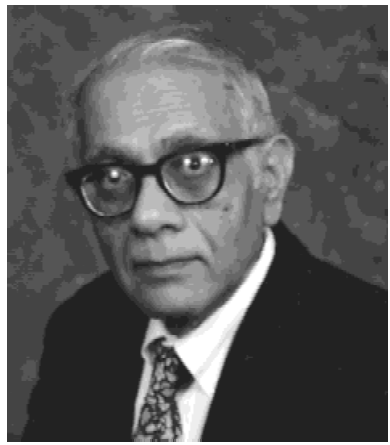
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ET INTERVIEW: PROFESSOR G.S. MADDALA

Interviewed by Kajal Lahiri
State University of New York at Albany



G.S. Maddala

Over the last three decades, G.S. Maddala (universally known as "G.S.") has been a familiar name to all doing econometric work. His substantial contributions to the discipline through numerous books and articles single him out as one of the most distinguished econometricians of our time. Because of his extraordinary ability to synthesize and exposit complex methodological results in simple intuitive terms, he has influenced econometric research in other areas of social science also. According to the *Social Science Citation Index*, G.S. has been one of the top five most cited econometricians during each of the years 1988–1993. He writes econometrics in plain English with a characteristic sense of wit and humor. There cannot be too many empirical economists around the world who have not been influenced by G.S.'s writings in some way or other. Often he has taken a critical look at evolving econometric techniques, particularly those that have no practical applications, and has not hesitated to go against the tide of the profession.

The present interview was conducted on May 1, 1999, at his home in Columbus, Ohio. G.S. has his own philosophy on what should be done in econometrics. I hope that the transcript of the conversation captures the influences that have

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shaped G.S.'s academic life and work and also brings out the salient features of his thinking. One of G.S.'s remarkable characteristics is his capacity for utter candor about matters that so many others prefer to avoid or temporize on. It won't surprise those who know G.S. well that this honesty shines through very strongly in the interview.

Perhaps you could start by telling us about your early background and how you got interested in econometrics.

I was born in 1933, the fifth of my parents' six children. I did not go to any great schools or colleges as many of you have. My father was a schoolteacher, but he believed that schooling was useless. So he lost his job every year, and we moved from place to place where he found another job. My mother was worried about my education and when I was 8 years old sent me to her brother in Bombay, which was 200 miles from where we lived. My uncle took me to a (supposedly) good school, the King George school. I was asked what grade I was seeking admission into. I said, "Fifth." I was given a test, and I scored zero. I was given a second-grade test, and I scored zero again. The headmaster of that school said to my uncle, "This boy is useless. We can't admit him in any grade. We don't take any students that will never finish high school." I told my uncle that I wanted to go home.

I went home, but I didn't want to go to school. My father thought it was all right. I stayed home for 3 years and worked on math, reading, and writing. When I was 11 years old, I joined high school (where my father was working). There was only one teacher that was good. He was teaching Sanskrit, the ancient Indian language. He wanted me to become a Sanskrit scholar and work hard to win a Sanskrit scholarship for college. When I was 14, I passed the high school examination (the same exam is given to all high school students in the state), but I did not win a Sanskrit scholarship. Since there was no college where we lived, I went to Bombay again. I joined a mediocre college that was close to where I lived. Being away from home, I was not eating properly. I became ill with tuberculosis and dropped out of college after 6 months in 1947.

The partition of India in 1947 was hard on our family. My older brother (who was working in Pakistan) died. My mother could not take the loss of my brother and my getting sick and dropping out of college. She became ill and died in 1948. My father quit work, and we went back to our native town in South India. My father started tutorial classes to teach Hindi (India's national language). I helped him in his tutorial school. In my spare time I went to the local library to read books. I read books on world history and Indian philosophy.

In 1953, a friend named Prasad told me that one could get a 2-year college degree at Ajmere (in North India) without going to college and just by taking an exam in three subjects. Since Ajmere was more than 1,000 miles away he sug-

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gested that we should go there together and take the exam. He also said that with a 2-year college degree and a Hindi diploma, one could get a teacher's job in a middle school, and this provided stable employment. We prepared for the exam. I chose history, political science, and mathematics as my three fields. A month before the exam, Prasad's father died and he couldn't go, so I went alone.

1953 was the centenary of the Indian Railways. There was a 1-month pass for 30 rupees ($6 at the exchange rate then) that allowed unlimited travel. There was also a railway exhibition in Delhi. I went to Ajmere and took my exam. Then I went to Delhi to see the railway exhibition and then on to Agra to see the Taj Mahal.

