Econometric Theory, 11, 1995, 597–624. Printed in the United States of America.

THE ET INTERVIEW: GREGORY C. CHOW

Interviewed by Adrian Pagan
Australian National University

Gregory Chow has been an important figure in econometrics for almost four decades. There can be few students of quantitative economics who have not been taught the "Chow test" for structural change in regression and equally few applied studies that do not report it. But Gregory's work has been much broader than this—techniques developed in papers on the stock adjustment model, dynamic responses, and control methods have all become part of the milieu of the practicing econometrician. It is notable that this work has never been "theory for theory's sake"; behind it has always been the desire to fashion tools that would be immediately useful for the analysis of economic data. It has also been strongly oriented toward the analysis of systems, and his interest in systems has played itself out in many ways—from simultaneous equation estimation and analysis through control methods to problems of the Chinese economy. There can be few econometricians who have made contributions across such a wide spectrum of issues.

© 1995 Cambridge University Press 0266-4666/95 $9.00 + .10

Gregory's publications include seven books and more than 140 articles. His professional output has been recognized by Fellowships of the American Statistical Association and the Econometric Society as well as memberships from the American Philosophical Society and Academia Sinica. He was the first president of the Society of Economic Dynamics and Control. In a wider context, he has played a central role in fostering relations between the economics communities in the United States and China and has advised the Chinese government on issues of economics education and reform.

I first met Gregory in January 1973 when I arrived to take up a position at Princeton University. He was extremely generous in the amount of time he spent on advising me, and his attitudes were very influential in my own later work. Therefore, it was a great pleasure to repay this kindness by conducting the present interview. The initial interview was conducted at Princeton in September 1989 but, due to a loss of some of the material on the tapes, had to be repeated in Rochester in April 1992. Hopefully this interview captures Gregory's attitude toward econometrics—an approach that is very close to that advanced by the founders of the Econometric Society and that was important in making econometrics an exciting field in its formative years.

Could you outline your early education. I noticed that you did a B.A. at Cornell. My impression is that Cornell wasn't really known for statistics at that time, so I wondered where your statistical training and interest in econometrics developed from?

I entered Cornell University as a sophomore in the fall of 1948, having finished my freshman year in China at Lingnan University in Guangzhou. I studied some applied subjects. I took a course on the economies of the Far East and was exposed to topics on economic development. For example, one book was by Colin Clark, called *The Conditions of Economic Progress*. His analysis was quantitative in orientation so it occurred to me that some mathematical/statistical analysis would be useful to economics. Now I have to say that my professors at Cornell did not do econometrics and I could not find any help from them, and so I had to do something on my own. I read Colin Clark, and then I went to the library and found something called *Econometrica*, which I could not understand. There was an article on "The Common Sense of Econometrics" in one of the early issues. That kind of article I could read, but nothing more technical. Maybe one out of every 20 articles I could get a little bit from. Still, I was enthusiastic enough to join the Econometric Society.

At Cornell, when I was studying economics, my training in mathematical statistics and mathematics was very limited, although the mathematics department was good. Feller had left but Wolfowitz was still there. In those days, less than 5% of the economics majors studied even calculus. I got interested in econometrics on my own during my junior year and started looking at Samuelson's *Foundations*, without understanding much of it. In my senior year, I took a course on ordinary differential equations, which was the last course in mathematics I took at Cornell. By the time I decided to study econometrics at the graduate level, Chicago was the best place, so I decided to go there.

You commented about looking at Samuelson's *Foundations*. Why did you think that it was econometrics?

It was really mathematical economics, but the broad definition of econometrics then included mathematical economics. I was interested in anything that was quantitative/mathematical and, while I was looking around, I found Samuelson's book. I got myself a copy and tried to struggle through it, making some comments and notes. Later on, when I knew a little bit more, I found out how foolish some of my comments were.

You left Cornell and went to Chicago. One of the things I have always noticed about the difference between English-style and American-style Ph.D.'s is the extent to which the students learn more from one another in the U.S. versions. So it's interesting to know who was in Chicago with you as a student.

Yes, you are right. Some of my fellow students at Chicago were outstanding. When I entered Chicago in the fall of 1951, I met Gary Becker—that was also his first year—and we attended Milton Friedman's price theory and Alan Wallis's statistics classes together. At that time he was my closest friend, and sometimes we got together to discuss Milton Friedman's problems sets. In my third and fourth years—writing my dissertation—I was a member of Al Harberger's workshop on public finance. That workshop also had Larry Fisher, Zvi Griliches, Dick Muth, and Marc Nerlove. I also attended Milton Friedman's money workshop, where I presented part of my thesis. David Mieselmen was collaborating with Friedman on their well-known paper comparing the relative predictive power of money stock and autonomous expenditure in the explanation of national income. It was about 1954 when M1 and M2 were invented in that workshop. I still remember Friedman defining them fairly casually, saying let us call this M1 and call the other M2.

Was Alan Wallis's class in statistics done by many graduate students?

In Chicago, there was a committee on statistics. The committee was an informal Department of Statistics and Wallis was chairman. They taught students in economics, statistics, and the business school, maybe even sociology. That philosophy is not such a bad idea. Nowadays, economics departments often offer the first statistics course, but if you have a strong statistics department the first course can be taught there. In Chicago, there were two types of courses; one was less mathematical, which Alan Wallis taught. He started with the logic of hypothesis testing and inference and then developed it from there. Parallel to that there was a course that was more mathematical. Jimmie Savage and Bill Kruskal often taught that. I also had a course on linear models from Kruskal.

How many students would have been in the economics intake?

In Milton Friedman's first-year class there must have been about 45 to 50 people.

In 1951; I'm amazed at the number.

You mean because there were fewer people studying economics or because the departments were smaller?

I thought the departments were a good deal smaller. In the 22 years you've been there, has Princeton's intake changed?

Not that much. Maybe slightly. Now in Princeton we have something like 25 to 26 entering, and when I first started in 1970 it was in the low 20s. So there is a minor difference.

What was Chicago like in 1952? Were there people there who really impressed you and had a long-lasting effect on your research?

You mean the faculty. I think that several people did. I was in Chicago from 1951 to 1955. It was truly an exciting place, and I took courses from Koopmans, Marschak, and Houthakker. In fact, in 1953 or so, Debreu was there, except that he was not teaching. Hayek was also there, not as a Professor of Economics but as a Professor of the Committee on Social Thought. He ran a workshop on the Methodology of Social Science in 1952 and had speakers like Fermi and Savage. Fermi was talking about methodology in physics. Savage was talking about probability. Who else would be better to talk about those topics? People like Milton Friedman and Alan Wallis also attended the seminar. Of all the people, I think that Milton Friedman had the most influence on my own thinking. Later I wrote papers that were not in agreement with some of his views—I am not necessarily a monetarist, and some people do not even consider me a strict adherent of the Chicago school. I don't believe that perfect competition is as good a model as many Chicagoans think. However, I enjoyed Friedman's lectures and he taught me how to think.

It's interesting that you say how influenced you were by Friedman. You attended his course in price theory and yet you never seem to have done a lot of price-related or micro work during the rest of your career. So was it just the way he approached things that influenced you?

Yes, when one approaches a problem it is important to get at the essence of things, whether it is price theory or monetary theory or econometrics, so I try to see things simply. I probably learned it from Friedman.

Do you think he was a big influence on everyone there, or was this just your particular experience?

