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THE ET INTERVIEW: PROFESSOR PHOEBUS J. DHRYMES

Interviewed by Aris Spanos
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Phoebus J. Dhrymes

Phoebus J. Dhrymes is one of the best known econometricians of the last 40 years. He has made substantial contributions to econometric theory through articles in leading journals and by way of a series of outstanding texts on the foundations and methods of econometrics. His early research began with an applied econometric focus on problems of production and investment. His later contributions concentrated on the foundations of econometric methodology, including systems of simultaneous equations. Throughout the econometrics community, Dhrymes is well known for his influential textbooks, some of which have been translated into several languages. His 1970 book *Econometrics: Statistical Foundations and Applications* provided an accessible and rigorous foundation for both students and teachers of econometrics. His subsequent books have continued to treat foundational issues and have tracked new areas of econo-

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metric interest through to his 1998 book *Time Series, Unit Roots, and Cointegration*. Reading his books reveals Dhrymes as a teacher, synthesizer, and master expositor. As he says in the interview that follows, "my books are not typical textbooks. I perceive them more as books that bridge the gap between ordinary textbooks and journal articles and as filters that distill and synthesize the wisdom of many contributors to the subject. On this score I was influenced in my writing by the way I learn when studying by myself."

Phoebus Dhrymes was born in 1932 in Cyprus. He arrived in the United States in 1951, and after a few months he volunteered to be drafted into the U.S. army for a two-year tour of duty. He resumed his studies in 1954. After getting a B.A. degree in economics from the University of Texas in 1957, he completed his Ph.D. at the Massachusetts Institute of Technology (MIT) in 1961. His first academic appointment was as an assistant professor at Harvard in 1962. He was appointed associate professor at the University of Pennsylvania in 1963 and became a full professor in 1967. Since 1973 he has been a professor at Columbia University.

The present interview was conducted on June 13–14, 1999, in Nicosia, Cyprus. A most refreshing aspect of the interview is the splendid candor displayed by Dhrymes in his answers to questions concerning his views on the development and the current state of econometric theory. Did Dhrymes define econometrics as "a sequence of fads"? Find out in the interview that follows.

1. BACKGROUND AND EARLY YEARS

Let's begin with your background. You were born in Ktima, a small town in Cyprus. Would you like to start by telling us how you ended up at the University of Texas studying economics? I found out that you were an exceptionally bright high school student. Did you get a scholarship to study abroad?

No, not all. We had a relative who lived in New York, and I came originally with the idea of studying and of working a little bit with him. Unfortunately, when I talked to him about these plans, a few weeks after my arrival, he declared that he did not wish to subsidize my education in the least.

When I arrived in the United States in 1951, it was in the midst of the Korean War, and all permanent residents were subject to the draft. At the time they were drafting, I believe, nineteen year olds, late nineteen year olds. When I arrived, I was not quite nineteen, I was maybe eighteen and a few months, maybe eighteen and a half. After a few months of working at transient jobs I began to think about my predicament and how to cope with it. But it didn't appear to me to be very sensible to start something and then interrupt it for two years of military service. At that period there were no student deferments as was later the case during the Vietnam War. When your cohort was called for service while you were enrolled in a university you would only be allowed to finish the semester, and then you went into the military. So I decided that I should volunteer to be drafted early. Evidently, by doing so I put myself at risk of being sent to the front and being wounded or even killed. However, I am not sure I was fully cognizant of the consequences of that decision. This was merely a voluntary consent to be drafted out of turn, I did not volunteer to enlist in the army as a "professional" soldier. So I did only two years of military service as required of draftees, instead of four, which is the minimum when you are recruited as a professional sort of soldier. And so, I went into the U.S. army; I think it was the sixth of February 1952.

In other words, you went to the United States with the intention to study, but you found yourself drafted into the U.S. army.

I found myself drafted into the U.S. army, and that was not an experience without rewards, I might say. After basic training in Kentucky I was very lucky to be sent to Germany rather than Korea. Ultimately, I was assigned to the 43rd Infantry Division, which was stationed in three German cities: Augsburg, Munich, and Nurenberg. I was stationed in Augsburg, with the quartermaster unit at the division headquarters. After a while my job consisted of seeing to it, through inspections, that the soldiers took good care of the clothing and equipment entrusted to them by the U.S. army. So, for the most part, I spent my time traveling to and from these three cities. Needless to say this was an excellent assignment which had the effect of freeing me from a lot of harsh aspects of military life; however, when there were military exercises (three times a year), I spent a considerable amount of time loading and unloading trucks.

Was life in the barracks boring, or were there any creative activities one could indulge in?

One positive aspect of my assignment was that I had a considerable amount of free time, which I spent in the library reading literary works or in the photography studio developing and modifying pictures I had taken. And in many ways that was the period when I really learned English extremely well, by reading.

How did you find yourself at the University of Texas?

When I came out of the army in 1954, I inquired about how to pursue my studies. There was a chance of staying in New York with my uncle. However, since he was unable or unwilling to help me financially this was not a feasible option because the tuition I had to pay in New York, either at Columbia or at New York University (NYU), to both of which I was admitted, was more than the allowance I would get under the GI bill—which was about $110 a month for 36 months. So, I inquired further afield with a view to finding a place that would not require much, if any, supplementation of my resources. I looked particularly, I remember, at Berkeley and Texas—the University of Texas at Austin. Then I found out that the cost of living in California, and the San Francisco area in particular, was quite high and it was unlikely that I would be able to rely entirely, or even mainly, on this stipend. And so by a process of elimination I went to the University of Texas, and, as you might say, the rest is history.

U.S. army, Augsburg, Germany, 1953.

I should also note that another consideration for this choice was the fact that this school was known to me because several graduates of the Greek Gymnasium in Paphos, the high school in the town where I grew up, had gone there earlier. But no particular characteristic of the university, beyond what I mentioned before, drew me there; in fact, I did not know much about the University of Texas.

Did you have any career in mind when you first went to the university? Was education part of the family tradition?

Well, yes, I think that I had in mind something. When I was in high school, during the summer, in addition to going to the beach, I spent my free time going to, or attending as a spectator, the district (criminal) court in Paphos. I was intrigued by the proceedings and the role of the attorneys, and for a while I thought of becoming a lawyer. By that time my older brother had gone to the University of Athens to study medicine, and my father was the only child in his family to have gone to high school. The others, who came from the small vil-

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lage of Dhrymou, a few miles to the northeast of the town where I grew up, by and large remained in the village.

That's where the surname "Dhrymes" comes from.

The name comes from that village, yes. My uncles and aunts did not attend school beyond the elementary level; my father, by contrast, went to the capital of Cyprus to attend the Pancyprian Gymnasium in the last decade of the nineteenth century. In the context of Cyprus at that time this was quite remarkable. My father was born in 1877 shortly before Britain replaced Turkey as the sovereign power. Under Turkish rule the education of Christians was severely circumscribed, if not entirely forbidden. So in that context he was indeed very well educated for his time (and place), and after high school he established himself in the town of Ktima some distance from the village in which he was born.

So, after you completed your military service with the U.S. army in Germany you decided to go to the University of Texas to study economics.

No, not at first. I had thought that I ought to study law, but for some reason that I cannot fully understand now, I decided that I couldn't possibly become an attorney in an English-speaking country because I would not have the facility to argue effectively in that language. This of course was wrong reasoning as I discovered later on. Since economics as a discipline was completely unknown to high school students in Cyprus in the late 1940s and early 1950s I thought I ought to study medicine. And my brother was then studying medicine in the University of Athens. So, I took the premed route, and I enrolled in various science courses; in particular, I recall a course in organic chemistry, which I took either the second semester of the first year or the first semester of the second. I did not like this course at all; it involved too much memory work and very little understanding. So I quit this plan. And gradually as I was groping for something else, I enrolled in a couple of courses in economics, and the subject was more agreeable; so I majored in economics, and I finished my B.A. degree in two and a half years.

Let me get this right—you wanted to become a lawyer but convinced yourself that it was not possible because of the language, you turned to medicine, but eventually you graduated in economics in two and a half years? How was that possible?

It was possible in those days. If you had accumulated 120 units you graduated. So by taking more units during the semester and the summer terms I was able to do it, and I did it in two and a half years. What clinched economics as my choice for a major was the following. One of the economics professors was an old gentleman by the name of Edward Hale, who taught macroeconomics according to Keynes. I was very impressed by him; he was very lucid and compelling. So the subject intrigued me, and that's how I wound up majoring in economics. My intention, when I went to Texas, was not that at all!

So you majored in economics by accident! Besides Edward Hale's macroeconomics, was there any other subject that inspired you, say, mathematics or statistics?

It was not really by accident; it was choice under time constraint! As to the other part, unfortunately, mathematics played no role. In fact, people in the University of Texas at the time discouraged the use of mathematics; they thought people who used mathematics in economics really did not know economics and were merely hiding their ignorance behind mathematical symbols. That was a universal view in that department at the time. It was peopled by institutionalists, most of whom were trained in the 1920s and 1930s, several of them at the University of Wisconsin, then the major center for institutionalist economics.

Were there any major influences when you were a student? Was there a professor that had a lasting effect on the direction of your work then or your later development?

Yes, well, I have to tell you the story. When I was about to graduate I applied, I was encouraged to apply, for a Woodrow Wilson Fellowship, and I indicated as my choices a number of the big schools as I understood them at the time—Harvard, Berkeley, Stanford, but oddly enough not Yale or Princeton.

You must have done extremely well on your first degree.

Indeed, I graduated from the University of Texas *summa cum laude* in two and a half years. So I was probably one of the best students they had there for at least a decade, if not a generation.

That's amazing!

What was usually the case with Woodrow Wilson competitions, there were one or two rounds of preliminary interviews of the candidates before arriving at the final selection point. Moreover, there was a regional allocation of the fellowships. The last hurdle was, as I recall, a final interview in late November (of 1956), as I was beginning my third year in college. A few days after I completed that interview some gentleman called me, I forgot where he was from, possibly the University of Michigan, and gave me the impression that I was one of the people selected and said to me, "You know, since Harvard has too many Woodrow Wilson fellows," he said, "probably they won't take too many more; on the other hand, MIT doesn't have many, and most likely they will take you, and in addition it has a good program in economic development"—

because in my application essay I indicated I was very interested in studying economic development.

Economic development was your choice because you intended to return to Cyprus (a developing country) or because of your interest in the subject?

No. It was because people at that age and time wanted to offer their services to the cause of "saving" or "improving" the world. "So economic development is why you want to go to graduate school?" he asked. And he said to me that MIT has a good program in economics and is very good in development so why don't you go there? I didn't know much about MIT at the time, so I listened to him and went to MIT.

You chose MIT over Harvard?

Yes. Because I was told if I insisted on Harvard they might not take me. So, that's how this started, and I think this had a profound influence on my future development.

At the time who were are the major professors at MIT?

Well the major figures in those days were Paul Samuelson, Robert Solow, Charles Kindleberger; I also remember Robert Bishop, who was not as well known at the time but was an excellent teacher. These were the people you came in contact with in the most popular or required courses in the first year, year and a half. Kindleberger did international trade, which I took in the first semester of the first year; Samuelson did microtheory, what one would call today the theory of household (consumer) behavior; and Robert Bishop did what one would call the theory of the firm. Robert Solow did a course on economic fluctuations (or business cycles) or something. It may have been called economic fluctuations, but I would characterize it as a course about who said what, when, and where; for example, Kondratieff said this about cycles, Hawtrey said this, some Austrian said that, Hicks said that, i.e., it was a tour of the literature and models on the "business cycle." I understand that later this course was renamed Economic Growth and Fluctuations.

I often felt that many of our instructors were very busy with their own lives and research activities and were not easily accessible. Some of them joked a lot; some were very direct and business-like. For example, Samuelson always joked around a lot to the extent that some of my classmates made it a point to collect all such jokes, no doubt to repeat them at a later stage of their career. It also appeared to me, perhaps wrongly, that he displayed mild contempt for his students, and he would ask, for instance, "Do you know what a derivative is?" and some students would raise their voices and say, "No, I don't know what a derivative is." And Samuelson would say, well, you know, "It's where you make an itsy bitsy change in this and get an eeny-weeny change in that."

