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TRANSCRIPT OF INTERVIEW OF

LAWRENCE R. KLEIN

BY

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RICHARD JOLLY: This is Richard Jolly on Friday, January 4th interviewing Professor Lawrence Klein in Atlanta. Professor Klein, thank you very much for this opportunity. Perhaps you might begin by saying something about your early life, how your early family background and other experiences affected your subsequent career, your values, indeed your internationalism and work on international issues, as well as at times in providing support for the UN.

LAWRENCE KLEIN: I was born in the center of the United States, in Omaha, Nebraska. My parents were both born in that general region. My father was born in a small town, Schuyler, Nebraska. My mother was born in Des Moines, Iowa, very close to Omaha. I was raised in American middle-class society and background values in that period, and was mainly influenced by education in the public schools of Omaha. One significant experience was the particular high school. It was not a high school that was especially based on area of residence, but served the whole city. Now it does that on a much broader scale because the city has expanded so much beyond the borders of that school's area. But many very intelligent people who have made significant contributions to American life have come out of that school.

RJ: Which school was this?

LK: Omaha Central High School. As a matter of fact, at the 100th centenary of the Nobel Prizes last month, there was a luncheon given by the American ambassador to Sweden, and a participant walked over to my table and said, "You know, we are probably the only two people in this room who have the distinction of being on the honor roll of Omaha Central High School." There were other people in that room who may have gone to Omaha Central High School, or who had some relationship nearby, but I was put on this honor roll only last year, when it was just instituted. The other laureate won the prize in chemistry two years ago, on the

conductivity of plastics. It was very interesting that he had a very good education in the same school that I did.

But there was something else to note. That was a very isolationist part of the United States. I think our exposure, in general, to world events and to what was going on in the world at large, particularly the build-up to the Second World War, was not really adequately put before me in that setting. Eventually I went to university in California, and then my horizons expanded a great deal.

RJ: But the 1930s were having an impact on the farming community, as well as on unemployment more generally.

LK: That is quite true. I could see first hand the workings of the Great Depression. That, really, was a lasting childhood impression, to grow up in the Great Depression, but I looked upon that more as an internal thing, as an American phenomenon, and not something that was pervasive over the world. As I later traveled a lot and met people from many world areas, I understood, for instance, why Japan didn't have the same shock from the Great Depression. John Hicks told me that when we were together in Japan. He said it was because Japan double-devalued in the 1930s. I would never have realized the significance of anything like that in my childhood.

The international ties, and of course the protectionism, were things that didn't matter very much to me then. Our family was always Democrat. We wanted Franklin Roosevelt to be elected and reelected, but we really didn't fully appreciate Roosevelt until, say, Pearl Harbor, or perhaps the preparation for the war before Pearl Harbor, with his strong commitment to side with Great Britain.

When I went to university, there were two things that attracted me. One was the technical study of economics, and the use of mathematics and statistics in that subject. In my last two years at the University of California at Berkeley, I really got a very heavy dose of that by some of the pioneers in statistics.

RJ: Who were they?

LK: Jerzy Neyman was a pioneer in statistical inference. I used to attend his lectures and those of people who worked in his laboratory there. There was an important mathematician, Griffith Evans. I didn't study directly with him, but I studied with his students. He was an early mathematical economist and was working with Ragnar Frisch and Irving Fisher and others on setting up the Econometric Society.

RJ: Which year was this?

LK: I went to Berkeley in 1940. During my first two years, I was at City College of Los Angeles followed by the last two years at the University of California at Berkeley. I learned a lot of technical material in the Los Angeles experience, but I got a much broader view at Berkeley from people who were immigrants, in some cases forced immigrants, because of what was happening in Europe. There were Germans, Austrians, Hungarians, Russians—and Neyman from Poland—people who were coming away from what was happening in Europe. Their experiences meant a lot.

Then there was the issue of the United States entering the war. I had had an accident as a child that injured a leg, and I was not accepted for military service. I finished my degree, and I had a scholarship to go to MIT (Massachusetts Institute of Technology) at the time of the second year in which they were providing advanced teaching in economics for the Ph.D. program. I was

the first doctoral recipient in that program. Paul Samuelson was my supervisor. That provided a continuation of the kinds of ideas I had been forming at Berkeley.

I was also influenced a lot by the brother of Simon Kuznets, George Kuznets. I was his research assistant for one summer in Berkeley. He was not a deep economist, but he was a fine statistician. He followed world events very closely himself. At that time I began to get a second line of interest in stabilizing the economy, not only in the United States but throughout the world, concerning the relationship particularly between the United States and Europe at that time.

RJ: Two questions. If I could just go back to Berkeley, was there any first-hand impression of poverty at the time, or of injustice? Those were the times when many Japanese were locked up in California for fear they might be linked to the other side in the war.

LK: Well there are two things that struck me about that. In California, there were many Japanese students, and some were Japanese-Americans—Issei-Nissei, later generations. Some were actually traveling students who had temporarily come from Japan to the United States. I remember I was friendly with some of them at various times, particularly at Berkeley. I wasn't aware of their being sent to camps because the bombing of Pearl Harbor came just six months before I finished at Berkeley, and they probably had their difficulties after that. But they were looked upon very suspiciously after Pearl Harbor. I remember staying friendly with them, but I didn't have a deep interest.

Later in life, I had very close relations with Japanese, and continue to work with them a lot today. But at that time, we looked upon Japan as very much "copycats." The idea was something like this: in the technical studies, before the days of the computer, we had slide rules. We needed slide rules for a physics class, or even for some of the mathematics classes. The Japanese slide rule cost a dollar, but it was very imperfectly graduated or calibrated. The

German slide rule was very accurate and cost ten or twelve dollars. I always kept that distinction in my mind that the Japanese copied, and copied imperfectly, while the Germans were very precise, and everything held together, and lasted longer. I carried that impression of Japanese scholars for some time, but I have changed my impression a lot. Now I tend to emphasize some of the contributions that the Japanese have made to technology, but at that time, we said that it was “copycat” and of inferior quality.

RJ: Let me then come to Paul Samuelson. If I remember rightly, Paul Samuelson at that time was developing his *Foundations of Economic Analysis*.

LK: That is what attracted me to MIT. I came across, not his book, because it hadn't been published yet, but articles in *Econometrica* in the Berkeley library. And I said, “That is the place I want to go to study.”

RJ: But you went into the more applied econometric side, rather than the refinement of theory side. Is that true?

LK: Yes, that's true. And I think it was the influence of Neyman, or, not necessarily Neyman himself, but the whole group around Neyman. There was one other man who was a student or an assistant of Griffith Evans from the mathematics department. His name was Francis Dresch, and he gave lectures in mathematical economics and econometrics at Berkeley. I saw Samuelson about two weeks ago, again at the Nobel ceremonies in Sweden. He said, “Have you heard of Francis Dresch lately?” I hadn't thought about Francis Dresch for years and years, but Samuelson was impressed by his work.