I think that in 1951 and 1952 he probably was the most influential figure in general. There were people who would study economic development with Schultz. Marschak was working on game theory. There were many stimulating people around, but I think the main intellectual force pushing the department was certainly Milton.

When you came to your doctoral dissertation, how did you select a topic?

Well, like many people you choose a subject in an area that you know something about. I had had a good course on the theory and econometrics of consumer demand from Houthakker. I think he was a leading figure. He had spent time in Cambridge, England. I enjoyed his course on consumer demand, and I knew durable goods was one of the outstanding topics, so I picked that.

At that time there was work being done in Europe by Dick Stone on the Stone-Rowe model of consumer durables. Were you familiar with that work, or was that work you didn't consider?

Well, we may have been doing it simultaneously. That paper was published in *Econometrica* the same year as my book – 1957. I finished my thesis in 1955 and the North Holland book came out in 1957. I think that the Stone-Rowe article in *Econometrica* was 1957. So I didn't know of their work.

Tell me about the work on durables.

There was one obvious idea that I had. The idea was that, as a first approximation, if you look at the stock of durable goods, you can just treat it like a nondurable. I told Houthakker, and he said, "Gregory I don't think it will work." But, anyway, I tried, so I regressed automobile stock on price and income. It looked good, even then. Of course, you lose all the short-term dynamics, but even that kind of a static model looked right, especially for the United States. To get dynamics, you adjust the stock to the desired stock, which is determined by some basic variables. This simple idea seems to be so natural. Now maybe somebody told me about it, but I don't think that I could cite a specific reference.

I presume permanent income was in the air? I'm surprised, then, that your first reaction wouldn't have been to say that the purchase of a stock of durables is a function of permanent income and get the dynamics in that way.

That's right, the idea of permanent income, or what Friedman used to call expected income, measured by a weighted average of past incomes, was being discussed. In my preliminary work, the income variable was measured income or current income. It was exactly the time when Friedman was doing his consumption function work, and I still remember the day when Friedman gave his seminar on it. It was exciting, but I didn't use the idea of permanent income at first. I just had automobile stock related to the "Commerce Department's income" and price. I presented it in Friedman's workshop and, of course, his criticism was that I had used the wrong income variable. So I said fine, let's try the permanent income variable. Then I had both incomes. I showed in my work that permanent income was a better variable for explaining automobile stocks, but current income is better for explaining purchases, which include savings.

After Chicago you went to MIT. What was it like in those days?

Well, MIT was interesting. Paul Samuelson and Bob Solow were there, among other influential economists.

This was the time Solow was doing his growth work.

Exactly. I think the first growth paper was 1956, and it was in 1955 that I showed up at MIT. I got to know Solow professionally. I saw him quite a bit in the department and at lunch. There was a Samuelson and Solow theory workshop that I attended. It was a different workshop from the kind that I had attended in Chicago. Friedman's money workshop was prepared, whereas the Solow/Samuelson workshop was not as prepared. I remember having lunch with a group including Samuelson and Solow and, knowing that after lunch they had this workshop, I would follow them to the class. As I was walking down, Paul would say to Bob, "Bob what should we talk about today?" He'd say, "How about uncertainty or expected utility or something? Why don't we talk about that?" Solow would say, "Okay." Then Samuelson would make a few comments about who started the basic idea and what's wrong with it. The whole thing was interesting. If you already know the subject and hear some of these interesting remarks, you learn something. But if you haven't read the articles, it is not easy to learn from such a workshop.

Possibly your most famous paper, on the Chow test, was written while you were at MIT. How did that come about?

It was in the spring of 1958 that I got a letter from Al Harberger. After I had left Chicago, a number of people were doing work related to consumer durables. Al Harberger's workshop, of which I was a member, produced several dissertations. So he decided to get a volume out and asked me to make a contribution. But by that time my automobile book [2] was already in print, and I had to write something different, so I decided to update it to see how the demand functions I had estimated stood up with data from 1954 to 1957. I finished my work in 1955, but the data were only to the end of 1953. I had these four extra years, and I had to devise some kind of a test for constancy of the parameters. The first problem to solve was when there is insufficient data to run a separate regression, so I talked about prediction in the paper. I also tried to relate prediction to the analysis of variance. There were three parts to my paper [9]. One is when there are insufficient observations, one is to test equality of subsets of coefficients, and the third, which is more generally known, is just to test the entire set when you have sufficient information to group the data into two groups. In my paper, I explicitly said that the third kind of test, which people often call the Chow test, is in some texts. Some people in England, including Andrew Harvey and perhaps David Hendry, would restrict the term "Chow test" only to the case where you do not have sufficient information. Other people just use the term "Chow test" even for the case where you have sufficient observations for testing the equality of these coefficients. Of course, each one is an F-test, whether people call it a Chow test or not. I am neutral on this. In fact, I was talking to a colleague of yours yesterday who felt it was not necessarily bad to have a special name for a special F-test. I'm happy I brought this topic to the attention of econometricians.

Given your interest in simultaneous models, I am surprised that you never seemed to work on a Chow test for that case. Is this correct, or did you run into difficulties?

I thought about it somewhat, but I was also interested in other things about simultaneous equations and testing stability is just one aspect of that. I could get results on some other things more easily. Often in research you publish what you can get.

Your interest in simultaneous equation problems produced a number of papers comparing estimators that were required reading when I was a graduate student [11,19]. What led you into this area? In retrospect do you feel that the research in the area of simultaneous equations was of great value to econometrics? I am particularly interested in the fact that you wrote on the computation of FIML so you must have felt that it was an important estimator.

The answer is very simple as to why I worked on that area. It happened to be the main subject of econometrics at that time, so it was natural to be a part of the mainstream. Why FIML? That also is very easy to answer. At that time, maximum likelihood was the dominant idea in estimation. Now I wasn't that extreme; there were other methods besides maximum likelihood. But when I studied statistics, Fisher's influence was strong. Since a lot of people, including myself, were not professional statisticians, we just took whatever was well accepted. So maximum likelihood became a natural thing to work with.

When I was a student, one of the things that struck me almost straight away was that FIML seemed to make too many demands. FIML always seemed to have so many disadvantages in terms of having to get the correct specification of every equation as to make me feel that I'd never want to use it.

No, I wouldn't agree with that. There's a trade-off between specification error and the size of models. I agree with you about misspecification errors, but you still want to be parsimonious because of finite samples. People justify dropping a regression variable even if its coefficient is not zero; if it's small enough, you should drop it. I was also a believer in small models. I didn't suggest that people should do FIML for 5,000 equation models, or even 50. The models I happened to work with were small ones. I took macroeconomics literally. If I teach a Keynesian system, multiplier accelerator, IS-LM, I should implement it econometrically, just as it is. Some people say that such models are only for classroom use. The model that you teach the macro students should not be the same model that you estimate econometrically. That's a philosophy that I did not adhere to. If the models are small, why not FIML?

So you would say that your interest in FIML is very much connected with the idea that models should be small and not necessarily large?

I think that there's virtue in building small models, although I would not say that large models are necessarily bad. I think that small models are useful, whereas some people don't think so.

Would you actually teach simultaneous equations anymore?

Oh yes, I would. I think the subject deserves some attention. The idea that the economy can be conceived of as a system of simultaneous equations is still a useful idea. It also has two related ideas: the idea of identification, and the distinction between the structure and the reduced form. The Lucas critique, for example, essentially makes the distinction among the parameters that are stable, the structural parameters, and the reduced form parameters, which are affected by a policy change.

So you therefore remain convinced that this is still the framework we should be working with, despite the development of VARs?

