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THE ET INTERVIEW: PROFESSOR JAN KMENTA

Interviewed by John Lodewijks [1]

Professor Jan Kmenta.

Jan Kmenta is the author of the internationally respected text *The Elements of Econometrics* [1] and co-editor of several books related to econometric model building. His published research over 40 years relates to many facets of econometric theory and practice, including the estimation of production functions,

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the evaluation of structural econometric models, and estimation in the face of missing data. This work has appeared in the leading journals of the profession. A 1966 paper [11] is of historical interest for being the second econometric model constructed of the Australian economy. In his work, through both constructive contributions and methodological critique, he has always sought to highlight and advance the evidently close connection between economics and econometrics.

Kmenta was born on January 3, 1928, in Prague, the Czech Republic. He was educated at the Jirasek State Gymnasium in Prague (1939–47) and the Czech University of Technology in Prague (1947–49). At the University of Sydney (1952–55), Australia, he graduated with a Bachelor of Economics degree (First Class Honors). He obtained a Master of Arts degree (1959) and a Doctor of Philosophy degree (1964) from Stanford University with a doctoral dissertation entitled "Australian Postwar Immigration: An Econometric Study." After teaching stints in Australia, Stanford, and Wisconsin, he taught at Michigan State from 1965 to 1973 and at the University of Michigan from 1973 to 1993. He is currently Professor Emeritus of Economics and Statistics, University of Michigan, and Visiting Professor, CERGE-EI, Charles University, Prague.

His various academic awards and prizes include the Alexander von Humboldt Foundation Award for Senior U.S. Scientists, the Michigan Economic Society Best Professor Award, the Royal Economic Society Lecturer at the University of Leicester, and the Karel Englis Medal of the Academy of Sciences of the Czech Republic. He was awarded an honorary doctorate from the University of Saarland, Germany, and served as associate editor, *Journal of the American Statistical Association* (1973–1979 and 1985–1992), associate editor, *Review of Economics and Statistics* (1975–1992), and associate editor, *Metrika* (1981–1985). Kmenta was listed as 40th among all economists ranked by the total number of citations (Medoff 1989).

This is an edited transcript of two tape-recorded interviews conducted with Professor Kmenta in Sydney on March 12, 2004, as he visited the University of New South Wales to present the seminar *Econometrics: A Failed Science?* I am indebted to one of Kmenta's former students, Eric Sowey, for suggesting this project and for his enthusiasm and support throughout.

A CZECH REFUGEE IN THE ANTIPODES

Professor Kmenta, you were born in Prague in 1928; could you tell us about your upbringing and early education? Your education was obviously interrupted by the war.

I was born in what you may call a middle-class family. My father was a mailman/postman. My parents were very high on education, so they insisted that I go to a selective high school. In Europe, they call them gymnasiums. So I went to a gymnasium in 1939 and graduated in 1947. Then I went to the University of Technology of Prague to study statistics. I was actually in a predicament as to what to study because my favorite subjects in high school were Latin and mathematics, and it was impossible in Prague to combine the two. You either had to do one or the other. So I was looking for something else, and I was told that insurance mathematics and statistics might be an interesting field to get into. So I registered at the University of Technology, and I was taking courses in statistics. After two years there, I got involved in a "students' resistance movement"; this was under communism, of course, and we were soon found out by the police and a friend of mine was arrested, so I left the country as a political refugee. I was in a refugee camp in Germany for a couple of years, and then I came here as one of the New Australians. There was a big wave of immigration at that time from the refugee consulates—Latvians, Poles, Czechs, and Hungarians, from the East European countries. Transport to Australia was financed by international refugees' organizations that were running the camps.

In fact, I really didn't want to go to Australia. I wanted to go to the United States, but the U.S. quota was small for Czechs, and I would have had to wait about five years in Germany to get to the States. The only two countries that were open to us were Australia and Venezuela. I wanted to learn English, so I chose Australia, and I came here. When we got here, at the time, all immigrants coming from refugee camps had to work as laborers under two-year contract. That meant the Employment Office sent you wherever there was a shortage of labor in Australia. So I was sent to a stone quary near Picton, about 30–40 miles from Sydney. I was there for a while and then managed to wangle my way back to Sydney. There I was working first in a factory and later in a TB hospital in Randwick (on the site of the Prince of Wales Hospital) as an orderly, for the rest of the contract.

The University of Sydney had parallel courses in the evening, so it was possible to work during the day and take courses in the evening, which is what I did from 1952 to 1955. That saved me basically; otherwise it was impossible to study because I had to work to feed myself and pay the rent, so I could not attend the day courses. I wanted to do statistics, but there was no statistics department at the University of Sydney. There were two ways of studying statistics: one was through the mathematics department; the other one was through the economics department. In the mathematics department, I would have to take physics, which I disliked. In the economics department, I would have to take economics, about which I knew absolutely nothing. So, I decided I'd try that, and I enrolled as a Pass student at the beginning, but the professor of statistics, Stuart Rutherford, called me and said that I should switch to Honors in the following session, which I did. I am very grateful to him, because otherwise I would have had trouble getting into academia later. Academia was like a trade union cartel, and you needed an Honors degree to advance. So I registered for the Honors degree course, which was a four-year course, and then I obtained a teaching fellowship at the University (1955–1956).

My Honors thesis consisted of a collection of mistakes in Gerhard Tintner's book *Econometrics* (Wiley, 1952), so that was not really a high pressure effort. Anything about econometrics would have been an innovation at that time because it was such a new subject, so that was very easy. The final examination was, of course, harder but it—along with the thesis—earned me graduation with First

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Class Honors. I was not working half as hard then as I was later when doing the doctorate at Stanford.

