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Essays in Econometrics

Collected Papers of Clive W. J. Granger
Volume I: Spectral Analysis, Seasonality, Nonlinearity, Methodology, and Forecasting

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Essays in Econometrics

This book, and its companion volume in the Econometric Society Monographs series (ESM No. 33), present a collection of papers by Clive W. J. Granger. His contributions to economics and econometrics, many of them seminal, span more than four decades and touch on all aspects of time series analysis. The papers assembled in this volume explore topics in spectral analysis, seasonality, nonlinearity, methodology, and forecasting. Those in the companion volume investigate themes in causality, integration and cointegration, and long memory. The two volumes contain the original articles as well as an introduction written by the editors.

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CLIVE WILLIAM JOHN GRANGER

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Volume I: Spectral Analysis, Seasonality, Nonlinearity, Methodology, and Forecasting

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To Clive W. J. Granger:
Mentor, Colleague, and Friend.
We are honored to present this selection of his research papers.

E. G.
N. R. S.
M. W. W.

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Introduction

Volume I

At the beginning of the twentieth century, there was very little fundamental theory of time series analysis and surely very few economic time series data. Autoregressive models and moving average models were introduced more or less simultaneously and independently by the British statistician Yule (1921, 1926, 1927) and the Russian statistician Slutsky (1927). The mathematical foundations of stationary stochastic processes were developed by Wold (1938), Kolmogorov (1933, 1941a, 1941b), Khintchine (1934), and Mann and Wald (1943). Thus, modern time series analysis is a mere eight decades old. Clive W. J. Granger has been working in the field for nearly half of its young life. His ideas and insights have had a fundamental impact on statistics, econometrics, and dynamic economic theory.

Granger summarized his research activity in a recent ET Interview (Phillips 1997), which appears as the first reprint in this volume, by saying, "I plant a lot of seeds, a few of them come up, and most of them do not." Many of the seeds that he planted now stand tall and majestic like the Torrey Pines along the California coastline just north of the University of California, San Diego, campus in La Jolla, where he has been an economics faculty member since 1974. Phillips notes in the ET Interview that "It is now virtually impossible to do empirical work in time series econometrics without using some of his [Granger's] methods or being influenced by some of his ideas." Indeed, applied time series econometricians come across at least one of his path-breaking ideas almost on a daily basis. For example, many of his contributions in the areas of spectral analysis, long memory, causality, forecasting, spurious regression, and cointegration are seminal. His influence on the profession continues with no apparent signs of abatement.

SPECTRAL METHODS

In his ET Interview, Granger explains that early in his career he was confronted with many applied statistical issues from various disciplines because he was the only statistician on the campus of the University of Nottingham, where he completed his PhD in statistics and served as lecturer for a number of years. This led to his first publications, which were not in the field of economics. Indeed, the first reprint in Volume II of this set contains one of his first published works, a paper in the field of hydrology. Granger's first influential work in time series econometrics emerged from his research with Michio Hatanaka. Both were working under the supervision of Oskar Morgenstern at Princeton and were guided by John Tukey. Cramér (1942) had developed the spectral decomposition of weakly stationary processes, and the 1950s and early 1960s were marked by intense research efforts devoted to spectral analysis. Many prominent scholars of the time, including Milton Friedman, John von Neumann, and Oskar Morgenstern, saw much promise in the application of Fourier analysis to economic data. In 1964, Princeton University Press published a monograph by Granger and Hatanaka, which was the first systematic and rigorous treatment of spectral analysis in the field of economic time series. Spectral methods have the appealing feature that they do not require the specification of a model but instead follow directly from the assumption of stationarity. Interestingly, more than three decades after its initial publication, the book remains a basic reference in the field.

The work of Granger and Hatanaka was influential in many dimensions. The notion of business cycle fluctuations had been elaborately discussed in the context of time series analysis for some time. Spectral analysis provided new tools and yielded fundamental new insights into this phenomenon. Today, macroeconomists often refer to business cycle *frequencies*, and a primary starting point for the analysis of business cycles is still the application of frequency domain methods. In fact, advanced textbooks in macroeconomics, such as Sargent (1987), devote an entire chapter to spectral analysis. The dominant feature of the spectrum of most economic time series is that most of the power is at the lower frequencies. There is no single pronounced business cycle peak; instead, there are a wide number of moderately sized peaks over a large range of cycles between four and eight years in length. Granger (1966) dubbed this shape the "typical spectral shape" of an economic variable. A predecessor to Granger's 1966 paper entitled "The Typical Spectral Shape of an Economic Variable" is his joint paper with Morgenstern published in 1963, which is entitled "Spectral Analysis of New York Stock Market Prices." Both papers are representative of Granger's work in the area of spectral analysis and are reproduced as the first set of papers following the ET Interview.

The paper with Morgenstern took a fresh look at the random walk hypothesis for stock prices, which had been advanced by the French mathematician M. L. Bachelier (1900). Granger and Morgenstern estimated spectra of return series of several major indices of stocks listed on the New York Stock Exchange. They showed that business cycle and seasonal variations were unimportant for return series, as in every case the spectrum was roughly flat at almost all frequencies. However, they also documented evidence that did not support the random walk model. In particular, they found that very long-run movements were not adequately explained by the model. This is interesting because the random walk hypothesis was associated with definitions of efficiency of financial markets for many years (e.g., see the classic work of Samuelson 1965 and Fama 1970). The Granger and Morgenstern paper is part of a very important set of empirical papers written during the early part of the 1960s, which followed the early work of Cowles (1933). Other related papers include Alexander (1961, 1964), Cootner (1964), Fama (1965), Mandelbrot (1963), and Working (1960). Today, the long-term predictability of asset returns is a well-established empirical stylized fact, and research in the area remains very active (e.g., see Campbell, Lo, and MacKinlay 1997 for recent references).