That's wonderful. Any other memorable recollections from your graduate days at MIT?

Not really directly related. No, I don't recall anything striking.

Let's move on to the Ph.D. What was your Ph.D. about?

My Ph.D. thesis was inspired by Solow's article in 1957, I believe, in which he attempted to measure what has come to be known as productivity but was then termed *technical change*. He essentially took a neoclassical production function involving capital, labor, and "time" but otherwise left the functional form unspecified, except that the "time" component entered multiplicatively so that its effect was implicitly specified to be neutral relative to labor and capital. Taking logarithmic derivatives one obtains an identity between the rate of growth of output and the rates of growth of labor, capital, and this unspecified scale function of time. By making sufficient assumptions he was able to equate the weights attached to the rates of growth of capital and labor to the share of these factors in total output. The residual of this process is the graph of the derivative of this scale function—which was then called the technical change function. All this was noneconometric and could be thought of as a nonparametric measurement of the entities in question, except that the number of data points was very limited; this fact would tend to detract from the cogency of his findings.

Was your dissertation primarily empirical?

In my thesis I had a two-pronged approach. First, I had a theoretical model in which there were n sectors, each endowed with its own Cobb–Douglas production function. The share of resources available to these sectors was fixed. I was able to show that at equilibrium this economy would be characterized by a unique (aggregate) Cobb–Douglas production function. This part formed a paper that was published in the *Quarterly Journal of Economics*, ca. 1962. In the second part, I took Solow's work and I "econometricized" it. It turned out that, to get his results, Solow made enough assumptions so that my explicit use of a Cobb–Douglas production function did not involve any appreciable loss of generality relative to his paper. One then sees that if the producing agents in this economy minimize expected cost we obtain an econometric relationship between the logarithm of the parameters of the Cobb–Douglas function and the logarithm of observed factor shares. If we take the residuals, using the estimates so obtained, we have the scale technological change function "contaminated" by the structural error. If we give an explicit functional form to the technical change function, we may thus estimate the productivity (or technical change) rate of growth as a parameter. I applied this framework to test the hypothesis that the productivity rates of growth in the U.S. manufacturing and service industries are equal. In principle, this could not be done in Solow's framework since it lacked the probabilistic context in which to place the test of such a hypothesis. Of course in his framework you could do the exercise twice and look at the two rates so computed, but you could not in principle tell whether their difference is "significant." This part of the thesis formed another paper which was published in the *Review of Economics and Statistics* [19].

Did you have adequate training in econometrics or statistics?

I had some exposure, but I wouldn't say proper training in such subjects. When I was a student, there was no training in econometrics as such. In fact, there were very few departments in the United States that offered systematic training in econometrics during the period 1957–1960. There were no such courses at MIT. What there was at the time was a course by Harold Freeman, that taught us in a semester the elements of probability and sampling; we learned the geometric distribution, the Poisson distribution, the binomial distribution, the hypergeometric distribution, etc., but the emphasis was not on estimation or testing hypotheses. I do not recall if we did bivariate regression; we may well have done so. The major imprint of that course still remaining with me was one of the problems he gave us, which was to prove that the most likely day to be the 13th of a month is Friday! I solved this problem by going backwards using a perpetual calendar; when you do so you "discover" that the world must have begun on a Monday!

Are you saying that you had absolutely no training in econometrics and statistics as the terms are understood today?

Actually, I should revise my statement; there was a course in econometrics that I attended at MIT, but this was not a course regularly offered at the department. During my time as a student there we had a visiting professor, Robert Strotz, then the editor of *Econometrica* and a professor at Northwestern—later to become the president of Northwestern. He offered a special course in econometrics which had three students. What we did in that course was, as Strotz aptly put it, "We read together Chapter 6 of Hood and Koopmans"; this is the chapter in the famous Cowles Monograph 14 (Hood and Koopmans, 1953) which deals with the exegesis of the theory of estimation for simultaneous equations systems as given in Anderson and Rubin (1949, 1950).

So that was a valuable course for you?

That was the only source of econometric knowledge I had up to that time, but as I recall I assimilated very little, if any, of that material. But even though I did not learn very much in that course, this very experience, as well as a similar one with Theil in a series of lectures he gave at Harvard in the academic year 1960–1961, had the profound effect of kindling my curiosity about the subject, which later led to more productive endeavors.

So you went through the Koopmans and Hood article very carefully.

You might say that, but I don't think we gained very much from this activity because the exposition was inaccessible to us as well as to our instructor, due to its mathematical complexity. In fact the only recollection I have of this course is the following: one day Strotz came in and asked us if we knew how to differentiate the determinant of a matrix with respect to its (i, j) element. Of course we said no, and thereafter he spent an hour trying to show how. If you knew or had some idea what the exercise involved, at the end of the hour you were confused, and if you didn't know you still didn't know. So the mathematics really overwhelmed the students, who were totally unfamiliar with such matters.

When did you complete your Ph.D. at MIT?

As I remember, I ended my student career in January 1961 when I was awarded the Ph.D. degree. I essentially wrote my Ph.D. thesis in the summer of 1960, and I submitted it to my advisers; from their comments I made a few revisions and had it completed in mid-November of 1960. I remember there was some problem with scheduling the time of my defense because one of my advisers—Evsey Domar—wanted to leave early for a vacation and there was a danger that the time of the defense would be pushed into the next semester. The other two, however, Robert Solow and Edwin Kuh, prevailed on him, and I finally had my defense rather informally after dinner in Domar's home, sometime in mid-December, so that I could graduate in January of 1961 (the end of the first semester of the academic year 1960–1961). And, of course I was, and am, most grateful for that.

Was your mathematical background at the time adequate for an academic career as an econometrician?

Even though I did extremely well in terms of grades as a graduate student in economics, my experience with Strotz, and a similar experience with Samuelson in a course based on his *Foundations* . . . (Samuelson, 1947) with a similar incident regarding the solution of nth-order difference equations, made me keenly aware of the fact that I was mathematically very unprepared to begin a professional career as an economist or econometrician. Bear in mind that I had no mathematics as an undergraduate other than a course in college algebra.

You didn't?

No. You may recall, as I mentioned earlier, that in Texas use of mathematics in economics was regarded as a confession of economic unalphabetism—to coin a word. So in the summer before I went to MIT I began to read on my own some trigonometry, calculus, and other similar topics, and when I passed my field examinations back in 1959 I spent the next year taking some courses in mathematics. MIT in those days required for the Ph.D. degree not only that you write a thesis but also that you should complete a minor. And to do so you had to take at least three upper-class undergraduate or graduate courses in another department. So even though I could have satisfied the requirements for a minor by offering my previous work in sociology, which I did as a graduate student at Texas (between obtaining a B.A. in January and departing for MIT

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in August of 1957), I decided not to. Instead, I offered a minor in mathematics, by taking three courses at MIT: Linear Algebra, Probability, and Complex Variables. These were upper-level undergraduate courses.

As it always happens when one compresses the learning experience, I did not truly comprehend what I was dealing with, even though, in terms of grades, I did extremely well in all these mathematics courses. So I felt the need, after I graduated, to really learn some more about the subject because, as I may have told you on some other occasion, I strongly hold to the view summarized in the ancient Greek adage that says, when literally translated, "Half knowledge is worse than ignorance"; I felt insecure about what I knew, and I wanted to know more about it. As I also discovered later, from personal experience, you cannot operate effectively at the very limits of your knowledge and understanding. You must operate well within those boundaries.

After completing your Ph.D. you decided to become a postdoctoral fellow at Stanford University. How did that happen?

When I obtained my Ph.D. I applied for, and got, a NATO postdoctoral fellowship; this could be held or exercised in a qualified institution in any NATO member country. My initial intention was to go to Holland at Henri Theil's econometric institute, as some other American econometricians did, for example, Art Goldberger. It was very well known at the time and, quite probably, was the most famous econometric institute in the world. This particular intention was tentatively formed during the academic year 1960–1961 when Theil visited the United States and gave a brief series of lectures at Harvard, which I attended; this is the experience I noted earlier. So I spoke with him about this possibility, and he said, "That's fine, you can come there, but you must formally apply" (to the Institute), which I did, and I was accepted. However, as the time approached for the actual implementation of that intention, and upon deeply thinking about it, I said to myself, "Yeah, if I go to Amsterdam," or wherever he was at the time, "then maybe I will write a couple of papers with Theil, so I will have a publication record, but I probably won't learn much that will open new vistas for me, and when I come back I probably will not know much more than I do now." So in the end I said to myself, "You'd better go someplace else, and learn some more statistics and mathematics." Whereupon, for reasons that now I do not fully recall, I decided to go to Stanford, which had very good statistics and mathematics departments. And indeed I was not disappointed.

Looking back, how important was the decision to go to Stanford for your postdoctoral fellowship?

Some of the courses I attended there had an important impact on my later work. One was a course in analysis based on Royden's notes (which later became an excellent textbook [Royden, 1963]); this course was not taught by Royden, who was on leave, but by Professor Leuwe, who was subsequently tragically killed by a disgruntled (mathematics) Ph.D. candidate whose thesis was not accepted. Another was a course in multivariate analysis taught by Ingram Olkin; this course was normally taught by T.W. Anderson, who was also on leave. This was rather fortuitous because Olkin used matrix algebra very skillfully to tackle important problems in multivariate analysis.

What was different about Ted Anderson's approach?

Anderson has a more geometric orientation, and I don't seem to have much of a geometric intuition. But matrix algebra was really something that I learned very well at the time and was immensely helpful to me in later times. I acknowledged my debt to Olkin when I dedicated one of my books (*Linear and Nonlinear Simultaneous Equations* [11]) to him and to Theil.

2. ACADEMIC CAREER

Your first position was as an instructor and then as an assistant professor at Harvard in 1962. How did that come about?

Well, it came about rather informally. As I remember, I ended my student career in January 1961 when I was awarded the Ph.D. degree. In the next (remaining) semester of that academic year I was asked to teach the statistics course to first-year graduate students at MIT. Moreover, it just so happened that the person (another recent MIT graduate) teaching Mathematics for Economists quit and left Harvard rather abruptly in late January or early February, and so I was retained to replace that person. This was my first contact with Harvard. Although it was exciting to me to be teaching at both MIT and Harvard so soon after graduation, the semester proceeded unremarkably. A short time before the semester ended, and as I was preparing to leave for the new academic year and take up my postdoctoral fellowship at Stanford, I was called into the office of the chairman of the economics department (at the time Arthur Smithies), who after some grilling asked me if I would like to come to Harvard. After thinking about it and talking to my advisers, I said yes to Smithies. No formal offer was made; it was just an understanding. So I went on my way, confident in the new course I set for myself, and I enjoyed my life and activities at Stanford very much.

If I understand you correctly, your offer from Harvard came about in a most natural way.

Yes. I presume the Harvard faculty knew of me because I had been teaching this course there for a few months prior to my meeting with Smithies.

But even at that time it was a very prestigious position for you.

Of course. Actually, I should mention that while at Stanford I received a phone call from Francis Bator, to whom I was a research assistant while a student at

MIT. He had just joined the Kennedy administration in the office of the national security adviser—or whatever was the name of the office at the time. He strongly urged me to come and join his staff, but, after substantial soul-searching, and for better or worse, I decided that I should not interrupt my academic career.

You spent two years at Harvard as an assistant professor. Any memorable experiences or events during that time? Any interesting courses you taught?

Well, I taught econometrics, both graduate and undergraduate. I believe that, before I arrived, econometrics was taught at Harvard for a year or two by Stephanos Valavanis, who wrote a book on the subject (Valavanis, 1959).

Could you elaborate on this, because very few econometricians have heard of the Valavanis book? Neither of the classic textbooks by Johnston (1963) and Goldberger (1964) refer to this book.

I think Valavanis's book was called *Econometrics*, or something like that. It was a very good book for its time. It dealt mainly with instrumental variables and maximum likelihood, and I am not certain that it contained much discussion on simultaneous equations. And he probably taught, what he taught I don't really know, but he must have taught some regression and possibly some simultaneous equations theory. Unfortunately, while camping in the mountains of central Greece during his vacation he died by the hand of some soldier who had deserted from the Greek army. I believe this was in the summer of 1958 or 1959. Thereafter they didn't have a specialist in econometrics. When I arrived there in 1962 econometrics was taught by Robert Dorfman and occasional visitors like Ed Mansfield, and it dealt mostly with the general linear model and related topics.

