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THE ET INTERVIEW: PROFESSOR MARC NERLOVE

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As the field developed, econometricians tended to become more specialized in specific areas like labor, empirical macro, time series, finance, and so on. Even thirty or forty years ago, when specialization was not as widespread, very few researchers stood out from the rest of the profession as scholars who left their imprint on very different areas. Marc Nerlove is among those very few whose scientific writings touched so many different areas of econo-

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metrics. His doctoral dissertation on estimates of agricultural supply elasticities, completed at the Johns Hopkins University in 1956, was a supreme example of the essence of econometrics, namely, the fine interplay of economic theory and statistical methods. Among its key ingredients was the link between a theory of adaptive expectations and distributed lag regression models to formulate and estimate dynamic price responses of agricultural supply. His research on returns to scale in electricity demand set similar high standards of empirical economics, this time exploiting duality theory to uncover parameters of interest. Jointly with Pietro Balestra he also made seminal contributions to econometric methods of pooling time series and cross sections. Marc did more path-breaking work in areas as diverse as spectral analysis of time series, seasonal adjustment, estimation of production functions, log-linear probability models for qualitative response analyses, and the analysis of business survey data. His contributions figure prominently also in other areas, such as economic demography, development, and labor economics.

In 1969, Marc Nerlove was awarded the prestigious John Bates Clark Medal by the American Economic Association for his achievements. Twelve years later, in 1981, he was president of the Econometric Society. During his distinguished career he held appointments at the University of Minnesota, Stanford, Yale, Chicago, Northwestern, and since 1986 has been University Professor of Economics at the University of Pennsylvania. He is also a fellow of the Econometric Society, of the American Statistical Association, and of the American Academy of Arts and Sciences, and a member of the National Academy of Sciences. At the Second World Congress of the Econometric Society he delivered the Henry Schultz lecture, and more recently he presented the F. V. Waugh Memorial lecture at the American Agricultural Economics Association inaugural in 1991. Marc also holds an honorary doctorate from the University of Mannheim.

Very much concerned with the quality of applied econometric research and applications in different areas, Marc has been a consultant to the World Bank (1979–1985), to the International Food Policy Research Institute (1981–1991), and to the Rand Corporation (1959–1989), among other appointments.

Anyone who has had the pleasure to work or interact with Marc is struck by his unique talent as a researcher but also by his unique gift for languages. Fluent in Portuguese, German, French, and Spanish, he is a world traveler par excellence. There are probably few countries left on this globe where he has not crossed borders.

ET Interviews provide readers insights on the developments in econometrics beyond what appears in scientific papers. On a Saturday, a bright and sunny spring day, I learned about many things . . . including Marc's very good taste for wine and on how to blend this into interesting research.

From an early age on you were exposed to the wide diversity of research and intellectual stimuli in an academic environment, since your

father taught at the University of Chicago Business School. Would you mind sharing with us some of your family background?

My father, Samuel Henry Nerlove, was born in Vitebsk, Russia, in 1902 and brought to the U.S. by his parents in 1904. He entered the University of Chicago in 1917 or 1918 and did his undergraduate and graduate work in what was then the Department of Economics and Commerce under Paul Douglas, Jacob Viner, and John Maurice Clark. He was appointed to the faculty around 1922 and to tenured associate professor in 1928. In that year, economics and business were split. My father went to the newly formed business school. Paul Douglas stayed in the economics department.

My mother was born in 1907 in Cambridge, Mass. She came to the University of Chicago to teach in the School of Social Service Administration and to do psychiatric case work at the University hospital. She and my father were married in 1932, at which time the University's nepotism rule forced one of the two to quit. In those days, of course, it was she, although she always maintained an active intellectual life and returned to her profession in the 1950s.

One of the most important influences my father had upon me had to do with another aspect of his career. At Chicago he taught, among many other things, what today would be called finance. As businesses, banks, and insurance companies failed in the depression of the 1930s, my father was highly critical of the way in which the courts arranged their reorganization. Matters were particularly serious with respect to life insurance and annuity companies. My father's criticism was open and he wrote several lengthy letters to the *Chicago Daily News*. One day in 1933, the exasperated chief judge of the district bankruptcy court called him and said, "OK, Sam, the next one is yours!" So my father became the trustee of a large bankrupt midwestern life insurance company, a more than full-time job in addition to his full-time professorship. The company held mostly foreclosed midwest farm mortgages. The farms were operated by their former owners as tenants and my father used to visit many of them (in the interest of his policy holders) to keep the rents flowing. When he was home for dinner, which was not often, we used to hear all about the farms and farmers. So you could say that although I was born and grew up in Hyde Park around the University of Chicago, I have agriculture in my "blood and bones." Certainly, it was only natural that I chose to work on agricultural supply for my Ph.D. dissertation.

What drew most of your interest and attention in your early schooling, say high school?

