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THE ET INTERVIEW: PROFESSOR L.R. KLEIN

Interviewed by
Roberto S. Mariano

Lawrence R. Klein

Lawrence R. Klein, 1980 Nobel Laureate in Economics "for the creation of econometric models and their application to the analysis of economic fluctuations and economic policies,"[1] has played a significant role in the development of econometric methodology and practice over the past forty years.

Long recognized as a leader in economic model-building, Lawrence R. Klein has always underscored the integration of economic theory, statistical methods, and practical economic analysis in his research activities. His pioneering and continuing work in large-scale modeling has served as a training ground in applied econometrics for many academicians, corporate executives, and government officials from all over the world. His lasting worldwide influence in the econometrics profession is a tribute not only to the intellectual prowess of his research but also to his willingness to give generously of himself in interacting with students and colleagues.

There are earlier published materials on Lawrence R. Klein which contain a more detailed discussion of his research contributions:

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1. Breit, W. & R.W. Spencer (eds.). *Lives of the Laureates—Seven Nobel Economists.* Cambridge, MA: The M.I.T. Press, 1986.
2. Ball, R.J. Lawrence R. Klein's contributions to economics. *Scandinavian Journal of Economics* 83 (1981): 81–103.
3. Klein, L.R. Autobiography in *Les Prix Nobel 1980*, pp. 269-272. Almquist and Wiksell International, Stockholm, 1981.
4. Marquez, J. (ed.). *Lawrence Klein—Economic Theory and Econometrics.* Philadelphia: University of Pennsylvania Press, 1985.
5. Wold, H. The Prize for Economic Science in Memory of Alfred Nobel in *Les Prix Nobel 1980*, pp. 263-265. Almquist and Wiksell International, 1981.

In this interview, Lawrence R. Klein provides us with his views on the philosophy and the evolution of econometrics as he recounts his exhilarating years with the Cowles Commission in the 1940s, his brief but productive sojourn at Oxford University in the 1950s, and his long enduring relationship with the University of Pennsylvania.

Perhaps we can begin by talking about your early schooling and your graduate education.

I think the most relevant thing is that when I went to university, I was very much interested in both mathematics and economics, and I took most of my subjects in these two fields. I had an intuition, not really spelled out, that somehow mathematics could be interesting in the formulation of economic ideas. When I was a student in my last two years at Berkeley (the first two years I was at Los Angeles City College, a junior college in the California system), there was little idea of such a correspondence. There were courses in mathematics and courses in economics but only one combination course for graduate students, and I was allowed to take it. But in Berkeley I was very surprised to find some people who were really pioneers in mathematical economics. One person who was with the group that founded the Econometric Society was Griffith Evans, Professor of Mathematics. I didn't study with him, but I did a lot of work with people who were his students. The other leading figure was Neyman, and I worked with a lot of Neyman's disciples at that time. There was rather a good environment in Berkeley around 1940 for someone interested in mathematical economics and econometrics, though it was not a leading center for mathematical economics. At that time mathematical economics was really fighting for recognition, and in many respects it was blocked or suppressed. But I was extremely surprised when I discovered the early articles by Samuelson in the university library. When I went from Berkeley to M.I.T. on scholarship, I was assigned to be Samuelson's assistant, and that was the beginning of a long story in this field. But after I graduated from Berkeley and before I went to M.I.T., I spent a summer working with George Kuznets, who was the younger brother of Simon Kuznets and a very good statistician, though his degree was in psychology.

Was he based in Berkeley also?

He was based in the Gianinni Foundation studying agricultural economics and I was his assistant estimating demand functions for California lemons.

That must have been one of the first exercises in applied econometrics that you worked on?

Yes, and that was a very productive summer working for him. But working as an assistant for Samuelson was something that is very hard to duplicate anywhere in the world. He generates ideas so fast. At that time, there was a whole succession of ideas concerning Keynesian macroeconomics and econometrics and the development of mathematical methods in economics. It was a very exciting time, and I felt very fortunate to be in that background.

In this recent volume of articles of yours compiled by Jaime Marquez, you were describing one of the first problems that Paul Samuelson asked you to work on, namely, the mathematical equivalence between the problems of identification in supply-demand models and in saving-investment analysis.

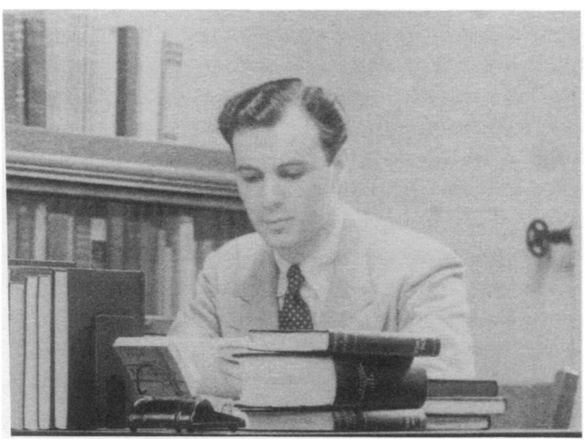
You see, at that time Haavelmo had spent some time at Harvard as a Rockefeller fellow. He had written an unpublished manuscript that was circulating around and later became the *Econometrica* supplement on the probability approach in econometrics. In that sort of working paper there was a very good treatment of the identification problem. Samuelson was very impressed by that. Samuelson worked on a lot of different problems. At that time he was concerned with the Keynesian problem, the problem of estimating the Keynesian system, but he was also concerned with the abstract question of identification as a separate subject, and he saw in this problem the possible connection and application. He was very clever. He said the identification problem in saving-investment analysis is an exact mathematical analogue of the identification problem in supply and demand analysis. At that time many people thought they could solve the problem by some trick, by splitting the sample or doing something special. There were some papers by Leontief and Frisch, and many people tackled this problem. But the problem was not formulated quite properly, and there was a different approach in the saving and investment literature, namely, to break down the consumption function or the savings function into tiny components. By treating each one separately and then adding up, people thought they had a separate identification of the functions. Samuelson said he doubted that, so he put me to work. I was his assistant. Being his assistant meant you just picked up a problem of interest. He steered me into that problem, and he had great insight as to the structure of that problem and the solution. I was rather glad that he gave me some guidance.

The major contributions in your lifelong work have dealt with macroeconometric modeling and applied econometrics. After working with Samuelson on this issue concerning identification, can you describe the path that led you to your major activities in macromodeling?

Well, Samuelson got me into my thesis, which I think was a rewarding suggestion, but it was more economics than anything else. It was on the Keynesian revolution and that was really the big topic at that time. At the end of that period, I went to a meeting of the Econometric Society. I met Marschak and Koopmans. And Marschak said to me that what this country needs—meaning the United States—is a new Tinbergen model, a fresher approach to it.

Do you remember what year this was started?

It was 1944, and Haavelmo had just started circulating his book and had ideas about estimation problems. He had written the article in *Econometrica* that started us thinking on this line about the statistical implications of systems of simultaneous equations. Mann and Wald had just submitted their article on the dynamic case. There was a tremendous amount of coherence from different quarters in discussing this particular problem. Marschak said he wanted to assemble a team: he wanted to have some statistical theorists, some economic theorists, and some model builders, and he asked me to come out to Chicago to build a model. That was my first job. When I took



Lawrence R. Klein in his office at MIT, Spring 1944.

my degree at M.I.T. there were two or three job offers. One was with the Federal Reserve and I forgot what the others were. But Samuelson thought I should go for one of those. Even though the salary was less, I was intrigued by Marschak's offer, and I knew that was the subject I really wanted to work on, so I went to the Cowles Commission. And I think, looking at it in retrospect, that was just the right thing to do.

As a natural step after your thesis at M.I.T., was your first work at the Cowles Commission on the macromodel then?

Yes, and it was a most unusual group of people there. To think of having Marschak, Koopmans, Haavelmo, Hurwicz, Anderson, Patinkin and eventually Arrow, Herman Rubin, Roy Leipnik, and Herman Chernoff, with many visitors like Jan Tinbergen and Ragnar Frisch. It was just a tremendous number of people who were unusually talented, and they all congregated in that one place. I doubt that we could ever attract such people again in one place. Now people have too many offers and too many other kinds of opportunities. But given the situation then in terms of people and openings and in terms of the influence of the war it was just very unusual. There was also a feeling that we had all the answers to the problems from a statistical point of view and from the point of view of econometric methodology and of economic content, so that it would be easy to have a well-organized, well-run economy after the war. We felt that these new methods would be extremely powerful.

So the concept then was to develop the methodology and develop the models with very explicit applications in mind towards policy analysis.

That's right. Marschak used to say in meetings of broader groups—National Bureau groups and others—he said, "Just give us three years and we will deliver the systems that you want." That was an indication of the kind of confidence that we instilled in each other.

That really started the development of the statistical treatment of simultaneous equations models.

Well, the whole group was broken into subgroups. There was one team working on the treatment of simultaneous equation problems. Another group worked on putting the model together, some from the point of view of economic theory and some from the point of view of data availability. Another group worked on computing. We carved up the problem. We had very heated and intensive seminars, and everybody was extremely enthusiastic, but it was very well orchestrated.

And after that, when was the Klein model I developed?

Well, that was at the end of that period. That started in, I think, October 1944 and the Klein model I was essentially ready some time in 1946, certainly by 1947. There were two things that got me interested in this work. I was interested in the formal equation relationship between Keynesian economics and Marxian economics via income and capital, so I tried to specify a macromodel that would look at this very relationship. In addition, we had a visit in Chicago by Kalecki, and I spent a lot of time talking about this model with Kalecki. That was stimulating and helpful for me. So we had that model put together, although the bigger model (what was then a big model but today very tiny) was our real aim. Thus, Klein model I was just a kind of diversion, but one that was more compact so we could study computational methods.

And after that the more complicated models got developed?

