*Econometric Theory*, **23**, 2007, 155–181. Printed in the United States of America.
DOI: 10.1017/S0266466607070065

# THE ET INTERVIEW: TAKESHI AMEMIYA

*Interviewed by James L. Powell*[1]

Takeshi Amemiya

Much of the credit for the outpouring of research on nonlinear models in the 1970s—particularly limited dependent variable models—should go to Takeshi Amemiya. His classic 1973 *Econometrica* article on estimation of the parameters of the Tobit model [11] set a new standard for mathematical rigor in theoretical econometrics. During his career, which spans nearly four decades, Takeshi's research contributions to econometrics have touched on most of the central models for empirical economics, including linear and nonlinear simultaneous equations models, distributed lag models, heteroskedastic and random coefficient models, qualitative response models, censored and truncated regression and selection models, transformed regression models, choice-based sampling models, and duration models. His 1985 text, *Advanced Econometrics* [44], became the standard reference for second-year graduate microeconometrics courses at the leading graduate programs in economics.

Takeshi's research accomplishments have been accompanied by numerous professional honors. He is a Fellow of the Econometric Society, the American Statistical Association, and the American Academy of Arts and Sciences and was awarded Guggenheim and Humboldt Fellowships. Takeshi also has a distinguished record of professional service; he served briefly as a co-editor of *Econometrica* and has been a co-editor of the *Journal of Econometrics* since 1982.

This interview was conducted in Takeshi Amemiya's office at Stanford University in March 2004. In recent years his interests have turned to comparative study of Greek and Japanese culture and mythology—his scholarship includes a Japanese translation of an English research monograph on Aristotle's *Ethics* [58]—and the discussion starts with this topic before turning to his unusual education, career as an econometric theorist, and variety of outside interests. His remarks reveal both his careful attention to detail and the dry humor that his colleagues and former students (like me) savor.

As a lot of us know, you have been interested in Greek culture in recent years. Can you talk a little bit about your interest in Greek culture and all things Greek?

It started when I got the Humboldt Fellowship from the German government. It is not something you apply for; Gerd Hansen of Kiel and Reinhard Hujer of Frankfurt recommended me. I went to Germany twice for three months, once in 1988 and once in 1990; on my first visit, I thought I should study the history of Germany, how the nation started. It goes back to the so-called German race moving from the north to the south, but I was wondering how people went to the north to begin with. So I found out about the movement of the Indo-Europeans, which originated somewhere between the Black Sea and Caspian Sea; they started moving, some toward India, some toward Europe, about 4000 B.C. In about 1600 B.C., they invaded Greece and established so-called Mycenaean culture.

In Greece there was already a very important culture called Minoan culture, but the Mycenaean civilization conquered the Minoan civilization. Mycenaeans started their own script, but this script was not very suitable to write down the Greek language; each letter is either a vowel by itself or a combination of consonant and vowel. This is actually the way Japanese letters are written, which is one of the connections between Japan and ancient Greece.

You told me earlier that some of the connections were with mythology or the religious structure.

The reason I initially started getting interested in Greece was that my interest in the history of Germany went further and further back, but after I started

Receiving Humboldt award, Bonn, Germany, 1988.

studying Greek culture, I got more and more interested because of similarities with Japanese culture. Both Greek and Japanese religions have many gods, who very much behave like human beings, and there are many other similarities between the two religions. Another similarity is that there is no clergy in either religion, nor any theology; it is almost like part of daily life. There is no metaphysical foundation for Greek theology, like there is for Buddhism and Christianity. There is no regular service, like going to church on Sunday; there are temples and shrines, but that's not a place for gathering; this is a place where gods live. There are many festivals; in ancient Greece, they say every other day was a festival, although there were only about 40 or 50 important holidays a year, when the assembly did not meet.

Now ancient Greek religion completely disappeared with the arrival of Christianity; the Christians were not very lenient with other religions. In Japan Shinto still exists and is still quite an important major religion. Buddhism came to Japan in the fifth century; it is a very lenient religion and did not try to destroy the traditional Shinto religion. Buddhism as practiced in Japan has incorporated many elements of Shinto and vice versa. Buddhists never had a crusade.