I was interested mainly in astronomy and physics and, through these, in mathematics, which I began to take very seriously after reading a marvelous book by Courant and Robbins [5] (published in 1941 – although I didn't start reading it until 1946!). Economics didn't interest me at all until I read von Neumann and Morgenstern [30] a couple of years later. Then I proceeded to devour my father's library under the illusion that I'd found the key to all economic behavior, maybe even all human behavior, in von Neumann and Morgenstern.

You wrote and published an essay on the theory of games in 1952. What made you decide to work in this area and, I guess, abandon it later?

This 1952 essay was my first published paper and was written largely during 1950–1951, a period in which I was wildly enthusiastic about game theory. As I learned more about economics, however, I saw how limited game theory was as either a theory of rational behavior or as an explanation of economic phenomena. Besides, in contrast to the 1980s and continuing today, in which game theory is a "go-go" field, it wasn't going anywhere at all then. But I think one day I want to go back to it. It's certainly a very beautiful field, and empirical work is too hard and grubby for all but the very young and energetic.

Your father published an interesting monograph on corporate incomes and their disposition during the decade preceding the Great Depression using accounting data from firms to study what one could call nowadays "stylized facts" (S. H. Nerlove [24]). Did your father's study and his research interests stimulate your own interest in empirical economics?

Although my father's work was pretty influential in the finance literature during the 1930s (see Crum [8]), it had no real influence on me, *per se*. Of course, indirectly, his whole approach to economics was of tremendous influence. He was a founding member of the Econometric Society in 1932 and believed quite fervently in its "credo." That, and our dinner table agricultural conversations, were my two main inspirations.

You entered the University of Chicago and obtained a B.A. with honors in mathematics. Can you tell us something about the courses you followed, the people that influenced you most?

In those days (1949–1952), the "Hutchins" program was in effect at Chicago. Required were 14 comprehensive examinations in humanities, natural sciences, social sciences, philosophy, history, languages, and mathematics. Fortunately, as I thought at the time, or unfortunately, as I now think, I "placed out" of all but five of the fourteen by initial examination. Since I was only 15 and since my father, being on the faculty, got reduced tuition, I decided to stick around and took courses in physics, chemistry, and mathematics at the Divisional level, and then, after reading von Neumann and Morgenstern, in economics. Finally, in 1952 I gave up and went to graduate school at Hopkins.

My most memorable teachers at Chicago were: Irving Kaplansky, William Herman Meyer, and Otto Schilling, in mathematics, and Frank Knight, H. Gregg Lewis, Lloyd Metzler, and Milton Friedman, in economics. I took theory from Lewis and Metzler and money (as macro is still called at Chicago) from Friedman. At that time, as well, I "discovered" Hicks' *Value and Capital* and Samuelson's *Foundations*, two books which have proved far more useful to me over the years than has von Neumann and Morgenstern.

After your B.A. you went on to The Johns Hopkins University to enter the Ph.D. program. Would you like to tell us about those years as a graduate student? What courses drew most of your interest? Whom did you interact with?

Hopkins was a strange and wonderful place in the years I was actually there, 1952–1954. (In 1954, I went back to Chicago to work with Harberger and Christ, who had returned there, and with T. W. Schultz and D. Gale Johnson.) Besides A. C. Harberger and C. F. Christ, who were assistant professors, also on the faculty were Fritz Machlup, Simon Kuznets, who came later, Evsey Domar, Clarence Long (later an influential member of the U.S. House of Representatives), and Ta-Chung Liu. I had courses from all of them: theory from Machlup and Domar, econometrics and statistics from Christ and Liu, and public finance from Harberger. Visiting in those years were Richard Stone, Ralph Turvey, Tord Palander, and Carl Iversen. Dick Stone was the most important for me, although I had courses with all four visitors.

Perhaps even more important than my professors were my fellow students, among whom the most significant influence was Richard Brumberg of Modigliani–Brumberg life-cycle-savings hypothesis fame and two Israeli economists, Amotz Morag and Michael Michaely. Both Brumberg and Morag died very young.

Later, at Chicago (1954–1956), I interacted mostly with Zvi Griliches, Yehuda Grunfeld, and Lester Telser. I took courses too from Milton Friedman, Martin Beckmann, T. W. Schultz, Henri Theil (visiting), and participated in Harberger's public finance workshop. Mostly, I worked on my Ph.D. dissertation, submitted to Hopkins in 1956.

As a graduate student you also spent time at the Cowles Commission as a research assistant for Tjalling Koopmans. Which projects were you involved in?

I was research assistant to Tjalling Koopmans and Jacob Marschak, the summer of 1953, between my first and second years at Hopkins. I worked with Koopmans and his associates on their book on the economics (mainly activity analysis) of transportation (Beckman et al. [2]). My work is the basis for Chapter 8 on problems associated with the rerouting of empty box cars.

For Marschak, I recall writing a paper showing the obvious and now well-known fact that the concept of a noncooperative Nash equilibrium (Nash [13]) was really a version of the old Cournot [6] idea. Marschak didn't think it was so obvious at the time, but pressure of course work at Hopkins the following year kept me from doing anything with the work.

Do you have any special memories or experiences from the time you were at Cowles?