Yes, but that took a few years. I wrote that up as a book. That book was published in 1950, but I think it was essentially completed by 1947. It was polished and there was a publication period involved. But the model was never maintained. I left the Cowles Commission in the summer of 1947 to travel for a year in Europe, particularly to visit Frisch's Institute in Oslo and to spend some time in Tinbergen's office, the Central Planning Bureau in the Netherlands, in the summer of 1948. Somebody else—I think Kenneth Arrow—was supposed to take over my work at the Cowles Commission that year. But his interests got diverted into something else so that the model was never maintained. Then in 1949–1950 I came to Michigan. I was working on a very special problem. I was working on the use of consumer survey data to study the consumption function and especially to investigate the influence of liquid assets on consumption. At that time people kept inquiring so much about forecasts and the state of the economy and those models that I decided to put together another model. Essentially, the work at the Cowles Commission was dropped and then restarted in Ann Arbor.

In your view, what are some of the major developments in macroeconometric modeling, starting, say, from Tinbergen's models?

I think Tinbergen's models were quite remarkable. He had a tremendous amount of insight and they were very well designed. One thing I didn't like about them was that they were linearized in first differences or percentage changes. But I think perhaps that is less of a problem than I thought it was at the time. So I wanted to do it somewhat differently. The other problem is that we started thinking about the size of systems, whether they should be bigger or smaller and whether they could be handled. I think the most important single development was really the Haavelmo view that we should relate the probability structure to the economic theory structure. The concept of specifying a model with a stochastic expression built directly into it and moving from the probability distribution of the random errors to the probability distribution of the economic quantities is a very powerful way of thinking about the system. I feel now that the specific methodological procedures that we use—maximum likelihood, limited information, two stage, three stage, or whatever estimation method—are not as important as this conceptual framework for thinking about system design. The idea of paying attention to that part of the system that is associated with its original structure and that part of the system that is associated with the reduced form and also that part of the system that is associated with the solution is a very important set of distinctions to be made—a very useful way of looking at systems.

In the 1960s, macroeconometric models enjoyed a great deal of popularity. Then, in the 1970s up to now such popularity has declined considerably. Do you agree with this observation?

I don't agree that that sentiment reflects the view of the whole user community, but I do think that such sentiment exists in the academic community. I interpret it in the following way. In order to do what I regard as useful and substantive work in economics with such models they must be fairly big and complicated. That involves a lot of work that some people would not want to undertake. It involves a lot of work with regard to preparation of data and maintenance of data files. It requires willingness to scrap results as data get revised and to rebuild the whole thing from scratch again quite frequently. That means that this kind of research has to be done as a team effort. That indeed was the way we started out in the Cowles Commission, but it then became a routinized team effort. Somebody had to be responsible for the data files, someone had to be responsible for system design, and someone had to be responsible for forecasting and applications. I think that young academics, especially in the American environment, want to get promoted fast and want to have their own names on a piece of work. They want a research project that is thoroughly manageable. I think that is a reason for people wanting to work with a small system—so that everything can be under their control and remain manageable. They want their own name on it, and most people seriously dislike the idea of working with somebody else's models. The team effort went very well at the beginning but doesn't go so well now, particularly given the criteria for advancement in the academic system. This became a mode of research that many young people who generate good ideas stayed away from. That's especially true in the United States. I find quite a bit of interest in team research in econometrics in developing areas, Socialist countries, and in areas that have not had such models. There is the same kind of keen interest in getting started that we noticed in the 1950s and 1960s in the United States. But in the United States, and to some extent in western Europe and Japan, there is a preference for the lone researcher to do his own thing apart from the team effort. Another aspect is the computing burden. When we started doing this kind of work it was extremely burdensome, and systems were kept small because of the complicated computer problem. In addition, there had to be a computer group that did all the drudgery work. When we finally came to use the large mainframe, that liberated us from our computing difficulties. But it still meant that we had to have access to the hardware, with a research budget and facilities. That also requires, though not entirely, a team effort. Data management became specialized. Now the situation is changing with the PC. That means that the individual researcher has complete command over the computational aspects of the situation and also over the economics. The lone researcher can handle bigger data files and more complicated modeling systems. It probably means, in addition, experimentation with smallish systems—not tiny but smallish—that have some novel features in them. That is quite good for research progress. There is also the matter of changing interests. When I first came to Pennsylvania, it was quite common for students from developing countries to want to build models of their own countries and submit that as a thesis. Well, that's been done so many times that it no longer is a suitable thesis. In order for one to use that approach, there would have to be some new twist or some new aspect spelled out, and that's not always so easy to find. Consequently, people have often gone off to much more manageable and acceptable dissertation research.

How has macroeconometric modeling kept pace with developments in economic theory?

The principal idea that was impressed on everyone at the Cowles Commission was that structural models must have a theoretical base in economics. We worked on the neoclassical specification of models. We worked on the aggregation problem, we worked on the market-clearing problem, and we recognized that all the modeling should have a theoretical base.

In the stimulative discussion atmosphere of the Cowles Commission, I was working on the linear expenditure system for studying cost-of-living indexes in the context of a neoclassical demand model, and Herman Rubin saw immediately how to use the integrability conditions to establish restrictions on the coefficients. In my opinion, that is a fruitful way to use economic theory for equation specification and is just the sort of development that would be generated by the approaches that we were then taking at the Cowles Commission.

In later years, I think some people became slaves of the neoclassical behavioral formulation without taking account of the aggregation problem. In their fear of being "ad hoc" they chose theoretical lines which were not always well conceived. Many of the things that people thought were theoretical were not very good if you take into account the aggregation problems that were involved. In my own approach, I have insisted that there must be a theoretical basis for equation specification, and there must also be a close correspondence with reality. There must be forecasting tests. I think many of the present generation of researchers are not careful with forecasting tests and are not careful with reality, but are over-impressed with pure theoryspinning that isn't going to lead to significant improvements in the system. I think that at the present time macroeconomics, say in the last five or ten years, has taken what seems to me to be a fruitless turn that is not going to produce very powerful results. I think that the imposing of predictive testing and economic theory, under the constraint of aggregation, is a better way to proceed. I adhere to the view that a system that is not well conceived will not stand up under severe forecasting tests. It might stand up once, it might stand up twice, but if you replicate the forecasting exercise often enough, frailties will show through. The only real thing we have to go by is predictive testing, and it takes a long time to build up a satisfactory record.

Along these lines what do you think of vector autoregressive processes and their uses in macromodels?

To some extent vector autoregressions are associated in my mind with the concept that Koopmans introduced, "Measurement without Theory." I think that they are eventually going to be misleading from that point of view. I look at the problem in the following way: When we first put our models together, people said that the relevant test should be the random walk, or today equals yesterday. Then, after that became a not very severe test – after it was shown that that was not a good standard – people went on to the next more sophisticated criterion, today's changes equal yesterday's changes. Then they went to autoregression, then they went to ARIMA models; and now they have gone to vector autoregression. So I regard vector autoregression as being in this sequence of moving from the most simplistic model of testing, which we call the naive model, to a semi-naive model which is, in the present state, a vector autoregression. In all these tests we have noticed that the systems that represent "measurement without theory" break down at turning points; they break down under unusual circumstances and they cumulate error fast. The vector autoregression is the first of such systems that doesn't seem to cumulate error very fast, at least at this stage of the process. I believe the real test will come when we watch a vector autoregression try to handle something as complicated as the oil embargo of 1973, the Iranian revolution of 1978–1979 or what I call the Nixon NEP program in 1971–1972. My prediction is that it won't be very useful when we need it most. Our structural systems, I think, served us well on each of those occasions. And I think that some future critical situation will be the true point of distinction between the two. Under present conditions, given the period of time during which we have looked at the performance of vector autoregressions and the macromodels that we presently have, I would conclude that for the short run they perform very nearly the same. Vector autoregression holds up better for the longer term than any of its predecessors – the ARIMA, the simple no-change, and so on. I think that we have to wait until we see a more crucial test, and I think the crucial test will not be so kind to the vector autoregressions.

There are some parts of the vector autoregression structure that I find curious or bothersome. One is that all variables are endogenous. I think that is not a useful way to structure a system. Secondly, I haven't really gotten all the details, but I believe that not all the terms in the vector autoregression are used, and some zeros are, on a judgmental basis, placed here and there until the model is fine-tuned. I would have to look more carefully at the placement of these zeros before making further judgments.

That's very subjective then. Can such judgmental calls be validated?

Yes, we have to see whether we are getting closer to a mathematical economic system and how much subjectivity is being introduced into the system. If in the end we deliver equal predictive performance from vector autoregression and from the large-scale system, I would say that I prefer the large-scale system because it has more informational content. It handles more variables and it provides more information, and that is what users want. The criterion that I use for model selection is to say: use the biggest and most detailed system that can be well managed by human agents, together with our computers, and not lose on the accuracy of some of the principal aggregates, and that can deliver these additional pieces of information. The vector autoregression wouldn't be my choice for the system to be used. It is not a bad system as a standard for comparison on some main aggregates, and in my opinion, that would be its main use.

I am working now on the problem of combining monthly and weekly information that are used for very short-run extrapolation by ARIMA methods, together with big detailed models for extrapolating over a more extended period of time, let's say from six months to two or three years. I think that that combination looks more favorable to me than a vector autoregression or any other kind of combination presently available, but I could see a vector autoregression being used for extrapolation of some of these higher frequency data at some point, although that's not the specific way that I am proceeding.

This new research area you are looking into essentially uses a time-series model to update or come up with preliminary estimates of data that are not readily available. Is this the idea?

Partial data are always available. In particular, if we build models in a quarterly time frame, then daily, weekly, or monthly data are available. Within any quarterly time period, we cannot ignore the content of these daily, weekly, or monthly indicators. My problem is how to relate information contained in the movement of these high frequency data to the movement of the quarterly data. I am doing it systematically through the expenditure and income sides of the social accounts in relating the key items in our models to the time-series movements of these high frequency indicators. The latter form the targets to which we adjust the major model in future extrapolations. I think that will prove to be very fruitful.

