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THE ET INTERVIEW: PROFESSOR CLIVE GRANGER

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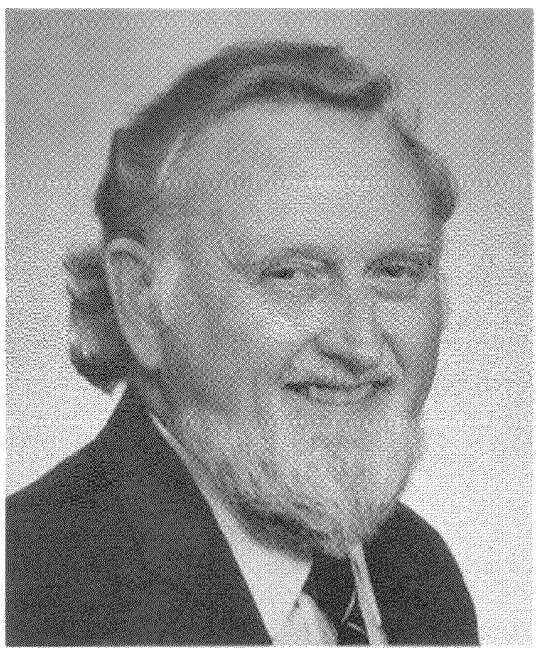


Professor Clive Granger.

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 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.

Clive Granger, Svend Hylleberg, and Rob Engle in La Jolla, California.

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 prophesy.

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 Cramer, 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 Cramer'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 something 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 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

Discussing a blank screen in Hawaii.

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 States. 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.

Yes, there was a big subsequent debate in England about econometric reporting and about the conduct of empirical research that led even-

Carol Kao, Henry Lin, Jesus Gonzalo, Tae Lee, and Clive Granger.

tually to the notion that it was necessary to report an army of diagnostic statistics to accompany each regression. The spurious regression paper gave rise to the alternative idea that if regression equations are in balance, then something must be happening in order to annihilate the integrated nature of the series. The idea of cointegration. Would you like to talk now about this idea and how your thinking evolved in this direction?

That was through a discussion with David Hendry. I do not remember where it took place now, but he was saying that he had a case where he had two $I(1)$ variables, but their difference was $I(0)$, and I said that is not possible, speaking as a theorist. He said he thought it was. So I went away to prove I was right, and I managed to prove that he was right. Once I realized that this was possible, then I immediately saw how it was related to the formulation of an error correction model and their balancing. So, in a sense, all the main results of cointegration came together within a few minutes. I mean, without any proof, at least not any deep proof, I just sort of saw what was needed. The common stochastic trend was an immediate consequence of what I wrote down. That is the basic way of viewing cointegration, and it just came from this discussion. Then I had to go away and prove it. That was another thing. But I could see immediately what the main results were going to be.

To a certain extent, it must have been clear to many people that this balancing of successful regression equations with trending data must hold. Econometricians had long been running regressions in levels that were clearly nonstationary, yet moving together in some general sense, and the residuals from these regressions, when plotted, clearly looked stationary.

Yes. It is one of these things that, once it was pointed out to people, all kinds of other things made sense, and I think that is why it was accepted so readily.

An idea whose time had come essentially.

I think it fit in also with economic theory ideas to some extent, such as equilibrium in macroeconomics. I am not actually pushing the equilibrium interpretation very strongly, but it sort of fits in with the ideas that macroeconomists have. I think macroeconomists at the time were so desperate for something else to do, and cointegration fitted in very well with what they needed. It explained all kinds of different regressions that you saw people getting results for. It is also one of the few things that I have done that is largely uncontroversial. In a sense, it is uncontroversial because people have accepted it uncritically, and I think there are things about it which can be criticized, and I have even tried to do that in some writings. But people have just sort of taken the whole thing and run with it. It certainly has been influential.

It is such an enormously useable apparatus. I believe that Box and Jenkins had a lot to do with this, because prior to Box and Jenkins we just extracted deterministic trends from series by regression and then used conventional time series procedures on what was expected to be stationary residual components. Now, we think about there being an additional element in the data — stochastic trends — and we need to take this component into account in empirical regressions. In a way, this line of thinking would not have emerged if it had not been for the Box–Jenkins emphasis on differencing the data. You noticed that unless something special was going on, regressions with undifferenced data would be spurious.

Right.

This seems to be a case where pinpointing a concept, and naming it, can be very important. It brings together previous thinking in a creative synergy, something that would not otherwise have occurred.

I think you are right. This is a case where the pure time series literature and the pure economics literature came together very nicely to help each other. So, someone brought up on pure econometrics in those days would not have taken over the Box–Jenkins ideas. You needed people who were trained in both areas to see that, to see what they got from bringing them together.

