consumption, investment, and national income. Rex had one in his book. Problem solved. On to computing.

The University had just installed a new IBM 1130 mainframe. I had run batch Fortran jobs on it in a numerical analysis course in the applied mathematics sequence. A new lecturer in the mathematics department, Garry Tee, taught part of that course and had amused us with stories of imaginary programming languages and scripting, which foreshadowed the future of modern computing. Garry is now a legend in the computer science department at Auckland.

The IBM 1130 took up an entire room in the new Chemistry block and it seemed to be the only part of the university where there was 24 hour security. With its flashing light console unit, the 1130 looked like a prop in a science fiction movie. The reality was that it had 8K core store memory and fell seriously short of space-age computing. Long programs that would not fit in memory had to be run in sequence storing data on disk and retrieving it as the next segment of code was linked and pulled into memory. My continuous time system produced a three equation nonlinear vector autoregression whose coefficient involved an exponential series in a matrix argument that was itself subject to algebraic restrictions. Extremum estimation of this nonlinear regression required numerical optimization. No canned optimization packages were available and the code had to be written from scratch. It was a couple of week's programming and debugging in those days. After a few trial runs I worked out that it might take an hour or more to run one regression on the 1130. So a full simulation was going to be a long haul. Batch jobs were limited and the only option was to get an operator's licence for the mainframe and run jobs overnight and on weekends.

Getting an operator's ticket for the 1130 was like sitting the UK driver test—most people failed it! The test took about 15–20 minutes. You had to cold start the machine and all the peripherals and run a batch job stacked with problem-inducing cards and solve all the issues within the allocated time frame. When a stoppage shut down the machine or the line printer exploded in a printing frenzy you had to read the error codes in hexadecimal on the flashing console lights, troubleshoot the problem, resolve it, and restart the deck. Sitting the operator's test was an ordeal of tribal initiation that the machine technicians had dreamed up to see if you understood the system and could deal with shutdowns and peripheral malfunctions. I remember one problem well. The examiner had planted a dummy in the hopper—two cards glued together so that it wouldn't even enter the hopper. Nasty. Not even a card jamb to diagnose—a full computer freeze up.

With an operator's licence in hand, I was able to book time on weekends and overnight. Overnight shifts were the longest—12 hours at a clip—and most productive. You'd turn up at 7:00 pm with cards neatly stacked in a box, sandwiches and a flask of tea for the early hours, and leave at 7:00 am. There was stiff competition for these long shifts and you might get one session a week or two if you were lucky. The crystallographers were the big boys. They ruled the machine like emperors. We were small fry from economics. With a few simulation runs completed in an overnight shift, I would store the results on hard disk and keep going until enough replications had accumulated to do some analysis. It took me 6 months to complete the simulations. The entire job and the analysis could be done in less than a minute on a laptop these days.

Some of the computations for my thesis were done on mechanical Facit calculating machines that the department of economics owned. Turning the handle on these machines to multiply numbers felt like something out of the early industrial revolution. A spinning jenny number cruncher. The department also had one new electronic calculator for which there was high demand and for which we queued for access. When the 1130 was down and when supplementary calculations were needed, these machines were indispensable. I remember spending an afternoon inverting several complex matrices on a hand Facit to get ready for an evening shift on the 1130. Turning a handle to multiply and divide numbers. It was good training for research in the trenches.

Two friends from economics (a Ph.D student Viv Hall and a young lecturer Hessel Baas) were running their jobs alongside mine on the mainframe. We were writing a lot of regression software and taking up serious computer time in empirical work and simulations.[7] So the economics department surprisingly became the biggest user of the 1130 for a few months in 1970. We even overtook the crystallographers for a while. It was enough time in the computer room to last for a decade—or so I thought.

The computing work was underway and the theory was taking shape. The exact discrete model corresponding to my continuous time system was a vector autoregression (VAR) with an intercept. Prima facie this was a nonlinear regression problem with predetermined variables. There were several complications in developing a rigorous theory. To start, various nonlinearities appeared in the continuous time coefficient matrix and the intercept coupled with cross equation restrictions. These nonlinearities were compounded by the matrix exponential that figures prominently in the exact discrete model. An awkward problem, because the inverse of a matrix exponential, the matrix logarithm, is a multi-valued function like the logarithm of a complex variable. In econometrics I saw that this was an identification problem. In time series and engineering the phenomenon is called temporal aliasing. Aliasing has the well-known manifestation of the stagecoach wheels in John Wayne movies appearing to go backwards for a while as the discrete frames of 35 mm film produce the illusion of the wheels reversing even when the coach is moving forward.[8] So, without further information, an underlying continuous time system is generally unidentified from discrete data.

