Econometric Theory, **15**, 1999, 389–424. Printed in the United States of America.

THE ET INTERVIEW: PROFESSOR GEORGE C. TIAO

Interviewed by Ngai Hang Chan
Department of Statistics
Carnegie Mellon University

Professor George C. Tiao.

George C. Tiao is a well-known figure among statisticians and econometricians. He has made substantial contributions to the fields of Bayesian statistics, environmetrics, time series modeling, intervention analysis, and outlier detection that have had profound and lasting influence. It is fair to say that much of the current research in time series econometrics began with his work or is influenced by his ideas.

George Tiao was born in 1933 in London and was raised in China during its most turbulent period. He came to the United States in 1956 and completed an

 0266-4666/99 $9.50

MBA degree in 1958 and a Ph.D. degree in economics in 1962 under the supervision of George Box and Arthur Goldberger. He stayed in the statistics department at Wisconsin for the next 20 years, serving as the chairman from 1973 to 1975, before joining the University of Chicago, where he currently holds the W. Allen Wallis Professorship of Econometrics and Statistics in the Graduate School of Business.

Among his many contributions and honors, he is the founding president of the International Chinese Statistical Association, the founding editor of the *Statistica Sinica*, an elected fellow of the IMS, RSS, and ASA, and an elected member of ISI and Academia Sinica. In 1993, he was awarded the Distinguished Service Medal given by the directorate-general of budget, accounting, and statistics of Taiwan. He was a longtime organizer of the annual NBER/NSF Time Series Seminar and initiated the annual conference on Making Statistics More Effective in Schools of Business that just celebrated its 10th anniversary. For the past two decades, George Tiao has been offering free short courses on statistics in Taiwan and China every year. These courses have had a profound impact and far reaching consequences in stimulating people's interest in statistics in Asia.

George Tiao enjoys traveling, and he is a familiar face and a regular participant at conferences in statistics and econometrics. The following interview was conducted in October 1997 at his office in Chicago.

1. BACKGROUND AND EARLY STAGE

Could you start by telling us about your early life such as the conditions in China during your school days? Were there any major events particularly inspiring at that stage of your life?

I got out of grade school in 1945, when World War II ended. I went through six different high schools from 7th to 12th grades. My parents worked for the Nationalist government; we moved around a lot, and it was totally chaotic. Just like everyone else in China those days, one took examinations to go to school. All I could remember was taking examinations all the time! I went to several of the very good schools in China in different cities; in Chungking, Shanghai, Nanjing, and so forth. We finally settled in Taiwan in 1950. It was only the last one and a half years of my high school that could be regarded as being stable. It was very disruptive; I hardly stayed in one place for more than a year, and I never studied with the same teachers or classmates. Because of the opium war, the rise of imperialism, the wars with Japan, and eventually the civil war, people in my generation witnessed China suffering for decades. The way we were brought up aimed at only one mission: to save the country. This turned out to be an influential factor in many things that I ended up doing later in my life. Only my college years were peaceful and quiet.

Before we embark on the college years, would you tell us why you chose economics? Science was perceived as the jewel of academics during those days.

This actually has something to do with the way we were brought up. I was fortunate to be born in a highly educated family. My parents worked for the central government at that time, and they were sent to the United States to study. They actually got married here in Illinois. Later they went to England to study at the LSE, and I was born in London. I was there for only four months and then went back to China. My parents took my education very seriously, and they always managed to get me into very good schools. They started saving money to send me abroad since I was born, but like everyone else, all their savings got wiped out during the war. They gave me special tutoring in three subjects: Chinese, English, and mathematics. My father wanted me to be in international law. He always admired lawyers specializing in international law. My mother thought I should be an engineer. But at that time, I witnessed the government's collapse in the mainland. Although there were many reasons contributing to its failure, poor management of the economy was an undeniable factor. I was thinking that if I knew more about economics, I could do something. That's why I entered college majoring in economics.

Could you tell us something about these special tutors?

I had this tutor in mathematics who taught me algebra. I was very lucky to have him as my tutor, and I am always grateful to his inspirations. I still remember this incident when he was teaching calculus at the National Taiwan University; he asked me: "George, why don't you take my calculus course?" I told him I was in economics, and he was very disappointed! He felt terrible, as if he had wasted all his efforts. Later, he became a very famous senior statesman and the president of National Central University.

Was he one of your inspirations?

Yes indeed. That's how I got started in economics. I had some very good teachers, and because of my training in high school, I ended up taking more theoretical courses. Many professors at that time were trained in Japan. Most of the economic theories were developed in the West. Because of my training in English, I was able to read directly from the source. But for some of the teaching faculties, they had to teach out of a Chinese translation of the Japanese version of the English source. I learned quite a bit either by reading the original source or my discussing ideas with the faculties. I still remember I was reading about dynamic relationships from Baumol (1951) with this professor. Being an economist, he knew more about economic theory, but I knew more mathematics. With all these difference equations, we tried to understand the interrelationship among various factors. We read the book together two to three times a week. He taught me about economics, and sometimes I explained the mathematical structures to him. I was fortunate to have such a background training. I was able to take more difficult courses in theory and spent a little more time in thinking about economic notions. I believe I came out with a little more than some other students.

You came to the United States starting out as a student in economics, then earned an MBA from NYU and returned to economics/statistics for the doctoral degree. Could you tell us something about this transition?

It's all by accident! It just happened. I got admitted to the economics department at NYU in 1956. I took a boat from Taiwan to Japan and from Japan to New York. Through my father's connections, I was able to work as a part-time trainee in a bank in New York City for about \$50 a week. That was already very unusual for a student in social sciences. Unlike our fellow students in engineering, most of the students in social sciences at that time did not receive any scholarship. My coworkers in the bank told me to get an MBA from the downtown center of NYU which was just two blocks away from the bank.

It's a convenient reason.

It's more than just convenience. I remember people in the bank told me that if I got an MBA, I might be able to find a job that offered enough to eat. But if I got a degree in economics, unless it's a Ph.D., I might not be able to find any job. As a 23-year-old kid knowing nothing about this country and seeing all my engineering friends driving fancy cars, this advice became very convincing. Also, people who gave me advice worked in the bank. For them, MBA was very natural. That's how I got into the MBA program and went through it without any trouble since I had most of the courses back in Taiwan. But all the time I needed to worry about the future. After graduating, I got a regular job in the bank and earned something like \$75 a week!

You got a 50% raise!

But now I had to really work full time! I worked there for about a year and kept worrying about the future. I met this guy in his early 40's who worked for the bank Irving Trust Company at 1 Wall Street, a very interesting address. He used to work for the Bank of China in New York, but because of the revolution in China, he got laid off and went to Irving Trust. I still remember this story. One day we were chatting, and he looked at me and said: "I'm in the 40's and you're making \$75 a week. You know how much I'm making? \$120! There is just no hope. Since you don't speak Cantonese, you can't even go to work for the branches in Chinatown. You can work here for 25 years, and you'll probably make \$130–\$140 a week. There is just no hope! Your best bet is to go back to school. Let me give you an example. My brother-in-law, Gregory Chow, who got his Ph.D. degree in economics, is now a famous economics professor at MIT." That sounded very intriguing to me; I was tempted to try. Gregory and I later became very good friends. I discussed this idea with my wife, and we decided to apply for schools. We first tried NYU, and there was no problem in getting admitted to its Ph.D.

program. The problem was money. No assistantship whatsoever at that time. I applied for a few other schools. One was the Harvard Business School, and I got nowhere, of course (laughter)! I also applied to the finance department of the business school at Wisconsin, and they gave me a scholarship of $1,500 a year. At that time, the business school was like a spin-off of the economics department, and the school had this requirement of a secondary field.

Was that how you got into statistics?

Sort of. At that moment, I thought the most natural minor subject for me would be accounting as I had been quite good at it. But then I had already done some accounting back in Taiwan and did some more when I was at NYU; I had absolutely no desire to do the same thing the third time. I was looking for a different subject, and a friend, Frank Jen, suggested statistics to me as a possibility.

The Jen in finance?

Yes, that's the person. So I went to the mathematics department and took a course from an instructor who had absolutely no idea of what statistics was about. We used the book by Mood (1952), which was very difficult. It wove the frequentist ideas with fiducial probability. Anyway, because of my background in mathematics, I was able to do the exercises and learned the mechanics by myself. Then Goldberger joined the economics department and Box joined the mathematics department a year later, and that's how I began my career as a statistician. I first switched from business to economics, and with the permission from economics, I ended up writing a thesis under Box on robustness of linear models. By the time the thesis was ready, I talked to Goldberger, and he said: "Well, this is all very nice, but it's not econometrics. Do something in econometrics and I'll approve your thesis." I had about three weeks time only, and I kept asking myself: "What will I do?" "Henry Theil was doing something about the problem that if a regression function was coming from different sources, how to combine these sources together when the variances are not the same," Goldberger said. I took a look at Theil's stuff and did it in a Bayesian way. I wrote a chapter about these findings and derived some of the posterior distributions. After reading this chapter, Goldberger said: "Ah ha! Now you can graduate!" That's how I got myself into statistics; it's all by accident! By the time I graduated, Arnold Zellner joined the economics department. When he saw my thesis, he got really excited and started our long-term collaboration.

2. WISCONSIN YEARS

Would you tell us something about how you started your career at Wisconsin?

Before my graduation in 1962, the robustness paper was accepted by *Biometrika*. I went to talk to Box about finding a job. His reply was: "I have a job for you if you want it." What happened was that he had this vision of building the best statistics department in Wisconsin, as far away from mathematics as possible but with connections to major applied fields through joint appointments and research collaborations.

But he started out in mathematics.