The results of the exam were to be announced in *Hindustan Times* in Delhi. I had no access to that paper, but my sister in Bombay promised to look for them. The newspaper also printed the names of the top 10 candidates in the exam. My sister saw that my name was listed first and that I won a gold medal. She sent me a telegram and suggested that I should go to a college. I was skeptical because I was a college dropout for 6 years, and all my classmates were far ahead.

I joined a local college (a mediocre one) and was advised to major in math. I completed my B.A. in 1955 standing first in the university and won two gold medals. I then went again to Bombay to do my master's in statistics.

I am curious about one thing. How did you get the idea of doing statistics? I am sure in 1955 pursuing an advanced degree in statistics was not very common.

After my B.A. there was only one choice—to do my master's in pure math. But my professor in college said that the prospects in India were not good for those with advanced degrees in pure math and that prospects were excellent for those in statistics. I could have gone to the Indian Statistical Institute, but I had no relatives in Calcutta. I decided to go to Bombay because I had relatives there. I finished my M.A. in 1957 with a first class.

How did you get the idea of coming to the United States to pursue doctoral studies? In the late 1950′s with a first class degree in statistics you could have obtained attractive job offers. Also, what prompted you to join the economics Ph.D. program in Chicago?

I decided to come to the United States because everyone in my class in the statistics department in Bombay went to the United States for a Ph.D. I was awarded an assistantship at the University of California at Berkeley (Department of Statistics), but I could not accept it because I had no money to travel to the United States. So I stayed in India doing some teaching jobs. In 1960, I won a Fulbright Fellowship which paid for both travel to the United States and an assistantship for 1 year. When I was ready to go to Berkeley, the director of the Bombay School of Economics asked me to switch to economics. He said that with a Ph.D. in statistics I would get only a lectureship (an assistant professor job) but that if I did a

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Ph.D. in economics (and came back in 3 years) he would give me a full professorship. I thought it was a good idea. He suggested (I don't know why) that I should go to the University of Chicago (not U.C. Berkeley).

That is how I switched to economics and ended up at the University of Chicago. It was just the job offer. I didn't know anything about the economics department at Chicago vs. Berkeley. I knew a lot about the statistics department at Berkeley. I had met Jerzy Neyman while doing my master's in Bombay.

Can you say something about your Chicago days? Who were your classmates, and what was the experience like?

At Chicago, Milton Friedman was the star performer at the seminars. Everyone was scared of him. It was fun having him there. My class turned out to be perhaps the best ever at Chicago, but I never knew it and nobody imagined it at the time. Among my classmates was Bob Lucas, who won a Nobel Prize (he along with all others got a "B" grade in Friedman's course!). The first paper I wrote was with Zvi Griliches, Bob Lucas, and Neil Wallace. Besides Bob Lucas, two others among my classmates (Sherwin Rosen and Sam Peltzman) are professors at Chicago. Six in my class became fellows of the Econometric Society (myself, Bob Lucas, Neil Wallace, Sherwin Rosen, Giora Hanoch, and Finis Welch), and there was no econometrician at Chicago to speak of except Zvi Griliches. Hans Theil and Arnold Zellner joined the department 1 year after we left.

Econometrics was very low keyed. Milton Friedman was a great believer in OLS. That's all every student (including myself) used in their theses. The one thing I learned at Chicago was to ask the "right" questions.

For my thesis, I was advised to work with Bob Basmann. When I saw him for the thesis topic, he suggested that I work on proving the nonexistence of moments of 2SLS in a three equation four variable model. I wasn't too excited by that topic but asked him how long it would take to get my Ph.D. He said it is a difficult problem and that it would take 3 to 4 years. I said that I had to go back to India in 2 years. I then went to Zvi Griliches. I am glad I did because he did not fly into fancy techniques and had his feet always firmly on the ground. Before jumping on any bandwagon, he would ask whether the questions being answered were worth asking.

After the publication of the seminal article by Bob Solow on technological change, there was a lot of interest in that area. I did my thesis on productivity and technical change in the U.S. bituminous coal industry. (During the energy crisis of the early 1970's, Paul MacCavoy said, "G.S. worked on coal, when no one was interested in coal.") The novel feature of my thesis (part of which was published in my paper [3]) was the use of horsepower as a proxy for capital.

The character of Chicago has changed quite a bit from the time I was there. It's all "high tech" now. At the time I was there, high tech was looked down upon. I don't think I'd like the current Chicago.

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What made you move to Stanford, Rochester, Florida, and then to Ohio State?