What kind of topics did you teach in this course?

I departed from precedent and I began to introduce some multivariate analysis that is relevant to economics, and some simultaneous equations that I had learned on my own. And I believe that this was the first time after Valavanis that the students at Harvard were exposed to the new developments in econometrics in a structured and systematic fashion. I'm not sure that Valavanis had ever discussed two-stage least squares; evidently, he couldn't possibly have taught three-stage least squares. As best as I recall, this came after his death.

Did you use the Valavanis book at the time?

No, I believe it was out of print. I taught off notes I prepared and possibly used part of a book by Theil entitled *Economic Forecasts and Policy* (1958). What I do remember about this book is that it represented the covariance matrix of the limiting distribution as

$$\operatorname*{plim}_{T\to\infty} TE(\mathbf{ee}'),$$

where $\mathbf{e}$ is the vector of "sampling variation" for a structural parameter vector, as obtained from the two-stage least squares (2SLS) regression. I recall that vividly because I remember asking Marc Nerlove, who was a professor of econometrics at Stanford when I was a postdoctoral student there. Incidentally, this was one of the things that mystified me about the exposition of structural estimators in the econometrics literature. Although the concept of the limiting distribution is used in the papers by Anderson and Rubin, most econometricians at the time did not distinguish between the covariance matrix of the limiting distribution and the limit of the covariance matrix of an estimator. Even Cowles Foundation Monograph 14 (1953), the very excellent exegesis by Hood and Koopmans of Anderson's and Rubin's work, refers to the covariance of the limiting distribution of a maximum likelihood estimator as

$$\operatorname*{plim}_{T\to\infty} TE(\hat{\boldsymbol{\theta}}-\boldsymbol{\theta})(\hat{\boldsymbol{\theta}}-\boldsymbol{\theta})', \quad \text{(see, e.g., equation (7.4), p. 178).}$$

A by-product of my attempt to clarify what Theil and Koopmans might have meant was my appreciation of central limit theorems and the limiting distribution of estimators. I introduced these concepts in my book *Econometrics: Statistical Foundations and Applications* [1] (written in 1968), which is the first "textbook" of econometrics to explicitly present the probability theory background in substantial detail and to show how weak laws of large numbers and central limit theorems are used in order to establish the limiting distribution of structural estimators. Evidently, this material was already in the literature of econometrics by virtue of the Mann and Wald paper (1943), as well as the Anderson and Rubin papers in the 1940s and 1950s (Anderson and Rubin, 1949, 1950). However, prior to my book only the book by Malinvaud, in French (1960) and in its English translation (1966), had made an attempt in that direction. Malinvaud's book, however, was not much in use in North American or UK universities, and practicing econometricians did not find it very accessible. Books more widely used, like those of Goldberger (1964) and Johnston (1963) as well as subsequent editions, did not deal with such issues. I did the same in connection with distributed lags in a book published the following year, *Distributed Lags: Problems of Formulation and Estimation* [2]. I should also note that these two books ([1] and [2]) contain the first discussion of nonparametric estimation, in connection with the estimation of spectral densities, to appear in an econometrics "textbook." The book(s) also included an extensive discussion of bandwidths and spectral windows (now termed kernels), as well as the "uncertainty" principle. The latter means that it is not possible, for a given kernel, to

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manipulate the bandwidth so as to simultaneously reduce the bias as well as the variance of the ordinate estimators.

I think we have to explain to the younger econometricians that, at the time, very few universities taught econometrics as a separate field.

Indeed, econometrics probably was not known as a field in the overwhelming majority of economics departments at the time (early 1960s). Harvard was one of the few departments that had within it personnel (though be it very junior) who appreciated what econometrics had turned out to be. Other such departments were Yale with Koopmans, although by that time he had left econometrics and begun to work on other subjects; Pennsylvania with Klein; Wisconsin with Goldberger; Stanford with Nerlove; MIT with Frank Fisher; and others I do not recall at the moment. Possibly Chicago if Zellner had gone there by then.

Is it fair to say that econometrics as we understand it today was not clearly demarcated until the early 1960s?

If by that you mean the general professional view on the subject, then yes, and probably the middle to late 1960s. But there were a few people scattered about the world who had a keen appreciation of the problems occasioned by nonexperimental data, and these were the founders and early propagators of econometrics in the middle 1940s and 1950s. Such people tended to be statisticians or mathematicians by training, such as Mann and Abe Wald, Anderson and Rubin, Koopmans and Tinbergen and Haavelmo, to mention but a few. The problem was that, in the early 1960s, even the best of professional economists who specialized in empirical work (econometrics as defined at the time) found the existing publications on the theory of simultaneous equations very inaccessible. Also in the early 1960s Frank Fisher wrote a book on the identification problem (1966) which was essentially an accessible exegesis of a terse (seven-page) paper by Abraham Wald (1950) on the subject. This was a major work at the time and had a big impact.

And of course the two well-known econometric textbooks by Jack Johnston (1963) and Arthur Goldberger (1964) came along, and they helped to define the field.

Of course. They provided an orderly way that we can teach the subject.

You spent only two years as an assistant professor at Harvard, and then you had an offer for an associate professorship at the University of Pennsylvania. How did that come about, and why did you decide to leave Harvard?

Well, in late 1962 or early 1963 I was approached by Lawrence Klein and Ed Kuh. Ed Kuh, who was one of my Ph.D. thesis advisers, was at the Sloan School at MIT, and Lawrence Klein was at the University of Pennsylvania. They were the principal investigators for the Brookings Project. The latter was a major undertaking, funded by the National Science Foundation (NSF), whose objective was to quantify the U.S. economy. They asked me to write an empirical paper about some sector; I forgot exactly how it was phrased, but I did write a paper titled “A Model of Short Run Labor Adjustment.” I promptly began the paper and completed it in 1963 (or very early 1964), and it was ready for publication in the first volume of results for the Brookings model in 1964. You might say that what I produced was a revolutionary paper at the time, in that it was the first paper to base the empirical specification of the (market) aggregate demand function for labor on an explicit cost minimizing behavior with a production function together with the partial adjustment hypothesis. In my specification the demand for labor depended on the wage/rent ratio, i.e., the relative cost of labor relative to the cost of capital. Up to that point the prevailing specification was ad hoc, and the demand for labor was written only as a function of (expected) output, which was taken to be the observed output, with a partial adjustment argument thrown in occasionally. This may have been motivated by input-output theory, which of course would not be consistent with partial adjustment behavior. Partial adjustment models, which are the precursor of the error correction model, were very common in the late 1950s and early 1960s. Thus, it was natural in that era that labor would be put in that mold, meaning that optimal labor is a function of expected output, and the change in observed labor was proportional to the gap between optimal labor and existing labor at the previous time period. Basically, there was no organized way in which the demand for labor was derived from an explicit, optimizing, economic process. By contrast, in that paper I derived the demand for labor using a standard optimization argument and the partial adjustment hypothesis. In 1963 this was revolutionary. Unfortunately, that paper was not included in the first volume on the Brookings model even though I prepared it and delivered it on time for the 1964 publication. I was told that noninclusion in the 1964 volume was due to space limitations and because the editors wanted to prepare a second volume that would elaborate on the specifications reported in the first volume. I was too meek to protest, and, unfortunately for me, the second volume did not appear until 1969, by which time the formulation employed in my paper was taken up by others who published their papers in major journals in the meantime.

They published papers on the same idea before you had a chance to publish yours?

Yes. However, all was not lost. This paper brought me to the attention of Lawrence Klein, who offered me a tenured position at the University of Pennsylvania. I discussed the matter with John Dunlop, then the chairman of the department at Harvard; he explained to me that, while they liked me, he couldn’t promise anything because the department could not make a tenured offer unless a tenure vacancy was assigned to it by the university. Unfortunately, he said, no such as-

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signment was anticipated in the near future. Whether this was sugar coating or a statement of fact I do not know. At any rate, at the time I thought: why should I hang around Harvard and wait for a favorable situation to evolve? So, I might as well go to Penn, which was then on its way to becoming an excellent place.

You had a tenured offer after two years of teaching? Was this usual at the time?

No, that was rather unusual; some of my fellow assistant professors remained at Harvard for five or six years, but they were not tenured there.

Your stay at Penn was undoubtedly one of your most productive periods. How was the environment there conducive to this?

Well the move there was very good for me, because Penn was on its way to becoming the premier department for applied econometrics.

Because of Klein.

Indeed. In addition, there was a lot of applied work going on, and a lot of theoretical problems arose. And I found them very stimulating, and I remember being increasingly drawn to them, although at the time I did not neglect applied work. The mix of theoretical and applied work goes back to the beginning, because in my thesis I econometricized the study of productivity as given in Solow's 1957 paper and I tested the hypothesis that the rates of change of productivity in the service industries and in manufacturing are the same. As I noted earlier this part was published in the *Review of Economics and Statistics* [19]. Beyond that, however, I also produced an estimation procedure for the parameters of the Cobb–Douglas production function, using factor share data only, as well as a bias correction when one estimates the (logarithm of) these parameters and then uses them to derive what has come to be known as total factor productivity (TFP). The paper resulting from this work was published in *Econometrica* (1962) [16]. The work on the Brookings model created a very congenial environment; very real problems were investigated around me, econometric theory issues came up, and interesting material reached me. My first three or four years at Penn, say, from 1964 to 1968, were a period of great intellectual ferment for me and for econometrics in general.

Given that you were so productive at Penn, why did you move to the University of California, Los Angeles (UCLA) in 1971?

Well, for personal reasons I needed to leave Penn at the time, and I took a leave of absence and I went to UCLA in late 1970 or early 1971 thinking that I might want to stay there. I also liked California because California, southern California in particular, is similar in its climate and topography to the part of Cyprus I grew up. I went back to Penn for 1971–1972, but the lure of California was still very much with me. At any rate I came back to UCLA for the academic year 1972–1973 for a final time to seriously contemplate staying there

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for good. But, having spent nearly two years there, I decided, perhaps wrongly, that the environment in southern California would not be conducive to raising children because it offered so many temptations. In this aspect I was quite wrong, as the children of my friends and colleagues at UCLA turned out to be very responsible and successful persons. At any rate this sojourn ended my fascination with California, but I did not go back to Penn; instead I went to Columbia, where I have stayed ever since 1973.

Any memorable experiences at UCLA that you recall, during the time you spent there?

I met a lot of interesting people there. In the economics department I met Jack Hirshleifer and Armen Alchian, and I reconnected with Mike Intrilligator, whom I knew slightly from MIT days. In systems engineering I met Masanao Aoki and V. Balachrishnan, and at the Business School I met Jacob Marshak. These were very interesting people, and I enjoyed my association with them very much.

A memorable event while I was there was the following. I had recently published a paper on a distributed lag model with autocorrelated errors in which I gave a proof of the consistency of the estimator I proposed [30]. This was published in the *International Economic Review* (1969), of which I was the editor at the time. While at UCLA I received a letter to the editor by Edmond Malinvaud. He was saying, basically, that people like us (meaning, I presume, mainly me, but he graciously included himself in the category) who produced theoretical papers that were a guide to many other econometricians should be more scrupulous regarding the rigor of their arguments. In particular, he took issue with my argument of consistency that involved the assertion—I don't now recall the exact details—but it had to do with the implicit assertion that for some sequence of functions g_n and for $\theta \in \Theta$ where Θ is compact, if $g_n(\theta) \xrightarrow{P} g(\theta)$ then $g_n(\theta_n) \xrightarrow{P} g(\theta^*)$, where θ^* is a limit point, or the limit, of the sequence $\{\theta_n : \theta_n \in \Theta, n \geq 1\}$. In fact this implicit statement is true for any sequence of (measurable) integrable functions g_n, provided the convergence is *uniform* (on Θ), and the result would be automatic if the g_n are continuous—which they were in that particular case. But I did not know it at the time, and, evidently, Malinvaud did not know it either. This led me to appreciate more the role of probability theory as it is done in mathematics departments. So I sought counsel from one of the mathematicians I got to know at UCLA (T. Ligget), who steered me in the direction of the measure theoretic formulation of the (strong) law of large numbers. When reading up on the subject I discovered that, if enough moments exist, proving strong consistency is a "breeze." This personal discovery led to a reply to Malinvaud in the form of a more carefully crafted paper, "On the Strong Consistency of Estimators for Certain Distributed Lag Models with Autocorrelated Errors" [41], which was published (in *International Economic Review*) together with his letter to the editor in 1971. If one reads that paper one should not fail to notice that it is very much out of character with the econometric literature of the late 1960s and early 1970s, in that it used constructs like

$$\mathcal{P}\left(\sup_{T\geq T_0}|\hat{\theta}_T-\theta^0|>\delta\right)\leq\epsilon,$$

and invoked the Borel–Cantelli lemma to show that the event $|\hat{\theta}_n-\theta_0|>\delta$, *i.o.* (infinitely often) occurs with probability zero, i.e., the estimator is strongly consistent. This incident provided a powerful motivation for me to learn more about probability theory in measure theoretic terms, which I pursued when I went to Columbia and that ultimately resulted in my book *Topics in Advanced Econometrics*, vol. I: *Probability Foundations* (1989) [10].