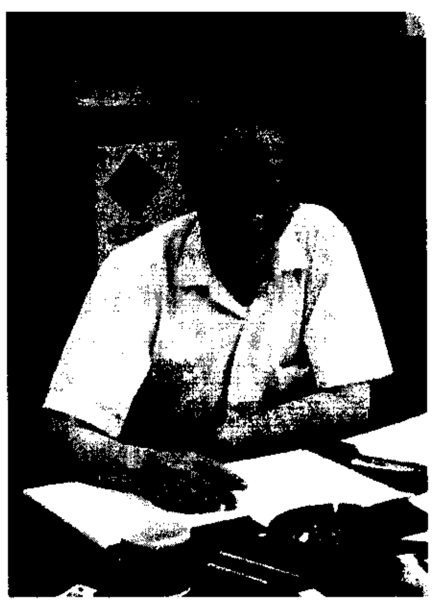
Although his initial appointment was in mathematics, he wanted statistics to be mentally and physically separated from mathematics to establish its own identity. In addition, he wanted the statistics department to occupy a central place having joint appointments with key scientific disciplines. He himself had a joint appointment with chemical engineering, and he wanted to hire people having joint appointments with the medical school, the business school, the agricultural school, etc. For me, it was natural because I was a mixture of statistics, economics, and business. He thought that it would be better for me to be jointly appointed with the business school than with economics since it would provide a better link to statistics. The dean of the business school was very kind. Together they asked me to teach just one course in statistics a semester for the first three years. The business school would cover 40% of my salary, and it came down to which course in statistics I should teach. Here came the nervous part. "Why don't you teach mathematical statistics?" Box said. He thought this would be the best arrangement for me. But my formal training in statistics at that point was really sketchy; I had only taken two to three courses altogether! I would feel more comfortable teaching a lower level course such as Hogg and Craig, say. But he gave me this course to teach; that was probably one of the greatest challenges in my life. To broaden my background, I sat in all these different classes: multivariate analysis by John Gurland, analysis of variance and experimental design by Norman Draper, and decision theory by Irwin Guttman. Basically I was sitting in classes like other students, but I taught mathematical statistics!

Would you share some of your experiences with us about working with George Box and about being perceived as a Bayesian statistician at that time?

I was very lucky, because the Bayesian thing just got revived at that time. It was controversial, and people could get very emotional. Some people might say that George Tiao didn't know any statistics, which was true (laughter), just for the reason that I'm a Bayesian. On the other hand, because of the revival of the Bayesian ideas, journals like *JASA* and *Biometrika* were very receptive to Bayesian papers. I still retain a copy of an encouraging letter from Pearson when he was the editor of *Biometrika*. I had no problem in publishing papers in these journals for several years; I was probably one of the few people who published the most number of papers in *Biometrika* in the 1960's. I could explore different things and reinterpret different ideas in a Bayesian framework. It helped my career quite a bit, although to certain quarters, the Bayesian idea was perceived as valueless. I was really fortunate to have Box first as a teacher and later as a colleague and friend. He was always kind and fair; I never had any problems working with him nor had any feeling that he had taken advantage of me. In fact, I've never heard any complaints from people who wrote papers with him. We might be overshadowed by him, but there's nothing we could do about it. He always has a lot of ideas, and by working with him, we learned the way he thinks.

After your Ph.D., you spent the next 20 years at Wisconsin, serving as chairman from 1973 to 1975. This was an exciting period for Wisconsin. Would you tell us something about the atmosphere?

When I joined the department in 1962, there were Box, Draper, Gurland, and myself. Irwin Guttman first came as a visitor. He and I ended up sharing an office for the next four years. The department started out with Box as the senior faculty member. It experienced the greatest expansion in the mid-1960's. That was the golden age, and many departments expanded during that decade. People like Jerry Klotz joined the department in 1965, Richard Johnson and Gouri Bhattacharyya came in 1966, and Steve Stigler and Grace Wahba joined in 1967. There was this misperception from outside that there existed two camps in Wisconsin at that time. There was Box and his associates, and then there was the non-Box camp from the West Coast. That wasn't quite the case; at least we didn't feel it



During 1974, chairman of the Department of Statistics, University of Wisconsin, Madison.

that way within the department. Still we always had to deal with this "tale of two camps" rumor. When I became the chairman, I went out of my way to "glue" the whole thing together. Gouri and Steve, presumably from the West Coast camp, were the biggest help. By the time I stepped down, statistics at Wisconsin was having a glowing reputation. I helped build many of the joint ventures during my tenure. I was the chairman for two busy years and then escaped to Taiwan. One of the proudest things I did later was the appointment of Jeff Wu. He was a real character who later left Wisconsin and moved on to higher plateaus.

Would you say something more about the joint ventures?

I didn't do too much with the engineering because of a personality problem. The medical and agricultural schools were our major collaborators. We also established a Ph.D. program in business statistics with Bob Miller in the late 1960's, and later we hired Jim Hickman from Iowa, who was famous in actuarial sciences. Through this program, we graduated students like Der-an Hsu, Bill Bell, Steve Hillmer, Mike Grupe, and Chung Chen, who was the last one out of this program before I left Wisconsin for Chicago.

How was the relationship with economics?

We had ongoing work both with the business school and the economics department. When Arnold Zellner was there, we had a lot of interesting joint works, and he literally spent half of his time in statistics, since he got interested in Bayes. He really pushed the Bayesian idea and brought the Bayesian framework to econometrics. We formed a weekly session to study Jeffreys' 1961 book together with Box, Guttman, and some others. By the time we got to testing, nobody was terribly interested except for Arnold. Most of us were trained in thinking about estimation, but when it came to social sciences, all of a sudden, we needed to test hypotheses. For the estimation problem, all these stable estimation ideas using locally uniform prior work, but for testing, none of them work. There were all these inconsistencies and many of us dropped out, but Arnold persisted. So now he owns the odds ratio! When Arnold left, the tie with economics became much weakened because he was the person pushing the Bayesian idea like a bull.

How about other people in economics?

Goldberger was not interested in Bayes. Arnold, Art, and I started out reading the book by Savage (1954). After about a month, Goldberger left; only Arnold and I persisted. I was very lucky to have the opportunity to work with all these people. Arnold and I ended up writing several papers together, but the relationship with the economics department has never been restored since he left Wisconsin. Although we had joint seminars, we didn't have as close a relationship as with the business school.

What was the relationship of statistics with the Mathematical Research Center (MRC) at Wisconsin?

Statistics was a cosponsor of the MRC, and Box was a permanent member of it. But as I mentioned earlier, Box had this unique idea of building statistics as far away from mathematics as possible. He always wanted Wisconsin to be a place known for its balanced education in statistics. If one wants to do just mathematical theory, one should go to Berkeley or Columbia. But if one wants to understand both the theoretical and the practical sides of statistics, Wisconsin is the place. One of his famous statements is "statistics must go hand in hand with practice." This doesn't mean that he disregards theory; he just thinks that the practical side of statistics is equally important. He used this principle to establish the joint appointments in statistics. That was the prime time of the Wisconsin years. I still remember this story from Irwin Guttman. When he was the chairman, the dean called him up one day and said: "We have an extra $50,000 to spend, and you can use it to hire four more people next year." It was really incredible, and that was the only time we had that kind of opportunity, the golden 1960's.

Back to Box, to implement his principle, he arranged this Monday night beer seminar at his house. He got people from all areas to present their problems. He was a very quick thinker, and he could size up the problem and offer suggestions. People would come for his advice, for free consulting services, and for free beer. It had been a very interesting and successful event, and his house became very famous every Monday night. So that's sort of my Wisconsin years, and I still have a lot of fond memories about it.

Before we move on to Chicago, would you say something about the development of the now famous classic Box and Jenkins? What about the term *ARIMIAN*, which is often attributed to statisticians trained at Wisconsin who are proficient in using ARIMA models?

I've never been called an ARIMIAN myself. One reason might be that I wrote only a few papers about time series in the 1960's. I was busy doing the Bayesian stuff and writing the book with Box. But I remember at that time, Box was pushing the time series idea with Jenkins. Jenkins came to Wisconsin for a couple of years. Unfortunately he developed the Hodgkins disease and went back to England. Although I was not involved very much at the beginning, I witnessed the development of their book. They had a room with all the graphs, control charts, etc., hanging all over the walls. They wrote their chapters and the first few papers about ARIMA modeling in that room by inspecting the charts and discussing how to apply time series to solve these control problems. Box, with an industrial consulting background, tried to understand these control charts and the theory behind them. That was the beginning of the book. It's kind of funny to see that although the control part of the book marked its beginning, it's the least mentioned part in the literature. Now, there is an updated version of the book with inputs from Greg Reinsel in Box, Jenkins, and Reinsel (1994).

3. CHICAGO YEARS

You moved to Chicago's business school in 1982. Chicago now has one of the best econometric units. Was it like that when you first arrived? Has it turned out much as you had hoped?

I came to Chicago in 1982, and it had really been a great change, in terms of everything. Chicago is a great place, and it is very convenient. Everything is close; I can go to the statistics department in one minute, economics in another minute, and geophysical science in three minutes. In fact, I have been doing joint work on the ozone project with the chairman of the geophysical science department here at Chicago. We often met in this office, and I always laughed at him: "Why do you come to the business school to do science?"

The business school used to have a pretty large group of statisticians. Bill Wecker was mainly responsible for building up this group. Arnold joined in 1966 to build up an econometrics unit, although he was very supportive of statistics. The two units lasted in parallel until Arnold's retirement. The school marks its name in quantitative analysis, with people like Eugene Fama studying stock returns and all that. It regards two things as most crucial to the MBA education: statistics and economics. That is why we have been having these two groups of people. When I first arrived, they needed someone to strengthen the statistics group and to build up its reputation. The first person they had in mind was Morris

Chicago in the mid-1980's.

DeGroot at Carnegie Mellon, but that didn't work out. Then they came to me, and I dragged on the decision for three years before I finally came in 1982. When I first came, we had people like Craig Ansley, Robert Kohn, and Ed George. Craig and Robert had already made their reputations in smoothing and Kalman filters; those were the heydays of state-space modeling. They left for one reason or another at the end, and it became a crisis. Fortunately, we were able to hire two very good people, Rob McCulloch and George Easton. Peter Rossi went to Northwestern after graduation and then came back, and when he came back I insisted the school hire him jointly between econometrics and statistics. He served as a link and did a very good job. Rob developed extremely well, and so did George. Ed George was the person who really helped me a lot in gluing the group together. By the time Ansley and Kohn left, we needed one person in time series, and we went after Ruey Tsay. That wasn't easy, but we succeeded in getting him in the end. I always regard that as a proud accomplishment. Now we have a group of two more econometricians apart from Peter, who is basically in charge of marketing. We will probably have more in the future. I am quite happy to see the way we have developed; especially now since we are all in one unit, it seems the group is going to stand a better chance of making our reputation even better. Everybody gets the title of "professor of econometrics and statistics." We always have had a close relationship with the statistics department, and in recent years, we have also been working more closely with the econometricians from the economics department. By reestablishing this latter link, this place will be ready to become a first-class econometrics and statistics unit.

