

## Lawrence Klein : Economist for all Seasons >>>

Interview of Lawrence Klein by  
Y.K. Leong and K.S. Tan

Lawrence Klein is a pioneer in the creation of computer models for econometrics and economic forecasting using mathematical techniques. From the formative years of his education at University of California at Berkeley and at MIT during the early war years, he moved to the Cowles Commission for Economic Research (then at University of Chicago) where he formulated a model of the United States economy and predicted an economic upturn after the war. He is well-known for the enhanced economic model called the Klein-Goldberger model and for the famous "Wharton Econometric Forecasting Model". He has built economic models of the United Kingdom, Canada, Japan and other developed and developing countries. He has served as a consultant to the governments of many countries; in particular, to China as it opened up to the west. The impact of his work on modern economics and his influence on present day economists are well recognized. He was awarded the Nobel Prize for economic science in 1980.



Lawrence Klein

He was president of the Econometric Society and of the American Economic Association in the late sixties, and founded Wharton Econometric Forecasting Associates (now Global Insight) in the sixties. In the seventies, he started "Project LINK" to connect the models of some international countries in one of the first attempts to produce a "world economy" model. Besides being a practitioner, he is a scholar who has written extensively on econometric and economic models.

He joined the Department of Economics at University of Pennsylvania in 1958 and became the Benjamin Franklin Professor of Economics and Finance at its Wharton School of Business in 1968. He is now an emeritus professor at Pennsylvania and continues to be active in research and consultation.

He was a key speaker at the program organized jointly by IMS and the School of Economics and Social Sciences of SMU in April and May 2004 on econometric forecasting and high-frequency data analysis. The Editor (Y.K. Leong) of *Imprints* and Kim Song Tan of SMU interviewed him on 6 May 2004 at the Institute. In the following edited and vetted

excerpts of the interview, the 84-year-old distinguished economist talks about his early university education, the scientific challenges of economics and a life-long dedication to the application of economics for the welfare of humanity.

**Imprints:** You mentioned in your Nobel Prize autobiography that you were attracted to mathematics and economics when you were in college. What made you decide to do your PhD in economics rather than in mathematics?

**Lawrence Klein:** For one thing, I started thinking about a long-term career as I entered college. I thought first about being an economist, and I stayed with that. I thought of mathematics as a tool to gain better understanding of economics. Also, when I was very early in university, we used to go out in teams to different colleges in the area to participate in mathematics competitions. I decided that I wasn't really going to be good enough a mathematician to win those competitions and that there were young people of my age who were better mathematicians. So I stayed with economics.

**I:** Was your early mathematical training sufficient for your later work in economics or did you pick up most of the required mathematical methods as you went along in your research?

**K:** As an undergraduate in university, I had about half my classes in mathematics and about half in economics. But when I went to graduate school, I went further in mathematics. It was a very good graduate school at MIT and the mathematics was very good. So I picked up more mathematics and this was a period when mathematics was just beginning to be used in economics on a bigger scale, while most of the early work followed what we might call classical methods. When John von Neumann and Oscar Morgenstern introduced the theory of games, one had to go to set theory and other kinds of mathematical reasoning. I made the shift. And a lot of work dealing with dynamic systems in economics requires stochastic studies of dynamics that involves stability properties of systems and differential equations of more complicated sorts; so that I had to, for some time, keep studying mathematics. But then I got more and more involved in the applications of mathematics to economics and in the applications of economics to real world problems. Gradually over the years, mathematics got more and more complicated and deeper for economics and professionals. So I didn't keep up studying mathematics endlessly. I shifted more towards doing things with economics and the mathematical basics that I had already started.

**I:** Do you think that mathematical rigor is necessary for economic training?

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**K:** Rigor is important. You could be wrong, thinking you are right by not being quite rigorous and finding that there are exceptions and things that you have missed. I think what one really wants is imagination above rigor. And then you go to your friends who are mathematicians and check to see if your imagination and intuition took you in the right direction.