Besides the professional stuff, I remember most the pleasure of getting to know Gerard Debreu, Henk Houthakker, Cliff Hildreth, and, especially, Roy Radner, with whom I shared a *very* small office—it was so small that for one of us to stand up the other had to squeeze into the knee hole of his desk! I have especially pleasurable memories of the annual Cowles picnic at the 57th Street Promontory and of Truus Koopmans and Françoise Debreu and their children.

Your dissertation explored the dynamics of agricultural supply, estimating farmer's responses to prices. Besides the family dinner table conversations, were there any other circumstances that brought you to the subject?

In early 1954, when Fritz Machlup said he would nominate me for an SSRC fellowship if I could come up with a topic and thesis prospectus fast, I recalled an *Econometrica* article by Karl Fox on spatial price equilibrium in the U.S. livestock-feed grains economy which I had read only a few months before. Fox had assumed all supply elasticities zero, which struck me as strange, since we'd been piling up huge agricultural surpluses since the end of WWII under the agricultural price support program in effect since 1933. Demand elasticities were uniformly found to be low, so both low elasticities of demand *and* supply couldn't be consistent with what had happened, and was happening. On checking further, I found that Fox's assumption was supported by what little empirical evidence was available. So I set out to find out what these elasticities really were. My resolve was strengthened by a visit to Paul Douglas, my father's former professor at Chicago and by then U.S. Senator from Illinois and a friend of my father. I asked him whether he believed such elasticities to be so low—he said he did not—and, if not, how he could vote for the farm legislation which was periodically enacted by Congress. He told me that, without good evidence to the contrary, it made political sense to go along with farm-belt senators who would support other legislation with which he was vitally concerned. But he encouraged me to find out.

At the heart of your dissertation was an economic rational for the distributed lag model attributed to Irving Fisher. Adopting an adaptive expectations framework, you developed a dynamic model of producer behavior and used it for the econometric analysis of supply response. How did these ideas emerge?

Irving Fisher introduced distributed lag models in the 1920s. My own interest was greatly stimulated by Koyck's [11] book on investment and by Milton Friedman's use of adaptive expectations, which came to a geometric distributed lag and more or less what Koyck had proposed, and which Friedman used in a time-series implementation of his permanent income hypothesis. Moreover, these geometric lags could also be derived from Richard Goodwin's dynamic accelerator models, which Lloyd Metzler had us read in the course I took from him in 1951–1952. Phillip Cagan, then a fellow graduate student at Chicago, was writing his dissertation on hyperinflations, under Friedman's direction, and using adaptive expectations. So these ideas were all around me, "in the air" at Chicago. All I really did was to see that they were the same thing and that such lags in adjustment or in expectation formation could explain why nobody had been able to measure supply elasticities significantly different from zero for major U.S. crops.

Looking back on it now, how successful was this approach in predicting agricultural supply?

I did manage to predict the effects of agricultural price support programs in the U.S. since 1933 on the basis of observations *before* 1933 pretty well, but I fear the very success of my work led to widespread application of the so-called Nerlovian model in inappropriate situations. I've written a fair amount on why I regard much of the application of the Nerlovian model, especially to agriculture in developing countries and even to more recent periods and other than annual crops in the U.S. and other developed countries, as inappropriate and misleading. (See especially Nerlove [14] and [15].)

By emphasizing the role of expectations in the formulation of dynamic (empirical) models, you touched on quite a few issues that became more fashionable in the three decades since, like stability of markets and expectations, aggregation and expectations, etc. You worked notably with Kenneth Arrow on these topics. How did this influence your thinking about empirical research?

Of course, from the very beginning I was interested in how people's expectations and the way in which they were formed influenced their behavior, both individually and in the aggregate. My work for Marschak on Nash equilibria reflected this interest as did my empirical research on agricultural supply. Ken Arrow was a major influence early on. So was Ed Mills, who taught at Hopkins after my years there.

When I was in the Army, 1957–1959, stationed near both Baltimore and Washington, I held a visiting appointment at Hopkins, Fall semester 1958. I would teach for three hours Saturday mornings, then have lunch with Mills at the Faculty Club. We talked about many things, but mainly about expectations and how best to implement theoretical models of expectation formation empirically. Mills had developed an empirical counterpart of perfect foresight he called "implicit" expectations, in which one simply substituted the actual future value for the future expectation. I argued that this was wrong because all the information which determined the actual future value was not available to economic agents at the time their expectations were formed and they acted upon them. Thus, an error was introduced, which, at best, would bias the estimated behavioral impact of the expectation toward zero. You can imagine my delight and surprise when I heard Jack Muth present a preliminary version of his famous *Econometrica* paper, "Rational Expectations and the Theory of Price Movements," at the Winter 1959 Washington, D.C., meeting of the Econometric Society. Muth solved *that* problem by taking the expected future value to be the statistical conditional expectation at the time acted upon, conditional upon all the information, including the model itself, available up to that time to the economic agent. But this concept was extremely hard to implement. (I wrote about the problem in a paper presented at the Farm Foundation in February 1960 [16].) Later I tried to get around the problems with what I called "quasi-rational expectations" (Nerlove [17] and Nerlove, Grether, Carvalho [18] Chaps. 13–14, 1979).