SEASONALITY

Seasonal fluctuations were also readily recognized from the spectrum, and the effect of seasonal adjustment on economic data was therefore straightforward to characterize. Nerlove (1964, 1965) used spectral techniques to analyze the effects of various seasonal adjustment procedures. His approach was to compute spectra of unadjusted and adjusted series and to examine the cross spectrum of the two series. Nerlove's work took advantage of the techniques Granger and Hatanaka had so carefully laid out in their monograph. Since then, many papers that improve these techniques have been written. They apply the techniques to the study of seasonal cycles and the design of seasonal adjustment filters. For example, many significant insights have been gained by viewing seasonal adjustment procedures as optimal linear signal extraction filters (e.g., see Hannan 1967; Cleveland and Tiao 1976; Pierce 1979; and Bell 1984, among others). At the same time, there has been a perpetual debate about the merits of seasonal adjustment, and since the creation of the X-11 program, many improvements have been made and alternative procedures have been suggested. The Census X-11 program was the product of several decades of research. Its development was begun in the early 1930s by researchers at the National Bureau of Economic Research (NBER) (see, for example, Macaulay 1931), and it emerged as a fully operational procedure in the mid 1960s, in large part due to the work by Julius Shiskin and his collaborators at the U.S. Bureau of the Census (see Shiskin et al. 1967). During the 1960s and 1970s, numerous papers were written on the topic of seasonality, including important papers by Sims (1974) and Wallis (1974). Granger's (1979) paper, "Seasonality: Causation, Interpretation and Implications," is the first of two papers on the topic of seasonality included in this volume. It was written for a major conference on seasonality, which took place in the late 1970s, and appeared in a book edited by Zellner (1979). In this paper, he asks the pointed question, "Why adjust?" and gives a very balanced view of the merits and drawbacks of seasonal adjustment. The paper remains one of the best reflections on the issue of seasonality and seasonal adjustment. The second paper in this subsection, "Is Seasonal Adjustment a Linear or a Nonlinear Data-Filtering Process?," written with Ghysels and Siklos (1996), also deals with a pointed question that was initially posed by Young (1968). The question is: Are seasonal adjustment procedures (approximately) linear data transformations? The answer to this question touches on many fundamental issues, such as the treatment of seasonality in regression (cf. Sims 1974; Wallis 1974) and the theory of seasonal adjustment. The paper shows that the widely applied X-11 program is a highly nonlinear filter.

NONLINEARITY

The book by Box and Jenkins (1970) pushed time series analysis into a central role in economics. At the time of its publication, the theory of stationary linear time series processes was well understood, as evidenced by the flurry of textbooks written during the late 1960s and the 1970s, such as Anderson (1971), Fuller (1976), Granger and Newbold (1977), Hannan (1970), Nerlove et al. (1979), and Priestley (1981). However, many areas of time series analysis fell beyond the scope of linear stationary processes and were not well understood. These areas included nonstationarity and long memory (covered in Volume II) and nonlinear models. Four papers on nonlinearity in time series analysis are reproduced in Volume I and are representative of Granger's important work in this area. Because the class of nonlinear models is virtually without bound, one is left with the choice of either letting the data speak (and suffering the obvious dangers of overfitting) or relying on economic theory to yield the functional form of nonlinear economic relationships. Unfortunately, most economic theories provide only partial descriptions, with blanks that need to be filled in by exploratory statistical techniques. The papers in this section address the statistical foundations of nonlinear modeling and some of the classical debates in the literature of nonlinear modeling.

The first paper, "Non-Linear Time Series Modeling," describes the statistical underpinnings of a particular class of nonlinear models. This paper by Granger and Andersen predates their joint monograph on bilinear models (Granger and Andersen 1978). This class of models is not as popular today as it once was, although bilinear models are connected in interesting ways to models of more recent vintage, such as the class of ARCH models introduced by Engle (1982). One of the classical debates in the literature on nonlinear models pertains to the use of deterministic versus stochastic processes to describe economic phenomenon. Granger has written quite extensively on the subject of chaos (a class of deterministic models) and has expressed some strong views on its use in economics, characterizing the theory of chaos as fascinating mathematics but not of practical relevance in econometrics (see Granger 1992, 1994). Liu, Granger, and Heller (1992), in the included paper entitled "Using the Correlation Exponent to Decide Whether an Economic Series Is Chaotic," study the statistical properties of two tests designed to distinguish deterministic time series from stochastic white noise. The tests are the Grassberger-Procacia correlation exponent test and the Brock, Dechert, and Scheinkman test. Along the same lines, Lee, White, and Granger (1993), in the paper entitled "Testing for Neglected Nonlinearity in Time Series Models" examine a battery of tests for nonlinearity. Both papers are similar in that they consider basic questions of nonlinear modeling and provide useful and practical answers.

The fourth paper in this section, "Modeling Nonlinear Relationships Between Extended-Memory Variables," is the Fisher-Schultz lecture delivered at the 1993 European Meetings of the Econometric Society in Uppsala. The lecture coincided with the publication of the book by Granger and Teräsvirta (1993) on modeling nonlinear economic relationships. This book is unique in the area because it combines a rich collection of topics ranging from testing for linearity, chaos, and long memory to aggregation effects and forecasting. In his Fisher-Schultz lecture, Granger addresses the difficult area of nonlinear modeling of nonstationary processes. The paper shows that the standard classification of $I(0)$ and $I(1)$ processes in linear models is not sufficient for nonlinear functions. This observation also applies to fractional integration. As is typical, Granger makes suggestions for new areas of research, advancing the notions of short memory in mean and extended memory, and relates these ideas to earlier concepts of mixing conditions, as discussed for instance in McLeish (1978), Gallant and White (1988), and Davidson (1994). At this point, it is too early to tell whether any of these will give us the guidance toward building a unified theory of nonlinear nonstationary processes.

The final paper in this section is entitled "Semiparametric Estimates of the Relation Between Weather and Electricity Sales." This paper with Engle, Rice, and Weiss is a classic contribution to the nonparamentric and semiparametric literature and stands out as the first application of semiparametric modeling techniques to economics (previous work had been done on testing). Other early work includes Robinson (1988) and

Stock (1989). Recent advances in the area are discussed in Bierens (1990), Delgado and Robinson (1992), Granger and Teräsvirta (1993), Härdle (1990), Li (1998), Linton and Neilson (1995), and Teräsvirta, Tjostheim, and Granger (1994), to name but a few. In this classic paper, Granger and his coauthors use semiparametric models, which include a linear part and a nonparametric cubic spline function to model electricity demand. The variable that they use in the nonparametric part of their model is temperature, which is known to have an important nonlinear effect on demand.

METHODOLOGY

The title of this subsection could cover most of Granger's work; however, we use it to discuss a set of six important papers that do not fit elsewhere. The first paper is Granger and Morris's 1976 paper "Time Series Modeling and Interpretation." This is a classic in the literatures on aggregation and measurement error. The paper contains an important theorem on the time series properties of the sum of two independent series, say ARMA(p,m) + ARMA(q,n), and considers a number of special cases of practical interest, like the sum of an AR(p) and a white noise process. A key insight in the paper is that complicated time series models might arise from aggregation. The paper also contains the seeds of Granger's later paper (Granger 1987) on aggregation with common factors, which is discussed later.

The next paper, Granger and Anderson's "On the Invertibility of Time Series Models," also deals with a fundamental issue in time series. Invertibility is a familiar concept in linear models. When interpreted mechanically, invertibility refers to conditions that allow the inverse of a lag polynomial to be expressed in positive powers of the backshift operator. More fundamentally, it is a set of conditions that allows the set of shocks driving a stochastic process to be recovered from current and lagged realizations of the observed data. In linear models, the set of conditions is the same, but in nonlinear models they are not. Granger and Anderson make this point, propose the relevant definition of invertibility appropriate for both linear and nonlinear models, and discuss conditions that ensure invertibility for some specific examples.