It is interesting that within the business school you have the blending of econometrics and statistics units. Is that what you have in mind?

To most of us there is really no distinction between econometrics and statistics. There was this distinction because in the past, econometricians did things that statisticians didn't usually do. For example, they worked on simultaneous equation models, errors in variables, and to some extent time series. Another thing you wouldn't find in a standard statistics book is the seemingly unrelated regression, one of Arnold's famous contributions. When statisticians teach multivariate regression, the X matrix is always the same, but that is not necessarily true in economic applications. Nowadays, there is more merging between these two subjects. Econometricians are now doing asymptotic theory and worry about proofs while statisticians are getting more into data-oriented approaches. I used to say to them: "You are just like us in the 1960's!"

What about students? Do you see some differences among the graduate students here from Wisconsin?

One thing that was unexpected when I first came here was students. I started the business statistics program at Wisconsin, and I thought such a program would naturally be larger at Chicago. But to everybody's surprise, this has not been the case. What happens is that after spending a couple of years with us, many of the good students ended up either in finance or marketing or went to work for industry. They still teach and do research in statistics but just get more pay! It's a free market; a student comes and goes. They use the business statistics program as a transit station. Students graduated from statistics departments usually stay within the statistics community. I think the reality is that if we want to build a business statistics program, build it from a statistics department. On the other hand, I must say that students really enjoy coming here. They left not because they got frustrated. There are ample resources and opportunities for students to get involved in real problems, which is hard to find elsewhere. So the Chicago years went pretty fast, 16 years now.

4. RESEARCH

Much of your early research was on Bayesian statistical methodology. But later it seems that you moved closer to adopting the frequentist's views. What was the reason for this shift?

I happened to be at the right time in the 1960's when the Bayesian idea was revived. That's how I ended up writing this book with Box, Box and Tiao (1973), on Bayesian statistics. Then I moved on to time series for a couple of reasons. One was that since most of the economic data I worked with were time series oriented, it was very natural for me to work in that area. The second reason was computation. One could find Bayesian solutions to most of the problems in theory, but the difficult part was the computational aspect. It was not until the recent advent of the Gibbs sampling that the computational problems became feasible. So Gibbs helped me to sell a few more copies of my old book with Box (laughter).

If you look at other people, for example, Ruey Tsay, most of them started out with a frequentist view and moved closer to the Bayesian paradigm later in their careers. My case was the opposite. I started out as a Bayesian and eventually became more like a frequentist in the following sense. If one wants to do inference based on a given set of data, then the Bayesian view of looking at the likelihood would be the right way. But when one works in developing time series methods, or any statistical methods, one needs to know the performance of the methods, and frequentist ideas become more natural.

In the Bayesian framework, are there any papers you wrote that you would like to comment on?

Apart from my thesis and papers with Box on robustness which proposed a useful framework to assess model assumptions, one early thing that I got interested in was the variance component or random effects model that Tan, my first student, and I worked on together. It was an interesting issue since variance component was not well taught in typical statistics courses at that time. Everybody knew how to do the ANOVA for a fixed effects model, but estimating the variance components was much harder although this problem came up very often in industrial applications. There was this thorny issue of negative component estimates. In fact, one of my earlier contacts with time series was to use an AR(1) model with negative autocorrelation for the within group errors as a possible explanation of negative component estimates. Then there was this paper that Box and I wrote on estimating the random effects which I thought was quite interesting. It led to the Bayesian computation of the Stein estimation and was published in *JASA* in 1968. Later on, Lindley and Smith expanded the whole thing in their famous paper in 1971 in *JRSS-B*. Box and I wrote a long chapter on estimating random effects for variance component models in our book partly based on this earlier work. Another paper Box and I wrote in 68 was on the Bayesian approach to outliers. It made quite an impact, and it was based on a very simple idea. If one has an observation from one of the two models, the simple model and the alternative model, how can one make inference? The problem was to get the full posterior distribution from the mixture. The computational part had to wait until the Gibbs sampling became available, but we did lay out the approach in that paper. We didn't put it into the book; I kind of regretted it, but we had to stop somewhere. One final thing I would like to say about the book has to do with the data translated likelihood idea behind what we called "noninformative" priors. We were happy with Jeffreys' argument, but Fred Mosteller felt that we should not be "hiding" behind Jeffreys and challenged us to come up with an explanation of our own. George always liked to plot things (by hand), and when he did the likelihood function of the arc-sine transformed Bernoulli coefficient for different x's in n draws, we got it.

What about time series; any particular work that you like most?

I probably have a lot more to say on time series. It can basically be classified into several main topics. But before I go into the first topic, I should mention how I got started in the first place. It began with a paper that Box and I wrote soon after I graduated. Box discussed with me what happened to the power of the t-test if the data were autocorrelated. What if the data were nonstationary? We worked out the solution for some special cases and published a paper on estimating level shifts in nonstationary time series in *Biometrika* in 1965. Then the next few years we had a lot of people, especially people from education and psychology, asking about what happened to our result if they had a different model. I kept getting phone calls from people asking me to consult and make a few hundred dollars here and there. I imagine if I got some business, Box would have probably gotten more! That's how I got started in time series, with the proper motivation!

The first topic is intervention analysis. Regulations for smog level began to change during the mid-1950's in Los Angeles, and a lot of air pollutant data have been collected since that time. This led to the Los Angeles smog project which I'll say more on later. When the smog level was changed, we encountered the issue of how to estimate the level change when the series was nonstationary. *JASA* invited Box and me to write a paper on this topic in the early 1970's. That led to the intervention analysis paper, which is probably one of my better known papers. For years people introduced me as the guy who did intervention analysis. The paper took us about two weeks to write. Then we added another section about possible extension to outliers detection which formed the basis of later work with my other students like Ih Chang, Chung Chen, Lon-mu Liu, Ruey Tsay, Carla Inclan, and others. The nice thing about this paper is that not only was it motivated by practical problems, but it also provided a useful approach for other problems such as detecting outliers and estimating missing observations.

The second topic that I worked on is seasonal adjustment, decomposition, and unobserved components models. The way I got into this was quite interesting. In the late 1960's, I started doing time series, and I did some industrial teaching in time series. There was a statistician from Dupont who came to my class, and he asked me this question: "All these ARIMA models and filtering are very interesting. But we've been using an X-11 program from the Census which seems to produce sensible results. George, could you tell us the connection between the two?" "I'll have to come back to answer your question," I said.

I began to get interested in this question. The X-11 used a set of filters which could be modeled. So maybe there's a model connecting the X-11 with ARIMA. It was something similar to the famous story on exponential smoothing. Holt and others developed the exponential smoothing technique in the 1950's, and it was regarded as a very nice way to forecast because it gave more weight to recent data. But no one knew the underlying model until 1960 when Muth wrote the famous paper stating that exponential smoothing gives the optimal forecast when the underlying model is an ARIMA(0,1,1). Many good statistical methods were developed by creative people. They came up with very good ways to do practical things. It was only later on that other people worked out the underlying models and the theoretical justifications. At that time, Bill Cleveland was a student of mine, and he was looking for a thesis topic. I suggested this problem to him, and it took us a long time to finally find a model for the main filters. That paper was published in *JASA* in 1976. It's through this X-11 project that I really learned about time series decomposition and the difficulties of identification of unobserved component models.

The related topic is about canonical decomposition, a paper written by Steve Hillmer and me in *Biometrika* in 1978. If I had the sum of a signal plus a noise, to what extent can this sum be decomposed? One extreme was the canonical decomposition, and the other extreme was no decomposition at all. I spent an enormous amount of time in the area of seasonal adjustment and decomposition until the mid-1980's. Unfortunately, we are still left with the identification problem. Later on, some people tried to redo time series from the decomposition point of view by downplaying the identification issue. Such an issue may not be that critical for engineering problems. If we receive a signal from outer space, there will be noise inherited in the equipment. We can estimate the variance of the noise by testing the equipment. Once that has been done, we can go about estimating the unobserved components. But when it comes to economics, we can't do that. The problem is that although we can usually identify the ARIMA structure of the overall distribution of the sum, any unobserved component cannot be identified. In terms of forecasting the future of the observed time series, it doesn't make any difference. Any one of the unidentified models gives the same forecast for a future observation. On the other hand, if we have an estimated component such as one of those produced by the Census Bureau, then we encounter the following problem. Whatever we do, the other person can come up with an estimated component from another model within this unidentifiable class with equal claim. There's no way we can say one is better than the other. Therefore, unless we have a strong prior to identify the problem, it is not solvable to me; it's arbitrary. Because the problem is basically not identifiable, we proposed what we think to be a more logical model-based decomposition procedure, canonical decomposition, for seasonally adjusted economic data. There are still a lot of people interested in this topic, partly because the government, for budget appropriation and other purposes, needs seasonal adjusted figures.

Interestingly, Bill Bell and Steve Hillmer were both at the Census in the early 80s but couldn't change the Census bureaucracy because of inertia and "long tradition." Augustin Maravall picked this up, developed a computer package, called TRAMO/SEATS, and spent a good part of the last 10 years pushing this approach throughout Europe. The recently formed European Statistics Office made a study of the U.S. official method, X-12-ARIMA, the model-based procedure, and some other methods including the state-space approach. They recently organized a three-day meeting in Bucharest to discuss these procedures and asked me to give the opening lecture. In the end, they recommended to all the member countries to adopt the model-based method. I think the Census Bureau would eventually go this way because they are really 75% of the way there. This is very exciting because it will stimultate a lot of further research as the model can change once the nature of the data changes.