Again, I have to emphasize the constant interplay of theory and empirical observation which is the heart and soul of econometrics and what distinguishes it from mathematical statistics. All the theoretical pieces I did in this area were offshoots of such interplay.

One excellent example of such interplay, I guess, is your research on the estimation and identification of production functions, including the 1963 study on returns to scale in electricity supply which Berndt [3] believes is the first empirical application of duality of production and cost.

Knowing Paul Douglas was perhaps not incidental: After all, he was Douglas of Cobb–Douglas. But the main impetus stemmed from two other sources: When I taught at Hopkins toward the end of my stint in the Army, I gave a course in what would now be called "mathematics for economists" and in that connection developed an extensive series of examples of basic theorems in the theory of the firm using the Cobb–Douglas function. In particular, I worked out the derived demand functions, the supply function, and the cost function (in the one-output case) in detail. Casting around for some empirical research in which to put all this to use, I hit upon the idea of estimating returns to scale in a regulated industry, namely, electric power generation, which I naively assumed to be a particularly simple case. So the second source of motivation for the paper "Returns to Scale in Electricity Supply," finished essentially in 1961, but first published in 1963, was to study scale economies in this context.

Again, realizing that all this was a superb illustration of the interplay between theory and empirical research, I was moved to publish the short 1965 monograph on the general problem of inference about production using the Cobb–Douglas function for extensive illustration.

Your electricity demand paper is a very simple and early example of nonlinear estimation, one to which undergraduate students can relate. Simple ideas do not always come easily. Did it take a long time to put all these elements together?

There are, I think, two interesting methodological aspects to the electricity supply paper: First, this paper represents, as far as I know (Ernie Berndt agrees), the first empirical use of duality. Second, in the study I found and interpreted a curious pattern in the deviations of the cost observations from the Cobb–Douglas cost function as a classic textbook effect of varying returns to scale and used a spline function technique (without actually calling it that) to estimate a function nonlinear in logs to approximate the true cost function, which could then, by duality, be interpreted in terms of an underlying nonhomogeneous, but homothetic production function.

I think anyone who taught principles of economics would have seen this, and I don't recall that putting this element in the picture was very time consuming, except that, in those days, I had to do all the computations by hand on a desk calculator. That took time!

Were you initially aware of the fact you were exploiting duality theory?

I wish I could say I knew all about duality and employed that theory consciously, but in all honesty I cannot say I did. I had read Shephard's [29] book, his Princeton Ph.D. thesis, as a graduate student. I understood the mathematics of it but not the significance. Maybe it was in my unconscious.

Anyhow, when I presented a version of the electricity supply paper at a seminar, Uzawa, who had been sitting in the back, apparently not listening, said: "You can always recover production function from cost function." I thought it was a question, but Uzawa said, when I replied that in this case I could, "Not a question. Always true." So there you are!

I would like to move to your time-series econometrics research. In the early sixties, the idea that a stationary time series can be thought of as a noncountably infinite sum of uncorrelated components obtained via Fourier transformations caught on in economics. I am thinking in particular about the important article you wrote in *Econometrica*, 1964, as well as the work by Hannan and the book by Granger and Hatanaka [9]. Looking back on this now, how successful do you think spectral methods have been as a tool of econometric research?

I think they are of limited, but nonetheless important, use *directly*, but of inestimable significance *indirectly*. The "frequency domain" approach has indelibly marked modern econometric research and influenced profoundly the way we all now think about economic time series.

Your 1964 paper as well as your later work, notably with Grether, took advantage of spectral analysis to study seasonality and seasonal adjustment procedures. Quite often researchers get involved with this subject because of practical problems. Durbin, for instance, became interested in seasonal adjustment directly from a practical study (see Phillips [28, pp. 140–141]). Could you tell us something about the origin of your interest in this area?

There were, again, two sources: First, in the late fifties I had worked with Fred Waugh and Ken Bachman at USDA (see [1]) who both encouraged me to work on livestock supply. As you know, this is a terribly complicated and difficult problem involving lots of capital theory and lots of modeling of expectation formation. At the same 1959 Econometric Society meeting mentioned earlier, I had lunch with my former teacher Milton Friedman, fresh from a year at the Behavioral Sciences "think tank" at Stanford and many conversations with John Tukey. Friedman suggested that spectral techniques might be just the thing to cut through the tangles and thickets of livestock cycles to the heart of the capital theoretic and expectational problems. They weren't, but by the time I'd learned enough about spectral analysis to know that, I'd invested too much to let go.

