Essays in Econometrics

Collected Papers of Clive W. J. Granger

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

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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.

That style of directing research is not used much these days, at least in economics or econometrics.

Well, I think it would not work for every group of people, but John was very good. I would show him the mathematics and he would agree with me eventually, but the problem in the end came out that we wanted to publish this because it was all very new, particularly the bispectrum or cross-spectrum, but John Tukey was too busy to actually publish his work, so he just allowed us to publish it. That is how the book came out. We did refer to him, obviously, as the originator of all of this area, but we could not wait for him to publish, because it still would not have appeared. I do not think that he has ever published his work in this field.

That, in itself, is rather extraordinary, isn't it?

Yes.

The Princeton project was an interesting example of successful coordination between people in mathematics and economics departments.

There were a variety of skills that happened to mix fairly nicely in this case. Michio was a very good economist as well as a good statistician. We all got along together very well. We did not actually learn much about economics, in a sense, from the particular example we were working on, but we learned a lot about spectral analysis. Then, from that, we could move on to do other experiments and other applications.

A fascinating synergy – bringing people together with different skills from different parts of the world to achieve something that would not have been done otherwise. The Cowles Commission was very good at doing this sort of thing in the 40's and early 50's. Did Cowles offer you anything interesting as an alternative?

No, they just said you are welcome to come.

So, after Princeton you went back to Nottingham. Was that a little deflating after having been over in the U.S., working on this exciting research project?

Well, Morgenstern was very nice to me, and I had worked very hard at Princeton for him. I had done everything he had asked me to do, and, of course, I was benefiting from it, enjoying it and so on. He invited me back every summer for three years, and so I did not lose the link with Princeton. Because of that, Morgenstern and I wrote a book together on the stock market, plus some articles. So it was not as though I was cut off from America; I kept sòmething of a link for a period with both places. I would spend a year in Nottingham lecturing and then come back to summer in Princeton, which was not physically all that pleasant, but intellectually it was great, and Michio was there still. If in fact, he was permanently present there.

So that lent some continuity to the project. Did Michio ever get over to see you in Nottingham?

No.

After getting back to Nottingham, did you find it to be a "lower energy" environment than Princeton?

At Nottingham I was the only person – the only statistician or econometrician there – and so there was almost no one to talk to. I could do my own work, and read and so on, but at Princeton there were just dozens of people to talk to. David Brillinger was there at that time, as were a number of really good econometricians, some students of Tukey, all of whom were worthwhile for me to interact with, as well as faculty, like Dick Quandt. There were many people around, including the game theorists.

There was one rather exciting episode that was not really related to econometrics. I do not quite remember the year, but this was the year when the American President was going to meet with the Russian President for the first time in many years. Morgenstern was an advisor to Eisenhower on game theory, and so he came roaring into the department one day saying, "You have got to learn something about bargaining theory. No one knows anything about bargaining theory [at least to this point in time]. So drop everything you are doing." He called in everybody "to sit down and do nothing but think about bargaining theory for two weeks, because we must tell the President what to do when he meets the Russian President to bargain. Because he has few ideas from a scientific viewpoint." And so it was rather fun, and we had some really good game theorists in town, Kuhn and so on. I think Dick Quandt was also involved. We just had these continuously running seminars discussing what bargaining was about. It was really exciting because you felt that if we did something, it might have an impact on world history at some point.

Rather like the Manhattan Project.

That's right.

So, did anything come out of it?

No, because the U2 plane incident happened, and then the meeting was canceled. In my opinion, we did not discover all that much about bargaining theory. We got a few basic principles, that sort of thing; we did not get anything very deep. But it was exciting. It was different from what we had been doing.

Very different. Back in England in the 1960's, some important things were happening in econometrics, especially at the London School of Economics (LSE). You were physically distant from London, but did you have any contact with the group there?

From my perspective, the main activity was indeed at the LSE. It was rather an insider-outsider thing. I was very much an outsider, as I was not really included in their activities. I would hear about them, and I would occasionally see something from them. I knew a lot of the people at the LSE, such as Bill Phillips, Jim Durbin; and Denis Sargan, and later I knew Ken Wallis and David Hendry. But I was never a close member of that group.

At that stage, they had not started the regular Econometric Study Group meetings in the U.K., which did help to bring people in econometrics together. They started around the time I went to England in 1971. Given the separation, did you feel it was a disadvantage being outside London?

No, I wished I was part of the group in some ways, because then I would feel more accepted. But, on the other hand, I think there was some advantage to not being part of the group.