Another area I have a continuing interest in is aggregation. One reason that I got interested was because economic data are often temporally and contemporaneously aggregated. I started working on this area in the early 1970's, and it gets more interesting through the consideration of dynamic relationships. Suppose that on a monthly basis my spending is affected by my income, but my income doesn't depend on my spending. Then I have a unidirectional relationship, as economists used to call it, Granger's causality. But what happens if I only have quarterly data?

William Wei and I looked at this problem together when he was my student. It turned out to be quite complicated. In general, a unidirectional thing can turn into a feedback model. This means that the analyses we made can be affected by the data that we observed. So the causality issue is muddled once the data are aggregated. The problem is that if the data are observed at intervals when the dynamics are not working properly, then we may not get any kind of causality. Whenever I see a causality paper, I always think of this example.

One issue that has not been closed until recently is temporal aggregation in the long-memory situation. Instead of temporal aggregation for ARMA models, what happens when we have fractional differencing models? The solution is given in Man's thesis, and it is like a transitory phenomenon. When we aggregate a long-memory model, we still get a long-memory model. I think we finally closed this area.

Your paper with Box on the canonical analysis of multiple time series was among the first ones which made use of the idea of canonical correlations. It had tremendous impact on the subject of modeling multiple time series, leading to your later groundbreaking work with Tsay on scalar component VARMA models and the work of Reinsel and others (mostly ARIMIANS!) on partial canonical correlation approach of multiple time series. Looking back, would you tell us something about the genesis of this idea and the follow-through that led to the discussion paper with Tsay that was published in *JRSS-B* in 1989?

For quite some time in the late 1960's, I was wondering about the idea of principal component analysis. We make a linear transformation to obtain the combination that explains most of the variance. I remember attending a seminar in the business school at Wisconsin and this fellow from marketing was talking about using principal component analysis in marketing. But the data he analyzed was obviously autocorrelated. I was thinking whether it was still all right to perform principal component analysis for time series data.

In 1970, Box and I were spending a year in England together, and I talked to him about it. Then we came up with the thought that if we looked at all these nonstationary series together, perhaps we could find the component which explained all the nonstationarity. That's the way we started out. Maybe all these nonstationary series were due to one or two latent components. How could we use the combination of the past to predict the future? This took me a significant amount of time because I didn't understand much about the relationship between unit roots and canonical correlation at that time. Then the story got completely changed after we worked out the examples. What excited us was that the more interesting part was not the component that underlies the nonstationarity but the stationary component that we found when we applied the linear combination, a kind of canonical correlation transformation, to the data. For this stationary part, we could make an economic story out of it. We started out one way and ended up with something else. There was this idea of a linear combination of apparently nonstationary components that could be stationary. We published that paper in *Biometrika* in 1977.

That was the thinking behind the concept of cointegration.

That's right. Engle and Granger formulated the whole thing in their 1987 seminal paper in terms of testing, and we didn't. But the end product was that a linear combination of nonstationary series could be stationary. There are two things coming out of this paper. One is the use of canonical correlations as a technique for time series which plays some roles in some of my other research later on. The other one is the cointegration idea behind it.

When Ruey was a student at Wisconsin, we often talked about doing more with canonical correlations in time series. As time went on, we got more and more interested in transformation. It's all along the same line of thinking, and methods like the scalar component model (SCM) are ways to find interesting transformations that would simplify a multiple time series model. Sooner or later, one realizes that for multiple time series, one has to do more than just order determination. When the number of series (k) under consideration is moderately large, even the simplest order can raise to k^2 parameters, and one encounters the curse of dimensionality immediately.

What I found interesting is that statisticians are used to thinking about transformation; it's part of our training. For economists, they're very resistant. They said: "Well, I have a theory. There's the data, and I want to test for it. If I transform the data, I don't know what I will end up with." That's probably one of the reasons why economists didn't pay much attention to our 1977 paper in the past.

As statisticians, we engage in more exploratory activities, and transformation leads us to discover new things. That may be the reason why psychologists are more receptive to transformations, because they don't have a rigid paradigm. I really feel that transformation is very important because we use statistical methods to help explore new ideas and transformation is one of the arsenals that we use.

Canonical analysis has played an important role in the development of cointegration and nonstationarity in time series analysis. How have these ideas evolved, and where do they lead us?

In recent years, I have some doubts about this whole business of unit root and cointegrations. Although I think it's a useful concept, it's been overdone. The unit root is just about dynamic association. It didn't bring in concurrent variation. When we see series moving together, it's really for two reasons. One is the dynamic relationship, and the other is the concurrent relationship. Cointegration only worries about the dynamic relationship, not the concurrent variation. Also, the dynamic relationship is scale independent. For example, consider two independent random walks where the variance of the first one is 10,000 times bigger than the second one. Consider two different linear combinations of these random walks. They look exactly the same since they're dominated by the first one, but they're not cointegrated. Yes, in 10,000 years or 100,000 years they may look different, but within any finite span of time, they look exactly the same. Therefore there is no linear combination of them which is stationary, and yet they move together. More work still needs to be done. When we see series moving together such as the so-called comovement in the econometric literature, how do we really characterize it? Cointegration is only one way, and it relies on an asymptotic concept. Nevertheless, I think that transformations for model simplification, model exploration, and things like that are important. I hope econometricians can pay more attention to transformation.

The unit-root problem basically got popular because of two reasons. One has to do with the equilibrium theory in economics which makes the unit-root test so important. Beyond that is the new mathematical theory, i.e., all these nonstandard limiting distributions that J.S. White, Dickey and Fuller, Peter Phillips, you and Wei, and other people worked out.

Ahtola, Ruey, and you have also contributed to its development. One interesting aspect of nonstandard asymptotics is that they are not limited to the unit-root problem alone. In the so-called critical branching process, one encounters almost the same type of asymptotic phenomenon. This is known as a critical phenomenon, namely, the underlying long-run behavior of the process changes abruptly when some of the parameters approach a critical boundary. That may be the reason why statisticians are having a continued interest in these asymptotics. Perhaps we should move on to other areas of time series. Forecasting has been one of your favorite research areas. Your idea of adaptive forecasting has attracted a lot of attention.

There was a story behind this. When Xu and I first published this paper in *Biometrika*, I talked to Tunnicliffe-Wilson, and he told me a story. When he was a student working with Jenkins in Lancaster, someone from London came up one day and asked them this question. "I know how to forecast one step ahead. But what if I want to forecast five steps ahead, shouldn't I minimize the five-steps-ahead forecast error?" Practitioners always asked this kind of interesting question. Tunnicliffe-Wilson recalled that everyone jumped on this poor fellow by telling him all the nice features of the minimum one-step-ahead forecast error. But that's only true provided the model was correct. Who said the question was silly if the model was wrong? That's how I got started on this problem. If the model is wrong, the alternative may be to minimize the multi-step-ahead forecast error. What we did was that we used a nonstationary model to approximate a stationary one. If the underlying model is truly stationary but lying on the borderline, for short-term forecast, it doesn't matter if we use a nonstationary model to approximate it. On the other hand, we should revert back to the mean if we are interested in long-term forecast, that's what stationarity is all about. This observation gives us the idea that we can stretch the usefulness of any model by adapting the parameter based on the forecast horizon. We also need to consider the efficiency issue if the model is wrong. Most of the time, we talk about the efficiency under the right model, but not when the model is wrong. This is very important since the model is always wrong. Therefore, we need to be very careful in choosing the model, and that depends on what kind of inference we would like to make. Short-term, intermediate, and long-term forecast may give rise to very different models. The notion of using different models for different forecast horizon is not new. Cox had this idea in the early 1960's, and Findley worked it out in the mid-1980's.

How does this idea of adaptive forecast link up with robustness and Bayesian robustness in a time series context?

It will be very interesting to connect them. In the econometric literature, there are many papers talking about the effects of misspecification. I think the area of robustness in forecasting can be very rewarding. One of the problems we face in our field is that people's attention is too focused on specific topics. Part of this problem is that there are not too many statisticians working on time series, not like the Wisconsin days. Statisticians go into something else, nonparametric, semiparametric, etc. On the other hand, economists are usually much more focused in one to two topics. When we went to an econometric time series conference in the last 5–10 years, all we heard were papers about cointegration or ARCH-GARCH. Having said that, I think the econometric profession stands a better chance of further growth, mainly because they have stronger ties with the business sector.

Although there are now available methods to fit multiple time series, its empirical application still remains a big challenge. People on the empirical side are somewhat resistant to fitting VARMA models. What should be done to expedite the transfer of this technology?

Ruey and I have been writing this book on multiple time series for a long time. What we need is a book that really explains the multivariate concepts, a book that gives many good examples, and a book that explains the SCM and stuff like that before more people can use them. One of the natural areas in which multiple time series can be used is economics. But why is it not widely used in economics? It has to do with the regression training. The only model economists are willing to entertain is VAR, because it's like a regression model. But the VAR will get into trouble very quickly if there is an MA term. They need to use a very long VAR, and in order to use VAR in practice, they have to worry about how to shrink the AR parameters. I always laughed at them: "It's just like doing MA." Littleman came up with this Bayesian VAR idea, but it's the same thing. People always have the fear of using MA because they have to use a nonlinear estimation procedure, which is no longer a serious problem nowadays. The MA part is very useful because many of the limiting models in temporal aggregation have MA components; none of them has pure AR components. Sooner or later, the VARMA model will be more widely accepted, and when that happens, people will be less committed to using VAR only. Also, the computer technology will make it easier for people to entertain a full VARMA.