Samuelson was very interesting because of his sharpness, and the fact that he was a *wunderkind*, but, especially because he had a very progressive social view. I can remember, among the graduate students in the program at MIT, we always discussed election results to see

how the Republicans and Democrats were doing, particularly when Roosevelt was running for reelection. Some Republicans were coming forward strongly and had support from a statistician at Harvard, William Leonard Crum, who was advisor to Republican candidates.

I always was impressed by Samuelson's progressive view, and he was a consultant then for a group in Washington that was working on family budgets and distribution of income. I think it was called the National Resources Planning Board. We were impressed, as young students, seeing Samuelson off to board the night train in Boston, going overnight to Washington to advise the political people on economic policy. That was, of course, a very, very big influence on us, as students.

RJ: The statistical work of various people on estimating armaments production, indeed some of the early input-output work at the time, did that interest you? Or was that more located in Washington or Harvard?

LK: Well, in Cambridge, that was Wassily Leontief's input-output group.

RJ: I was even meaning, not obviously before Leontief, but I have forgotten who—

LK: Mason?

RJ: Yes, I think.

LK: Mason was, I think, more in international economics, and others from Harvard were with the forerunner to the CIA, which was the Office of Strategic Services. I didn't associate much with that group, but I did spend time with Leontief. Later, I have become very much involved in input-output and Leontief techniques in combination with what I would call the "Jan Tinbergen approach" to macroeconometrics.

RJ: Perhaps you should explain when you left MIT to go to the Cowles Commission. Is that when you started working on the Tinbergen models?

LK: Yes. There are two steps in that respect. The first step was that I was assigned to Samuelson as a research assistant, as soon as I got to MIT. And he said to me, “The Keynesian cross,” by which he meant the relations between savings as a function of income and investment as a function of income, is a complete analogue, from a statistical point of view, of the supply and demand cross, the “Marshallian cross.” There had been an article by an American statistician, Mordecai Ezekiel, dealing with what we call the “identification problem” in econometrics. He had done some questionable things in interpreting the “Keynesian cross.” Samuelson asked me, “Will you take this as a research assignment?”

I did. It became an issue of how to identify the saving function and the investment function, in the same way that the early agricultural statisticians had to identify the supply curve and the demand curve. Samuelson had great technical insight.

There is one other thing I want to mention about Samuelson. At MIT, another graduate student and I organized a series of seminars on econometrics in which we brought together people from the MIT statistics group in the mathematics department and people from the economics department. We had a small budget to invite a lot of speakers from outside. Some of them that I invited with this other student would be Americans, like Harold Hotelling. But we invited Abraham Wald. We invited Trygve Haavelmo. We invited Willy Felner and others who represented gifts from Europe to the United States, by courtesy of [Adolph] Hitler.

Somebody discovered a mimeographed write-up of the proceedings of those seminars about five years ago, and a journal that publishes history and development in statistics asked if they could reprint it. I said yes. And Samuelson asked if he could write an introductory story on that. He wrote a story about how much Americans learned from the immigrants at a high scientific level, how American academic life and American scientific achievement got a

tremendous boost by having people like Abraham Wald, and Willy Felner, and Trygve Haavelmo. Samuelson wrote a very touching summary. Another seminar visitor was Richard von Mises. And in commenting on that, Samuelson drew a sharp contrast between Richard von Mises and Ludwig von Mises. It was very insightful and it brought out a very personal element in Samuelson that other people didn't recognize. They thought he was just too smart and too objective and that he didn't think about things like this. But he wrote a very touching summary of the speakers and what their backgrounds had been.

The second step about my going to the Cowles Commission to develop models in the Tinbergen tradition follows from my giving a paper, based on my dissertation, which Samuelson supervised. The name of the dissertation was "The Keynesian Revolution." And when I gave the summary paper at a meeting of the Econometric Society, the first since their postponement during the war, Jacob Marschak approached me and said, "What this country needs," meaning the United States, "is a new Tinbergen model to help in postwar recovery and planning." I said, "That is just the job I really want." I disregarded all other possibilities and went to Chicago to join his team.

He also was very optimistic about the work of Haavelmo, Tjalling Koopmans, and others. Koopmans had been with Tinbergen, and Haavelmo had been with Frisch. Haavelmo said that when he developed his approach to econometrics, he was really trying to give some intellectual support for what Tinbergen was doing empirically.

So I went there and built statistical models. I stayed there approximately three years, and just before I left, we made predictions for the U.S. economy. The standard prediction could be summarized in one sentence as saying, "By the middle of 1946, we can expect to find six million unemployed in the United States." That was the number carried over from the 1930s. I started

out thinking that was a danger. And indeed, Samuelson had written some papers for *The New Republic*, or *The Nation*, saying that there would be a problem getting back to full employment after the war. The Cowles Commission was asked by Theodore Yntema, a former director of the Cowles Commission, who was then the director of the Committee for Economic Development—his chief economist was Albert Hart. They were located in Chicago just across the Midway from the social science building, where the Cowles Commission was located. They came to us and said, “Will you use your model to make a projection for the postwar United States? The Committee for Economic Development wants such a projection, we have our own ideas, but we want some confirmation.”

We said, “We are not quite ready.” But we agreed to be talked into giving it a try. Albert Hart made all of the fiscal and monetary policy assumptions. I supervised the calculations and the equation system. We came to the conclusion that there would be no great setback after demobilization. We tried to take account of the benefits that would be given to returning veterans going into university life and getting degrees, the pent-up demand, and how it might be distributed between goods people couldn’t have during the war, and goods that were just beginning to come on the market. We were also thinking a good deal about the use of the forced savings during the war. People paid taxes, but a great deal of unspent income was in the form of the purchase of Series E bonds. They were risk free, and they paid an interest rate.

RJ: They could be cashed in any time after the war?

LK: They could be cashed on short notice. We had a name for them: liquid assets. And America had a very high value of liquid assets after the war. It was an unusual situation and unlike the Great Depression situation.

RJ: What about the transition disruptions of people moving from troop employment to civilian life?

LK: In 1945, there was a jump, when price controls were relaxed, and rationing was relaxed. There was a jump in prices. There was an initial rise in unemployment, but it didn't last. And our target was always the summer of 1946, to look for better conditions then, rather than during the immediate prewar period. We said, "No, there will be no unusually long unemployment lines."

RJ: Which turned out to be true.

LK: Yes.

RJ: Can I ask, technically, during the calculations at this time, that must have been slide rule and hand operated calculators?

LK: It was very tedious. It took us days to make one calculation that is done in a millisecond now. And there was no preprogramming. It was all by approximation and matrix inversion in solving equations. It was done as an internal project. It was never published. Then sometime in the 1960s, Albert Hart was going through his library. He came across all of our calculations, and he sent it in to *Econometrica* with a note, which read, "Here is an econometric projection that was different, and successful, and that you might want to publish." They wouldn't take it.