Was there such a thing as econometrics at the University of Sydney at that time?

No, this was just the beginning of econometrics. There was only statistics at the University of Sydney in the economics department, but Rutherford was interested in this new subject of econometrics. The first textbook, Gerhard Tintner's *Econometrics*, given to me by Rutherford got me interested in the subject because it was obviously what economics needed.

After your Honors degree you took a lecturer's position at the University of New South Wales. What was UNSW like at that time?

I helped set up the first economics department there in 1956. The founding father was David Rowan, who came from the University of Melbourne basically for the purpose of establishing the economics department. He was of course involved a lot in administration because he had to build up the whole structure. He brought four youngsters into the department, the first of whom was Neil Runcie, a leftover from the NSW University of Technology, which was the previous name of UNSW. Then came John Pitchford, who had just graduated from ANU in macroeconomics, and a First Class Honors graduate from Sydney from my year, a classmate of mine, Ted Kolsen, doing micro, and myself. So there were four of us basically holding the whole economics department together at that point.

There were large enrollments—large classes to teach. It was basically the right thing for the university to start the economics program because not everybody could get into Sydney University and, in addition, there was an implied expectation that UNSW would be more, should we say, technically or business oriented, so that brought in quite a number of students. Thus it was quite a large department at that time.

I was at UNSW for two years before going to America and then for one year (1959–1960) after I got back. I had to come back to Australia under the return requirements of the Fulbright scholarship. And in the meantime I married Joan, who was a student at Stanford, so she came here with me. When I got back I had a single project in mind, which was to finish the dissertation for the doctorate from Stanford. I then moved to the University of Sydney (1960–1962) because Rutherford wanted me to go there and it was easier than establishing a new economics program at UNSW.

And then, in 1962, we lost you to America permanently. In hindsight, would there have been an offer that would have kept you in Australia? After all, you had several *Econometrica* articles forthcoming.

No. At that time, it was both the pull of my wife and also the pull of the opportunities—Ken Arrow, my dissertation supervisor at Stanford, had arranged two nice job possibilities for me, one at the University of Minnesota and the

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other at the University of Wisconsin. Also I was basically the only one teaching econometrics as such in Australia at the time. I remember that K.O. Campbell was the big professor in agricultural economics and he had these two bright students sitting in the front of my econometrics class at the University of Sydney, one of whom was Alan Powell. This would be in the year 1962. There was considerably more excitement in the American scene. There were also all these émigrés who came to America that livened up the place. They were young people then and made quite a splash in the profession. Compared to that, those four young people that David Rowan hired at UNSW were the only crop that he could get. There was not very much going on there at that time. There is no question that the action was in America. Now it does not really matter that much. The only disadvantage of you being here instead of being anywhere else is that it takes a little longer to get to America or to Europe. So today it does not really make such a difference. But at that time, it did make a difference where the action was.

A GRADUATE STUDENT AT STANFORD

At the time, Australian professors were steering all their best students to Cambridge or Oxford. Very few of them went to America. So why did you buck the trend and head to the United States?

In those days, the big names in Australian economics, Arndt, Butlin, Cameron, and Corden, formed a clique that held economics in their hands, and they all came from Oxford or Cambridge, and so they made sure they were steering their students there. Yet it was fairly clear that Oxford and Cambridge were not at the forefront of econometrics. In fact, they were lagging behind. The main people advancing econometrics in the UK were Richard Stone at Cambridge and Denis Sargan at LSE. But econometrics was dominated by the Dutch basically—Tinbergen and Theil. The Americans, when they see something new, they go full force, full steam into it, and this is what was exciting about going to America.

I don't think I would have been happy at Oxford or Cambridge because of the system. What I really liked was the fact that after I did the First Class Honors I went to graduate school in the United States and basically started two years of coursework where the understanding was that you would be wrestling with the latest developments in theory and applications. During those two years we were discussing the latest articles, the crest of the wave for the advance of the subject, and that appealed to me very much because I knew I was very far from being a very well educated economist.

In our undergraduate education, even at Honors level, we did not have a very good conception of economics at the frontier. We were (hopefully) learning basic micro, basic macro as undergraduates, but when I came to America, we had basically advanced micro and macro, mathematics and econometrics the first year, and you only specialized later. At the undergraduate level we did not

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know the latest journal articles. We had textbooks and things like that, but it was not a graduate type of education where you already take for granted that everybody has done basic micro, basic macro and you take it from there.

So I spent two years at Stanford on a Fulbright scholarship. The Fulbright scholarship at that time was very hard to get because there was only one for each state of Australia in all disciplines. I got it mainly because of the fact that I was interested in econometrics, which was not taught in Australia, so it was a good thing to send someone from Australia to the States. This was in 1957, and I was at Stanford until 1959.

Was it a shock to go into that program after you were at Sydney University? Who were some of your fellow students at Stanford?

It was a pleasant shock. We were a bunch of bright students, and there was a very informal relationship between professors and students in America. One of my fellow students was Karl Shell, who is editor of the *Journal of Economic Theory*. In my year, there was also Belton Fleisher, who is a labor economist at Ohio State University, and we still see each other fairly often. There was also Menahem Yaari, who is in Tel Aviv and is very well established in economic theory. There were about ten of us.

What was the academic environment like at Stanford?