You're right, we definitely need a textbook to explain more about the scalar component model. In addition, we also need software to accompany it. I once taught a course in multiple time series and used *S-Plus* in the class. The trouble with *S-Plus* is that it has only one model for multiple time series, VAR. There's no VARMA. This has created all kinds of problems. It's already difficult enough for the students to learn the concept, let alone

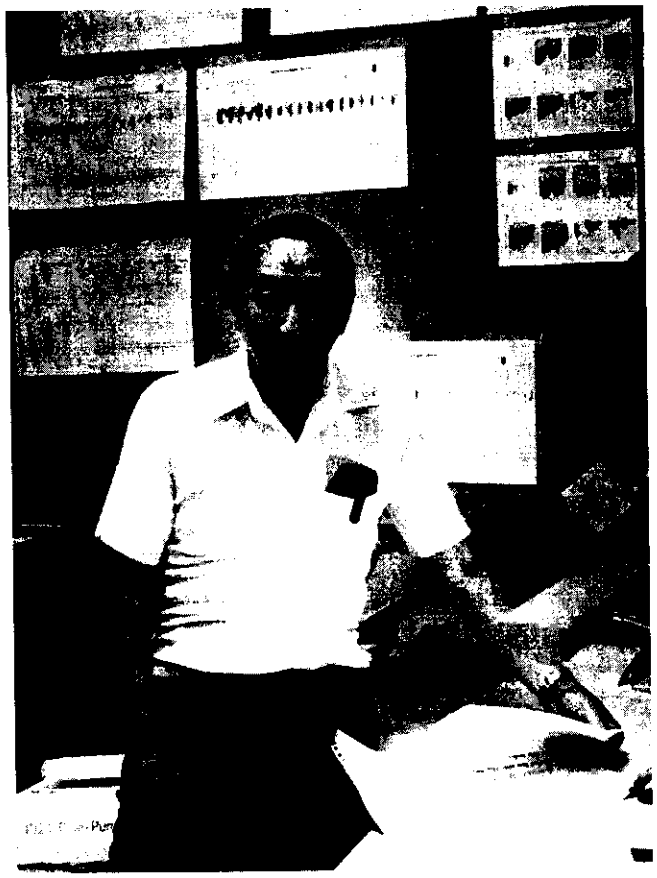
developing software by themselves. As good as it is, the Scientific Computing Associates (SCA) package is not very popular among practitioners.

We need to complete the textbook that Ruey and I swore to finish. Otherwise all this work on multivariate time series will all go down the drain. This is probably the right time to do it because now the computer is so much faster and better. For the software, the thing to do is to get Microsoft to buy SCA, or even better, give it to Microsoft for free!

Let me change the subject a little bit. Would you tell us something about your connection with the ozone project? You once testified before a congressional committee on ozone depletion. Although the readers of *ET* are mostly related to economics, I am sure we would all be delighted to learn more about your experiences and involvements in this dimension.

It was by coincidence. In 1973, a guy called us at the department and told us that they had a project in California about air pollution. We decided to take a look at the data. Richard Johnson and I went to California for a site visit, and we were very impressed by their seriousness. That's how we got started with the project. Because of the sensitive nature of the air pollution problem at that time, Box used to laugh at me that since I was the person who signed the contract, if anything went wrong, I would have been the person who ended up in jail! It was a wonderful project which lasted for five years from 1973 to 1978. It gave me the opportunity to look at large-scale data sets. Then another opportunity arose in 1978, this time from Bill Hill, a former student of Wisconsin who worked for a chemical company concerned about ozone, mainly the effects of CFC on the ozone layer. I thought this was a real life project but I was overwhelmed by other commitments. It would be nice if some young people could get involved in it. I first talked to Jeff Wu. He thought about it for one night, came back the next day, and said: "George, if you cannot afford doing it full time, neither can I!" Then I talked to Greg Reinsel, and it has lasted for the last 20 years. It is now getting more and more interesting, and it has a lot of important implications.

One of the unique features of this project is that it has two groups of people working together. There is a group of atmospheric scientists who know all the science and build sophisticated mathematical models. Then there are statisticians like us who approach the problem from the data. The modelers used to call us the bottom-line or end-of-the-tunnel analysis. What's interesting is that by looking at the data, we sometimes come up with features that they've never thought about. They usually started out with a hypothesis which they would like us to check from the data. When we found something new, they adjusted their models, and these things kept coming back and forth. We provide the empirical side for them to adjust and to modify their models. New results and phenomena are found this way. It's kind of mutually adaptive; they rely on us, and we rely on them. It's very different from social science where we would use the data directly to fit a model. I really think that the same thing could go for economists. It's not enough to have



1974–1975, at the Air Pollution Data Analysis Laboratory, Department of Statistics, University of Wisconsin.

economic theory and look for data to test it. In their training, economists should do more about looking at data and from there to propose new hypotheses and theory. It's here how transformation and all these things could help them. That's my practical view about statistics.

Recently, due to the advent in computing technology, there have been a lot of discussions in data mining. What is your opinion about its relevance to econometrics and statistics?

To me, data mining is absolutely essential. People always criticize correlations and have standard jokes about correlation happening at the wrong place. Think

Boulder, 1987, with Greg Reinsel, a former student, Daming Xu, and atmospheric scientists from the National Oceanic and Atmospheric Administration (NOAA).

about what would have happened if we didn't do correlation analysis. That would be terrible. A lot of new things in science and medical research would not have been discovered. From data mining you discover new phenomena, and that may lead a substantive area to new theory.

One last question I have is about mathematical finance. It is now a well-recognized discipline. Many mathematicians and probabilists have contributed to its development. Statisticians, however, have played a marginal role so far. What do you think is the reason for this lack of participation from statisticians?

I don't know the real reason. On the empirical side, we get involved in different capacities such as looking at stock returns, building models for volatility, etc. But I think the option pricing fantasy began with the celebrated Black–Scholes formula, which is a mathematical device. That may be the reason why more mathematicians engaged in this field at the beginning. It's a recent phenomenon, and its history is relatively short compared with other traditional statistical topics. There is a cultural gap in the background. Traditional statisticians usually don't get much training in finance. They may know something about econometrics, but not finance per se. Topics like survival analysis or sequential trials come in more naturally for statisticians. But I think this situation will change rapidly in the future. We have now more statisticians working in this area. For example, Per Mykland here at Chicago, your department at Carnegie Mellon started the Computational Finance Program. When you combine the idea of option pricing with data, you'll see more statistical contents. Here in Chicago, we get involved in students' theses on this subject from different aspects: data analysis, models building, risk assessments, and so forth. It's a rich and fruitful area for statisticians, and I'm sure it will be one of the most promising areas for statistics in the next 5 to 10 years.

Another important area for statistics in the business school is marketing. Now with all these scanner data, we get all kinds of information about spending habits and how would that be affected by advertising and sales promotions. This is a gold-mine for statistical analysis. To me, this is an area as important as finance. One good thing about marketing is that you can get a pretty good fit. We know people will buy when prices come down!

5. TEACHING, STUDENTS, AND SUPERVISION

Shall we talk about teaching? What is your general view about education in statistics? Where do you see us going?

I have absolute faith in the importance of statistics. It's the information age, and decisions are based more and more on data and the computer. More people will take courses in statistics, and its use will be more widespread. We should have a very bright future. The unfortunate thing is that when we look at the membership of the ASA, it has not been growing accordingly. When we look at the statistics departments, we see an increase in the enrollment of service courses, not in majoring courses. This is an interesting phenomenon, and it is not just limited to the United States. It happens in Taiwan, China, everywhere. In China, they encounter three problems. The first one is that it is very difficult to get good students majoring in statistics. The second one is that the statistics program is usually too difficult for the students; and the third one is that students don't get good jobs after graduating with a statistics degree!

It's more like a self-fulfilling prophecy!

That's right. So my feeling has always been that it's the way we train our students. If we want to see our profession grow, we must reevaluate our strategy in the masters program. This is the place from which we sent most of our students to government and to industry. If we succeed in getting a bigger base, then we'll have a better chance of getting more incoming Ph.D. students. How can we do that? It's very simple. Just imagine a banker who gets all these smart statistics master degree students coming to apply for a job. Compare these applications with a student from economics who has a couple of courses in statistics. The banker would think the students from statistics must be very smart since they have a mathematics major and have all these powerful statistical methods courses in their masters program. But it will probably be at least six months before they can communicate. It would be much better to go for an economics person who has some econometric courses and is able to do real statistics right away.

What we should do is to institute a minor requirement in our masters program by requiring a student to take one area of application. If a student succeeds in mastering a minor subject, he can, first of all, stand a better chance of finding a job. Moreover, if he has some experience in an area of application, he can probably branch out to other areas when needed. We'll probably have to trim down some of the requirements in theoretical statistics, but I think the payoff outweighs the cost. One successful example of this model is Columbia, where their masters program increased from a handful of students to more than 70 students within two years. They are now implementing a similar program like this in Taiwan.

Would we be running the risk of dictating a student's career prematurely? For example, if a student is going to the pharmaceutical industry, having a minor in economics may not be as useful as a minor in biology.

If a student has already decided his career interest, then he can choose the minor subject he prefers. But in most cases, for mathematics students with no substantive application background, going through the masters program by learning more mathematics and statistics won't equip them with better ideas about applications. The problem is that if a student doesn't know much about a substantive area, it's very difficult for him to do any in-depth data analysis. He has to know some background of the application area. Students come to statistics because they want to learn statistical methods that can help them find better jobs. I really think the masters education is crucial for our profession.

Throughout the years, you have directed a number of Ph.D. students; many of them later became prominent figures in their own fields. If you are to offer advice to potential supervisors, what would you like to tell them? Likewise, do you have any general advice for prospective Ph.D. students?

I work with my students quite a bit, I mean, a lot. Maybe that's the tradition that I learned from Box; that's the way he operates. I work very hard for long hours and try to be encouraging. Because of that, I can regard many of their problems as part of my work. I consider myself to be quite lucky since most of the students end up quite well. Most young people are quite enthusiastic and hard working. They work hard probably because they think I work very hard on the problems, too.

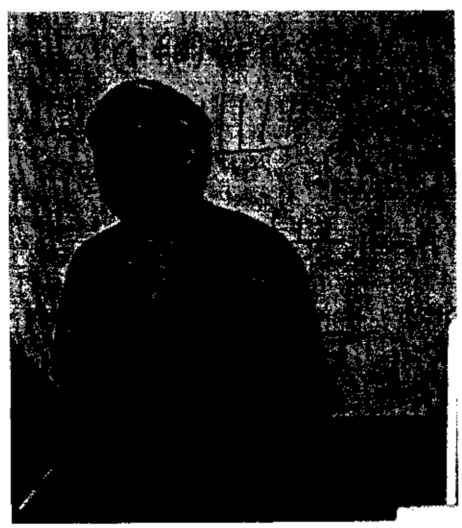
For prospective Ph.D. students, I have similar advice as to the masters program. Try to get into an application area as early as possible. That doesn't prevent one from doing other applications later. In my case, because of my background in economics, if I want to develop a new statistical methodology, I would often go back to economics to see if it can be applied there. If it works, I would feel very happy about it. It helps me psychologically to realize that I'm developing things of some uses. To me, this is very important although it might be totally irrelevant to a pure mathematician.