Fortunately in my case there was further information. The continuous time coefficient matrix and intercept were restricted. Some of their elements were zero and the rest depended on just a few structural coefficients—rates of adjustment, propensities and elasticities. What we might now call deep structural parameters. Some algebra of the functional transformations showed that the true continuous time coefficient matrix was identified in the discrete time reduced form using just one of these restrictions. It demonstrated the power of prior information. The aliasing problem was solved by mobilizing economic theory restrictions. A new discovery and a potential research paper. Exciting stuff! Another unit root.

Now that the continuous system parameters were identified in the discrete time VAR, the asymptotic theory looked straightforward. Allowing for predetermined variables, the limit theory could be derived using nonlinear regression theory, giving consistency and asymptotic normality to estimates obtained by maximum likelihood or minimum distance procedures. I assumed stationarity and ergodicity. It was 1969. No one in econometrics was talking about nonstationarity, unit roots or stochastic trends. Box and Jenkins (1970) had not appeared and its impact came later in the 1970s and early 1980s. I was familiar with Whittle (1964) and Yaglom (1962) and had seen some discussions of accumulative (partial sum) processes. I also knew White's (1958) pathbreaking paper and Anderson (1959) was also relevant. I was not familiar with Billingsley's (1968) classic work—that would wait until 1973 when I learnt about this remarkable book from Jim Durbin and my Essex colleague, Ken Burdett. My own path to unit roots began later in 1975 as I worked on Edgeworth expansions of the distribution of the serial correlation coefficient for my 1977 *Econometrica* paper. That's a story for another day. In 1969, I followed tradition and kept to stationary and ergodic VARs.

Estimation of the structural parameters in the continuous system was accomplished by two methods: using the minimum distance estimator (MDE) of the exact discrete model by nonlinear regression; and applying three stage least squares (3SLS) estimation to the nonrecursive discrete approximation to the continuous system. The latter approximation, used by Rex in his 1966 paper, is closely related to the traditional Euler approximation that is now popular in the financial econometrics literature.[9] I showed that the MDE is consistent, asymptotically normal, and, under Gaussianity, asymptotically efficient. The 3SLS estimator was inconsistent and asymptotically normal about its pseudo true value.

The simulation results turned out to reveal some fascinating differences between the MDE and 3SLS procedures. The MDE approach produced results that were very close to an oracle estimator (based on generalized least squares estimation of the pseudo linear model with a known error covariance matrix) for a sample size as small as 25 discrete observations. So, even in small samples using the exact discrete model gave little bias and good efficiency in estimation. The 3SLS estimates were biased and turned out to be particularly poor for one of the speed-of-adjustment parameters. In interval estimation, the coverage probability of confidence intervals for the structural parameters constructed from the MDE was close to the nominal 95% level, whereas the corresponding intervals for the 3SLS procedure showed substantial distortion—in one case with coverage probability below 40%. Overall, the simulation results were immensely encouraging for direct econometric estimation of the exact discrete model.

With the research on the thesis completed, I turned to writing up. The work went smoothly and was mainly done in Rex's absence. In 1970, Rex took up an appointment as Keynes Visiting Professor at the new University of Essex in the UK. Before he left New Zealand he encouraged me to write up the work as a paper and submit it to *Econometrica*. By mid 1970, I had first drafts of the thesis and the paper finished. After several months of polishing and revision they were ready to submit. The thesis was bound and submitted. The paper went off to *Econometrica* in November 1970.

## 4. AFTERMATH

I now had 350 scripts to mark. So there was no sitting around waiting to hear from *Econometrica*. Just an earthy welcome to the responsibilities of academic life.

In fact, the editorial response came earlier than expected—in March 1971. If only journal turnaround time was as good these days! Barely 3 months—without email or online journal management software to assist. The white envelope sitting in my mail box held an imposing bold *Econometrica* insignia on the outside. It had traveled 9,000 miles and had the bearing of an official document from the Palace of Westminster. The envelope bulged with a formal decision letter and reports typed on heavy linen paper.[10] I went back to my office, sat at my desk, and did a moment's meditation before slitting the seal. The Editor was Frank Fisher. It was a good letter. The referees liked the paper, found no errors, and made some recommendations. Fisher invited a revision. The prospects started to shiver through my system.