My supervisor was Kenneth Arrow. He was very good, but his interest was not directly in econometrics. It was not his area of research, so I did a fair bit of research with Arthur Goldberger, who was also teaching at that time. As Arrow's research assistant I was asked to plot the data on wages and unemployment. Basically, the Phillips curve, which was pretty much in the air at the time and Arrow was fishing for it. That was a very exciting time. The other thing that I enjoyed was that it was a very competitive environment. We had seminars, we had to make presentations in courses, and we were always waiting for somebody to make a mistake to jump on him. It was not quite as vicious as Chicago, but there was competition and it was a game, and everybody played by the same rules. We were all extremely hard working. On Sunday night, after having a date with my future wife, I would bring her back to the dormitory at 10:30 and then go to the library and meet virtually every one of my classmates there, working until the closing time at two o'clock. So it was day after day. We were just working like crazy all the time because there was pressure: you had to pass courses, you had to prepare for presentations and for all the seminars. It was a very hard working time of my life, probably the hardest time. There was only a limited time when you could do things without falling behind in your courses.

Why did you choose a dissertation topic on migration? Was it because of your own experiences?

Yes, I was interested in migration. I really did not know much about it. There were two questions: the effect of migration on Australia, that was probably the

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major point of my interest, but there was also the other aspect of it, how the environment worked on the migrants, the adjustment process that was taking place. The main chapter was the macroeconomic impact, and it was basically an adaptation of J.W. Nevile's 1962 model, which was the first econometric model of the Australian economy. It was adapted to allow for the effects of immigration. I found out that there were only two macro areas that immigration had an effect on: demand for imports and consumption. I found that the import coefficient was very high. Every immigrant was supposed to increase the amount of imports by 1,900 pounds, which was a lot of money. Arrow didn't believe it. Then I figured out that actually what happened was that in the imports were also counted the possessions of the migrants. So that jumped it up, that satisfied Arrow.

THE EMERGENCE OF ECONOMETRICS

How did your dissertation model with its 15 structural equations and identities mesh with other models at the time?

The first American model was Klein's model at the University of Michigan. That started in 1952. That model was about 50 equations and a number of identities. That model still exists. By now it is about 100 equations and a number of identities, and in principle it is still a simultaneous equations model. That was the first one, and then there were follow-ups. But Klein was the father of American macroeconometrics, and he got a Nobel Prize for it.

What was econometrics like then?

What we mean now by econometrics is basically the study of regression models and other complicated models for estimation and forecasting purposes. In those days, the Tintner textbook in econometrics was basically a "hunting trip." It was searching for statistical topics that might be relevant to the study of economics such as discriminant analysis, as used in anthropology. His own hobbyhorse was the variate difference method, which did not catch on, but he thought it might. Tintner was continually looking for something that might be applicable to economics. He knew what the economic modelers were doing and wanted to take it a step further, so there was the matter of how to use statistics to estimate and possibly test economic models. This was before the simultaneous equations time; there was nothing about that in the book. There was ordinary least squares estimation of course.

After that there were textbooks in econometrics that were basically—almost entirely—oriented toward simultaneous equations, modeling and estimation. This was really not fulfilling because you had to start from scratch, you start from the simplest possible model, and you could not jump immediately into more complicated models. So at that time, econometrics was in a very premature state.

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The first real econometrics textbook of the kind that we know would be Jack Johnston's book, *Econometric Methods*, that came out in 1963. Johnston had taught the subject matter of the book at the University of Manchester in the late 1950s, and then he went as a visiting professor to the University of Wisconsin and gave that course there. I learned my econometrics from Arthur Goldberger, who had learned it in Holland from Theil. I think Goldberger's course was probably the first proper course in econometrics ever taught, even before Johnston. It was basic econometrics. This was the basis for Goldberger's textbook *Econometric Theory*, which came out in 1964. At that time Johnston was at Wisconsin, teaching a very similar course. In fact, when I came back from the States after absorbing Goldberger's teachings, Rutherford gave me a page proof copy of Johnston's book, and I said, "This is Goldberger's course; this is what I had at Stanford." I don't know if Goldberger had some kind of contact with Johnston or not, but it certainly was pretty much the same story: they both figured out that you start with a regression model, follow with then dropping assumptions, and make generalizations. It was really a very exciting time. These were the real beginnings of econometrics. Goldberger got his degree at the University of Michigan, where Lawrence Klein was at that time. They worked together there, and then they both went to Holland.

What about all this work by Frisch and Tinbergen over in Europe?

That's a very interesting story. The initial work was Tinbergen's 1939 *Statistical Testing of Business-Cycle Theories*, in two volumes for the League of Nations. There was his Dutch model (1936) before that—but volume 2 contained his American model. But the Klein model was more educational rather than forecasting because it finished in 1939. Then we had the structural break of war, so Klein's model was going into the postwar period, allowing for the structure of breaks in the meantime.

Klein also had his 1950 book *Economic Fluctuations in the United States, 1921–1941*, in which he has three models. Model I, with about 8 or 9 equations, is the one people have since used repeatedly in order to illustrate new techniques of simultaneous equations modeling. Model II is purely a reduced form model, so it is a single equations basic type of model, which the Federal Bank of St. Louis uses as the foundation of its more elaborate model today. Model III was very nice. About 20 equations, very closely tied to economic theory, and I really thought it was a lovely model, and I was very surprised to find out that Klein basically let it go and thereafter produced, with Goldberger, another model (Klein and Goldberger, 1955), which was quite different from the one that he had in his *Economic Fluctuations* book. The Klein/Goldberger model frankly is not at all as elegant as Klein's model III. There is a separation between the real sector and the monetary sector (see Cornwall, 1959), not at all a nice model. So I was very surprised that Klein would do that while he had this very nice model.

