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ET INTERVIEWS: PROFESSOR J. D. SARGAN

interviewed by Peter C. B. Phillips

JOHN DENIS SARGAN

For more than twenty years, Denis Sargan has been the central pillar of the econometrics community in Britain. His conceptual advances and technical skills have placed him at the forefront of research endeavor in a wide range of fields. His contributions to estimation, testing, time series, nonlinear modeling, finite sample theory, and identification have helped to build the framework of modern econometrics. His output of successful doctoral students, now approaching 40, has no parallel in the subject in Britain. Successive waves of young econometricians, both within and without the London School of Economics (LSE), have been influenced by his work and stimulated by his example. The energy and vitality of British econometrics owes much to his presence.

During March 19–21, 1984, the SSRC Econometric Study Group held a conference in Denis Sargan's honor at Oxford University. This conference was an opportunity for the British econometrics community to thank Denis Sargan publicly for his many contributions. The occasion was his retirement from the distinguished Tooke chair of political economy at the LSE. It was a time for scientific contributions and personal reminiscences. No one present on this occasion will forget Denis Sargan's after-dinner speech, in which one quickly sensed the personal humility and kindness that have endeared generations of students and colleagues to him, nor will it be forgotten how he ended up by enjoining his audience to "do their own thing" in econometrics.

The week before the Oxford Conference on March 14, 1984, I met Denis Sargan in his office at the London School of Economics. We talked about his work, his teaching, the influences on his research, his intellectual background, and his more general views on the subject of econometrics. The interview published here is a transcript of our conversation. Through this interview we hope to bring Denis Sargan closer to a wider audience, beyond those who have been privileged to know him in their careers and those who had the opportunity to be at the SSRC conference in Oxford.

Would you like to tell us about your early schooling; for example, where you attended school, what subjects you preferred in those days and whether you had an aptitude for mathematics?

The school I attended from the high school stage onward was Doncaster Grammar School, in a fairly small town and I took the general courses until I was fifteen and did quite well in them. At that stage I had to decide whether to go on to the arts or the science side, and I decided to go on the science side. Up to that stage I had no particular feelings for mathematics as a subject, but there was a very good schoolmaster who would push me along to get a scholarship at Cambridge. Certainly until that stage I had had no thoughts of working in that field, but he was an inspiring teacher and he got me to the stage that I felt I could obtain the required scholarship to Cambridge. In fact I did complete these training years rather quickly, taking two years rather than three, and he was anxious that I move as quickly as possible because of the onset of the 1939–45 war.

So you then went to Cambridge and studied mathematics for two or three years?

Yes, I went to Cambridge in 1941 and then again because of war time practices I was allowed two years at the University before being called up to war activities. At that stage I had thought of taking other subjects than mathematics, perhaps engineering, but I was persuaded to stay in mathematics. There were very few options in the choice of courses, and I took what was presented to me. The only special course that I took during that period was statistics, which was incidentally, a course given by Harold Jeffreys on Bayesian probability. There were very few people taking those courses, and he wasn't a very inspiring teacher which perhaps explains why I didn't become a Bayesian probabilist at that stage.

I think you told me at one time that one of the other people in the class was Lindley.

Yes, there were three people taking the course who were regarded as being serious students. One was an Indian student whose name I've forgotten and the other was Lindley. He of course stayed with Bayesian statistics in a way which I haven't. But I've always been interested in the concept of Bayesian statistics and always thought of it as having some strong elements, although I don't feel convinced it has a monopoly on good ways of doing things. Yet I think it's a very interesting way of looking at statistical problems.

Was there another field of mathematics that you found interesting at Cambridge?

At that stage I was more interested perhaps in the applied side of mathematics, what was called natural philosophy at Cambridge, and I had the idea that I would perhaps be working in theoretical physics ultimately. But of course knowing that in two years' time I would be going off to wartime activities, I wasn't, at that stage, making any decisions about what I would be likely to be doing. It was a peculiar time at Cambridge because most of the more interesting mathematicians had been removed for war work. In particular, Professor Newman, who became professor at Manchester later, was the most interesting among my mathematics teachers. But he was removed at the end of my first year there, and I didn't find my teachers particularly attractive in their presentation. So at that stage I was quite undecided about what I intended to do.

Would you like to tell us about your war work and whether it was in any way connected with intellectual activity?

I was drafted into a junior scientific officership in the Ministry of Science, and that actually meant that I was attached to the air force and concerned with the interpretation of tests of new weapons as a civil servant. In fact the activity was not very intellectual. It involved going on submarines, bombers, or search aircraft concerned with detecting submarines and the development of radar and weapon systems of various kinds. My presence there was in order to help with scientific design and statistical analysis, and though the work was interesting it was not very decisive in any sense. The weapons had usually been developed by the time we saw them, and the testing that we did was more of a concession to the operational personnel who were going to use them, so that they would give their advice on how they should be used rather than concerned with the development of the weapons themselves.