The revisions suggested were minor and easy to attend to. I had it ready in three weeks and resubmitted. One comment in the reports made a powerful lasting impression. It related to my referencing Durbin's (1960) paper on unbiased estimating equations, which provided a new way of thinking about

The author en route to the University of Auckland in 1970.

centering and efficiency issues that extended to models with lagged variables. I had felt it was relevant to the theory in my paper because it was a nonlinear VAR, the MDE was asymptotically optimal under Gaussianity and I had used an oracle estimator for comparisons in my simulations. The referee bluntly stated that Durbin had made many important contributions to econometrics but this was not one of them and I should remove the reference. The words were written with the authority of a senior person who knew what he was talking about. They struck hard as was the intent and riveted into memory. I was perplexed. Malinvaud had cited Durbin and seemed to view that work favorably. Obviously, senior people had very different views. This was science. Matters were not always cast in stone. Opinions differed. I had read Durbin's paper. I sat down and read it again and confirmed my view that it presented a new perspective for thinking about estimation and efficiency that included autoregressions. I agreed with Malinvaud. But let it go. The reference disappeared in the revision.

Almost 40 years later, I gave the Durbin lecture at University College London in 2009. Jim Durbin and his wife attended. I opened with a laudation of Jim. After these tributes, I mentioned the episode with the referee and described how perspectives in the profession can change so radically over time. Unbiased estimating equations were now one of the backbones of modern econometrics. They formed the foundation of methods like GMM, which lever off moment conditions and have transformed empirical research over the last three decades. Textbook writers venerate the approach. Manski (1988) wrote a book motivated by the idea. Davidson and MacKinnon (1993) used it as the central thematic of their textbook applauding the approach in their preface and referencing Godambe's (1960) paper, which put forward an idea similar to Durbin's but without the time series setting.[11] Interestingly, Durbin (1960) indicated that some of the arguments he presented on the properties of unbiased estimating equations for autoregressions were not restricted to stationarity. He also indicated extensions, following a suggestion by Barnard, to nonlinear estimating equations that relate closely to much later GMM ideas in econometrics. The passage of time and a massive body of subsequent research have proved the referee's objection in 1970 to be groundless. Sadly, Durbin's paper is seldom cited. It is all part of the give and take of peer review. But it sends out a warning signal to be careful in dismissing new ideas too quickly. As Einstein put it: *If at first the idea is not absurd, then there is no hope for it.*

The revision submitted, I went on with my teaching and reading. I had received notice that I had been awarded a Commonwealth Scholarship to go to the LSE to do a Ph.D under Denis Sargan. New Zealand is a long way from the major centers of learning in North America and Europe. The distance felt far greater in 1970 before the age of long haul jumbos and the Internet. Rex had done his Ph.D at Cambridge under Richard Stone in 1954. He suggested I come up with a list of places to do further study and people to work under. I gave him a list and he put a line through all of the places I suggested except the LSE. The world's leading econometrician was Denis Sargan, he declared, and the LSE was the strongest center of econometrics. Unequivocal. So the application had gone in and I was now to move back to the UK, my childhood home. With Bergstrom and Simkin gone already and friends Alastair MacCormick and Viv Hall about to leave in a few months time, there was no academic reason to stay. Auckland was part of my soul. It would never leave me. I trusted I would be back.

The second response from *Econometrica* came quickly in April 1971. The paper was accepted. It appeared in the November 1972 issue. The acceptance was the final unit root of my apprenticeship at Auckland. It set a course for the future. The train had left the station.

I was soon to arrive at the LSE, meet the remarkable Denis Sargan, and move on to join Rex Bergstrom and a growing constellation of young stars at the University of Essex as I finished my Ph.D. A long journey and many new stations lay ahead. The territory was unknown. It would be occupied by projects and papers, teaching and students, journals and reviewing, editing and organizing, supervising and caring, computing and programming, thinking, worrying, writing, reading, and learning. The journey was a gift. It would bring new people and family into my life. Before long, as the habits of a lifetime took hold, I would come to recognize the wellspring of deep and enduring satisfaction—the satisfaction that comes from simple creative work, intellectual or physical, and sustains us in our varied pursuits in life. The Welsh poet Dylan Thomas had never heard of econometrics. Yet he characterized the rewards with poetic beauty as the common wages of their most secret heart.