Maintaining your own research agenda and working independently?

Yes. I remember one instance where Paul Newbold and I had done some work on spurious regression, a Monte Carlo study, and I gave a talk about it at the LSE. It was met with total disbelief. Their reaction was that we must have gotten the Monte Carlo wrong – we must have done the programming incorrectly. I feel that if I had been part of the LSE group, they might well have persuaded me not to have done that research at that point.

I wish I had been there at that time! A fascinating story.

Later they became quite strong supporters of that point.

Indeed.

It shows how when people are so convinced that they are right that they have difficulty accepting the ideas of another person who holds a different opinion.

I remember that there was a strong negativism about the Box-Jenkins methodology at the LSE at that time. It was apparent at several of the Econometric Study Group meetings held there. Whereas up at Essex, there was a great deal of support for Box-Jenkins modeling methods – we had seminars on it in the statistics group with Chris

Winsten and others. Around that time, in 1974, you moved to UC San Diego. Would you like to tell us how this transition came about?

While I was at Princeton, one of my friends there was Dan Orr, who was a graduate student at the time. Eventually, he became the head of the department at San Diego, UC of San Diego, and he invited us out for a six-month visit. We really liked the place, liked it physically and liked the people very much. Then a couple of years later, he offered me a position there. At that time, I was getting rather fed up with England for various reasons. I had been at the same university at Nottingham for all my career. I had been an undergraduate and a graduate and had stayed on up to full professor in the same university, which is really not that good of an idea, I think. If it were not for Princeton, I would have been totally inbred. Also, the British economy was starting to go bad at that point. So I just felt the need for a change of scene. If you are going to move, you can move 6,000 miles as easily as 60, really. I mean, once you have packed up, it is not that much different. So we decided to go to San Diego for five years and see if we liked it. If we did not like it, we would return to Britain. Well, after five years, there were no jobs in Britain. The British economy had really gone bad and there was no choice to make. We were happy in San Diego at that point, and there was no alternative, so we stayed on.

But then, five years or so later, a lot of academics were leaving Britain.

Yes, exactly. When I left Nottingham, I made two forecasts: one was that the British economy would do less well than the U.S. economy, and the second was there would be better weather in San Diego than in Nottingham. Both forecasts turned out to be perfectly correct. So I was happy about them.

So you were not at all apprehensive about making this big international move?

Well, as we had visited for six months, we pretty well knew what we were getting into, because we knew the place and we knew the people. And we had good friends there. We were a bit apprehensive about some things. The children were more worried than we were, in a sense. As far as academic life was concerned, it clearly was going to be an improvement, I think, over Nottingham, but I was sorry to leave Paul Newbold. He and I were getting along very well and being very productive. Paul actually came to San Diego for the first year with me when I first went to San Diego. Yes, looking back, there were some difficulties in transition. But you have to make some adjustments sometimes.

Were your children in junior, middle, or high school at that time?

I think they were only ages 6 and 10.

That is probably a good stage to be moving with children.

Yes, my daughter was 6, so she moved okay. My son certainly had problems. He was worried about whether he would fit into the new environment.

San Diego has now turned into a first-rate department with a world-class econometrics unit. What was it like when you arrived? Can you give us some thoughts on what has happened in the interim?

Yes, when I arrived it was very quiet in econometrics. John Hooper was there, who did not publish very much and was not active in research at all. There were other people there who knew some econometrics but were not active in the area. So I was not going to a place that was strong in econometrics in the slightest. The group got built up by accident, in a sense. Rob Engle joined us because he and I were on a committee together for a conference in Washington, and because he happened to be looking for a position he just asked me if I knew of somewhere that was looking for an econometrician, and I said, "Yes, we are." He came out. We liked him. He liked us and joined us, and that was a terrific appointment. Then, Hal White came out as a visitor and again he liked the place very much, and just asked if there was a position. Again, we were delighted to say yes. And so that, again, was a terrific appointment. So neither of them were planned. This was not really empire building in the sense that somebody had a plan and an ambition to build a group. It just happened.

So destiny determined all these appointments, including your own. In a sense, they were almost incidental.

Yes, I think the fact that the faculty has stayed together has been more work than getting things together in the first place. It is clear that there have been offers for people to move and there have been counteroffers at San Diego, but the department has been very supportive of the group, and so people have been content to stay. They have been happy enough in San Diego and the salary differences are not that much between other offers and San Diego. And so the fact that we have managed to keep together has been one of the major reasons that the group looked so strong. There has not been much movement around. Stability, I think, is important.