RJ: So it has never been published to this day?

LK: No. There is a book that is edited and authored partly by me, Ronald Bodkin of the University of Ottawa, and Kanta Marwah from Carleton University. They were both students of mine. It is called *A History of Macroeconometric Model Building*. Albert Hart had given me the

original papers we had used, and there is a discussion of that calculation in the book with Professors Marwah and Bodkin.

RJ: Let me ask you a bit about Tinbergen. When you were looking at his model, did you get in touch with Tinbergen himself at that time?

LK: Of course during the war we didn't. But right after the war, immediately after V-E day—not V-J Day but V-E day—we had two visitors at the Cowles Commission, one was Frisch making contact again, and one was Tinbergen. We had a third one, Michal Kalecki, but Kalecki had already gone to the ILO (International Labour Organization) in Montreal.

RJ: Where had Tinbergen and Frisch been during the war?

LK: I think—well, I am positive about Frisch. He had been sent to a concentration camp at Grini, where many Norwegians were sent. Odd Nansen has written a book of recollections of Grini, and he talks about Frisch in that. Grini had a lot of politically involved Norwegian people who were intellectuals, and who were against the occupation forces. They were all sent to Grini. It was a camp that treated academics and intellectuals alright. It wasn't like Buchenwald, Dachau, or the worst of the German camps, but it was a place where many were kept.

Tinbergen—I believe, I am not positive, but I could find out—Louis Emmerij would probably know—was a conscientious objector in Holland during the war.¹ And I remember that Tinbergen told me that he wanted to get a copy of a book that Colin Clark wrote called *The Economics of 1960*, I think. It was written during the war period. He got it from a contact in Sweden. It was probably Erik Lundberg. One of the two got that and then sent it to Tinbergen.

¹ Louis Emmerij notes that Jan Tinbergen (born in 1903) worked in the Central Statistical Office during the Great Depression, after finishing his studies in mathematics and physics. During WWII, as a conscientious objector, he was in the Netherlands as a professor of economics in Rotterdam.

That's what he told me when he came to visit the Cowles Commission afterward. He tried to stay in touch with economic affairs during the war.

He came and gave lectures at the University of Chicago. We compared notes at that time on the approach that we were taking, and what he had done at the League of Nations. You see, at the Cowles Commission we then had Haavelmo, who was a very close student of Frisch, and we had Koopmans, who was a very close student of Tinbergen. Jacob Marschak knew both Frisch and Tinbergen from Europe.

RJ: When you look back at Tinbergen's work in the early building of the econometric model, do you feel it was the work of a giant?

LK: Oh yes. When I compiled the *Book of Landmark Papers of the 20th Century*, which has just been published, I had a set of ground rules that I wouldn't include any Nobel economist in the collection because they already had all their publications cited over and over again. But there is one of Frisch and there is one of Tinbergen that I definitely would have included. In the book on the history of econometric models, there is a paper by Anton Barten, and he discusses the Tinbergen model that preceded the League of Nations model. The League of Nations model was built on United States data, and Tinbergen had previously built a model of Holland. That model is discussed in the history book, but it deserves to be recognized as a landmark paper.

Tinbergen told me that, although he had the dispute with [John Maynard] Keynes, or Keynes had the dispute with him, he was a great admirer of Keynes. And what he was always trying to do in his book was to put Keynes into econometric language and format. And he said that he was very sympathetic with that. I think he came through with the right position in the exchange, and Keynes was wrong on that particular occasion.

RJ: Could you just explain what that point of dispute was?

LK: Keynes said, "You can't test my theories with numbers and data." He said there is a problem with colinearity, and there are problems of data, and there are other kinds of difficulties. The future doesn't replicate the past, and things like this. He said Tinbergen was doing the wrong thing.

RJ: Really. So Keynes was very wrong to be blunt.

LK: Yes. He was very nasty about it. And at the same time, Keynes had taken into the wartime offices in London James Meade and Richard Stone. He had not asked them to put together a model, but to put together the national income and product counts. They were the foundation piece for the modeling.

RJ: Is this the moment to ask about your going from Cowles to Europe?

LK: Well, I went from Cowles to Canada for a job to build a model similar to the one that I had built at the Cowles Commission, after the war. That was in the summer of 1947. We put together a more modest model, but one that was used for many years in the Ministry of Commerce and the Treasury in Canada. Then I went for a year to be with Frisch and Haavelmo in Oslo. I had a traveling fellowship from the Social Science Research Council (SSRC). I worked on theoretical problems. Then, when my wife and I were in Oslo, we went for a trip to visit Tinbergen in Holland for a few days. For a few days, I stayed at the Central Planning Bureau and worked there on econometrics, and then went to Cambridge and talked to Richard Stone. He had just started his Department of Applied Economics in Cambridge. I then went back to Oslo, and then came back to the United States again. I spent a year at the National Bureau of Economic Research. That was devoted just to general econometric work.

RJ: Were you in Oxford at the Oxford Institute of Statistics?

LK: Later. I came back from Norway, spent a year at the National Bureau of Economic Research in New York. Then I went to the University of Michigan. There was a man who had been at the Cowles Commission—George Katona, a Hungarian and a specialist in the psychological dimensions of economics, and in particular in survey research. I went to Michigan for a very technical problem. There was great dispute in the economics profession about how solid these liquid assets were as purchasing power for American families. In the language of macroeconomics, it was called the “Pigou effect.” Arthur Pigou had argued against Keynes, saying, “Well, if there is a recessionary situation and prices fall, as they did in the Great Depression, then the purchasing power of liquid assets goes up. And that offsets the income decline.”

The Survey Research Center at the University of Michigan had been doing surveys of consumer finances for the Federal Reserve. And I went there as a researcher to see if we could get a more definitive verdict on the role of liquid assets and spending. That has come up again in the present circumstances, because when the Federal Reserve changes interest rates—

RJ: It tends to push up asset values.

LK: It activates the wealth effect. It’s not called the Pigou effect anymore; it’s called the wealth effect. That is because Americans hold a much broader spectrum of securities—the average citizen does—than in the past. But coming out of World War II, they had bank accounts and little else.

RJ: And the bonds.

LK: Yes, and that was it. Five or six percent of the families were involved in the stock market at that time.

RJ: So, coming, if I may, to the Klein models as such, I am not sure whether you would describe what you had been doing in that time as Klein Model I, or Klein Model II.

LK: Well, Klein Model I was methodological for trying out the Haavelmo ideas on estimation. It was very compact. A large model—I forget whether we called it I, II, or III, but the larger model, which was the one we used for those postwar projections in 1946, was the first to be used extensively in applications. And that is described in a book called *Economic Fluctuations in the United States, 1921-1941*. Then, when I went to Michigan, I said, “I am going to start from ground zero and build a new model, to take account of things I’ve learned, and new flows of data.” We began, for the first time, to get some computer power.