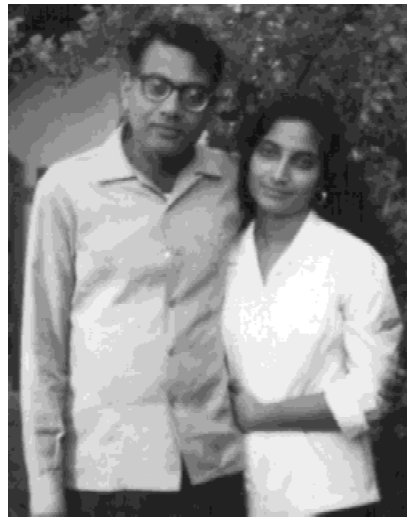
After taking my Ph.D. in 1963, I went to Stanford, where I was offered an assistant professorship. I did not see the point of returning to India right away. The director of the Bombay School of Economics did not want to wait for my return and gave the professorship to someone else. That is how I remained in the United States.

Year 1963 was perhaps the peak of my life. In 1953 I was a college dropout with no plans of ever attending a college even for a B.A. By 1963 I was an assistant professor at Stanford. But in 1953 I was very happy and contented. Since 1963 I have been mired in a rat race and lost all the spiritual values that sustained my life earlier.

At Stanford, I was lucky to have met Marc Nerlove, who was like Griliches, though more technical. He has been a good friend to me all my life. I have learned a lot from him. I met Amemiya too, but he was more theoretical, and I did not have much in common with him.

In 1967 I was offered a tenured job at Rochester. Arrow, Nerlove, and Uzawa had left Stanford, and I was advised to leave too. That is how I moved to Rochester.

At Rochester I had some good students. First, I had P. Mallela, who wasted his talent in econometrics on the bridge table. Then I had you [Kajal Lahiri], Forrest Nelson, and Lung-Fei Lee. All along I learned a lot from my students, and many of the areas I worked in, I got pushed into because of my students.



G.S. and Kay Maddala at Stanford, California, August 1967

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In 1975, I moved from Rochester to the University of Florida as a graduate research professor. Whether it was a good move is debatable, but my wife could not take the winter weather in Rochester after our children were born. At Florida in 1979 they started the Center for Econometrics and Decision Sciences with university support. I was the director until I left Florida. In 1987, at Florida, we had a new dean who was very much antieconomics and took away the money from the Center for Econometrics. That is why I moved to Ohio State University in 1992.

You worked on several areas of econometrics. Let's start with the work on production functions.

My thesis was on production functions and productivity in the bituminous coal industry. I estimated Cobb–Douglas functions by OLS. So, my thesis had very little econometrics in it. Since then different functional forms (CES and others) for production functions have appeared. In the computation of total factor productivity, the way the indices of the different inputs are combined depends on the underlying production function. In a paper prepared for the National Academy of Sciences [34], I argued that it did not make much difference what functional form was used. I have not worked much in this area, but I have followed the work of Zvi Griliches and Dale Jorgenson over the years.

One major development in this area has been that of frontier production functions. I voiced my skepticism about this work in [36]. A much longer version of this paper (unpublished) is reprinted in Chapter 6 of Volume I of *Econometric Methods and Applications* (Edward Elgar, 1994).

A very good book (quite neglected) in this area is that of the Australian economist W.E.G. Salter, *Productivity and Technical Change* (Cambridge University Press, 1960). Reading it, one would see the problems with the work on frontier production functions.

My criticism of this work has been largely ignored. The literature has exploded, and I lost interest in this area. An appropriate quote which I got from Zvi Griliches that describes all this work is the Russian proverb: "The dogs are barking, but the caravan keeps moving."

Your next area of interest was distributed lags. How did you get involved in that area?

My interest first arose by the popularity of the Koyck model among graduate students at Chicago. After that I worked on Jorgenson's rational lag model and estimation of the distributed lag models in the distributed lag and autoregressive forms (see [11]). The paper on GLS with an estimated covariance matrix [9] arose after I read Amemiya and Fuller's paper on distributed lags. This has been extended by Pagan, "Two Stage and Related Estimators and Their Applications," *Review of Economic Studies* (1986), to a general class of models.

During the 1960's and early 1970's time series analysis was mostly on distributed lag models and tests for serial correlation. My papers [14] and [15] with

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Dave Grether arose from our work on distributed lag models. So was my paper [16] with A.S. Rao. Another area popular in the early 1970's was the Box–Jenkins methods. I did not do any work in that.

At Stanford, Marc Nerlove was giving a course on spectral analysis. One day, Paul Baran called me into his office and said, "Don't talk with this guy Nerlove. He will ask you to work in spectral analysis. A young guy like you should not waste time on such silly things. You should work on India's economic development. Come and see me on Monday." I thought it was a good idea, but on that weekend Paul Baran died of a heart attack. I did not work on India's economic development, but I did not study spectral analysis either.