So, if it had not been for the economics community worrying about this issue, how long do you think it would have been before statisticians invented cointegration?

Actually, I am really impressed by how statisticians do invent things before they are applied, so I just do not know the answer to that. I mean, you see all kinds of things, like some of the nonlinear methods, being developed before they are used anywhere. Still I wouldn't be surprised, for example, if statisticians had cointegration in some of their data, but they did not know about it, and they did not look for it.

The Box–Jenkins methodology was essentially univariate, and there had been no successful attempt to extend it to the multivariate case, partly because one simply cannot eyeball matrices of correlations. Because this literature persisted as univariate, I think it was more difficult for the idea of cointegration to emerge from it.

Yes, although I believe it is mentioned in some early papers by Box and others.

Let's talk about long-memory models, which are now a big subject, especially with regard to stochastic volatility modeling. You wrote an important paper with Roselyn Joyeux on long-memory models in 1980. There has been a tremendous amount of subsequent work. Would you like to talk about some of the developments?

I think that is a fairly interesting class of models. And, for a long time, I did not work on the area after the first paper or two, because I did not think there were any good examples in economics. Some people suggested they had found examples, but I was not convinced. But, recently we have gotten some extremely good examples for long memory by using daily returns from speculative markets and they have significantly positive autocorrelations at lags up to 2,000, very clear evidence, unbelievable. And we find this for many different speculative markets, commodity markets, stock markets, interest rates, and so on. There are obviously a number of different models, all of which produce this same long-memory property. And what I would hope to see next is some more general discussion about alternative models that have this property and then how to distinguish between them. We should not just say that, because the data has a certain property and the model has a certain property that the model therefore generated that data. We can often find several models that have the same property. So I suspect that the next stage in this area is going to be comparing alternative models, not just fractionally integrated models, which are the ones that statisticians mostly look at right now. I think some of the other models might actually be more interesting and make more economic sense than the fractionally integrated model.

What models do you have in mind?

Well, there are some switching regime models that will do it. Some duration models that will do it. William Park at North Carolina has a duration model in which you have shocks coming from a certain distribution, and when they occur they have a duration attached to them. So they persist for a number of time periods. You can get for a particular survival probability distribution some long-memory processes from these duration models. So it is like having a news sequence to the stock market, but some news lasts only a few hours. Other news lasts several days. And some news may last a month. And if you have the right distribution for the length of the survival of the news, you can actually get a long-memory process out of it. It is a nice paper.

Yes, I have seen it. It is a cute idea. Again, it depends on a statistical artifact. Here there is a heavy-tailed distribution generating regimes that can have very long duration and these are the source of the long memory. It certainly is another way of looking at it. Your original idea was to use aggregation.

Yes.

Economics has yet to contribute any useful ideas to the formulation of models that might be appropriate for long memory in stochastic volatility. I think we would benefit enormously if there were some relevant economic model.

I agree totally. One of the differences between models is that forecasting will be different. I would like to have a theory if possible.

A purely statistical theory that relies on the arrival of heavy-tailed shocks in determining duration and consequently memory may not be very useful in practice, say, in forecasting.

Well, it might be possible to look at how many shocks there were in the last period. I have done it for the MA(1) case and have found there is a slight improvement in forecastability. You can sometimes work out whether there had been a shock that had lived in the previous time period and work out whether another shock could have lived this time. You get a different forecasting situation.

Another of your major interests over the years has been nonlinearity, which, again, is now a very active field. Some economists, for instance, have been working on deterministic nonlinear models, and you have worked a lot on bilinear stochastic models. How do you see this subject emerging in the next few years?

I think bilinear models are not going to have much future. I do not see much evidence of them helping forecasting, for example. Bilinear modeling is a nice example of a way to generate nonlinearity, where we can also work the mathematics out. I think that it is a good example for classrooms and textbooks. Bilinearity is also useful in working out the powers of certain tests of nonlinearity and linearity, because many tests of linearity have bad power against it, so it turns out. So it gives an example of the limitations of certain tests of linearity. I happen to be a strong disbeliever of chaos in economics, I should add. I have never seen any evidence of chaos occurring in economics.

But I think there has been a great deal of misinterpretation of the evidence they think they have for chaos. I believe that there is a lot of nonlinearity in economics at the microlevel, but I do not see that we get much of that left at the macrolevel, after aggregation, temporal, and cross-sectional. There is not much nonlinearity after aggregation. I think we should look for nonlinearities and am pleased we do that. I do not myself have much hope in achieving very much using aggregate macro-nonlinear models. Interest rates, I think, are an exception, because these are already a strict aggregate. We can find nonlinearities there. I expect the area where we are going to have the most nonlinearity is in financial data. There we have a good chance of picking up something interesting.