The third paper in this section is Granger's "Near Normality and Some Econometric Models." This paper contains exact small sample versions of the central limit theorem. Granger's result is really quite amazing: Suppose x and y are two independent and identically distributed (i.i.d.) random variables and let z be a linear combination of x and y. Then the distribution of z is closer to the normal than the distribution of x and y (where the notion of "closer" is defined in terms of cumulants of the random variables). The univariate version of this result is contained in Granger (1977), and the multivariate generalization is given in the paper included here. The theorem in this paper shows that a bivariate process formed by a weighted sum of bivariate vectors whose components are i.i.d. is generally nearer-normal than its constituents, and the components of the vector will be nearer-uncorrelated.

The fourth paper, "The Time Series Approach to Econometric Model Building," is a paper joint with Paul Newbold. It was published in 1977, a time when the merits of Box-Jenkins-style time series analysis versus classical econometric methods were being debated among econometricians. Zellner and Palm (1974) is a classic paper in the area. Both papers tried to combine the insights of the Box-Jenkins approach with the structural approach to simultaneous equations modeling advocated by the Cowles Foundation. The combination of time series techniques with macroeconomic modeling received so much attention in the 1970s that it probably seems a natural approach to econometricians trained over the last two decades. Work by Sims (1980) on vector autoregression (VAR) models, the rational expectations approach in econometrics pursued by Hansen and Sargent (1980), and numerous other papers are clearly a result of and in various ways a synthesis of this debate. Of much more recent vintage is the next paper in this subsection, entitled: "Comments on the Evaluation of Policy Models," joint with Deutsch (1992). In this paper, the authors advocate the use of rigorous econometric analysis when constructing and evaluating policy models and note that this approach has been largely neglected both by policy makers and by econometricians.

The final paper in this section is Granger's 1987 paper, "Implications of Aggregation with Common Factors." This paper concerns the classic problem of aggregation of microeconomic relationships into aggregate relationships. The paper deals almost exclusively with linear microeconomic models so that answers to the standard aggregation questions are transparent. (For example, the aggregate relationship is linear, with coefficients representing averages of the coefficients across the micorpopulation.) The important lessons from this paper don't deal with these questions but rather with the implications of approximate aggregation. Specifically, Granger postulates a microeconomic environment in which individuals' actions are explained by both idiosyncratic and common factors. Idiosyncratic factors are the most important variables explaining the microeconomic data, but these factors are averaged out when the microrelations are aggregated so that the aggregated data depend almost entirely on the common factors. Because the common factors are not very important for the microdata, an econometrician using microdata could quite easily decide that these factors are not important and not include them in the micromodel. In this case, the aggregate model constructed from the estimated micromodel would be very misspecified. Because macroeconomists are now beginning to rely on microdatasets in their empirical work, this is a timely lesson.

FORECASTING

By the time this book is published, Granger will be in his sixth decade of active research in the area of forecasting.[1] In essence, forecasting is based on the integration of three tasks: model specification and construction, model estimation and testing, and model evaluation and selection. Granger has contributed extensively in all three, including classics in the areas of forecast evaluation, forecast combination, data transformation, aggregation, seasonality and forecasting, and causality and forecasting. Some of these are reproduced in this section.[2]

One of Granger's earliest works on forecasting serves as a starting point for this section of Volume I. This is his 1959 paper, "Estimating the Probability of Flooding on a Tidal River," which could serve as the benchmark example in a modern cost-benefit analysis text because the focus is on predicting the number of floods per century that can be expected on a tidal stretch. This paper builds on earlier work by Gumbel (1958), where estimates for nontidal flood plains are provided. The paper illustrates the multidisciplinary flavor of much of Granger's work.

The second paper in this section is entitled "Prediction with a Generalized Cost of Error Function" (1969). This fundamental contribution highlights the restrictive nature of quadratic cost functions and notes that practical economic and management problems may call instead for the use of nonquadratic and possibly nonsymmetric loss functions. Granger illuminates the potential need for such generalized cost functions and proposes an appropriate methodology for implementing such functions. For example, the paper discusses the importance of adding a bias term to predictors, a notion that is particularly important for model selection. This subject continues to receive considerable attention in economics (see, for example, Christoffersen and Diebold 1996, 1997; Hoffman and Rasche 1996; Leitch and Tanner 1991; Lin and Tsay 1996; Pesaran and

[1] His first published paper in the field was in the prestigious *Astrophysical Journal* in 1957 and was entitled "A Statistical Model for Sunspot Activity."

[2] A small sample of important papers not included in this section are Granger (1957, 1967); Granger, Kamstra, and White (1989); Granger, King, and White (1995); Granger and Sin (1997); Granger and Nelson (1979); and Granger and Thompson (1987). In addition, Granger has written seven books on the subject, including *Spectral Analysis of Economic Time Series* (1964, joint with M. Hatanaka), *Predictability of Stock Market Prices* (1970, joint with O. Morgenstern), *Speculation, Hedging and Forecasts of Commodity Prices* (1970, joint with W. C. Labys), *Trading in Commodities* (1974), *Forecasting Economic Time Series* (1977, joint with P. Newbold), *Forecasting in Business and Economics* (1980), and *Modeling Nonlinear Dynamic Relationships* (1993, joint with T. Teräsvirta). All these books are rich with ideas. For example, Granger and Newbold (1977) discuss a test for choosing between two competing forecasting models based on an evaluation of prediction errors. Recent papers in the area that propose tests similar in design and purpose to that discussed by Granger and Newbold include Corradi, Swanson, and Olivetti (1999); Diebold and Mariano (1995); Fair and Shiller (1990); Kolb and Stekler (1993); Meese and Rogoff (1988); Mizrach (1991); West (1996); and White (1999).

Timmerman 1992, 1994; Swanson and White 1995, 1997; Weiss 1996). A related and subsequent paper entitled "Some Comments on the Evaluation of Economic Forecasts" (1983, joint with Newbold) is the third paper in this section. In this paper, generalized cost functions are elucidated, forecast model selection tests are outlined, and forecast efficiency in the sense of Mincer and Zarnowitz (1969) is discussed. The main focus of the paper, however, is the assertion that satisfactory tests of model performance should require that a "best" model produce forecasts, which cannot be improved upon by combination with (multivariate) Box-Jenkins-type forecasts. This notion is a precursor to so-called forecast encompassing and is related to Granger's ideas about forecast combination, a subject to which we now turn our attention.