Looking back at the development of econometrics over the last forty-five years, what would you consider as major breakthroughs over this period?

This is one idea that I thought a lot about over the years, whether there is ever a breakthrough. Going back to the 1940s, we thought that simultaneous equations methods were going to be a breakthrough and that they would have enormous power and accuracy. Next we said that when we moved from annual to quarterly data we would have a new breakthrough, and then we said if we can draw upon cross-section data we would have an additional breakthrough. Later we said if we had anticipations data we would have a breakthrough. The next big step was to have been through the use of control theory. Now people are saying if one models with consistent expectations, rational expectations, or vector autoregressions, then we have promising new tools. When we went to nonlinearities and easy handling of them and ARIMA methods through computers, we thought that we would have breakthroughs. I regard none of these as a complete breakthrough in terms of making very significant gains in accuracy of economic-econometric judgments. But a lot of things build on one another, with very tiny improvements. Over the years, I think one of our most significant improvements has come from a very intensive computer analysis of the dynamics of systems and the time shape of lags. I think that has contributed greatly to doing better work. We have much better control over the dynamics. Duplicating the accounting structure of our social system in models has been another important step. But nothing really has been a complete breakthrough for solving the problems that confront us. I believe that the noise level in the economic system is always going to be very big, and we shall never be able even to approach complete eradication of this factor. So we shall always be far away from breakthroughs.

How about the role of expectations—and the way people have tackled that problem both from an econometric and an economic viewpoint?

I think expectations are very important and I think that the model builders have recognized it from day one. The present generation of economists are not leading us in any fruitful direction for studying expectations. Expectations are endogenized and introduced in a very mechanical way. This method has very little behavioral content and very little informational content. In my opinion, the best way is to go to the source of expectations and find out what people actually expect or anticipate and to endogenize that within the framework of models. That means that we should integrate sampling investigations on subjective expectations together with market and accounting data for the economy and treat that as one big system with the subjective expressions of expectations as endogenous variables. I think that is a very straightforward procedure, and one that will prove to be the best. This approach will have true informational content because we will be trying to model people's stated expectations in a realistic way. We must take account of the life of these expectations. In fact, it is rather short, and that means we have to have repeated subjective observations. I find the European business test surveys, the surveys of consumers, the various surveys of inflation, the statistics on orders, the statistics on housing starts, and all the things we call anticipations variables to be very important. They need to be integrated directly into the models.

Can you describe some major works which have integrated these anticipations variables?

For a long time, the Wharton model and the Michigan model have used these. Even going back to 1960 after I returned to the United States from Oxford and started the whole series of generations of the Wharton model, the first thing I did was to introduce consumer-purchase expectations, business-investment expectations, housing starts, and other kinds of anticipatory data directly into the models. We don't make huge gains, but I think we make gains by using them. I think that's the proper behavioral way to introduce expectations.

Is there simultaneity in the way anticipations are introduced? Or is the integration only partial in the sense that only one direction is taken into account, from expectations to the formation of decisions?

What we do is to say that the modern discussion of expectations has one useful piece of scientific content, namely, that expectations are based on the latest information that is available to agents. We have people's stated expectations, and we simultaneously know the state of the stock market, the state of the bond market, the movement of inflation rates, and the movement of monetary instruments. We should relate expectations to such pieces of information as are available to everyone at the same point of time. Most of those things are also generated within the model, we have feedback from market conditions to the expectations and from the expectations to those indicators, so they can be fully endogenized. We have something that many investigators are neglecting: we have observations on people's statements of what their expectations are. The thing that I find bothersome in many present treatments is that people try to separate out what is anticipated and unanticipated just on the basis of indirect observations of data and the imposition of assumptions on those data without having any direct behavioral basis for saying we have observed something that is either anticipated or not anticipated. As I may have mentioned to you on earlier occasions, I think that some of the issues are similar to our treatment of the errors of measurement in econometric modeling. We had long discussions about this problem at the

Cowles Commission. We decided that we would not base the probability structure of our models primarily on the distinction between the true value and the observed value of economic variables. That distinction involved the generation of errors of measurement. Unless we have special information about how accurately something is measured in a relative sense, we will not be able to implement the theories of inference based on measurement error. But we lack such information. The fact that we lack that crucial piece of information has meant that there has been a lack of identification in systems, and many estimation methods break down directly. Now we find the present generation of econometricians trying to do the impossible, trying to separate something they don't observe into anticipated and unanticipated components. These are subjective components, and we have no confidence that they are getting sensible answers. There is a complete unwillingness to confront expectations variables with what people say their expectations are. That is our only shred of observational material that can be brought to bear on the solution of the problem.

So then the idea is to use available data on expectations and anticipations to validate various models of formation of expectations, and beyond that really to use these anticipatory data to construct appropriate models?

Yes, with feedback in both ways. I think that's the important step to take, and I think it is a very straightforward procedure.

Group photograph: Winter 1947, University of Chicago. Front (left to right):D. Patinkin, Sonia (Adelson) Klein, and Estelle (Mass) Werpel. Rear (left to right): E. Boorstein, D.H. Leavens, L.R. Klein, G. Cooper, T.C. Koopmans, H. Rubin, G. Perazich, J. Marschak, J. Hartog, T. Haavelmo, S. Schurr, Selma (Schweitzer) Arrow, and Gertrude Nissenbaum.

Before settling down in Philadelphia, you spent a few years in Oxford. What were those years like?

Oxford was a very interesting, and in many respects, productive episode for me, although I didn't stay long enough to get fully involved. When I went to England in 1954, econometrics hardly existed at Oxford. Champernowne taught statistics and some econometrics, but theoretical and applied econometrics were not in favor. John Hicks was certainly sympathetic and interested in these problems, but his interests and Roy Harrod's interests were more in economic theory. In London there were a few people that had some interests in econometrics, particularly Bill Phillips who was there at the time. In Cambridge the group around Dick Stone and the Department of Applied Economics were working on some theoretical and applied problems in econometrics. But Oxford was in many respects far behind. There was some activity in Manchester and Birmingham too. Econometrics had a place in England, but the field was considerably less active than in the United States, and Oxford was behind most of the other places in England as far as specialization or interest in econometrics was concerned.

In Oxford I was at the Institute of Statistics, and essentially I was given the green light to do what I thought could be done within the confines of the Oxford system in teaching, attracting attention in seminars, and doing research activities in econometrics.

There were two things of unusual interest. The Oxford savings surveys were patterned very much after the Michigan surveys, which I had just left. That meant we had an interesting body of data to use to look at problems of estimating saving functions and some of their special aspects. Generally, my mandate was to use the survey technique in econometric analysis. But there was no macromodel building for the United Kingdom, and we started at the Institute of Statistics on a project for an Oxford model of the United Kingdom. That went very well. The data were sparse and not as good, at that time, as the American data, but all the support that was needed was given. We had a lot of hand computer support, and the university computing center was just getting established. We could use whatever hardware was on the premises, but it was very primitive and kept in conditions that were not ideal for work.

Who were some of the people who worked and interacted with you when you were in Oxford?

There was a famous Oxford scientist who was interested in computing—Dorothy Hodgkin, a crystallographer. Her interests and ours coincided, so many times we would pool resources and information in order to get some computer power out of Oxford. Colin Clark was the director of the Institute for Agricultural Economics in Oxford at the same time, and he continued his interest in applied econometrics, but he worked mainly on problems on the United States and not on the United Kingdom. I did a little bit of consulting and talking with him about his U.S. work. Peter Newman was working for him at that time, and I had a very good interchange with him on econometric problems. This work kept me tuned up for following the problems of the U.S. economy.

I think one of the main developments at the Institute of Statistics was that Jim Ball appeared as a student from Queens College, Oxford. After he took his Oxford exams for an undergraduate degree, he came to work with me for a year at the Institute of Statistics. We worked together on the first version of the U.K. model done in Oxford.

The problem with that model was that we tried to do some innovative things by using whatever quarterly data were available and by not seasonally adjusting the series before they went into the model equations. We included seasonal variables as explicit indicators. We didn't try to estimate the social accounts from the indicator data, but we tried to estimate the model directly from the indicators of items in the social accounts, such as industrial production and profit reports. I think we did a fair number of interesting things, but that model didn't hold together well enough. I am sure that if I had stayed in Oxford the kinds of models that we have now in England and all the other industrial countries would have evolved. Nevertheless, that work was an interesting exercise for me.

I made a number of acquaintances with British economists that have survived over many years. Also, Oxford in 1956 was the scene of my first meeting with Michio Morishima. He came to spend the year at Oxford after being in rather close touch with Ursula Hicks and John Hicks. When he came, as a result of discussions with the Hicks, I saw a great deal of him. We had corresponded, prior to his arrival, on problems with the Leontief system, but the first face-to-face meetings and lengthy discussions occurred in 1956 when he came to Oxford. Looking back, the Oxford years were very nice, and quite a few interesting things happened.

Back in those days at Oxford, what were the computer facilities like?

At that time the main computers were punch card computers—to a large extent tabulators. The first electronic components were just coming forward. The tabulators of IBM were electromechanical but high speed in comparison with the desk machine. Then a new generation of computers came on the scene. We soon got the new vintage of equipment in Oxford.

In Michigan, the savings surveys had been almost entirely automated with the then prevailing IBM equipment. At the time I left Michigan and went to Oxford, we were just beginning to use the computer on the Klein–Goldberger model. Indeed, right after I left, Artie Goldberger did his thesis work, which was a large-scale simulation and multiplier exercise, with that model.

In Oxford, we used the computers for tabulating survey results, but we also used it for econometric estimation. I remember some real problems in those days at Oxford because the climate was fairly damp and wet. We had to keep special heaters under the equipment so that it wouldn't collect too much moisture for efficient functioning of the electric circuits or for feeding of cards. We had a lot of problems, but we were able to do some elementary programming.