So was it a mixture of both office work and field work?
Most of the war I spent on remote airfields where these tests were taking place in the field. We had some tame submarines and torpedo boats. We were concerned, for example, with the testing of the radar range, and the statistics required were very elementary, more a matter of giving a standard error to an arithmetic mean, and a little bit of regression.

After the war you got into economics and econometrics. Can you tell us how that transition was accomplished?
This was really a matter of chance. I went into a bookshop and found a copy of Keynes's general theory. In my general reading before that, I had read Marx's *Das Kapital* and some textbook on economics whose author I've forgotten, and decided on the whole that economics was rather a dismal science. But reading Keynes made me more optimistic about what economics could accomplish, and also the amount of mathematics that it contained made me realize that there might be a possibility of doing interesting work of a mathematical kind applied to economics. I had some background in statistics at that stage and this was enough to make me decide when I went back to Cambridge after my period in the civil service in 1946 that I would take courses in statistics, particularly time series given by Bartlett, and I would also sample some courses in economics. In the course of that first year back in Cambridge I decided I would stay a second year, taking advantage of the regulations that allowed a degree in economics to be taken in one year if a previous B.A. had been obtained, using the second year to study for a B.A. in Economics. I made contact with people in the Department of Applied Economics in that year, but at a lowly level. I took part in some seminars. I knew Jim Durbin who had followed a course similar to mine and was working in the Department of Applied Economics at that time. I was aware of the work they were doing. As a consequence of these contacts and my general reading of the literature on time series and its applications, I decided I would do some work in this field.

There were a few Americans who visited the statistical laboratories and the Applied Economics Department at Cambridge in those years: Guy Orcutt and Geoffrey Watson, for example. Did you ever get to meet them during your time there?
No, I think I may have had an introduction but I did not talk with them to any extent. Guy Orcutt was rather senior to me at the time; Geoffrey Watson and Jim Durbin were more or less contemporary. Jim Durbin and I were at the same college at Cambridge and were social acquaintances, but did not interact very much. At the time I was very busy trying to get a degree. I had

a good deal of reading to do, not only in economics but also mathematical economics. This was the time when Von Neumann and Morgenstern published their book on the *Theory of Games*. I was reading around things of this sort and not specifically working on the statistical theory side; I was really absorbing some economics at that stage.

At what stage did you encounter the Cowles Commission work, and the Koopmans' Volume 10, and what influence did this encounter have on your own research direction in the 1950s?

Yes, this was very important. In 1948 I went to the University of Leeds, and I started to teach rather old-fashioned courses in economic statistics. At that stage I had been impressed by the work of Tinbergen for the League of Nations in 1938, and I was myself interested in getting together a rather simple model of the Klein 1 variety for the U.K. but the problems that I faced were those of inadequate data. The Department of Applied Economics at Cambridge were reestimating most of the social accounting time series for the U.K. in the interwar period, and even if one estimated models on the prewar data, they would soon be made obsolete as soon as the Department of Applied Economics published their reestimates of these time series. At that stage I also became interested in the elementary methods of estimation and stumbled upon the method of instrumental variables as a general approach. I did not become aware of the Cowles Foundation results, particularly the results of Anderson and Rubin on LIML estimation until their work was published in the late 1940s. I realized it was very close to instrumental variable estimation. The article which started me up was the article by Geary which was in the JRSS in 1948. That took me back to the earlier work by Reiersol, and I pretty early realized that the Geary method was very close to LIML except he was using arbitrary functions of time as the instrumental variables, particularly polynomials in the time variable. One could easily generalize the idea to the case, for example, of using lagged endogenous variables to generate the instrumental variables. That is really where my instrumental variable estimation started from. I was actually using it to estimate macroeconomic models fairly early, but the models didn't turn out very interesting to my way of thinking. I developed various ideas on time lags at that stage, and I actually had an early version of the Phillips curve in my model. I spent a lot of those years when I had spare time using an electric Marchand calculating machine in a Leeds University basement and getting out estimates which I didn't get published myself. But some of my early Ph.D. students did work of this kind and that is where I started to be concerned with the theory of these things. Very often I found myself reacting against things other people had published rather than knowing the direction I wanted to take myself, so that I was often reacting in some of my earlier work against, for example, some of the work on spectral methods published by M. G. Kendall. When the Cowles Commission monograph was published, I saw that immediately and I realized that this was important, pioneering work that moved the whole thing on from where it had been earlier. Their work certainly did influence my own research interest at that stage. I really wanted to start to work on simultaneous equation estimation. I actually tried to revise my earlier empirical work using their methodology, but I was still feeling that I couldn't quite believe in the Phillips curve and some of the conclusions that were drawn about the ease with which a small increase in unemployment would cure inflation. So that I began to think particularly about specializing in the direction of wage/price inflation models as the empirical side of my work and, at the same time, before I went to the United States, in 1958 I started to consider the problem of estimation of simultaneous equation models with serially correlated errors. This was published in the two articles on instrumental variables in 1958 in *Econometrica* and 1959 in the *JRSS*.