Fortunately, at just that moment (around early winter 1961), I got a call from Aaron Gordon who was chairman of a commission established by the newly elected President Kennedy to evaluate unemployment statistics in the U.S. Bob Dorfman was working on the chapter on seasonal adjustment, would I work with him? Wow! A chance to work with Dorfman *and* an ideal opportunity, or so I then thought, to apply all my hard-won knowledge of time series, and of frequency domain methods in particular. I'm not sure in hindsight that it was such a perfect application. In any case, later work by me, Grether, and others suggested that some of my conclusions in the 1964 paper were flawed. But this was, in any case, the first paper published in *Econometrica* using frequency domain methods and contributed, I would like to believe, to the forces set in motion by Hannan's work and the 1964 book by Granger and Hatanaka, which led ultimately to the incorporation of these methods in the econometric tool kit.

During the seventies many economists became familiar with the Box and Jenkins approach to time-series analysis and more specifically with ARIMA models. Their model specification approach had several weaknesses. One can think of at least two major ones, namely, the relatively poor behavior of the sample ACF and PACF relative to the theoretical functions—as we know from, for instance, work as early as 1960 by Hannan, and of course the unit root problem. In your own research on time series, you took advantage of the ARIMA structure in the context of unobserved-component models. What led you to blend these two ideas?

Long before Box and Jenkins [4], I had been working on unobserved-components (UC) models, stemming from the year in Rotterdam with Theil (Nerlove and Wage [23] and Couts, Grether, and Nerlove [7]). Peter Whittle was my discussant on the first of these papers at the Copenhagen Meeting of the Econometric Society in 1963. He was kind enough to give me a set of page proofs of his book [31], in which my models appear as examples in several exercises! A deflating experience, but not, let me tell you, as deflating as it was to try to read Whittle's book. As a result of that labor, I began to understand the importance of both ARIMA models and UC models. The general idea of UC models is developed in my 1967 paper in the volume celebrating the 100th anniversary of Irving Fisher's birth and in my 1970 Fisher–Schultz lecture, published in *Econometrica*, 1972.

Of course, I don't mean to minimize the importance of Box–Jenkins, whose pioneering work provided the first practical methods for formulating and estimating ARIMA processes. It's just that Theil and Whittle, not they, were *my* source of inspiration.

Unobserved-component models give rise to a host of issues, including identification and uniqueness. In particular, in a multivariate context, these issues become very complex, especially when one thinks about combining time-series models with economic models of dynamic optimization. You have worked and thought about these problems for a long time. How do you think about these issues nowadays?

Starting with Box and Tiao, Harvey's "structural models," the work of Nelson and Plosser, and continuing with the work of Stock and Watson, UC models have really caught on. Of course, they are essential in modeling seasonality and for building a bridge to the older work in economic statistics of, for example, Jevons, Bowley, and Kuznets. Danny Quah, currently at LSE, is doing the most systematic work attempting to integrate many UC models in a common framework. Most exciting for me is the incorporation of such models in the multivariate analysis of time series, particularly generalizations of the idea of cointegration in a UC context.

My own work seems to have influenced Bob Hall and later developments in the study of the permanent income hypothesis stemming from the *JPE* paper [10] on the subject.

What would you characterize as seasonal components in a bivariate system where, say, one of the series is exogenous?

This seems a pretty technical question for an interview, even an ET interview. Let me attempt an intuitive answer: Let's say the basic idea is to "explain" seasonality in one series by seasonality in the related, presumably exogenous, series. (This is essentially similar to the idea of cointegrated series: nonstationarity in one series is "explained" by nonstationarity in a related series.) Thus, the innovations in the explanatory series seasonal component are related to innovations in the explained series seasonal component. Of course, common stochastic trends are to be found in the trend-cycle components. Allowing for such trends also to affect seasonals is an interesting way to go, although there are pretty serious identification problems involved.

In more recent years, many researchers used state space representations of unobserved component models. I am thinking of the work by Aoki, Chow, Harvey, Engle, and Watson and the index models by Geweke, Sargent, and Sims. Looking at this research now, what would you characterize as the areas where it has been more successful than other econometric methodologies? Is there any area where it has particularly failed or where you think improvement is possible?

I think state-space modeling is definitely the way to go with respect to UC models. The original Kalman approach suffers from its restrictions to the linear case. Recent research extending state-space techniques to nonlinear situations is therefore particularly valuable in my view. But one must be very careful here in applying the usual approach to nonlinearity which amounts to formulating what are essentially local linear approximations. In this case, the errors no longer have the same stochastic properties that are assumed in the usual linear Kalman formulations. Recently, a student of mine, Hisashi Tanizaki, wrote a Ph.D. dissertation in which he used globally valid approximations. Monte Carlo results suggest that the standard methods may lead one into serious error.

May I briefly divert your attention to a slightly different subject. In the early sixties, you decided to cross the Atlantic and spend a year at the Econometric Institute of the Netherlands School of Economics—Rotterdam (now called the Tinbergen Institute at Erasmus University). Did you experience a big difference between European and North American academic research institutions?

There's a tremendous difference between Europe and America, more then than now. Theil was then the director, and he was "Captain of the Ship" in every sense. Of course, I'd known Theil in 1955, when he was visiting Chicago, and he was terribly kind and and supportive and a great inspiration as well, but he was indisputably "the boss." Many people there, especially foreign visitors, did not take kindly to such a degree of dominance, which, of course, you don't find in American institutions—at least in economics. But I didn't really mind and accepted the difference in "style" as part of the experience. Besides, I got a lot out of that year and a lot from Theil.