*In my craft or sullen art*
*I labour by singing light*
*Not for ambition or bread*
*Or the strut and trade of charms*
*On the ivory stages*
*But for the common wages*
*Of their most secret heart.*
(Dylan Thomas, *Deaths and Entrances*, 1946)

## NOTES

1. See Butcher (2003).

2. In a classic paper, Silvey (1959) coined the term Lagrange multiplier test. The LM test was first used in econometrics by Ray Byron and caught on quickly during the 1970s, having a big impact on practical inferential methods. Mrs A. Silvey translated the first edition of Malinvaud's (1966) treatise *The Statistical Methods of Econometrics,* the book that was to become my constant companion throughout 1969.

3. This course, originally called Statistical Methods, was taught over 1906–1990 and may have been the longest continuously taught course at the University of Auckland (Court, 1995). My contribution to the sequence was two years teaching over 1970–1971.

4. A discussion of Aitken's contributions to econometrics and some tales of his eminence as a mental arithmetician are given in Phillips (2010).

5. Court (1995) provides a detailed history of econometrics at the University of Auckland to 1990. See also the biography of Bergstrom (Phillips, 2007) and his obituary (Phillips, 2005).

6. As in the curious alternative usages of the past tense and past participle "learnt" and "learned".

7. Phillips and Hall (2004) discuss some of these econometric software developments in the general context of the history of computing at the University of Auckland in the 1960s.

8. The phenomenon is not restricted to motion pictures. Under continuous illumination (such as from the Sun) human visual perception is also subject to temporal aliasing. Apparently, the physiology is not yet (as of 2012) fully explained. It is thought that human visual perception may work from a series of still frames like those in a movie camera or through a more complex filtering mechanism that produces aliasing effects.

9. The relationship between these approximations has been studied more closely in some recent research (Phillips and Yu, 2009; Wang, Phillips, and Yu, 2011).

10. An old trick that I learnt later was to check the watermark in the bond paper for a university seal that might point to the identity of a referee.

11. The first edition of this textbook mentioned Godambe (1960) but not Durbin (1960). Durbin was referenced in later editions after I brought his work to the authors' attention. Godambe developed the estimating equation approach as a justification for maximum likelihood estimation in a single parameter regular case, using arguments similar to Durbin in terms of a Cramér–Rao bound theory for estimating functions. But there was no discussion of time series examples in Godambe (1960), so it was not relevant in my case.

## REFERENCES

Aitken, A.C. (1925) *The Smoothing of Data*. D.Sc dissertation, University of Edinburgh.

Aitken, A.C. (1926) On the theory of graduation. *Proceedings of the Royal Society of Edinburgh* 47, 36–45.

Aitken, A.C. (1934) On least squares and linear combinations of observations. *Proceedings of the Royal Society of Edinburgh* 55, 42–48.

Aitken, A.C. (1939) *Determinants and Matrices.* Oliver and Boyd.

Aitken, A.C. & H. Silverstone (1942) On the estimation of statistical parameters. *Proceedings of the Royal Society of Edinburgh* 61, 186–194.

Anderson, T.W. (1948) On the theory of testing for serial correlation. *Skandinavisk Aktuarietidskrift* 31, 88–116.

Anderson, T.W. (1959) On asymptotic distributions of estimates of parameters of stochastic difference equations. *Annals of Mathematical Statistics* 30, 676–687.

Bergstrom, A.R. (1962) The exact sampling distributions of least squares and maximum likelihood estimators of the marginal propensity to consume. *Econometrica* 30, 480–490.

Bergstrom, A.R. (1966) Nonrecursive models as discrete approximations to systems of stochastic differential equations. *Econometrica* 34, 173–182.

Bergstrom, A.R. (1967). *The Construction and Use of Economic Models*. English Universities Press.

Billingsley, P. (1968). Convergence of Probability Measures. New York: J. Wiley.

Box, G. E. P. & G. M. Jenkins (1970) *Time series Analysis: Forecasting and Control*. Holden Day.

Butcher, J.C. (2003) *Numerical Methods for Ordinary Differential Equations.* Wiley.

Court, R. (1995) *The Development of Econometrics and Statistics at the University of Auckland*. Unpublished paper, University of Auckland.

Davidson, R. and J. MacKinnon (1993) *Estimation and Inference in Econometrics*. Oxford University Press.

Drygas, H. (1970) *The Coordinate-Free Approach to Gauss Markov Estimation.* Springer.

Durbin, J. (1960) Estimation of parameters in time-series regression models. *Journal of the Royal Statistical Society Series B* 22, 139–153.