Those articles had a very definite theoretical orientation and at least one of them was concerned with problems we now describe as nonlinear estimation in the sense that you had quite general nonlinear functions of the parameters built into them and had developed an asymptotic theory for estimators and test statistics associated with that model. So it seems that you had worked to pull together several themes at that stage in your theoretical work.

That was based on considering how to estimate single equations from the simultaneous equation model with autocorrelated errors, and the obvious approach was to look at the autoregressive case first. Indeed, for a long time I was a little skeptical that there was much advantage in moving to the ARMA model simply on the basis that all these things are approximations to a general stochastic process for the errors and that the autoregressive error model was much easier to work with. I thought about the problem of estimating this in a general kind of way. I realized that the simplest approach was to think about it in terms of nonlinear constraints on the coefficients or of coefficients as general nonlinear functions of sets of parameters. I realized that the second approach was the simplest and very often one could parameterize the nonlinear constraints in a way that made it quite simple to use the second approach.

There are also some important specification tests mentioned in a variety of those articles. I suspect that even then you were concerned with the problems of misspecification in econometrics.

Yes, though of course these were natural generalizations of similar tests of misspecification which Anderson and Rubin had developed for the LIML estimator which in themselves were nothing more than likelihood ratio tests. It did seem possible to develop quite a few different types of asymptotically equivalent tests of this type for the general nonlinear in parameters case. In particular, I had this test for dynamic specification very early on where one is looking at the posibility of relaxing the constraints on the coefficients of an equation obtained by eliminating the structural equation errors making use of the autoregressive equation. This occurred to me relatively early on as a rather interesting kind of discrimination to make and one which would often lead to respecifying the structural equation. A test of that sort seemed necessary to me as one often starts off with a rather simple lag structure in a structural equation and wants to test the adequacy of that specification.

And that was the theme that came through in your later research in the 1960s and 1970s as well.
Yes, my 1964 paper really laid out that significance test, but even in the 1959 *JRSS* paper there was a reference to such a significance test.

Could you tell us now about your visit to America in the late 1950s and what differences you found between American intellectual activity in economics and econometrics and that in the U.K.?
Of course the Americans had developed econometrics in all directions more than had the English. It was notable, for example, that the first macroeconomic model of the British economy to be estimated was estimated under Klein at Oxford in 1956–1957 and apart from this and some of the work carried out at the Department of Applied Economics there was relatively little interest in econometrics in the U.K. at that time. I found that there were many more people interested in talking to me about econometrics in the United States. I was lucky to spend a year in the Midwest with Chipman at the University of Minnesota and then to move to the University of Chicago in the following year and find Frank Fisher visiting, and Zvi Griliches and various other people all interested in talking to about econometrics. Perhaps I should say that until I went to America I devoted equally as much time to mathematical economics as to me econometric theory. One of the things I did in America was to decide that the use of computers had got to the stage where estimation procedures which had previously not been possible would be developed as a routine in econometric estimation and therefore to decide that I would try to move into this field and develop some estimation procedures for myself on the computer. I also realized I was spreading myself too widely and that perhaps if I intended to work on both applied econometrics and econometric theory I would drop out of mathematical economics. And this was perhaps partly caused by what I might call the fairly amateurish attitude the British had in those days compared with the American professional orientation that made me decide that I had to work harder in a narrower field and specialize my interest. It certainly was very stimulating to have not only long stays at Minnesota and Chicago, but also to spend some time on the West Coast in the summer of 1959 and visit the East Coast, particularly the Cowles Foundation in 1960.

Shall we move now to your work in econometrics in the 1960s? I would be interested to know how your work on Edgeworth expansions started. I know you gave a paper on the subject at the Copenhagen meetings in 1963.

I had been worried for some time that all our theory except for linear models was asymptotic theory, and I realized that the Edgeworth expansion was a way forward. The trouble with the theory as I understood it was that it mainly dealt with central limit theory and improvement on central limit theorems and this was not general enough to discuss the properties of most econometric estimators. The 1963 paper gave an elementary discussion of the 2SLS estimator, and the method of proof of the theorem I used was perhaps rather naive. It was not until 1971 that I published a paper recording my work in this field, and in the meantime I had developed the ideas of the 1963 article and in particular had carried out exact computations of the finite sample distributions based upon the Imhof routine, so that Mikhail and I had considerably improved the early work which I had presented in 1963. I don't think in the meantime any other econometricians had taken much interest in this work, and I think my 1971 paper was the first occasion that people became generally aware of the advantages of using Edgeworth expansions for the study of properties of econometric estimators. I didn't myself attempt to publish the 1963 paper, but I did send out copies to my usual receivers of offprints.

Looking at that period from outside, I sense one major difference between the problem you were addressing and that which had concerned statisticians. You mentioned the way statistical work was locked into refining central limit theory. At this stage were you aware of the problems of moment existence and the fact that many of the estimators you wanted to treat by way of Edgeworth expansions perhaps didn't have moments to a high enough order to validate those expansions by conventional techniques? Was this a concern of yours in the 1960s as you were trying to establish a theory of validity for expansions?