And there has been growth and new strength in other areas. You now have Jim Hamilton, for example.

Absolutely, another very good appointment.

So, looking back over your career in England and the U.S., how would you characterize the main differences between the U.S. and the U.K. systems?

The U.K. system is self-stifling. The more research you do, the more administration you get to do, because as you get promoted in Britain the more committees you are put on and the less time you have to do research. Whereas in the States, there is much more time to do research over the whole year. Not only do we have teaching assistants to mark our scripts for us, which is a big help, but we also have research assistants to help us do some of our computing and data collection or whatever. I can spend a lot more time doing research in the States than I could in Britain. There are also more colleagues to talk to in an American university than in a British university. In a British university, you are lucky to have one other good person. In Nottingham, for years, I had nobody. Then I had Paul Newbold, which was like night and day. Having at least one good person to talk to was just terrific. In San Diego, I have several good people to talk to all the time, plus visitors. The one negative thing, I think, in the U.S. as compared to Great Britain, is that, in my experience in Britain, it is easier to talk to people from lots of different disciplines over lunch, in meetings and different committees. We would meet and talk about their problems or other intellectual matters. I do not find I do this in San Diego. Most departments do not interact very much. Whether that is just San Diego, I do not know, because I do not have enough experience in other universities in the State. But it seems to be a pity. I had expected when I went to San Diego that I would continue to be involved with people in other departments, but there is no cross-disciplinary discussion. I think that universities will suffer from that.

Is this also the case with the statistics group at San Diego? Have they been interested in fostering links with the econometrics group?

I think that it is a purely personal matter, depending on who happens to be in the group at the time. We have had people in the group there who have been very anxious to link up and do things jointly and other people who have not. The statistics group there has changed over the years. There is no overall plan of any kind.

Sometimes students can help to bring departments together. If there are good students in the mathematics and statistics departments who are interested in applications in other areas like economics, that can bring faculty together if only through joint thesis advising. Have you had any examples like this is San Diego, students coming over from statistics and mathematics?

I have been on several Ph.D. committees in the Math Department, but they are all extremely technical probabilistic-type Ph.D.'s, and I can hardly understand even what the topic is, let alone the details of the thesis.

Let's move on now to your own research. I want to start by asking you the unanswerable question that I think everyone would like me to ask. That is, what is the key to your own success in writing highly readable and influential papers over so many years?

I would claim to try and do research that other people find useful. And I think if I have any ability, it is a good taste in finding topics that I can make a contribution to and that other people then find interesting.

Some people would call it a nose for the right problem. Do you feel that instinct operating as you are thinking about problems to work on or areas to work in?

I am usually working on several problems at once. I mean, I always have lots of things that I am thinking about and I will often drop topics that I do not think other people will find interesting. Even though I might find something fairly interesting myself, I just do not do it because I have a preference for topics that will have an impact somewhere. This goes back to my original idea of doing applicable work as opposed to just things to work on.

So, this is a theme that you have maintained from the time you were a student at university.

Yes. I do not know why.

Is it personally satisfying to feel that you are still following much the same trajectory in your research?

Yes, it gives you a kind of focus on things, a viewpoint that allows you to make decisions.

In the same general vein, what do you find interesting or impressive about other people's work?

I find that if I can understand what the purpose of the research is, a simplicity of statement, and if the point being made is very clear cut, a simple point, then I am impressed by that. I do not mind whether there is a lot of technique or not in the paper. I am ambivalent about that. What I really want to see at the beginning is a statement about what is being done and why and that there is some sort of clear result to which I will say, "Well, that is really interesting." That impresses me. I do not like papers that are really complicated and that, in the end, have conclusions that are very complicated. Then it is too difficult for me to work out whether there is anything in there, anything that is worth having.

This is partly a matter of communication and partly a matter of the real objectives behind research. When you are looking for topics to work on yourself, do you have a hunch about whether or not something is going to work out?

Yes, in fact, often with a lot of the ideas I have, already I have got some intuition about what the final result is going to look like, even before I start doing any mathematics or writing anything down. It does not always work out that way, but usually I know what the shape of the article is going to be before I start. And, from that, I think that I can sell it or not sell it, or work out whether it is interesting to other people. Quite a lot of the topics I work on have arisen from some applied area. So in a sense, if you solve something, you know that group is going to be interested in the topic. Sort of a ready-made audience for a solution. But, then again, I think, most people do not want very complicated answers to their questions. If you can tell them a nice simple answer, if there is a simple answer, then that is what they want.