Well, take real-business cycle models. If I were to work on real-business cycle models that have "deep structural parameters," I would try to estimate them by traditional methods. Other people try to guess them by calibration. They can play their game. I don't want to get into an argument with them. I would estimate the parameters in a traditional way. If you take the multiplier/accelerator model, it also has a VAR representation except that it may impose some kind of prior restrictions. A VAR model also has its own parameter restrictions.

So after MIT, you went to Cornell?

Yes, Cornell, was my alma mater and offered me tenure.

Would you say that you were disappointed with Cornell?

Yes, the atmosphere was quiet and less exciting than Cambridge. Then I got an offer from Ralph Gomory to join IBM, which I was willing to try out for a year. I took a year's leave from Cornell, but the atmosphere at IBM was so good to me that, after 2 or 3 months, I decided to resign from Cornell. It was like being a professor on leave. You had no duties, except to write some papers. I was fairly productive at IBM. I did work on simultaneous equations, a paper on the demand for money [13], the demand for computers [18], multiplier accelerator model [17], and started working on dynamic economics and optimal control.

That must have been an unusual move even then. Today, a lot of students tend to think that if they don't get an academic job, they are something of a failure. What sort of work did you do for IBM?

My main duty was to do basic research, but often there were company problems involving economics and econometrics. Later on, I also consulted with the Vice President for Finance and Planning on corporate planning problems. I gave them some economic views about these problems. One topic was the demand for computers [18]. They were interested in that, and my work was used as part of their forecasting process.

Your paper on the short- and long-run demands for money appeared while you were at IBM [13]. Given that you must have been exposed to Friedman's classic work on money demand while at Chicago, was that paper really a product of an earlier period?

No. Although I had been exposed to this topic, having attended Friedman's workshop on money, I picked up the subject some years later. I left Chicago in 1955. We're talking about 8 years later, around 1963 or 1964.

In my own work I have had fun going back and looking at how my applied work has fared over the years (some others have had fun too!). Have you ever gone back to see how your automobile and computer models have stood up to developments? Do you think it's a good idea for one to go back over past work?

Yes, I have examined how my automobile and computer models stood up to later developments. In the case of automobiles, the test of stability of demand functions by using later observations led to a paper in Harberger's volume on the *Demand for Durable Goods* [8]. In that chapter, not only did I find the new observations from 1954 to 1957 consistent with the previously estimated demand function, but I also made a 10-year forecast from 1958 to 1968, forecasting automobile sales in 1968 to be in the order of 8.5 to 9 million cars, compared to around 6 million in the late 1950's. This turned out to be correct 10 years later. I recollect this successful 10-year forecast with pleasure. My work on the demand for computers was used by colleagues at IBM to do forecasting, and the model stood up quite well for many years. In particular, I supervised a senior thesis at Princeton by Bryan Miller entitled "An Extension of the Chow Study on Technological Change and the Demand for Computers." Miller's work updated my equation from 1966 to 1973 and found that my equation remained valid. I think that it is a good idea to reexamine one's empirical work. By the way, my multiplier-accelerator model for the macro U.S. economy [17] stood up quite well into the late 1980s. Part of this result is contained in a 1993 article in *Review of Economics and Statistics* [142].

Looking at an academic history, it occurred to me to ask about your students. Which of them have gone on to become well-known figures?

Andy Abel did a Bachelor's thesis with me on optimal control, and Ed Burmeister did his Master's thesis with me. For a long period I was in a business school, or at IBM, so I didn't have too many Ph.D. students. Soon after I joined Princeton in 1970, I ran a summer workshop on optimal control, which John Taylor attended, and he was then a graduate student at Stanford.

I was particularly interested in students who you supervised doctoral dissertations for.

There were Ken Garbade and Carlo Carraro.

So you didn't supervise people from Princeton like Nick Kiefer and James McKinnon?

No. I was not their main supervisor. They both studied econometrics with me and we interacted, but I wasn't their main supervisor. During that period my main interests were dynamics and optimal control.

Are you disappointed that there were so few people?

I am in a sense, but colleagues like yourself and Ray Rair got interested in the subject. So did many people in the profession.

What do you put it down to? One of the reasons I was interested in going to Princeton in 1973 was because it seemed to me that the work you were doing was very interesting material and it was really right at the edge. In retrospect, it seems odd that there weren't a lot of students doing theses in this area. Was it too difficult for them?

It's hard for me to answer that. Some people can attract students. The second-year or third-year students might find me somewhat threatening, as I'm sometimes quite critical.

Are there any areas that you regret not working on—that you could have worked on but for one reason or another you put off?

No. If I think there is an interesting topic in economics that I haven't worked on sufficiently, I can work on it now.

When I was recently cleaning up my office, I found an old referee's report. This was actually a report on one of the very first papers on rational expectations. The author was working with a demand and supply example and he was looking for a maximum likelihood estimator, or something like that, and he finished his paper saying that he couldn't find a two-stage least-squares analogue. My report said you could. All you had to do was write down the expected value and solve for the expectations. It's a function of the reduced form and then you go back and substitute that in. I'd forgotten I'd ever done this referee's report. At the time I thought rational expectations was useless. I couldn't see why anyone was interested in it.

Now I appreciate your question. Have I missed opportunities I could have taken up? If I think for 2 days maybe I could come up with one. But I'm not a person to regret. I look forward. It seems to me I cannot blame myself for not getting into hot topics because, whatever I did, I was interested in it.

Let's take the example of nonparametrics. That's become a hot field in recent years and I don't think you've ever written anything on nonparametrics.

No. I have never written anything on it, but I don't regret it because I don't have a comparative advantage in nonparametrics.

One area you did work on was spectral methods [24,25]. Thinking back on your training, there would not appear to have been a lot of courses that had any mention of spectral analysis. Did you teach yourself all of this, and what actually made you look into that literature?

That is also a case where I do econometric theory after being stimulated by empirical work. After I built the multiplier-accelerator model, I wanted to see whether or not that model explained business cycles. Part of the definition of a business cycle was its spectral properties. I got Whittle's book and I also read a book by Quenouille about multivariate time series. So I learned the subject myself. I believe that spectral methods are an important part of stochastic dynamic economics.

I'm not so clear on that. In what sense are they useful? It seemed to me that these methods never really made much of an impact in econometrics, except as a pedagogical device.

Okay, yesterday in your seminar you pointed out that Mandelbrot drew this diagram of cumulated average squares of a series and, by looking at the diagram, you can spot that the series is not stationary. Now, in a way, spectral methods do the same thing. People ask questions like, What is the typical spectral shape of some economic time series? By comparing the spectral properties deduced from an econometric model to those estimated directly from time-series data, one finds out how good the model is. Also, spectral methods are useful for inference in the time domain.

Yes, I can see that potential, but I am saying that, in practice, it doesn't seem to me that people ever used it. Suppose I asked you the question of how many economists, having looked at a spectrum, would throw away the NBERs reference cycles and cycle dates if they could not in fact find the 3- to 4-year peak in the spectrum?

There are no inconsistencies between the two. When the NBER people say that the average length of a cycle happened to be so many years, it doesn't mean that there is a peak in the spectral density in that range. These are two different statements. Suppose annual GNP is a random walk. So, in any year there is a probability of one-half of there being an increase and one-half of being a decrease. Then, if I define cycle length as the mean distance between peaks, you get an average length of 4 years, because the probability of a peak in any one year is the probability of an increase followed by a decrease, or one-quarter. But that doesn't mean it has a peak in the spectrum corresponding to a frequency of one-quarter.