I was aware of Nagar's work on moments and I was aware that in some cases the moments did not exist. It did seem a great advantage that one could develop asymptotic expansions for distribution functions despite the nonexistence of the moments but in any case it seemed that what we were really interested in, in significance testing and in many other fields, was to approximate the distribution functions rather than the moments themselves. And so, given that I had this idea that for almost anything with an asymptotic normal distribution there was a corresponding Edgeworth expansion, it did seem to be a worthwhile field in which to try and develop a theory.

This developed into a much deeper series of investigations later in the 1970s with a series of papers on asymptotic normally distributed variates and asymptotically chi squared variables. Were there any other major themes in that research pushing you in those investigations?

Maybe I was a little optimistic but I thought it was certainly possible as computers developed for any expansions to be computed for almost any econometric estimator and I was certainly intent on writing a computer program for computing these Edgeworth expansions, possibly making use of the estimated parameters of the models concerned, so that one first estimated the model and used the estimated model to develop the Edgeworth expansions. In some cases if one considers t ratios, the Edgeworth expansions would give revised significance limits which depend upon relatively simple functions of the parameters of the model generating the data. So this two-stage procedure (i.e., estimating the models, then using the estimated parameters to develop a better approximation to the significance interval) seemed to be a competent, practical possibility which, of course, depended upon the decreasing cost of using computers, providing a general program of the appropriate kind could be written. Apart from this practical possibility, the Edgeworth expansion itself is rather revealing about the properties of the finite sample distribution but also the poorness of the asymptotic approximation. It's also clear that apart from the Edgeworth expansion for the asymptotic normal distributed variable, we have the corresponding expansion for the chi-squared test of significance that we could use. Some of the early Monte Carlo simulation work made it appear that the asymptotic approximation to the distribution functions were relatively accurate as soon as our sample size is in the region of 50–100, which is usually achieved with quarterly data. But it became clear as soon as work was conducted with dynamic models, models with autoregressive structure or with time-series models in general that the distributions of the estimators are far from normal and that significance tests based on the asymptotic normal distributions can be very misleading. It seem worthwhile, therefore, to develop Edgeworth expansions for such dynamic models, and my later work in the late 1970s was really concerned with this problem of estimation for stochastic difference equation systems and ARMA models.

Parallel to that work on Edgeworth expansions in the early 1970s you also worked on moment existence criteria for econometric estimators. Can you tell us your goals in that particular field and how it related to the work of Nagar and Basmann?

Of course, if one wants exact formulae for the moments of econometric estimators in finite samples these can be found for relatively simple estimators, but even then the formulae are very complicated to compute, even for mainframe electronic computers, so the work of Nagar was particularly valuable in suggesting that it might be relatively easy to approximate the moments in samples of the size we deal with. However, it is clear that the approximations are not always valid, particularly when the moments are not finite. It seemed worthwhile to establish conditions that ensured that the Nagar-type moment approximations are valid at least in the usual order of magnitude sense, that the errors in retaining a few terms are of given orders of magnitude in relation to sample size.

Some of the work that you did in this area has passed into econometric folklore; that is, people know the results without their even having appeared in print, for example, your work on the tails of reduced form estimators and the nonexistence of moments of the FIML structural form estimators. I would like to use this background to ask a very personal question which may be of some interest to our readers. What circumstances led you not to publish those results on the reduced form estimator moments and structural form FIML moments?

Well, it's not unusual for me to have things lying around for a long time without my working on them. I certainly have submitted articles on this work and been asked to revise them and in this case the referees made a suggestion which made me realize I could perhaps extend the results I'd achieved. At the time I was busy with other work, and I simply put the articles aside for later revision. I have in subsequent years looked at these revisions and realized I'd forgotten what the point was of the extension that I was about to make. It perhaps would be sensible for me under the circumstances to publish the article more or less in the form in which I submitted it originally back in 1974.

In the long article on FIML that you gave at the World Congress in 1970 there is a short section using the normalization argument to show that FIML structural form estimators don't have moments. I always thought that it was a pity that this has not been published.

This was only part of a lengthy review, and the remainder of the paper was superceded by my later work. At a later stage I thought I saw a way of simplifying and generalizing things considerably. In 1982 when I went to Florida, I looked at it again and finally decided not to publish.

To change the subject matter but before we leave the 1960s, can you tell us what it was like at LSE during the 1960s with yourself, Bill Phillips and others in econometrics, and how it differs from the LSE today?