In the mid-sixties you wrote an extensive survey of macroeconometric models, categorizing the different approaches and comparing details of many of the models. How did this study come about?

Lawrence Klein, then editor of the *International Economic Review*, commissioned me to do the job. It was not a lot of fun, but I learned a lot and got two papers in addition to the one Klein commissioned out of the experience.

Not many econometricians have left their mark in both time-series and cross-sectional analysis. You are obviously among the handful who did. How did your seminal *Econometrica* paper with Balestra on pooling of time series and cross sections come about?

First of all, let me point out that there is a long tradition of research based on cross-section data in agricultural economics, particularly the studies of production and cost in the subfield of farm-management economics, which goes back to the work of John D. Black at Harvard shortly before and after WWI. Of course, I was familiar with this literature from working on agricultural supply problems, but, in addition, my own work on returns to scale in electricity supply was based on cross-section data.

Early on I realized that what is past in time is not necessarily predetermined in a cross section. This emerged in crude form in the electricity paper and there is some discussion in the Cobb–Douglas monograph, but I must say that I really only understood the problem when Balestra, then a graduate student at Stanford and an SRI researcher, came to me with some really strange OLS regressions he did in connection with the SRI study on the demand for natural gas based on data for 36 states over a six-year period. We wrote the 1966 *Econometrica* paper in 1963–1964, and Balestra did his dissertation under my direction on the topic.

You say you found the regressions that Balestra brought to you in connection with his SRI study "really strange." Would you care to elaborate?

Oh, therein lies an interesting tale, and one which illustrates very well indeed the essential interplay between theory and statistical inference in econometrics.

Balestra's idea was to formulate a distributed lag model of the demand for natural gas based on a capital adjustment model. His theory was that the demand for gas for space heating depended on the stock of furnaces that used gas and their associated ductwork, etc. Because this stock was fixed in the short run and capable of only gradual expansion, the long-run elasticity of demand should be much higher than the short-run elasticity. The distributed lag parameters in the equation he estimated depended on the depreciation rate and on the speed of adjustment of actual to the desired stock of gas-using capital. Because the estimated parameters associated with lagged gas demand was greater than one when the equation was estimated on the pooled sample, we inferred a *negative* depreciation rate for gas-using equipment (provided there was no overshooting adjustment). This was, of course, unreasonable. Rather than discard our theory, we decided to look harder at the statistical procedure we were using and quickly realized that state-specific time-invariant latent effects that we were not able to measure explicitly introduced a peculiar and strong form of serial correlation in the disturbances of the pooled regression equation. So we formulated a variance-components model. From those simple beginnings derives a rather extensive literature on panel-data econometric methods. Our work had antecedents in earlier work, for example, that of Meyer and Kuh on investment behavior, and, while such models were widely known and used in biostatistics long before our work, I think it's safe to say that we were the first to show what a crucial difference explicit modeling of the disturbance in this way could make in the estimation of a dynamic relationship.

There are some obvious tensions between the benefits of using microdata sets as opposed to time series of aggregates and the costs of dealing with large cross sections, including heterogeneity, censoring, etc. With panel data it is often difficult to extract dynamics from the relatively short time-series span. Having worked quite extensively in most of these areas, how would you characterize their relative merits?

I'm always inclined to regard difficulties as challenges to ingenuity and insight. All of the problems you mention are fascinating and reasons *for*, rather than *against*, working with microdata sets. Work of the mid-eighties by Sargan and Bhargava on estimating dynamics from short-time panel data (two time observations!) is a particularly good example of such marvelous opportunities.

Another area of (mostly) cross-sectional data analysis is that of qualitative response models. In your paper with Press you proposed making the parameters of a log-linear model a function of independent variables, like writing main-effect parameters as linear combinations of such variables. What drew your attention to this area?

My interest in log-linear probability models stemmed from a dissertation done at the University of Chicago under my direction by Mahar Mangahas. Mangahas was analyzing the adoption of modern techniques and varieties by rice farmers in the Philippines. I didn't like the fact that he treated one variable as dependent and the rest as independent, when it was apparent that the various techniques and choice of rice variety were jointly dependent. My then colleague Leo Goodman had done a lot of work on the analysis of contingency tables using log-linear models. Jim Press, another colleague in the Business School, who did multivariate analysis, suggested the multivariate generalization of the logistic model. I simply saw that the two must be the same with main-effect parameters functions of some additional explanatory variables.

These conversations with Press led to our 1973 Rand monograph and several other joint papers (Nerlove and Press [21,22]), as well as many, many papers alone and with various collaborators through the years since.

Would it be indiscrete to ask why your paper with Press remained as a (widely quoted) unpublished Rand Corporation document?

Yes, it would be indiscrete. Let me say that Jim and I have often wondered whether our Rand Report would have become so widely known and quoted if we had actually published it.