Three papers in this section focus on forecast combination, a subject that was introduced in the 1969 Granger and Bates paper, "The Combination of Forecasts." This paper shows that the combination of two separate sets of airline passenger forecasts yield predictions that mean-square-error dominate each of the original sets of forecasts. That combined forecasts yield equal or smaller error variance is shown in an appendix to the paper. This insight has led to hundreds of subsequent papers, many of which concentrate on characterizing data-generating processes for which this feature holds, and many of which generalize the framework of Granger and Bates. A rather extensive review of the literature in this area is given in Clemen (1989) (although many papers have been subsequently published). The combination literature also touches on issues such as structural change, loss function design, model misspecification and selection, and forecast evaluation tests. These topics are all discussed in the two related papers that we include in this section – namely, "Invited Review: Combining Forecasts – Twenty Years Later," (1989) and "The Combination of Forecasts Using Changing Weights" (1994, joint with M. Deutsch and T. Teräsvirta). The former paper has a title that is self explanatory, while the latter considers changing weights associated with the estimation of switching and smooth transition regression models – two types of nonlinear models that are currently receiving considerable attention.

The literature on data transformation in econometrics is extensive, and it is perhaps not surprising that one of the early forays in the area is Granger and Newbold's "Forecasting Transformed Series" (1976). In this paper, general autocovariance structures are derived for a broad class of stationary Gaussian processes, which are transformed via some function that can be expanded by using Hermite polynomials. In addition, Granger and Newbold point out that the Box and Cox (1964) transformation often yields variables that are "near-normal," for example, making subsequent analysis more straightforward. (A more recent paper in this area, which is included in Volume II, is Granger and Hallman 1991). The sixth paper in this part of Volume I is entitled "Forecasting

White Noise" (1983). Here Granger illustrates the potential empirical pitfalls associated with loose interpretation of theoretical results. His main illustration focuses on the commonly believed fallacy that: "The objective of time series analysis is to find a filter which, when applied to the series being considered, results in white noise." Clearly such a statement is oversimplistic, and Granger illustrates this by considering three different types of white noise, and blending in causality, data transformation, Markov chains, deterministic chaos, nonlinear models, and time-varying parameter models.

The penultimate paper in this section, "Can We Improve the Perceived Quality of Economic Forecasts?" (1996), focuses on some of the fundamental issues currently confronting forecasters. In particular, Granger espouses on what sorts of loss functions we should be using, what sorts of information and information sets may be useful, and how forecasts can be improved in quality and presentation (for example, by using 50% rather than 95% confidence intervals). The paper is dedicated to the path-breaking book by Box and Jenkins (1970) and is a lucid piece that is meant to encourage discussion among practitioners of the art. The final paper in Volume I is entitled "Short-Run Forecasts of Electricity Loads and Peaks" (1997) and is meant to provide the reader of this volume with an example of how to correctly use current forecasting methodology in economics. In this piece, Ramanathan, Engle, Granger, Vahid-Araghi, and Brace implement a short-run forecasting model of hourly electrical utility system loads, focusing on model design, estimation, and evaluation.

Volume II

CAUSALITY

Granger's contributions to the study of causality and causal relationships in economics are without a doubt among some of his most well known. One reason for this may be the importance in so many fields of research of answering questions of the sort: What will happen to Y if X falls? Another reason is that Granger's answers to these questions are elegant mathematically and simple to apply empirically. Causality had been considered in economics before Granger's 1969 paper entitled "Investigating Causal Relations by Econometric Models and Cross-Spectral

Methods" (see, for example, Granger 1963; Granger and Hatanaka 1964; Hosoya 1977; Orcutt 1952; Simon 1953; Wiener 1956). In addition, papers on the concept of causality and on causality testing also appeared (and continue to appear) after Granger's classic work (see, for example, Dolado and Lütkepohl 1994; Geweke 1982; Geweke et al. 1983; Granger and Lin 1994; Hoover 1993; Sims 1972; Swanson and Granger 1997; Toda and Phillips 1993, 1994; Toda and Yamamoto 1995; Zellner 1979, to name but a very few). However, Granger's 1969 paper is a cornerstone of modern empirical causality analysis and testing. For this reason, Volume II begins with his 1969 contribution. In the paper, Granger uses cross-spectral methods as well as simple bivariate time series models to formalize and to illustrate a simple, appealing, and testable notion of causality. Much of his insight is gathered in formal definitions of causality, feedback, instantaneous causality, and causality lag. These four definitions have formed the basis for virtually all the research in the area in the last thirty years and will probably do so for the next thirty years. His first definition says that ". . . Y_t causes X_t if we are able to better predict X_t using all available information than if the information apart from Y_t had been used" (Granger 1969, p. 428). It is, thus, not surprising that many forecasting papers *post* Granger (1969) have used the "Granger causality test" as a basic tool for model specification. It is also not surprising that economic theories are often compared and evaluated using Granger causality tests. In the paper, Granger also introduces the important concept of instantaneous causality and stresses how crucial sampling frequency and aggregation are, for example. All this is done within the framework of recently introduced (into economics by Granger and Hatanaka 1964) techniques of spectral analysis.

The next paper in this part of Volume II, "Testing for Causality: A Personal Viewpoint" (1980), contains a number of important additional contributions that build on Granger (1969) and outlines further directions for modern time series analysis (many of which have subsequently been adopted by the profession). The paper begins by axiomatizing a concept of causality. This leads to a formal probabilistic interpretation of Granger (1969), in terms of conditional distribution functions, which is easily operationalized to include universal versus *not* universal information sets (for example, "data inadequacies"), and thus leads to causality tests based on conditional expectation and/or variance, for example. In addition, Granger discusses the philosophical notion of causality and the roots of his initial interest and knowledge in the area. His discussion culminates with careful characterizations of so-called instantaneous and spurious causality. Finally, Granger emphasizes the use of post-sample data to confirm causal relationships found via in-sample Wald and Lagrange multiplier tests.

Continuing with his methodological contributions, the third paper, "Some Recent Developments in a Concept of Causality" (1986), shows that if two $I(1)$ series are cointegrated, then there must be Granger causation in at least one direction. He also discusses the use of causality tests for policy evaluation and revisits the issue of instantaneous causality, noting that three obvious explanations for apparent instantaneous causality are that: (i) variables react without any measurable time delay, (ii) the time interval over which data are collected is too large to capture causal relations properly, or that temporal aggregation leads to apparent instantaneous causation, and (iii) the information set is incomplete, thus leading to apparent instantaneous causality. It is argued that (ii) and (iii) are more plausible, and examples are provided. This section closes with a frequently cited empirical investigation entitled "Advertising and Aggregate Consumption: An Analysis of Causality" (1980). The paper is meant to provide the reader with an example of how to correctly use the concept of causality in economics. In this piece, Ashley, Granger, and Schmalensee stress the importance of out-of-sample forecasting performance in the evaluation of alternative causal systems and provide interesting evidence that advertising does not cause consumption but that consumption may cause advertising.