Spectral analysis has a lot of colorful terminology like *prewhitening* and *recoloring*, *windows* and *window carpentry*, etc. One paper I found very interesting and useful in this area is the paper by Hamon and Hannan in the *Australian Journal of Meteorology* (1964), "Regression in Time Series," which is the most simplified exposition one can find in this area.

How did you get involved in panel data analysis?

My thesis contained panel data, and the courses I had on analysis of variance in statistics were all relevant for panel data analysis. The special problems that arise in econometrics are from the presence of lagged dependent variables, i.e., dynamic panel data models. At Stanford, Marc Nerlove was working on variance components models using dynamic panel data (the Balestra–Nerlove paper and Nerlove's papers on Monte Carlo studies of different estimators and problems with the ML estimators). I studied variance components models in my courses on analysis of variance, and my paper [10] in 1971 clarified some issues with the use of variance components models in panel data.

In 1975 I did a Monte Carlo study replicating Nerlove's study but using the unconditional likelihood function. Nerlove had generated data for each cross-section unit but discarded the first 10 observations and used the 11th observation as the initial value. Given the way the data were generated, one could write down the distribution of the 11th observation. When I did it, the anomalous results that Nerlove obtained for the ML estimator disappeared. I don't know why, but I did not publish that paper. Because of its importance, I decided to have it reprinted in my collected papers. (It is Chapter 19, "Some Problems Arising in Pooling Cross-Section and Time Series Data," of Volume I of *Econometric Methods and Applications* [Edward Elgar, 1994]. Incidentally, I noted that I thank you for helpful comments and that the paper also has the idea in Mundlak's 1978 paper.)

In 1971 I also worked on the likelihood approach to pooling [13], and I have answered in [97] some criticism of that paper. Almost all the literature on panel data is based on pooling the information in the different cross-section units. The question of whether or not to pool is never discussed. I address this problem in my 1991 paper [64]. This is elaborated in [82], and the paper in *JBES* in 1997 (see [92]) also has a complete discussion (see Table I) of all the different

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methods—classical, empirical Bayesian, and Bayesian—used in pooling in panel data analysis.

During recent years there has been a lot of work on panel data unit root tests, starting with the Levin–Lin tests and the Im–Pesaran–Shin tests. I feel that there is a lot of algebra and less of a discussion of the issues involved and the purpose of using these tests. I voiced my criticism in Chapter 4 of my book *Unit Roots, Cointegration and Structural Change* (Cambridge, 1998) and in several papers (see [103, 105, 106]).

I plan to do more work in this area in the coming years.

You worked on Bayesian inference. How did you get interested in it? What are your views on the scope of Bayesian inference in econometrics?

I was studying Bayesian inference a bit while I was a student at the University of Chicago because Harry Roberts, a Bayesian, was on the econometrics exam committee. But I became really interested in Bayesian inference after I read George Barnard's paper, "The Bayesian Controversy in Statistical Inference," published in the *Journal of the Institute of Actuaries*, 1967. Over the years, I had quite a few disagreements with Arnold Zellner. He was convinced that I was anti-Bayesian and always lectured me on Bayesian inference. But Bayesians like John Pratt, Jacque Dreze, and Jay Kadane, for instance, felt that Arnold Zellner was unnecessarily attacking me. There was an interchange between me and Arnold Zellner in *Econometrica* after publication of my *Econometrica* paper [13], but it did not get published because John Pratt, a Bayesian, felt that Zellner was unnecessarily attacking me and suggested it not be published. Then we had another exchange in 1976 as well (see [24]).

In 1972 I wrote a paper, "Weak Priors and Sharp Posteriors," but it got published in *Econometrica* after 4 years (see [23]). Seymour Geisser had earlier



The Maddala family (G.S., Kay, Vivek, and Tara) in Gainesville, Florida, 1977

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suggested a noninformative prior for multivariate systems, and it was used by Zellner (in his book) and Dreze (in his presidential address) for noninformative analysis of the simultaneous equations model. I argued in my paper that the Geisser noninformative prior had the consequence of producing neat, nice posterior in underidentified systems. Dreze saw my point and argued that the same problem arises with 2SLS; it produces neat, nice estimators even in underidentified systems.

The problem of noninformative priors in simultaneous systems has been solved during the recent years by Herman Van Dijk and his students, Peter Phillips and his students, and Arnold Zellner, who all argued that the problems that I pointed out can be solved with the Jeffrey's prior.

I got discouraged by the weak priors paper not getting published in *Econometrica* for a long time and did not do any more work in the area of Bayesian inference. During recent years I got interested in the Bayesian approach to unit roots (largely through the papers by Chris Sims and Peter Phillips). All this is surveyed in Chapter 8 of my book (with In-Moo Kim) *Unit Roots, Cointegration and Structural Change* (Cambridge University Press, 1998).