Peter Vandome, who worked with Jim Ball and me on the Oxford model, had come from Cambridge, and he was interested in computer work. He did excellent work in the early days in harnessing whatever computer power we could get to do our calculations. We now do calculations from start to finish—data management, estimation, model solution, simulation, diagnostic checking—without human hands ever touching the system and without ever intervening. In those days, each piece of the total effort had to be done first in a modular way as a separate step and then put together. It was an interesting period when the computer was just beginning to be used for economics.

During your Oxford years, you were also pretty busy with your research on econometric methodology.

In this period I gave lectures in econometrics at Oxford. I did some tutoring for the colleges and worked on problems at the Institute of Statistics, but I had time to do two or three theoretical things.

In that period, I worked on the problem that was originally associated with Hans Theil's introduction of the two-stage-least-squares (TSLS) estimator. There was little appreciation of the meaning or, let's say, the motivation for the two-stage least-squares estimator. I motivated it in a way that was slightly different from Theil's. I motivated it in terms of instrumental variables. I found that approach to be very informative. While I was in Oxford, I met Theil at a conference in Paris, and I talked with him about this matter. I found that he had not been thinking of the two-stage least-squares estimator along the same lines, but I felt that this would be a nice way of looking at the problem.

It was really a carryover from the work at the Cowles Commission, where we knew that many estimators were variations of the instrumental variables approach. I could easily show, in detail, how the instrumental variable interpretation of the two-stage least-squares estimator worked out.

At the same time I became very interested in another concept that was a carryover from the Cowles Commission work, namely, why does it become more efficient, in a statistical sense, to impose valid a priori restrictions on an econometric system? There I found a very interesting analogy to something that I had learned when I was a graduate student working as an assistant for Paul Samuelson. I also learned something about the analogy from classes at Harvard with E.B. Wilson, the celebrated physicist, mathematician, biologist, and classical scholar. He taught economics in the department at Harvard. The analogy was that in consumer behavior, as we impose more and more restrictions on a system, the diagonal elements of a matrix that gives the elasticities get smaller and smaller.

Samuelson had an interesting idea called the Le Chatelier–Braun principle. As one borders certain kinds of matrices with more and more elements there is a systematic change in the diagonal elements of the inverse. These diagonal elements were interpreted as elasticities in consumer theory. In terms of econometrics, conditions on the likelihood function produce a similar structure. I observed that if we border the likelihood function with more and more restrictions that are valid then we get smaller and smaller terms in the diagonal of the inverse which are indicative of the variances. That provided a very interesting analogy with the theory of consumer behavior as it was worked out by Samuelson. This turned out to extend some work that I did in a class paper with E.B. Wilson, and he had liked the concept in that context.

At the Cowles Commission we used to say if one has more information, mainly nonstatistical or other external information, then one narrows the range of parameter variation that is due to sampling error. The estimated variances should be lower. This was something that we took for granted, but there had never been a formal demonstration. What I was able to show is that when we border the matrix with more and more valid restrictions, not only do the diagonal elements of the inverse change systematically, but an entire quadratic form associated with the diagonal terms changes systematically. That quadratic form was nothing other than the same form we use in standard-error-of-forecast calculations. I thought that I had a rather interesting kind of demonstration of correspondence between some ideas in economics and in statistics. Samuelson had proved the relevant bordering theorem for diagonal elements but not for quadratic forms; the extension was fairly straightforward.

Those were two problems that I worked out during this period in Oxford. Also in this period I had many discussions about the Phillips curve with Bill Phillips and others. In fact, the Phillips curve is very close to ideas that I had used in order to close the Keynesian system for the determination of absolute prices and wages. It was also very close to the wage determination equations in Tinbergen's models of the 1930s. I think Phillips put the idea very interestingly, but in many respects it was a complete analogy of what Tinbergen had done and what I had done in terms of determining wage rates and the price level in Keynesian type systems.

I worked on these problems quite a bit with Jim Ball. Jim Ball, Peter Newman, and I also discussed in those Oxford days a great deal about growth theory and growth models. In those discussions I could see the idea of stability of major ratios as limiting conditions in economics, such as the saving ratio, the capital-output ratio, and the wage-share ratio. I found, indeed, that if we put enough of these together we can construct a total growth model of the economy—just in terms of stable ratios. We didn't do that during the days in Oxford, but at least the ideas were formulated. When I left Oxford for Pennsylvania that was one of the first problems I worked on.

So the work in Oxford with sampling surveys (in particular, the savings surveys), model building, various theoretical problems, Phillips curve problems, and growth problems was, in my opinion, very fruitful.

Did your paper on the interpretation of the two-stage least-squares as an instrumental variable method come out in the late 1950s?

Yes, I worked out the idea probably in 1955 or 1956. I came to Oxford in 1954, and I met Hans Theil in 1955 at a Paris conference. I had worked it out, and asked him about it. He said that interpretation seemed to be valid. He had interpreted TSLS as an Aitken estimator, but it was an unusual Aitken estimator because the sample was not made up of data points in the usual way. I used the sample in the ordinary way and liked my approach better from a pedagogical viewpoint. I simply said that the first-stage regression on reduced forms was linear combinations of predetermined variables, which were indeed used as instruments in the second stage. Certain identities and orthogonality properties of least-squares estimators made the second-stage calculations the same whether as a second-stage regression or as an instrumental variable estimator. That was essentially what I was able to do. My way of looking at the problem made very transparent which was the first-stage regression and which was the second-stage regression.

That paper was published a year or two later in a rather obscure journal in Italy. I sent it there because the editor had asked for some contributions in order to get his journal started. It was a journal that was run on a personal basis. When the editor, who devoted a lot of care to it, died, the journal stopped.

You mentioned that there was some inkling of this relationship between TSLS and instrumental variables in the discussions that went on in the Cowles Commission.

That was the interesting thing about the work at the Cowles Commission. There was an oral tradition at Cowles because we had a lot of seminars. Many of the ideas talked about at these seminars became accepted techniques or analytical arguments. In many respects, if you look at subsequent developments in econometrics, you might be able to say that such and such an idea was really a part of the oral tradition at Chicago at that time. One could also say they were somewhere in Herman Rubin's thesis, which had a lot of these Cowles results. They may have been obscure or certainly not published, but nevertheless they were there all the time.

Also, in the development of the paper by Anderson and Rubin on limited information maximum likelihood (LIML), at some stage I would presume the two-stage least-squares would have come up—as some intermediate step towards getting at the LIML.

We may have thought along those lines. We talked a lot about instrumental variables but we didn't think specifically along those lines for the Theil-type estimators. After Koopmans first met Theil abroad and came back to America, I had a conversation with him. He found the fact remarkable that one could consider the characteristic root of the limited information moment matrix as taking on three values: zero, one, or the optimal value that maximizes a certain likelihood function. Corresponding to each of these three values were different well-known estimators, and two-stage least-squares was one of them. People looked at them in that way, and it was hard to see why it did what it was supposed to do and why it was consistent. The Aitken estimator approach that Theil took somehow wasn't quite transparent. I thought about all these discussions at Cowles, I am sure, when I took up the problem of interpretation of TSLS.

There was another estimator that we had interpreted at Cowles as an instrumental variable estimator. It was the Wald estimator that split the sample into two or more different parts. In older regression analysis or correlation analysis, that was called the method of subgroup averages. One finds enough averages in the sample to determine the unknown coefficients of linear equations by making the lines go through the common point of those averages. The Wald technique of fitting a bivariate regression to sample averages of both variables is an instrumental variable estimator with the instruments taking on certain zero-one values. This fact was well-known at the Cowles Commission, and I found it very helpful later when I was in Oxford thinking about these problems. This is probably what led me to the instrumental variable interpretation of TSLS. In many respects we were using the instrumental variable method in several different modes, although we didn't realize it. It was probably the unifying theme to different estimators. Arthur Goldberger later showed that the limited-information estimator was a particular instrumental variable estimator, and I think that somewhere in the discussions at the Cowles Commission such an idea came up, although it was not made explicit.

Was it during this time too that you worked on distributed lags?

I got interested in that problem in Oxford when I reviewed Koyck's book on the geometric lag distribution. After reviewing that, I thought about the problem some more and wrote a paper on the consistency and likelihood properties of the Koyck-type estimator. I left Oxford one summer and went to Yale to spend some months at the Cowles Foundation. I had some discussions there with Roy Radner about properties of estimators and some ideas emerged that enabled me to show how you could estimate the Koyck-type lag from the untransformed data, that is, without the partial first difference type transformation, and get straightforward consistent estimates of all the parameters. That was a useful and interesting paper for me. It was conceived in Oxford because I was reviewing Koyck's book, and then I polished it up when I visited the Cowles Foundation.

From Oxford, when did you move to Penn?

In 1958. At that time Pennsylvania had just gone through an educational survey in which it decided to examine individual departments, one by one. Certain key departments were targeted for beefing up and expansion, among them economics. This was a place that didn't have a good grounding in econometrics, and the Economics Department was just right for a transition. A number of very distinguished people were retiring who had done excellent work in more traditional economics of the pre-war type. Irving Kravis, Irwin Friend, Dick Easterlin, and Sidney Weintraub were the leading people here, at productive stages of their careers, wanting to use the resources for expansion and to take advantage of this wave of retirements. When I visited Yale from Oxford in the summer of 1957, I made a trip here and talked to Irv Kravis and others. I decided then to go back to Oxford for another year and after that year to come to Pennsylvania.

Here again the situation was an open ticket to develop econometrics. That really was my assignment. As in Oxford I had clear ideas about how to do this, and when I came back to this country to live permanently in Pennsylvania, I got a research grant from the Rockefeller Foundation at the very beginning. It looked like a big grant then, but it was rather small by present standards, to do research in model building. I wanted to return to American modeling, to take up a lot of the issues that had come up at various times. One of these was to use sample-survey data for indicators of consumer attitudes or business indicators of investment intentions. These were ideas carried over from both Ann Arbor and Oxford. I also wanted the system to be quarterly, because quarterly data were then becoming available on a broader scale, and we were getting more interested in short-run stabilization. I wanted all the accounting identities in the system to hold precisely in nominal values, while the system itself would be specified in real terms. That was a defect of the Klein–Goldberger model. The identities for that system were in real rather than nominal terms. All these pieces of model building that were not done properly in previous attempts should now be put right. When I got to Pennsylvania I started teaching statistics and econometrics and I went right to work on the problem of recreating an American model.