Okay, you moved to Princeton from IBM. Why? Had IBM changed, or did you think that this was a great opportunity to get back to academia?

In fact, it was a hard choice. When Dick Quandt called and asked me to consider the position at Princeton, my honest answer would have been "no thank you." But to be polite I said instead, "Well Dick, why don't I call you back in a week from today?" When Friday came, I had lunch with Alan Hoffman. He worked on linear programming and graph theory. I was going to call Quandt in the afternoon to say thank you very much, I have thought about it and I am not interested. I told Hoffman this plan at lunch, and he said, "You shouldn't do that. At least you should go down and take a look before you say no." So he talked me into it. I was so happy at IBM I didn't think I was going to come. But then later on, I came here and started thinking it was a very attractive offer. I thought, well, maybe IBM is good now, but eventually I cannot guarantee this kind of good life will last forever. I also knew that, when I get older, it will probably be better to be a professor than to do research full time. So I ended up here, and I was honored to succeed Oskar Morgenstern, as the Director of the Econometric Research Program.

Well that leads us into the control theory interests. How did that come about, and why did you think it was important?

It was also empirically motivated. The dynamic analysis of econometric models and the application of such models for policy analysis and design seem to follow naturally from the estimation and testing of such models. I was playing around with models and looking at their spectral properties, and this led me to ask whether or not such models could be useful for policy analysis? Optimal control is a set of indispensable tools for dynamic analysis and policy applications of econometric models. I wrote some papers on how to derive optimal control rules, partly to try to understand the subject, especially by using methods that other economists and I were familiar with [28,32,36,39]. When I wrote a 1970 paper [28] applying the familiar method of Lagrange multipliers to solve the optimal control problem, I had read Peter Whittle's *Prediction and Regulation by Linear Least Square Methods*, which applied spectral methods to solve the optimal control problem in the univariate case. Later I read Aoki's work on optimization of stochastic systems. My exposition in 1972 in *International Economic Review* [32] might have added something to the control theory literature.

There remains a critical question of what the role of control theory is. Three arguments have been advanced against it: time inconsistency, the Lucas critique, and the fact that our models are likely to be misspecified. You don't have these in physical systems. I'm interested in your reaction to these objections.

The possibility that government policy may affect certain parameters in some econometric models has been recognized in the economics profession for a long time – for example, by Marshak in his paper "Economic Measurements for Policy and Prediction" in the volume edited by Hood and Koopmans, *Studies in Econometric Methods*. If the income tax rate is changed, the parameters in the reduced form equation of a simplified Keynesian model explaining consumption by exogenous investment would be changed. Knowledge of the structural parameters helps one infer how the reduced form parameters would be changed. This point was recognized by Robert Lucas in his well-known critique on econometric policy evaluation. Lucas was making Marshak's point in a dynamic context. The basic statistical methods of estimation such as the method of maximum likelihood remain valid when applied to the estimation of the parameters of "structural" equations introduced by the Cowles Commission. You don't change the method – you just change your way of application. In the same way, optimal control methods are valid in studying the effects of governmental policy on the economy when some "reduced form" parameters in certain behavioral equations such as the consumption function or the investment function may change. It is by now familiar that these econometric equations may be considered as the optimal control rules derived from solution of dynamic optimization problems on the part of economic agents. Government policy can be treated as a part of the environment facing economic agents. When the environment changes, the resulting optimum policy rule will change. But if you know the "structural" parameters such as the parameters of the consumers' preference functions or the investors' production functions, you can use the techniques of optimal control to derive the new behavioral equations after governmental policy changes [78,81]. Optimal control remains the major technique for policy analysis.

What about time inconsistency? Do you think of that as a serious problem?

One implication of time inconsistency is that you cannot apply dynamic programming, but you can extend optimal control techniques in the context of dynamic games [93].

Control methods are sometimes said to presume too much precision of knowledge. I have some sympathy with this viewpoint; as you say in your paper with Moore [31], "Fairly different lag structures can fit the data almost equally well," and this seems to be a problem for optimal control exercises, as I have the impression that these are very sensitive to the dynamic specification.

Your question concerns the lack of precision and possible misspecification of econometric models. I have written on both problems as they affect the application of optimal control techniques [61,73,103]. Some of the suggestions made appear to be valid today.

When one sits down and looks at these three objections, you can sometimes get overwhelmed by them and say, well, the difficulties are so serious that this may be a bad tack for us to be taking.

Well, no. The use of econometric models and optimization methods for economic policy is not a simple matter. But I think we can do better than those who do not know these tools. Who is more qualified to study macroeconomic stabilization policy? Can you tell me an alternative? You can criticize a doctor; sometimes the patient dies, and we don't have the solutions to all medical problems, but I would rather get an imperfect doctor than to consult randomly when I am sick.

I don't think it is quite that simple. Bill Phillips, when I was a student of his back in the late 1960's, used to talk about this issue quite a lot. Bill's line was that the economy was run pretty well for 30 years, from post-war up to the late 1960s, and the policymakers didn't use any control theory then. Nevertheless, they had an intuitive understanding of how it worked and how to make adjustments. One way of thinking about what these people were doing was that they were working in the old classical control framework of integral, proportional, derivative controls, setting up feedback rules and automatic stabilizers. They didn't feel that going to an optimal control over a classical control, if you want to put it in those terms, had a lot of benefit. And the main reason for this attitude was that they felt that the imperfect understanding of the dynamics of the economy was such that it was going to be very difficult to exploit the control techniques. A current example of this problem is that financial deregulation in many economies has meant that the impact of interest rate movements on investment is hard to predict and has led to some policy mistakes. How do you get around this problem with optimal control?

In this particular case, you should use some Bayesian way to impose the effect of interest rates on investment, as you don't have past observations. Going back to an earlier point you made about simple rules, they may in fact be robust rules. I think it would be useful for people to go through optimal control exercises and to compare the relative performance of these robust rules with optimal rules in some econometric models. You can vary the model and see whether or not these rules that you mention stand out well.

Do you think though that this is just a low point for interest in optimal control and that it is likely to change again?

It is not a low point. I think it is a high point. Optimal control is widely applied to the study of economic behavior; it is part of econometrics and part of economics. You might not use it in a traditional way. It has found new applications.

I get the impression, certainly in America—not so much in Europe, as Europe is different—that people lost interest in models and control methods, and I wondered if you have any thoughts on the matter.

I have told people that econometric models were more seriously used in Taiwan for macropolicy purposes than in the United States. In Taiwan there is some office that corresponds to our Office of Management and Budget. In preparing government budget proposals, this office has an econometric model and it would use simulation and sometimes optimal control as a way of examining the consequences of different government policies. So why do they do this in Taiwan and in some European countries? Perhaps it is just not fashionable today in American academic circles to apply optimal control methods and econometric models to study macroeconomic policy in the traditional way.

You have actually done quite a bit of applied work as well as theory. Do you have a particular philosophy about how you would go about it?

My interest in applied work has been influenced by the Chicago environment. I tend to start with an applied problem and do a lot, I mean a lot, of theorizing before looking at the data. I remember pacing the floor somewhere in the Social Science Building of the University of Chicago for weeks and months thinking about an appropriate theory for the demand for consumer durables before looking at the data for automobiles. I also remember pacing the floor somewhere in the IBM Research Center in Yorktown Heights for months before writing down a simple four-equation model for the U.S. macroeconomy or before writing down one equation for the demand for computers. I want the macro model to be close to "theory," because if macroeconomists write down equations, like the consumption function, the investment function, and the demand for money, why don't we find out whether or not such aggregate relationships stand up? Before I see the data, I feel I already understand them or at least ought to understand them. An opposite approach used by others is to start by examining the data in many different ways until finally a model emerges. Let a hundred flowers bloom. There are advantages and disadvantages to both approaches.