RJ: This is the mid-1950s?

LK: Yes. I went there in 1949. We began to get computer power in 1951 or 1952. They had all this very interesting information from surveys of consumers. That was the point at which Colin Clark wrote in the *Manchester Guardian* that, after 1952, there would be a settlement of the war in Korea, and then the United States would go back into the Great Depression. I remember talking a little bit later to Richard Kahn at Cambridge, and he said, “You know, we shifted the portfolio of Kings College away from the stock market after Colin Clark wrote that paper.”

I wrote, with my student assistant, Arthur Goldberger, a paper that was published in the *Manchester Guardian* saying Colin Clark is wrong. After the Korean War, there would be a recession but it would be only a 10 percent drop in real output. He was talking about a catastrophic drop. Between 1929 and, say, 1931 or 1932, the drop was about 50 percent. We said, “No, it was only a drop of 10 percent.” It would be short, and then there would be a recovery.

RJ: Now did the Marshall Plan come into any of your work at that time? Did anyone try to involve you in the forecast of the impact of the Marshall Plan?

RK: No, because the Marshall Plan was mainly foreign assistance. It did more for Europe than—well, it stabilized the world and that helped the United States. But at that time, we had not gone into international forecasting. But when I went to the Oxford Institute of Statistics, I built a model for the U.K. That was part of the idea of my going there, that we would build a model for the U.K. like the two models, the one at Michigan and the one at the Cowles Commission. In addition, Oxford had the savings surveys which were very much patterned after the Survey Research Center's consumer surveys for the Federal Reserve. We wanted to do the same kind of analysis of British consumers that we did of U.S. consumers.

At the University of Michigan, I was investigated by the House Un-American Activities Committee (HUAC) for having taught some classes in Boston and Chicago in the mid-1940s. The Boston class effort was for a fellow student at Harvard, who had to return to his home country, and left a class stranded for an adult evening session. It was called the Samuel Adams School, and I taught macroeconomics there along the lines of my thesis work on Keynes, supplemented by Samuelson's growing involvement in an undergraduate textbook. The Samuel Adams School was investigated by the HUAC and other agencies for being a communist front organization, but I knew little about the administration of the school and mainly wanted to test the waters of pedagogy, especially with respect to the reception of the Keynesian views on macroeconomic policy implementation.

When I moved from MIT to the University of Chicago, as a fresh Ph.D., I was contacted by a similar school, the Abraham Lincoln School, to teach seminar adult evening classes. At that time, I was receiving mimeographed chapters of Samuelson's forthcoming textbook and was

very much interested in seeing if the ideas could be absorbed by nonspecialists. Also at that time, the meetings at Bretton Woods, the role of institutions like the IMF (International Monetary Fund) and the World Bank in stabilizing the world economy, and the founding of the United Nations were much discussed. I had a deep interest in seeing how they would fit with Keynesian theory that I was putting into a book that was essentially based on my dissertation. This book was being written for MacMillan Company, and I was deeply involved in combing the underlying theoretical analysis, developing a mathematical formulation, and providing intelligent guidelines to average citizens. This formed the basis of what I was teaching at the Abraham Lincoln School in evening classes.

During this period, I briefly joined the Communist Party in Chicago but had little activity in it and dropped out within a year or less. In particular, I had formulated a theoretical correspondence between Marxian and Keynesian macroeconomic models and formalized the former for comparison with the latter. When I found that Communist Party members had no interest or ability to think in this direction, I dropped out.

A few years later, in the mid-1950s, congressional investigators visited many academic institutions, and in Ann Arbor, at the University of Michigan, they investigated me. It was not the McCarthy group, for they were from the Senate. It was the corresponding House group. The University of Michigan refused to give me a tenured professorship which had been proposed by the economics department, so I moved to Oxford to do the same things in research and teaching at the Institute of Statistics that I had been doing at the Survey Research Center in Ann Arbor.

I stayed in Oxford for four years and was inclined to remain after I was appointed reader in econometrics, but a very attractive offer from Pennsylvania, plus assurances from the university administration that I would have full academic freedom, became irresistible. At the

same time, the University of Michigan reversed its position, but I was too comfortable at Pennsylvania to accept their revised offer.

The move to England during the [Joseph] McCarthy years and the return to the United States involved both professional and family interests. The family ties became more demanding after a few years in Oxford and played an important role in my deciding to return.

While in England, I met Michal Kalecki on his return from North America to Poland, via Oxford. I tried to convince him to return to Oxford, but he was determined to go back to Poland, where he continued to be intellectually productive. He filled gaps in my knowledge of the development of economic analysis during the 1930s, when I was just a schoolboy. He described the thinking at Oxford, in the Institute of Statistics, and his personal interactions with Keynes just prior to the publication of the *General Theory*. From the time that I reviewed Roy Harrod's *The Life of John Maynard Keynes* and noted Kalecki's independent anticipation of the theory, he felt that the attitude of UN and other colleagues to him changed markedly.

We said, "We know what we did in the Cowles Commission. We know what we did in Michigan. We know what we did in Oxford. Now let's do a new U.S. model." We built a whole series that has been called the "Wharton Model" because it was located in the Wharton School. The interesting thing about that period was that the Social Science Research Council wanted to have a master U.S. model that would be definitive and used by many people. So we put together a model that was supported by the Social Science Research Council, with some foundation support. I think the Ford Foundation, the Rockefeller Foundation, and the National Science Foundation put some funds in.

We said we would have a specialist on capital formation, a specialist on consumption, a specialist on labor markets, a specialist on monetary economics. And we put together a model

with about fifteen or twenty specialists. One area of specialization was input/output—to have that as a centerpiece within the system. Then, after a few years, it was housed at the Brookings Institution, so it became the SSRC-Brookings Model.

There was a committee at the Social Science Research Council, a Committee on Economic Growth and Stability, that supervised the model work. We had a meeting in the late 1960s, and the concept arose that disturbances in one country spread to another. And particularly at that period the dollar was changing from dollar shortage to dollar glut, and causing ripples around the world. We wanted to study what we called the “transmission effect.” So I said, “We can put together a model for the whole world,” but we started out thinking of the OECD (Organization for Economic Co-operation and Development) world or the leading industrial country world. “We can put together a model that would show the transmission among the main industrial countries.”

At that time, I had been doing some parallel work with a colleague at Pennsylvania, F. Gerard Adams. We were doing it for UNCTAD (United Nations Conference on Trade and Development). That was when Raúl Prebisch was secretary-general. We got involved with UNCTAD in modeling developing countries, and not so much individually but as a group.

RJ: As a single group?