When I came to LSE we were just thinking of setting up taught courses at the graduate level. Before, there had been hardly anything in the way of courses in econometrics at LSE. One must not forget that Rex Bergstrom was there so there was quite a nucleus of people interested in econometrics who were anxious to divide the teaching in this field between themselves and to set up a rather specialized course in econometrics and mathematical economics for our M.Sc. degree. Within two years of my arrival, the possibility occurred of filling two professorships simultaneously, and Eli Devons, who was chairman of the department, persuaded Terence Gorman and Frank Hahn to move simultaneously to London. At that stage, the attraction of LSE as a place to study mathematical economics and econometrics was obviously much improved and we had a very good group of students coming through at the postgraduate level. On the other hand, LSE was not very well equipped from the point of view of electronic computing, and it was not very simple to ensure that new programs could be written and would be available for more complicated types of econometric estimation. One of the first things that I was concerned to do was to write some simultaneous equation programs. But there was a good deal of interest in applied econometrics among the faculty of the economics department and a certain amount of work was carried forward. This developed particularly when my Ph.D. students David Hendry and Cliff Wymer were working for their Ph.Ds and engaged in writing suitable estimation programs. At that time it seemed optimal to allow the Ph.D. students to develop the programs in a way which ensured they developed at the same time new econometric methodology. Part of my time in the late sixties was absorbed by being chairman of the department, and this, together with my teaching, meant that I actually did relatively little research that was published at that time though the work that I did formed the basis of articles that started appearing in the 1970's.

This may be a good point to talk about the teaching of doctoral students. Over the years LSE has become rather famous for its output of Ph.Ds in econometrics under your supervision. I wonder if you'd like to tell us about the nature of doctoral education at LSE and your own strategy towards doctoral training and how you help students get going in their research.

Well, I certainly have had a large number of students passing through my hands as supervisor. I've been lucky that the department has attracted a large number of very good students with suitable qualifications for starting research in this field. It is of course very difficult to accumulate an appropriate theoretical background. And also an interest and real knowledge in economic models is usually required as part of a Ph.D. student's background for the empirical sections of his research. I've always tried to encourage students to combine applied work with theoretical work with the feeling that for the majority of students the choice of an ultimate research field may be motivated by practical considerations, for example, the possibilities of employment. And I have had a mixture of students who have moved in a variety of directions but the majority have moved into either academic teaching or applied research, and for them the choice of topic for research is often a matter of seeing a field where there is the possibility of quickly obtaining research results. LSE has a large number of foreign students who are often more interested in general econometric theory than in research on topics that are not related to their own background. So, many of my students have worked on econometric methods despite a not very strong training in econometric theory. I've always been willing to take on students who have done reasonably well in their initial training and tried to steer them towards suitable topics.

However, for a minority of my students it is certainly clear that a good student will himself find a field that interests him and then complete within the relatively short period of two to three years, which several of my students have done. I'm very happy then for the student to take charge of the field and work on his own to a large extent.

Is there anything you'd like to say in comparing the American doctoral training program with the one that has been operating at LSE for the last ten or fifteen years?

It's difficult to say very much because in a sense that we've instituted ourselves is a program of course work over one year which we try to do at a very high level, partly to take advantage of government finanical grants. After one year within a narrow field, the student has an equivalent level of training similar to that after a two-year coursework program in an American university, but it's difficult to compare standards. Very often the students taking our courses have had a more specialized background at an earlier stage, and we certainly plan our one-year course on the basis of a good background in statistical theory and econometric theory. A minority of our students do take two years over this coursework and do enter the Ph.D. field with a more mature and ready attitude towards research, so that a more extended period of training might be very useful to a majority of our students taking Ph.Ds. A student who comes to Ph.D. work without such a specialist background is going to take an extra year in any case in order to get the necessary general background before starting specialized research.

Steering the conversation back to your own research, early on in our discussion you mentioned Harold Jeffreys and the work on Bayesian statistics which you had encountered as a student at Cambridge. I know that one of your own early articles on the subject was on this very topic of subjective probability in economics and you concluded the article with this rather interesting remark: "Perhaps the moral of the paper might be simply that there is a considerable fascination and perhaps also danger in erecting these houses of cards on uncertain foundations."

I know that in later work you did not seem to return to these problems of scientific foundation and yet I am sure you have thought about them. Is there anything you'd like to say about that matter now?

Well, perhaps I can say something about Bayesian econometrics in general, which is that I've always thought it would be nice to be a Bayesian but you would have to think so hard about the problems of setting up priors and loss functions and so on, that perhaps it would be more worthwhile to think about more robust procedures which don't require this amount of prespecification on your attitudes and your utility functions. At the time I wrote the article, I was a non-Bayesian. I still think there is a problem in economy of logical thought in the relationship between subjective and objective probabilities. Harold Jeffreys rather fudges the issue as to whether there are objective probabilities, whether one has to have a stochastic model of the way in which the world works before one can apply subjective probabilities to the stochastic models.

My attitude at that time was that one did have to have objective stochastic models and then you could apply Bayesian methodology to giving probabilities to the parameters of objective stochastic models having particular values. I did not know of the work of de Finetti in perhaps making it possible to avoid the use of such objective stochastic models, and I can see no harm in believing that the objective stochastic model is a valid way of modeling the way in which the world works. I myself am interested in Bayesian methodology then, but on the whole I prefer to use more classical statistical processes in my own work. I don't believe that the ideas I have in this field are of sufficient general interest for me to try and publish them.