We now have a proliferation of different approaches in modeling nonlinearity. We have partial linear models, semiparametric models, general nonparametrics, neural net models, and wavelet methods, just to mention a few. Do you have any instincts as to which of these are ultimately going to be the most valuable?

My impression is that they probably are going to fit data quite accurately but not necessarily going to be of much help in postsample work. I keep going back to the methodology question of evaluating a model by its postsample properties. I have only run one very small experiment, so I am giving too much weight to my own personal experience. We generated some data with nonlinearities and the simple models picked them up very well and then forecast quite nicely out of sample. But then we ran some data with no nonlinearities in it and still found a similar pattern, that is, the method overfitted in-sample and then forecast very badly out of sample. So my worry is that simple measures of fit are not going to be a very good way to judge whether or not these methods are finding anything in-sample, and thus postsample evaluation is going to be critical to these techniques. Hal White had convinced me from some experiments he has done that the neural net is a very good way to approximate the nonlinearity in-sample. The problem is that we cannot interpret any of the coefficients we get from any of these models. There is no relationship to any economic theory from those coefficients to anything that we started with. I think what they are mostly useful for is saying, yes, there is some nonlinearity in the data that appears to be of this type. We should go back and work out where is it coming from, what does it mean, and so forth.

Overfitting has always been a problem. One can get a model that looks very good within sample, but when you try to project with it the good sample period fit affects forecasts in a deleterious way. Overfitting also affects inference, because of the downward bias in the regression error sum of squares. Time series analysts believe that you need good model determination methods in order to resolve these issues. In this case, the nonlinear factors have to perform well enough to warrant their inclusion in the model. We need mechanisms like these to narrow down a wide class of possible alternatives. Do you think that these methods should be used systematically in nonlinear models?

I have not seen many examples yet. If people are finding good forecasting results in the stock market, they would not actually be showing them to me, so I do not know quite whether I am getting a biased sample of evidence.

If they are successful, we will know about it eventually.

Yes. We have got to do it anyway, so whatever the arguments are, it is important to use these techniques on our data to see what we find.

How far do you think we have come with regard to economic forecasting in the last two decades?

I suspect that we have not improved very much. It is possible that the economy itself has got slightly more difficult to forecast, and it is possible that economic data have deteriorated in quality too. So, if the measurement error has increased, then our forecasts are going to deteriorate. I think we can probably look at certain areas and say we may be better at forecasting inflation than we were or something like that, so there are some areas where we have improved. But other areas are extremely difficult to forecast, like interest rates. I think that we are not using the evidence from a cross-section of forecasters effectively. That is, if there are lots of forecasters using different techniques, and they are all doing equally badly, or equally well, that tells us something. I do not know how to phrase that correctly, but, somehow, if there are people who are basing their forecasts totally on economic theory and others are basing their forecasts totally on ad hoc methods and other people are basing their forecasts on Box–Jenkins methods, and they are all presenting forecasts of equal quality, not necessarily the same forecasts, that tells us something about the economy, and we have never used that information properly, in my opinion. Not only could we combine these in some way, but maybe there is something to learn about the economy from the success or otherwise of different techniques being applied to the data set.

We do know that all of the models that we are using are wrong. So, if a group of forecasts from very different models and heuristic methods are all very similar, then it suggests that all those procedures may be missing out much the same thing, perhaps some critically important data. In this case, there may be some hope of improving forecasts by bringing in new data.

Yes, it is always possible.

Like continuous data recording of some form, or the pooling of micro and macro data.

Sure. Yes, perhaps replacing series that are explanatory series but slow in being recorded with other series that are more frequently recorded. For example, rather than wait for exports that come in two months late, use some customs data that are available weekly or something. There are things like this that you can do to improve the quality of forecasts, but I also think there are a certain amount of things we are missing in improving quality. I do not see at the moment where there is a big breakthrough to be made. I think we can improve forecasts by doing small things, but at the moment I do not see how to take a big step.

Your idea of combining forecasts has met with pretty much uniform approval. It seems to work because if both models are wrong some convex combination of them may do a little better. Do you have more thoughts on this, where this idea may be taking us?

Not really. Usually, using optimum weights does not much improve over using suboptimum weights on that combination. We tried using some nonlinear combinations and we did not find that that helped much either. I am surprised people have not worried more about multistep forecast combining. Almost all the work has been on one-step forecasts. The other thing I would like to see done, and I have not tried this myself, is to combine forecast intervals. If people provide forecast intervals, how do you combine those to get a combined forecast interval?