**K.S. Tan:** In that context, how do you find the current trend in economical study which a lot of people complain is becoming too mathematical and not relevant or practical enough?

**K:** I won't say it's too mathematical but it's often too abstract. I think that some of the theoretical work in mathematical economics has drifted away from the important problems.

**T:** Do you see that as a potential problem in the sense that those who are in the university teaching economics are so well trained in mathematics that they feel compelled to continue along this path and go further and further away from the real problems that they might see?

**K:** They really do, but, of course, we have an important obligation to teach and give students ideas about economics. We should keep in mind that we are doing economics and not pure mathematics.

**I:** How much were you influenced by the style and philosophy of Paul Samuelson in economics?

**K:** That was important for me. When I was an undergraduate, I independently had the interest in finding how mathematics could be used in economics as a tool. One day I went to the library at the University of California and was thumbing through issues of "Econometrica" when I found in early issues (just beyond the first decade of the journal) articles by Paul Samuelson. I was so impressed by them that when I had a chance to go study under him at MIT, I realized that he was at that time, and still is, the greatest American economist of that period.

**T:** There are also people who say that people like you, Samuelson, Solow in your generation, are really great thinkers, not just economists, whereas the current crop of economists are just economists, technicians. Do you buy that argument?

**K:** The people you mentioned and some others like them are interested in mathematics to deal with the problems of the world we live in and to quantitative economics as a tool, but there are many people today who get away from that concept.

**I:** It seems that economics is, in some sense, observational

and empirical and yet one cannot conduct controlled "economic experiments" in the sense of experiments in the physical sciences. Does it then make sense to talk about "economic truths"? If it does, are there objective "laws" in economics in the sense of scientific laws in physics or chemistry or even biology?

**K:** Of course, physics, chemistry and biology are very different. The broad concepts of science include some sciences that are very respectable but have no controlled experiments: meteorology (it's not an experimental science), seismology (it's hardly an experimental science) and astronomy. Yet they go far with mathematics. Apart from meteorology and seismology, astronomy is very precise. Now, in addition to controlled experiments, which are important, the defining thing is the ratio of noise to signal. In astronomy the noise to signal ratio is very low. Meteorologists have gone very far, but if you judge meteorology by looking ahead as far as one month or more, the findings don't look impressive. But if you judge meteorology by the next minute, the next day, or two days, it looks impressive and it's getting better. Economists should follow some of the techniques that meteorologists use. They tie in to the computer much more intensively. They send balloons into the atmosphere, fly aeroplanes through the hurricane's eye and learn more. We don't get enough of that extreme information flow in economics. Seismology understands what happens during an earthquake but they don't understand how to control or predict it. Now to some extent people are trying to introduce controlled experiments in economics. I often thought about that issue, say, like going to an institution such as a prison, change the economic values and look at the outcomes. It's possible to have some experimentation in economics. Nobel Prizes were awarded to some economists who did experimental work and went further than collection of data. But, by and large, economics is not an experimental science, and we must try to do the best we can with that limitation. The lecture I have just given was an attempt to show how we might improve our ability to forecast the economy by small steps even though we can't experiment.

**I:** Who were the economists who were given Nobel Prizes for their experiments?

**K:** Vernon Smith of George Mason University. He shared the prize with Daniel Kahneman of Princeton. [The citation for Smith reads: "for having established laboratory experiments as a tool in empirical economic analysis, especially in the study of alternative market mechanisms". - Imprints]

**I:** Economic phenomena appear to be governed by random decisions in human behavior in a way not unlike those encountered in the history of mankind. Yet economics, but

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not history, has been hugely successful in making economics into a science. Is this due to a gigantic leap of faith on the part of economists in the methodology of mathematics?