To what extent do you think that attitude was formed out of the days when it wasn't easy to do a lot of applied work because of problems with computers. In the 1950's for example, or even the 1960's, running a regression was a big thing, and running a lot of them was a real pain, because it took a day to get the output back. In those days, people were very reluctant to let the data speak for itself because they felt that doing so meant it would take them forever to do the work, and it was better to think about things theoretically first. I get the impression that, with the development of PCs, this style of work is probably disappearing.

What style of work is disappearing?

Well, suppose I want to look at data now. I can just sit down on the PC and within seconds I can have a graph of what it looks like. I can run a couple of regressions to see what the relationships are. I can fit time trends. I can do fantastic amounts of work within an hour. In a sense, it is the old Sherlock Holmes problem: Do you wait until all the evidence is in before you begin theorizing, or do you theorize and then gather the evidence?

I think your point is probably valid. Computers are now readily available to process a lot of data in a lot of different ways. That probably affects the way we do research. In my own case, I think I was influenced by my time in Chicago. To what extent it was my own idea or my colleagues, or my fellow students, or my professors, I don't know. Chicago people take theory seriously, and so I would not want to go to the data without knowing the theory. Now, maybe I am at one extreme of the Chicago tradition. Other people may do more data mining.

So you haven't shortened the period of time spent on introspection about theories?

It depends on what you want to do.

Take the China model for example. You've got this book on the Chinese economy. How long did it take to develop that model?

I had a simple view of the Chinese macroeconomy that came entirely from theory. The simple piece that I wrote [105] was theory first, data later. Let me tell you the way I wrote that paper. I was writing this textbook on the Chinese economy and there is a chapter on macroeconomics. I wrote the book so students in China could understand some economics tools. I started the chapter with Harrod-Domar, as I think every student should know Harrod-Domar. I looked at some Chinese data and asked whether or not it fit. Now of course Harrod-Domar is a growth model with no fluctuations. To have fluctuations you can incorporate some lags. Introducing lags you get consumption as a function of lagged consumption and income. You have a production function relating capital stock and the level of income. If you difference that, it becomes the relation between investment and the first difference in income. That's a multiplier-accelerator model. So having come up with that, I examined how well this model fits the Chinese data. So I wasn't mining the data first. I started with a theory.

So when you do this do you get surprises?

Well first of all, in the Chinese case, if the model didn't work out I could always have the explanation that it was a Western model. But the surprising thing was that the model performed beautifully. One thing I discovered is the empirical validity of the accelerator model. I have written a number of papers on it. It's in my work on automobile demand, the multiplier-accelerator model, and business cycles. I think that it is a basic economic law. In every paper I have done explaining capital investment or purchase of durables, if I regress that variable on current income and lagged income I get coefficients of opposite signs but equal magnitude.

Okay. On teaching, when you went from IBM to Princeton, one of the things that you were obviously going to have to do was to teach and, except for lectures at Columbia, you had been out of that for some time. Did you find that useful for your research in any way, or was that just something you had to do?

Well, the optimal level of teaching is not zero. If you can decide on the subject matter you want to teach, and select the students, then I think everyone wants to do some teaching. But sometimes you are asked to teach a subject in which you are not that interested. That is not too pleasant. Some amount of teaching is good. When I have research problems and tentative solutions, I often force myself to lecture on the subject in order to obtain suggestions and criticisms from the students and to produce some useful results.

Have you ever had the case where a research idea came out of teaching?

One example that comes to mind is my note [54] on Theil's "BLUS Residuals." I had to prepare lecture notes using Theil's book. It was a popular book. Then there was this chapter on BLUS residuals. Remember it? I had to lecture on it. I knew the students wouldn't like it if I just followed his exposition. So I tried to prepare a simple lecture, and I think my note added a little bit to the understanding of the subject.

In econometrics today, I have a feeling that the technology we are seeing now is really concerned with dotting the *i*'s and crossing the *t*'s. It is not technology that is appropriate to the level of problems we want to analyze. Do we really gain a tremendous amount out of a lot of our young people knowing how to do those sorts of proofs in great detail?

I am somewhat sympathetic toward current development in econometric theory. I think this is a natural development, as the science of economics in general is becoming more technical. Such a criticism was made when the technical levels were lower. I am not critical of this development because important and relevant research is always a small fraction of total research whether the irrelevant work is technical or not. I do not object to the increased abstraction of the current work as my tolerant predecessors did not object to my more technical work. There are always people dotting the *i*'s and crossing the *t*'s. They were there before and they are there now. They will be there in the future. The same kind of comments were made about the Cowles Commission. There are always some people doing mainstream and important research. But there are always people working on the periphery and dotting the *i*'s. So I am not so sure today we are worse than before.

Okay, the last question: If you were a young man beginning your career again, what sort of areas in econometrics would you concentrate on, and what sort of training would you need?

It is easy to say something about training. I think more mathematics would be better. As for research areas, one has to follow what excites him or her at the moment.

The point I am getting at is what would you think of as the exciting areas in econometrics today?

Today, perhaps we don't have any major topics of research in econometrics that get a lot of people excited. I mean, for example, something like the Cowles Commission program on simultaneous equations or optimal control. At one point these were major themes. Today I don't know of any areas that are comparable.

Do you think that this means we are going to see very few people come into econometrics because there isn't this big exciting climate? We saw a lot of people come in who were excited by the vision of large-scale models and simultaneous equations. I get the impression that a lot of people went into it for that reason in the 1960's and then, after that died away, we have found it very difficult to get people into econometrics.

That was a big growth period. Even without simultaneous equations, it revolved around the application of mathematical statistics to economics. Now econometrics has become an established field. It will stay with us for a long time even though there may be few radical developments. People keep on writing papers and are interested in it.

The real question is whether or not you get stagnation after maturity, as Steindl hypothesized.

No, it seems to me that in the foreseeable future quantitative methods will be used for analyzing data. I don't think you can avoid it. So people have to do econometrics. There are bound to be new ideas, better than what is already known, so there is always progress. The field is there. It's an important field, as far as I know. There is no other way unless you have a direct access to God, who tells you the truth about economics. As we don't have that, econometrics remains.

BIBLIOGRAPHY OF GREGORY C. CHOW

1956

1. The demand for automobiles in the United States. *Econometrica* 24 (July), 342.

1957

2. *Demand for Automobiles in the United States: A Study in Consumer Durables.* Amsterdam: North-Holland Publishing Company (Spanish translation, 1965).
3. Review of A.M. Tuttle: Elementary business and economic statistics. *The Journal of Business* 30 (October), 286–287.

1958

4. The formation of stock prices. *Econometrica* 26 (October), 604–605.
5. Statement of Gregory Chow. In *Administered Prices: Hearing before the Subcommittee on Antitrust and Monopoly of the Committee on the Judiciary, United States Senate Eighty-Fifth Congress, Second Session*, pp. 3167–3195. Washington, D.C.: U.S. Government Printing Office.

1959

6. Review of J. Morice: La demande d'automobiles en France. *Econometrica* 27 (July), 528–530.
7. The selection of variates for use in prediction: A generalization of Hotelling's solution. Presented at the Econometric Society Meetings in Washington, D.C., December. In L. Klein, M. Nerlove, & S.C. Tsiang (eds.), *Quantitative Economics and Developments*, pp. 105–114. New York: Academic Press, 1980.