The university here had the first large-scale computer—the ENIAC, in the 1940s—and had later-generation equipment from UNIVAC, but there wasn't much software for econometrics. We did our work partly with hand computers and partly on UNIVAC. We did most of our work by hand, except the work with model solutions, which was done eventually with UNIVAC.

One day I had a telephone call from a friend whom I had known in Canada when I was there in the summer of 1947 to build a Canadian model. He said that he had an unusual student who was just a super computer expert on econometric techniques. His name was Morris Norman, and I arranged for him to come here on a scholarship in our graduate program. With Morris Norman, Ross Preston, George Schink, Michael Hartley, Tom Cooley, Chris Higgins, and Paul Taubman, we set out to deal with the problem – how can you harness the computer for the needs of the econometrician? Morris had the main programming instincts, and the others worked with him. Many of the original programs for data handling, estimation, and especially for model solutions were written here.

Was it around this time that you developed the first generation of your U.S. quarterly model?

Yes, we built it to see how it could be used. The Joint Economic Committee asked for an analysis of the inventory cycle or more general inventory-induced fluctuations. That was one of the first steps, and Joel Popkin worked with me on that.

When the Kennedy Administration began to function in terms of economics and consulted economists for their analysis, we thought it was an opportune time to prepare fairly regular forecasts extrapolated from the system. We used to send them around to friends and people in the administration.

When was Wharton Econometric Forecasting Associates (WEFA) born and under what circumstances?

One day some economists visited from General Electric and said they had an idea. They knew I was interested in model building and related problems. They had ideas about data banks and about model building with the data from the banks. Their idea was for us to provide their corporation with results, and I thought that the proposition was interesting. Another day, an economist, a former student, from Standard Oil of New Jersey asked if I could help them build a model for forecasting in their economic research department. Still another day, an IBM economist came with the same request. They already had a rudimentary model and wanted me to look it over. I started thinking about this. Many large American corporations were individually starting their own projects. Wouldn't it be more sensible if we were to form a small consortium and pool resources to do everything here?

I had lunch in New York with economists from GE, IBM, Standard Oil, and Allied Chemical. We decided it looked like a good idea. I came back here and talked to Willis Winn, the Dean, about it. He liked the idea and said he would back it up. We wrote to two or three other companies. We had the original group (without Allied Chemical) and the John Deere Co. I had written to Lester Kellogg, who was then the economist there. One of the people from GE knew the Bethlehem Steel economist and he came. I also asked the Mellon Bank to send someone. The Mellon Bank was in for a short time but then dropped out. The original five were IBM, Deere, Bethlehem Steel, Standard Oil, and GE. We held meetings here every quarter. We put models together and discussed tables and results. We sat in a small Wharton seminar room and looked at the results four times a year.

That was the start. Then, by word of mouth, people heard about us from other companies and asked if they could be included. The project started to grow on its own momentum, and we had probably close to a dozen participants in a short time. I went to Carnegie-Mellon one day for a lecture, and I met Leonard Silk, who was then an economics editor for *Business Week* magazine. He said, "I understand you are providing information to a few companies." And he said, "You can't do that confidentially in the university because everything has to be in the open, everything has to be made available to the public at large. Why don't you make it available to *Business Week*? We will write up an article and present your results. In that way, you will not have done any confidential or secret research." I said yes, that suited me. Once our work was written up in *Business Week*, people came from all over. The project grew very rapidly. It grew so fast that we couldn't manage it between classes, so we decided to form a corporation – WEFA. F. Gerard Adams and Michael Evans of the Wharton faculty were participants.

That was how WEFA started, but there was another motivation for its birth. When I first came to Pennsylvania, I had the small grant from Rockefeller and participated in a larger grant from the Ford Foundation for five years of econometric research. With Gerry Adams I established the Economics Research Unit (within the department). We also had a grant from the National Science Foundation. But I could sense two things about those grants. One is that they were to be regarded as seed money by the donors and that they were not going to be repeated or lasting. Secondly, the Ford Foundation would probably not want to keep spending money on economics to the same extent. Of course, it took longer than the length of those grants before the Ford Foundation pulled back from supporting economics at the level it used to, or before the National Science Foundation had trouble supporting the social sciences. But I could see the lean days coming – particularly regarding activities that would need repeated support year after year. So I concluded that we must develop our own sources of support. When I had the idea of an econometrics consortium, I said that we would provide business with forecasts if they would provide us with money to plow into our research efforts, particularly to support graduate students. There was a quid pro quo arrangement with the industrial and the business sectors. We collected money from them, and we supported about fifteen students in the pipeline. We were able to carry on research and to develop methods for computerizing, reproducing, and streamlining the calculations and for having a better communications system. The Econometric Forecasting Unit (EFU) was established in this way within the Economics Research Unit.

So Wharton Econometric Forecasting started off as an activity in the Economics Research Unit within Penn's Economics Department. It was only later that it became a separate corporation. How about the work on the Brookings model and subsequent follow-up research activities?

At the time we founded the predecessor of WEFA, we used the money that came in to support research activities in developing new software for econometrics and to support graduate students through their whole predoctoral careers.

To my mind we probably developed the first comprehensive package for econometrics. There were other attempts in England, Henk Houthakker with Dick Stone was one of the first to use the high-speed computer for certain kinds of econometric problems. In this country, Harry Eisenpress at IBM and others were working on the treatment of simultaneous equations, both for estimation and solution.

I had a very good group of students. Essentially the first thing that we did, that I think was quite important, was to find out how to deal with big nonlinear equation systems. That worked out extremely well. It was an offshoot of the Brookings model project. It was one line of research that we pushed hard. We got a very good hint from Ed Kuh, dealing with the M.I.T. approaches through the DYNAMO software package. I am not sure of the origins, but Ed Kuh gave us the suggestion for dealing with the Brookings model, and that was the start of the widespread use of the Gauss–Seidel method. For us it was extremely favorable in comparison with Newton's methods. We then worked on putting together the estimation and simulation packages for both linear and nonlinear systems.

Other pieces of research came up at that time. There was an interest in stabilization policies, and we worked a lot along the lines of Phillips' techniques for level, derivative, and integral stabilization rules. A little later we got involved with the whole development in optimal policy and optimal control, but at the early stages we were looking at the problem only in terms of passive adaptation or reaction functions for stabilization.

We worked a lot on the concept of commodity modeling and, in particular, we used that for studying stochastic simulation problems and various rules for stabilizing commodity prices, apart from the whole problem of how to model commodity markets.

An interesting development in connection with the Brookings model project was that it functioned as a team effort in which each person on the team had responsibility for a certain piece of the model. Although we did not put together the definitive model we wanted, I think we learned a tremendous amount about model building from that venture. In particular, we developed best practice methods for parts of the economy. The work on the investment function was Dale Jorgenson's and Bob Eisner's contribution, best practice for dealing with housing was Sherman Maisel's contribution on the relation between starts and completions. We had the input of Ed Kuh and Charlie

Holt for dealing with model simulation, and we did a lot of work on production function estimation, particularly on the question of blending input-output systems with macromodels. Frank Fisher's insight was extremely important there. I believe that a tremendous amount of good came out of that project, but the main thing that interested me was the way we developed little pieces of the whole econometric story from that effort.

The model finally got housed at Brookings and, being there, we had the problems of managing large data files that covered the diverse parts of a fairly large model for its time. Gary Fromm, Jim Craig, the data manager, and Mike McCarthy conceived the idea of a data bank, which was very new at that time. It was very clumsy and elaborate in terms of modern practice, but we had a punched-card system that spread over extensive data files. According to our idea of a bank, people could make data withdrawals and deposits. The whole idea of data management, combining storage or retrieval aspects with data transformation for seasonal adjustment, time averaging, and all kinds of informational arrays, was just being developed.

We learned from the Brookings experience how to operate models, how to maintain them, and how to test them. The problem of estimation or inference, a carryover from the Cowles days, was treated quite carefully, but we realized that the degrees-of-freedom problem for full-information calculation was too formidable. All during the 1960s, I worked closely with Harry Eisenpress of IBM on algorithms that he was developing for full-information maximum-likelihood and other techniques. His work was applied mainly to the Klein–Goldberger model or updates and extensions of it. That was a very small, compact system. With the Brookings model, we did a lot of our exploratory work with OLS estimates, but we did a lot of two-stage least-squares estimation also.

I was always very impressed by the article of Kloek and Mennes on principal components. We pushed those ideas further, in terms of using principal components either as instrumental-variable estimates or for limited-information and two-stage least-squares type estimates. At the same time, we had the developing power of the computer and the ability to experiment with different numbers of principal components in seeking best combinations that gave good simulation properties.

One of the lines of development was to use the whole model solution to generate instruments with feedback into the system.

The versions of the Wharton model that followed the Brookings model used principal components in two-stage least-squares estimation. Morris Norman continued in his thesis on different estimation methods using iteration for generating instrumental variables for the system. A new generation of Wharton models was developed after the original version was turned over to the Department of Commerce because they wanted to get started in modeling and I wanted to devote my attention to the Brookings model project. The Commerce Department, instead of starting a model from scratch, took over both our model and some people who were trained here at Pennsylvania as students or visitors. Al Hirsch had a post-doctoral year and Joel Popkin and George Green went directly from their studies to work with the Office of Business Economics (OBE) on model development.

It was our arrangement with OBE that we would get them started by turning over all our files and equations to them and then cease to use that model for our other activities because they wanted something they could develop on a confidential basis for a trial period. That arrangement worked very well.