I think the controversy between Bayesian and classical econometricians is largely unnecessary. Even econometricians of the classical persuasion can learn a lot from the Bayesian approach. I am not a die-hard Bayesian (saying that is the truth and the whole truth), but I always like to think about the Bayesian solution to each econometric problem. For me the Bayesian approach is not a religion, as it is to many Bayesians. Someone once said, "Bayesians are like the Hare Krishna people. If you get too close to them, you become one of them." I keep my distance.

How did you get interested in limited dependent variables and write such an influential book on that subject?

When I moved to Rochester, I found out that Dick Rosett was interested in "probit models." He had done his thesis under Tobin. At first I was not interested in that area. Then Forrest Nelson became a graduate student and worked with Rosett on the "two-limit probit model" and used it in a study on health insurance. He got me interested in this area, and we did the 1974 paper [19] on disequilibrium models. Forrest Nelson also did a Ph.D. dissertation under me in the area of limited dependent variable models. Another paper we did together was on specification errors in limited dependent variable models (see [109]). Somehow we never published it, although it has been cited in a number of publications. Then Lung-Fei Lee joined our department in 1974 and did his dissertation on two-stage estimation methods. I have learned a lot from my students all during my career. My interest in this area was thus motivated by Nelson and Lee.

In 1974, Forrest Nelson, Dick Rosett, and I planned on writing a book in this area. In 1975, Dick Rosett moved to the University of Chicago and dropped out. In 1974, Forrest Nelson had a LIMDEP program but never got it popularized (and now Bill Greene has his LIMDEP program, which everyone uses). Forrest Nelson was very slow and wanted to do everything perfectly. So, we were not making much progress with the book, and I went ahead and wrote it by myself in 1981.

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I sent the manuscript to several publishers: McGraw-Hill, who published my 1977 book *Econometrics*, Academic Press, Harvard University Press, MIT Press, and a couple of others. They all rejected it saying it has no demand, and I shelved the manuscript.

Then one day Colin Day came to my office and said that Cambridge University Press was starting a new series, Econometric Society Monographs. He asked me to contact him if I had any writing plans. I said that I already had a manuscript. He took it because he had none yet. I was glad to get rid of it, and I was so convinced that it would be a flop that when the galleys came, I didn't even care to proofread them. I thought no one would read the book anyway.

There are several errors in the book, and corrections have been suggested by several readers and my own students. By the time I assembled them all, the book had sold so many copies that I did not see any point in doing it. I planned a second edition in 1991, adding some new chapters and making corrections on the old book. But in 1992 I moved to Ohio and got sidetracked by my other books [4–12]. I am still planning on bringing out a second edition, but I don't know whether I'll finish it.

How did you get involved in pseudo data?

I was doing some consulting work for EPRI in Palo Alto, and at the same time Jim Griffin, from Houston, was also doing consulting work for EPRI and was using "pseudo data." Al Halter at EPRI asked me to look into this. I did not see much merit in this work. Pseudo data are data obtained from a process (input–output) model by simulating the outcomes for certain scenarios. I wrote two comments [39, 41] on Griffin's work. A detailed analysis of the pseudo data problem—what to simulate, why to simulate, and how to simulate—is in my 1982 paper [44]. These issues were not discussed, and a lot of papers were generated using "pseudo data."

As with many of the problems I got into, I thought that one should step back and think why we are doing what we are doing.

What are your thoughts on the use of survey data in economics and your research in this area?

As I said earlier, many of the areas I have worked in have been initiated by my students. I started work on survey data after you. You and your students have done a lot of work in this area. Apart from the 1983 paper [49] with you, and the survey paper of 1991 [61], I have worked only a little bit on survey data on expectations in the foreign exchange market [72].

There is this enormous literature on rational expectations. It is all a theory about how people should behave. Also it is a theory, as Mike Lovell remarks, on "model consistent" expectations. The "rational" expectations change with the model we start with. It is very important also to look at how people actually behave. Thus, survey data on expectations and how best to use these data in econometric model building are very important.

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You started work on time series. How did you get into that?

As with many of the areas in which I have worked, I got into time series because my students at Florida pushed me into it. They all wanted to do dissertations in time series. One of my students, Byeong Kim, wanted to work in ARCH models. I persuaded him to work on limited dependent variable models with panel data [68]. Unfortunately, he did not have much interest in this area. He recently died of cancer.

I no longer persuade students not to work in time series. In 1993 In-Moo Kim actually persuaded me to write a book with him on unit roots. This is the origin of my book *Unit Roots, Cointegration and Structural Change* (Cambridge, 1998). It was finished long before, but the publication got delayed by a whole year due to the change of editorship at Cambridge University Press.