Yes, I think that comes over clearly in empirical research. People like ordinary least-squares regression, vector autoregression, techniques like this that are easily used and understood. A lot of your papers emphasize ideas and concepts, and although they have technical derivations in them, you do not ever really dwell on the mathematics. You seem to want to get through to the useable end-product as quickly as possible. Another feature of your papers is that you have a clear desire to communicate what you are doing. Do you feel that that comes naturally or is that something that you work hard to achieve in your writing?

I think it is something that I do think about when I am writing, but I also think that the British educational system does teach you to write fairly well compared to some other educational systems.

Not to mention any in particular?

Exactly. Certainly, in England, I was forced to write an essay at university every week for a year or two, so you just get quite good at writing essays, and that is relevant for writing down fairly clear conclusions. That is not unimportant.

Scientific communication is difficult partly because it is so multifaceted. There are technical concepts, the mathematical development, all the working processes, the empirical calculations, and then the conclusions. Often, people are encouraged to emphasize the theorems, the derivations, the technical novelty, as distinct from the useable results. I do not want to dwell too long on this point, but I do think that this is one feature that distinguishes your work from others. If you can offer any more insights on your writing, then I think it will be valuable to people.

Partly it is my limitation on technique. My math is okay, but it is not terrific. I do not do a lot of high-powered mathematics, because, in a sense, I am not that comfortable with it. I can follow it, but I do not necessarily want to develop it or to bring new mathematics to an area that is already well developed. I have enough mathematics to survive in what I am doing. I typically want to get an idea across, and so I am much more inclined to do it in terms of simple bivariate cases, and then say we can clearly generalize this, and let someone else do that. Because once people have got the idea, their generalization is not all that difficult and you often do not learn all that much from a generalization. I think it is the first idea that matters. That is what I am trying to get across.

Do you find that it is useful to stand back from your work and take a long, hard look at it? Or, to think in general terms about where you are going rather than the minutiae of working it all out? For example, with cointegration, there are clearly a lot of details that need to be worked out. Even the Granger representation theorem is not a trivial thing to resolve. Is thinking about what you are producing and where you are going important to you?

No, I just rely on intuition. I just feel there is a result there, and I try to get most of the result myself and I am comfortable with presenting that and then letting other people do it properly. I would say that I try and get an idea and then I develop it a little bit and when the mathematics gets too difficult, I get out and let someone else proceed with it. That is true with the work on causality, for example. The causality idea is a very simple idea, but it can be put in a much more mathematical and technical framework, as now has been done by several people. Now, whether or not we learn much from all that technical stuff is a different matter.

In mathematics and statistics, some people find that they get a lot of false starts, spend a lot of time doing something, and nothing comes of it. Have you found that in your work?

Yes, I was thinking of this the other day. I plant lots of seeds, a few of them come up, and most of them do not. So, all the time, I have lots of little ideas I am working on or thinking about, and some I find that I am not going to get anywhere with, and so I just drop them. And others seem very promising and I will dig into those much deeper, read more, and try and find things that are relevant for it. I do not often get a long way into a subject and then have to drop it. I typically find out pretty quickly if I am getting out of my depth, or if it is not looking very promising.

Do you have any projects that you have been working on or thinking about for long periods of time like 25 or 30 years and you still have not solved, that kind of thing?

No, no, I drop things.

Let's talk about methodology. As you know, methodology has been a big topic in econometrics now for a decade or more at conferences and in the literature. Where do you see us going on this?

Let me just talk about time series for a moment. In time series, we are getting swamped with different alternative models we can fit. We have got hundreds of different sorts of nonlinear models, for example. We have dozens of different variations of ARCH model, and so on, as well as long-memory and short-memory models. Putting it all together, we have got so many different models now that we have to have a methodology of deciding which part of this group to aim at and use. That is a problem. And, as we get more computing power and more data, that is going to become more of a problem, not less of problem, because more and more models are potentially useable in a data set. What we are seeing now is different people who have different favorites just using those favorite models on their data and saying, "Look guys, it works," and not doing comparisons. The one advantage we have in time series is that we can do postsample analysis. We can compare models using forecasting ability as a criteria, because we can make forecasts and then compare them to actual observations. So, I think, in forecasting and in the time series area, provided the postsample is generated by the same type of mechanism as the sample itself, we do have a pretty clear way of comparing models and evaluating alternatives. Now, let us say this is either not available or has not been used in other areas of econometrics. For example, you do not see the same methodology used in panel data work or in cross-section analyses. I think that the methodology in these areas is in less good shape than in time series, because they do not have a proper evaluation technique. So, there are obviously many problems in methodology in time series, but at least we do have, in my opinion, a reasonable way of deciding between models.