Having decided that southern California was not a place to raise children, why didn't you return to Penn? As you said, it was the premier department for applied econometrics.

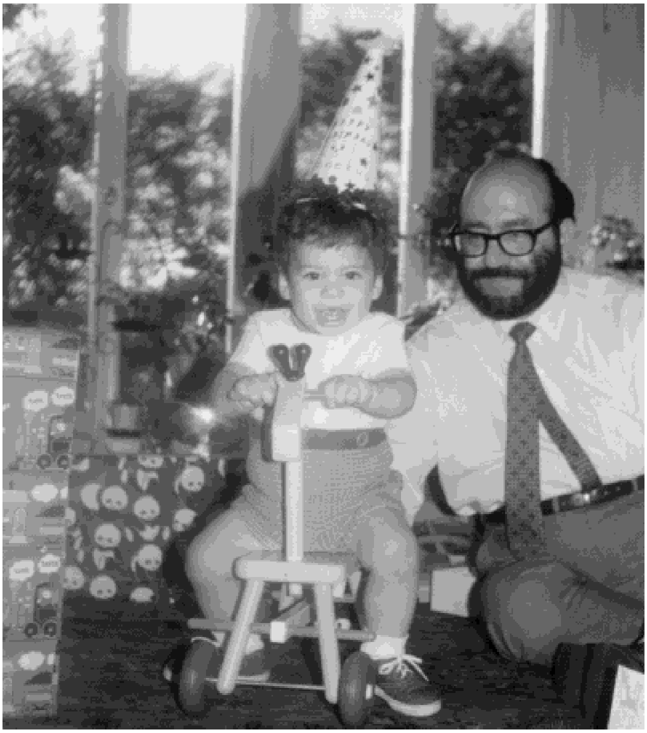
Yes, but once I was in play, so to speak, I was attracted to Columbia because it was seriously trying to rebuild after the riots of the late 1960s, and, also, I found the prospect of living in New York City very appealing. So I went to Columbia, and I haven't regretted going to New York.

It is well known that one's impact in a field is also a function of the academic success of one's students. Don't you have some lingering regrets for not having as many academically successful Ph.D. students at Columbia?

It is true that one's students are instrumental in enhancing one's impact. Most of my Ph.D students at Columbia were more interested in international organizations, the Fed, Wall Street, and similar venues, rather than academia.

You stayed at Columbia for more than a quarter of a century, but you did not manage to create a tradition in econometrics there.

Well, I didn't. You see, I am not the messianic type; I am very eclectic in what I do and seldom write variants of the same paper. My early period at Columbia also coincided with the time I began to have children, and I devoted a lot of attention and time to them, and so I toned down my professional activity. In addition, Columbia didn't have a tradition of econometrics, and it was difficult to convince people to hire several other econometricians. If I were a different person, and if I insisted upon my arrival that more econometricians be hired, perhaps the situation would have been different. The Columbia tradition has been in economic theory, with the brief exception of Burns and Mitchell. Because the university environment wasn't much to their liking they founded the National Bureau of Economic Research.



Phoebus and Phoebus Jr., first birthday, 1976.

3. JOURNAL EDITING

Let's just touch on a somewhat different subject. You've always been involved in editorial boards of major journals, but you also helped to found the *Journal of Econometrics* and served as managing editor and editor of a major journal, the *International Economic Review* (*IER*). From what I know the *IER* was a minor Japanese-sponsored journal in the early 1960s. How did you get involved with editing it and eventually transforming it into one of the major international journals?

Well, I don't know that I should get all the credit for the success of the *IER*. When I came to Penn in the summer of 1964 the *IER* was already three or four years old. It was set up with a Ford Foundation grant of, I think, $10,000 and was printed in Japan—because at the time it was much cheaper than printing in the United States. Its joint editors were L.R. Klein (University of Pennsylvania) and M. Morishima (University of Osaka). Local and printing expenses were underwritten by the Kansai Economic Federation, which is sort of a Chamber of Commerce for western Japan. It (Kansai) also gave indirect support by having all their members subscribe to the *IER*. In return, they had control over the

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copyright and the journal back issue plates and were the ultimate authority in financial matters for the journal. There was a deficit, for a number of years, and Kansai would cover it. By the time I became involved they were already tiring of the enterprise and were considering a reduction of the number of copies printed and not saving the plates of back issues. Klein was very busy on the Brookings Project at the time, so he asked me first to help him in September of 1964, almost as soon as I got there. So I helped him (basically I ran the journal with frequent consultations with him) without any title in 1964. In 1965 he recognized my role, and I was officially named the managing editor. As the managing editor, and later that year the American editor, I spent a great deal of time trying to improve the condition of the journal. In the summer of 1966 at the invitation of M. Morishima I went to Japan, in part to give a paper at a conference and in part to talk to the management of Kansai and the printer about the *IER* and its future. The meetings were very cordial, but I did not get any promise of long-term support, nor a denial of such either. But it was evident to me that their support would not have continued indefinitely.

How well known was *IER* when you took over?

When I took over the journal in 1965 the number of subscribers was around 400 to 500 or so, far too few for the *IER* to be on its own. I steered the journal to become more technical in tone; I improved the editorial process by speeding the refereeing phase and giving personal attention to disputes between authors and referees. Often I would arbitrate points of dispute myself or have a third reader give me an opinion if I did not have sufficient expertise on the matter. I also published a lot of papers on theoretical econometrics and high level applications in econometrics. The journal was becoming accepted in the mainstream; in the late 1960s, very early 1970s, it ranked close to *Econometrica* as a venue for econometrically oriented papers.

But now the *IER* is not published by Kansai, is it?

Indeed, not. In the summer of 1970, at the invitation of the Japanese editor, I went to Japan to negotiate a separation agreement with Kansai. The Japanese editor was not able or willing to do this himself; he thought he couldn't bring it about but that a foreigner would have a better chance of succeeding. And so I went to Japan, and in particular Osaka, where Kansai is based, and I had extensive conversations with a number of high officials of the federation. I explained to them that we had made significant progress in establishing the journal as a thriving scholarly enterprise, and that we were making good progress toward financial self-sufficiency, and that it was time to relinquish their control and vest it in the two academic institutions currently running the journal. They listened carefully, but they didn't give me an answer. I left then on my way to Australia (Monash University) empty-handed. A few months later, however, they agreed to relinquish control and the copyright and to give us all the back issues and plates in their possession. The successor publisher of the *IER* was an entity that was a partnership between the University of Pennsylvania and the

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Osaka University Institute of Social and Economic Research Association—the latter because Japanese (public) universities were not allowed to have private interests. This arrangement remains to this day. I remained the American editor of the *IER* after its restructuring, although after 1971 only nominally so. When I left to go to UCLA, day to day operations were carried out by E. Burmeister.

There is no doubt that the *IER* had really become a major journal by that time.

You asked not only about the *IER* but also about the *Journal of Econometrics*.

Actually, if I'm not mistaken, you are one of the founding editors of the *Journal of Econometrics*, and my question is, what was the reason for a new journal, given that there was a major econometrics journal, *Econometrica*?

Well I recall that we had discussions at that time, 1972 I believe, or maybe late 1971. I don't remember exactly when, but people felt that *Econometrica* was not publishing enough econometric theory and high level econometric ("high tech") applications. In addition, the editorial process was not very efficient, and it took a long time to get something published there. Also, at the time, there were not many publication outlets for a person writing theoretical or applied econometrics papers. And so I was approached by North-Holland—I believe D. Jorgensen was an adviser to North-Holland's North America division, and he recommended to them that they talk to me. I told them that I had just been through a spell of editorship and even though I was very sympathetic to the project I did not wish to bear the sole editorial responsibility; they should try and find other interested people. They recruited Dennis Aigner (then at Wisconsin), who was to be the day to day managing editor, and I and another person, I believe it was Arnold Zellner, would be the co-editors. And so this was how the *Journal of Econometrics* was founded in 1973—to give an outlet to people who were writing theoretical econometrics, especially, but also high level econometric applications, who had a hard time getting their voices heard.

In general, how did you perceive the roles of both the referee and the editor, knowing that both influence the way a discipline is likely to develop in the future?

Well, it is still my view that the editor's role is to facilitate the flow of current research output to the profession. I did not view this as a personal exercise of privilege or a personal exercise of power; I really viewed it as the discharge of an obligation under trust. Now I am aware of course that, as an editor through the choices I made regarding work that should be published, I influenced in some small way the development of the field, but my view was that I shouldn't have a vested interest in the field going in one specific direction rather than another. This would be an exercise of my prejudices, and a respectable editor should never indulge them. That does not mean that everything that is submitted should be accepted; it only means that published work should be subordinated to certain standards of scholarship, novelty, logical precision, and of course relevance to the field in question. Another obligation of the editor is to render unto all submissions a fair hearing. And when I was an editor I spent a reasonable amount of time trying to listen to, to read, the complaints of authors relative to their referees' reports, whether I knew either of them or not. And I would often ask the referee to clarify something, if I was convinced by the author that he (the author) had a legitimate grievance.

That's wonderful, but I'm not aware of any current journal editors who take the editorial process so seriously.

At the present this is not practiced very much, if at all, perhaps because there are too many papers, or because editors do not want to displease their referees. As a consequence, many papers get published that should not be published, and many papers are rejected which should be published. And I know this from acquaintances and personal experience. Perhaps this is not as important now as it was in the 1960s and 1970s, given the evolution of technology and the ability to disseminate quickly the results of current research. Nonetheless journals serve as a very important filtering device. One should always bear in mind that the major function of an editor is to help in the dissemination of new knowledge, provided it meets the criteria I noted earlier.

Primarily, criteria in scholarship?

In scholarship, yes.

I know that you are particularly sensitive to young people and new ideas from young people. How did you approach that issue in your editing days, in view of the fact that, often, young people have a certain difficulty explaining their ideas, or they might not use sufficiently diplomatic language when they write?

Well, one thing I can say is that when I was an editor I was always very sensitive to these issues, and in cases where I could diagnose such a problem, I would go out of my way to help improve the paper—if I was convinced that there was something there.

4. BOOKS IN ECONOMETRICS

Although over the years you have published several very influential papers, you are better known in the profession for your very successful textbooks in econometrics. Why do you find writing textbooks interesting even though there is a general tendency in the profession to undervalue the importance of textbooks?

