Eric Ghysels, Norman R. Swanson and Mark W. Watson, 

This is the second volume of a collection of articles by Nobel prize winner Clive W.J. Granger compiled by Eric Ghysels, Norman Swanson and Mark Watson. The first volume contained a selection of articles representing the fundamental contributions of Granger to the fields of spectral analysis, forecasting, seasonality, non-linearity and the methodology of time series econometrics. For this second volume, the editors have selected a number of important contributions in some of the fields for which Granger is probably best known, that is Causality, Integration and Cointegration, and Long Memory. Many of the papers contained in this volume are among the most influential articles published over the last 40 years. At the time of this writing, for example the seminal paper on cointegration by Engle and Granger published in 1987 in *Econometrica* received, according to Hendry (2004), more than 2800 citations and is certainly amongst the most often cited paper ever published in economics. Similar remarks could be made on Granger and Newbold’s path-breaking study of the danger of “nonsense regression”, known as the spurious regression problem, published in 1974 in the *Journal of Econometrics*. This is a fairly unique achievement in the history of the economic and econometric literature. It's easy to imagine the hard time the editors had while preparing these volumes and especially when deciding about the selection of papers they wanted to include. Out of the numerous papers Granger published on the topics of cointegration, causality and long memory, the editors did clearly retained the most fundamental ones, without forgetting some particularly interesting papers that have probably slightly been overlooked in the literature but that are historically nonetheless important in the development of these concepts and ideas. As it has been emphasized many times since he receive the Nobel prize, the work of Granger has fundamentally changed the face of both empirical economics and theoretical time series econometrics since, as it appears very clear after re-reading the contribution reprinted here, most of his work is motivated and deeply rooted in practical issues that applied economists face when analyzing economic data. The volume is divided into three different parts related respectively to Causality, Integration and Cointegration, and Long Memory.

The concept of causality in itself has always been, and still is, the subject of many contradictory debates among econometricians and statisticians. The classic Granger 1969 *Econometrica* paper that is included in this volume formalized the concept of non-causality in terms of unpredictability, that eventually received the label Granger non-causality, and made the concept operational and hence, within the limit of its definition, testable. For the importance of the concept and its the consequence on
the way causality has been viewed and discussed in time series econometrics is analyzed and reviewed by Hendry and Mizon (1999). Interestingly, the volume also reproduces two subsequent studies published in the *Journal of Econometrics* in 1980 and 1988 where Granger clarified his view and reformulated his conditions for non-causality in terms of density functions. Although almost four decades have passed since the publication of this seminal contribution, the concept of Granger non-causality still attracts researchers working on a variety of problems like the extension to non-stationary multivariate processes, to dynamic possibly non-stationary panels, to nonlinear processes, continuous time processes, ….