So you see big differences between microeconometrics and time series econometrics in terms of the capability to compare and evaluate different models?

Yes, the criticism that I put to microeconometricians is that they do not phrase their output in terms of errors from a decision-making mechanism. They do not say that they are trying to generate a number that is going into a decision and the decision mechanism will lead to an error, and there is a cost to such errors and that we can compare different models with the cost of the error. I am not saying it is easy to do, I am just saying they are not even thinking in those terms. But we do think in those terms in forecasting and are hopefully learning by so doing.

Of course, time series analysts have been working for 25 years on model determination criteria, and we now know a great deal about *these criteria in a time series context. Do you favor a classical statistical approach to this, or do you see some advantages in the Bayesian paradigms here?*

I have always told Arnold Zellner I am not a Bayesian because I lack self-confidence. That is, you have to have enough self-confidence to have a specific prior on things, and I do not think I know enough about things to have a specific prior. I may have a general prior on some things. I think that a good Bayesian, that is, a Bayesian who picks a prior that has some value to it, is better than a non-Bayesian. And a bad Bayesian who has a prior that is wrong is worse than a non-Bayesian, and I have seen examples of both. What I do not know is how do I know which is which before we evaluate the outcome.

Let's talk more about your personal research now. You have already told us something about the history of spectral analysis. Is there anything more you would like to say about this? For example, in the 50's and 60's, economists were very concerned in macroeconomics about business cycles and, no doubt, that was one of the driving forces behind getting into the frequency domain approach.

Well, I think it was. But Oscar Morgenstern was not greatly involved with business cycles at the time, and it was not emphasized to us when we were doing it. John Tukey certainly was not thinking about business cycles. He was thinking about any kind of important frequency band. We were certainly trying to get away from narrow peaks in the spectrum. We were thinking about important regions of the spectrum. So we were not thinking about pure cycles, which some engineers emphasize. We were thinking about whether or not some band was important. Initially, the work we mostly did involved interest rates, exchange rates, and stock market prices. We certainly looked for a business cycle band and seasonal bands and so on, but we were not specifically looking at the business cycle. And, once we got to the cross-spectrum, then we did look at the business cycle particularly, because we considered leading indicators. One way to decide whether or not the indicator was leading was to look at the effect of the phase diagram around the business cycle frequencies. But, I think the business cycle was not the driving force in that. It was really to see whether the decomposition was going to be useful in some way for interpreting economic data.

So what would you say was the main empirical outcome of your work at this stage?

Well, the typical spectral shape was the first thing that came out. Whenever we did a spectrum it looked sort of the same shape, and I felt that was interesting, but dull.

Your paper on the typical spectral shape was published later, wasn't it? It came out after the book.

Yes, that was because *Econometrica* kept it for four years. After two years, I think, the editor said to me, "It has still not been refereed yet. We think it must be okay, so we will publish it."

This paper created the first stylized fact in spectral analysis. Some authors have been trying to create a second stylized fact by looking at the spectrum of differenced series. Have you seen any of this work?

No. I always felt that the cross-spectrum was more important than the spectrum, because of the typical spectrum shape. Potentially, we are always interested in relationships in economics rather than univariate series, and the cross-spectrum has much richer interpretations. But it turned out, I think, that the cross-spectrum is not that easy to interpret because of the potential feedback in models.

Which connects to issues of causality, a second area where you worked that has had a huge impact on the subject, particularly empirical work. Would you like to tell us about the origins of your work on causality?