LK: Well, Latin American or Asian or African. We contributed to an UNCTAD report, based on our models of the late 1960s on what the expansion of the developing world would look like. I got involved in that because I had sent two or three students from Pennsylvania to the UN.

RJ: To UNCTAD? To the UN in New York?

LK: Yes, in New York. That was when UNCTAD was in New York. Prebisch was in New York. Sidney Dell helped to organize UN participation in New York.

RJ: Not Alf Maizels?

LK: No. We worked on commodities with Maizels. But Sidney Dell was coordinating this study. Sometimes the center of gravity was in Geneva, and sometimes the center of gravity was in New York. It went back and forth. But in the 1960s we were working almost entirely in New York.

RJ: Was that when you began to think of the LINK Project as such?

LK: Yes. We had a conference that was called, "Is the Business Cycle Obsolete?" During the [John F.] Kennedy/[Lyndon B.] Johnson administrations, the United States had a decade-long expansion. Many people said, "That means business cycles are no longer with us." So we held a conference in London, not that the participants believed in the obsolescence of ideas. The conference had support from the IMF, and I think some UN support, and some Social Science Research Council Support. A volume was published with the same title as the conference. We reported on models for individual countries and what we could learn about international transmission mechanisms. I remember Pieter de Wolff came from the Netherlands to that conference. Stanislav Menshikov came from the Soviet bloc. I have given up so much of my library these days that I don't know if I have the complete list of characters.

RJ: Tinbergen didn't come?

LK: No.

RJ: And Richard Stone didn't come?

LK: I don't think so.

RJ: Or any of his Department of Applied Economics men?

LK: No. The man who edited the book, *Is the Business Cycle Obsolete?*, I think was Martin Bronfenbrenner. Bert Hickman of Stanford was there. There was one Sovietologist, Herbert Levine. He had been one of Bergson's students at Harvard. And the support for the conference may also have come from the Ford Foundation.

After the conference, I said, "We could build a model. We could find a person in Japan. We could find a person in England." Jim Ball was supposed to come from England, but there was a death of one of his children and he couldn't come. But he was to represent the British side. We thought about this. Right after the war, Jacques Polak had written to me saying that he had thought about the concept of an international model. He wrote an article in the *Review of Economic Studies*, I think, in the 1930s on the outlines of such a model. I don't know if he came to that meeting or not, but I had been in touch with him a lot. Tinbergen wasn't there, but de Wolff was there.

The steering committee got money from the Ford Foundation, from the IMF, and from the UN to put together the LINK Model. At first, it consisted of about ten major industrial countries' representatives and models. Some groupings in the developing countries participated. I always insisted that, as rapidly as possible, we should have the CMEA (Council of Mutual Economic Assistance) countries—Eastern Europe and the Soviet Union—in the system, and that we should integrate individual developing countries as soon as they have databases and scholars who could work with the available data and produce models. At first, the United Nations wanted to have control over the LDC (least-developed countries) representation in that model on a group basis. But gradually we found people in individual developing countries who could handle some of the work.

Tinbergen had supervised some of the Indian economists. There was a Nigerian, Sam Olofin, who had studied in the United States and had a degree from the University of Ibadan. I had supervised a thesis on Mexico at Pennsylvania, and we had good data on Mexico. So one-by-one, we started bringing in developing countries. We started out with about ten or fifteen participating model-building groups, and within ten years it was up to eighty. In some cases, I actually got grants. I got one grant from the Rockefeller Foundation for the Ibadan work. They sent people over—one man who had a Ph.D. from a United States university. And they started working on the building of a Nigerian model.

RJ: Someone told me that you had used some of your Nobel Prize money to support work on the LINK Project. Is that true?

LK: It's not exactly true, but it would be true in this sense: once you get the Nobel Prize, you get a lot of honoraria. I had one large honorarium—\$50,000 or \$60,000, which was large at that time—and I contributed half of it to supporting the LINK Project, to keep everything going, and to get people to participate from developing countries. The actual Nobel Prize money was deposited as a lump sum, and then I bought a summer-house for our family. But the spin-off—

RJ: —supported the LINK Project. To me it is interesting that, if I understand it, both the U.S., and perhaps the U.S. model, and some of the British models, and then the LINK models were independent or semi-independent of government.

LK: Yes, they were.

RJ: Now that is, to me, a very interesting decision, though not an obvious one at that time.

LK: No. And there was one other very big one, and that was the Japanese model, which was at Osaka University.

RJ: Was that a political policy issue early on, or was it that the expertise lay with the university world?

LK: Within the U.S. government, in this period, there were no models. I could be political and tell you some of the things that I do know about that. Leontief once said to me that he didn't get what he wanted for an input/output model because Charles Wilson, the head of General Motors, was in the cabinet, and he chose support for the space program rather than input/output research. Leontief said that input/output got squeezed out and the space program got supported. That is one attitude.

I went to a series of meetings at our Census Bureau sponsored by the American Economic Association. Then, Arthur Burns was chairman of the Council of Economic Advisors. He and Julian Shiskin got the Census to take over the economic indicators from the NBER. When I was put on the committee, we went to the Census for meetings, and I can remember committee members who said, "We are going to change things politically. We are going to have models, and we are not going to rely mainly on the indicators."

Then there was model-building within the government. I gave the Department of Commerce all the database and all the computer-ready equation system of a model I had built at the Wharton School, because we were going into a new model which was more ambitious. And they got started with the model that I had already built. That model was in the commerce department for about fifteen or twenty years, and then they dropped it.

RJ: Let me come back to the UN and the LINK Model. A whole pile of questions comes to my mind. One, was there any attempt to use the model to look at some of the issues of East-West relationships, and in a way that tried to defuse the political tensions and look at either common areas of economic interaction, or even common areas of economic interest?

LK: You see, many people said that you couldn't model the East—the East being essentially the CMEA countries—because everything was forced and planned then.

RJ: And they had a different GNP (gross national product) system, right? They had net material product (NMP).

LK: That was a real problem, but we managed that problem very quickly. We had parallel calculations of the total concept, and the corresponding estimate of GDP, by separate treatment of services and other accounting conventions. And indeed, I have been very active in the modeling of China, and we dealt effectively with the same problem.

RJ: Recently?

LK: In 1980. We started on China models in a big way. There, we got the Chinese statisticians to make their own calculations of the western concept.

RJ: Using western definitions.

LK: Not before 1980, but after 1980.

RJ: But to come back to the issue, you were saying that CMEA was a planned system and how can you combine that with the western model.

LK: In the first place, you see, consumer decisions were not micro-managed. People could make decisions about their budget shares, given availability of goods and services. Secondly, foreign trade from the late 1960s onward, after bulk purchasing and bulk shipping were reduced in the Soviet Union, they started accepting world market prices. We followed some guidelines. We used German export and import prices, which were very detailed by commodity, and became relevant for the Eastern European countries. We used to get reports from the people who worked at UNCTAD, who had ties to the Soviet Union, on the U.S. agriculture department's estimates each year for the height of the crop before harvesting, and

then formed our estimate of what the Russian yield would be, and what the import needs would be. We used all of that material operationally.