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The reason for this I discovered by chance by having a student of mine writing a dissertation on Klein's model III. I was interested in finding out the properties of that model, putting it through a dynamic simulation, and then it dawned on me why Klein did not like it—it was an explosive model. It was not a stable model. In those days, we did not deal with unit roots; nobody believed that anything was explosive. I suspect that Klein figured out, as my student did, that the model was explosive. These are fascinating stories. I figured it out just by chance, because this student of mine, who is now at the University of Cincinnati, did this for his dissertation and it was an eye-opening thing for me because by this time we knew more about dynamics than at the time when Klein wrote that book.

What are your memories of the early days when you started off as a member of the Econometric Society and later as associate editor of *JASA* and the *Review of Economics and Statistics*? The numbers of econometricians was obviously much smaller then. Was there a feeling of camaraderie among these giants of econometrics whom you moved among?

Yes, definitely there was in the early days when econometrics was basically in its birth. It developed with the Cobb–Douglas production function and the macroeconometric modeling of Tinbergen. The camaraderie was clearly displayed in the enthusiastic and confident way in which econometricians behaved in those days: we were the saviors, we were really going to advance economics through exposing economic theories to empirical observations by the use of rigorous statistical methods.

Who were the ringleaders? What role did the Cowles Commission play? It itself had a lot of opponents and eventually moved out of the University of Chicago and ended up in Yale in 1955.

The founders of modern econometrics were really the Cowles Commission people. Among them you find names like Haavelmo and Koopmans (Nobel Prize winner in 1975) and some well-known statisticians' names like Girshick and Wald. Over time the Cowles Commission changed its orientation. The original orientation was definitely concentrated on econometrics. The original intention was to forecast stock market prices, and in fact you can find an article by Cowles where he writes about stock markets and stock market prices. The econometric influence was entirely in the simultaneous equations area. By this time in the early 1950s there was an emergence of popularizers like Klein, Theil, Stone, and Sargan who were taking over the discipline. There was no longer a need for this collaboration of statisticians, mathematicians, and economists because the popularization of econometrics made it accessible to economists. Mathematics and statistics for economists were being taught in courses, so the original function of the Cowles Commission was taken over by econometricians at large.

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By the time the Cowles Commission moved to Chicago the main focus of research was away from econometrics and concerned with things such as information theory that Marschak was pushing. While it was in Yale, some econometric content returned to it because of Peter Phillips's influence on the profession. He and other people were particularly intent on bringing unit roots to the attention of econometricians.

I do not think that Friedman would have been an opponent of the simultaneous equations approach used by Cowles at Chicago. His main thesis was a monetary thesis, that money made the difference in economics, but you can incorporate a monetary thesis into econometric models without great trouble, so I don't think that was his opposition. He definitely was involved in econometrics when it came to the permanent income hypothesis, so it's not that he would be opposed to econometrics. In fact, the econometric aspect of his involvement was the disparity between cross-section results and time-series results in estimating the marginal propensity to consume. So Friedman came up with his hypothesis about permanent income being the primary causal influence on consumption.

My guess is that Friedman might not necessarily have been influential in the move of the Cowles Commission from Chicago to Yale. It is more likely that the Cowles Commission started being less interested in econometrics. It probably also had to do with the financing because the Cowles Commission was drawing a lot of grant money. When this interest was taken over by universities involved in the original orientation of the Cowles Commission, some of these funding sources were no longer available.

Was it difficult for econometricians to get into the major economics journals in the early 1960s? Was there a bias against mathematical or quantitative research?

I would say the opposite. Econometric articles were more valued, especially applied econometric articles. Theoretical economics was harder to get in because it was getting more and more complicated, and there were many more people who had the basic education to advance econometrics. When I came, I had my undergraduate degree in statistics and economics, and I had studied statistics for two years in Prague, so I was a big exception in the economics profession. Economics was mainly verbal, and economists did not have much mathematics, so at that time for me it was not all that difficult to make a little bit of an impact on the profession. But as people acquired more and more mathematics and statistics, this resource was much less scarce, so in that sense it was more difficult to get publications in. But other than that I think the scientific revolution, starting with Samuelson's *Foundations*, had caught on and we still are in it. It is just a question of how we go about it and what type of science we choose—but we still have the scientific revolution.

There are people of the old days who disliked the fact that Samuelson or anybody else was putting economics in mathematical terms because they were

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saying, "This is not what I meant." But then they were not capable of expressing what they really meant in rigorous ways. If you could not clearly and rigorously express yourself then nobody quite knew what you meant. The mathematical expression reduced the ambiguities. Now I think there is still room for innovations in terms of nonmathematical contributions, and the new institutional economics is a very fertile example of the fact that there is still room for new ideas. These new ideas are basically not mathematical ideas, but they make inroads into the subject. So there is room for both. Fresh ideas are always good, and having more rigor introduced into the expression of fresh ideas is also valuable.

What bothers me is the gap between the theories and applied work. This gap is between econometric theorists and econometric applications and between economic theorists and economic applications. There appears to be an isolation of economic theorists from applied work. As a result, the stochastic specification of models is very casual, especially when it comes to the disturbance term. Yet a careful consideration of the nature of the stochastic disturbance is crucial. There are few instances of well-specified observational tests of a theory. The fact is that once you go from economics to economic applications, you have to strike econometrics; there is no other way of going to applications. And that basically calls for a merger of the two. A separate department of econometrics is not very healthy for the profession. It must be meshed with economics.

Did you have anything to do with the Brookings–SSRC Model Project (1961–1967)? It seems everybody in econometrics or economic theory had some part of that model: Klein, Duesenberry, Kuh, Fromm, Orcutt, Theil, Almon, Eckstein, Eisner, Fox, Fisher, Evans, Dhrymes . . .