Part 2 of the volume concentrates on what have been the most important developments in time series econometrics over the last 20 years, namely the advent of the concepts of integration, cointegration and error correction models. This part starts with the study by Newbold and Granger that revisited the old nonsense regression issue pointed out initially by Yule in 1926 to the case of two independent random walk processes. While the paper was initially published in 1974; it wasn’t really until the early/mid 80’s that the researcher community recognized the fundamental underlying ideas in particular after it became clear that the answers/solutions to the danger of spurious regression was rooted in serious cointegration analysis. The concept of cointegration initially introduced in the early 80’s by Granger has helped the unification of a variety of issues that where only partly understood in the literature or used in empirical work but without a solid theoretical motivation. Cointegration for example gave a statistical basis for error correction models and so enabled economists and applied econometricians to build dynamic models mixing short run dynamics with long run equilibrium relationships motivated by economic theory; all this in a coherent statistical framework. The development of this framework took some time as emphasized for example nicely in Hendry (2004). It is therefore welcome that the editors also choose to present the various contributions in their chronological order, starting with the spurious regression study of Granger and Newbold, followed by two papers from the early 80’s that actually where the first to introduce the concept of cointegration, years before the Engle and Granger study was published in *Econometrica* in 1987. The remaining papers of this part are contributions that introduced further development in which Granger’s input has been instrumental such as the famous Hylleberg, Engle, Granger and Yoo (HEGY) paper in *Journal of Econometrics* where the authors extend the idea of integration and cointegration to seasonal processes; the paper with Gonzalo on the extraction of common permanent (the \( \ll \)common trends\( \rr \)) components; as well as the work of Granger with Hallman and Swanson where extensions to nonlinear transformation of integrated times and to non-linear cointegration are introduced. Interestingly enough, they also included a somewhat overlooked paper by Granger and Haldroup published in 1997 in the *Oxford Bulletin of Economics and Statistics* that studies the conditions under which it is valid to extract common stochastic trends from small dimensional sub-systems. Many of these papers have one common features: the elaboration and introduction of ideas in a simple and understandable way that has most likely led subsequent famous successful researchers like Sören Johansen or Peter Phillips to enter these fields; but also numerous applied researcher to adopt these methodological developments in their empirical work.
The last part of the volume contains three papers on the use of long memory model in economic modelling. Granger was again one of the first econometricians to recognize that, although integrated variables have the appealing property of being simply to understand and to work with, they actually represent a rather specific class of models that do not explain the hyperbolic decay rate of autocorrelation functions that are encountered with some economic time series. Starting from ideas initially developed for other fields such as the study of climatological or river flow data; Granger and Joyeux in 1980 in their *Journal of Time Series Analysis* paper adapted these ideas and came up with processes, $I(\delta)$ processes, allowing to capture more general and flexible low frequency behavior. In his subsequent paper published in 1981 in *Journal of Econometrics* Granger showed that such long memory behavior could actually be generated by the aggregation of a large number of heterogeneous autoregressive processes. The last study included in this volume showed the empirical evidence of long memory in absolute value of assets returns. Since these early papers on long memory in econometrics, numerous development have seen the light. The presence of long memory in absolute financial returns or realized volatility seem to have become a stylized fact, and numerous advances have been made in terms of statistical methods for estimation and testing long memory components in economic series.

Re-reading all these contributions is fascinating and eye-opening on the fundamental effect that Granger had on research in economics. It is a fact that it is nowadays almost impossible to conduct theoretical or applied work in time series econometrics without using Granger's contributions or without relying on recent advances and developments that are rooted in his work. Most time series econometricians and applied economist dealing with economic time series data certainly have copies of all of at least of some of these papers somewhere in their office. This volume, as well as the first one, however constitute a perfect opportunity to have a global view on Granger's contribution. By bringing all these together and by including a few somewhat overlooked papers, this book constitutes a highly recommendable addition to your bookshelf.

Jean-Pierre Urbain*

REFERENCES


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This volume aims to make the latest international insights on business history available to non-specialists. Each of the 18 chapters is an up-to-date survey written by an expert. The book is segmented into three parts. Part I gives an overview on the general issues and debates. Part II covers the current state of business history in various countries. Part III reviews comparative research in three research areas. The last chapter on the opportunities for business history at the beginning of the twenty-first century is written by Alfred D. Chandler, Jr., the doyen of business history.

After the introduction by the editors, Galambos gives an overview of the historiography of business history and the influence of Chandler. Business history was born at the Harvard Business School in the mid-1920’s. Between 1930 and 1960 it was an isolated sub discipline. This changed however due to Chandler, who constructed a historical framework of the economic and technological efficiency of administrative co-ordination and learning within large enterprises, and promoted internationally the study of business history. Since the 1960s many scholarly histories based on archives were published as well as a wide range of articles and surveys on various aspects of business history, which matured as an academic discipline both in the United States and Europe. Furthermore some dedicate journals were founded. Important aspects in this evolution were the convergence with economics, history, finance and business management, and the methodological controversy among business historians. Next Lazonick considers how to integrate economic theory and business history by introducing the theory of the innovative enterprise. This focuses on ‘how the structure of strategic control mobilizes the collective power of the skills and efforts of participants in the enterprise’s specialized division of labor to transform technological and market conditions’ (p. 60). In this way and equipped with some analytical tools, business historians could question some of the economists’ a priori assumptions about among others labour division. In the last theoretical chapter Zeitlin rejects the theological and deterministic model of ‘… the existence of a unilinear logic of material progress that must be adopted by all wishing to advance to higher levels of productivity, income and wealth’ (p. 63), that has become the core theme of industrial history. He presents an alternative approach aiming to gain more knowledge about past organisational and productive forms, governance and strategic choices.