I said that the Greek religion disappeared, but that is also mainly superficially; in a deep undercurrent, in a way, Greek religion still remains. Right after Greek Orthodox took hold in Greece, they started worshipping many saints. There was one saint called Dionysius; if you go to a church of Dionysius, you see vines in the church, and there is a connection between Dionysius and Dionysus, the Greek god. When you travel in Greece, alongside the highway, where there was some traffic accident, you see something that looks almost like a small dollhouse, and there is some statue of a saint there and flowers. This is something you see often in Japan. So I was quite interested in this, which is something you don't see in this country—flowers, perhaps, but no shrine.

There are more similarities between ancient Greek religion and the present Japanese religion, which is Shinto. Shinto has a bad connotation because, during World War II, the Japanese emperor was the representative of the Shinto religion, so it was associated with the Japanese military. But that is not the essence of Shinto religion. Traditionally, Shinto is a very peaceful religion. The Japanese government in World War II tried to exploit the religion, and, as a result, it was interpreted by foreigners as a very militaristic religion, which is not its essential nature.

Another similarity between Shinto and ancient Greek religion is that neither says much about the afterlife. So they are really religions that emphasize this life. People go to Japanese shrines and ask that they will find good spouses and their children will get into good universities, and so on. In that sense it is very secular.

There is something of a conflict in Greek culture. Have you heard of this interesting story about a very devout ancient Greek woman who believed in Apollo? She had two children and always performed all the rituals for Apollo. She asked a favor of Apollo, that her children would become happy. Apollo said, "I will grant your wish right away," and instantly killed her two children! Ancient Greeks thought that it was better not to be born and that, once you were born, the faster you die, the better. I haven't quite reconciled these two aspects of ancient Greek religion, that this life is really a place of hardship and unhappiness and this religion that emphasizes the importance of this life and not the afterlife.

Toward the later times in classical Greece, in the fourth and third century B.C., new aspects of religion developed, especially in the shrine in Eleusis, called Eleusinian mystery cult. Once you are initiated in this religion, you will have a better afterlife.

So moral codes and social norms are separate from the religious structure?

Well, yes. Ruth Benedict, author of *Chrysanthemum and Sword*, characterized Japanese culture as a culture of shame rather than guilt, and a noted classicist, Dodds, in his *The Greeks and the Irrational* says it was the same for the ancient Greeks. At first Hector wanted to run away from Achilles but decided to fight not because of his inner convictions but because he thought about what the people of Troy would say if he ran away. Of course people are essentially the same everywhere and in every age, but an average Japanese does have a tendency to weigh shame more than guilt.

In terms of your ongoing interest, you not only study Greek culture, but you are doing scholarly research and teaching some courses in Greek economics?

Yes, Stanford instituted these "Sophomore Seminars," which are given in September, before the academic year starts. They select 12 students for each professor. A professor announces what he wants to do, and we meet every day for one month, so it is roughly comparable to a half a quarter course. It is conducted mainly in seminar style, rather than a lecture. Students and the professor get to know each other very well; the professor will invite students to dinner, and so on. I gave this course twice and taught about the Greek economy and economics; after doing this for two years in a row, I proposed a regular course called "Economy and Economics of Ancient Greece." It is offered jointly between the economics department and the classics department; I have taught this course for three years now, for a total of five times. Usually I get about 20 students, so we can still engage in Socratic dialogues. I have a courtesy appointment in classics as well.

Speaking from experience, I would think that a course in economics of ancient Greece would be more attractive than offering an undergraduate seminar in econometrics. Does your course satisfy some humanities requirement as well?

Stanford has various undergraduate requirements; my course satisfies requirement 4A, which is called "world culture." The definition of "world culture" is "non-European culture," so my course did not automatically satisfy this condition. I sent a letter to the Undergraduate Study Committee, which said that the ancient Greek culture was a non-European culture, because it was much closer to Japan than to modern Europe, and they agreed with me, so it is classified as a course which satisfies the world culture requirement.

And it also satisfies an undergraduate requirement for the economics major, so it is a two-for-one course?

Yes, if it was only classified as satisfying the world culture requirement, I wouldn't have as many as 20 students.

And you have done some translation of ancient Greek texts?