As I said earlier, the proverb that statistics goes hand in hand with practice is highly relevant. Statistics works as a tool for scientific investigation. Because it is a tool, we need to find the application to motivate it. By anchoring into a ıbstantive application area, we get into the habit of thinking about the motiva-on, and that is very important for the development of statistics. Look at Fisher; ıany of his works were motivated by his interest in genetics. Same goes for Box, ·hose background is chemistry. During World War II, he spent seven years in a ɔison gas research unit before he returned to college, and by then, he had already ·arned most of Fisher's designs. It became a habit for them to look for problems ıd inspirations from a practical world. I think this kind of mixed background ·ould carry a long way in statistical research.

DEVELOPMENTS IN ASIA

You have been involved with the developments in statistics in Asia ex-ɜnsively. You are instrumental in establishing the Institute of Statistics in ɜiwan, developing statistics and econometrics programs for many univer-ities in China, Hong Kong, Taiwan, as well as other Asian countries. With ɿe rapid economic developments in this region, what kind of general irections should these institutes take in the future?

here can be plenty of developments for the use of statistics in Asia. Take Hong :ong, for example. Because of its unique investment environment, there is a ood potential for the development of business statistics, something similar to ·hat we did at Wisconsin in the late 1960's. If one can establish a program in :atistics which encourages a student to go into a business area and get motivated



Lecture in Beijing, 1997, at People's University of China.

by practical problems from that area, there is every good reason to expect such a program will flourish.

They are trying to do something like that. One of the difficulties is to get good students to enroll in the business school and keep them from going away. As you explained earlier, there are some intrinsic difficulties in developing statistics within the business school, but I hope some kind of joint venture with statistics departments can be materialized. What about China and Taiwan?

I don't know what to say because China is so big. But the idea about a masters program with a substantive minor may be the way to go. In China, almost all the students in statistics are coming from mathematics. These are real mathematics majors, and all they know in their entire lives is mathematics. A substantive area of application becomes really crucial for their training. Otherwise, they'll all think like mathematicians, for example, keep on proving theorems. In China, their thinking is very different, and getting them to do data analysis is difficult. Their computer facilities are no comparison to what we have today, although much better than years ago. One can't build Rome overnight. The first thing to try is to have more courses taught in applied statistics. Even that could be a very big challenge. I went there in the mid-1980's and instituted an examination in statistics together with other colleagues like Peter Bickel, Don Rubin, Gouri Bhattacharyya, and Jeff Wu. The purpose of the examination was to establish a standardized measurement, like a GRE score in statistics, for graduate applicants from China. This test consisted of two parts, one in theory and one in applied. All we did was to contribute problems in applied statistics. Because of that, many universities were forced to teach applied statistics courses. I think the impact on that turns out to be more significant than just getting a few capable students coming to the United States to study.

In China, they also realize the importance of education in statistics. Their State Statistical Bureau is very eager to educate their employees and the public. The difficulty they face is that instructors don't communicate well with one another. It's just like us in the 1960's, but worse. In the United States, because we change the way we think and teach statistics, we're now seeing statistics getting into every discipline, law, environment, biology, etc. For China, I think applied statistics with methods and applications is the way to go.

Taiwan is probably the most developed in statistics. First, almost all of the statisticians there are trained in the United States. Whatever happens here gets transferred to Taiwan quickly. Also, since many of us have been visiting Taiwan on a regular basis, academic interactions among statisticians between these two places are strong. The Institute of Statistics is doing very well, with C.Z. Wei being the current director. The journal *Statistica Sinica* is also building up its reputation. Second, they have a lot of resources to implement new ideas. I think this has something to do with the budget director of the government being a statistician!

July 1988, with M.T. Chao (director of Institute of Statistical Science, Academia Sinica, Taipei), signing agreement to establish *Statistica Sinica*.

7. GENERAL

We've pretty much covered all the bases. Maybe you would tell us something about yourself.

I'm 64 years old now. I'm a pretty simple-minded and optimistic person. What do you want to know?

You're a well-known international traveler. Rumor has it that if one can't reach George on the phone, try his number at the other end of the geodesic! How do you manage all these travels? What do you do to adjust for these time changes?

I sort of get used to it. I sleep very well on the plane. My students used to laugh at me by saying that "When George gets on the plane, as soon as the plane leaves the gate, he's gone." I usually don't realize that the plane has taken off. So the time shift doesn't pose a serious problem. If I fly to Europe, I can go to work right away because I arrive there in the afternoon. When I go to Asia, it's getting more difficult because I usually arrive in the evenings. I struggle a little bit and try to get a good rest for the night. That's how I manage nowadays. I'm not young anymore.

That's very interesting. When you're at home in Chicago, what's your typical schedule?

I usually come to the office around 9:30 in the morning. I do most of the business at school during daytime and go home about 5:30 to watch the PBS news; it's the only television program I watch regularly. It's one hour of news with analysis, and I usually have dinner at the same time. After that I go to sleep, and then I get up at 8 P.M. and I work until about 1 A.M. At night, I do a lot of business with Taiwan and China. It's very convenient because that's their daytime. I have the communication infrastructure like fax machines, phone lines, and computers all set up at home. That's my daily life; it has been like that for many years.

It has been wonderful to speak with you on various things. On behalf of *ET*, may I thank you for your precious time and giving us this opportunity to learn more about yourself and your insights.

Thank you for offering me this interview.

REFERENCES

Baumol, W. (1951) *Economic Dynamics.* New York: Macmillan.

Box, G.E.P., G.M. Jenkins, & G.C. Reinsel (1994). *Time Series Analysis: Forecasting and Control,* 3rd ed. Englewood Cliffs, New Jersey: Prentice-Hall.

Box, G.E.P. & G.C. Tiao (1973) *Bayesian Inference in Statistical Analysis.* Reading, Massachusetts: Addison-Wesley.

Jeffreys, H. (1961) *Theory of Probability.* Oxford: Oxford University Press.

Mood, A. (1952) *Introduction to the Theory of Statistics.* New York: McGraw-Hill.

Savage, L.J. (1954) *The Foundations of Statistics.* New York: Wiley.

PUBLICATIONS OF GEORGE C. TIAO

BOOKS

1973

1. With G.E.P. Box. *Bayesian Inference in Statistical Analysis.* Reading, Massachusetts: Addison-Wesley.

1978

2. Co-editor, with D.R. Brillinger. *Directions in Time Series, Proceedings of the IMS Special Topics Meeting on Time Series Analysis,* May. Institute of Mathematical Statistics.

1984

3. Editor. *The Collected Works of G.E.P. Box,* vols. 1 and 2. Wadsworth.

1995

4. Co-editor, with N.G. Polson. *Bayesian Inference.* The International Library of Critical Writings in Econometrics. Hants, UK: Edward Elgar Publishing, Ltd.

PAPERS

1962

1. With G.E.P. Box. A further look at robustness via Bayes' theorem. *Biometrika* 49, 419–432.

1964

2. With G.E.P. Box. A note on criterion robustness and inference robustness. *Biometrika* 51, 169–173.
3. With G.E.P. Box. A Bayesian approach to the importance of assumptions applied to the comparison of variances. *Biometrika* 51, 153–167.
4. With A. Zellner. Bayes' theorem and the use of prior knowledge in regression analysis. *Biometrika* 51, 219–230.
5. With A. Zellner. On the Bayesian estimation of multivariate regression. *Journal of the Royal Statistical Society, Series B* 26, 277–285.
6. With A. Zellner. Bayesian analysis of the regression model with autocorrelated errors. *Journal of the American Statistical Association* 59, 763–778.
7. With A. Guttman. A Bayesian approach to some best population problems. *Annals of Mathematical Statistics* 35, 825–835.
8. With I. Guttman. Some useful matrix lemmas in statistical estimation theory. *Canadian Mathematics Bulletin* 7, 279–300.

1965

9. With W.Y. Tan. Bayesian analysis of random-effect models in the analysis of variance: I. Posterior distribution of variance components. *Biometrika* 52, 37–54.
10. With I. Guttman. The multivariate inverted beta distribution with applications. *Journal of the American Statistical Association* 60, 793–805.
11. With G.E.P. Box. A change in level of a non-stationary time series. *Biometrika* 52, 181–192.
12. With G.E.P. Box. Multiparameter problems from a Bayesian point of view. *Annals of Mathematical Statistics* 36, 1468–1482.

1966

13. With W.Y. Tan. Bayesian analysis of random-effect models in the analysis of variance: II, Effect of autocorrelated errors. *Biometrika* 53, 477–495.

1967

14. Bayesian comparison of means of a mixed model, with application to regression analysis. *Biometrika* 54, 109–125.
15. With G.E.P. Box. Bayesian analysis of a three-component hierarchical design model. *Biometrika* 54, 109–125.
16. With I. Guttman. Analysis of outliers with adjusted residuals. *Technometrics* 9, 541–549.
17. With R. Lochner. Tables for the comparison of two normal variances. *Biometrika* 54, 683–684.

1968

18. With G.E.P. Box. A Bayesian approach to some outlier problems. *Biometrika* 55, 119–130.
19. With G.E.P. Box. Bayesian estimation of means in the random effect model. *Journal of the American Statistical Association* 63, 174–181.
20. With N.R. Draper. Bayesian analysis of linear model with 2 random components. *Biometrika* 55, 101–118.

1969

21. With S. Fienberg. Estimation of the latent roots and vectors with special reference to the bivariate normal distribution. *Biometrika* 56, 97–108.

1970

22. With D. Lund. The use of OLUMV estimators in inference robustness studies of the location parameters of a class of symmetric distributions. *Journal of the American Statistical Association* 65, 370–386.