But when I wanted to make a shift to the idea of distributing forecasts through the Econometric Forecasting Unit on a much more extensive basis in order to support our research program, we started building a new generation of Wharton models. That was when Mike Evans joined the team from Brown University, where he got his Ph.D. He brought with him the model that he had developed in his thesis. For a while we made forecasts with his model and an updated version of the Wharton model but very soon merged them into a single model. That was, I guess, a second generation of the Wharton model and was different from what we had turned over to the Commerce Department and also different from the Brookings model.

It was not long before the Econometric Forecasting Unit had gotten so big that it couldn't be managed as an academic sideline. So with the permission of the University we formed a private nonprofit corporation, located off-campus, named Wharton Econometric Forecasting Associates, and the University advanced the initial capital in the form of a loan to get the project started.

What year was WEFA formally incorporated?

In 1968 the talks started, and in 1969 the actual charter was officially drawn up. Within the Economic Research Unit in the Economics Department, we first had an Econometric Forecasting Unit with some outside support. What had started out as five companies quickly became twenty and then twenty-four before the end of the 1960s. We had ample research support for our graduate students, but we were extremely busy. We did not have proper copying facilities or the complete use of the computer. Eventually those facilities became available, and we were able to distribute very informative tables. We conceived the idea of making the forecast presentation, which was a solution of a set of simultaneous equations, in transformations that displayed the same tables that the Department of Commerce produced for GNP and related components. That was all done by the graduate students that I mentioned earlier. We found it very exciting to have a meeting to discuss the forecast and to make calculations while the meeting was in progress. We were able to report the results back to the attendees. We also did one other thing—we put the system on time-sharing with a small company in Princeton (Applied Logic) and we made it available to some of our users by means of a teletype terminal. By the end of the 1960s, we had the use of the computer, the copying machine, data banks, and time-sharing via remote access. We were using consistent estimation methods, with principal components and instrumental variables. Everything was going rather well, and I think that our forecasts were extremely useful at that period. Certainly the users were very supportive of our efforts, which figured significantly in their decision making.

We were approached by two groups who wanted to form a private corporation involving the Wharton model: Mathematica, in Princeton, and the DRI group with Otto Eckstein, both of which we knew and respected. A third option was to remain independent. The University wanted to remain independent, but there was a lot of discussion and suggestions for some degree of cooperation with outsiders. Eventually, DRI's proposal to set up the Wharton model as an option in the DRI system was accepted, and they paid us a significant royalty for that.

We got started in the early 1970s operating as a private corporation, with our models distributed in batch system only, except for some remote-access time-sharing through DRI. Later on, we found out that DRI, quite understandably perhaps, was promoting its own models more than ours, so we didn't renew the agreement after five years. We had royalty money that was quite instrumental in keeping our research efforts going, but we decided at WEFA at that time to evolve our own time-sharing system.

We had one other joint venture at this time, with Charles River Associates, particularly in association with Frank Fisher of MIT. That dealt mainly with the problem of commodity modeling for the General Services Administration of the U.S. government, which had the responsibility for managing U.S. stockpiles of commodities. That association continued our basic interests in modeling. Charles River supplied a lot of the intensive market information on commodities, and we simulated the models. We also helped the General Services Administration learn the techniques of building and managing their own commodity models, as a public service. We kept this up for a number of years. We also started to get interested in the problem of state and regional modeling, and we spent a great deal of effort on modeling the city of Philadelphia. Later in the 1970s, we modeled New York City, the State of Pennsylvania, and New York State.

Eventually, one of our students, Paul Beaumont, submitted a thesis in which he modeled the U.S. economy from the bottom up, by modeling all fifty states plus the District of Columbia. Actually, that's now a new WEFA project. The products often have interesting roots that derive from research activities that took place up to 15 years ago.

When did your LINK project get started?

In 1968 another event occurred. The Social Science Research Council Committee on Economic Stability, which was the group that got the Brookings model project started, was looking for new activities. By way of brainstorming in a meeting that was talking about the problems of the United States within the world economy, it was suggested that we look into the transmission mechanism. We had in mind mainly the transmission of economic disturbances or business-cycle developments among the main industrial economies.

This discussion was a follow-on from a conference held in London a year or two earlier. It was sponsored by the committee with some support from OECD on the question of whether the world business cycle or national business cycles within the world economy had been abolished by the fine-tuning achievements of the Kennedy and Johnson Administrations. We took a skeptical view, and we held a conference entitled "Is the Business Cycle Obsolete?" We had hoped at that time that the conference decision that the business cycle was still with us (but maybe in somewhat modulated, moderated form) would generate an international research project. When we received no follow-through from some of the international agencies, I got very much interested in the idea of trying to do internationally what we had done in the Brookings model project on a sector basis.

I thought that model builders in most of the major countries could be asked to contribute the best version of their own countries' systems and then put them together systematically. I did not quite see at that time how this would finally be done, but I thought it was a promising idea.

The committee approved an exploration into this subject and a subcommittee consisting of Bert Hickman, Aaron Gordon, Rudi Rhomberg, and me looked at that problem in the summer of 1968, when I was spending a sabbatical leave partly at Berkeley and partly at Stanford. We called a meeting, supported through the Ford Foundation, of people from Canada, Holland, Germany, Britain, and Japan. It was a very positive meeting, and we decided to go ahead.

From that point forward we planned the LINK project. Through joint discussions we worked out algorithms, procedures, and designs of what we hoped to get. The International Monetary Fund (IMF) provided seed money to hold meetings and get activities started in countries that didn't have ongoing projects but were very important, and we got NSF support as well. At that point the LINK project was launched. We held our first worldwide meeting in the summer of 1969 in Japan, and it went very well.

From these beginnings, LINK has now blossomed into an integrated system covering seventy-nine developed and developing countries all over the world. What are the major developments in the LINK project over the years? Can you also give us an idea of the organizational setup for handling and maintaining an econometric system of such a magnitude?

Although it was originally conceived as an investigation of a short-run transmission problem among the industrial countries, it became clear to us rather early that the support of the IMF would be enhanced if we could treat the problems of the developing countries, which are also members of the Fund. In discussions with the United Nations' research staff, a similar idea came through that we go beyond the immediate transmission problem and extend the horizon of the investigation to a term of five or ten years, including not only the developing countries but also the socialist countries. That was a very good line of development.

We set up the LINK project with meetings twice a year. The fall meeting lasted a week to discuss the problems of international modeling, while the spring meeting was shorter and concentrated on the year's early forecasts.

Over the years, we have also gone into world commodity modeling and we have prepared models for socialist and developing countries. Originally the developing countries were handled on a regional basis, but recently we have expanded the system to seventy-nine separate models including all the major developing countries. That project has taken its own natural line of development.

The LINK project involved a new set of considerations because we had to learn to manage data that were of very different quality among countries, and we had to learn to manage very large heterogeneous data files. Also we had to solve much larger equation systems. At the same time, hardware and software improved enormously, and we are able to handle the problem and do much more than we ever thought possible. We started out with thirteen OECD countries, developing regions, and superficial treatment of the socialist countries. Now we model each individual major socialist country, each individual major developing country, and all of the industrial countries separately.

I am pleased with the project because we were able to use our meager research funds to do some pump priming in countries that never had model projects. I think that our prime case was Italy. The Bologna group got started in the LINK project, along the lines of the WEFA concept. The Italian Science Foundation was willing to provide some initial support jointly with a small LINK contribution. Now that is a self-sustaining model. The same thing happened in a few other countries.

At WEFA we got interested in the problem of modeling the Soviet Union, using grants from the United States government in the early seventies. We worked very closely with Herb Levine, Don Green, and other specialists on the Soviet economy. We asked what kind of model we could put together, and the various generations of SOVMOD emerged. We extended our efforts to other socialist countries, helped by research scholars who visited Pennsylvania from Poland, Hungary, and Czechoslovakia. We began to learn more about the nature of those economies and how to integrate them into the world system.

At the present time, one of our greatest interests is the Chinese economy. We have been helped a lot by the expert knowledge of Larry Lau, both on China and on model development. That aspect has gone very well.

Over the years through other contacts, we have developed models of Mexico, India, Korea, and other developing countries. They have been used as dissertation subjects. As I said already, I'm not so sure that's a good dissertation subject in the 1980s for a Ph.D., but in the 1960s that was a very suitable subject. We had a large number of foreign students, probably more than 50% of the graduate enrollment in our economics department. Many of them found it instructive and convenient to model their own countries or some aspect of their homelands. We have drawn upon that whole body of information in the LINK project.

Our prime case has been the Mexican model, where side by side with the development of WEFA we started in 1969 the Mexican model project that now exists by itself on a sustained basis. I have always found that case interesting because it showed that we could take the whole econometric technology—data base management, estimation, simulation, forecasting, policy scenarios—and transfer it to a developing country with effective software, hardware, and applications. In addition we've found that input/output modeling and financial sector modeling fits in very well with the Mexican model. So the development of models for third-world countries has gone along very nicely, together with those for centrally-planned economies.

Now we have a totally integrated system. One by one we expanded our coverage of the OECD countries until every OECD country is represented. Countries like New Zealand, Ireland, Portugal, Turkey, and Spain, which had been more or less on the fringe in model building, are now either completely integrated with participation from those countries or modeled at LINK Central and maintained until a resident modeling team appears. That worked out beautifully in Spain, where a very strong modeling team at the Autonomous University of Madrid has appeared. New Zealand had a long-standing strong effort from the Reserve Bank, and other smaller OECD countries will do the same. The LINK system now exists on a large scale; deals with longer-term simulations; treats the developing countries and socialist countries; has commodity disaggregation; and integrates primary commodity models.

Two major events occurred that had profound impacts on the project. The first was the abandonment of the Bretton Woods system of fixed exchange rates. When we started the LINK project, exchange rates were fixed. As exchange rates got freed up in market determination (with some intervention), we wrestled with the problem of exchange rate determination. We now have exchange-rate equations, but the data base is relatively sparse and complicated. We are still learning about exchange-rate determination, but that is not as much of a mystery or as much of a problem from a purely econometric point of view as it used to be.