**K:** In the work that I do in economics and econometrics, when something very big happens, like the OPEC decision in 1973 to limit oil production, to limit oil exports, to raise the price of oil four-fold or eight-fold, it is almost an arbitrary decision, unexpected. Now I say we cannot predict that OPEC would do that, but once OPEC has done that we can predict the outcome. I think we did very well with that. A number of the predictions that I have been involved with were of that sort. During the closing days of the Second World War, I shifted from MIT and was asked to help build a model to predict whether the United States will revert back to the Great Depression as soon as demobilization and peace were achieved. We made such a prediction. It was against almost everyone else's view, and it turned out to be right: America *would not* go back to the times of the Great Depression. And there have been similar times - after the Korean War, the Vietnam War, and even now, the present war. So we say we can't do a really good job in predicting those events but when those events have occurred we can do a reasonably good job in judging the outcome.

**I:** Are there such things as economic laws? Do economic laws exist?

**K:** I wrote a paper once on "Some Laws of Economics". One of the interesting laws I looked at from time to time is called "Engel's Law", which he (Engel) found by studying social groupings of people in Europe - the percentage of a family's income spent on food declines as income rises. It may be a fairly weak law, but it holds. At the 100<sup>th</sup> anniversary of Engel's Law, the econometrician Professor Houthakker wrote a paper surveying countries all over the world to see if Engel's Law held. And then there was a very important event. When it came to China, he said he couldn't get data from modern China (that is to say, the beginning of the communist regime in China) but he found some Chinese family budgets from around 1920, or so, like the ones Engel found, and he said, "Yes, Engel's Law held." I was fascinated by those remarks. When I went to China for the first time in 1979, I got hold of a paper by a Chinese American economist surveying consumers in Tianjin and he got almost the same coefficient that Houthakker had found from the twenties in China for Engel's Law. So that particular observation by Engel had great longevity. There are many others that I cited like that. They don't give you enough information to know as much as you want to know about the economy. They don't cover a big enough part of it. There are some laws like that which have held up through centuries or decades.

**I:** When was Engel's Law formulated?

**K:** In the 19<sup>th</sup> Century, in 1857. The people in the sample were Belgians, and Engel (Ernst Engel) was German or Prussian.

**I:** To what extent do the non-quantifiable elements of politics and culture contribute to the economic modeling of a country?

**K:** They contribute a lot. My last example in the lecture that I just gave was the use of sample surveys, not completely non-quantifiable, but not very quantifiable, of people's attitudes after the attack on the World Trade Center. The way we use surveys for consumers: to determine if you were better off, worse off (on a 5-point scale), much better, much worse or about the same. Let's say there is limited quantifiability. We found these very important in giving us guidelines on what consumers were going to do after that big event. There are many such things like that, and I claim that it's important to study subjective attitudes in decision making, political structure, and legal structure of politics and culture. Yes, they contribute. We should be aware of them and we should take them into account to the extent possible. Sometimes that extent possible can be stretched because we learn new methods of finding out about political and cultural events.

**I:** You constructed economic models for several countries like United States, Japan, Canada, United Kingdom and others. Were these models used by the various governments in planning their national economic policies? Have you done any economic model for a developing country?

**K:** The answer to the first question about whether they were used: yes; many governments have used these models or models evolved from them. I don't think you should use the word "planning"; I think it's in formulating their economic policies, such as interest rate policy or tax policy. It is not planning but it is using the models for doing the government's work and definitely used in that respect.

Any economic models for developing countries? Yes, for many. Right now, I have been involved, for a number of years, in modeling of China and I'm working on models for Russia. Russia is a transition country moving from planned economy to market economy. I have worked on Mexican models a great deal. I've helped a lot with different African models and different Asian country models.

**I:** Do you think that there is still a gap in communication, if not in interaction, between the majority of economists and the majority of mathematicians?