Yes, I translated Urmson's book entitled *Aristotle's Ethics* [58]. Jim Urmson is a well-known philosopher from Oxford who retired from Oxford at 60 and became a Stanford professor in the philosophy department. At the time he came here, the retirement age was 65, so he taught for five years at Stanford and then went back to Oxford. But in those five years my wife and I got to know him

With Professor Jim Urmson, Stanford, 1997.

and his wife very well, and after that we occasionally visited them at Oxford, and they became close friends of ours. I translated his book into Japanese. Whenever Urmson quoted Aristotle, I always translated from the original Greek, rather than translating from his English translation. This translation was published in 1998. In the year before, I visited Urmson at Oxford for one week, and every day I went to his house with questions about the book.

Shall we talk about your education and your travels? I would say you are very peripatetic.

What do you mean?

I believe it means "walks around a lot." You didn't really walk—you took a lot of boats, I think, as a child; not always voluntarily, as I read in your autobiography.

My father worked for a ship line called NYK, which stands for "Japan Shipping Company." It was the biggest shipping company in Japan. Before the war, it had regular service between Japan and the United States and also between Japan and Europe. My father became head of the branch office in Lima, Peru; he went there first, and then my mother and sister and I joined him. I had two

https://doi.org/10.1017/S0266466607070065 Published online by Cambridge University Press

brothers, but they were already in high school, so they stayed in Japan. I went to Peru in April of 1941 and enrolled in the first grade in a Japanese school in Lima. Then World War II broke out in December of the same year, and the Peruvian government was the first among the South American governments to sever its relationship with Japan. So we were sent back to Japan but not right away; we were first sent to a place called Seagoville, which is a small town near Dallas, Texas. We stayed there for about a month and a half.

So this was a relocation camp?

Yes, but I only have pleasant memories of it. By the time we went to Texas I was seven years old. We were treated very well. We didn't have to engage in forced labor; the relocation was for the purpose of exchanging us for Americans who were in Japan. The food was very good, and we were able to buy many things. But the Japanese Peruvians who were forcibly rounded up and taken to the United States for a possible exchange with American prisoners of war in Japan did not fare so well. They were subjected to forced labor and sent back to Japan. Only recently under the Clinton administration, the U.S. government apologized to these Japanese Peruvians and offered a meager compensation of $5,000 per person.

When did you finally get back to Japan after the war broke out?

We were sent to Texas in about May 1942, and we eventually arrived back in Japan in August of the same year. My parents and my sister must have been worried because they didn't know when they would be able to go back to Japan. Suddenly we were told that a ship was going to leave from New York, so we took a train to New York. Many famous people were on the ship, which was a Swedish ship called *Gripsholm*. You must have heard of Kakutani? At that time we did not discuss fixed points. Professor Tsuru, a very well-known Japanese economist who was a good friend of Paul Samuelson, was there; later he became the director of the research institute in Hitotsubashi University in Tokyo. He eventually hired me in 1966, when I had been an assistant professor at Stanford for two years. I didn't remember him at all, but my parents knew his wife very well; she was a pianist, and so was my sister, so they played a piano duet on the boat.

Since I had only good memories of my life in Peru and also of my brief time in the United States, in my subconscious and conscious mind I had a desire to go back to the United States. Just when I was looking for a university, they opened a new university called International Christian University in Tokyo. The idea of establishing this university actually started before the war, but it was suspended by the war and was finally established just before I was to start college. I knew there would be many American professors, and they emphasized the English language, so I enrolled there. In the freshman year, the Japanese students study almost only English, and there were many foreign students, mainly

American students, who would study only Japanese that first year. From the second year on, both Japanese and foreign students could take bilingual courses.

Did you take math then? You must have had math at some point in your undergraduate or graduate career.

Well, in high school I had some. In Japanese high school you study differential calculus. My brother, who died several years ago, was a professor of mathematics, and he was twelve years older than I. So, partly because of his interests, I was always interested in mathematical problems from my childhood. Probability was my favorite topic even when I was, say, ten years old. But in ICU, I did not take many courses in mathematics. I was an economics major, but I was not seriously interested in economics, either, and certainly not econometrics.

You didn't study economics as preparation for graduate school?