RJ: In retrospect, were your estimates pretty good?

LK: They were good, yes. The USDA (U.S. Department of Agriculture) did a very good job of judging in advance. That is, by June or July, we got good estimates.

RJ: Did this mean, then, that the CIA and other national groups in the U.S., and indeed in Europe, had real interest in these results—more, perhaps, than they would in other more general work coming out of a UN-related group?

LK: They did, yes. They came to me often to ask what we were doing, and what the numbers were showing. At that time, I had a trainee from Novosibirsk in Siberia at the University of Pennsylvania for some time, maybe over a year. He went back to Novosibirsk and monitored the U.S. economy through models. Then the local CIA agent would keep coming around to me and asking if he could see what Novosibirsk was estimating for the United States. The CIA people said to me, “We want to encourage model-building because it is much better if not only the Soviet Union, but all CMEA countries use systematic methods and objectives.”

RJ: My impression of the very few U.S. intelligence reports at the time was that they tended to overestimate what was subsequently seen to be economic growth rates of the Soviet Union.

LK: They overestimated the ability to sustain them. No, I used to be concerned in LINK solutions and other calculations about the percentage of Soviet GDP originating in the military. I used to talk a lot to Sovietologists in the universities, both in the U.S. and the Soviet Union about whether they were spending ten percent, fifteen percent, twenty percent. And I must say, at some meetings that I have attended in recent years with [Mikhail] Gorbachev, he spoke of much

higher percentage values, though I am never sure whether he is talking about percentage of the government budget or percentage of the GDP. But he gives higher values. It had to be over twenty percent, which would be very high for a poor country. We got very much interested in the question of the tradeoff. It's a classic economics textbook example of "guns versus butter."

RJ: When was the first time that you started doing any work on the tradeoff between military spending and consumption or investment—nonmilitary spending essentially? And when was the first time that you brought that into the LINK Model?

LK: It must have been in the 1970s. Of course, as soon as China started to grow—I went to China in 1979 to lead an American delegation of economists, to make contact—we started modeling for China. Then I went a couple of years later to a meeting in Hungary. A leading Hungarian economist had to leave our meeting to go to Moscow for a meeting. He came back and said, "You know, at that meeting, they didn't want to know how Hungary was planning the economy. They really are worried about China." We then started, in the early 1980s, the calculation of the guns and butter tradeoff. We might have started earlier, but we certainly started it then.

RJ: Because the UN held a major meeting on disarmament and development in 1978, a special session, and again in 1982, and I think a third one in 1986. For the first one, Inga Thorsson, the former under-secretary of state for the Ministry of Foreign Affairs in Sweden chaired a group for which the UN produced some technical report on the tradeoff. I thought they used some econometric model—this would have been 1978—that looked at the impact on employment of a given shift from military to investment, or from investment into consumption.

LK: I can't remember exactly, but I know, in the 1980s, I gave some lectures in China on that issue. And in Eastern Europe—in Poland and probably in Hungary—we worked on the tradeoff problem. But I can't remember exactly what year we started doing it.

RJ: Had Leontief also done some work for the UN?

LK: Leontief did work that was published in *Scientific American*. But he did it in the following way. Suppose a country shifts resources from the military sector to the civilian sector, and the level of unemployment is maintained. You see, that's the main thing. We said, "If you make that shift, you might have a feedback effect with rising unemployment."

RJ: He didn't have a complete model. Is this your point?

LK: He said that if you maintain the same level of unemployment and you shift people around, then there is an improvement if you concentrate on butter rather than on guns. He published that in the *Scientific American*.

RJ: But it wasn't an econometric model-based analysis?

LK: He didn't look at the problem of demand. I presented a general paper in 1971 to the American Philosophical Society—this society was founded by Benjamin Franklin, "for promoting useful knowledge" at meetings in Philadelphia. I was made a member at that time, and was asked to read a first paper. I gave a paper focussing on the U.S. economy, asking whether war was essential for the maintenance of prosperity in the United States entitled, "The Role of War in the Maintenance of American Economic Prosperity." I went back, of course, to the Civil War, and the earliest period for which we had information on military spending and civilian spending. So from the point of view of the United States, I have worked on that issue a long time.

RJ: Was that the first time you had done any—

LK: Well, after the Vietnam War, the Arms Control and Disarmament Agency of the State Department sponsored a book, and I submitted a chapter with a Japanese associate, Kei Mori. We estimated that pulling out of Vietnam would not cause a recession in the United States. It was very much like what we had done with the Korean War, and what we had done with World War II. But it was done particularly in the circumstances of the Vietnam period. Many people thought that once the Vietnam War ended there would be a recessionary impact.

After that calculation was done, I got much more interested in the arms reduction scenarios. We made that calculation by saying that if the money that is saved from smaller military spending after Vietnam is put into productive capital that produces a future upward shift in productive investment that is not present with military spending, then you get—

RJ: A growth effect, too.

LK: Yes, a supply side effect in addition to changing demand. Then we started doing that for many countries.

RJ: Which led to the work in the early 1980s.

LK: Yes.

RJ: There is also, in ECAAR (Economists Allied for Arms Reduction) not merely a technocratic interest, but it seems to me some value commitments, a feeling that the military process has gotten out of hand and is pushing a higher level of armaments. Have you ever felt that at different times?

LK: I was at the meeting—it must have been about 1990—when ECAAR was formed. It was at meetings like this for the Allied Social Sciences—they were in New York at that time—and people attending said, “If ECAAR is to function as an interesting and viable organization, it should not be just an arms reduction organization. There is some level of

armament that is needed. It should not be a grouping of conscientious objectors, or ‘peaceniks.’ It should be one that uses the ideas of economics to analyze the arms race, or other military problems, in a professional, technical way, so that the organization will command respect and produce results that are plausible and believable.”

RJ: Let me come back to LINK again. We have talked about the LINK Project. We have talked about how it was used in the 1980s to look at the tradeoff between disarmament and development. Do you think the UN could have used LINK much more?

LK: Oh yes. The principal use of LINK at the UN has been to provide projections and scenarios for the world economy. In the popular press, and among many professional economists, the attitude is that the United Nations has a constituency and that they pander to that constituency. When we sold Wharton Econometrics to the private sector, I got a promise of about \$1 million, spread over ten years to support LINK. I met with both Goran Ohlin and the Secretary-General, who might have been [Javier] Pérez de Cuéllar, saying, “What we really want to do is to make the UN pronouncements on the world economic situation, and prospects, and scenarios credible. And we want it to have the same credibility that the IMF gets, that the OECD gets, that the World Bank gets. People won’t look on and say, ‘They are just talking for a select group.’” We said that would be the main contribution that LINK could make to the UN.