The rise of large-scale macroeconometric models like the Brookings model, the Wharton model, the Data Resources Inc. model, and so on, dominated the 1960s. My part was actually negative because at that time I was associated with Karl Brunner, who was a big critic of these huge macroeconometric models, mainly because he thought that they were getting away from economic theory. These were criticisms of a methodological kind. Some of the criticism came from Robert Basmann, who thought macroeconometric models were unscientific because they were using ranked variables, and of course you can do anything with ranked variables, so there was some truth to that. There was also the question of pretest bias involved too: you keep on playing with the equations until you get what you wanted to hear in the first place. So there were all sorts of criticisms raised against this industry of macroeconometrics. Brunner got support from Rochester, because the dean there was also concerned about this, and began organizing conferences that were basically meant to be critiques of the Brookings model. I co-edited two books in 1980 and 1981 with James Ramsey dealing with these issues. But practically everybody was involved with this research—there were big grants; there was a lot of money involved.

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You've been involved with journals for a long time. When you look at journals like the *Review of Economics and Statistics* or *Econometrica*, which are some of the highest ranked journals in the profession, what are the main changes that you've seen over the time that you were involved with these journals?

With respect to the *Review of Economics and Statistics* a lot probably depended upon the editor. The *Review of Economics and Statistics* was for a long time edited by Houthakker, who was at Harvard, and he did an extremely good job. He took personal interest in the contributors, and if you sent him a little note about something he would come back with "Yes, this is interesting; can you make a theorem out of it?" or something like that. What has changed a lot in *Econometrica* is that it has become very technical. In the early days, you took *Econometrica* and basically read it from cover to cover, with minor exceptions of some specialized things. It is impossible to do that now because the articles are so difficult to read, so "congested," you might say. Some of them are very difficult mathematically. I made my personal rule that when I see an article in which the author uses max. and min. then I read it. If he or she uses sup. and inf. then I don't. There is a lot of mathematical pretension going on in the profession, and I am not really happy with that.

I guess a cynic might say that elder statesmen, feeling liberated from conventional academic constraints on speaking their minds, often lambaste the very profession that they were instrumental in establishing and nurturing for going in new directions. You have various examples of people like Leontief giving presidential addresses criticizing the profession. But this is in their older age. You probably in your youth were just as critical of those "old-timers" objecting to the way the profession was heading.

There is constructive criticism that aims to improve the situation. There is also destructive criticism. For example, distributed lags are dead now. Time-series researchers have suppressed them completely because they have much more sophisticated models dealing with this sort of thing. This is a matter of fashion. Should we criticize this fact? I happen to have liked distributed lags, but someone else might have criticized that, and they would have been proven right by the market test. However, academic markets do not always work optimally.

Look at rational expectations modeling. People of my generation would say that this has been overdone. There is something good in this, but my criticism would be that it has been carried too far. By now, the basics of it are taken for granted, and it is not mentioned much. There was a promise that simultaneous equations models would be adapted to rational expectations modeling. Nothing of that materialized. There were attempts to develop new numerical methods of building rational expectations into simultaneous equations models. Nobody ever hears about it now.

Obviously parents criticize their children and children criticize *their* parents, so there are generational problems wherever you go. And in some respects,

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parents are right, and in some respects they are not necessarily right, and I think that's true in our profession also.

If you were to look at reforming the profession, particularly in econometrics, you would still teach things like Granger causality and vector autoregressions (VARs), but you would stress the qualifications; is that the approach that you would suggest?

Yes, absolutely. Granger causality; the name says it all. It is not a test of causality, or you would not be calling it "Granger causality." There are limitations to it, of course, there are obviously things to be said about it. I have nothing against using it, but it should not be used in a blind way, a mechanistic way. One should pay attention to the fact that it basically only tests things that happen in the future that could not have been predicted by happenings in the past. It's fine, but it's not causality, and nobody, no philosopher, could call it a true causality test. Granger would not call it that.

The problem with VARs is their lack of structure, of course. You can have VARs, and provided you talk about them as reduced form equations where some of the knowledge has been ignored and some of the knowledge need not have been ignored and could have been drawn on, then it's perfectly OK as far as it is concerned. So there is some value in doing that.

You asked me what I would like to see changed. Undoubtedly, the development which should have been prevented was the takeover of macroeconometrics by the atheoretical time-series people. Both economists and econometricians should have got together and started worrying about dynamic relationships. The movement from one equilibrium to another needed clarification. There were some tentative articles in the 1970s where you had adjustment cost models or partial adjustment models that were fairly primitive because the parameters just sit there and remain constant. The parameters needed some economic interpretation, and in fact the parameter itself is a function of the cost of adjustment. Nobody has quite developed that far enough so that you would model and test the theories about how you move from one equilibrium onto another. I was hoping at one point that we were making progress—there were some articles by Roger Craine and a few others that I liked—but it sort of died; nobody hears about it any more. Maybe it is too difficult.

Today other things are easier to do for people with different tolerances. But if that had been worked out, you would not have this mechanistic time-series modeling taking over macroeconometrics. We would have proper modeling based on theoretical considerations. So that is I think the major failing, and I am sorry that I myself have not been talented enough to make contributions in that regard. But I was hoping that there would be talented people who would make that contribution. I am very unhappy about this time-series and cointegration crowd overtaking macroeconometrics. It upsets me because it is a waste of economic knowledge that we have not incorporated in this. So that would be my biggest regret in terms of the development of the discipline.

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Other than that, of course, the excitement of the early days you cannot really beat. All of a sudden you could take economic relations and put them in mathematical form and then go and test them. It was great that you could do those things, that you could refine the ways of doing the testing, that you could drop some of the assumptions that are involved in the testing, and so on, and at the end come up with a better way of doing it.