The second thing that had a big effect on the LINK model was the change in oil prices after 1973. As I look back upon that episode, I think we came out well. In the first place, when we made our original commodity disaggregation in the LINK project, at least for trade, we separated out energy. That proved to be an extremely wise decision. We also separated out food and agricultural products. When prices started to move by very big amounts in the early 1970s, we had a place in the system to examine the impacts. The initial reaction in the autumn of 1973, as we contacted one LINK participant after another, was that there would be a world recession, on the order of magnitude of the last big world recession of 1957–1958. I think that we had an extremely good forecast by November 1973. Our forecast was not as good on the issue of inflation. We had estimated more inflation, but not nearly enough.

Then every econometrician, particularly within the United States, had to pay much more attention to energy modeling. The individual models said more about the distinctive influence of energy in the economy, and the LINK model showed how high energy prices affected the international trading system. At the present time, we are busy studying falling oil prices and widely fluctuating exchange rates within the system. I think that we're getting interesting answers.

What were some of the other substantive economic issues that you have looked at in the LINK project?

There are two substantive problems in economics that we have looked at in great detail. One is the rise of protectionism. We don't have any surprises, but I think we have an interesting quantification of the concept that macroeconomic results of protectionism are perverse. That's not a surprise to most economists. But the theory of protectionism was worked out mainly for individual commodity markets on a micro basis, using the law of comparative advantage. We did not superimpose those concepts on macroeconomic performance, but we have validated what economists have suspected—that overall performance would be injured by protectionism.

We started our protectionist studies seven years ago, and we have found that the very detailed country disaggregation has paid off, because many of the protectionist moves that originated in the United States are directed at individual countries. This is especially relevant for Japan, Korea, Taiwan, and Brazil and gives us a handle on that kind of problem.

A second very interesting line of analysis has been policy coordination. The LINK system is naturally set up for introducing specific combinations of fiscal, monetary, and commercial kinds of policies across countries at the same time. I think our experience with the first oil shock involved a coordinated price change that had amplified effects. We have been interested in this question of the degree of amplification of multiplier effects through policy coordination. There could be offsets or there could be amplification, but that is what the system is designed to show. We have looked at coordinated easy money, or coordinated money and fiscal policies. These have been tailored to different situations, and they are extremely interesting.

Our latest line of interest has been to do this in telecommunication experiments. A couple of years ago, we made an experiment based on the Summit meeting of the major powers by teleconference between London and Washington, in which we had LINK members at each site, in touch with a computer in Philadelphia. We simulated the results through an ongoing audiovisual discussion, on a live basis.

We later had an experiment at a LINK meeting in New York, in which we did the same thing by using written message communications under the BITNET-EARNET system of IBM, connecting multiple sites in New York and Europe. That dealt with protectionism. Our latest trial was actually done on the eve of the May 1986 Summit live from Tokyo. We had trans-Pacific and trans-Atlantic hookups with three sites, one in Zurich, one at AT&T offices in New Jersey, and one in Tokyo. That session dealt with monetary and fiscal policy coordination. It was technically successful and quite interesting.

These methods of communication, together with our interest in policy coordination, have been complementary to one another. At the same time, we are learning a great deal about transmitting information. In the LINK project, we first mailed out big boxes of cards. It was very clumsy. Then we started sending tapes. Now we have automatic transmission of data files throughout the world by BITNET and other systems, right into the central computer in Philadelphia. The system is set up so that people can, in principle, access our files almost anywhere in the world and make system calculations. We are coming, in the development of this project, very close to a worldwide economic information system with modeling and data bases. We can also talk to one another at the same time.

At WEFA itself, there is a separate but parallel activity on world modeling on a commercial basis. Can you describe this to us—as well as other worldwide systems currently in place?

With the development of the LINK system, it wasn't long before other systems developed. A commercial version of the LINK system which used scaled-down LINK models was specified for fast calculation by Keith Johnson, who had worked on LINK as a student in Pennsylvania. This has been used by WEFA for their world model. Although the WEFA model is based on the LINK design, it has been completely respecified and streamlined. It is a thriving activity, with users around the world and is a very good technical system.

The OECD INTERLINK model was developed by Lee Samuelson, John Llewellyn, and others in the 1970s. The IMF had their own systems of various types for a long time, as has the World Bank. The Japanese government, through the Economic Planning Agency, has a world model, and I think the British Treasury has a world model. There have been many other efforts, particularly at Tsukuba University and Soka University in Japan. The Federal Reserve System has a world model on which some of our former students and associates have worked.

The Common Market has brought together different models of European countries, including the United Kingdom, and simulated them simultaneously in what could be called the Euro-LINK. In South Asia and the Pacific, ESCAP is doing the same, following the lines of Shinichi Ichimura's research, which puts together a sub-LINK for Pacific countries.

There was a conference at Brookings about two months ago in which many of the world models were brought together and compared in various simulation exercises. I think that this is a line of activity that will have its own development.

In the 1970s, you also embarked on the Model Comparison project under the auspices of the National Science Foundation (NSF) and the National Bureau of Economic Research (NBER).

Yes, that's quite interesting. The National Science Foundation had many conference groups, called Committees on Econometrics and Mathematical Economics (CEME). Each group took up a special line of interest, and the group that I chaired with Gary Fromm was on comparing models. We brought together the main U.S. models and looked at common multipliers, common extrapolations, and optimal control exercises. That control exercise was very instructive in trying to design national welfare function targets and computational algorithms.

Group photograph: Luncheon at the Quadrangle Club, University of Chicago, winter 1947. Left to right: Dvora Patinkin, T.C. Koopmans, S. Schurr, G. Perazich, L.R. Klein, Sonia (Adelson) Klein, Estelle (Mass) Werpel, H. Rubin, Selma (Schweitzer) Arrow, and J. Marschak.

In the SSRC-Brookings project, the LINK project, and the Model Comparison project there has always been one guiding feature, that is, *best practice*. In intensive discussions sitting around the table to see what each other group is doing, we learned a lot about *best practice*. This was the procedure in the Model Comparison group. The results were published in several issues of the *International Economic Review* and then put together in a book by the University of Pennsylvania Press.

The Model Comparison effort has expanded. There have been Japanese comparison groups, a Canadian group, and a British group. The British group at Warwick have gone much further in automating and systematizing procedures. We dropped the original activities in the United States, but recently we called it together again and are now in the process of lining up models for uniform simulations, with uniform initial conditions and other inputs. We are comparing multiplier properties and plan to take up ex-ante forecast comparisons on standardized assumptions. We hope to decompose forecast error to see how much is due to different model structures, to different assumptions. This group meets every three or four months and is very actively engaged in achieving some degree of uniformity in procedures for analysis of model performance.

Earlier, we talked a little bit about the negative attitude towards large-scale macroeconometric models now. Would you like to elaborate on this further?

I think that there are two points. One is intellectual and scientific; the other is more personal. From an intellectual and scientific point of view, I think there is a perception that large-scale models have, in some sense, failed.

People have said they have failed to predict the effects of the supply shocks on the inflation of the 1970s, or the change in the structure of the economy. I think that these charges are not correct. I really believe the economy didn't change in structure, but that exogenous inputs changed a great deal within a similar structure. Procedures that follow such a line can produce quite good results for the period. The different inputs account for the change in the industrial composition. I also believe that when people go back to make a scientific analysis of what happened in the 1970s, they will find that the large-scale models were out in front in predicting recession and inflation. They did no worse than other approaches on the degree of inflation. I think that it will be very difficult to find an alternative approach that does consistently better. It may be that for some short episode one can find an alternative that is better, but for consistency and replication, it will not be possible to find a suitable substitute.

I think there is also a feeling on the part of some young macroeconomists that expectations have been the major element in causing the big swings in the economy. In my view, expectations are important. In the original inspiration of national macromodel building, expectations had always played a big part. The main point is that a new generation wants to treat expectations differently. As I have said before, I have the feeling that the only way to handle expectations satisfactorily is to explain people's expectation behavior by means of the best information we can get as to what expectations are and why they are as we measure them. I have great confidence in sample survey techniques, and we use them in our models. They have been investigated in Pennsylvania dissertations, and I would stick with the view that the best way of dealing with expectations is to model stated expectations as they are ascertained in sample surveys.

I am a proponent of combining different sources of information, and the information source in this case is cross-section data from survey investigations. They should be integrated within macromodels, just as I think input-output systems should be integrated. I think that basically, we are information-short, since we can neither generate as much information as we want nor use the kind of information that we would like to have. We should milk whatever sources of information we can get, rather than transform or manipulate conventional time-series samples. The best way to deal with the problem is to enlarge the sample by getting new information. That's precisely what we are doing by using cross-section surveys taken from the people who create the expectations.

I believe that the approach of rational expectations (or, better expressed, own-model-generated expectations) is asking too much of the data. It asks the data both to generate the expectations and provide the model estimates with simulation. That is overworking the data.

Now, I think that for expectations—unless we get fresh information—we have an identification problem. From an econometric point of view, we used to characterize the problem of using the same data to estimate first the variance-covariance matrix of observation error and then coefficients based on these as eating one's own tail—to make the sample try to do both things. I think that the people who want to use the sample to generate expectations and then estimate the model are also eating their own tails. They are not getting new insights as to how expectations are formed—they are assuming that their methodology is correct without validating that assumption. Many people seem to like that procedure, but I think it faces a fundamental problem.

There's little attention paid to whether they are right or not, only to the fact that it is a procedure that makes expectations endogenous. I deplore the willingness to make very strong assumptions about the way expectations are formed, simply for the sake of getting very definite analytical results. As I said earlier, people want manageable problems, problems that can be worked on with their own signature on authorship. It is very important for our academic system's rewards. In the earlier days of econometric model construction, there were team efforts, and people were satisfied to be members of the team. We had a tremendous team at the Cowles Commission, and everybody was happy to be a team member. I think I detect now a great unwillingness of careerists to enter team activities. Joint articles bring a smaller reward than does sole authorship. Articles in books and collections have lesser rewards than solo articles in refereed journals. Without regard to how important or innovative things are, things have to have a certain style for acceptability, and this has pushed many scholars into pursuits that they can manage themselves.