I should point out that T. Paul Schultz and I have another 1970 Rand Report, similarly unpublished, which achieved a certain notoriety nevertheless. I'm inclined to believe that the title, "Love and Life between the Censuses," may have had something to do with that, however.

The log-linear probability model brings me to the subject of your presidential address at the 1981 European Meeting of the Econometric Society in Amsterdam entitled *Expectations, Plans and Realizations in Theory and Practice*. I noticed that in your dissertation you referenced work done in the mid-fifties by Theil and Jochems who used German business test data on expectations. Was the work you presented as presidential address the realization of a long-term plan made at that time?

Since 1968, when I was visiting professor there, I have been spending time in Mannheim with Heinz Koenig, whom I admire greatly. The work with Mangahas and later Press was beginning to gel at some point after 1968. Koenig suggested that work with the Ifo data would be a perfect application for the methods Press and I were developing. This suddenly "clicked" with all the stuff on expectations with which I'd been concerned for decades. Later, presenting this work in Malinvaud's seminar in Paris, Malinvaud asked why I bothered with the Ifo data since his INSEE data was so much superior, at least in his view. He kindly gave me access, and therein lies the tale. Long-term plans had nothing to do with it.

In recent years you have abandoned the log-linear probability model in favor of threshold models using polychoric correlation methods. Can you tell us something about these techniques, their origin, and their use?

I can get you references if you want them: Karl Pearson [27] introduced the bivariate form in a paper published in 1901. Multivariate generalizations, including the polychoric correlations followed. Recent papers by Olsson and others [25,26] develop more practical computational procedures. The idea is basically very simple: If you observe a contingency table and, for example, you assume that it arose from a continuous pair (or triple, or . . .) of latent variables distributed bivariate (or multivariate) normal, can you figure out what the correlation between the two variables (or correlation matrix) and thresholds must be? Crossing these thresholds, the latent variables trigger observations in the cells of the table.

Latent variable models of all kinds are very common in the analysis of multivariate problems. Models based on thresholds and polychoric correlations allow the analysis of ordered categorical variables by structural-equation methods, which is not possible using log-linear probability models, which are inherently nonstructural.

I would like to move on to more general questions. In the course of your career you taught and trained many students. How do you view the teaching of econometrics nowadays? Do you think too much emphasis is put on econometric theory as opposed to practice in graduate course work?

Being at Yale these days, you're probably spoiled by the very good environment for econometrics there. Out here in our various wildernesses, the environment is not as favorable. I've tried to suggest on several occasions during the course of this interview the essence of econometrics is the interplay between economic theory and statistical theory in the analysis of real problems and real data. Throughout most of our profession, with the exception of the agricultural economists, it is primarily theory that is valued, especially abstract economic theory, but the work of econometricians, especially that which is largely methodological, and, of course, highly mathematical, is tolerated a lot better than messy empirical work. And while lots of economists are interested in policy issues, there are few around who can appreciate or make use of even moderately sophisticated econometric techniques in the analysis of associated quantitative issues.

I, myself, try to combine theory and practice in what I teach, but the students, to the extent that they are interested in econometrics at all, would rather do pure econometric theory and leave practice for later or to others. Fortunately, there are always a few students, even here, who value the three-way interplay that *is* econometrics.

How *do* you teach applied econometrics?

Here at Penn we have a two-semester "core" sequence taken by all graduate students and a two-semester advanced sequence which is to be taken in preparation for the field exam, which is supposed to "certify" econometricians. The "core" sequence is designed to prepare all graduate students for advanced work in every field, e.g., labor, development, international, etc., *and also* for the advanced year. The first core semester is basically math stat through and including the general linear model and testing general linear restrictions in regression analysis under standard conditions (variance–covariance matrix of disturbances known up to a scalar multiple). The second core semester deals with regression complications, such as heteroskedasticity, serial correlation, misspecification, multicolinearity, as well as with limited dependent and categorical variables, panel data, expectations and dynamics, errors in variables, seemingly unrelated regression, and simultaneous equations estimation and identification. On top of all of this, our syllabus calls for a good introduction to the main areas of applied econometrics such as demand analysis, production and cost analysis, and various topics in macroeconometrics. The task is overwhelming and basically impossible. I've written out extensive notes and included two long chapters on demand analysis and production and cost analysis. I'd like to do more. Fortunately, Ernie Berndt's book *The Practice of Econometrics* came along just as I was about to try to write up my own "hands-on" applied topics, but instead I now use five chapters from the Berndt volume. But there is far too much to cover in one semester. Rather than expand the course to two semesters and treat math stat as a prerequisite, I fear the Department will rather opt to reduce the contents and level, which will impact both on the ability of the students to do reasonably sophisticated econometrics in areas such as labor and development and on their ability to pursue really advanced econometrics in the second year should they wish to do so. C'est la vie!

Doesn't this problem exist with respect to all "tool" subjects, economic theory included?