INTEGRATION AND COINTEGRATION

Granger's "typical spectral shape" implies that most economic time series are dominated by low-frequency variability. Because this variability can be modeled by a unit root in a series' autoregressive polynomial, the typical spectral shape provides the empirical motivation for work on integrated, long memory, and cointegrated processes. Granger's contributions in this area are usefully organized into four categories. The first contains research focused on the implications of this low-frequency variability for standard econometric methods, and the Granger and Newbold work on spurious regressions is the most notable contribution in this category. The second includes Granger's research on linear time series models that explain the joint behavior of low-frequency components for a system of economic time series. His development of the idea of cointegration stands out here. The third category contains both empirical contributions and detailed statistical issues arising in cointegrated systems (like "trend" estimation). Finally, the fourth category contains his research on extending cointegration in time-invariant linear systems to nonlinear and time-varying systems. Papers representing his work in each of these categories are included in this section of Volume II.

The first paper in this section is the classic 1974 Granger and Newbold paper "Spurious Regressions in Econometrics," which contains what is arguably the most influential Monte Carlo study in econometrics. (The closest competitor that comes to our mind is the experiment reported in Slutsky 1927.) The Granger-Newbold paper shows that linear regressions involving statistically *independent*, but highly persistent random variables will often produce large "t-statistics" and sample R^2s. The results reported in this paper showed that serial correlation in the regression error together with serial correlation in the regressor have disastrous effects on the usual procedures of statistical inference. The basic result was known (Yule 1926), but the particulars of Granger and Newbold's experiments were dramatic and unexpected. Indeed, in his ET Interview, Granger reminisces about giving a seminar on the topic at the London School of Economics (LSE), where some of the most sophisticated time-series econometricians of the time found the Granger-Newbold results incredible and suggested that he check his computer code. The paper had a profound impact on empirical work because, for example, researchers could no longer ignore low Durbin-Watson statistics. One of the most insightful observations in the paper is that, when considering the regression $y = x\beta + \varepsilon$, the null hypothesis $\beta = 0$ implies that ε has the same serial properties as y, so that it makes little sense constructing a t-statistic for this null hypothesis without worrying about serial correlation. The basic insight that both sides of an equation must have the same time series properties shows up repeatedly in Granger's work and forms the basis of what he calls "consistency" in his later work.

The Granger-Newbold spurious regression paper touched off a fertile debate on how serial correlation should be handled in regression models. Motivated by the typical spectral shape together with the likelihood of spurious regressions in levels regressions, Granger and Newbold suggested that applied researchers specify regressions using the first-differences of economic time series. This advice met with skepticism. There was an uneasy feeling that even though first-differencing would guard against the spurious regression problem, it would also eliminate the dominant low-frequency components of economic time series, and it was the interaction of these components that researchers wanted to measure with regression analysis. In this sense, first-differencing threw the baby out with the bath water. Hendry and Mizon (1978) provided a constructive response to the Granger-Newbold spurious regression challenge with the suggestion that time series regression models be specified as autoregressive distributed lags in levels (that is, $a(B)y_t = c(B)x_t + \varepsilon_t$). In this specification, the first-difference restriction could be viewed a common factor of $(1 - B)$ in the $a(B)$ and $c(B)$ lag polynomials, and this restriction could be investigated empirically. These autoregressive distributed lag models could also be rewritten in error-correction form, which highlighted their implied relationship between the levels of the series (useful references for this includes Sargan 1964; Hendry, Pagan, and Sargan 1981; and Hendry 1995).

This debate led to Granger's formalization of cointegration (see ET Interview, page 274). His ideas on the topic were first exposited in his 1981 paper "Some Properties of Time Series Data and Their Use in

Econometric Model Specification," which is included as the second paper in this section of Volume II. The paper begins with a discussion of consistency between the two sides of the previously mentioned equation. Thus, if $y = x\beta + \varepsilon$ and x contains important seasonal variation and ε is white noise that is unrelated to x, then y must also contain important seasonal variation. The paper is most notable for its discussion of consistency in regards to the order of integration of the variables and the development of "co-integration," which appears in Section 4 of the paper. (As it turns out, the term was used so much in the next five years that by the mid-1980s the hyphen had largely disappeared and co-integration became cointegration.) The relationship between error-correction models and cointegration is mentioned, and it is noted that two cointegrated variables have a unit long-run correlation. The paper probably contains Granger's most prescient statements. For example, in discussing the "special case" of the autoregressive distributed lag that gives rise to a cointegrating relation, he states: "Although it may appear to be very special, it also seems to be potentially important." And after giving some examples of cointegrated variables, he writes: "It might be interesting to undertake a wide-spread study to find out which pairs of economic variables are co-integrated."

Granger expanded on his cointegration ideas in his 1983 paper "Time Series Analysis of Error Correction Models" with Weiss, which is included as the third paper in this section. This paper makes three important contributions. First, it further explores the link between error-correction models and cointegration (focusing primarily on bivariate models). Second, it introduces methods for testing for cointegration. These include the residual-based tests developed in more detail in Engle and Granger's later paper and the tests that were analyzed several years later by Horvath and Watson (1995). The paper does not tackle the unit-root distribution problems that arise in the tests (more on this later) and instead suggests practical "identification" procedures analogous to those used in Box-Jenkins model building. The final contribution of the paper is an application of cointegration to three classic economic relations, each of which was studied in more detail by later researchers using "modern" cointegration methods. The first application considered employee income and national income (in logarithms) and, thus, focused on labor's share of national income, one of the "Great Ratios" investigated earlier by Kosobud and Klein (1961) using other statistical methods. The second application considered money and nominal income, where Granger and Weiss found little evidence supporting cointegration. Later researchers added nominal interest rates to this system, producing a long-run money demand relation, and found stronger evidence of cointegration (Baba, Hendry, and Star 1992; Hoffman and Rasche 1991; Stock and Watson 1993). The third application considered the trivariate system of nominal wages, prices, and productivity, which was studied in more detail a decade later by Campbell and Rissman (1994).

The now-classic reference on cointegration, Engle and Granger's "Co-Integration and Error-Correction: Representation, Estimation and Testing," is included as the fourth paper in this section. This paper is so well known that, literally, it needs no introduction. The paper includes "Granger's Representation Theorem," which carefully lays out the connection between moving average and vector error correction representations for cointegrated models involving $I(1)$ variables. It highlights the nonstandard statistical inference issues that arise in cointegrated models including unit roots and unidentified parameters. Small sample critical values for residual-based cointegration tests are given, and asymptotically efficient estimators for $I(0)$ parameters are developed (subsequently known as Engle-Granger two-step estimators). The paper also contains a short, but serious, empirical section investigating cointegration between consumption and income, long-term and short-term interest rates, and money and nominal income.

Granger's 1986 "Developments in the Study of Cointegrated Economic Variables" is the next entry in the section and summarizes the progress made during the first five years of research on the topic. Representation theory for $I(1)$ processes was well understood by this time, and several implications had been noted, perhaps the most surprising was the relationship between cointegration and causality discussed in the last subsection. (If x and y are cointegrated, then either x must Granger-cause y or the converse, and thus cointegration of asset prices is at odds with the martingale property.) Work had begun on the representation theory for $I(2)$ processes (Johansen 1988a; Yoo 1987). Inference techniques were still in their infancy, but great strides would be made in the subsequent five years. A set of stylized cointegration facts was developing (consumption and income are cointegrated, money and nominal interest rates are not, for example). The paper ends with some new ideas on cointegration in nonlinear models and in models with time-varying coefficients. This is an area that has not attracted a lot of attention (a notable exception being Balke and Fomby 1997), primarily because of the difficult problems in statistical inference.