Well, it is true that there is a general tendency to undervalue the importance of textbooks, but my books are not typical textbooks. I perceive them more as books that bridge the gap between ordinary textbooks and journal articles and as filters that distill and synthesize the wisdom of many contributors to the subject. On this score I was influenced in my writing by the way I learn when studying by myself. In many ways, as I might have told you at an earlier time, I basically write the books for my own benefit. I want to clarify what it is that we know, or think we know, about the subject because often the picture is not clear by reading a research paper. In a published paper there is no room to provide a complete argumentation, and often the person who writes at the time doesn't fully see the entire picture. And so what I try to do in my books is to really take the essence of what we know about a subject matter and present it in a complete and orderly fashion. A case in point is the material (papers) I published on simultaneous equations [1, 11], about which many people have told me that if I had written it using the traditional notation it would have had more impact. And my reply is that I did not write it to have an impact but to promote learning. My objective is to make the material clear and more intelligible to people. My notation was developed in order to unify the two different notations developed by the Dutch school and the Cowles Commission group, so as to ensure that everything is the same whether you consider a single equation or a system of equations. When I teach the subject at Columbia, my students over the years have had no problem understanding two- or three-stage least squares because the notation enables them to see these models as a simple generalization of the standard general linear model. In addition, it enables them almost effortlessly to access the maximum likelihood literature on the subject.

In the same vein, what are the features that render a textbook, as you perceive it, an important contribution or just another version of the same well-known blueprint?

To take simultaneous equations as a case in point, my view is that if somebody wants to appreciate what simultaneous equations are, one has no way of doing so by reading the original contributions in journal articles. It requires an enormous effort, and a long time, to put everything together. On the other hand, if one reads my book on simultaneous equations one should be able to absorb from beginning to end the fruits of the intellectual effort of numerous people over a period of 30 or 40 years. By contrast, a typical textbook merely lifts out portions of the literature and stitches them together without presenting an integrated and uniform exposition of the subject. In a different context, my students, or young professionals who were not my students, often tell me that they found my little book *Mathematics for Econometrics* [5] extremely useful because it summarizes several important mathematical results needed for simultaneous equations and other aspects of classical econometrics. One can find nearly all such results by searching through mathematical journals, or several mathematics textbooks, but this requires an enormous effort.

Your book *Mathematics for Econometrics* summarizes the mathematical results you accumulated in your attempt, in the first instance, to un-

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derstand and then explain the simultaneous equations model, but it has wider relevance to all classical econometrics.

Attempts, like mine, to systematize a field often lead to new results that are added to the accumulation of known results to produce a coherent story.

You published your first book, *Econometrics: Statistical Foundations and Applications*, in 1970 [1], and it became one of the standard graduate textbooks. The feature that caught my attention, even as a student, was that for the first time an econometrics textbook takes probability theory and statistical inference seriously. Up to that point, econometrics textbooks would treat these topics almost as an afterthought in a few pages of definitions. What prompted you to go in that direction?

In many ways the book reflects the way I managed to understand econometrics. As I was reading through the various papers (by prominent people) that constituted the econometrics literature at that time, I felt that there was too much hand waving and not enough invocation of proper probability theory. At the time I had a chance to learn some probability theory and multivariate analysis at Stanford, and I felt that these tools enabled me to put forward a coherent discussion of econometric techniques. Another topic that I felt I needed to explain in depth was limit theorems, because their discussion in other econometric textbooks did not go far enough.

Is it fair to say that in writing this book you put together a textbook that you would have liked to learn from as a student?

Yes, this is the kind of book I would have liked to have studied from. Indeed, this is the way I write all my books.

Rumor has it that at an important conference you defined econometrics as "a sequence of fads." Is the rumor true? And how did you reach this conclusion?

I did not say that exactly, but I did make remarks to that effect. I noted that over the years we were offered a list of "new" things that weren't really new but that became faddish and defined orthodoxy during a certain period. After the passage of some time it became clear that nothing was really resolved by them. A few things come to mind, like the error correction model, which is a descendant of the partial adjustment model. Another topic that has occupied people for a long time in terms of many heated discussions is that of causality, which eventually boiled down to testing whether a set of coefficients are equal to zero using an F-test. This is hardly something to get excited about. Another striking example is the famous vector autoregressive (VAR) model of the 1980s, which was not really new because it was researched by Mann and Wald in the 1940s, and at any rate it is nothing more than a simultaneous equations model with no exogenous variables and rather bewildering normalizations and a priori restrictions.

Would you consider the "distributed lags" literature in the 1970s such a fad?

No, I consider the distributed lags literature a precursor of time series econometrics.

But when econometricians were developing distributed lags in order to reduce the number of unknown coefficients in dynamic equations using ad hoc formulations, the statisticians were developing the Box–Jenkins autoregressive integrated moving average (ARIMA) model.

That's true, but the problem is that the level of mathematical sophistication of the average economist was not sufficient to absorb the Box–Jenkins model. Moreover, distributed lags were developed in the 1950s and 1960s, but the Box–Jenkins ARIMA model had an impact in the 1970s.

But it's fair to say that what the econometricians were doing in the distributed lags literature at the time was not in tune with the mainstream time series literature.

Wait a second. Are you referring to lag polynomials trying to impose restrictions on the (lag) coefficients?

Yes, I remember that as a student I had a very hard time taking this literature seriously, because the restrictions imposed on the coefficients seemed to me very ad hoc and often counterintuitive; remember the inverted V and W polynomial lags?

Yes, but when I refer to distributed lags I do not mean the polynomial lag structure—often referred to as Almon lags—I mean models where one has lagged endogenous variables among the explanatory variables. That's the part of the literature I worked on in the 1970s, and I consider my recent book *Time Series, Unit Roots, and Cointegration* [13] a natural extension of that literature. And I do not disagree with your characterization of the polynomial lag literature. I remember that I wrote a paper or two on the practice of "tying down" the polynomial generator of the lags. Many econometricians considered it innocuous, but in fact it restricts the class of shapes one allows the estimators to produce. A particularly objectionable practice was to "tie down the polynomial at both ends," i.e., at the beginning as well as at the end of the lag structure. But a simple consequence of continuity is that if something is zero both at the beginning and the end it must have a minimum or maximum in between. Often in empirical applications this produced the U or upside down V shape—from which one tried to derive some economic conclusions. I pointed out that such conclusions are unwarranted since the shape has been dictated by the investigator. Unfortunately, the practice persisted for some time thereafter. However, failure of practitioners to heed my warnings is not only a thing of the distant past. I have been pointing out since 1990, or thereabouts, that the empirical implementation of so-called structural VARs cannot produce information on the structure of the economy, owing to the fact that their structural estimators are only *just identified*. Hence, their validity is not testable, nor can it be defended against alternative findings (perhaps of opposite sense) that are obtained by a different set of just identifying conditions. They both have as their authority the reduced form estimators, which they share!

Looking at the timing and the content of your books, one cannot help but think that they constitute an outpouring of your research interests and teaching, which often did not coincide with the timing of the "fads." For example, your book on the simultaneous equations model (SEM) [11] was published at a time when the SEM was out of fashion. How do you explain that?

I meant to write the book on the SEM earlier, but I never had the time to write it, and I also wanted to write it a certain way. That doesn't mean that I couldn't have written a book on simultaneous equations in the 1970s, but it wouldn't be what I wanted it to be because certain results on dynamic systems of equations were not easily explainable at the time. Actually, one of the major innovations in that book was that *all* prior restrictions were imposed by means of Lagrange multipliers. This idea occurred to me ca. 1981 while I was in Uruguay. One of the problems with this formulation, which possibly accounts for the fact that in the 50 or so years of its existence the SEM was not formulated in this particular fashion, was that in order to obtain an explicit representation for the structural estimators it is necessary to invert a matrix of the form

$$\mathbf{A} = \begin{bmatrix} \mathbf{A}_{11} & \mathbf{A}_{12} \\ \mathbf{A}_{21} & \mathbf{A}_{22} \end{bmatrix},$$

where $\mathbf{A}_{11}$ *and* $\mathbf{A}_{22}$ are *singular* matrices. I was not aware of any results in the econometric literature that give conditions for its inverse, let alone an explicit representation for the inverse. The result then known, which also appears in the first edition of *Mathematics for Econometrics* [5], assumes that *one of these two sub-matrices is nonsingular*. It may well be that such results were available in the mathematics literature, but I was unable to find them—even after some inquiries with mathematicians that specialize in linear algebra. At any rate, I worked out a solution ca. 1984, which I hastily incorporated as an addendum in the second edition of *Mathematics for Econometrics* [9]. Another reason is that I did not always find it possible to write things when I wanted to write them for a variety of reasons, some personal and some due to other professional commitments. When I finally began writing the SEM book [11], I worked out the implications of the representation of the inverse of the matrix **A**. This was written up as a paper, "Specification Tests in Simultaneous Equations Systems," published in the *Journal of Econometrics* in 1994 [77]. It turns out that this formulation yields routine tests for the validity of (over)identifying restrictions. These tests were not well understood in the 1970s.

Is it fair to say that most of your papers actually emphasize the proper use of techniques more than concepts and methods? In contrast, your books exhibit more of a methodological dimension and dwell on foundational issues?

Well, in my view that is the right way to do things. You know, when you write a book about a subject you deal essentially with the foundations of the subject. Unfortunately, we might have succeeded all too well in informing the econometrics profession about techniques, and we need to redress the balance with discussions of methodology. Nowadays, the various econometrics packages have made applied econometrics "too easy" in some sense. They enable people to apply very complicated and sophisticated techniques with very little understanding by the applied econometricians that use them. And I think it's important for the training of applied econometricians to understand both the underlying econometric theory and the methodological aspects of empirical modeling.

Let me proceed to another of your textbooks, *Introductory Econometrics* [4], which covers more traditional topics. Can you elaborate on the motivation and the objectives of this second textbook?

In my teaching of what was the typical first year course in econometrics, roughly speaking, regression, I was dissatisfied with what was then available in textbooks; either the discussion was too shallow, or they did not spend enough time explaining what were the consequences of failure of the basic assumptions to hold. So I contemplated this book as being about the general linear model and what can go wrong with it, as well as a very quick introduction to the SEM. By the time I got to write it in 1977 I included a chapter on limited dependent variables—a subject frequently encountered in the literature by then. But to make the discussion to my liking I needed to have a systematic presentation of certain mathematical results. So I decided to collect all the mathematical results needed for a thorough discussion of econometric techniques in one easily accessible form. The mathematical appendix of that book was so successful that was later published separately under the title *Mathematics for Econometrics*, now in its third edition (2000) [14].

You seem to have a knack for collecting mathematical jewels needed to prove several key theorems in econometrics.

I'm most proud of the mathematical appendix in my 1970 book [1], which began this tradition and brought into the literature of econometrics what are currently known as kernels and kernel estimation in connection with spectral analysis. It also contained a collection of several important results in matrix algebra, one of which deals with the decomposition of positive definite matrices into a product of lower or upper triangular matrices (triangular decomposition). Much later, in the 1980s, it became widely known to econometricians as the *Choleski decomposition*, in connection with vector autoregressive models

(VAR). The problem with that appendix was that it was too far ahead of its time.

The Choleski decomposition is important because it arises naturally in the context of sequential conditioning.

Indeed, but I was surprised later, when it became so popular after it acquired a name, that there was no reference to its existence in the econometric literature of the early 1970s.

That brings me to your fourth book, with the title *Topics in Advanced Econometrics*, vol. I: *Probability Foundations* (1989) [10]. The mathematical sophistication of this book is much higher than the traditional discussion in econometrics. What was the main objective in writing this book?

As I looked at the development of econometrics in the 1970s and early 1980s, I became more and more convinced that the probability and statistics tools available to the average econometrician were not up to the task for a deep understanding of the developments, especially in time series, taking shape during those decades. In my attempt to understand these new developments I began to delve deeper into probability theory. I was fortunate to have Y.S. Chow as one of my colleagues at Columbia, and through him I was led to advanced probability theory, one of the topics to which he made many contributions and on which he wrote a textbook (Chow and Teicher, 1988). Initially I thought I could solve the inadequate background problem by urging my students to take courses at the statistics department, but that did not work. So I wrote this book as an attempt to bring these topics closer to the mainstream of econometrics. I am not sure whether I succeeded in that. I was encouraged, however, when R. Gallant published a textbook with the same aim in mind (Gallant, 1997). I now use both books in my courses here at Columbia, and a lot of students find mine easier to read, provided we omit the proofs of theorems. Basically, I published this book because I felt it was time to raise the probability theory training of students in econometrics. It is also my view that measure theoretic based probability theory is much more intuitively appealing than the usual analysis based probability, i.e., one that is taught solely in terms of density functions, etc.

Let me return to distributed lags, because the most successful of your books has been the one on this topic. The book went into a second edition and was also published in Russian.