Paul Samuelson had that excitement. He had the excitement of putting it in mathematics, but he did not take the next step.

Yes, he did have that excitement, and he did not take the next step, exactly. W.C. Mitchell made that statement in 1950. He was saying that the theorists did get this mathematical reformulation, but we did not get the next step of tidying it up and marrying it with statistics.

In Australia, you had separate departments of econometrics, separate from the departments of economics; that's not the American way is it?

No, not at all. In fact, this is the European way of separate fiefdoms. You do not only have econometrics and economics departments, you have separate departments of economic theory, labor economics, and so on. I am very much fighting against that. I am very much fighting against the separation of economics and econometrics. I really believe that an economist should be an econometrician and an econometrician should be an economist.

AN ECONOMETRICIAN IN AMERICA

Let us go back to your early career moves. You returned to Stanford in 1962 as an acting assistant professor, and then Arrow arranged for you to get job offers at Minnesota and Wisconsin. You took a position at Wisconsin from 1963 to 1965. Can you tell us about your experiences there?

Wisconsin was attractive to me because Goldberger was there as a professor of econometrics and I had been his student at Stanford before. I preferred a one-year appointment, that gave me a full year for research, to a stable six-year appointment at Minnesota as a tenure-track assistant professor. I was working with Guy Orcutt at Wisconsin on the microsimulation model that he was pushing at that time. He brought in a lot of good people, and a lot of publications came out of it in good journals. Orcutt kept on getting money for his project but never managed to complete it. We got the consumption sector, we got part of the investment sector aggregated, but the public sector never got off the ground. But Zellner's papers on seemingly unrelated regressions (Zellner, 1962; Zellner and Huang, 1962; Zellner, 1963) and some other important papers got written. Orcutt got very disappointed, as the project did not come to the fulfillment of his aims and wishes, so then he moved to the Urban Institute in Washington for a while before finishing at Yale. There was criticism from macro-

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economists who did not like this microsimulation that Orcutt was pushing, but it was a very fertile environment for doing research.

When I was at Wisconsin the academic market was just so rich; there was a great shortage of econometricians at that time. I had calls from all over the place to go to other universities. At the end I finished up at Michigan State, partly because they gave me a huge salary increase from what I had before, but mainly because they wanted me to build up econometrics there. I was able to get all sorts of money, numerous possibilities for running workshops, inviting visitors, so I did that. I was pretty happy at Michigan State while I was doing that.

You were at Michigan State from 1965 to 1973. Was it a supportive environment for econometrics? How did the older professors react to what you were doing?

I was the econometrician, and there were a couple of young assistant professors from Chicago, Tom Saving and Boris Pesek. These people were prolific in publishing and heavily into research. But there was also an entrenched old-timer group, and there were some frictions, but the friction was not mainly between us and the old-timers but between the chairman and the old-timers. It was the chairman who brought us in and was pushing our side. And the old-timers did not like it. Eventually, the chairman was disgusted and he left, and most of the rest of us left too. The department at that time pretty much disintegrated. But now it has been built up again, and it is very good and very strong in econometrics again. In fact, it was my student from there, Peter Schmidt, who is running it now. They have Dick Baillie from England, who is also there; Jeff Wooldridge, who has just had a couple of books out. They are excellent, much better in econometrics there than in Michigan now.

Tell us about the birth of the *Elements of Econometrics* (1971).

This was really my course in econometrics at Wisconsin. I was very much absorbed by Goldberger's ethos of simplification. The book proved popular because I had very simple explanations for maximum likelihood estimations and things like that. Students liked it a lot, and then the book people who went out and hunted for potential texts approached me. Macmillan book distributors asked me whether I would be interested in writing a text. So I said yes and wrote it and finished it while I was at Michigan State.

How long did it take from once you started writing the book? You were very active writing articles as well in all sorts of theoretical and applied areas in econometrics. So was writing the text worth it?

The book came out in 1971, and I started it in 1965–66—so it took 5 years—and was intended for undergraduates and first-year graduates. It is now in second edition and on its ending streak, but on the whole it sold over 60,000 copies, so that's quite a number of readers. I got a lot of citations from it because it was an easy reference for people who were doing research. It was certainly

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worth it. I got so much mileage out of it, and it has been translated into other languages. In January 2004 I was at a conference in New Delhi and had a delegation of students coming to see me because their professors had used the book. The year before, I was at a conference in Rio de Janeiro, and since the book happened to be translated into Portuguese, I had people coming to see me there.

The second edition came out in 1986 and incorporated all the new things at that time. But this was before the time-series revolution, Peter Phillips's unit roots, and all that sort of thing.

What has been your reaction to these recent developments?

I am somewhat critical of them. I have just finished writing a little article about the 2003 Nobel Prize in economics that questions this research effort. Basically, what I criticize there is the movement of econometrics away from economics because the people involved with the atheoretical time-series movement tend to be much more mechanistic in their modeling, much less grounded in economics, compared to those who do behavioral simultaneous equations modeling. (The VAR modelers are atheoretical but still use simultaneous equations.) So I criticize this quite a bit because it is an irony, seeing that the first Nobel Prize in econometrics went to Tinbergen and Frisch, who were the founding fathers of *Econometrica* and the Econometric Society. The first issue of *Econometrica* came out in 1932, and the aim of the Econometric Society was clearly stated to be a "Society for the Advancement of Economics in Relation to Mathematics and Statistics." It is very clear that econometrics was to be the servant of economics. Improving economics is what we should be aiming at. We should be concerned about advancing economics, and econometrics was a tool toward that. The time-series revolution is a movement away from that trend because it does not necessarily advance economics. It provides a way of seeing if series are moving or not moving together, but it does not pay attention to *why* they should be moving together. It talks about fluctuations and trends. And the trend behavior is shown as a function only of time! Economists have all that education in economic analysis, and all they can say about the trend in gross domestic product (GDP) is that it is a function of time? That is very sad. In fact it is incredible that anybody can take it seriously. So I am very strongly against it. The good side of this movement is that it draws attention to the dynamics, and that is of course what economics was neglecting before, but in terms of advancing economic knowledge, I don't think there is anything of that in there.