Cointegration is one of those rare ideas in econometrics that had an immediate effect on empirical work. It crystallized a notion that earlier researchers had tried to convey as, for example, "true regressions" (Frisch 1934), low-frequency regressions (Engle 1974), or the most predictable canonical variables from a system (Box and Tiao 1977). There is now an enormous body of empirical work utilizing Granger's cointegration framework. Some of the early work was descriptive in nature (asking, like Granger and Weiss, whether a set of variables appeared to be cointegrated), but it soon became apparent that cointegration was an implication of important economic theories, and this insight allowed researchers to test separately both the long-run and short-run implications of the specific theories. For example, Campbell and Shiller (1987) and Campbell (1987) showed that cointegration was an implication of rational expectations versions of present value relations, making the concept immediately germane to a large number of applications including the permanent income model of consumption, the term structure of interest rates, money demand, and asset price determination, for example. The connection with error correction models meant that cointegration was easily incorporated into vector autoregressions, and researchers exploited this restriction to help solve the identification problem in these models (see Blanchard and Quah 1989; King et al. 1991, for example).

Development of empirical work went hand in hand with development of inference procedures that extended the results for univariate autoregressions with unit roots to vector systems (for example, see Chan and Wei 1987; Phillips and Durlauf 1986). Much of this work was focused directly on the issues raised by Granger in the papers reproduced here. For example, Phillips (1986) used these new techniques to help explain the Granger-Newbold spurious regression results. Stock (1987) derived the limiting distribution of least squares estimators of cointegrating vectors, showing that the estimated coefficients were T-consistent. Phillips and Ouliaris (1990) derived asymptotic distributions of residual-based tests for cointegration. Using the vector error-correction model, Johansen (1988b) and Ahn and Reinsel (1990) developed Gaussian maximum likelihood estimators and derived the asymptotic properties of the estimators. Johansen (1988b) derived likelihood-based tests for cointegration. Many refinements of these procedures followed during the late 1980s and early 1990s (Phillips 1991; Saikkonen 1991; Stock and Watson 1993, to list a few examples from a very long list of contributions), and by the mid 1990s a rather complete guide to specification, estimation, and testing in cointegrated models appeared in textbooks such as Hamilton (1994) and Hendry (1995).

During this period, Granger and others were extending his cointegration analysis in important directions. One particularly useful extension focused on seasonality, and we include Hylleberg, Engle, Granger, and Yoo's "Seasonal Integration and Cointegration," as the next paper in this section. A common approach to univariate modeling of seasonal series is to remove the seasonal and trend components by taking seasonal differences. For example, for quarterly data, this involves filtering the data using $(1 - B^4)$. This operation explicitly incorporates $(1 - B^4)$ into the series' autoregressive polynomial and implies that the autoregression will contain four unit roots: two real roots associated with frequencies 0 and π and a complex conjugate pair associated with frequency $\pi/2$. Standard cointegration and unit-root techniques focus only on the zero-frequency unit root; the Hyllberg et al. paper discusses the complications that arise from the remaining three unit roots. Specifically, the paper develops tests for unit roots and seasonal cointegration at frequencies other than zero. This is done in a clever way by first expanding the autoregressive polynomial in a partial fraction expansion with terms associated with each of the unit roots. This simplifies the testing problem because it makes it possible to apply standard regression-based tests to filtered versions of the series. This paper has led to the so-called HEGY approach of testing for seasonal roots separately. It has been extended in several ways notably by Ghysels et al. (1994) who built joint tests, such as testing for the presence of all seasonal unit roots, based on the HEGY regressions.

Many of Granger's papers include empirical examples of the proposed techniques, but only occasionally is the empirical analysis the heart of the paper. One notable exception is "A Cointegration Analysis of Treasury Bill Yields," with Hall and Anderson, which is included as the sixth paper in this section. The framework for the paper is the familiar expectations theory of the term structure. There are two novelties: first, the analysis is carried out using a large number of series (that is, twelve series), and second, the temporal stability of the cointegrating relation is investigated. The key conclusion is that interest-rate spreads on 1–12 month U.S. Treasury Bills appear to be $I(0)$ except during the turbulent 1979–82 time period.

A natural way to think about cointegrated systems is in terms of underlying, but unobserved, persistent, and transitory components. The persistent factors capture the long-memory or low-frequency variability in the observed series, and the transitory factors explain the shorter memory or high-frequency variation. In many situations, the persistent components correspond to interesting economic concepts ("trend" or "permanent" income, aggregate productivity, "core" inflation, and so on) Thus, an important question is how to estimate these components from the observed time series, and this is difficult because there is no unique way to carry out the decomposition. One popular decomposition associates the persistent component with the long-run forecasts in the observed series and the transitory component with the corresponding residual (Beveridge and Nelson 1981). This approach has limitations: notably the persistent component is, by construction, a martingale, and the innovations in the persistent and the transitory components are correlated. In the next two papers included in this section, Granger takes up this issue. The first paper, "Estimation of Common Long-Memory Components in Cointegrated Systems," was written with Gonzalo. They propose a decomposition that has two important characteristics: first, both components are a function only of the current values of the series, and second, innovations in the persistent components are uncorrelated with the innovations in the transitory component. In the second paper, "Separation in Cointegrated Systems and Persistent-Transitory Decompositions" (with N. Haldrup), Granger takes up the issue of estimation of these components in large systems. The key question is whether the components might be computed separately for groups of series so that the components could then be analyzed separately without having to model the entire system of variables. Granger and Haldrup present conditions under which this is possible. Unfortunately the conditions are quite stringent so that few simplifications surface for applied researchers.

The final three papers in this section focus on nonlinear generalizations of cointegration. The first two of these are joint works with Hallman. In "Nonlinear Transformations of Integrated Time Series," Granger and Hallman begin with integrated and cointegrated variables and ask whether nonlinear functions of the series will also appear to be integrated and cointegrated. The problem is complex, and few analytic results are possible. However, the paper includes several approximations and simulations that are quite informative. One of the most interesting results in the paper is a simulation that suggests that Dickey-Fuller tests applied to the ranks of Gaussian random walks have well-behaved limiting distributions. This is important, of course, because statistics based on ranks are invariant to all monotonic transformations applied to the data. In their second paper, "Long Memory Series with Attractors," Granger and Halman discuss nonlinear *attractors* (alternatively $I(0)$ nonlinear functions of stochastically trending variables) and experiment with semiparametric methods for estimating these nonlinear functions. The last paper, "Further Developments in the Study of Cointegrated Variables," with Swanson is a fitting end to this section. It is one of Granger's "seed" papers – overflowing with ideas and, as stated in the first paragraph, raising "more questions than it solves." Specifically, the paper not only discusses time-varying parameter models for cointegration and their implications for time variation in vector error-correction models, how nonlinear cointegrated models can arise as solutions to nonlinear optimization problems, and models for nonlinear leading indicator analysis but also contains a nonlinear empirical generalization of the analysis in King et al. (1991). No doubt, over the next decade, a few of these seeds will germinate and create their own areas of active research.