1960

8. Statistical demand functions for automobiles and their use for forecasting. In A.C. Harberger (ed.), *The Demand for Durable Goods*, pp. 147–178. Chicago: University of Chicago Press.
9. Tests of equality between sets of coefficients in two linear regressions. *Econometrica* 28 (July), 591–605.

1961

10. Review of William Warntz: Toward a geography of price. *Journal of the American Statistical Association* 56 (March), 209–210.

1964

11. A comparison of alternative estimators for simultaneous equations. *Econometrica* 32 (October), 532–553.

1965

12. Review of A.D. Bains: The growth of television ownership in the United Kingdom. *Econometrica* 33 (July), 657–659.

1966

13. On the long-run and short-run demand for money. *The Journal of Political Economy* 74 (April), 111–131.
14. A theorem on least squares and vector correlation in multivariate linear regression. *The Journal of the American Statistical Association* 61 (June), 413–414.

1967

15. An alternative proof of Hannan's theorem in canonical correlation and multiple equation systems. *Econometrica* 35, 139–142.
16. Review of D.S. Huang: A microanalytic model of automobile purchase. *Econometrica* 35 (January), 164–165.
17. Multiplier, accelerator, and liquidity preference in the determination of national income in the United States. *The Review of Economics and Statistics* XLIV (February), 1–15. Also in M.G. Mueller (ed.), *Readings in Macroeconomics*, pp. 411–429. New York: Holt, Rinehart and Winston, 1971.
18. Technological change and the demand for computers. *American Economic Review* LVII (December), 1118–1130.

1968

19. Two methods of computing full-information maximum likelihood estimates in simultaneous stochastic equations. *International Economic Review* 9 (February), 100–112.
20. The acceleration principle and the nature of business cycles. *Quarterly Journal of Economics* LXXXII (August), 403–418.

21. Long-run and short-run demand for money: Reply and further note. *Journal of Political Economy* 76 (November/December), 1240–1243.
22. Statement of G.C. Chow. In *Compendium on Monetary Policy Guidelines and Federal Reserve Structure: Pursuant to H.R. 11*, pp. 106–109. Washington, D.C.: U.S. Government Printing Office, December.

1969

23. Review of Gerhard Tintner: Methodology of mathematical economics and econometrics. *Zentralblatt für Mathematik* 164 (S.1-3000, May), p. 203.
24. Spectral properties of non-stationary systems of linear stochastic difference equations (co-authored with R.E. Levitan). *Journal of the American Statistical Association* 64 (June), 581–590.
25. Nature of business cycles implicit in a linear economic model (co-authored with R.E. Levitan). *Quarterly Journal of Economics* LXXXIII (August), 504–517.
26. Note on the estimation of long-run relationships in stock adjustment models. *Journal of Political Economy* 77 (November/December), 932–936.

1970

27. Friedman on money: A review article. *Journal of Finance* 25 (June), 687–689.
28. Optimal stochastic control of linear economic systems. *Journal of Money, Credit and Banking* 2, 291–302.

1971

29. Review of L.R. Klein, M.K. Evans and M. Hartley: Econometric gaming: A kit for computer analysis of macroeconomic models. *Econometrica* 39 (September), 868.
30. Best linear unbiased interpolation, distribution, and extrapolation of time series by related series (co-authored with An-loh Lin). *Review of Economics and Statistics* LII (November), 372–375.

1972

31. An econometric model of business cycles (co-authored with G.H. Moore). IBM Research Center, January 1969. Also in B.G. Hickman (ed.), *Econometric Models of Cyclical Behavior*, Vol. 2, pp. 739–812. New York: Columbia University Press.
32. Optimal control of linear econometric systems with finite time horizon. *International Economic Review* 13 (February), 16–25.
33. Review of Aaron Strauss: An introduction to optimal control theory. *Econometrica* 40 (March), 408–409.
34. Optimal Control Program: User's Guide (co-authored with D.R. Chapman). Research memorandum 141, Princeton University, Econometric Research Program, May.
35. Review of Phoebus Dhrymes: Econometrics: Statistical foundations and applications. *Econometrica* 40 (July), 786–787.
36. How much could be gained by optimal stochastic control policies. *Annals of Economic and Social Measurement* 1 (October), 391–406.
37. Introduction to stochastic control theory and economic systems (co-authored with Michael Athans). *Annals of Economic and Social Measurement* 1 (October), 375–383.

1973

38. Multiperiod predictions from stochastic difference equations by Bayesian methods. *Econometrica* 41, 109–118. Also in Fienberg and Zellner (eds.), *Studies in Bayesian Econometrics and Statistics*, pp. 313–324. Amsterdam: North-Holland Publishing Company, 1975.

39. Effects of uncertainty on optimal control policies. *International Economic Review* 14 (October), 631-644.
40. Problems of economic policy from the viewpoint of optimal control. *American Economic Review* LXIII (December), 825-837.
41. On the computation of full-information maximum likelihood estimates for nonlinear equations systems. *Review of Economics and Statistics* LV (February).

1974

42. Introduction to selected papers from the Second NBER Stochastic Control Conference (coauthored with Michael Athans). *Annals of Economic and Social Measurement* 3, 1-9.
43. Report on a Joint Study and Discussion of the Future Fiscal and Economic Policies of Taiwan. Jointly prepared and submitted by six academicians of Academia Sinica and The Central Bank, Republic of China, August.
44. A family of estimators for simultaneous equation systems. *International Economic Review* 15 (October), 654-666.
45. Review of R.S. Pindyck: Optimal planning for economic stabilization: The application of control theory to stabilization policy. *Journal of Econometrics* 2, 195-198.
46. Identification and estimation in econometric systems: A survey. *IEEE Transactions on Automatic Control* AC-19 (December), 855-862.

1975

47. Maximum likelihood estimation of linear equation systems with autoregressive residuals (coauthored with R.C. Fair). *Annals of Economic and Social Measurement* 4 (January), 17-28.
48. *Analysis and Control of Dynamic Economic Systems.* New York: John Wiley & Sons (Beijing: Friendship Publishing Corporation, edition in Chinese, 1984).
49. Introduction to stochastic control applications. *Annals of Economic and Social Measurement* 4 (April), 207-214.
50. A solution to optimal control of linear systems with unknown parameters. *Review of Economics and Statistics* 57 (August), 338-345.
51. Introduction to Selected Papers from the Third NBER Stochastic Control Conference. Research memorandum 172, Princeton University, Econometric Research Program, February.
52. Review of K.P. Vishwakarma: Macro-economic regulation. *IEEE Transactions on Automatic Control* AC-20 (October), 721-722.

1976

53. Review of R.P. Smith: Consumer demand for cars in the USA. *Journal of Economic Literature* 14 (March), 114-115.
54. A note on the derivation of Theil's BLUS residuals. *Econometrica* 44 (May), 609-610.
55. Control methods for macro-economic policy analysis. *American Economic Review* 66 (May), 340-345.
56. The control of nonlinear econometric systems with unknown parameters. *Econometrica* 44 (July), 609-610.
57. Best linear unbiased estimation of missing observations in an economic time series (coauthored with An-loh Lin). *Journal of the American Statistical Association* 7 (September), 719-721.
58. An approach to the feedback control of nonlinear econometric systems. *Annals of Economic and Social Measurement* 5 (Summer), 297-309.