1971

23. With M.M. Ali. Effect of non-normality on inferences about variance components. *Technometrics* 13, 635–650.
24. With M.M. Ali. Analysis of correlated random effect: Linear model with two random components. *Biometrika* 58, 37–51.
25. With H.E. Thompson. Analysis of telephone data: A case study of forecasting seasonal time series. *Bell Journal of Economics and Management Science* 2, 515–541.
26. With J.E. Glass & T.A. Maguire. Analysis of data on the revision of German divorce laws: Analysis of data as a time series quasi-experiment. *Law and Society Review* 5, 539–562.

1972

27. Asymptotic behavior of temporal aggregates of time series. *Biometrika* 59, 525–531.

1973

28. With G.E.P. Box. Some comments on "Bayes" estimators. *American Statistician* 27 (1), 12–14.

1975

29. With G.E.P. Box. Intervention analysis with applications to environmental problems. *Journal of the American Statistical Association* 70, 70–79 (invited paper).
30. With D.J. Pack. Modelling the consumption of frozen concentrated orange juice: A case study of time series analysis. Academic Economic Papers, *Academia Sinica* 3, 1–33.
31. With G.E.P. Box. & W.J. Hamming. Analysis of Los Angeles photochemical smog data: A statistical overview. *Journal of the Air Pollution Control Association* 25 (March), 260–268.
32. With G.E.P. Box & W.J. Hamming. A statistical analysis of the Los Angeles ambient carbon monoxide data. *Journal of the Air Pollution Control Association* 25 (November), 1129–1136.

1976

33. With B. Afonja. Some Bayesian considerations of the choice of design for ranking, selection and estimation. *Annals of the Institute of Statistical Mathematics* 28, 167–185.
34. With W.C. Cleveland. Decomposition of seasonal time series: A model for the Census X-11 program. *Journal of the American Statistical Association* 71, 581–587.
35. With M.S. Phadke & G.E.P. Box. Some empirical models for the Los Angeles photochemical smog data. *Journal of the Air Pollution Control Association* 26 (May), 485–490.
36. With M.N. Wang, K.S. Yang, L.M. Lin, Y.C. Liu, & C.T. Shieh. Review of the labor force statistics in ROC. *Journal of the Chinese Statistical Association* 14, 5344–5351.
37. With G.E.P. Box. Comparisons of forecasts and actuality. *Applied Statistics* 25, 195–200.
38. With W.S. Wei. Effect of temporal aggregation on the dynamic relationship of two time series variables. *Biometrika* 63, 513–524.

1977

39. With G.E.P. Box. A canonical analysis of multiple time series. *Biometrika* 64, 355–366.
40. With W.Y. Tan & Y.C. Chang. Some aspects of bivariate regression subject to linear constraints. *Journal of Econometrics* 5, 13–35.
41. With M.S. Phadke & G.E.P. Box. Empirical-mechanistic modelling of air pollution. In *Proceedings of the 4th Symposium on Statistics and the Environment*, pp. 91–100. Washington, D.C.: ASA.
42. With G.E.P. Box. Applications of time series analysis. In H.A. David (ed.), *Contribution to Survey Sampling and Applied Statistics—Papers in Honor of H.O. Hartley*, pp. 203–219. New York: Academic Press.
43. With S.A. Hillmer. Statistical analysis of the Los Angeles Catalyst Study data—rationale and findings. *The Los Angeles Catalyst Study Symposium*, pp. 415–460. EPA-600/4-77-034. (Reprinted in *Environmental Science and Technology* 12, 820–827 [1978] under the title "Statistical models for ambient concentrations of carbon monoxide, lead and sulfate based on the LACS data.")

1978

44. With G.E.P. Box & S.A. Hillmer. Analysis and modelling of seasonal time series. In A. Zellner (ed.), *Seasonal Analysis of Economic Time Series*, pp. 309–333. Bureau of the Census, Department of Commerce.
45. With S.A. Hillmer. Some consideration of decomposition of a time series. *Biometrika* 65, 497–502.
46. With I. Guttman. Effect of correlation of the estimation of a mean in the presence of spurious observations. *Canadian Journal of Statistics* 6, 229–247.
47. With M.S. Phadke & M. Grupe. Statistical evaluation of the trends in the ambient concentration of nitric oxide in Los Angeles. *Environmental Science and Technology* 12, 430–435.

1979

48. With W.P. Cleveland. Modeling seasonal time series. In *Revue Economie Appliquee* 32, 107–129. (Special volume on seasonal adjustment and time series problems.)
49. With S.A. Hillmer. Likelihood function of stationary multiple autoregressive moving average models. *Journal of the American Statistical Association* 74, 652–660.
50. With J. Ledolter. Statistical models for ambient air pollutants, with special reference to the LACS data. *Environmental Science and Technology* 13, 1233–1240.
51. With J. Ledolter. A statistical analysis of New Jersey CO data. In *Proceedings of APCA ASQC Conference on Quality Assurance in Air Pollution Measurement*, pp. 282–293.

1980

52. With S.A. Hillmer. Smoothing of time series from a Bayesian viewpoint. In A. Zellner (ed.), *Bayesian Analysis in Econometrics and Statistics: Essays in Honor of Harold Jeffreys*, pp. 271–280. Amsterdam: North-Holland.
53. With I. Guttman. Forecasting contemporal aggregates of multiple time series. *Journal of Econometrics* 12, 219–230.
54. With M.R. Grupe. Hidden periodic autoregressive-moving average models in time series data. *Biometrika* 62, 365–373.
55. With L.M. Liu. Parameter estimation in dynamic models. *Communications in Statistics* A9 (5), 501–517.
56. With L.M. Liu. Random coefficient first order autoregressive models. *Journal of Econometrics* 13, 305–325.

1981

57. With G. Reinsel, M.N. Wang, R. Lewis, & D. Nychka. Statistical analysis of stratospheric ozone data for the detection of trend. *Atmospheric Environment* 15, 1569–1577.
58. With G. Reinsel & R. Lewis. Statistical analysis of stratospheric ozone data for trend detection. In *Proceedings of the Environmetrics '81 Conference, Washington, D.C.*, pp. 215–236.
59. With R.S. Tsay. Identification of nonstationary ARMA models. In *Proceedings of the Business and Economic Statistics Section, ASA*, pp. 308–317.
60. With G.E.P. Box. Modeling multiple time series with applications. *Journal of the American Statistical Association* 76, 802–816.
61. With S.A. Hillmer & W.R. Bell. Modeling considerations in seasonal adjustment of economic time series. In *Proceedings of the ASA-Census Conference on Applied Time Series Analysis of Economic Data*, pp. 74–100. ER-5, Bureau of the Census.

1982

62. With S.A. Hillmer. An ARMA-model-based approach to seasonal adjustment. *Journal of the American Statistical Association* 77, 63–70.
63. With G. Reinsel & R. Lewis. A statistical analysis of total ozone data from the Nimbus-4 BUV satellite experiment. *Journal of Atmospheric Sciences* 39, 418–430.
64. With J. Ledolter & G.B. Hudak. Statistical analysis of the effect of car inspection and maintenance programs on the ambient CO concentrations in Oregon. *Environmental Science and Technology* 16, 328–335.

1983

65. With G. Reinsel, R. Lewis, & M. Bobkoski. Analysis of upper stratospheric ozone profile data from the ground based Umkehr method and the Nimbus-4 BUV satellite experiment. *Journal of Geophysical Research* 88, 5393–5402.
66. With R.S. Tsay. Consistency properties of least squares estimates of autoregressive parameters in ARMA models. *Annals of Statistics* 11, 856–871.
67. With R.S. Tsay. Multiple time series modeling and extended sample cross correlations. *Journal of Business and Economic Statistics* 1, 43–56.
68. Use of statistical methods in the analysis of environmental data. *American Statistician* 37, 459–470.

1984

69. With R.S. Tsay. Consistent estimates of autoregressive parameters and extended sample autocorrelation function for stationary and nonstationary ARMA models. *Journal of the American Statistical Association* 79, 84–96.
70. With J. Ahtola. Parameter inference for a nearly nonstationary first order autoregressive model. *Biometrika* 71, 263–272.
71. With G. Reinsel, J.J. DeLuisi, C.L. Mateer, A.J. Miller, & J.E. Frederick. Analysis of upper stratospheric Umkehr ozone profile data for trends and the effects of stratospheric aerosols. *Journal of Geophysical Research* 89, 4833–4840.
72. With J. Ahtola. Some aspects of parameter inference for nearly nonstationary and nearly noninvertible ARMA models II. *Questuo* 8, 155–163.

1985

73. With R.S. Tsay. Use of canonical analysis in time series model identification. *Biometrika* 72, 299–315.

74. Autoregressive moving average models, intervention problems and outlier detection in time series. In E.J. Hannan, P.R. Krishnaiah, & M.M. Rao (eds.), *Handbook of Statistics*, vol. 5, pp. 85–118. New York: Elsevier.

1986

75. With G. Reinsel, J. Pedrick, G. Allenby, C.L. Mateer, A.J. Miller, & J.J. DeLuisi. A statistical trend analysis of ozonesonde data. *Journal of Geophysical Research* 91, 13121–13136.

1987

76. With J. Ahtola. Distribution of autoregressive parameters with complex roots on the unit circle. *Journal of Time Series Analysis* 8, 1–14.
77. With J. Ahtola. A note on asymptotic inference in autoregressive models with roots on the unit circle. *Journal of Time Series Analysis* 8, 15–19.
78. With G. Reinsel, A. Miller, D. Wuebbles, P. Connell, C. Mateer, & J. DeLuisi. Statistical analysis of total ozone and stratospheric Umkehr data for trends and solar cycle relationship. *Journal of Geophysical Research* 92, 2201–2209.
79. With G. Reinsel. Impact of chlorofluoromethanes on stratospheric ozone. *Journal of the American Statistical Association* 82, 20–30.

1988

80. With I. Chang & C. Chen. Estimation of time series in the presence of outliers. *Technometrics* 30, 193–204.
81. With G. Reinsel, S. Ahn, M. Pugh, S. Basu, J. DeLuisi, A. Miller, P. Connell, & D. Wuebbles. An analysis of the seven year record of SBUV satellite ozone data: Global profile features and trends in total ozone. *Journal of Geophysical Research* 93, 1689–1703.
82. With G. Easton & H.V. Roberts. Making statistics more effective in schools of business conference report. *Journal of Business & Economic Statistics* 6, 247–260.