No, not at all. I wasn't planning to become an economist. In Japan economics is the major you choose if you have no particular interest. At that time there were exchange restrictions, and you could not change yen into dollars freely. So the only way you could come to this country is, either you could get dollars from someone you know in this country or get a fellowship like a Fulbright Fellowship. My academic record in ICU was very poor; I didn't even have a "B" average, so there was no way I could get a fellowship like a Fulbright. But my mathematician brother went to a Canadian university, Queens University in Kingston, Ontario, and he taught there for two years. He saved $1,000 in those two years. I looked for an American college to which I could go with just $1,000, and Professor Ayusawa, who was a Quaker, recommended Gilford College in North Carolina, a good Quaker college.

Actually, I asked my adviser, Professor Gleason, if I could go to graduate school, and he looked at my record and said, "No, you cannot go to graduate school." So I went to college as a nonmatriculated student. The fee for Gilford College was $900, including tuition and room and board.

Were you specializing then in economics?

Yes, then for the first time in my life I got seriously interested in economics. There was a professor who taught money and banking and international trade, and I really got interested in those subjects. But still at that time I was not planning to become a professional economist, because I already had a promised job in Japan to work for an English newspaper. To work for the newspaper I thought that some elementary knowledge of economics would be useful. I also thought I might take a master's degree in journalism and was thinking of applying to the School of Journalism in Columbia. But I decided that, even if I were to go back to Japan and work at this newspaper company, a master's degree in economics might be more useful.

There was no possibility that I would be accepted by a good university at the time because my record in college in Japan was not very good, and Gilford

https://doi.org/10.1017/S0266466607070065 Published online by Cambridge University Press

College did not have a very high reputation in the eastern establishment, so I applied only to less-than-best places, and one of the universities I applied to offered a small scholarship, the American University in Washington. The amount of scholarship I got was only $350 a year, plus tuition. All the classes were offered in the evening, from 7:00 P.M., for the purpose of allowing government employees to take classes; in the daytime I worked for the school library and earned the necessary money.

Then how did you get to Johns Hopkins from there?

In American University I decided, for the first time, that I should become a professional economist. I realized that I was not good for anything else, so by elimination I chose this. I decided to work toward a Ph.D. At that time my main interest was socialist economics, so I wanted to study with Paul Baran in Stanford. Fortunately I was rejected by Stanford and got a good fellowship from Johns Hopkins.

Was it a coincidence that it was close to the American University?

No. Johns Hopkins is a small place, so the chairman did all the work—here the chairman, director of graduate study, and director of graduate admissions are all different, but at Johns Hopkins, Richard Musgrave, who was chairman, did everything. He told me later that Johns Hopkins never admitted anybody from the American University before, but he looked at my application and decided to gamble. I owe a lot to Richard Musgrave.

So you went there, and you were still planning to study socialist economics?

No, there was nobody who specialized in those areas, but I was still very much interested in social planning and policy. Edwin Mills had a very big project to study water resource development in the state of Maryland. After one year of taking the fundamental courses I joined his research project. I eventually wrote a paper estimating the demand for water in the state of Maryland through the year 2000.

Empirical work is what led you to econometrics?

Yes. In order to do serious study in water resource development you really must become an expert in the chemistry of water, so every day I went to the geology department and studied chemistry. But I never liked chemistry. I got really bored by this research, so much so that I developed a slight fever.

At the end of my second year, one day Carl Christ came to see me. Carl suggested that maybe I should write a thesis proposal in econometrics for a Ford Foundation Fellowship. The deadline was only a week away, but I did write a proposal in Bayesian econometrics. I got the fellowship, resigned from Edwin Mills' project, and in my last year went into econometrics. The two fields I took my exams in were international trade and public finance. I learned most of what I know about economics by reading Richard Musgrave's *Theory of Public Finance*. I had to read it three times to understand it. That was a great education.

Obviously the original interest in Bayesian econometrics didn't last your entire career. You started out as a time series econometrician? Your thesis was on time series?

Well, the title of my thesis was "Two Essays in Econometric Theory." The two essays are completely unrelated to each other. One was time series [1], and at that time Geoff Watson, famous for the Durbin–Watson statistic, was chair of the statistics department. Actually, he established the statistics department; until he came, Johns Hopkins only had a biostatistics department belonging to the medical school. He stayed there for several years and then left for Princeton. Did you meet him there?

No, the statistics department at Princeton was gone by the time I got there.