Modeling trends is also important in this context, and you have written on this topic also. Would you like to share some of your thoughts with us on this subject?

It has always intrigued me that for many macroseries the trend is clearly the most important component of the series, even though we do not know how to define trend, strictly speaking. But it is also the component that we analyze the least. For a long time we just stuck in a linear trend or some simplistic deterministic trend, and I have always felt that this was a very rich, unexplored area of time series modeling. In a sense, you have got to get that bit right to get the rest of the modeling right, because misspecification at one point always leads to overanalysis of other points. I think one can have quite complicated, say, nonlinear, stochastic trends but that is a very underdeveloped area still. I believe this is an area that will be developed in the next few years.

Yes, the trend-generating mechanism of accumulating shocks is essentially a linear operation. One can envisage some richly interesting alternative possibilities, but finding the right mathematics to do the analysis is always a major obstacle, isn't it?

Well, the growth processes in stochastic process theory are fairly well developed and they are quite general.

Have demographers or others been working on nonlinear stochastic mechanisms for growth do you know?

I think they have rather specific models.

Trending mechanisms in economics that are presently used seem to be so simplistic. Ultimately they boil down to the accumulation of technology shocks and demographic changes, don't they?

Yes.

So we have an impoverished class of economic models and econometric models to address trends. This is an irony given that, as you said, the trend is the most important visible feature of the series.

Exactly.

Over your career you have worked on a huge variety of topics. Is there any topic that you have not yet worked on that you would like to work on in the future?

The area that I want to think about is the whole evaluation process—both in econometrics and in economics generally. I want to know how people evaluate the theory and how people evaluate a technique and how to value the model. And I suspect that we are going to need to do that properly to understand whether we are making progress, whether we are developing in the right direction at all. I think that a lot of literature is losing the viewpoint that we are here to learn about the actual economy. They are playing games when they write papers. I would like to see consideration of what the objective of a piece of work is and then a statement about whether that objective has been achieved. By stating an objective, you would eventually be able to evaluate whether that objective has been reached or not in a paper, or in the model. We just need to have more thought about proper evaluation procedures, which may be tied to questions about utility functions and cost functions and many other things. There are a lot of threads out there that can be drawn together into a more general way of thinking about evaluation.

There is an accountability issue here, isn't there, about one's own personal research and that of a community of researchers. In some respects it was resolved under older regimes like the one that you were involved in at Princeton and in the early Cowles Commission program of research. There, a research director took responsibility for a research program, ensured that it was kept on track, and attracted people in to pursue a certain set of objectives and research goals. That does not seem to happen so much these days, does it?

I agree.

So do you think that this organizational feature is a factor in the problems that we have been talking about?

Absolutely. And, in fact, I go beyond that. I wish that the economics profession would be challenged to solve a real problem occasionally. The analogy I would use is how Kennedy challenged the scientific community to put an American on the moon by a certain date at a certain cost. That was a specific challenge to solve all kinds of problems like control theory problems and rocket theory problems and so forth, to be done within specific time and cost constraints. And they achieved it. Now, there was enormous benefit to the economy and the world of technology from so doing. I do not know whether the economic profession could succeed in such a challenge. Someone should think of an appropriate challenge to the economics profession. For example, although I am not actually pushing this as the right question, to lower black teenage unemployment by 20% in the next eight years. One might ask how that should be and could be achieved. That is a question that the economics profession, and perhaps sociologists, could get together and reach a conclusion about. In other words, ask, can we solve a problem that is a major problem in American society? If we cannot do it, then that is a reflection on the quality of the economics profession. Whatever the outcome, that would be an interesting project for the whole profession.

This is a fascinating thought. When Kennedy put forward the challenge to the nation to reach the moon by 1970 it had enormous positive externalities to society, even though the cost was gigantic. As I see it, the leaders of the profession have to bear this responsibility. Do you have any thoughts about mobilizing the leaders or elders of the profession to give some directive such as the one you have just mentioned?

No, I do not have any hope that they have any interest in doing that. I suspect that they are fully satisfied with the present situation. I do not see why they would find it relevant to enter such a hazardous procedure.

The democratic world may be governed by majority, but it is pushed along by minorities. In the past, there have been visionary people in our intellectual community, like Ragnar Frisch, who set forth challenges to the profession and moved us forward in entrepreneurial ways.

Global warming is an example now of this type of challenge, but the challenge is not specific. There is a lot of money being put into research on global warming, but there is no problem to be solved that we are being given. We have simply been told to do research on global warming.

Econometrics, as we all know, has now grown into an enormous subject, reaching into every field of economics and assimilating advances in computing capability in extraordinary ways. This makes designing a teaching program and a course sequence in econometrics complex and difficult. How have you been meeting this challenge at San Diego?