RJ: Do you think the UN has used LINK in this way?

LK: It has. You see, I think the new reports over the last few years in the *World Economic and Social Survey* represents a very good job and is very convincing. I am not sure everybody has changed their minds, but it comes off very well.

RJ: But if I understand it, the IMF hasn’t got a competitive LINK.

LK: They do. An Australian, Warwick McKibben, was in Washington for quite awhile. Now he is back in Australia. But the UNU (UN University) in Tokyo has used his scenarios quite a bit. If you look at the book called *The Peace Dividend*, edited by Norwegian economists, you will find three world models reported on in that book. One is LINK, and Peter Pauly has a chapter there with Hung-yi Li. McKibben has a chapter, and I think that's an IMF point of view, if I'm not mistaken. And then there is one other chapter, which is a group of three or four people who worked either under contract or as employees of the World Bank. Then OECD has had an international model for a long time. It was put forward first by Lee Samuelson—not related to Paul Samuelson—who was on the OECD staff.

Then, on a smaller basis, there were two others. There was an international model for the European Union (EU) in Brussels or Luxembourg. The other was from the Economic Planning Agency of Japan. The Federal Reserve, for a while, had one. They have gone to individual country levels now.

RJ: Coming back to the UN, could the UN have used the LINK Project, not merely to make plausible econometric projections of the likely scenarios, but to make more explanations, say, of alternative scenarios at the time of the OPEC (Organization of Petroleum Exporting Countries) oil price increases, or for recent oil price increases?

LK: Yes, they could have. I am positive of that.

RJ: But they didn't.

LK: But they didn't. I will tell you why, but I won't tell you the names of the people involved. There was a staff member in UNCTAD who didn't like it. During the 1970s, we did oil price simulations, and he didn't like our showing that certain countries that were classified as

developing countries had a gain because they were exporting oil, and that gain occurred at the expense of poorer conditions in the major importing countries.

RJ: I hate to say it, but surely that was obvious in general.

LK: But he didn't want it said in a UN document. He called me into his office and said this was dangerous. I had an article that was on the peace dividend scenario, saying, "What if the UN had a force that stopped the war in Yugoslavia? What would the world have looked like?" A person told one of my former students that I could go ahead and work on that, but he couldn't. He was at the UN, and he would get into a lot of trouble.

RJ: And surely the oil price increase was only one scenario for changing the terms of trade between the industrial and developing countries. And one could have explored other price changes, which was the essence of UNCTAD's work.

LK: Oh yes. There was a certain trepidation on the part of some UN people about getting into that problem. They thought they would generate nasty interventions from other countries. The countries that were gaining, I guess, didn't want to be looked at as gaining at the expense of somebody else.

RJ: Very unfortunate.

LK: You see, we did it but we didn't play it up in a big way. And they were never asked for—

RJ: If you were given a free hand by the Secretary-General today, and asked to advise, what future projections for the next twenty-five years should the UN explore by way of scenarios, linked to reductions of poverty, linked to better income distribution between developing and industrial countries—

LK: Well one that I clearly wanted to see get off the ground is to look at AIDS (acquired immune deficiency syndrome), because the economic feedback from AIDS in countries that need people in good health, who are sturdy and between the ages of twenty and fifty, who are being hit hard by AIDS, is having a very perverse productivity effect. The payoff on an AIDS analysis could be enormous if that were studied carefully. *The New York Times* had a very interesting editorial a few months ago saying that the pharmaceutical companies are much more interested in treatment medicine than vaccination and preventive medicine, because it is more profitable. Things like that could be investigated in terms of what would be saved if you did it one way or put resources in another direction.

That is one that I have thought about a lot and would like to see. Another, of course, is on income distribution, education, and public health. In a few trial examples, a colleague and I have estimated a model for Chile, and a model for the Philippines, in which we have age groups, and we have feedback between the demographics and the economics. We have population growth related to fertility rates and education. I think one could put all that into a very neat model exercise. But they tend to be larger models and need more staff to execute them. I think they would produce very interesting results; that has not been the place where the people who hold the funds want to put them.

RJ: Let me ask you, in the last ten or fifteen minutes if I could, about your impressions of the UN. Less your involvements, but looking at the United Nations and thinking, perhaps more generally, where it could have perhaps been more powerful, or perhaps where it has been more influential with ideas in the economic and social arena than perhaps many people realize.

LK: The UN has done an enormous amount of good for the world. I wouldn't say that the UN is efficiently run, but I don't know if you can run an organization like that efficiently.

However, they work on good problems. And it has been a political battle in the United States to get the contribution up to a decent level. But much more good could come. I think that is part of the answer to the contemporary problems we are having. We are not paying enough attention to income distribution, health distribution, living standards, and poverty levels. That is where the focus of the UN should be.

In addition, I conceived that paper on the UN standing army in the book on the peace dividend saying that is really the main long-run hope for world peace. Now the UN is not in a position to field such an army, but they could go step by step, have rapid reaction forces on a smaller scale, see if they work, and go more into the concept of a mercenary army. You hire people to be your soldiers and see if that would work. The UN should accept some of the offers that have been made, say from Canada, to use old army facilities, army posts for training, and get a real effort started along this line.

That is the real hope. If you read the articles of 1946 carefully, you would see that this was what we hoped the UN would become. And it didn't. But I feel they just haven't tried hard enough.

RJ: Interesting. In terms of your experience, perhaps in Pennsylvania, of the private sector linked with foundations, perhaps linked with international finance support, does that experience give you thoughts on what the UN might do?

LK: Well, yes. But I have been disappointed on one account. I think that Russia got very shabby treatment after 1989, in terms of this way of looking at things. West Germany had to plow \$100 billion a year into East Germany for three or four years and it got a certain amount. Probably in the long run, it will get a lot out of that. It is really incredibly small to think of only \$10 billion or \$15 billion going into Russia. We brought down Russia and then walked away

from it. It could have been done so much better. All the calculations were wrongly based. The appropriate order of magnitude wasn't realized.

RJ: Well there was talk, surely, at the time of \$24 or \$25 billion.

LK: But you see, if you look at East and West Germany, it has to be—

RJ: Yes, that was small by the standards of—

LK: That could have been an interesting calculation.

RJ: Was LINK used to make that calculation?

LK: No. But you see, I went to a man who used to be the provost at the University of Pennsylvania, Vartan Gregorian, and I said, "Would Carnegie consider putting up some money so we can do a study of how much Russia needs to get on their feet again?" He pushed it off to his underlings, who pushed it out of sight. They didn't even want to have a discussion on it.

RJ: But Ford, Rockefeller, and indeed Carnegie at the time wouldn't rise to the challenge?