It was because of the cross-spectrum. I was trying to interpret the phase diagram. I realized that I needed to know whether or not one series affected the other or whether or not there was a bidirectional relationship. The interpretation of the phase diagram mattered, whether or not there was a one-way relationship or a two-way relationship, so I needed a causality-type definition and test. I attempted to invent such a definition, and was having difficulties in doing that. I had a friend at Nottingham called Andre Gabor, whom I was working with, and his brother was Dennis Gabor, who was at Imperial College and who won the Nobel Prize in physics for holography. A very nice man and a brilliant physicist. I had dinner with him, Dennis Gabor, one night and he said to me that there is a definition of causality in a paper by Norbert Wiener, and he gave me a reference. I looked up this paper and I could not find this definition in the paper. But I had such high respect for Dennis Gabor that I kept reading and reading this paper until eventually I found that there was a definition in it. What was misleading to me was that there was a section of the paper with the word causality in the heading of the section, but the definition was in a later section of the paper. Anyway, the definition there was the one that is now called Granger causality or Granger noncausality. That is what I used in the spectral analysis book to disentangle the bivariate relationship of empirical series and therefore reinterpret the phrase diagram. As I thought that this was an important concept, I published it separately in a journal called *Information and Control*. That article was pretty well ignored, so I published another article in

Econometrica on this definition, which again was ignored, until Chris Sims came along with an application of that definition that was very controversial because he was discussing a relationship between money and income and came out with a conclusion that did not suit some people. Then, a lot of attention was given to the definition. So it was the application that made the definition well known. Part of the defense of the people who did not like the conclusion of Chris Sims's paper was that this was not real causality, this was only Granger causality. So they kept using the phrase Granger causality, everywhere in their writings, which I thought was inefficient, but it made my name very prominent.

Yes, it certainly attracted an enormous amount of attention. How do you feel now about causality? Do you feel that the operational definition that we have is the right one and the one that we should be staying with, or do you have some further thoughts on it now?

I feel that it is still the best pragmatic definition – operational definition. I feel that when we get to a universally accepted definition of causation, if that ever should occur, I imagine that this will be part of it but not necessarily all of it. I think there are more things that need to go in than just this pragmatic part. The philosophers who have been thinking about causation for thousands of years initially did not like this definition very much, but in recent years several books on philosophy have discussed it in a much more positive way, not saying that it is right, but also saying that it is not wrong. I view that as supporting my position that it is probably a component of what eventually will be a definition of causation that is sound. But, all I am worrying about is just a statistical definition that we can go out and apply. Now, whether we use the word causation or not, I do not care much in a sense. It is just a word that I used at that stage, and I used it because Wiener had used it. And, if he can use it, so can I.

It could easily have been predictability.

Yes, exactly.

Are you happy with the mechanisms that people use to test causality? I think that this is surely one of the reasons that it has been so successful, that people can build VAR's and do causality tests on subblocks of the coefficients so easily.

No, I am not happy about it.

What would you like to see people doing?

The definition is a predictability test, not a test of fit, and so the fact that your model fits in-sample does not mean it is going to forecast out of sample. The test that I push is that you actually build in-sample models with or without the possible variable, so you have two models, and then you ask which model actually forecasts the better out of sample, using a comparison of forecasts test. That is a true test of the forecasting ability of the models and the definition is the forecasting definition.

Do you have any recommendations about the forecast horizon to be used and precisely how to mount the test?

Yes, I use a one-step horizon, that is always a problem and you could discuss that, and there is always the cost function. Again, we can use least squares, but that is not necessarily the right cost function. There are several different tests of how to compare forecasts. There is a test that Lehmann suggested that is quite efficient and easy to use. It is in the Granger–Newbold book and there are better versions of that test that have appeared more recently, and are rather more complicated, but there are several tests available to compare forecasts.

That is typically not what people do. People still regularly use VAR's for testing causality.

I have written a couple of papers saying that I do not like that – for example in the *Journal of Economic Dynamics and Control* in 1980 [5–9] – and another on advertising and consumption, with Ashley and Schmalensee – in *Econometrica*, also in 1980 [60]. Perhaps people do not read those parts of my papers.

Hopefully, this will direct attention to that work. Can we now talk about spurious regressions? You mentioned earlier how you spoke about the paper at the LSE and it got a rather hostile reception. How did your thinking emerge on that paper?

That arose just because Paul Newbold was trained by George Box and was an expert in Box–Jenkins techniques. We were just thinking it through. In the Box–Jenkins way of thinking about things and the balancing in equations, you cannot usually have two $I(1)$ variables and the residuals be $I(0)$. So we realized that there could be a problem, that would explain some of the things that we were seeing. We were worried that so many papers were being written in which the Durbin–Watson statistic was not being reported, and if it was reported then it was extremely low. The R^2 was high, Durbin–Watson's were low and we were worried about what that meant. And so we thought that this was an interesting problem and so we tried a Monte Carlo study, a very small Monte Carlo for these days.

But, probably one of the most influential Monte Carlo studies of all time.

It certainly made a structural change in the literature regarding the way people reported their results, anyway.