59. Discussion of population and economic development in Taiwan. In *Conference on Population and Economic Development in Taiwan*, pp. 555–556, 651–654, 675–677. Republic of China: The Institute of Economics, Academia Sinica.
60. Review of G. Fromm and L.R. Klein, eds.: The Brookings model: Perspective and recent developments. *Journal of the American Statistical Association* 71 (September), 771.

1977

61. Usefulness of imperfect models for the formulation of stabilization policies. *Annals of Economic and Social Measurement* 6 (Spring), 175–188.
62. Review of M. Aoki: Optimal control and system theory in dynamic economic analysis. *Journal of Econometrics* 6, 143–144.
63. A Reformulation of Simultaneous Equations Models for Markets in Disequilibrium. Research memorandum 213, Princeton University, Econometric Research Program, August.
64. Regression with One-Sided Errors in the Dependent Variable. Research memorandum 214, Princeton University, Econometric Research Program, August.
65. The computer and economics. *Proceedings of the American Philosophical Society* 121 (September), 350–354.

1978

66. Evaluation of macroeconomic policies by stochastic control techniques. *International Economic Review* 19 (June), 311–319.
67. Comment on "A time series analysis of seasonality in econometric models" by Charles Plosser. In Arnold Zellner (ed.), *Seasonal Analysis of Econometric Time Series*, pp. 398–401. Washington, D.C.: U.S. Department of Commerce, Bureau of the Census.
68. The control of large-scale nonlinear econometric systems (co-authored with S. Bernstein-Megdal). *IEEE Transactions on Automatic Control* AC-23 (April), 344–349.
69. An econometric definition of the inflation-unemployment trade-off (co-authored with S. Bernstein-Megdal). *American Economic Review* 68 (June), 446–453.
70. Are econometric models useful for forecasting? *Journal of Business* 51 (October), 565–568.
71. The Estimation of Total Investable Resources. Research memorandum 236, Princeton University, Econometric Research Program, October.
72. *Economic Planning and Efficient Utilization of Resources* (co-authored in Chinese with S.C. Tsiang, M.H. Hsing, John Fei, & Anthony Koo). Taiwan: Economic Planning Council.

1979

73. Effective use of econometric models in macroeconomic policy formulation. In S. Holly, B. Rustem, & M. Zarrop (eds.), *Optimal Control for Econometric Models*, pp. 31–39. New York: St. Martin's Press.
74. Optimal control of stochastic differential equation systems. *Journal of Economic Dynamics and Control* 1 (May), 143–175.
75. The Economy of the People's Republic of China: Past Performance and Future Prospects. Research memorandum 257, Princeton University, Econometric Research Program, December.
76. Comments on four papers concerning the factors affecting the changes in income distribution in Taiwan (in Chinese). In *Proceedings of the Conference on Income Distribution in Taiwan*, pp. 593–595. Taipei: The Institute of Economics, Academia Sinica.

1980

77. Comparison of econometric models by optimal control techniques. In J. Kmenta & J. Ramsey (eds.), *Evaluation of Econometric Models*, pp. 229–243. New York: Academic Press.

78. Econometric policy evaluation and optimization under rational expectations. *Journal of Economic Dynamics and Control* 2 (February), 47–59.
79. Review of *Practical Experiences with Modelling and Forecasting Time Series* by Gwilym M. Jenkins. *Journal of Economic Dynamics and Control* 2 (June), 209–212.
80. Estimation of rational expectations models. *Journal of Economic Dynamics and Control* 2 (August), 241–255.
81. Estimation and optimal control of econometric models under rational expectations. In R. Lucas & T. Sargent (eds.), *Rational Expectations and Econometric Practice*, pp. 681–689. Minneapolis: University of Minnesota Press.
82. Ten economic problems facing the People's Republic of China (in Chinese). *China Times* (Taipei, Taiwan), 4 February 1980, p. 2.

1981

83. A model of Chinese national income determination. *Journal of Political Economy* 93 (August), 782–792.
84. Optimal control of nonlinear systems program: User's guide (co-authored with E.H. Butters). In G.C. Chow, *Econometric Analysis by Control Methods*, pp. 57–84. New York: John Wiley & Sons.
85. Evaluation of econometric models by decomposition and aggregation. In J. Kmenta & J. Ramsey (eds.), *Large-Scale Macro-Economic Models: Theory and Practice*, pp. 423–444. Amsterdam: North-Holland Publishing Company.
86. Selection of econometric models by the information criterion. In G.E. Charatsis (ed.), *Proceedings of the Econometric Society European Meeting 1979*, pp. 199–214. Amsterdam: North-Holland Publishing Company.
87. On the control of structural models: Comment. *Journal of Econometrics* 15, 25–28.
88. A comparison of the information and posterior probability criteria for model selection. *Journal of Econometrics* 16, 21–33.
89. Estimation and control of rational expectations models. *American Economic Review, Papers and Proceedings* 71 (May), 211–216.
90. Has government policy contributed to economic instability? (co-authored with S. Heller). In G.C. Chow (ed.), *Econometric Analysis by Control Methods*, pp. 131–148. New York: John Wiley & Sons.
91. Econometric analysis of Soviet economic planning by optimal control (co-authored with D.W. Green). In G.C. Chow, *Econometric Analysis by Control Methods*, pp. 177–206. New York: John Wiley & Sons.
92. Review of Pan-tai Liu, ed.: *Dynamic Optimization and Mathematical Economics. Optimal Control Applications and Methods* 2, 204–205.
93. Solution and Estimation of Simultaneous Equations under Rational Expectations. Research memorandum 291, Princeton University, Econometric Research Program, October.

1982

94. Estimation and optimal control of models of dynamic games. In M. Deistler, E. Fürst, & G. Schwödiauer (eds.), *Games, Economic Dynamics, and Time Series Analysis*, pp. 279–290. Wien Würzburg: Physica Verlag.
95. Outline of an econometric model for Chinese economic planning. *Journal of Economic Dynamics and Control* 4, 171–190.
96. Analyzing econometric models by control methods. In G. Chow & P. Corsi (eds.), *Evaluating the Reliability of Macro-Economic Models*, pp. 149–162. London: John Wiley & Sons.

97. Report of the Committee on U.S.–China Exchanges in Economics. *American Economic Review, Papers and Proceedings*, 72 (May), 429.

1983

98. Report of the Committee on U.S.–China Exchanges in Economics. *American Economic Review, Papers and Proceedings* 74 (May), 417.
99. *Econometrics.* New York: McGraw-Hill Book Company. (Beijing: Friendship Publishing Corporation, in Chinese, 1985).

1984

100. Random and changing coefficients models. In Z. Griliches & M. Intriligator (eds.), *Handbook of Econometrics*, vol. II, ch. 21, pp. 1213–1245. Amsterdam: North-Holland Publishing Company BV.
101. Note on maximum-likelihood estimation of misspecified models. *Economic Modelling* 1 (April), 134–138.
102. Report of the Committee on U.S.–China Exchanges. *American Economic Review, Papers and Proceedings* 73 (May), 464–465.
103. Economic Research in China. Mimeo, Princeton University, December.