At the undergraduate level we have not done anything special. Our students are all quite well trained in mathematics and computing, so we just get them to do a number of statistics courses that we call econometrics, which includes one hands-on practical course. These courses are not compulsory, but they are strongly advised. At the graduate level, we have five compulsory econometrics courses, although one of those is linear algebra, which is a necessary background course. And then we have two or three advanced econometrics electives, so a student who wants to concentrate on econometrics has a lot of courses that he or she can take over a period of two or three years. Even then, we do not cover all the topics by any means. It depends on who is available and what we are willing to teach that year.

Do you have any mechanism of ensuring that the fundamentals are covered in the mainline courses, say, by laying out a core curriculum?

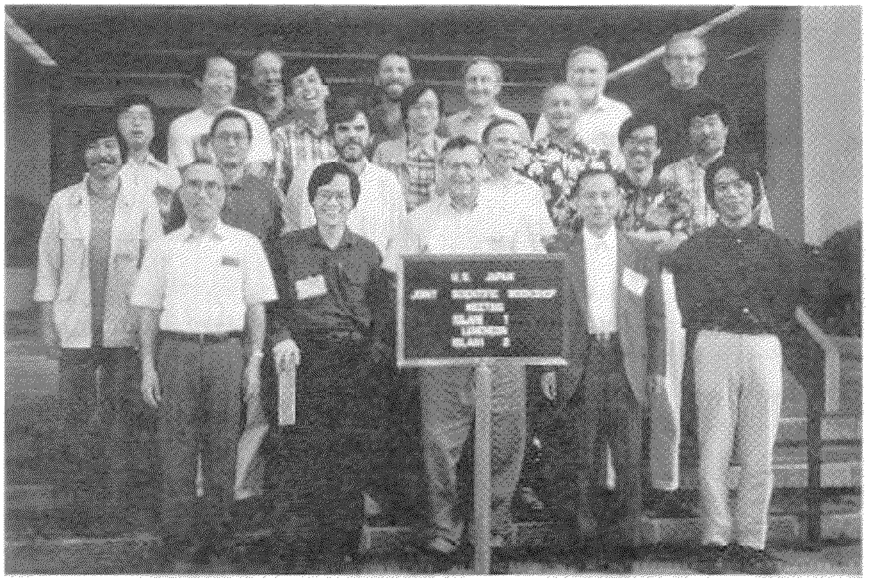
Yes, the five compulsory courses include linear algebra and then a basic course in probability and statistics. Then there are three rather standard econometrics courses: first of all, regression and then simultaneous equations; then, an asymptotics course that Hal White gives; an inference course; and then I or Rob Engle do a time series course. Right now, every student has to do an empirical project and around that there are some lectures given by someone who does microeconometrics. There is thought of another elective just on microeconometrics. This is not currently being offered but it is planned.

Do you have any general philosophy about the education of graduate students in econometrics?

We are so busy teaching them techniques and proofs that they do not really see enough applications and data. Many of them would benefit, I suspect, by working with the techniques and with empirical applications of the techniques. I think they learn an awful lot from doing it at the same time. We do not do enough of it.

What are your own favorite courses to teach and how do you integrate this type of philosophy into your own teaching?

Well, one of my undergraduate courses is on business and economic forecasting. We talk about leading indicators and the recent forecasts of the economy that have come from macromodels and so on, so I just discuss topical things with them. I also make then do a real-time forecasting exercise, so that at the beginning of the quarter they start to forecast something for the end of the quarter, of their own choice, and at the end of the quarter they can compare how well their forecasts correspond with what actually happened.



U.S.-Japan Joint Seminar on Time Series Analysis, Honolulu, Hawaii, January 24-29, 1993: first row—Masanori Okamoto, Ruey Tsay, Emanuel Parzen, Mituaki Huzii, Yuzo Hosoya; second row—Makio Ishiguro, Katsuto Tanaka, Joseph Newton, David Brillinger, Genshiro Kitagata, Yosihiko Ogata; third row—Naoto Kunitomo, Tohru Ozaki, Yoshihiro Yajima, Masanobu Taniguchi, Will Gersch; fourth row—Robert Shumway, David Stoffer, Peter Brockwell, Clive Granger, David Findley; missing—George Tiao.

