Invited Session to Commemorate Clive Granger, ESAM, Canberra, 10 July 2009

Opening remarks by Kenneth F. Wallis

It is an honour to be asked to participate in this session to commemorate Clive Granger. My contributed paper is on a topic in forecast evaluation, a subject on which Clive made many important contributions. However his influence on my work goes back a long way, and I have been given a few minutes to describe this relationship, by way of a personal tribute. The profession enjoyed an amazing flow of ideas from Clive that lasted 50 years, and I have time for just a small selection. I emphasise the personal nature of my remarks, which are mostly set in the 1960s and 70s, with an audience of fellow econometricians in mind, and I am not competing with any official obituary.

The name C. W. J. Granger first entered my world in 1964, when I was starting dissertation research at Stanford. My supervisor, Marc Nerlove, advised me to read *Spectral Analysis of Economic Time Series*, just published by Princeton University Press. We usually cite this book as "Granger and Hatanaka" although the title page says "By C. W. J. Granger in association with M. Hatanaka" and Clive had done most of the writing of the book. It was the main output of Clive's year at Princeton on a Harkness Fellowship (1959-60), and he finished the writing up in return visits from Nottingham to the US in the following three summers. I believe they had had problems getting the work published in article form, and Oskar Morgenstern had encouraged them to produce the book. From my point of view as a PhD student this was a good outcome, since the book is at the same time research monograph and advanced textbook, and I learned an enormous amount. At the time there was no alternative book, and it became a *Citation Classic*. Returning to the book in the last few days, it is astonishing to be reminded how many of the things we now take for granted first appeared in its pages – insights into the power of frequency domain methods and ways of harnessing them for one's own research purposes. And the book contains the seeds of Clive's first two *Econometrica* articles. First was "The typical spectral shape of an economic variable", which he wrote up at Stanford in Summer 1963. In his Nobel autobiography he says it was published "three and a half years after submission", but in fact it appeared in the January 1966 issue of the journal, which I make two and a half years later, but no doubt it felt like a long wait – an experience we have all shared, waiting for our first articles to come out. The second is the famous causality article, published in the July 1969 issue of *Econometrica*.

The basic definitions of causality and feedback were already there, in chapter 7 of the book, and much of the argument in the article remains in the frequency domain.

I first met Clive at a conference of econometricians organised by Gordon Fisher and Terence Gorman, at Reading University in the Easter vacation, 1967, my first year at LSE. He was an enthusiastic conference goer, always keen to present his ideas and discuss those of others. Some of this enthusiasm obviously rubbed off, since I was soon asked to take on the organisation of the continuation of this sequence of conferences. But the next shared conference I remember was the first Research Council conference on macro modelling, held at Southampton University in April 1969. We both feature in the published record of the discussions at this meeting. What does not appear in the conference volume is the advance presentation Dale Jorgenson gave, of the forecast comparisons by Ronald Cooper to be presented at an NBER conference later that year. Cooper's results, showing how several large-scale quarterly models of the US economy could be outperformed in forecasting by univariate autoregressions, had considerable impact on both our subsequent research activities.

Clive has told in his Nobel autobiography how he came to forecasting as a line of research that "had great potential" by reading an advance copy of Box and Jenkins' book in 1968. Two important articles quickly appeared, both in the British OR journal, which Clive correctly forecast would give speedier publication than *Econometrica*. One is the seminal article with John Bates on forecast combination, which opened up a whole new sub-field in forecasting. At the latest count it had gathered 802 citations on Google Scholar, which may include one by me and Jeremy Smith for an article in last month's *Oxford Bulletin of Economics and Statistics* on what James Stock and Mark Watson called the "forecast combination puzzle". The other *OR Quarterly* article, on forecasting with generalised cost functions, has fewer citations but is currently enjoying a resurgence of interest, as research finds increasing evidence that responses to forecast surveys in several countries exhibit important departures from the quadratic loss function that we find so convenient in analytical work.

In 1970 Clive obtained a Research Council grant, recruited a Research Associate, Paul Newbold, and the Nottingham Forecasting Project got under way. It was clear to many of us that the message from Cooper's results was that, since the statistical forecasting models

emphasise dynamic and stochastic specification, it was here that the structural econometric models were deficient; indeed, they often possessed substantial residual autocorrelation. In a couple of papers on statistical forecasting and forecast combination, Clive and Paul sought to demonstrate the lessons that econometricians might learn. Some of us at LSE were working to integrate the two approaches, developing what Chris Gilbert subsequently called "the LSE tradition in time-series econometrics" – a name we had not thought of at the time. When I was awarded a Research Council grant for a project on "Links between statistical time series analysis and econometric model-building" the obvious thing to do was to jump on the train to Nottingham and spend a day with Clive, not only to exchange research ideas but also to learn how to deal with the Research Council. I found this extremely useful, although I'm not sure that I ever persuaded him that the idea that the univariate time series model might be thought of as a final equation of the econometric model was as useful as I thought it was.

The Nottingham Forecasting Project was unusually productive. A personal favourite is their paper on "Forecasting transformed series", published in *JRSS*B, which later inspired a couple of short pieces by me. Their results were useful, because by then I was involved in hands-on research on practical macroeconometric models. Although commonly described as non-linear models, these are typically linear in simple instantaneous transformations or simple functions of variables, the first case being the one Clive and Paul had considered.

By the time I made my first visit to UCSD Clive had been there for a couple of years, and the Nottingham Forecasting Project was no more. However Paul had spent a year there, helping to finish things off, and the result was their book, *Forecasting Economic Time Series*. I was highly delighted to be presented with a signed copy by Clive soon after my arrival. Like the spectral analysis book, the forecasting book is a judicious blend of research monograph and advanced textbook, which is so difficult to bring off as successfully as in these two examples. The forecasting book is again full of ideas and is widely used in teaching and research, with a second edition in 1986 and almost 1700 Google Scholar cites.

While these remarks have been mostly concerned with Clive's work on forecasting, the topic of my paper today, perhaps I can be allowed one final recollection, of my second visit to UCSD and what has turned out to be his jackpot idea, namely cointegration. Clive's concept of cointegrated series first appeared in his article in the *Journal of Econometrics* in 1981. This issue of the journal contains papers and abstracts from a conference held in Florida in

April 1980. It might then be asked, why is it that we date cointegration from 1981, rather than 1980, the date of the conference? The answer is, because cointegration was not in the paper Clive delivered at the conference! The conference paper introduced the notion of a consistent regression equation, namely one in which the time series properties of the variables on each side match up, and this was discussed in terms of integrated series, mostly considering fractional integration. Coincidentally, David Hendry and I were on leave at UCSD in Spring Term 1981, and I remember one day Clive came and put cointegrated series on our blackboard. The conference paper was being prepared for publication, and he took the opportunity to add a new section to get his new idea out very quickly, neatly avoiding refereeing delays. Although this case "may appear to be very special", he said, "it also seems to be potentially very important". What a forecast that was!

So many ideas, so many memories, thank you.

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