LK: I didn't really beat the bushes and go on the road. But I went twice to Gregorian. I have been giving some lectures in the last three or four months to teams of six people at a time who come from the Ministry of Economic Development and Statistics in Moscow to Philadelphia, to what is now called DRI-WEFA. I didn't give all the lectures, but I gave two lectures to each group. There were six groups. Then I gave lectures to a group of customs officers. I look at the reaction of those people, I look at the databanks that we are able to develop on the present Russia and see that there is a doable project. I wish that there would be a grassroots model enthusiasm by the ordinary civil servant who would like to come up to world research standards. Gregorian wouldn't even listen to it.

RJ: But it is true that the UN could have done more, perhaps, using LINK. Certainly the Economic Commission for Europe (ECE) made some very sensible recommendations in 1989/1990, that you mustn't rush the transition, and it needs bigger support. I don't think that was modeled econometrically, as might have been done.

LK: Yes, we could have done much for that.

RJ: Let me ask you about the Nobel Prize. It must have been exciting to receive it. Did it change your life?

LK: It puts you in the eye of a storm, for a while. All over the world, you find there are autograph collectors, photograph collectors. The demands on your time are enormous. A lot of people want to have a conference and there is a desired critical mass of Nobel laureates, whether they know the subject or not, that they want to have. It is a minefield, and you have to know what to accept and what not to accept. It puts very heavy demands on your time.

RJ: Now in the late 1970s, if I understand it, you were asked to be chairman of the Council of Economics Advisors for President [Jimmy] Carter?

LK: No. When I was asked to help him on the campaign, I told him that I wanted to stay in academic life. And I told this to his close advisors. You see, a man who had been at the Wharton School went to an interview that Jimmy Carter had with *Business Week* magazine in 1975. *Business Week's* editor said to Jimmy Carter, "What kind of economy do you want to see for the United States?" He said, "I want to see two percent unemployment, a two percent interest rate, and a two percent inflation rate." This man walked out of the room, and said, "Mr. Carter, you had better get an economist."

The dean of the Wharton School, who was a friend of mine for many years, called me out of a meeting at the St. Louis Federal Reserve Bank dealing with model-building, and said,

“Would you be willing to help Jimmy Carter?” I said what everybody said then: “Who is Jimmy Carter?” Then I met him and I liked him. We really won that campaign, I think, on the economic arguments. When it came to the Pennsylvania primary, we had a really good sounding program. Whether it was a program that would work in practice or not was another issue, but it sounded very good. That really put him over.

All during the time he was in Washington, in the White House, I used to come down and help with judging the economy. But being the center of the political action is not very desirable. I wanted to stay in academic life.

RJ: In terms of giving advice to different parts of the UN over the years, do you feel the UN makes good use of expertise such as you have?

LK: Fair use. I don't think they make as good as they could.

RJ: What might they do differently?

LK: You see, there was a man, an Indonesian, who was the head of the UNU.

RJ: Soedjatmoko.

LK: Yes. Pérez de Cuéllar had just been brought in. He assembled a group of people in New York and said, “Let's just have a brainstorming session with no agenda.” We had about twelve people and the Secretary-General, and we had a conference room at the UN Plaza Hotel. Some of the people who were high in the hierarchy, under-secretaries-general, made foolish comments about the rich countries just rigging the exchange rates to rob the poor, et cetera. Amartya Sen was in that meeting, and I was in that meeting. We said, “That's not how exchange rates are determined. That's not what the issues are. You can't just go out there and move them around and get the numbers that you want.”

It is very difficult to get sophisticated ideas through to some of the people who have power in the UN hierarchy. I think if there would be more of the thought sessions, and if the Secretary-General would take them into account more effectively, they would do more good. The current situation is that the Secretary-General is so overburdened with the military action now going on in Afghanistan and the Middle East that there is very little opportunity to turn attention to anything.

RJ: Have you been on many eminent commissions, independent commissions?

LK: There was one on disarmament and development.

RJ: That was a UN one.

LK: Yes, that was UN. Then there was the UN commission in the 1960s, to estimate a Development Decade.

RJ: Over the 1960s?

LK: Yes.

RJ: Was that with Tinbergen or Hans Singer?

LK: I think the main force behind that was Prebisch. We did the work with others, but I think that was Prebisch's.

RJ: But that was before LINK.

LK: Yes. That was before.

RJ: And that was in New York?

LK: Yes.

RJ: You didn't attempt a full-fledged international model.

LK: We didn't have it then. But we made a lot of econometric calculations.

RJ: On disarmament and development, you have talked a lot about it in general. I didn't quite link it with a particular commission.

LK: There was a little statement by eminent persons on speaking out on disarmament and development. Prebisch was there on his eightieth or ninetieth birthday. I have a reprint of that report at home. It's a very small document. Then I gave a lecture on that in France a little bit later. That was in 1986.

LK: That was attended by people who were mainly high political personalities from individual countries, but not 100 percent because I was there and maybe there was another academic or two.

RJ: In terms of UN links with the Bretton Woods institutions, tensions with the Bretton Woods institutions, do you have any comments on that?

LK: When I was very young, and the UN was at Lake Success—I was at the National Bureau of Economic Research then—I got a call from Kalecki and Mosak. I had just come back from a year in Europe and they wanted to know what was going on in Norway, what the Western European outlook was, and so on. I tried to help them. Then, of course, Kalecki was pushed out. I used to see something of Kalecki. He was very bitter, but he's not an easy person to get along with.

RJ: Was he attacked by Joseph McCarthy at the time?

LK: I don't know if he was attacked by McCarthy, but he was attacked by that group. Some people thought that Mosak didn't stand up for him enough.

RJ: Finally, any thoughts about the impact of ideas—ideas from the academic world, ideas mediated through the UN and others in terms of influencing policy?

LK: You see, I believe that it would be quite valuable if the UN had a better academic world contact. The IMF, the World Bank, and the OECD have very good rapport. It is partly because they are much better financed. They have much more liberal resources to spend. It seems to me that if a person like Ted Turner, being in Atlanta, who wants to see the UN—

RJ: And who has already given a large sum.

LK: —could make it attractive to have retreats every six months or so on international issues and the UN.

RJ: It would be a very practical idea. Are there any final thoughts?

LK: Just before this 100th anniversary of the Nobel Prize, there was a Canadian laureate in chemistry who sent around a letter to get signatures, about fifty or more, asking for some better approach to understanding in the world. I think it was drafted before the terrorist attack in the United States. Then, after the terrorist attack, it was pushed much more, saying it is much more urgent and so on. I think if it were well-chosen, and had a good agenda laid out in advance so it didn't ramble over too much, a group like that could have very thoughtful weekend dialogues with the secretary-general, and leading under-secretaries-general.

RJ: And both advise the UN, and also use the UN as a location to make public statements. Very good. Well, let me thank you very much for this interview, and for your work, if I may. Thank you.

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