Yes. As I see it, the students need to know a lot of technique (and underlying theory, both economic and econometric) before they can do really meaningful applied work. But the key problem is how to hold their attention and motivate them to learn techniques *without* giving them some exposure to applications which use what they are learning. Time is too short in the usual graduate program and the sequencing difficulties are too great. I guess we shouldn't stop trying to find a way to do this without lengthening the graduate program in economics unduly, but I personally feel that I, at least, have never been really successful.

Besides the field of econometrics you are also active in areas like endogenous fertility and economic growth and the related subject of female labor supply. You also continue to work in agricultural economics, notably on the topic of agricultural reforms in developing countries. It is somewhat unusual to work in so many fields. What motivates you to do so?

Variety is the spice of life! I consider myself blessed to have had so many interesting opportunities—and cursed to have so little time for all of them. Besides, it seems to me that the best work in econometrics derives from work on substantive problems. The most interesting developments in econometric methods are motivated by tackling quantitative methodological issues which arise in specific applied contexts.

During the course of your career you witnessed many phases in the development of econometrics. In retrospect, what milestones seem particularly important to you now?

That's a *big* question. Suppose I make a short list:

1. Henry Schultz's *Theory and Measurement of Demand*, 1938.
2. Tinbergen's multiple equation macro models, 1939 and 1947 (but written much earlier).
3. Haavelmo's 1944 Supplement to *Econometrica*, "The Probability Approach in Econometrics."
4. The Cowles Commission developments, post-WWII into the 1950s, including Klein's macro models, Anderson's development of limited-information maximum likelihood, and, of course, the work of Koopmans and Marschak published in Cowles Monograph 10 (1950) and 14 (1954).
5. Richard Stone's *Consumer Expenditure and Behavior in the United Kingdom, 1919–1938* (1954).
6. Many developments in microeconometrics beginning in the mid-1950s with, for example, Tobin's famous paper, and continuing as an active research area today.
7. Griliches' great work of the 1960s resurrecting hedonic analysis.
8. Duality in the theory of production, applied extensively since the mid-1960s in econometric investigations by McFadden, Jorgenson, and others.
9. Modern time-series techniques and their application to the study of macroeconomic phenomena, particularly the work of Sims and Sargent.
10. Semiparametric methods pioneered by Chamberlain with many applications to large micro data sets.

May I stop now?

You surely can. Let me perhaps just ask you a related question about foundational issues. Several researchers have pondered the merits of Bayesian methods in econometrics. Most recently, for instance, Peter Phillips, Chris Sims, and others, have debated their use in testing for unit root nonstationarity. Do you have any thoughts on this general subject you would like to mention?

I think the Bayesian approach is right in principle but very difficult to apply in practice. It gets rid of a lot of knotty problems, but generally at high cost. Arnold Zellner has been more successful than most in applying Bayesian methods to real data. Probably I'm most sympathetic to Ed Leamer's approach in *Specification Searches* [12].

Throughout this interview you emphasized the need to blend statistical theory with economics and emphasized applied work. Having done so many applied empirical studies, what is your overall view of the present state of applied econometrics?

Not in good health. Having recently served as chairman of the committee to award the Frisch Medal, I was impressed by how many papers paid lip service to real empirical analysis by merely including some "illustrative" example and how few there were with a real substantive focus.

There are plenty of empirical and quantitative studies published, of course, but these usually involve competent use of very standard econometric techniques, or, more often, fundamental misunderstanding of even fairly elementary econometric ideas. I miss the fine interplay that characterized the best of past applied work.

So, what significant developments in econometrics do you expect to happen or do you want to see happening over the next decade?

That's a big question. You do want this interview to end, don't you? Let's stick to what I would like to see happen, since econometricians are notoriously bad forecasters, and I am no exception.

What I would most like to see is the development and widespread use of really practical methods of building models based on dynamic optimization and realistic models of learning, information use, and expectation formation. I've tried ([17 and 18, Chap. 14]), but the problem is terribly hard. Very good people, cleverer than I, such as Sargent and Hansen, are working on such problems now, so there is hope.

There's more on my wish list, but the hour is late.

Speaking of the lateness of the hour, isn't it about time that we head off for a drink? Oh, that reminds me: I see that one of your most recent working papers deals with the estimation of hedonic price functions for wine. Is this an expression of a latent desire to do some experimental "field" work and participate in more wine tastings?

Ah! Well, I actually have a serious professional interest in wine from an economic point of view—apart from being only an amateur drinker of the stuff. For many years now, Jean Waelbroeck and I have discussed various economic issues associated with the wine industry, especially in the EEC, where it is a source of many difficulties and problems. Last year opportunity knocked: through the good offices of Lars Werin of the University of Stockholm, I was able to get quite a lot of data from Vin och Sprit on the prices and quality attributes of all wine marketed in Sweden. Although the hedonic price function in its usual form is a *confluent* relation at the world level, a "small country" assumption applies to the case of Sweden, and the implicit hedonic valuations of Swedish consumers can be examined by regression of quantities sold on prices and quality attributes. The results are extremely interesting, although not necessarily helpful in a restaurant!

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