This whole issue of scientific foundation and methodology has been stirring up quite a lot of interest in econometrics in the last five years with reappraisals by Ed Leamer, David Hendry, Christopher Sims, and others. Do you see the subject emerging in any particular direction from this group of evaluations in the years ahead?

I find the work of Ed Leamer, in particular, very interesting. I've always been interested in the problem of choosing between very large alternative sets of models. Of course I could not be at LSE without having some knowledge of the Popper discussion on the development of scientific theories and in particular his objections to the use of Bayesian methods in the physical sciences. But of course what one can discuss fairly rigorously is the asymptotic behavior of certain methodologies for choosing between large sets of models, particularly those where one has the choice of an integer which represents the increasing nestedness of a set of models as the sample size becomes large. But apart from this discussion of consistent choice, it is difficult to see how one can tackle this problem except perhaps from a decision theoretic point of view. Though even from this point of view, one has a great deal of difficulty in practice in deciding what kind of losses and prior probabilities one is going to use. It certainly seems that something must take the place of the Neymann–Pearson approach, and the two things that I have mentioned, the discussion of asymptotic consistency and the possibility of a decision theoretic approach, may not be of great practical use if in practice the amount of information that we have does not suffice for the asymptotic theory to be a good approximation. And the problem of deciding what loss function to use makes the Bayesian approach rather impractical. So I feel that we shall often be in the state of not having enough information to know what models we should be using and whether to choose a particular model on the basis of relatively little information.

Part of the thrust of the attack by people such as Christopher Sims on, for example, simultaneous equations methodology, has centered on the lack

of credibility that an investigator would typically attach to the many prior restrictions that might be delivered from some background economic theory to help one apparently identify a system of equations in practice. This and other considerations have led these investigators towards the estimation of unrestricted systems of equations. I suspect that some of your own work on dynamic specification could be seen as working in parallel to these ideas. I am thinking particularly of your work on common-factor testing and the choice between error and systems dynamics. Looking back on that work that you initiated in the 1970s do you feel that it has been as successful in applications as you might have hoped?
It hasn't been used to the extent perhaps that I might have hoped. I may be partly to blame in that there are only a few programs available for using the COMFAC procedure, and without programs people don't find it possible to use it. But perhaps I should answer the question why such COMFAC procedures haven't been taken into the common computing programs that people might be using. I suppose it is that it is a little unusual as an attempt to simplify models by factorizing out an autoregressive part of the dynamics, whereas it appears to most people a simpler approach to have autoregressive dynamics when they find that their errors seem to be autocorrelated. The COMFAC procedure is a procedure which starts from a more general specification and tests downwards rather than a diagnostic procedure in which having started with the simplest specification one then finds, "this will not do," and then adds autoregressive error structures to cure the problem. It always seemed to me that it is worthwhile to start from an unconstrained model in order to get the advantages of having simple Wald-type tests for constraints, the kind of constraints that are involved in the common-factor specifications.

Many of the empirical model builders, particularly in macro economics, work with systems that are inherently nonlinear. Since the COMFAC procedure was developed for linear systems of equations, I wonder if this might not have been a factor in the practical use of the procedure by macro model builders for example?
Yes, I certainly am aware of that problem, that it is a relatively simple kind of specification that is available for testing. I have, for example, unpublished work on the use of similar procedures for sets of equations in which one attempts to factorize out a vector autoregressive specification by looking initially at regressions of endogenous variables on lagged endogenous variables and on current and lagged exogenous variables. Part of the difficulty is to find problems that people would apply this to. It's not usually the case that people want to test completely unrestricted reduced forms against a reduced form with an autoregressive error structure. Usually people prefer to work with a structural form, and if they are working with a structural form, then it probably turns out that the likelihood ratio test for this type of autoregres-

sive specification is a good way of testing whether the dynamics are due to autoregressive error structures or to basic lags in the economic model.

While we are on the subject of model selection, I wonder whether there are any other criteria, like the Akaike criterion or encompassing, that you see as promising at the moment in helping to guide the investigator to the specification of an equation system?
I do not particularly like the Akaike criterion. But similar kinds of model selection procedures, like the quasi-Bayesian or Hannan procedures where, in effect, you have a change in the likelihood function which is designed to ensure consistency asymptotically in the selection of the current model seem to me to be better procedures. These are, however, not very different from standard significance test procedures when testing a nested set of models. I am certain that there is interesting work to be done in this model selection field. I'm quite interested in alternative procedures such as the two kinds you mentioned. I myself am a little doubtful whether the kind of standard significance testing approach is anything more than a rather crude rule of thumb. One certainly has to somehow intuitively allow that some models are more plausible than others and perhaps one should be more careful in rejecting some models than others. One has also to be aware of the data mining problem, and perhaps make use of nonnested testing procedures in some cases. So it may be appropriate to have two polar models which one is intending to compare in which nonnested testing methodology will be required. And in some cases, the encompassing method of testing between different polar cases may be appropriate, especially if it is designed to be in effect equivalent to an efficient nonnested testing methodology. It is still clear that econometricians are faced with very serious problems in the complications of the models and the large number of alternative models they may be trying to decide between and that we are far from being able to solve this with the amount of data that we have.