They actually do learn from performing a real forecasting exercise. I may say they get more from that than they get from the course, actually, because they are forced to think about actual forecasting. Another course I do is on economic demographics and population growth, which I do as an applied econometrics course. The students really enjoy that because there are a lot of surprising facts about the size and growth of populations, interaction between different aspects in populations, and so on. I discuss a number of recent empirical studies relating population and economics, the effects of birth rates on wage rates, and such. There are all kinds of interrelationships that you can find, and there are some nice examples to talk about. I do not give that course every year, just occasionally. At the graduate level, I teach an advanced time series course, but I do not do much practical work in it. I discuss certain results, but there is not enough time to do more. Ten weeks is not really enough to cover all macro-time series analysis these days. And then I do some advanced topics courses, but they vary from year to year, depending on what I have been working on recently, and so the topic can change. I do not do as much for graduate students in practical terms as I would like to.

There is a lesson from your undergraduate teaching experience that students, and maybe researchers too, often perform better and learn more when their backs are against the wall and they have to deliver. In the case of your undergraduate course, this was by means of enforced practical forecasting experience.

They also think that they understand something, but when they have to actually do it, they realize that they do not understand it as well as they thought they did, and that may make them understand it better.

It is one thing to say that you can get a man to the moon; it is an entirely different matter to accomplish it.

Exactly, yes.

That leaves us with thesis advising. You have done a great deal of advising since you have been at San Diego. Can you tell us about that experience?

Yes, we have a good steady flow of pretty good students and some very good students, and I enjoy most of the supervision. I learned something from Oscar Morgenstern when I was at Princeton. I see every one of my students every week at a regular time for a short period of 20 minutes or half an hour. This way it forces them to come in and show me what they have done over the last week. If they have done nothing, they have to come in and say that they have done nothing. Most of them are too embarrassed to say that, so they always have done something. That is just good training for them, to have done something every week, despite all of the other work that they have to do, and just keep on producing. This is the key element that accumulates to something worthwhile at the end of the quarter, and, on the whole, they like that. Also, the fact that I limit their time means that they make best use of it, so that they come prepared usually and have some questions and they do not want to miss the meeting and so on. It is a discipline on them and on me. I think they benefit more than by having a sort of vague arrangement of drop-in-when-you-want-to type of statement. I do give them things to read before the meeting sometimes, and we discuss things. And, of course, if they give me papers I will read them. And then, occasionally, for some special occasion, I will see them other times.

That sounds like a very workable system, similar to the one that I use. I believe thesis students really do need structure. It is no good saying you are going to see them from time to time and leaving it to them to call in and report.

Exactly.

What general advice do you have for graduate students coming into economics and thinking about doing econometrics?

I am firm in the belief that they must remember that the objective of all these exercises is to learn about the actual economy. And that they should study real data along with the theory. I have always found I learn a lot from looking at data and trying methods out on data. Again, when a method that should work doesn't, you have to ask why it did not work. So you go back and find out why and sometimes that improves the method a lot. It is this interaction between theory and application that I think generates better research.

This is the keep-your-feet-on-the-ground part of their education.

Yes.

What about reading? You have mentioned books that were important to you. Do you think that it is important to make sure that gaps in students' learning are filled by pointing them to literature that they may not yet have read?

Yes, I am always doing that. I am trying to keep up with journal articles and books that I am aware of and I am always lending my books out to graduate students. I think one of the main jobs of a supervisor is to be aware of what students should be reading, because they have little idea where to start. The whole search procedure to them is just a mass of material, whereas we on the whole are experienced enough to know how to filter that material out into the good and the not-so-good. I spend quite a lot of time looking up things and being aware of good papers and interesting articles and books, and then telling my students about them.

Do you want to talk about books that have influenced you, like the Davis book?

Yes. The reason I first did work in time series analysis was that I had decided to do a Ph.D. in the area of statistics that was going to be relevant for economics. As I knew just enough that most economic data was time series back in the mid-1950's, I went to the library and there was only one book there on economic times series. This was by H.T. Davis, *A Course on Economic Times Series*. It is a Cowles Commission book. From this fact, that this was the only book, I knew that there must be something that I could do, as it seemed to be a wide open field. Davis had written in 1942 or thereabouts, the early 1940's. That was why I choose the area of economic time series to study. Now it turned out that the Davis book was a very advanced book for the early 1940's. It had the beginning of spectral analysis in it for one thing, plus some quite interesting work on autocorrelations. It was an excellent book for its age and one that does not get sufficient credit. So that certainly was important to me in those days. I mean, then there were just a few econometrics books out and most had no time series in them. It was certainly before Box and Jenkins, so there really was not a great deal else to read at that time.

There was the book by Maurice Bartlett on stochastic processes. This gives a very readable introduction to stochastic processes but, in fact, is mainly about time series. Apparently, this book had a major impact on the education of many British statisticians and did so even prior to its publication because Bartlett's notes were widely available in Britain long before the book appeared in 1955. There was another time series book, by Quenouille, called *Multiple Time Series*, that appeared around 1958. It was a short book, but had a great deal in it.