The 1971 book *Distributed Lags: Problems of Formulation and Estimation* [2] was written more or less at the same time as *Econometrics: Statistical Foundations and Applications* [1] during the academic year 1968–1969, while I was at Stanford, on leave from the University of Pennsylvania. At the time I was improving my knowledge of time series, and the book on distributed lags was my way to systematize my knowledge. There was even a section on "spectral analysis," because I became aware of new developments in time series through Parzen and Jenkins, who were at Stanford at the time. We talked a great deal about spectral analysis, and I incorporated a fair amount about the subject in both books ([1] and [2]); the latter includes a semiparametric rendition of the rational distributed lag model—on the assumption that the error process is only covariance stationary. At about the same time or shortly thereafter, one of Parzen's students (Grace Wahba) published a paper on the rational distributed lag model (Wahba, 1969).

This brings me very conveniently to your last book, entitled *Time Series, Unit Roots, and Cointegration* [13]. What was the main objective in publishing this book?

As I mentioned earlier, I view this book as a natural extension of my distributed lags book; it provides a coherent account of the developments in time series econometrics of the last 20 years or so. When I began to read the literature on these new developments it became clear to me that a lot of the tools needed by the average econometrician to understand this material were not conveniently available. Things became much clearer when I came across the work of Peter Phillips in the late 1980s because he gave the literature the proper mathematical formulation. That was the key: to set up the mathematical framework in which this whole literature can be properly understood.

Is that the reason you dedicated this book to Peter Phillips?

Yes. I consider his work fundamental in shedding light on this whole literature. Introducing into the discussion the functional central limit theorem and the continuous mapping theorem added much needed clarity to this literature. For me this was a major intellectual contribution not only to econometrics but to statistics as well.

Is it fair to say that your book *Time Series, Unit Roots, and Cointegration* [13] constitutes a distillation of what you've learned over the last decade or so in your attempt to understand the new developments in time series econometrics?

Yes, it is. In addition, it contains some new material on cointegration tests that are not based on the null of cointegration fully exploited, as well as tabulations for the test statistics of such tests, including those of Johansen (1995). All my books were born out of my attempt to understand a certain problem and the literature it engendered. Indeed, most authors of books of a similar type would tend to say that what they wrote represents their personal synthesis and understanding of a given subject.

Isn't it unusual to admit that your professional life has been a continuous learning process the fruits of which are published in books?

Well, I think this is the essence of scholarly life; as Socrates is reputed to have said: I learn even as I grow old.

5. RESEARCH STYLE AND RESEARCH AGENDA

I'm interested in your views on the methodology of econometrics because I have seen a renewed interest in the issue of when data provide good evidence for a certain theory. Since the early days there has been little dialogue between theorists and econometricians. Theorists feel no obligation to take empirical evidence seriously because they often consider other people's empirical evidence unreliable. How could you address the question of the reliability of empirical evidence?

You have to realize that the nature of theory is not well understood in economics. A theory (of an entity or a topic) is a grand conceptual scheme into which everything we know fits, and if some evidence doesn't you have to find a way in which you can accommodate it, or modify the theory. The only such theory we have is that households maximize utility and firms minimize costs. What somebody writes down about a specific phenomenon doesn't constitute a theory. For example, if I write that wages depend on the age of the worker and his education, this is not a theory; it is a particular conjecture about a specific phenomenon, and as a conjecture it has to be tested against data. Now econometrics was always thought to provide the tools to do exactly that. But has it achieved this? It's probably fair to say that it hasn't done so, and there are a number of reasons why. One of them is that as economists we are studying phenomena that are not subject to experimentation as in the natural sciences.

We do not seem to pay much attention to such problems in econometric modeling these days. Do you think that it's important to institute a dialogue between theorists and econometricians, so as to ensure that theory and evidence go hand in hand?

Yes, indeed. If you go back and you read what people in the Cowles Commission were writing in the 1950s and 1960s, you will see that this is what was envisioned for econometrics. One envisioned a process by which the empirical evidence would influence the conceptualizations of economic theorists. The problem has been that in succeeding generations the perception that has come to dominate is: "You pays your money and you takes your choice," meaning that the empirical evidence is very ambiguous and therefore should not bound the imagination of theorists. Although, in my view, this is very exaggerated, nonetheless one is hard put to find in economics strictly stable empirical regularities. Writing in the 1950s and 1960s Lawrence Klein used to speak about the "great ratios," the ratio of consumption to income, the capital labor ratio, the share of labor in national income, and so on. In his view these were the great and abiding constants in economics. Unfortunately, later history has not been very kind to this perception.

What did you generally look for when you decided whether to work on a topic or not? Is your research driven by applied problems, or do you have a grand scheme of how econometrics should develop in order to meet its objectives?

I don't have a grand scheme. I try to work on things that I have an interest in or on problems that arise in empirical work that is done either by me or those around me. In that sense being at Columbia was something of a disadvantage because of the lack of empirical work on the part of my colleagues, so that this source of inspiration was not always there. For that reason, more recently my research interests have been mostly personal. I pursue something because it's of interest to me.

Did you have any false starts in your research? Have you worked on a topic for months or years and eventually given up?

Yes, I did. At some stage I thought I would become an expert on small sample distribution theory. After a number of years of putting together a lot of results, I realized the futility of the exercise and did not pursue that subject any further. Another topic that I did not pursue very much after an initial investment was Bayesian econometrics. I came across Bayesian techniques in the very early 1960s. I took a course with Howard Raiffa (of the Raiffa and Schleifer *Applied Statistical Decision Theory* [1961] fame) in 1960 at Harvard Business School. After trying to apply these methods to a number of problems in economics, I decided that they were too extreme in relation to their demands on information. I wrote a paper on using Bayesian methods to estimate the mean function of some distribution during the summer of 1961 while I was at Yale, but I never published it. Recently, this sort of exercise is found in the form of "Bayesian updating" in some models of innovation or technical change.

This was a decade before Arnold Zellner's well-known book that introduced Bayesian methods to econometrics.

Indeed, Zellner made many, many important contributions to econometric modeling using Bayesian methods.

Returning to your research as it developed over the years, I have the impression that your early papers were applied but gradually your research became more and more theoretical. Did you plan that, or did it just come naturally as you realized that your comparative advantage was in the direction of the more technical aspects of modeling?

No, that came naturally because if you look at the early 1960s, most people who later came to write theoretical papers didn't start as econometric theorists. They grew up as practitioners but felt the need to learn more about theory to understand and extend the methods they were using. That's how I became an econometrician, and most of the people of my generation like Griliches, Fisher, Jorgenson, and Nerlove had similar experiences. I began as an applied econometrician, but as I became more and more knowledgeable in theory, I became more and more interested in technical issues, and they began to claim more and more of my efforts. Besides, I get bored very easily; I couldn't write 10 papers on the same basic subject. I prefer to write papers on different topics, even though when one wants to make a name for oneself one should stick to one topic. But I didn't like to do it.

Is it fair to say that your earlier research was basically a natural extension of your Ph.D. on production functions? You have been successful in placing most of your early papers in the top journals, from *Econometrica* to the *Review of Economics and Statistics*.

Yes, it is true that my research on production functions occupied me for a while. The earliest paper I published was in the *Review of Economics and Statistics* (1963) [18], which was the empirical part of my Ph.D. thesis. Looking back at it I wouldn't change its contents to any significant extent.

So turning to the other published papers, beginning in the early 1970s you had a series of papers on the SEM estimators, and their interrelationships, research that culminated with your book entitled *Topics in Advanced Econometrics*, vol. II: *Linear and Non-linear Simultaneous Equations* [11]. Would you say that this was one of your main research projects over the years?

Yes, the simultaneous equations model (SEM) occupied center stage on my research agenda until the middle 1980s. I have to stress that originally this research was driven by my desire to understand what I was reading and what I was doing in my applied research. It all began when I came across the problem of the nonexistence of (finite sample) moments in the case of the two-stage least squares estimators during the academic year 1961–1962, while at Stanford. This created a severe intellectual shock, given the fact that highly authoritative sources of the time, as I noted earlier, described the large sample variance (covariance matrix) of a structural estimator as

$$\operatorname*{plim}_{T\to\infty} TE(\hat{\boldsymbol{\theta}} - \boldsymbol{\theta})(\hat{\boldsymbol{\theta}} - \boldsymbol{\theta})'.$$

How could multiplying by T something that was unbounded yield a finite probability limit as the sample size (T) increased without bounds? I began exploring the problem by digging into various probability theory books, which led to my first book [1], where I tried to demystify the properties of limited information estimators for people without a very strong background in mathematics or probability theory.

Let me ask you about your research in applied finance. How did that research come about?

This research was initiated in a very circuitous way. When I was at the University of Pennsylvania I was reasonably good friends with Irwin Friend (the chairman of the Department of Finance at the time), who was working on a project financed by the U.S. Congress on reforming the banking system. I remember writing a paper for that project on savings and loan banking and the nature of their assets and liabilities. After I left Penn, I was invited in the early 1980s to, and presented a paper on savings at, the Festschrift celebration on Friend's 65th birthday. During this conference I was surprised that another former colleague of mine from Penn, Steven Ross, was so critical of certain remarks to the point of deriding Irwin Friend's arguments. Steve was then expounding on the virtues of arbitrage pricing theory (APT), about which he had just published a paper with Richard Roll (Roll and Ross, 1980).

Irwin, who had some doubts about APT, asked me to join forces with him and investigate the matter further. So I looked into the subject, and I read Steve's paper on arbitrage pricing theory. Its conceptual framework is that the rate of return on a risky asset is the risk-free rate plus some (risk) "premia" due to various "risk" factors, plus an idiosyncratic error. So basically, as exposited in the empirical application (for the purpose of testing the relevance of APT) by Roll and Ross, their formulation of APT was the standard variance components factor analysis model. Then I began to investigate the econometric procedures used by Roll and Ross to test their formulation of APT. I soon realized that there was a problem with their empirical finding that only three to five factors were involved in determining the risk premia (which are essentially the coefficients of the factors so determined) for risky assets.

What was the problem with this study?

The problem was with the way they applied factor analysis to stock prices. They had taken groups of 30 stocks, repeatedly, and for each group they determined (from time series of closing prices) the number of factors, by the standard principal components variant of factor analysis. Having so analyzed a substantial number of groups they concluded that at most three to five factors were responsible for the riskiness of risky assets. However, this is a faulty analysis. In the classic paradigm of factor analysis there are n students taking m tests. Their scores are recorded, and, through a variance decomposition process, one determines the number of factors (say, k) "responsible" for their scores. In the context of the Roll and Ross application the number of students is the number of trading days for which stock prices are observed. The number of tests is the number of stocks being considered. Since the universe was presumed to be *at least* the number of stocks traded on the New York Stock Exchange, the number of stocks to be considered was very large. Certainly, the computing capabilities then available were insufficient to the task. By dealing with a number of groups of 30 stocks Roll and Ross thought they could circumvent the computational limitations. In so doing, however, they committed a serious error—evidently due to inadequate understanding of the underlying econometric theory. Our paper [63] pointed out the empirical fallacies of their analysis and showed that if factor analysis were properly applied, the number of factors determined would be an increasing function of the number of stocks "factor-analyzed."

From personal experience I know how difficult it is to publish papers which call into question the conventional wisdom. Was it easier at that time?

Not at all. We actually had a very hard time publishing this paper. I have to admit also that if I had not been as well known at the time, and if Irwin Friend had not been a past president of the American Finance Association, that paper would never have been published. In fact, its publication was held up so that Roll and Ross could prepare a reply that appeared in the same issue (Roll and Ross, 1984).

I couldn't help noticing that some of your earlier papers were published in *Australian Economic Papers* and the *Australian Journal of Statistics*. Is there a reason for that? There is also a paper with the intriguing title "On the Game of Maximizing $\bar{R}^2$" [38].