Perhaps the time has come to move to more general questions. Considering your work overall, do you see yourself as more of a toolmaker than a tool user?
Yes, I suppose I do. I myself thought that I had done something in the labor field when I introduced the idea of the real wage as an important target that the wage bargainers were aiming at, and this certainly gives some explanation of the behavior of wages. I certainly did a bit of tool using at that stage. Since then I have done some applied econometrics. I am actually doing a little now with testing models of rational expectations. But I would certainly regard my major contribution to be as a toolmaker.

Are there any articles of which you are particularly proud, and are there any which you would care to forget?

Well, there are certainly some articles that are a little old-fashioned, out-of-date. Perhaps they were so when I wrote them. But I do not think I will actually specify which they are! As to the articles I think are particularly interesting at the time, perhaps the work on autoregressive estimation and also the 1964 article which combines applied econometrics with methodology in the Colston papers involved most of the interesting work that I did earlier on. And I suppose later on, my work on Edgeworth approximations was useful in pointing out how widely it was possible to use them in econometrics.

Looking at developments in econometrics over time, perhaps over the cycle of your own career, I wonder if you see any particular milestones in the emergence of econometrics as a separate discipline?

I think that econometricians have learned more about time-series analysis, perhaps having been stimulated by developments in operations research, the control engineering field, and the optimum control field. All of these things have ensured that econometric theorists have really got to be much more professional statistical theorists than they had to be when I started out in econometrics in 1948. The consequence of this is that there are much more rigorous and interesting developments in estimation, even for the more complicated models; for example, the model that had already been developed by Tobin at the period when I started up is now much more understood. The theory of estimation with additional complications such as autocorrelated errors has all been developed, and the corresponding rather difficult asymptotic theory has been developed at the same time.

When I started out, I doubt whether many people were capable of reading a textbook such as Doob's textbook on stochastic processes. By now, however, I imagine that a large number of econometricians are understanding the theory of Martingales and making use of this type of theory in econometric applications. It does seem that on the theoretical side, although the theory we are using is applied to models that are probably more complicated that most statisticians have to work with, we have seen over this period the development of statistical theory required to cope with this level of complication. At the same time on the applied side, I suppose the computer revolution strikes one. We are more and more able to estimate models of any degree of size and complication even though we do not understand the finite sample properties of our estimators. We can certainly specify an estimator and develop the corresponding programs for computing such an estimator at any level of complexity. This has encouraged people to consider more complicated model formulations where the simplification of linearity in variables and parameters is gradually disappearing and where the corresponding simplicity of the econometric theory required to discuss the properties of the estimators is also disappearing. Of course this means that the starting econometrician hoping to do a Ph.D. in this field is also finding it more difficult to digest the literature as a prerequisite for his own study, and perhaps we need to attract students of an increasing degree of mathematical and statistical sophistication into our field as time goes by.

Most econometricians are in the business of making predictions. What are your views on recent papers that are likely to emerge as classics in the econometric literature or, more generally, in econometric thinking? About the forties and the fifties for example, one cannot help but think that Haavelmo's work on bringing in the probability approach seriously into econometrics and then the Cowles Commission Volume 10 are both landmarks in econometrics. As for recent work, do you think, for example, the movement into non parametric work may turn out to be of some importance?

Clearly there is a lot of interesting work going on in problems of robustness, problems of nonnormality. I think there has been a great development in panel data work largely because the data is there, eventually accumulating at least one series of panel data that covers an interesting period of years. And I suppose the same is true of quantal choice models. We know a good deal about how to approach the limited dependent variable type of problem, but I don't think I can pick out any paper which crystallizes these fields and where important work is leading to really new developments. Here I have been talking about developments which have taken place. I hope there have been important developments in finite sample theory as well, where clearly people are going to go on working, despite the complexities. But I always find it difficult when a student asks to be directed to a field which is going to be vital to suggest what it should be. There are so many fields and the subject has got to the stage where it has taken in a very wide area and people do have to specialize and concentrate on filling out what is there already. I do not see anything completely new developing.

I wonder if this might be because of the many different types of data and models one has in economics. For instance, we can think of the work in time series that Peter Whittle seems to have started and that Ted Hannan developed in the sixties with spectral regression theory. Perhaps in the late sixties and early seventies this may have been regarded as a very promising wing of research and yet somehow it does not seem to have gone very far in econometrics.

Well, it has had its impact but there are bound to be people who go into this complicated field, and almost anything you can think of now that involves dynamic data has really been affected by the work of those years, and the work on time series analysis in operations research. For example, the work by Andrew Harvey on Kalman filtering and the way that that development is ensuring that very many problems such as missing variables or aggregation can be handled in a very uniform way. Things which have been developed in this time-series field have had a very wide impact on econometric problems.