Similarly with Sims at Minnesota with his VAR models. He is a great attacker of behavioral simultaneous equations modeling. In fact in 1980 in "Macroeconomics and Reality" he said that macroeconometric models have not only been criticized, they have been discredited; a strong statement! Lucas has been a little more guarded because he had that theory of adjusting parameters, Sims did not have that. Sims was basing his criticism on the forecasting performances of macroeconometric models and the fact that he did not like some of

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the assumptions that were being made by macroeconometric modelers. So he came up with this VAR, and it survived. But the people who are really into economics, who take economics seriously, are basically using the VAR in the sense of reduced form equations, but they worry about not letting the coefficients be entirely free, as they are in the original VAR models. They impose some structure on those coefficients. A structure based on the structural equations on the assumptions that they contain. So there are some sensible modifications being undertaken. When I criticize VARs, some people that I know at Michigan, who are working on this, immediately jump up and say that they are trying to put some structure on the coefficients, that it is not just listing variables and then running their lags. So VAR modeling survives.

The other thing that survives from simultaneous equations is the instrumental variable method of estimation. Of course it is often a misnomer to claim to be using instrumental variable estimation because the instrumental variables should properly come from other equations of your model, though often they don't.

Two-stage least squares is nothing but an instrumental variable estimator using, as instruments, a certain linear combination of the exogenous variables in the model. But how do you find an instrument, in general? Basically it seems to be done in an ad hoc way. That's *not* how it should be done. There should be a theoretical basis. With simultaneous equations, that basis came from having equations, and you looked for the exogenous variable in the equations other than the one you were seeking to estimate. There is also the issue of hanging onto a particular method which by itself really does not have much justification.

So you left Michigan State for the University of Michigan in 1973 and stayed until your retirement in 1993. What was there at Michigan that attracted you?

It was a move that I was not sorry for. With the mass exodus from Michigan State of the younger people, and the so-called old school taking over, it was no longer such an exciting place to be. The University of Michigan was higher in the rankings than Michigan State, so it was a move up. I also had some friends there—Shapiro and Hymans—who were already working on the Klein economic models. It was a more cosmopolitan university—we had speakers coming from all over the place. At Michigan State we had visiting speakers, but you had to work to get them to come. At the University of Michigan, people would come because they wanted to be at Michigan. At Michigan State, you had to do something to attract them.

The department at Michigan had very high standards. The first year I got there, which was in 1974, a decision was made about the future of seven assistant professors. It was taken very seriously. What was being discussed was not the people themselves but the papers they had written. So we were discussing, "Is this paper really that good?" and somebody would say, "It will probably be published in the *American Economic Review*," and somebody would say, "Yes,

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but in paragraph 3 on page 17 it says . . .” and things like that, and so at the end five of these people were fired. Some of them were pretty good, but it was the idea that we only keep the best. It was not like that at Michigan State. It was not as heartless, and there was more sympathy for the people concerned. But at the end there was no problem for those assistant professors who were let go from the University of Michigan to find jobs elsewhere.

Who were the key names, the key people at Michigan?

The biggest name was Gardner Ackley, the macroeconomist and former president of the American Economic Association. He was definitely the biggest name there. But there was a very strong international trade group (Bob Stern, Alan Deardorff) and a strong labor group (George Johnson and John Bound). The econometrics group was also strong at that time. Public finance also. In fact the whole department in every field was fairly well represented by people who published in top journals.

A EUROPEAN AT HEART

We’ve talked mainly about the American scene, but you obviously have very strong links with Europe. Would you regard yourself as a “European” at heart? What does that mean for you?

Yes, of course. During the communist era, until the end of 1989, only the West was open to me in Europe. I got an Alexander von Humboldt Senior Scientist Award to spend a year at the University of Bonn in 1979–1980, mainly doing research in econometrics. I also spent several summers at the University of Saarland in Saarbruecken and a sabbatical year in the Netherlands Institute for Advanced Studies. So I had a close connection with Europe all the time. And when the “velvet revolution” came to my native Czechoslovakia, I immediately went to Prague. My mother died only a short time after that.

A friend of mine from a later generation, a young Czech who was professor at Pittsburgh, immediately managed to collect some money, mainly from the World Bank but also from other sources, to start an American-type graduate program in Prague. This is called the Center for Economic Research and Graduate Education (CERGE), which was started in 1991. The program was at first run by visiting professors. I went there in 1992–1993, and I spent the whole year teaching econometrics there. Since that time, I have been going there twice a year for about a month each time to do some teaching but mainly to work with students on their dissertations. I am on the Executive Committee there also, so I have some influence over the development of the center that has been very successful.