Yes, I thought that was way advanced, well ahead of its time, as well. That was before Box and Jenkins, and it had a lot of material on multivariate time series. It was fascinating. It also had the jackknife in it, but it was not called by that name.

The next important book on time series was a little book that had a big impact—Peter Whittle's book on prediction and regulation.

That was an interesting book, but annoying because he would get to a certain point and then would not go to the next logical step. So, for example, in his book, he never proved that one-step forecast errors were white noise, which strikes me as one of the major properties of optimum forecasts.

That raises the big question of what models you should be using for multiperiod forecasts. Do you have any thoughts on that? Whether you should be using different models for multiperiod ahead forecasts from one-period ahead forecasts?

I have come to the conclusion that for nonlinear models, we should build different models for each horizon. I do not see building a one-step nonlinear model and then trying to iterate that out to multistage, because you will just have misspecified the models. Even if you do not misspecify the multistep model, forecasts are difficult to find, but with the misspecified one-step model you know you are going to get awful forecasts in the multistep. So, I would rather build a different nonlinear model for each step, and then use these to forecast. The linear case is less clear, whether there is an advantage to building one-step and then just keep iterating out or whether to build different models for each one. I do not really hold a position on that.

I have found in some of my practical work that if you use model selection in multiperiod ahead forecasts you might include some trends or lags that you would not have included if you were just looking one-period ahead. Coming back to Peter Whittle's book, I wonder whether he felt that just doing the Wold decomposition was enough to make the point about one-step forecast errors being uncorrelated.

Yes, I suppose so.

Let's move on to talk about some other matters. Would you like to describe a typical working day scenario to us? Do you work at home, in the office, or both? How you carry your work back and forth, that sort of thing?

I live very near campus, but I drive in. I go into work fairly early, by eight usually, and I do my best work in the morning on campus. I am more awake in the mornings than in the afternoons. I always have a rest after lunch, about half an hour, something again I learned from Oscar Morgenstern. In the afternoon, I usually do some sort of office work or administration, whatever I need to do. And I try to put an hour aside to do some sort of exercise, so I go for a walk in the winter in the park or on the beach. In summer, I body surf, not every day, but most days. In the evenings I do a couple of hours of refereeing or just reading things or papers that people have given me to look at, not deep work, but things that have to be done. I am in the habit of never working on a Saturday. I feel that I need one day a week that I do not think about anything academic. On Sundays, I do not have any real sort of regular agenda. I often do things on Sunday, but I usually work at home.

What form does your rest take at lunchtime? You said that Morgenstern set an example there for you.

I actually have a chair that I can sleep in, a tip-back chair.

Do you turn the telephone off?

Yes, and a sign on my door saying, "Go away," or something.

Can you nap for an hour?

Half an hour, yes. Twenty minutes sometimes.

Winston Churchill used to do that.

I know. I once asked a doctor about it, and he said it is a good idea. If your body says in needs it, then do it. And I really feel a lot better in the afternoon by having done it. I do not see any social reason why we should not do it. If other people let you do it, then do it.

We have covered a lot of ground already. But I think there are one or two important matters left. For instance, what directions would you like to see the econometrics profession take in the next decade?

It is clear that panel data work is going to become more and more important and then I think it will bring together different parts of present-day econometrics in a useful way. We need to rethink the way we are analyzing panels now to incorporate the new panels which have long time series aspects to them. I think that would be exciting. I would like to see much more use of relevant economic theory in model building. Some of this we have been talking about in the econometrics profession for thirty years. I am worried by the fact that the Econometric Society includes both economic theorists and econometricians and that in their meetings the two sides never talk—or virtually never talk. There are very few joint sessions involving both theorists and econometricians. The econometricians keep asking for fully formulated dynamic models, and we do not get them properly specified and so we are forced to go back to building our models using ad hoc techniques. There is plenty to do in terms of improving the quality of the research and the quality of the models in econometrics. Whether that will happen, I am not so sure.

I wonder if the NSF could devise some scheme whereby, instead of giving individual researchers money, they went back to a system in which they supported a group of researchers and insisted on there being complementarity in the group so that it involved both theorists and econometricians. These groups would have to design a coherent research program that would be evaluated for support. If there was enough money involved, then presumably there would be sufficient incentive for research teams to get together, much the same way as the original Cowles Commission researchers did or the Princeton team did, to design longer term projects that set some real goals and challenges in economics and econometrics and force some interaction between theorists and econometricians.