Is that so? Well, Hopkins also lost its statistics department right after Watson left. I believe even now they don't have a statistics department, though they certainly must have biostatistics.

So he was the main adviser of your dissertation?

No, Carl Christ was the main adviser, but I also got a lot of advice from Watson. One part of my thesis was an extension of Watson's result about using the spectral density to find a lower bound for the efficiency of least squares in a generalized least squares setting; I extended his result to simultaneous equations models.

You did this in the one year you switched from public finance and international to trade econometrics?

That's right. Actually I was planning to do it in two years, but my father became terminally ill, so I wanted to show him my diploma. I worked frantically, not sleeping very much, to finish the thesis in one year. When I got the diploma in June I shook hands with the younger brother of President Eisenhower, Milton Eisenhower, who was the president of Johns Hopkins. My fa ther died two weeks after I arrived in Japan; the first week he was still conscious, so we were able to talk.

I am sorry. That was very close timing. When did you start sending papers off to be published?

One part of my thesis was this time series essay. The other part was about principal components in two-stage least squares. The principal components paper was published in the *International Economic Review* [2], and that's probably the first journal article I published.

There was one that appeared in *Econometrica* before that, in April 1966 [1]. Not a bad start! "Specification Analysis ..."

Oh, the same year! Those were the two chapters of my thesis. I came to Stanford as an assistant professor in 1964; when I came here all the students who were admitted by Stanford when I was rejected were still here as students, so I became their professor.

Recently I served as director of admissions for four years, and people sent letters and complained, asking me why they were rejected. Some were very obstinate. But as soon as I told them I was also rejected by Stanford they became quiet. In that sense I was an ideal director of admissions.

When you came to Stanford, I take it the job market wasn't what it is now?

We had the Allied Social Sciences meetings in December, and I was interviewed by about twelve schools, so it was the same as it is now. The difference is, now you really plan ahead and send papers and line up interviews even before you arrive at the meetings. In my year many people just went to the meetings and called up people to line up interviews, so it was actually worse in those years than it is now.

Someone must have recommended you in order to get an interview. Did you have a lot of action on the job market?

I was interviewed by Marc Nerlove, and I still remember, Marc told me that the day before he came to the meeting, which took place in Boston, he was swimming in Stanford. I was also interviewed by Arnold Zellner for Wisconsin. He actually gave me an offer, but I had to turn it down. Arnold still talks about that; he wishes I went to Wisconsin. Then I would have become a Bayesian.

So you were at Stanford for a couple of years, and then ... ?

Right after I got my Ph.D. in 1964, my major goal was to go back to Japan and get a good teaching job in a Japanese university. There were some offers, but only from second-rate universities. In Japan, universities have positions according to field, and each field has one professor and maybe a couple of assistant professors. Unless there's an opening in that field, it is very difficult to get a good job. In the year I was graduating there were no job openings in the major universities.

I was told by your former student Kazu Nawata that there is a strong preference for former undergraduate students.

That's right. I was at a disadvantage because I did not graduate from Tokyo or Kyoto. Professor Tsuru, who I mentioned earlier, initiated something new at

Hitotsubashi University; until that time, Hitotsubashi did not hire anybody but graduates from its own university, but he wanted to experiment with hiring graduates from other universities. So in 1966 he made me an offer, at the Institute of Economic Research, which is strictly for research; no teaching is involved. It was a tenured offer, as most first-time job offers had tenure in Japanese universities. At that time I thought I would go back to Japan permanently, so I resigned from Stanford after serving as assistant professor for two years. In those days, getting tenure at Stanford was a lot easier than it is now; I could not have gotten tenure if the situation had been like it is today. When I was about to go to Japan, Ed Shaw—who was one of the most influential senior faculty at Stanford—said that, as long as he was alive, I could always go back to Stanford with tenure. That was a nice promise—nobody can make a promise like that now!

After doing research in the institute for two years, I decided to go back to Stanford. There were not many theoretical econometricians in Japan in those days. There was nobody I could talk to professionally at Hitotsubashi. The institute had a strong tendency for applied research, so many senior faculty wanted me to engage in applied research rather than theoretical econometrics. Also, during those two years I had met my wife, so I accomplished a major purpose of my return to Japan.