Part II consists of eleven chapters, each giving an overview of state of business history in a particular country or region, such as the United States, France, Italy, Spain, Greece, Britain and the Netherlands, the Scandinavian countries and the German speaking nations. Each chapter goes into the origin of the research area and the national development. It is noticed that in most countries banking history is one of the oldest forms of business history. Since two decades business history also took off in Latin America, Japan and China as demonstrated by the chapters concerned.
Part III is on the rather unexploited research field of comparative business history. For quite obvious reasons there is still little comparative research. After all, international comparison requires a thorough understanding of the economic, social and institutional context. Still attempts have been made on the topics: family firms, multinationals, business-government relations. These three chapters are perhaps the most interesting as they provide a framework for cross-national comparisons. For example much has been written on the causes and determinants of multinational growth, but on the impact of foreign multinationals on host countries much less is known.

Summarising. The variety of topics clearly shows that business history is a very broad field of research including everything from the history of individual firms to entire business systems. From the beginning business historians tried to attract the attention of economists, economic historians and business scientists. But sometimes they failed due to a lack of a common ‘language’ and a way of doing things. Having a strong national focus it is surprising that there has been little convergence with institutional economics and institutional history, yet. Although the book is meant to be for non-specialists, in my opinion some chapters require some familiarity with the subject, otherwise one gets easily lost in the given magnitude of information. Another point of criticism concerns the editing, as there is much repeating in describing the origin of business history. Furthermore considering the title of the book one wonders if business history does not exist in the Soviet Union, South Africa, more Asian countries or for example Australia. Nevertheless this book is good starting point.

Joke Mooij


This book, written by Jean Gadrey and first published in French and updated in 2003, discusses the probable myth of the new economy. Gadrey is not afraid to give his opinion on the new economy that finds its example in the USA as role model of the free market economy. To the author’s opinion, the new economy is a discourse, one that is often misleading and placed at the service of powerful private interests. Since there is no theory of this mythical age, the American model takes the place of theory and proof. The new economy is idealized and references to the American growth model that emerged in the second half of the 1990s are selective. According to Gadrey, we are very far from a radical change in economic practices, production, consumption and ways of life. The revolution is not going to happen. Although new technologies bring in unquestionable innovations, it strengthens the principles of the traditional economy. Gadrey is calling into question the ‘ultra-free market’ thinking in terms of the existence of optimal models of technology diffusion, growth, flexibility, market and capitalism. The Internet and digital revolution are acting as a sort of Trojan horse in spreading market deregulation across the globe.
The book consists of 148 pages and originally contained eight chapters in the French version of 2001. Two small chapters, a preface and a postface extend the English-language edition. Both chapters contain to some extent new information, particularly the aftermath of the NASDAQ-bubble. In the introduction, Gadrey puts forward some precautionary principles and warns European policymakers for the consequences of following the footsteps of the United States. Next, Gadrey describes the six main characteristics of the new economy: high-growth economy, importance of production and diffusion of ICT on the expansion of service jobs, highly flexible labour force and labour market, market economy, and finally, requirement of a new mode of corporate governance. The subsequent chapters work out and criticize these characteristics. The main line of his discourse is that the performance in terms of economic growth and more jobs are overstated, and the risks of flexible markets in terms of less social cohesion are understated. To overcome this way of thinking, Gadrey states that it is better not to look at economic growth primarily but to look at indicators that place social developments at the heart of its objectives as well.

My overall opinion on the book is mixed. I truly agree that the new economy does not exist and that we do not need new economic rules. It is even better to use the term renewing economy as each general purpose technology rejuvenates the growth process in the economy by creating a bunch of new opportunities. And, indeed, the United States is not the role model. Although its citizens are on average far richer than Europeans are, the American richness is unequally distributed according to the high poverty rates. Indeed, we have to be alert on developments that destroy social cohesion and creates dualism (have's and have's not). However, Gadrey exaggerates the flip-side of the ‘ultra-free’ market thinking. It is a fact that the economic growth performance of the United States is by at large better than that of the European countries as a whole. The Americans are particularly richer because they work longer. But why? Gadrey leaves undiscussed the issue whether it is completely voluntarily that Europeans work less compared to Americans.

In the future, ageing, technological progress, increasing international mobility and more heterogeneity among people will increase the price of social cohesion and public services. These developments are almost left aside in the book but are very important in the trade-off between equity and efficiency. For instance, the technology process seems to be skilled bias increasing the earnings inequality. To assure public services of a certain quality level and equity, changes in institutions are needed. One way out is introducing market incentives in (semi) public services such as yardstick competition. Yardstick competition is a regulatory instrument that can be used if direct competition between agents is low or absent. The regulator compares the performance of various agents and rewards or punishes agents on basis of their relative performance.