LONG MEMORY

Even though integrated variables have been widely used in empirical work, they represent a fairly narrow class of models capable of generating Granger's typical spectral shape. In particular, it has been noted that autocorrelation functions of many time series exhibit a slow hyperbolic decay rate. This phenomenon, called long memory or sometimes also called long-range dependence, is observed in geophysical data, such as river flow data (see Hurst 1951, 1956; Lawrence and Kottegoda 1977) and in climatological series (see Hipel and McLeod 1978a, 1978b;

Mandelbrot and Wallis 1968) as well as in economic time series (Adelman 1965; Mandelbrot 1963). In two important papers, Granger extends these processes to provide more flexible low-frequency or long-memory behavior by considering $I(d)$ processes with noninteger values of d. The first of these papers, Granger and Joyeux's (1980) "An Introduction to Long-Memory Time Series Models and Fractional Differencing," is related to earlier work by Mandelbrot and Van Ness (1968) describing fractional Brownian motion. Granger and Joyeux begin by introducing the $I(d)$ process $(1 - B)^d y_t = \varepsilon_t$ for noninteger d. They show that the process is covariance stationary when $d < \frac{1}{2}$ and derive the autocorrelations and spectrum of the process. Interestingly, the autocorrelations die out at a rate τ^{2d-1} for large τ showing that the process has a much longer memory than stationary finite-order ARMA processes (whose autocorrelations die out at rate $\rho\tau$ where $|\rho| < 1$). In the second of these papers, "Long Memory Relationships and the Aggregation of Dynamic Models," Granger shows how this long-memory process can be generated by a large number of heterogenous AR(1) processes. This aggregation work continues to intrigue researchers, as evidenced by recent extensions by Lippi and Zaffaroni (1999).

Empirical work investigating long-memory processes was initially hindered by a lack of statistical methods for estimation and testing, but methods now have been developed that are applicable in fairly general settings (for example, see Robinson 1994, 1995; Lobato and Robinson 1998). In addition, early empirical work in macroeconomics and finance found little convincing evidence of long memory (see Lo 1991, for example). However, a new flurry of empirical work has found strong evidence for long memory in the *absolute value* of asset returns. One of the most important empirical contributions is the paper by Ding, Granger, and Engle, "A Long Memory Property of the Stock Market Returns and a New Model," which is included as the last paper in this section. Using daily data on S&P 500 stock returns from 1928 to 1991, this paper reports autocorrelations of the absolute values of returns that die out very slowly and remain significantly greater than zero beyond lags of 100 periods. This finding seems to have become a stylized fact in empirical finance (see Andersen and Bollerslev 1998; Lobato and Savin 1998) and serves as the empirical motivation for a large number of recent papers.

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CHAPTER 1

The ET Interview: Professor Clive Granger

Peter C. B. Phillips

Since the 1960's, Clive Granger has been one of our most influential scholars in time series econometrics. His writings encompass all of the major developments over the last 30 years, and he is personally responsible for some of the most exciting ideas and methods of analysis that have occurred during this time. It is now virtually impossible to do empirical work in time series econometrics without using some of his methods or being influenced by his ideas. In the last decade, the explosion of interest in cointegration is alone a striking testimony to the effect that his ideas have had on our discipline. For several decades, his work on causality, spurious regression, and spectral analysis have had profound and lasting influence. Most scholars would deem it the accomplishment of a lifetime if their work were to have the impact of a single one of these contributions. To have had repeated instances of such extraordinarily influential research is surely testimony to Clive Granger's special talent as a researcher and writer.

Possibly the most defining characteristic of Granger's work is his concern for the empirical relevance of his ideas. In a typical Granger paper, this message comes through in a powerful way, and it serves as a useful reminder to us all that ideas truly do come first in research and that mathematical niceties can indeed come later in the successful development of interesting new econometric methods. Another hallmark of the Granger style is the accessibility of his work, which stems from his unusually rich capacity to write highly readable papers and books, some of which have gone on to become citation classics. These demonstrable successes in communication show us the vital role that good writing plays in the transmission of scientific knowledge.

Like many Englishmen, Clive Granger loves to travel. He is a familiar face and a regular invited speaker at conferences in econometrics, time series, and forecasting throughout the world. Wherever he goes, he

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is greeted by former students and welcomed by admirers of his research. It seems fitting, therefore, that the interview that follows was recorded away from his home in March 1996 at Texas A&M University, where we attended a conference on time series analysis hosted by the Department of Statistics. We met again in Rio de Janeiro in August 1996, at the Latin American Meetings of the Econometric Society, and concluded a penultimate version of the transcript while enjoying a further opportunity to talk econometrics and time series. Clive Granger's research has been an inspiration to us all, and it is a pleasure and honor to present this conversation with him to a wider audience.

Welcome Clive. Thank you for agreeing to do this interview. In the first part of the interview, I would like to cover your educational background and some of the highlights of your career. Can you start by telling us about your early intellectual interests – at school and at home.

I cannot say I was especially distinguished at anything, except mathematics. I was always relatively good at mathematics compared to my peers. This got me promotion in school and advancement to grammar school in Britain, which was important in those days, and then eventually to university. Otherwise, I had very wide interests, but nothing that I would say was worth recording.

Which grammar schools did you attend?

I attended two. They were the Cambridgeshire High School, just outside Cambridge, and West Bridgford Grammar School in Nottingham.

At school, were you already thinking about a career later in life?

I always wanted to use my mathematics, but not to be a pure mathematician. My hope was to find an area of applied mathematics that was going to be helpful or useful in some sense. I felt that pure mathematics in itself was rather sterile, being interesting, but not directly useful to people. I considered a variety of possible application areas and my first thought was meteorology. At high school on one occasion, we all had to stand up and announce what our future career was going to be. In those days I stuttered a bit, and I stood up and I tried to say meteorology and I could not say the "m," so I said statistician because at least I could say the word. That switched me into becoming a statistician, so stuttering partly determined my future career.

Almost a self-fulfilling prophecy.

Exactly.

When you went on to university, did you start studying statistics immediately or did that come later?