Actually I had worked in the area of distributed lag models in the 1960's (that was called time series analysis then) but got more interested in the areas of panel data and limited dependent variable models and lost interest in time series. The pathbreaking paper by Granger on cointegration in 1981 was presented at a conference that I had organized at the University of Florida in 1980 and appears in the Annals issue of the *Journal of Econometrics* that I edited. I thought it was an interesting paper, but I never dreamed it would produce a revolution. I also read Bob Engle's paper on ARCH models and U.K. inflation but did not think I was learning much from it about U.K. inflation that I did not already know. Nor did I think much of the Nelson–Plosser paper that produced the unit root revolution. Thus, in the early 1980's, I missed all three revolutions in time series: the unit root revolution, the cointegration revolution, and the ARCH revolution. In 1991 I woke up to all these (thanks to my students) and felt like Rip Van Winkle, who went to the mountains and woke up after sleeping 20 years! And I didn't even go to the mountains! In the late 1980's, my students at Florida wanted me to teach them the Wiener processes. I didn't know anything about them. I told them, "Weiner means a hot dog, and wiener process must be a process for making hot dogs. What does that have to do with econometrics?"

How did you get interested in bootstrap methods, and what future do you see for it?

As with the other areas, my students initiated my interest in bootstrap methods. Jinook Jeong, my student at the University of Florida, first suggested that we work on this. We did two papers together [74, 86]. After I moved to Ohio State, I suggested to my student Hongyi Li that he write his thesis on bootstrap methods for cointegrated systems. I have four papers with Hongyi Li [84, 88, 94, 104] on bootstrap methods. Both Jinook Jeong and Hongyi Li are doing further work in this area. I also have a chapter (Chapter 10) in my book on unit roots on bootstrap methods. There is a lot of scope for future econometric work on bootstrap methods (and empirical likelihood methods on which Kitamura is working). The literature on asymptotic inference is enormous, but that on small sample inference

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is lacking. Bootstrap methods are going to bridge this gap, and there will be a lot of work on investigating the validity of the bootstrap methods in complicated models and suggesting alternatives (as in the papers by Hall and Horowitz).

Who are some of your most influential teachers either in India or in the United States?

My education in India was very spotty, so I did not have any influential teachers there. At Chicago, my influential teachers were Zvi Griliches, Milton Friedman, and Harry Johnson. I talked earlier about the influence of Griliches on my work. I found Milton Friedman to be the clearest writer on economic problems. I did not work under him directly, but I have read most of his writings and learned from him "simplicity" and "clear thinking." Harry Johnson took a liking to me and wanted to protect me from the "Chicago Style." I had a course from him on monetary theory, and it was so comprehensive that I had no other course in that area. He was a prolific writer (Jim Tobin called him an "economists' economist") who had an amazing amount of energy. He taught simultaneously at the University of Chicago, the London School of Economics, and the University of Toronto. Every week he was commuting! They used to say that among all economists he held a record for miles traveled, pages written, and booze drunk (he used to empty a full bottle of whiskey at our cocktail parties and still walk straight!). I always wished I had one-hundredth of his energy.

Did you study under Prof. C.R. Rao? When did you first meet him?

I did not study under Prof. C.R. Rao. I wish I had. I heard that he is a great teacher. When I was doing my master's in statistics at the University of Bombay, I thoroughly studied his book, *Advanced Statistical Methods in Biometric Research* (Wiley, 1952), from cover to cover. His classic *Linear Statistical Inference* is an expanded and updated version of the 1952 book.

After my M.A., I went to the Indian Statistical Institute (ISI) to do my Ph.D. under C.R. Rao. Then I met two of his students in the ISI cafeteria, S.R.S. Varadhan (who is now at the Courant Institute in New York and is a member of the National Academy of Sciences) and K.R. Parthasaradhy (a famous probability theorist). I heard their high level discussion and got scared and did not pursue the idea of doing a Ph.D. at ISI. C.R. Rao often introduces me as "almost my student."

I met C.R. Rao again in the late 1970's when he came to the University of Florida for a seminar. I invited him for dinner at our house. It turned out that he and my wife came from the same town in India. We thus became friends. Later, when we visited Penn State University, my wife taught his wife how to drive a car.

In 1990, Professor Rao invited me to edit, with him, the *Handbook of Statistics*, Volume 11, on econometrics. Later, we edited Volume 14, *Statistical Methods in Finance*, and Volume 15, *Robust Inference*.