That came about because in the summer of 1970 I visited Alan Powell at Monash University (in Australia) for about 2–3 months. I came in contact with various Australian econometricians in Melbourne, Sidney, and Canberra, and I was invited to submit some papers to local journals; they suggested the *Australian Journal of Statistics* and the *Australian Economic Papers*. As I remember, I published two papers in the latter. One is the paper you mentioned, and the other is one that shows the equivalence between ML and feasible Aitken (GLS) estimators in a system of general linear models (often referred to as seemingly unrelated regressions), provided the latter is iterated [42]. At any event both are asymptotically equivalent. In the *Australian Journal of Statistics* I published a paper titled "A Simplified Structural Estimator for Large Scale Econometric Models" [43]. The $\bar{R}^2$ paper to which you refer shows the following: given that we have k explanatory variables in a regression, and we wish to maximize $\bar{R}^2$, under what circumstances should we introduce the $k+1$ variable? The answer is: when the latter's t-ratio is equal to or greater than one. This is really not a probabilistic argument; it is basically a computational argument. It is conceivable that such a result was already in the statistics literature, but neither my colleagues there nor I knew of it, so I derived it completely on my own. Both papers in the *Australian Economic Papers* resulted from discussions with colleagues in Australia.

Do you have any thoughts on the directions you would like econometrics to proceed through the next decade or so? What areas do you feel are promising to pursue further?

I believe greater understanding of financial time series is a very important topic. In a broader sense, we have already created an impressive array of procedures to handle a very wide variety of problems that arise in empirical research. We should now devote a great deal of attention to improving the training of applied econometricians, because as the discipline has developed the bulk of ap-

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plied work is no longer done by "econometricians" but by persons who are labeled "labor economists," macroeconomists, "applied microeconomists," etc., whose understanding of the subtleties of econometric theory is less than perfect to say the least. In addition, from a more methodological point of view, I would like to see a thorough and careful investigation of *all* empirical implications of a model. In particular, I think that examining a model's implications piecemeal is not very helpful in determining the empirical relevance of a model. Also in the literature of econometrics there is very little work confirming previous research. Often it is quite impossible to reconfirm the results presented in an empirical paper, however conscientiously one might try. As a consequence, we are always presented with new results, especially in macroeconometrics, that are not commensurate with previous findings, without a serious attempt to reconcile them and often without comment.

Isn't that a symptom of the basic problem of unreliable empirical evidence? The situation you describe is a classic example of empirical evidence that does not stand up to closer scrutiny. In my mind the most important cause of this state of affairs is the fact that the overwhelming majority of estimated statistical models are misspecified, rendering any inference based upon them unreliable.

I am not sure I would put it that way; the problem appears to be that as fashions change the relationships estimated are completely disconnected from the past. I remember, for example, sometime in the early 1980s I did a paper with one of my students [61] comparing the forecasting performance of a version of the Wharton model as compared with Box–Jenkins time series forecasts, following up on previous published work on a similar subject. The first journal it was sent to declined publication on the ground that proper comparison dictated that we should use VARs. Of course, VARs were completely impossible to implement in that context because of their excessive parameterization. This, however, did not prevent the editor and his referee from so opining, in deference to the new orthodoxy. It is true that a lot of published empirical evidence is likely to be unreliable, because of inappropriate data, or inappropriate econometric procedures, but it's not clear how one remedies the situation. For example, some computer packages provide an array of "diagnostics"; while this is a welcome change from past practices, it gives a false impression of reliability. Thus, the typical test for heteroskedasticity tests the hypothesis that the error variance is a linear function of (some of) the explanatory variables; the typical "misspecification" test tests the hypothesis that the explanatory variables enter quadratically; some offer tests for the normality of the regression errors, and so on. Such results do not necessarily ensure the integrity of the specification, nor is the rejection of normality of any particular significance because of central limit theorems. So in the absence of controlled experimentation it is inherently difficult to vouchsafe the reliability of empirical evidence to the degree one would wish to have in such matters. Consequently, the journals are faced with a very difficult task in imposing standards when a clear consensus is unavailable.

I would imagine that the first step in that direction might be for the journals to insist on the statistical adequacy of the estimated models using thorough misspecification testing. They should ensure that the author provides enough evidence that probabilistic assumptions underlying the estimated model are not rejected by the data.

I am not sure that we can arrive at a consensus of what constitutes proper vetting in this context. Suppose one has a model and a procedure that produce a certain asymptotic result; how could his results be contested or checked if we are dealing with a sample of size 80?

That is a somewhat different issue which concerns the accuracy as opposed to the reliability of empirical results. The statistical adequacy issue can be dealt with if the journal is housed in a department with a sizable graduate program. The editors can then employ an army of competent graduate students to reproduce the empirical results and perform a thorough misspecification testing on these models to assess their statistical adequacy.

But of course this assumes that there is a broad consensus.

The only real consensus needed for statistical adequacy concerns "what the underlying probabilistic assumptions are," which should not be difficult to agree upon.

You should remember that in the last 20 years or so, a very disturbing development in the empirical literature is the fragmentation of the discipline. In the "old days" of the 1960s and 1970s or the 1950s, the people who were working in production functions, industrial organization, investment, consumption, labor, and a number of other topics were primarily econometricians who wrote their own computer programs and had common concerns about specifications, about appropriate research methods, and, of course, about appropriate treatment of the evidence. Over the years we have been too successful. We have created the appearance of sophistication in the form of computer programs that can easily apply all these "sophisticated" procedures without the user having a clear insight into, or understanding of, the process. Journals have also proliferated, so old results are always rediscovered in highly specialized journals, even though 20 or 30 years ago the same topics had been researched in econometric journal publications. Because of the fragmentation, many procedures are quite acceptable within the group primarily served by the specialized journal but may not be acceptable in another group.

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Are you saying that the availability of sophisticated software that does everything for the user is a double-edged sword, in the sense that it can lead to mindless but superficially "sophisticated" empirical analysis?

And has really undermined serious econometric investigation.

As well as the whole empirical modeling process.

Quite possibly, but this problem has been around for some time and has many variants. I remember, when I was a student ca. 1960, I took a course in complex analysis with Levinson, who was a well-known mathematician at MIT. And he was bemoaning the state of affairs in certain areas of mathematics where the easy access to computer simulation had "robbed the younger generation" of certain ingenuity in determining the shape of the graph of a given function. In the absence of computers one had to find out what the curvature was by examining various derivatives of the function, and this afforded an opportunity to exercise one's ingenuity. While in that case the problem is rather innocuous, in the case of econometrics the problem is much more severe. It comes from the fact that the people who write the programs and the people who use them are very different. Those who write the programs are generally conversant with econometric theory, while those that use them, more often than not, are only vaguely familiar with econometric theory.

6. TEACHING, STUDENTS, AND SUPERVISION

Do you consider yourself a natural teacher, a natural researcher, or both? Do you find research and teaching to be complementary?

Yes, and I consider myself both; I wouldn't want to do only one. There has to be variety in my activities because it would be too boring to teach the same thing over and over again, and, on the other hand, sitting in one's office and writing without the intellectual stimulus of teaching is not very agreeable to me either.

Do you find that often teaching forces you to consider various things from a more basic or elementary point of view so as to be able to explain them to students, and that affects your research?

Yes. I think teaching forces you to clarify a great many issues. Especially given my personality and the fact that I don't want to take anything for granted. I want to explain to myself first of all. And therefore this leads me to ask questions that most people don't ask. For instance, I'll give you an example; there's a paper of mine that maybe will come up later, which will illustrate this point. If you are interested in forecasting from an econometric model involving simultaneous equations, the question arises as to what are the standard errors you should attach to your forecasts. This problem was addressed by Goldberger, Nagar, and Odeh in the early 1960s, perhaps as early as 1961; I don't

Philip, Phoebus Jr., Alexander, and Phoebus, 1987.

remember exactly. In that paper (Goldberger, Nagar, and Odeh, 1961), to the best of my recollection, they simply expanded the nonlinear function of the induced restricted reduced form parameters (as obtained from the structural estimators) by Taylor's series and used the standard errors of the latter as they were available in the literature of the time. This is basically a technology that was available through the book *The Advanced Theory of Statistics* by Kendall and Stuart (1968–1973). But having gained some expertise on the application of central limit theorems to solve the problem of limiting distributions for structural estimators, I wondered what would be the distribution of induced restricted reduced forms, i.e., the reduced form parameters induced by the structural estimators, the entities whose standard errors were obtained by Goldberger et al. From the point of view of the practitioner the problem was solved. There were standard errors for the induced restricted reduced forms; what more could one wish? This, however, was not satisfactory to me because it did not allow for comparison among several restricted reduced form estimators. At any rate, my work that tried to answer this question led to the paper "Restricted and Unrestricted Reduced Forms: Asymptotic Distribution and Relative Efficiency," which was published in 1973 in *Econometrica* [49]. This paper

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showed that restricted reduced forms are, asymptotically, linear transformations of the structural parameter estimators, which in turn enables one to rank various reduced form estimators in terms of efficiency. Incidentally, it also showed that structural and reduced form estimators of any type are of the form $\mathbf{A}_T\boldsymbol{\xi}$, where

$$\boldsymbol{\xi}_T = \frac{1}{\sqrt{T}} \operatorname{vec}(\mathbf{X}'\mathbf{U}),$$

$\mathbf{X}$ is the matrix of the predetermined variables, $\mathbf{U}$ is the matrix of the structural errors, and $\mathbf{A}_T$ is a suitable matrix that converges in probability. Evidently, the nature of $\mathbf{A}_T$ depends on the problem considered. Even though this was an important paper, in that it unified regression, simultaneous equations, and several of its variants it received relative little attention, except for several papers that tried (unsuccessfully) to overturn its conclusion that limited-information-induced restricted reduced forms are not necessarily efficient relative to unrestricted reduced forms.

Do you consider yourself a self-taught mathematician who worries about the mathematical coherence of the argument, because that's what comes out in your books?

You might say this, but I don't know if it's true. My books are not typical textbooks in the fashion of current econometrics literature. They do not contain, unreworked, a bit from this paper, a bit from that paper, and so on. They try to develop a subject from beginning to end in a more or less unified manner; they take into account the literature, and if there are gaps in its development I fill them by "original" research. Their objective is to teach the rudiments and refinements of the subject to someone who is interested but does not know much about it. In many ways, I write them first and foremost for myself. To explain to myself what the subject is all about.

So, you write what makes sense to you after tapping into the literature until you fully assimilate the subject.

Correct.

Have you taught any courses beyond statistics and econometrics over the years?

Quite probably not; I don't know when was the last time that I taught anything other than econometrics, whether theoretical or applied. It's possible there was one time I may have taught a course in micro- or macroeconomics because somebody left in the middle of the semester and I had to help with it. And there was a situation, while I was at Penn, when someone (in another institution, possibly Haverford) died in the middle of the semester and there were some students that were orphaned, so to speak.

How do you see yourself as an adviser to Ph.D. students?

Well, it depends. If I happen to be involved in a project at the time, then I give them a problem and ask them to work on it.

Send them away?

No, I give them a problem and we discuss it, and then they come back and discuss with me their progress or ask for advice on how to proceed.

At regular intervals?

At regular intervals and on demand. If I am not involved in any project that they are interested in, I tell them to find a topic and then once they decide what they want to do, to come and talk to me. I will talk to them at length, try to sharpen their formulation and give them some advice or suggestion on how to move on. Generally, I spend a fair amount of time with them, listening to their problem and trying to assist them in solving it. I can spend an hour or two hours or whatever, if the occasion requires, to explain some things or to clear up some things and give them an opportunity to clarify in their own mind what it is that they want to do. So that's basically how it works; it's very time consuming, but it's a very worthwhile investment, because I think the training of younger people is of course the most effective way in which knowledge and skills are perpetuated. When it becomes somewhat burdensome is when the advisees do not work in the area where I work. For instance, I advised a lot of students at Columbia in the 1970s and 1980s in many applied areas, e.g., trade or development or consumption; all of them had an econometric component, but these were not areas in which I had done applied work. In the process they learned a great deal about making sure that the data they employ correspond to the theoretical concepts embodied in their models, about checking their data for errors and inconsistencies, about the proper econometric techniques, about the adequacy of models, about predictive tests, and about many other aspects of empirical research which are not taught in courses. In the 1990s I have reduced this activity considerably, as the department has acquired more faculty doing applied econometrics. I try to confine myself to econometric theoretical issues, but students doing only theory are very few, at present.