1985

104. *Econometric Analysis by Control Methods.* New York: John Wiley & Sons. 1981. (Beijing: Friendship Publishing Corporation, edition in Chinese, 1985).
105. On the efficiency of enterprises and the price mechanism (in Chinese). *Economic Research* (People's Republic of China).
106. *The Chinese Economy.* New York: Harper & Row, Publishers (Singapore: World Scientific Publishing Co. Pte. Ltd., 1987; Tianjin: Nankai University Press, 1985).
107. Chinese statistics. *The American Statistician* 40 (August), 191–196.
108. On two methods for solving and estimating linear simultaneous equations under rational expectations (co-authored with Philip J. Reny). *Journal of Economic Dynamics and Control* 9, 63–75.
109. Report of the Committee on U.S.–China Exchanges. *American Economic Review, Papers and Proceedings* 74 (May), 454.
110. Chinese Economic Reform: Some proposals. Mimeo, Princeton University, November. Published in Chinese in *Economic Reporter* 6 (2, February), 507 (1986).

1986

111. Report of the Committee on U.S.–China Exchanges in Economics. *American Economic Review, Papers and Proceedings* 75 (May), 452.

1987

112. Development of control theory in macroeconomics. In C. Carraro & D. Sartore (eds.), *Development of Control Theory for Economic Analysis*, pp. 3–20. Dordrecht: Martinus Nijhoff Publishers.
113. Hypothesis testing. J. Eatwell, M. Milgate, & P. Newman (eds.), *The New Palgrave*.

London: Macmillan Press Ltd. Also in J. Eatwell, M. Milgate, & P. Newman (eds.), *Time Series and Statistics*, pp. 109–117. London: Macmillan Press, Ltd., 1990.

114. Development of a more market-oriented economy in China. *Science*, 16 January 1987, 295-299. Also in Y.C. Jao, V. Mok, & L. Ho (eds.), *Economic Development in Chinese Society*, pp. 39–48. Hong Kong: Hong Kong University Press, 1989.
115. Money and price level determination in China. *Journal of Comparative Economics* 11, 319-333. Also in B. Reynolds (ed.), *Chinese Economic Reform: How Far, How Fast?* pp. 29–43. San Diego: Academic Press, 1988.
116. Report of the Committee on U.S.-China Exchanges in Economics. *American Economic Review, Papers and Proceedings* 76 (May), 399.

1988

117. Socialism with Chinese characteristics and China's economic development. Presented at the China Institute of America, Symposium on Chinese Culture in the Modern World, New York, April.
118. Report of the Committee on U.S.-China Exchanges in Economics. *American Economic Review, Papers and Proceedings* 77 (May), 523–524.
119. Economic analysis of the People's Republic of China. *Journal of Economic Education* 19 (Winter), 53–64.
120. Market Socialism and Economic Development in China. Research memorandum 340, Princeton University, Econometric Research Program. Presented before the International Seminar on Economic Reform in China, 1979–1988, Shenzhen, November 7–13. Also in *Wen Wei Po* (Hong Kong), November 11, 13 (in Chinese).
121. Economic and political reform in China and the future of Hong Kong. Mimeo, Princeton University, Econometric Research Program. Presented before the International Seminar on Economic Reform in China, 1979–1988, Shenzhen, November 7–13. Also published as "Economics Education and Economic Reform in China" in *Proceedings of the American Philosophical Society* 33, 64–74 (1989).

1989

122. Teaching economics and studying economic reform in China. *China Economic Review* 1. Chinese translations published in *Commercial Times* (Taiwan), March 7, 8, 9, 10, 11, 12, 13, and 14, and in *Hong Kong Economic Journal Monthly* 144 (March), 40–47.
123. Report of the Committee on U.S.-China Exchanges in Economics. *American Economic Review, Papers and Proceedings* 78 (May), 426–427.
124. Rational versus adaptive expectations in present value models. *The Review of Economics and Statistics* 71 (August), 376–384.
125. Prospects of China's economic growth, foreign economic relations and cultural exchanges with the U.S. *Princeton Alumni Weekly*, September 27, 16–17. Also in *Hong Kong Economic Journal Monthly* 151 (October), 22–24 (1989; in Chinese), and *The Future of China: The Scholar's Views*, pp. 71–75. Teaneck, NJ: Global Publishing Company 1990 (in Chinese).
126. Preface/introduction. In T. Min (ed.), *Essays on Modern Economics*, vol. 1, pp. 1–3. Beijing: Commercial Press.

1990

127. Report of the Committee on U.S.-China Exchanges in Economics. *American Economic Review, Papers and Proceedings* 79 (May), 490–491.

128. Notes on Linear Time Series Models with Unit Roots. Mimeo, Princeton University, Econometric Research Program, October.
129. The Multiplier-Accelerator Model in the Light of Cointegration. Research Memorandum 357, November.
130. Econometrics, dynamic analysis and control theory. In Chinese Quantitative Economics Association, Beijing (ed.), *Jiangyi Jiliangxue Jiangyi* (*Lectures on Econometrics*), ch. IV, pp. 189-226. Beijing: Aerospace Industry Publishers.

1991

131. Book review, C. Davis and W. Charemza, eds., *Models of Disequilibrium and Shortage in Centrally Planned Economics. Journal of Comparative Economics* 858 (April), 392-394.
132. The Chinese economy: Substantial growth in the 1990's. *Economic Insights* (May/June), 32-34.
133 Report of the Committee on U.S.-China Exchanges in Economics. *American Economic Review, Papers and Proceedings* 80 (May), 413.
134. Rights to assets and economic behavior under Chinese socialism. Presented at the conference on The Economic Contest between Communism and Capitalism: What's Ahead?, SUNY, Buffalo, May 10. In *Academia Economic Papers* 20 (2, September), 267-290 (1992).
135. Note on Equilibrium Business Cycles with Technical Innovations. Mimeo, Princeton University, Econometric Research Program, November.

1992

136. Dynamic optimization without dynamic programming. *Economic Modelling* 9 (January), 3-9.
137. Statistical Estimation and Testing of a Real Business Cycle Model. Econometric Research Program memorandum 365. Princeton University, March.
138. Report of the Committee on U.S.-China Exchanges in Economics. *The American Economic Review* 82 (2, May), 615.
139. The Integration of China and Other Asian countries into the World Economy. Mimeo, Princeton University, Econometric Research Program. Invited paper presented at the Mont Pèlerin Society general meetings, September, in Vancouver, British Columbia.

1993

140. Capital formation and economic growth in China. *The Quarterly Journal of Economics* CVIII (August), 809-842 (1993).
141. Simulations in economics and business. In D. Greenwald (ed.), *The McGraw-Hill Encyclopedia of Economics*, 2nd ed., pp. 909-911. New York: McGraw-Hill.
142. A two-step procedure for estimating linear simultaneous equations with unit roots. *The Review of Economics and Statistics* LXXV (February), 107-111.
143. Solving Optimal Control Problems by Locally Quadratic Approximations to the Lagrangean Function. Princeton University, Econometric Research Program Seminar, March 15.
144. Report of the Committee on U.S.-China Exchanges in Economics. *The American Economic Review* 83 (2, May), 512.
145. Optimal control without solving the Bellman equation. *Journal of Economic Dynamics and Control* 17 (July), 621-630.
146. How and why China succeeded in her economic reform. *China Economic Review* 4 (Fall), 117-128.

1994

147. Computations of optimum control functions by Lagrange multipliers. In D.A. Belsley (ed.), *Computational Techniques for Econometrics and Economic Analysis*, pp. 65–72. Boston: Kluwer Academic Publishers.
148. *Understanding China's Economy*. Singapore: World Scientific Publishing.

1995

149. Multiperiod competition with switching costs: Solution by Lagrange multipliers. *Journal of Economic Dynamics and Control* 19 (January/February), 51–58.