**K:** There are definitely gaps. If mathematicians are broad-minded enough and the economists are patient and careful enough, we still communicate quite well. But at the

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extremes, there have been major debates in our National Academy of Sciences whether the social sciences (that includes not only economics but also political science, sociology, anthropology) - whether some of them - should be included in the National Academy of Sciences. They have been there for a long time, but the academy had to make a conscious effort starting in the sixties or seventies to open up class groupings for social scientists, and some mathematicians have been very harsh in complaining about that. There were fights in the academy with mathematical members over the election of some social scientists. There was a very big fight at the Institute for Advanced Study in Princeton, over the hiring of a sociologist, by some mathematicians at the Institute. So these things happen.

**I:** Do you think that a mature mathematician could learn enough economics to make a non-trivial contribution to economics or do you think that he or she should possess some innate “economic acumen or intuition” in order to do so?

**K:** I don't know if it should be innate but I think that if mathematicians want to comment on the role of economics in social and political life, or economics as a social scientific discipline, then they should learn something about the way the economy functions and the way economic decisions are made, and then there will be better communication.

**I:** I will wrap up my questions with one question that is quite philosophical. You have dedicated your life to creating and developing a whole generation of economic models. Other than the Nobel Prize, what is the greatest satisfaction that your life-long work has given you?

**K:** Well, to see the models used. For me, one of the great things was that when I started in the faculty in the University of Pennsylvania, we knew the models and the application of models by the business and the public and government communities, and in doing so, we raised enough financial support so that over the years, between 10 and 15 PhD students every year were being supported by us. We paid their university fees and living expenses, and now they have gone out into the world and many have been very successful. That gives me a lot of satisfaction. We were able to use our approach to apply economics using mathematical, statistical, numerical methods to support enough students so that they have successful careers.

**T:** In your view, do you think that economic theorizing has reached a fairly mature stage, or do you expect to see another revolution coming in the same way that the Keynesian revolution, rational expectations, real equilibrium studies changed the thinking of economists?

**K:** I think that there is plenty of open room for creative

thinking of a major sort to come and I think that the Keynesian revolution was very important. I think Leontief's work put activity up a bit; that is very important; and Samuelson's work was partly Keynesian. There were many others like that. But I feel that some of the work being done now is not getting far. I think that economists accept “rational expectations” as though it is realistic and correct; it is a hypothesis and I don't think it has been validated. There are others. I don't see a big event or a big change in the way of thinking among the most modern branches of economics that have the same impact as the Keynesian revolution had.

I think the information technology revolution had a very big impact, certainly a very big impact on what I do. It's not an economic theory, but it enables us to judge economic theory and principles much better. That was the basis of the lecture that I just gave. How can we improve economic forecasting by drawing upon the computer, the flow of information, the dissemination of information and the dissemination of policy preferences? There may also be a breakthrough, eventually, using the new kinds of techniques and facilities in the same way that I claim that meteorologists have definitely added, one day, two days, sometimes just half an hour, to the validity of meteorological forecasts. It helps utilities, helps the airlines, helps state planning. The economist will use the same information facilities to develop more accurate judgments and predictions.

**T:** You were involved over the years in work on China. From our understanding of economics, do you think that China's becoming a super economic power on par with America is a certainty? Can we say that it is a certainty that the Chinese economy will be a super economic power equal to the US?

**K:** I think you can say that China's catching up to the United States by aiming at a moving target would be unusual. I don't see China overtaking on a per capita basis eventually. I won't say it's impossible but it's not my judgment. On the other hand, China's present projection, I regard as plausible. China's leaders say that since reform (since 1978) China more than quadrupled in GDP by 1980. The new target is to quadruple again between 2000 and 2020. In looking at that, I'd say there's an excellent chance of doubling by 2010. I don't say they won't double between 2010 and 2020, but they will have to work harder than they have. I've been combing records. No country has had 40 years of that size growth in terms of established statistics. One reason why I think the decade will show whether it is favorable for China's plan is the preparation for the Olympics in 2008 and the preparation for World Expo in 2010. I think that those are going to keep China very busy providing the infrastructure and facilities for those major events, and China wants to show the world what she can do in those events that will give China the opportunity.