C.R. Rao is one of the top five statisticians in the world of all time. Yet he is so modest and unassuming. It's a pleasure working with him because he is so help-

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ful. I wish I had been his student. For this 75th birthday in 1995, I edited (along with Peter Phillips and T.N. Srinivasan) a Festschrift for him on econometrics. He was very pleased with the volume.

You have been on academic visits to a number of exciting places like Caltech, Yale, CORE, etc. Did you find any of these places stand out in your memory where you found the time to be particularly rewarding?

I was on sabbatical during the 1970–1971 year. The first semester I spent at Yale I worked with Dave Grether, and we wrote a couple of papers ([14, 15] on my vita). I also met Jimmy Savage, and we had quite a few productive discussions. My wife was very unhappy at Yale. The second semester I spent at CORE. I had close contact with Jacques Dreze (and Richard and Mouchart). They were all Bayesians, and since I was (sort of) a Bayesian I did not learn much. I felt that I should have gone to LSE rather than CORE (then it would have been more productive). But I got to see a lot of Europe. In Belgium you had to get an "identity card" soon after arrival. The officer in charge asked me my profession. I replied, "Professor." He did not believe me and wrote "student." So with that student ID card, I got cheap student fares for all of my travels in Europe. I saved a lot of money that way.

I was at Caltech during the year 1979–1980 as a Fairchild Eminent Scholar. I liked Caltech a lot. It is a very good place to work—very peaceful. I finished my book *Limited Dependent Variables* there. Ted Anderson was also visiting Caltech the same year, and I had a lot of contact with him. I had contact with the political scientists and historians as well. It was a very good experience. They were going to make me a permanent offer at Caltech, but my wife wanted to get back to Florida. It would have been a golden opportunity for me. The next 10 years of my life at Florida were unproductive and miserable. My stay at Florida resulted in the loss of my most productive years. It was a disaster, from which I never recovered. Of all the places I visited, I liked Caltech the best. I have good memories of that visit.

Although you have worked on a variety of topics, are there any that you would have liked to have worked on but did not?

One area that I would have liked to work on is semiparametric and non-parametric inference. Also simulation based inference. But among the areas in which I have worked, there are many things I should have done but did not do (because of my being in Florida). I should have revised my 1977 book *Econometrics*. I did not. In 1974 I planned a book on panel data. I moved to Florida in 1975 and did not write it (Cheng Hsiao wrote it but much less comprehensive than what I had planned). Later I planned a book on survey data (with you but never finished it). I planned a revision of my book *Limited Dependent and Qualitative Variables in Econometrics* (Cambridge University Press, 1983) but never finished it either. I do not know if I will ever be able to get back to these projects.

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If you could go back 30 years what would you do differently in research? Would you change your emphasis?

I would not change my emphasis, but I would pursue the things I started working on more intently. For instance, I started out well on panel data but did not pursue the area. I started out well on my 1977 book *Econometrics* (McGraw-Hill), but I did not revise it. There were several opportunities in my career that I lost by carelessness. Also my whole attitude in these matters has been influenced by the Indian philosophy that nothing matters.

Who are some of your favorite econometricians?

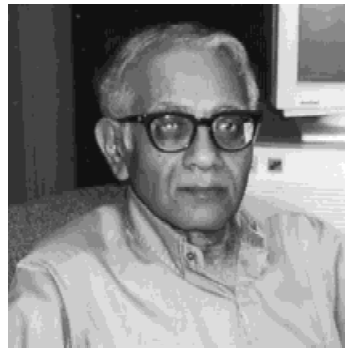
Of course, Zvi Griliches has been very influential in my work and, as I mentioned earlier, Marc Nerlove as well. I was not a student of Art Goldberger, but we have had constant professional contact and he has been a great influence on me. The others I have had contact with are Jim Durbin, Peter Phillips, and Denis Sargan. They are all my favorites.

Who are your favorite economists?

Of course, it is Milton Friedman whose ideas on several economic issues are very refreshing. He is one of the clearest thinkers I know. Also Kenneth Arrow; I had him as a colleague at Stanford. And Harry Johnson I mentioned earlier.

What was your reaction when you learned that Amartya Sen won the 1998 economics Nobel Prize?

I met A.K. Sen when I was at Stanford (he was visiting at Berkeley), and I got to know him very well. He was a very popular teacher. Students at Berkeley just loved him. I also heard him give a couple of seminars there. He was very impressive.



G.S. Maddala in his home in Ohio, 1997

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I know he fully deserves the Nobel Prize, but I did not think he would ever win it. His views are all very much "anti-Chicago," and the Nobel Committee has been very much "pro-Chicago." I don't think they ever invited him for a seminar at Chicago. I read his book *Economics and Ethics* (a series of lectures delivered at Berkeley). It's a fascinating little book. There I saw a reference to one of his "anti-Chicago" papers, "The Chicago School and Adam Smith's Ship."