Godambe, V.P. (1960) An optimum property of regular maximum likelihood equation. *Annals of Mathematical Statistics* 31, 1208–1211.

Grenander, U. & M. Rosenblatt (1957) *Statistical Analysis of Stationary Time Series.* Wiley.

Hurvicz, L. (1950) Least squares bias in time series. In T. Koopmans (ed.), *Statistical Inference in Dynamic Economic Models*, pp. 365–383. Wiley.

Jennrich, R.I. (1969) Asymptotic properties of non-linear least squares estimation. *Annals of Mathematical Statistics* 40, 633–643.

Koopmans, T. (1942) Serial correlation and quadratic forms in normal variables. *Annals of Mathematical Statistics* 13, 14–33.

Koopmans, T. (1950) *Statistical Inference in Dynamic Economic Models*, Cowles Commission Monograph No. 10. Wiley.

Kruskal, W. (1968) When are Gauss-Markov and least squares estimators identical? A coordinate free approach. *Annals of Mathematical Statistics* 39, 70–75.

Malinvaud, E. (1966) *The Statistical Methods of Econometrics*, 1st ed. North Holland.

Malinvaud, E. (1970) The consistency of nonlinear regressions. *Annals of Mathematical Statistics* 41, 956–969.

Manski, C. (1988) *Analog Estimation Methods in Econometrics*. Chapman & Hall.

Pearl, J. (2000) *Causality: Models, Reasoning, and Inference*. Cambridge University Press.

Pearl, J. (2014) Trygve Haavelmo and the emergence of causal calculus. *Econometric Theory*, forthcoming.

Phillips, P.C.B. (1977) Approximations to some finite sample distributions associated with a first order stochastic difference equation. *Econometrica* 45(2), 463–485.

Phillips, P.C.B. (2005) Albert Rex Bergstrom 1925–2005. *New Zealand Economic Papers* 39, 129–152.

Phillips, P.C.B. (2007) Albert Rex Bergstrom: 1925–2005. In J. King (ed.) *Biographical Dictionary of Australian and New Zealand Economists*, Edward Elgar.

Phillips, P.C.B. (2010) Two New Zealand pioneer econometricians. *New Zealand Economic Papers* 44, 1–26.

Phillips, P.C.B. & V.B. Hall (2004) Early development of econometric software at the University of Auckland. *Journal of Economic and Social Measurement* 29, 127–133.

Phillips, P.C.B. & J. Yu (2009). Maximum likelihood and Gaussian estimation of continuous time models in finance. In T.G. Andersen, R.A. Davis, J.P. Kreiss, & T. Mikosch (eds.), *Handbook of Financial Time Series*, pp. 497–530. Springer.

Popper, K. (1945) *The Open Society and Its Enemies Vol 1 & 2,* 5th ed. Princeton University Press.

Popper, K. (1959) *The Logic of Scientific Discovery.* Routledge (2002 Edition).

Popper, K. (1961) *The Poverty of Historicism.* Routledge (2002 Edition).

Seber, G.A.F. (1966) *The Linear Hypothesis: A General Theory*. Oxford University Press.

Silvey, S.D. (1959) The Lagrangian multiplier test. *Annals of Mathematical Statistics* 30, 389–407.

Simkin, C.G.F. (1993). *Popper's Views on Natural and Social Science.* Leiden.

Solodovnikov, V.V. (1965). *Statistical Dynamics of Linear Automatic Control*. Van Nostrand.

Strotz, R.H. & H.O.A. Wold (1960) Recursive vs. nonrecursive systems: An attempt at synthesis. *Econometrica* 28, 417–427.

Turnbull, H.W. & A.C. Aitken (1932). *An Introduction to the Theory of Canonical Matrices.* Blackie.

von Neumann, J. (1942). Distribution of the ration of the mean square successive differences to the variance. *Annals of Mathematical Statistics* 13, 367–395.

Wang, X., P.C.B. Phillips, & J. Yu (2011) Bias in estimating multivariate and univariate diffusions. *Journal of Econometrics* 161, 228–245.

White, J.S. (1958) The limiting distribution of the serial correlation coefficient in the explosive case. *Annals of Mathematical Statistics* 29, 1188–1197.

Whittle, P. (1963) *Prediction and Regulation*. English Universities Press.

Wold, H. (1954). Causality and econometrics. *Econometrica* 22, 162–177.

Yaglom, A.M. (1962) *An Introduction to the Theory of Stationary Random Functions*. Dover.