There is a tendency now to look at small models. With the advent of the personal computer, between the wall socket for electric power and the desk chair, a researcher has a private empire. It is possible to download from major data bases, to create new data bases, to get easily acceptable software, and to have all the econometric tools right at one's fingertips. It is a choice between a small manageable PC system to try out estimation techniques and test small-scale economic ideas, and membership on a research team that deals with details of a big system.

There is definitely a class of problems that is best investigated with small systems under the popular expression "learning to walk before you run." That makes good sense. But not all the truly interesting economic problems can be handled in small systems. I feel that my present interests—protectionist measures, coordinated international policy measures, the intricate feedbacks of energy effects within a country—are all missed in small aggregated or partial systems. There's no commodity detail, there's no country detail. You cover up many of the interesting points while working within the framework of the small system.

In addition, I think that any small system eventually has a breakup, when something else—the third factor—goes wrong. Economists like simple things and they like easily exposed things. They like to go to the blackboard to draw two-dimensional diagrams to show their students how things work, but often you can't really describe economics that way. There are frequently periods when things seem to go very well according to these simplistic rules and then suddenly blow up because the third factors are not in place. In a bigger system they can be accounted for.

I think that much of the simplicity of monetarism in very small systems blew up in the face of the energy and commodity price changes in the 1970s. Now monetarism is having a difficult time with financial deregulation. Velocity has not been steady. A larger system is able to incorporate effects of the exogenous swings in cartel-based prices or weather disturbances or critical international strategic changes. It can also build in institutional changes, like those that we now see in money markets.

I think that economic life is necessarily complicated, detailed and explainable only in terms of a big system. I like small systems, but purely for methodological and pedagogical reasons, not for the real thing.

So, in the next decade, how would you like econometric research to proceed, given all the problems with I guess what is fashionable now in terms of doing rational expectations. For example, there are problems with sample-survey data on expectations. Should we not address these problems too?

My feeling is that it is better to improve the sample surveys than to ignore them and go for the simple mechanistic model generation of expectations. My preferred direction is expensive because good sample surveys are costly and cumbersome to carry out, but there has been very good progress in the post-war era in improving sampling and survey techniques.

There's another issue connected with large versus small systems, concerning the methods of inference. Complicated or sophisticated methods of inference can be studied rather easily in small systems. That was the procedure we used at the Cowles Commission when ideas on simultaneous equations were put forward. One issue is that there is a degree of freedom limitation. Many economists have called some of their estimates maximum likelihood, but I don't really believe they are because they truncate or decompose their systems by declaring something to be exogenous when it is not.

Secondly, there is a very big problem concerning data accuracy, tied up with the whole question of observation error. The end result is that data are frequently revised. The United States national income accounts are revised every month and then on a comprehensive basis every few years. Every decade there is a benchmark revision on a census basis. Given that frequency of revision, we have two ways of proceeding. One way is to have a very small compact system that gets reestimated every time new information becomes available. With present hardware and software, that's not difficult; it can be automated. If you say that it is more important to capture economic substance in big systems by taking disaggregation to great detail, then it is cumbersome to reestimate frequently by sophisticated methods. With the sophisticated methods, changes in one part of the system may cause changes in estimates of other parts of the system. In order to have something that is quite practical and relatively easy to use, I believe that we must sacrifice consistency, or sometimes efficiency. More often I think that it is mainly consistency. There may be gains in overall efficiency through being able to handle systems more readily, and that really explains the reason why so much econometric work now does not proceed along the equation system line, for estimation. Simpler procedures are used because of the frequent need for reconsideration and reestimation as the data change. As the data change in small portions, it is easier to reestimate that portion. There is trade-off between consistency and efficiency.

And perhaps, also, sensitivity to measurement errors. The more sophisticated techniques may be more adversely affected.

I think that really accounts for the fact that most mainstream large-scale econometric projects don't go as heavily into the methodology of estimation as do small compact systems. In all these lines of research there are tradeoffs, and my preference is for getting the economic detail.

On the question of the rule of parsimony, some people would argue that the small system that produces a given set of results is a desired system. That has been interpreted as a preference for the smaller system that produces a given degree of accuracy in GNP, inflation, interest rate, and unemployment forecasts.

As I said in connection with the contrast with small VAR models, I have a different approach. I say the largest system that we can manage quite easily from the point of view of data and computational management and that doesn't deteriorate in these designated macro magnitudes is the one to use because you get the same degree of accuracy in the macro magnitudes, and, in addition, you get many vital bits of information on other parts of the system. I think there's an overemphasis on a few macro indicators because most users have only a moderate interest in particular details. Once a special detail becomes important in a general sense there is a much bigger interest in it. I find it is not very satisfactory that every time a problem arises in a new market, or a new nook and cranny of the economy, we must estimate a new model. I think we need a large overall model system that maintains a given degree of accuracy in the main macro magnitudes and then provides ability to deal with specialized factors at the same time. It means we always have a system on line that can be accessed and driven to some kind of first response within twenty-four hours.

I'm somewhat intrigued by how one might be able to deal more explicitly with measurement errors because this problem seems to be universal and even more intense especially when you are trying to model developing economies.

That's very important.

And perhaps the non-Keynesian nature of the system should be taken into account so that some respecifications may also be necessary and, I guess, a more conscious attempt to address the institutional aspects in developing economies. But for measurement errors, I wonder if the preliminary figures for national income accounts components could be used to model those measurement errors?

Yes, the discrepancy between preliminary figures and the final figures should be indicative of error. If the information basis improves we may get some new results that enable us to say something about the covariance matrix of measurement errors. That would be the kind of information that should be used. I see that as a very positive development—to try to sort out the two types of errors. That requires practical research on getting information on the degree of measurement error and theoretical research on the best methods of estimating the models that have this dual stochastic structure.

How about the specification part in terms of how appropriate the Keynesian system might be, for example, for developing economies?

That's an interesting question and I have long thought about it. I think the best way to deal with that problem is for economists to spend some time in other kinds of economies to try to find out how decision-making goes on or how the system works. From my own perspective, the supply-side structure, the input-output structure, the availability of resources, the availability of imports, strategic imports, the degree of control that monetary authorities have over money supply and credit conditions are all specific aspects that should be modeled right into the system. In addition to the dual production structure, we need to develop an interpretation of how some of these things work in the third world. We should try to draw upon their resident knowledge in specifying models. I think developing countries do have a stable saving or spending propensity, but they don't have some other things that we take for granted in the advanced industrial economies. I think that the decision-making process is often not a close marginal calculation of rationality that we would impute to people in our own country.

At the present time, it is important to model explicitly the various dimensions of the foreign debt problem for developing countries. That means that all the accounting identities must be in place, that serving burdens are evaluated, and that the relationship between money supply and capital inflow be taken into account.

Would you like to round up the interview with your thoughts on your current and future research activities and your feelings about future research directions in econometrics?

I feel that there is one problem which I have some responsibility for. It is a very specific problem—the idea of making subjective adjustments to econometric models through the application of changes in constant terms or add-factors. On various occasions, I have tried to explain why this is necessary, and why systems fitted to small samples lose extrapolative power very rapidly.

One line of research that I have going at the moment is to try to make that adjustment process more objective. Particularly, I have tried to make it in a replicable form so that anybody else coming to use the model would get the same adjustments. My present approach is to construct simple time-series models of high frequency data based on latest information, by days or weeks or months—for use in somewhat lower frequency macromodels. I propose to use the time-series estimates for the very short run and to force the macromodels to agree in selected dimensions to time-series estimates for periods up to six months in duration and then carry on after this period by themselves. In this way, by forcing the macromodel to duplicate this very short-run time-series-based model for six months, we effectively decide upon the adjustment factors, at least the strategic ones.

The motivation for this comes from the time-series work of Granger and others indicating that, for very short-run analysis, the serial effects are so high that the time-series model works well for the main aggregates, or even beyond the main aggregates. So I am using that information as a way of combining the high-frequency time-series models with a lower-frequency structural model. I hope that will lead to making the adjustment process more objective. I must add that I have also been following the research reports of the Federal Reserve Staff, who are investigating the same lines of model adjustment.

That is one line of research. I feel I want to clear up that one part of econometrics, because in general I am not very happy with subjective approaches and I want to see as much objectivity put into a statistical procedure as possible. I think that this is a very good way of doing it.

With the LINK system, I have been very much interested in various kinds of simulations on protectionism and coordination policies, but the one thing that I think is really more important for a society is to understand the arms race and disarmament. Models or scenarios of the economics of disarmament and the arms race among the big superpowers should be studied in an extended LINK system. I find that extremely interesting right now.

Another line is to get more financial detail into models, into models of developing and socialist countries, and into the LINK system in general. I feel that our treatment of exchange rates has been imperfect because we have not dealt fully with international capital flows in financial models. Within the developing and socialist countries, too little attention has been paid to financial factors. So that looks like a promising line of research.

These are some of the things that I have been working on recently and probably will pursue for some time, until I get some new results.

In summing up my work, it has really been an attempt to make received economic analysis useful in decision-making or to put empirical and realistic content into economic analysis, based on a feedback from the world, as we have observed it, into the formulation of economic analysis. This economic analysis should be used in order to guide economic decision-making. This has been my objective for forty years. We thought it would be much easier, at the beginning—in the 1940s, but it is not all that easy. Yet I would not say, as many economists do in their presidential addresses, how overwhelming problems are and how little our subject helps in providing answers. Emphasizing how little we know, prevents us from giving advice for decision-making. I would rather approach the issue in a more positive way and say yes, we need to know more, but estimate how much more and what kind of things in order to offer better advice. Decision making subject to econometric estimate of uncertainty is the most important issue for which we should be developing methodologies.

NOTE

1. Prize citation in the Alfred Nobel Memorial Prize in Economic Sciences, 1980.

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