Do you have any regrets for not having more econometric theory students?

I think about it from time to time. When I was making decisions about where to go, I did not think that this was a problem. My view was, you produced your research output and if you gave superior formulations to existing problems, or if you solved a new problem, others would automatically adopt them. I discovered that that's not the case and that if you wish your formulation to have wide currency you must produce the students who will employ your formulation or else you must engage in self-promotion.

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Could you give me an example of what you have in mind?

As you may know, I have developed a notation that makes it very easy to handle simultaneous equations both from the maximum likelihood and the 2SLS, 3SLS viewpoint. I have also derived a set of very easily implementable tests of overidentifying restrictions, by the simple device of imposing *all* a priori restrictions in the simultaneous equations model by means of Lagrange multipliers. I have devised a test for cointegration rank based on the *unrestricted* estimator of the relevant parameter in the error correction model. Monte Carlo results indicate that when the (rank) null hypothesis is false this test does better than Johansen's for sample sizes one is likely to have in applied work. I have also shown that if we use the Kullback information apparatus, its simple identification condition, viz., that its global minimum is unique, enables us to derive all the necessary and sufficient (rank) conditions for the identification of simultaneous equations models. These results do not appear to be known widely. Certainly recently published econometric textbooks do not make mention of them. I am not sure whether it would have changed anything in my choice of a university home, but when I was making these choices I was not cognizant of this fact. As a personality trait I did not set out to, nor do I have the desire to, dominate any particular area.

Do you have any general advice that you would give to prospective graduate students in econometrics? What would you say if an undergraduate comes to you and asks: Is it a good idea to specialize in econometrics, and, if so, what kind of skills should I have in order to be successful as an econometrician?

Well, in econometrics, of course, you have to have some special skills in probability and inference theory before you undertake econometric studies.

Don't you also need some advanced mathematics in general?

Right. It goes without saying that you need to know some measure theory, matrix algebra, some analysis, and so on. And then the student has to learn to combine theory with empirical evidence. This requires the student to understand not only the complexities and concepts of economic theory but also the connection between these concepts and the available data. In applied econometrics it's very important to be able to relate the theoretical concepts to the right data. For instance the consumer price index is not the appropriate data series to use when discussing international trade (terms of trade) questions. The problem is that students often have very meager knowledge of institutions or of how data are collected and organized. Often, this leads to serious errors.

You have been a teacher for about 40 years teaching econometrics. How did the teaching (of econometrics) change over this period? Have

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you seen dramatic changes as to the content, and/or the emphasis in teaching econometrics?

Well in terms of my own personal experience, I guess I have been teaching econometrics in one form or another since 1961; therefore that would make it approximately 39 years. And, yes, there have been very dramatic changes, more in some schools than others. For instance, when I first taught in 1962, to talk in detail about the general linear structural econometric model (SEM, as you called it earlier) was a bit too bold. And to talk about some aspects of the relative efficiencies of various structural estimators using asymptotic theory was completely out of the question. I remember when I went to Columbia in 1973, just to give a course based on the more straightforward and elementary aspects of my book *Introductory Econometrics* [4] was considered very advanced. Changes have been made in other departments as well, but perhaps they were not as dramatic. At MIT, I am sure that changes were not as substantial because they had already routinely attained the level of that book by the early 1970s. By a process of diffusion, econometrics had become an essential part of economists' graduate education by the early 1970s, at least for the major economics faculties. Columbia was a laggard in that regard; when John Taylor and I arrived there in 1972 and 1973, respectively, the only other person who taught econometrics in the immediate past was Gregory Chow, who was then employed by IBM and came to Columbia once a week to give his lecture. So from that stage it's a long way to being able to teach an integrated sequence involving basic probability and inference, the general linear model, limited dependent variables, and simultaneous equations, which is what we do in the first year graduate course today.

How do you think computing capabilities, which have increased dramatically over the last two decades or so, are likely to change econometrics teaching? Should there be more emphasis on applied work, because it is so easy now to give assignments which involve real data modeling as well as computer simulations?

I imagine that will be the case. I hope that will happen, as students learn how to use computer programs, how to formulate problems econometrically and choose the right type of procedures. Also computer simulations ought to play a more important role. I find the latter to be a very useful research tool as well. In many instances you may have a difficult set of problems to solve, and it's not possible to solve them quickly. You may put forth a conjecture for a possible solution expecting it will yield, say, a good test. Before you invest heavily in developing a theoretical justification you can design a small-scale simulation and form a rough idea of the properties of the procedure. If the results are encouraging, you proceed with the theoretical justification. If not, you might decide to abandon this conjecture. I have used this approach when I was working on the book on time series. Very often there was a problem whose solution was not easily obtained. Instead of spending a great deal of time thinking and worrying about it, I just designed a Monte Carlo experiment and tried out my conjecture on it. If the results were a little encouraging, I was willing to expend the energy to establish rigorously its properties. If not, I abandoned it. So simulation could also be very important as an adjunct to theoretical research, but not if you use it as an excuse to avoid thinking about the problem and rely solely on its findings!

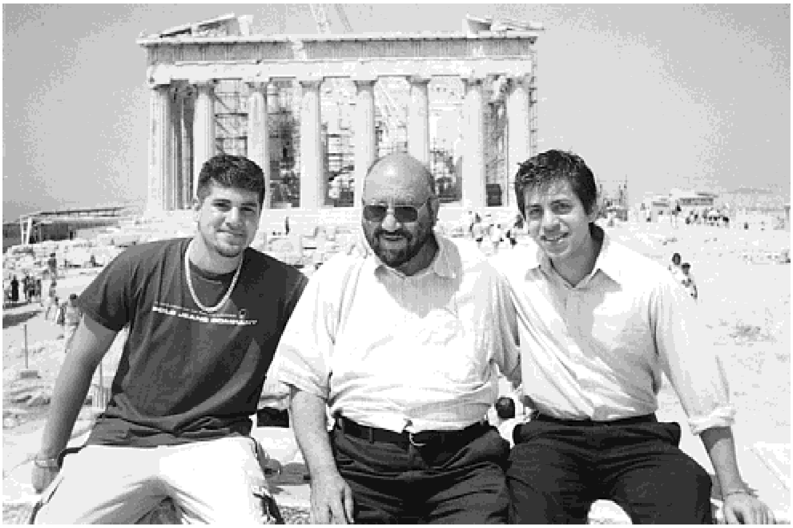
7. GENERAL QUESTIONS

Let's go to the last section on general questions. Do you have any other particularly strong intellectual interests beyond economics and econometrics?

Well, I am interested in history. I am also interested in analyzing current U.S. and international politics, but I am not interested in politics as a career. I read the *New York Times* daily, and the *Economist* less frequently, as well as other publications with serious commentaries on political aspects of various regions of the world.

Do you have any particular ideological viewpoint that often colors your political analysis?

Not any more, but when I was younger I was always attracted to socialism.



Alexander, Phoebus, and Phoebus Jr., in Athens, 2001.

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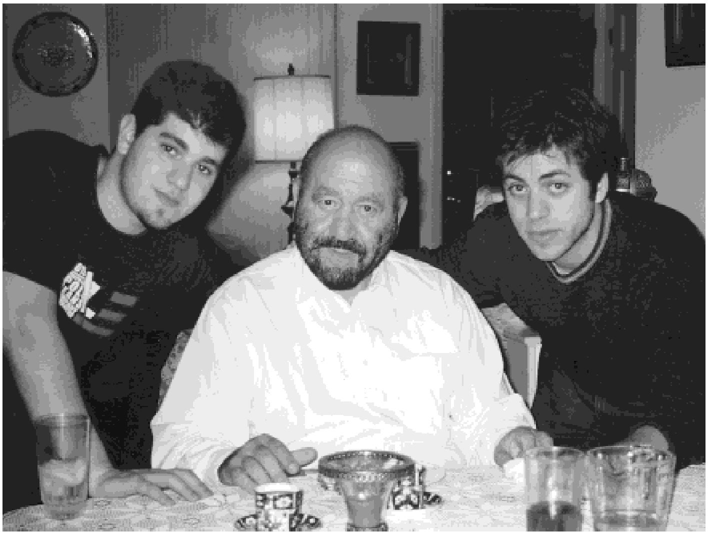

Alexander, Phoebus, and Phoebus Jr., in New York, 2001.

Were you a socialist, or somebody with compassion for the working class?

I don't really know; what's the difference? I am not passionate about any particular class affiliation. My father was a government functionary, he was not an oppressed worker, so I do not have a familial heritage along these lines. I am attracted to the kind of social democracy that tends to mitigate the income distribution vagaries of the market and which, to some extent, is still practiced in western Europe. But I have also learned over the years that many social welfare schemes that appear to make life easier for working people have very adverse impact on incentives and lead to appreciable decline in the productivity of the economy. I have also been severely disappointed at the consequences of public ownership. So basically, I learned that grand ideologies do not necessarily make for a good society, and I have become more pragmatic; I learned to choose from the options available and not to pine for an ideal system. What matters about economic resources is not so much who owns them per se but how efficiently they are used and who gets how much of what they produce.

Are you saying that you like socialism as an ideology but you also recognize the fact that the mechanism of a competitive market allows for a more efficient allocation of resources?

Yes, indeed, the market allocates resources more efficiently, and interference often leads to inefficiencies. On the other hand, the income distribution it produces is not too much to my liking.

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Can you describe a typical working day scenario for us? Where do you do most of your research? At the office, at home, or a combination of the two?

Oh, most of my research I do at home. I work at home early in the morning or late at night.

Are you a morning person or an evening person?

I am both. But in the past, due to family distractions, you know, I used to work mostly at night, but I didn't stay up until 5:00 or 6:00 in the morning; sometimes I stayed up until 2:00 or 3:00 at most.

Looking back over all of your work, publications, and all of that, do you have any personal favorites among the papers or the topics you have worked on?

Yes, I do. The first is "Alternative Asymptotic Tests of Significance and Related Aspects of 2SLS and 3SLS Estimated Parameters" (*Review of Economic Studies*, 1969) [32]. What is important in that paper is that it formulates the estimation problem of the SEM by first transforming all structural equations through left multiplication by $\mathbf{R}^{-1}\mathbf{X}'$, where $\mathbf{X}$ is the matrix of observations of all the predetermined variables of the system and R is a nonsingular matrix such that $\mathbf{R}\mathbf{R}' = \mathbf{X}'\mathbf{X}$. This is a useful transformation in that it immediately produces the optimal method of moments estimator for single equations and in fact suggests that in the transformed context least squares is 2SLS and feasible Aitken estimation is 3SLS. This is basically the same method used later by others in obtaining the nonlinear "2SLS" and "3SLS" estimators and ultimately the generalized method of moments estimator. The latter differs from the other nonlinear variants only to the extent that the "instruments" are explicitly defined within the model and that the error term has a more general distribution. The second is "Restricted and Unrestricted Reduced Forms: Asymptotic Distribution and Relative Efficiency" (*Econometrica*, 1973) [49]. This paper derives from first principles a particularly simple formulation that shows that induced restricted reduced form parameters (as induced by the estimators of structural parameters) are asymptotically *linear transformations* of the structural parameter estimators. Beyond that it also implies that all standard classical econometric problems, such as the general linear model, seemingly unrelated regressions, 2SLS, 3SLS, induced restricted reduced forms, and indirect least squares, are problems of the form: find the limiting distribution of $A_{jT}\xi_T$, where $A_{jT} \xrightarrow{P} A_j$ and ξ_T is a (the same) sequence of scalar, or vector, random variables that obey an appropriate central limit theorem. The third is "Identification and Kullback Information in the GLSEM," (*Journal of Econometrics*, 1997) [79]. This paper derives the rank conditions for identification of structural models using the minimum contrast framework. In fact, the asymptotic contrast function is the Kullback information, say, $K(\theta^0, \theta)$. By the properties of Kullback information K

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attains its global minimum for $\theta = \theta^0$, where θ^0 is the true parameter vector, and $\theta \in \Theta$, where Θ is the (compact) admissible parameter space. Identification for contrast estimators requires that the global minimum of the contrast be unique. From this property we can infer all necessary and sufficient conditions for identification in the SEM.

I would like to thank you for a most informative interview.

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