1989

83. With R.S. Tsay. Model specification in multivariate time series. *Journal of the Royal Statistical Society Series B* 51, 157–213 (With discussion.)
84. With L.M. Liu & G. Hudak. A statistical assessment of the effect of the Arizona car inspection/maintenance program on ambient CO air quality in Phoenix, Arizona. *Environmental Science and Technology* 23, 806–814.
85. With G. Reinsel, J. DeLuisi, S. Bsu, & K. Carriere. Trend analysis of aerosol-corrected Umkehr profile ozone data through 1987. *Journal of Geophysical Research* 94, 16373–16386.

1990

86. With C. Chen. Random level shift time series models, ARMA approximations, and level-shift detection. *Journal of Business and Economic Statistics* 8, 83–97.
87. With R.S. Tsay. Asymptotic properties of multivariate nonstationary processes with applications to autoregressions. *Annals of Statistics* 18, 220–250.
88. With R. Bojkov, L. Bishop, W.J. Hill, & G. Reinsel. A statistical trend analysis of revised Dobson total ozone data over the northern hemisphere. *Journal of Geophysical Research* 95, 9785–9807.
89. With G. Reinsel, J. Pedrick, D. Xu, A. Miller, J. DeLuisi, C. Mateer, & D. Wuebbles. Effects of autocorrelations and temporal sampling schemes on estimates of trend and spatial correlation. *Journal of Geophysical Research* 95, 20507–20517.

1991

90. With D. Pena. A note on likelihood estimation of missing values in time series. *American Statistician* 45, 212–213.
91. With C. Chen, L.M. Liu, & R.S. Tsay. Outlier and intervention analysis in dynamic regression model. *Journal of the Chinese Statistical Association* 29, 1–29.

1992

92. With D. Pena. Bayesian robustness functions for linear models. In J.M. Bernardo, J.O. Berger, A.P. Dawid, & A.F.M. Smith (eds.), *Bayesian Statistics*, vol. 4, pp. 365–388. Oxford: Oxford University Press. (With discussion.)
93. With A.J. Miller, R.N. Nagatani, X.F. Niu, G.C. Reinsel, D. Wuebbles, & K. Grant. Comparison of observed ozone and temperature trends in the lower stratosphere. *Geophysical Research Letters* 19, 929–932.
94. With X.F. Niu, J.E. Frederick, & M.L. Stein. Trends in column ozone based on TOMS data: Dependence on month, latitude and longitude. *Journal of Geophysical Research* 97, 14661–14669.

1993

95. An introduction to multiple time series. *Medical Care* 31, YS71-YS74. (Supplement.)
96. With D. Xu. Robustness of MLE for multi-step prediction: The exponential smoothing case. *Biometrika* 80, 623–641.
97. With R.S. Tsay & T.C. Wang. Usefulness of linear transformations in multivariate time series analysis. *Empirical Economics* 18, 567–593.
98. With X.F. Niu. Space-time models for the analysis of satellite temperature and ozone data. In *Proceedings of the 24th Symposium on the Interface, Computer Science and Statistics*, pp. 85–94.

1994

99. With C. Inclan. Use of cumulative sums of squares for retrospective detection of changes of variance. *Journal of the American Statistical Association* 89, 913–923.
100. Time series analysis. In *McGraw-Hill Encyclopedia of Economics*, 2nd ed., pp. 987–991.
101. With G.C. Reinsel, D.J. Wuebbles, J.B. Kerr, A.J. Miller, R.M. Nagatani, L. Bishop, & L.H. Ying. Seasonal trend analysis of published ground-based and TOMS total ozone data through 1991. *Journal of Geophysical Research* 99, 5449–5464.
102. With R.S. Tsay. Some advances in non-linear and adaptive modeling in time series analysis. *Journal of Forecasting* 13, 109–131.

1995

103. With X. Niu. Modelling satellite ozone data. *Journal of the American Statistical Association* 90, 969–983.
104. With A.J. Miller, G.C. Reinsel, D.J. Wuebbles, L. Bishop, J. Kerr, R.M. Nagatani, J.J. DeLuisi, & G. Mateer. Comparison of observed ozone trends in the stratosphere through examination of Umkehr and balloon ozonesonde data. *Journal of Geophysical Research* 100, 11209–11217.
105. With A.J. Miller, R.M. Nagatani, X.F. Niu, G.C. Reinsel, D.J. Wuebbles, K. Grant, L. Bishop, J.B. Kerr, W. Planet, & R. McPeters. Trends of stratospheric ozone and temperature. In *Diagnostic Tools in Atmospheric Physics, Proceedings of the International School of Physics*, pp. 261–278. Ohmsha: IOS Press.

1996

106. With G.C. Reinsel. Statistical assessment of atmospheric ozone data for prediction. In J.C. Lee, W.O. Johnson, & A. Zellner (eds.), *Modelling and Prediction Honoring Seymour Geisser*, pp. 423–440. New York: Springer-Verlag.
107. With A.J. Miller, S.M. Hollandsworth, L.E. Flynn, G.C. Reinsel, L. Bishop, R.D. McPeters, W. Planet, J.J. DeLuisi, G. Mateer, D.J. Wuebbles, J. Kerr, & R.M. Nagatani. Comparison of observed ozone trends and solar effects in the stratosphere through examination of ground-based Umkehr and combined SBUV, SBUV/2 satellite data. *Journal of Geophysical Research* 101, 9017–9021.

1997

108. With E.C. Weatherhead, G.C. Reinsel, J.E. Frederick, J.J. DeLuisi, D.S. Choi, & W.K. Tam. Analysis of long-term behavior of ultraviolet radiation measured by Robertson-Berger meters at 14 sites in the U.S. *Journal of Geophysical Research* 102, 8737–8754.
109. With A.J. Miller, L.E. Flynn, S.M. Hollandsworth, J.J. DeLuisi, I.V. Petropavlovskikh, G.C. Reinsel, D.J. Wuebbles, J. Kerr, R.M. Nagatani, L. Bishop, & C.H. Jackman. Information content of Umkehr and solar backscattered ultraviolet (SBUV) 2 satellite data for ozone trends and solar responses in the stratosphere. *Journal of Geophysical Research* 102, 19257–19263.

1998

110. With R.S. Tsay, K.S. Man, Y.J. Chu, K.K. Xu, C. Chen, J.L. Lin, C.M. Hsu, C.F. Lin, C.S. Mao, C.S. Ho, R.W. Liou, & Y.F. Yang. A time series approach to econometric models of Taiwan's economy. *Statistica Sinica* 8, 991–1044.
111. With A.L. Montgomery, V. Zarnowitz, & R. Tsay. Forecasting the U.S. unemployment rate. *Journal of the American Statistical Association* 93, 478–493.

COMMENTS

1967

1. Contribution to discussion of "Topics in the investigation of linear relations fitted by the method of least squares," by F.J. Anscombe. *Journal of the Royal Statistical Society, Series B* 29, 44–47.

1976

2. With G.E.P. Box. The case of the missing nuisance parameter. Contribution to the discussion "Strong inconsistency from uniform priors," by M. Stone. *Journal of the American Statistical Association* 71, 122.

BOOK REVIEWS

1967

1. *Inference and Disputed Authorship: The Federalist*, by F. Mosteller & D.L. Wallace, *Journal of the American Statistical Association* 62, 306–309.

1968

2. *Applied Regression Analysis*, by N.R. Draper & H. Smith. Review of the International Statistical Institute.

TECHNICAL REPORTS

1974

1. With D.J. Pack. A Class of Models for Dependent Binary Data. Technical report 388, Department of Statistics, University of Wisconsin, Madison.

1983

2. With R.S. Tsay. Identification of Multiplicative ARMA Models for Seasonal Time Series. Technical report 7, Statistics Research Center, Graduate School of Business, University of Chicago.

COMPUTER PACKAGE FOR MULTIPLE TIME SERIES ANALYSIS

1979

1. With G.E.P. Box, M.R. Grupe, G.B. Hudak, W.R. Bell, & I. Chang. The Wisconsin Multiple Time Series (WMTS-1) Program: A Preliminary Guide. Department of Statistics, University of Wisconsin, Madison. An extensive computer package for the analysis and modeling of multiple time series.

SPECIAL REPORTS FOR AIR POLLUTION DATA ANALYSIS PROJECT

1973

1. With G.E.P. Box, M. Grupe, S.T. Liu, S. Hillmer, & W.S. Wei. Los Angeles Aerometric Ozone Data 1955–1972. Technical report 346, Department of Statistics, University of Wisconsin, Madison.

1974

2. With M.S. Phadke, M. Grupe, S. Hillmer, S.T. Liu, & W. Fortney. Los Angeles Aerometric Carbon Monoxide Data 1956–1972. Technical report 377, Department of Statistics, University of Wisconsin, Madison.
3. With M.S. Phadke, M. Grupe, S.T. Liu, W. Fortney, & S. Wu. Los Angeles Aerometric Data on Oxides of Nitrogen 1957–1972. Technical report 395, Department of Statistics, University of Wisconsin, Madison.

1975

4. With M.S. Phadke, M. Grupe, S. Wu, A. Krug, & S.T. Liu. Los Angeles Aerometric Data on Sulphur Dioxide, Particulate Matter, and Sulfates. Technical report 410, Department of Statistics, University of Wisconsin, Madison.

1976

5. With M.S. Phadke & S.C. Hillmer. An Intervention Time Series Model for the Los Angeles Catalyst Data. Special report written for EPA.

1978

6. With J. Ledolter, G. Hudak, J.T. Hsieh, & S. Graves. Statistical Analysis of Multiple Time Series Associated with Air Quality Data: New Jersey CO Data. Technical report 529, Department of Statistics, University of Wisconsin, Madison.
7. With J. Ledolter, S. Graves, J.T. Hsieh, & G. Hudak. Statistical Analysis of the Los Angeles Catalyst Study Data. Technical report 539, Department of Statistics, University of Wisconsin, Madison.