In front of office at Serra House, Stanford, 1966.

https://doi.org/10.1017/S0266466607070065 Published online by Cambridge University Press

She was amenable to coming to the United States?

Yes, but with an apprehension because she was a Japanese literature major. So I came back after two years. I wrote to Ed Shaw, "Now I want to come back." At that time I only had three publications: the two I mentioned earlier, and a paper I wrote with Wayne Fuller [3].

Two *Econometrica* articles and one in *IER*—not a bad record!

But no other paper published at all. In my first year as assistant professor at Stanford, Wayne Fuller was visiting the statistics department, and we often met each other in econometric seminars. I forgot exactly how we started collaborating on this paper, but I was interested in the distributed lag model, and also I was interested in spectral densities because that was part of my thesis. Wayne Fuller, of course, is a time series statistician, so I guess both of us were interested in the same subject.

While we were doing research on this paper, there was one problem neither of us was able to solve. Herbert Rubin, who was a co-author with Ted Anderson, was also visiting the statistics department. He was a very sharp, smart man. Wayne and I went to his office and asked him about the problem we were working on. He gave us a very good suggestion to help us solve the problem.

Part of the attraction of coming back to Stanford must also have been the statistics department, given that you had such good experiences talking with visiting statisticians here, and also Ted Anderson.

Actually, Ted and I came to Stanford at the same time. I came back in 1968, which is also the year he moved here from Columbia. I was very lucky that Ted came in the same year; he suggested we should apply together for NSF funding. Ted was already famous, and I was less famous, so I probably would not have gotten NSF funds if I had applied by myself.

It is hard to do the counterfactual here. However, speaking as a former graduate student and research assistant, I am very glad you did, since it got me through my studies here. You and Ted supported quite a few students doing econometric theory, which is hard to come by now, I guess.

Right. I was very fortunate that I worked with Ted, although we did not write any joint papers. He was helpful on many occasions when I had difficult problems. I have an impression that he knows everything.

I see. I do, too.

And he never makes a mistake. So recently I was surprised that he found an error in a paper he wrote several decades ago. I told him, "I thought you never made a mistake," but he must be human after all.

He did find the error, too. So were there any other colleagues or experiences early on?

Yes, when I was here, G.S. Maddala was also an assistant professor here. We became very good friends. He didn't get tenure and moved to Rochester.

When I was here, you and Ted made up the econometrics group, but there must have been other people coming through in the field.

Yes, when I first got here Marc Nerlove was still here. By the time I came back in 1968, Marc was gone and G.S. was gone. That was when Ted and I started teaching econometrics together. I taught what is now Econ 273, Advanced Econometrics, the first year I arrived here as assistant professor, and I started teaching from measure theory, because as a new assistant professor I was very ambitious. The first day I had fifteen students; the second day I had only three. Those three stayed on and were excellent econometricians. One of them was David Grether at Caltech. I bought a used car from him for $100, a 1954 Pontiac. I bought it because David said this was a car that was driven by an old lady, and I believed him; at that time I didn't know it was quite a hackneyed phrase, but it turned out to be an excellent car.

Could you say a bit more about any other influential colleagues at Stanford or elsewhere?

Going back to Hopkins, the statistics department at that time was very good. C.R. Rao was visiting one year, so I took a course from him. Pitman was also teaching; there are very few surviving students who took courses from Pitman. Nowadays people don't even know who Pitman was, although everybody knows about "Pitman drift."

Here at Stanford, besides Ted I also talked a lot with Ingram Olkin, Charles Stein, and Herman Chernoff. Did you know Herman at MIT?

Briefly. I think I met him when he and Peter Huber switched between Harvard and MIT, but I can't exactly remember who was where, when.

Well, Charles Stein is a person I have great admiration for. He is a statistician but also a pacifist. He was one who quit Berkeley because Berkeley professors were forced to declare that they were not members of the Communist Party. Charles declined signing anything, so he moved to Stanford. During the Vietnam War he was always wearing a black armband. And he is a true scholar, not after money or other rewards. Among all the faculty members at Stanford, he is the one I have greatest admiration for.

He and I used to play the Japanese game of Go. The nice thing about Go is that two players with different abilities still can enjoy playing against each other by giving a handicap. When I played with Charles, I would always let him place five stones initially, but I still used to beat him. He often makes bad moves, and whenever he makes a bad move and realizes it was bad, he says, “Stupid! Stupid!” I really enjoyed Charles Stein calling himself stupid!