The purpose of this American program is to attract students from the former Soviet bloc countries and educate the future economic elite in this region. By now there are about 40 Ph.D.s who have come out of that program, and they

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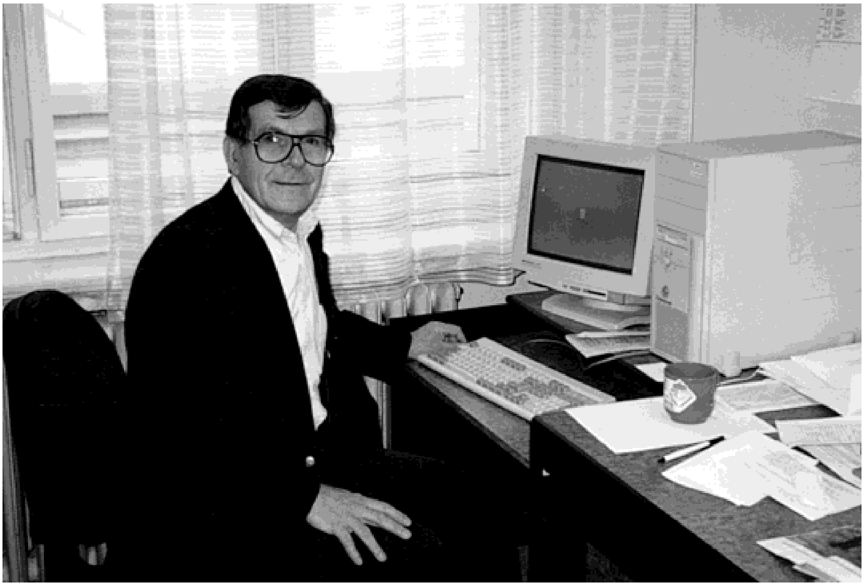

At the Center for Economic Research and Graduate Education (CERGE) in Prague.

are in influential government positions or are professors in an academic environment. It is still funded largely from the American foundations, but about 40% of the budget now comes from local sources.

In your contacts with European econometricians, is there a different style to econometrics in Europe than there is in America? Is there a different approach?

There was a long period when there was really no difference. I went practically every year to a European Econometric Society meeting, and the graduates there who were presenting papers were just like the American graduate students. They read the same books; there was really no difference. The only difference that may have come up was David Hendry's "revolution." Hendry has his own ideas about what is wrong with econometrics and how to fix it. It caught on very strongly in England, but it has not caught on at all in America. It is not that Hendry's ideas were necessarily that revolutionary. Chris Gilbert from Oxford keeps on calling it a revolution and contrasting the *American Economics Review* type of econometrics methods with Hendry's approach. I think the distinction is overdrawn. Hendry has a laundry list of conditions that a model has to satisfy before one can be happy with it. There is nothing particularly wrong with that. The only thing that was new was the encompassing part, and I don't consider this as so important, but there is something new there in going from the general to the specific. There is a certain appeal, to me anyway, as against going

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from the specific to the general because you have all this pretest bias involved. You are often just testing the level of significance if you do the general-to-specific properly, while if you are testing up, there is no way of doing that. So it seems a more scientific approach, but there is of course the disadvantage that you have to start this with a very general model. I don't really have a good explanation as to why Hendry's approach has not caught on in America. But the fact of the matter is that it has not.

You seem to have a very recent interest in international development, economies in transition, and new institutional economics. Do you want to tell us a little bit about why you have broadened into those areas?

I happened to be a visiting professor at the University of Saarland at the time when this was the focal point of new institutional economics. The main professor there, Rudolph Richter, editor of the *Journal of Institutional and Theoretical Economics*, was organizing a conference every year, so I was invited each time, basically as a guardian of purity so to speak. I was there to judge the value of the new institutional approach and second to see if there were any empirical implications of any of these papers that were being presented. I was supposed to comment or suggest ways to deal with the empirical side. At first the ratio of words to substance was really high, but the papers became better and better and more and more rigorous in time.

The reason why I got interested in it is that institutional economics deals with markets, with the factors that determine the rules of the market game. I think we take it for granted in economics that you have well-defined supply and demand schedules and supplies will come and demand will come and exchange will take place. We know this happens in auction markets. In other situations there is not much about it that is known, and new institutionalists are very concerned about it. One of the major innovators in this area is Oliver Williamson, and he will probably get a Nobel Prize for his work. Coase is the original one who introduced some of the implications of transactions costs and property rights and things like that. Williamson pushed this a little further into corporate governance.

Rudolph Richter has written a long essay about the development of institutional economics, and there are several kinds of institutional economics that put an emphasis on different market aspects. I sincerely believe that it is important because I have seen in the Czech Republic how important the institutions were. Other things that institutional economics pays attention to are the implied social contract. There are certain assumptions commonly made that people are essentially honest and that they will not run away without paying. This was not happening in transition economies. So that definitely was close to my heart and close to my interests. It does not have much to do with econometrics because the empirical content of institutional economics is very weak; there is very little of it. It was brought to my attention through observing the situation in the Czech Republic.

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Receiving the first Karel Englis Honorary Medal from the Academy of Sciences of the Czech Republic in 1998 (with Rudolf Zahradnik, president of the Academy, and Jan Svejnar, director of CERGE).

It has helped me to learn more about economies. It was a neglected aspect of economics that I never had, either as an undergraduate or a graduate student, learned or even become acquainted with. By now, there are courses in institutional economics that are being taught and even two societies of institutional economics. I just think we have been negligent in not paying attention to it.

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Finally, Jan, how would you like to see econometrics develop in the future?

My main "mission" right now is to persuade the profession of the necessity of combining economics with econometrics. My favorite philosopher of science, Karl Popper, claims—convincingly in my opinion—that every theory should be accompanied with a description of observational circumstances that would refute it. This necessarily means combining theories with observations. I would like to see every economic theory allow for the role of chance, which is part of real-life situations. The importance of building stochastic elements into any theory related to human behavior is the essence of that! The bottom line is that every economic theorist should at the same time be an econometrician and vice versa.

NOTE

1. The interviewer is at the School of Economics, University of New South Wales.

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