I think that would be a good idea. There has been a slight move in the direction of trying to bring econometrics groups and statistics groups together. I am not sure how successful that has been. We tried to put in for some of these grants, but they were trying to solve multiple problems. They were not for specific problems, they were for large problem areas really. And they also wanted a lot of teaching and conferences organized and efforts to include minorities, so there were a lot of other things involved—baggage in a sense—going along with the general idea of a team work. They did not seem quite ready to specify closely enough what was wanted.

The NBER has tried over many years to sponsor activities that are of a much more specific nature and, in doing so, has brought together smaller groups of researchers for conferences. But I do not think that you see much of this carrying over into research programs, which is what happened in earlier eras like that at Princeton, Yale, and Chicago. I think that the profession as a whole would benefit greatly from this.

Yes, so do I.

Because Americans often respond best to financial incentives, it seems to me that the only way to get this going in North America is to organize it formally through the funding institutions, like the NSF.

It is happening a little bit in Europe. There are a couple of places that have groups studying nonlinear methods—for example, at Cambridge and at Aarhus. They have visitors come for periods to link in with the local people. Whether it is going to work, I do not know, but there are attempts to have a common interest. But, again, I think these could be focused more sharply. Nonlinearity is still too wide an area.

Looking back over all of your own work, what personal favorites do you have among your papers and the topics that you have written on?

I am going to mention two that we have not talked about, which I really enjoyed doing. One was on aggregation, an area where I have always found results that were both surprising and easy to get, which is a combination that I really appreciate. One paper appeared in *ET*. The results suggested that micromodeling could miss a feature that, when you aggregate up to the macrolevel, became the dominant feature, and this could explain why sometimes the microtheory did not help the macrotheory. That work has been rather ignored until recently, and there are now workers in Italy and Belgium using it. It was just one of these papers where you get the idea, and you can sit down and write it in a few days, really enjoy doing it, and then it gets published. The other area I enjoyed working on was some years ago at Nottingham, where we did some pricing research in supermarkets. We actually got permission in some supermarkets to change the prices on the shelves of certain goods and observed how demand changed. We could plot demand curves of these commodities and then fit various functions to these demand curves. What I enjoyed about it was that generating your own data somehow is more interesting than just reading data that comes off a computer tape, because you really see how it was done and what the problems were. You knew everything about that data set. I can recommend to people occasionally that they consider just going and asking someone for data or find their own data set. I feel that economists are not inclined to do that now. Economists do not ever think in terms of that type of real-life experiment. I am not sure that is true, but that is my impression.

They had a similar scheme at Essex University in the late 1960's and early 1970's in the university shop. The economists came in and were allowed to set up a differential pricing scheme according to the time of day, to try to encourage people not to shop at rush hour. Apparently, the consumers hated it because of the time delays in purchasing. Perhaps, it would work better now in these days of scanners and computerized pricing.

We also once helped with an electricity time-of-day pricing scheme. It was an experiment done on the East Coast, but we were involved in analyzing the data, and that was also interesting. That was where they were trying to increase off-load demand for electricity by changing the time-of-day price of electricity. That worked very well, and people did not seem to mind that particularly. Because you could heat your pool at off-peak hours and there were no problems essentially, just maybe some planning. Yes, I think it is rather fun organizing a survey and organizing experiments sometimes.

They do this extensively in many of the other social sciences.

There are other experimental economists who do experiments within labs, which is also interesting work, but I feel that the real world is also particularly good for experiments. It is not all that expensive to do. You just need to go out and find someone who will let you do it.

Just a thought about the presentation of data. Have you thought of other ways of presenting data rather than simply as graphical time series? Some thoughts come to mind that include audio as well as visual effects and color video graphics. With modern computing technology, some of these possibilities present themselves and I think that, on the whole, as a profession, we are generally a little bit behind the times in this respect. Do you have any thoughts on this?

Certainly in time varying parameter models we could make some good video images. No, I have not done it.

Some experimental economists have been using audio effects to reveal data, as well as visual imaging. As market trades occur and prices go up, they change the pitch of the sound that records the transaction. It is really remarkable to me that the ear can pick up changes, subtle changes, that you might not notice so much on a graph.

No, I have not done that.

One final question. What is there left to accomplish after having done so much and having written so many papers over the years?

I have not got many plans. I have got a couple of books I want to write, but they are in their early days yet. I am currently working on a project modeling the deforestation process in the Amazon, based on a large panel data set. That has been very interesting because the data set, although it is mostly of good quality, still has lots of problems. It is inherently difficult to measure anything in a place like the Amazon, and this is on fairly large areas. So we have to do all kinds of robust estimation. I really learned quite a bit about data handling from that project, and we are applying for some more funds to continue work on this. If successful, that will be my project in the next couple of years, and then once I have started on panels I might as well continue on them as that is a worthwhile new area for me.

Thank you very much, Clive.

Thank you.

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