At some point when you were here, early on, you made an incomplete switch from time series to cross-section econometrics, though you still have a tendency to use subscript “t” rather than “i.”

One paper used both “T” and “N” to denote the sample size!

With panel data problems, I guess you need both. At what point did you move away from spectral analysis toward nonlinear models?

In the early 1970s. It really happened on a specific day, when Mike Boskin came to my office and asked me about the Tobit model. In those days the term *Tobit model* was not very popular. It referred to the 1958 *Econometrica* article by Tobin. I did read that paper as a graduate student, but—this shows my ignorance—I didn’t think much of the paper when I read it. Mike asked me many statistical problems connected to the Tobit model, how to do inference and proving asymptotic properties of the maximum likelihood estimator, and so on. So that was the occasion that got me interested in Tobit models.

Up until that time most of your time series work dealt with statistics that could be written in closed form and manipulated directly?

Yes, part of my dissertation was time series simultaneous equations models, and the paper I wrote with Fuller [3], was about distributed lag models, and I had a paper called, “Generalized Least Squares with an Estimated Covariance Matrix” in *Econometrica* [10]. But I did not write many papers, because time series analysis is very difficult, and I could not write many papers on the subject.

I became very prolific after moving to limited dependent variables models. In the early 1970s I was writing six papers a year. Now I write one paper in six years.

I know you have a paper with Professor Boskin on the truncated log-normal regression model [12]. That seemed to be more empirical than your other papers, which I assume was due to your co-author’s interests.

That’s right. More recently I wrote a couple of empirical papers using Japanese data. One was a sort of generalized Tobit model—you estimate what kind of investment you would choose, stocks or bonds, and then, for each, how much you invest [53]. Another paper using Japanese data was about what occupation you will choose after you retire from a major company at age 55 [50]. In those days, the 1980s, the retirement age was 55; some people would move to smaller companies and work full-time. So the choices were working full-time, part-time, retiring, or engaging in self-employment; there my role was to work on the theoretical side, while my co-authors got the data and did all the dirty work.

I can fully understand how difficult empirical work is. It really requires a special talent. I don’t have that talent at all.

https://doi.org/10.1017/S0266466607070065 Published online by Cambridge University Press

We've talked about the 1970s. In addition to the Tobit paper, another important paper from the 1970's was about nonlinear two-stage least squares [15]. Did that idea, like the idea for the Tobit paper, come from someone with an application who came to you to do inference? How did you come up with that idea?

I don't exactly remember what was the impetus for writing that paper. But usually the way I find interesting topics for research is by reading papers by other people and finding either errors or necessary generalizations. There was a paper written by—I forget the authors—that proposed doing nonlinear two-stage least squares in the wrong way, by inserting fitted values into the nonlinear regression function.

Yes, the "forbidden regression," as Jerry Hausman used to call it. When I was at MIT, there were occasionally field exam questions having to do with the forbidden regression. I thought I remember you saying that the nonlinear two-stage least squares idea came when you were on leave, either at MIT or somewhere else?

No, that was the paper in which I showed that nonlinear three-stage and nonlinear full information maximum likelihood (FIML) were different [24]. That paper I was working on when I was visiting the computer center at MIT, which was headed by Ed Kuh. I thought for a long time about why nonlinear three-stage is different from nonlinear FIML, and I didn't quite know the answer. One day I took my daughter to a circus show in Boston; while I was watching the circus, an idea came to me.