The same is also true of panel data work, where random effects models and variations on random effects models are all an important way of dealing with some of the problems in econometrics. They are all fairly closely related and, in essence, based upon ML estimators of slightly more complicated models. A large number of similar kinds of developments are all concerned with making models more applicable to the data you happen to have. On the other hand, it still is true we are very far from understanding in theoretical terms the large macroeconomic models that a lot of economists want to use. But there does not seem to be any solution to that in the sense that unless the models have specific statistical properties it is difficult to produce any theoretical ideas about how capable these very large models are of predicting what is going to happen next to a particular variable. You have to rely upon some kind of assumption that the predictions are not very sensitive to slight misspecifications here and there in the remainder of the model, that the misspecification errors are not building up in a way which makes the model unusable.

Do you see VARs as helpful in overcoming those difficulties?
I think there are some problems in econometrics that are never going to be satisfactorily solved. The attempt to explain very large interlocking sets of variables by relatively small models is perhaps one that is not going to work very well. It will work in a statistical sense over some time period, but how long that time period will be is something of a problem and very difficult to explain except by building bigger models, which are very difficult to work with in any case. I find that one of the chief objects of econometric theory is to explain why it is so difficult to make good predictions. It isn't a way of specifying methods which will make good predictions, because it may be very difficult to find any that will work outside some particular frame of reference. Econometricians professionally are in the business of being optimistic about this, but when one gets down to it, one feels that it is remarkable that we are successful with relatively simple models, given the complication of the real world.

We would like to try to visualize you in your work. Could you describe a typical work scenario?
I've always felt it was easier to work in the school than at home so that when I had a new idea to develop I could work on it more easily in the school. In the "old days" I used to do all my work in my office here. The only snag is that one has so many possibilities for interruption if one is available at the office. So, in addition to the work I do most of the week, I'll also normally reserve one day a week for work at home and try to take everything I need home for that day. I suppose what I do is collect all the articles I am likely to be referring to in the work being done, and keep them in a folder, take them home, sit at a desk, and think about what I am doing. I suppose that

is a typical scenario. I'm usually carrying things about in my bag so I can also work at the office during intervals of other business.

I often find that I am working on ideas that I've had several years back that I've put on a piece of scrap paper with a suitable heading to remind me what it's supposed to be about. For example, I feel like I'd like to do some computer work on a topic that I had thought about theoretically some time back, and so then I have to wait until I get a suitable research assistant to write the programs for me or gather data if it is an applied field. Sometimes I am really taking something which occurred to me way back out of an earlier piece of research: for example, looking at a special case where I am "Monte Carloing" something in estimating models and wanting to study the way in which Monte Carlo estimates behave. Or perhaps back in an earlier article I noticed that the condition for an estimator to be consistent did not depend upon the kind of first-order identification conditions which occurred as standard conditions in textbooks. Having noticed that some time ago it becomes a matter of having to find time to actually study what happens in these cases: how the resulting estimators behave in the way that is different from what would happen if they did satisfy these same first-order conditions. So very often my work is based upon ideas that I had some time ago and haven't had time to explore yet.

As far as beginning researchers are concerned, what do you see as the most pressing problems or areas for younger people to address?

The problems yet to be solved are often the difficult problems. So it is difficult at the beginning of the Ph.D. to see what is likely to be a promising thing to work on. What one can see is a whole range of topics in which interesting research is going on. Then, to some extent, it seems a matter of interest, what the student would want to start on. Clearly some fields have been too popular in the sense that a lot of people have been working in the field and you may feel it is not easy to see what remains to be done. Perhaps a field where this is true is alternative types of testing procedures: Lagrange multiplier tests, likelihood ratio tests, and Wald tests. But of course even when you pick that out as a field which has been thoroughly investigated it still is such a wide field that there probably remain marginal problems where solutions would be reasonably easily available. My initial research work was often stimulated by disagreement with other people, and I feel that this may also be a good thing for beginning researchers to think about. Read a wide range of articles in the field in which you are potentially interested and see whether one can spot anything you think is overemphasized, misemphasized, or downright wrong. I think one just has to look around the frontiers of a science like this and see what one can see. I, for example, think finite sample theory is a very interesting field to work in. It has the disadvantage that it requires a rather difficult background in mathematics and statistics to make any advances. So a beginning researcher has to think about his own limitations and perhaps decide either to do more background work in order to move into a difficult field or think about whether he's more interested in a minor methodological advance which might be of considerable importance in some applied area. There is still, I think, a good deal of work to be done on such topics as general nonlinear estimation with applications to fields like production and consumption theory, where an interest in the relevant economics and perhaps in the specification of suitable functional forms might be very useful. I am certainly finding it very interesting to try and do a little work in the field of rational expectations. This is a very interesting field in combining time-series and prediction problems with estimation and testing problems. Of course, one of the difficulties in the area is that the existing workers are rather good at dealing with rational expectations problems. I am sure this is a field where major research is required both at the theory and the applied levels.

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