Finally, an unresolved thought reading this book. If the new economy is a myth why then be concerned about its ‘undesirable’ effects.

Henry van der Wiel

This book deals with the logit model in a very clear, informative and from time to time humorous way, which makes the book suitable for undergraduate level courses on discrete choice models. Logit models are statistical models which can be used when the dependent variable is a state (choice) variable and not a continuous variable. Some parts of the book have already been published by Cramer in an earlier book entitled 'The Logit Model: an Introduction for Economists' which appeared in 1991.

The content of the book is as follows. Chapter 1 describes the logit model shortly, and discusses some similarities and differences between the logit and the ordinary least squares model. Chapter 2 introduces the binomial logit model, which is the core model throughout the book. The binomial logit model is a binary discrete probability model relating one or more determining variables with the binomial outcome variable of interest. In this chapter the background of the model is given, its main properties are discussed as well as the latent regression characteristics the logit model embodies. Chapter 3 discusses maximum likelihood estimation of the logit model and explains the principles and statistical properties of ML parameter estimates. It also gives an example of the use of the binary logit model by presenting and discussing Cramer’s 1980 study on private car ownership. Chapter 4 is a continuation of chapter 3 and deals with statistical tests suitable for ML estimations and goodness-of-fit tests. Chapter 5 pays attention to the effects of imperfections in the data and the model like outliers, misclassification of the outcomes in the study and the effect of omitted variables on the parameter estimates. Chapter 6 is on the analyses of separate samples. In case of separate samples the logit model can also be a powerful analytical tool. Collecting data from separate samples can be very efficient if one of the outcomes is very rare in the population. State dependent sampling and case–control studies are examples of studying separate samples and are commonly used in medical sciences.

Chapter 7 deals with the estimation of the multinomial logit model and chapter 8 focuses on theoretical aspects of this model. Chapter 7 discusses the ordered multinomial model but pays most attention to the estimation of the multinomial logit model. Cramer uses his private car ownership study to give an example of the multinomial logit model in action. Chapter 8 introduces the general logit model. It discusses Mc Fadden’s model of random utility maximisation which constitutes the microeconomic foundation of the general logit model. McFadden has proven that the logit model can be derived from utility maximising choice behaviour of individuals. Both the multinomial logit model and the conditional logit model are popular specifications of the general logit model. Cramer’s 1987 study on the choice of a mode of payment illustrates the similarities and the differences between these two specifications of the logit model.

Chapter 9 is the main novelty of this book and I really enjoyed reading this part. Chapter 9 starts in the first half of the 19th century when the logistic model was introduced by the mathematician Verhulst as a way to describe population growth.
In 1920 the logistic function was reinvented by the biologist Pearl who did not know about Verhulst’s work until a few years later. The probit model also has its roots in the 19th century and was also reinvented several times. The probit model was originally designed in medical sciences, of which the studies of Gaddum and Bliss (early 1930s) are best known, to analyse stimulus response models in order to derive the distribution of individual tolerance. Two decades later this model was first used in economics and market research.

Cramer thinks that the logit model was first introduced by Berkson, a statistician, in 1944, challenging the probit model. However, the probit model was already quite popular and the virtues of the logit model were unrecognized until the end of the 1960’s. But from then on, with the help of Cox, the logit model became very popular, mainly because it was much easier to work with than the probit model. In the 1960s the logit and the probit model were not only used in medical and pharmaceutical sciences but also in economics and other social sciences. The fast introduction of PCs in the 1980s and statistical packages in the 90s did the rest and made logit and probit models easy-to-use empirical research tools.

Summarising, I think Cramer has written a very clear and instructive book on logit models. Cramer also pays much attention to showing examples on the models at work, often drawing on his own work. However, if you are interested in the newest developments in logit analysis and are looking for the most advanced estimation techniques, you are at the wrong address, although it provides some useful references. The most interesting part of Cramer’s book is the last chapter. Here, the roots of the discrete choice models and the developments of these models in biology, medicine and pharmaceutics are discussed. Cramer describes this all very well and I hope other readers will enjoy reading about the background of discrete choice as much (or maybe even more) than I did.

Nicole Jonker