During recent years, the Nobel Committee has been giving prizes to only economists at Chicago (or Chicago-style economists). So, I thought that they would never give the Nobel Prize to A.K. Sen. Among Indians, I thought they would give it to J.N. Bhagwati because lately his views on trade have been "Chicago-type" pro–free trade.

But last year the Nobel Committee expanded the scope for the Nobel Prize from economics to other social sciences, and A.K. Sen ended up with it. It was a good choice.

Did you ever think of going back to India to work or retire?

Actually, when I was a student at Chicago I was sure I was going back to India. I came there on a Fulbright Fellowship (and had an exchange visa that required that I go back). Moreover, as I mentioned earlier, the director of the Bombay School of Economics promised me a full professorship if I came back in 3 years. So, I had a job waiting for me.

In December of 1962, everyone in my class went to the ASSA meetings looking for a job. I got a ride and went along with them. I had a few interviews—none so great, but I had fun (because I was not really looking for a job). After I came back I was surprised that I got a call from Stanford for a campus visit. I hadn't visited California before, so I went there. Zvi Griliches suggested that I should take this opportunity to visit Disneyland. I did not expect a job (nor did he expect Stanford would hire me). I had fun at Disneyland, and when I returned I saw a letter from Ken Arrow offering me a job at Stanford! I was told by the foreign student advisor at Chicago that with my exchange visa I was entitled to 18 months of "practical training" after which I could go back to India. I accepted the job at Stanford and wrote to Bombay for an extension of the job there. They refused to wait and gave the professorship to someone else. I was told not to worry, that I would get some job in India with my experience at Stanford.

After 18 months at Stanford, they tried to convert my exchange visa into an immigrant visa. They were experienced in this, having done it earlier for Uzawa. I became wait-listed during which time I could not leave the United States, and I got my immigration visa in 1967. I had no job in India to return to, so I stayed in the United States. My father was not happy. He felt that I would be corrupted by American values and culture. He was right. Looking back I feel that 1963 was the peak of my life. It has been downhill ever since. I got caught in the rat race, and lost all my spiritual values. But the same thing might have happened even if I had returned to India.

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Do you have any thoughts about the philosophy of educating students in econometrics that you would like to share with us?

I think we should train students to work on real world problems of interest. The useful econometric techniques will come from problems that arise in practice. There is a lot of empty theorizing that is of little practical value (this is not the case with econometrics alone, but it is with all areas of economics). This wasn't the case at Chicago when I was a student there (even Chicago has changed a lot—for the worse). Many theoretical papers are being written, and if there is any application, it is the use of some data as a "peg" on which to "hang" the theory. Others doing theory say, "It is not my business; let others find applications." There is a professional payoff to all this theorizing. This is an unfortunate situation, but I don't know what to do about it.

Do you have any general advice to give prospective students in econometrics?

I answered this in the previous question. However, given that the profession values empty theorizing more, students will not do well in the job market if they follow my advice. This is the case not only in econometrics but in all other areas of economics. It is very important not to lose sight of the basic issues to be tackled and put them in a proper perspective. Sometimes the problem for which an elaborate solution is being offered turns out to be of trivial practical importance. Sometimes the solution offered is what I would call a Laurel and Hardy solution—as in the case of panel data unit root tests (where the initial question is transformed into an entirely different question and a solution offered).

Laurel and Hardy had a piano to deliver to Mr. Jones living on the third floor of an apartment building. They struggled hard and finally got the piano to the third floor apartment. They congratulated themselves for a job well done and came down. Then Laurel noticed that they had delivered the piano to Mr. Walker and that Mr. Jones lived on the third floor of the building next door. Now they have to bring the piano down all the way and carry it to the third floor of the next building. Then Laurel hits on a simple solution to this complicated problem. He switches the nameplates of the two men living in each building. Now the problem has been solved. The piano has been delivered to Mr. Jones. Laurel and Hardy shake hands to congratulate themselves on a job well done and walk away.

What do you think will be the thrust of work in econometrics in the next decade or so? In what areas do you plan to work?

In my paper "Econometrics in the 21st Century" [102], I outlined some of my thoughts on areas of econometrics that are worth working on in the next decade and that will be pursued.

As for me, I have been suffering from heart failure since February of 1995, and I better not say what I'll be working on in the future. I'll be writing as long as I live, and my work will be guided by the same motto as in the past, "Think first

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why you are doing what you are doing before attacking the problem with all of the technical arsenal you have and churning out a paper that may be mathematically imposing but of limited practical use." Simplicity should be one's motto.

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