No, when I was applying to universities, I was looking at statistics departments and, of course, mathematics with statistics. Nottingham University, at that time, was just starting up the first-ever joint degree in economics and mathematics, and that struck me as a very interesting application. It was brand new in those days in Britain. And so I applied, even though Nottingham was my home town, and it was always thought a good idea to go away to another city. I liked the description of the degree because it mixed two things – one thing I thought I could do, and one thing I thought was going to be interesting, economics, and I liked very much the people there in Nottingham. They did not get too many applicants the first year, so I think that got me into that degree rather easily. So, I went to Nottingham to enter that joint degree, but at the end of the first year, the Math Department persuaded me to switch over to mathematics but to concentrate on statistics. My idea always was to go back and at some point try to finish off the economics part of the joint degree, but I never did that formally. Then, when I finished my math degree at Nottingham, I did a Ph.D. in statistics, but always with the idea of doing statistics that was useful in economics.

Did they have a statistics unit within the Mathematics Department at Nottingham?

No.

Just some people who were interested in statistics?

Yes. There were a couple of people there who taught statistics, but they were really pure mathematicians, just doing service teaching. And there was one pure mathematician, Raymond Pitt, the professor, who was an extremely good probability theorist. So between them, I got a rather formal training in statistics, with no applications of any kind.

So you went into this line of study thinking that there would be a strong connection with applications, but ended up being more of a mathematician by the time you had finished.

Right.

After you completed your degree, you had to steer yourself into applications. Were you able to do any reading in economics during the degree? I presume you did a few courses in economics as you went along?

Yes, but the way it was structured I could only do economics in the first year. That was rather frustrating, because the economists, though I held them in very high repute, were not very mathematical. Their discussions were always in words, which I would then try to rephrase mathematically, but that was not always that easy, because they did not always understand what I was trying to say and what they were trying to say did not always translate very clearly, in my opinion. In the first year, as a mathematician, I had trouble understanding the economists.

So looking back now, what do you think the major influences were on you during your university education?

I think I got a very sound, pure mathematics training, but I kept alive the interest in learning more about economics and applying mathematics and statistics in economics. The economists there were convinced that the future in economics lay in the mathematical and quantitative side of the subject, even though they themselves were not trained in that area. The head of the department at Nottingham, Brian Tew, was a brilliant economist, a specialist in banking and macroeconomics, who was not mathematically trained at all. He was not a believer in much of macrotheory and held the hope of new results coming from quantitative studies, particularly econometrics. That is why he encouraged me always to come back to economics and to apply new techniques to that area.

They must have thought very highly of you as a student to make the move of appointing you to a lectureship before you had finished your Ph.D. How did that come about?

That was a time when the British universities were expanding very rapidly, and getting an appointment was not particularly difficult. Nottingham had a new position in mathematics that they advertised, and they asked me whether I would apply, even though at that time I was only in my first year as a graduate student. I was lucky to get this opportunity, but I could hardly say no to my professor in that circumstance. They wanted me really to pad out the list of people to choose among. It turned out that they only had two applicants; the other one was much better qualified than I was but somehow managed to irritate the Appointments Committee, and so they selected me. Thus, I was appointed to be a lecturer, totally unqualified in my opinion, particularly compared to today's new appointments in universities. But it was just a chance event because of the high growth rate of British universities at that time.

So you completed your thesis and lectured in mathematics at the same time.

Right.

What sort of teaching assignments did you have in the early years?

As I was the only statistician, or official statistician, in the university, I was supposed to do service teaching for the Mathematics Department. This I did and taught in mathematics and for any other group who needed statistics courses. The only people who actually wanted a service course was economics, which I provided. The problem initially was that I knew all about Borel sets and things from my own learning of statistics from Cramér, but I did not know how to form a variance from data. I mean, I literally had never done that when I first started teaching, so I had to learn real statistics as I went along. I also taught a geometry course and various general courses in math for engineers and service courses of that type. But the best thing about my position there was that I was the only statistician on campus. Faculty from all kinds of areas would come to me with their statistical problems. I would have people from the History Department, the English Department, Chemistry, Psychology, and it was terrific training for a young statistician to be given data from all kinds of different places and be asked to help analyze it. I learned a lot, just from being forced to read things and think about a whole diverse type of problems with different kinds of data sets. I think that now people, on the whole, do not get that kind of training.

> *That does sound unusual. Statistics departments now service those needs with a group of people rather than just one person. So you encountered many different types of data in this work, not just time series, which was the main type of data in economics in those days.*

Yes.

> *Did you manage to maintain contact with the Economics Department during this time?*

Yes, although I actually published things in areas other than economics at that time, material that arose from some of this consulting work.

> *I gather from what you said a few moments ago that one of the main books that influenced you was Harald Cramér's* Mathematical Methods of Statistics?

Yes, that was the book that we used for our course work in probability and statistics.

> *Did you have to read it cover to cover?*

Pretty well, because my teacher was extremely strong on measure theory, as that was his major area for research at one time.

> *After you had been at Nottingham for a few years, you got an opportunity to go to Princeton. Would you tell us about this?*

There were some scholarships available to people from Britain and, in fact, also Australia, to go to the States, called the Harkness Scholarships of the Commonwealth Fund. They were fairly competitive, but I was lucky enough to get one. What they did was allow you to go to the States for a year or even two years, to choose wherever you wanted to go to and just do nothing but research for a period. They also gave you money to travel around the States and you had to guarantee to go back to your own country for several years afterwards. The idea was to get promising people from these countries to go to the USA, to understand the country better, and then go back to tell other people about, from inside as it were, what life was like in the U.S. and the way the country thought about things and did things. So I wrote to various places in the U.S., saying I had this scholarship and can I come and do some research. I got just two positive responses, one was from the Cowles Commission at Yale and one was from Princeton, from Oscar Morgenstern. Morgenstern said, "Come and join our time series project." As that sounded very promising, I decided to do that. I went to Princeton and the time series project turned out to be Michio Hatanaka and myself. But we were to study under John Tukey about spectral analysis. John Tukey had developed univariate and bivariate spectral analysis, and Oscar Morgenstern had been told by Von Neumann some years previously that Fourier methods should be used in economics, and Oscar had always wanted to have a project that used Fourier methods. Tukey had agreed to supervise a couple of people in Morgenstern's group in these methods and so Michio and I were the people designated to be taught these new methods. That was an extremely rewarding experience. I have tremendous admiration for John Tukey, intellectually and personally. We were taught in a very unconventional way. John Tukey was always unconventional in anything that he did. We would meet once a week and we would use real data, and he would just tell us to do a certain computation on this data. Michio, who knew more about computing than I did, would program and do the computation, and I would try and write down the mathematics of what we were doing. The next week, John Tukey would interpret the results we got from the computation and then tell us to do something else, the next computation. And so over a period, we built up this experience of working with data and interpreting it. At the same time, I was working out mathematically what we were actually doing, which John was not explaining.

How remarkable.

It was a very interesting way to learn.

It sounds like a team of rocket scientists, with the head scientist telling the juniors what to do and the juniors then trying to decipher what the instructions meant.

Exactly.