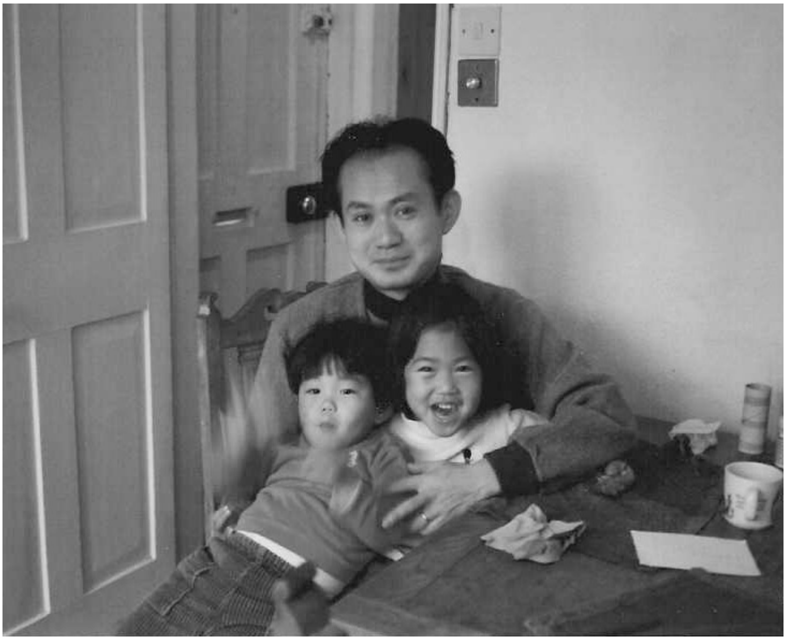
So that's where ideas come from: the circus!

So I wrote it up. Then somebody pointed out that that idea is in Charles Stein's old paper, but I found it independently. Of course, I acknowledged it later. With Charles Stein, whatever you do, he must have done it a better way.

As a student, I remember you saying that going on leave was very productive, the best way to get work done. Did you do that frequently?

Well, once I had a sabbatical in 1973, the year my son was born. Since it was right after he was born, we could not go very far, so I just went ten minutes away, to the statistics department. Economics students did not come to bother me in the statistics department, so I had much more quiet there. That year Ingram Olkin was on leave, so I used his office.

In 1975, I visited MIT for one semester, April to July. Within the United States that was the only time I went away from Stanford. I went to Europe many times. In 1976 I had a Guggenheim Fellowship and visited the statistics department of the London School of Economics. That was quite a memorable year. Jim Durbin was there, and another person I got to know very well was Alan Stuart, who wrote the famous books with Kendall, *The Advanced Theory*



With children in London, visiting LSE as a Guggenheim Fellow, 1976.

*of Statistics*. I also met the famous Japanese mathematical economist, Morishima, and Sargan was there, although in the economics department.

Later, in 1985 I visited the Advanced Institute of Vienna for one month, and in 1988 and 1990 I visited Germany for three months each time, and in 1993 I visited the Free University of Amsterdam for three months. That was another really enjoyable trip; Amsterdam is one of my favorite cities. In 1999 I visited Munich Center for Economic Studies for two or three months, and I visited Munich again just to give a seminar last August. That was my eighth visit to Munich.

When I was a student here, you were working on surveys. When did you know it was time to survey qualitative response and Tobit models?

The Tobit models survey I initiated by myself, for a special issue of the *Journal of Econometrics* [43]. For the qualitative response models survey, Moe Abramovitz was the editor of the *Journal of Economic Literature*, and he said he wanted to initiate this new series of surveys, and asked me what were interesting subjects in econometrics, so I said that the most interesting one was qualitative response models. He asked who he should ask to write the survey, so I said, “I’ll do it.” That was one of that journal’s articles with many citations [35].

You were definitely pushing the envelope with the typesetting capabilities of the JEL at the time. I remember they weren't used to Greek letters and not so good with subscripts and summation signs, so your paper set a new standard for them. I might have been around when you were proofreading it or getting the typesetting done.

Oh, so were you acknowledged? Yes, you are. I mentioned quite a few people—Boskin, Chamberlain, Cosslett, Goldberger, Hausman, Heckman, Lee, Maddala, MaCurdy, McFadden, Nelson, Nold, Pencavel, Powell, Rosen, Savin, Schmidt, and Zellner.

Yes, I am very happy to be on this list of names.

That reminds me. Did you know Fred Nold? You know the sad story about him? He was killed in a very freak accident, around fifteen years ago. I kept urging him to finish his Ph.D. thesis, but before he finished he got some grants from government agencies. I was very saddened by his death.

Was he an econometrician or an applied researcher?

He was about to write his thesis in theoretical econometrics. He and I have a joint paper about qualitative response models [18]. Fred also had a joint paper with Mike Boskin. Fred was an excellent tennis player. I used to play tennis with him regularly, and Mike Boskin did, too. He was a student, and then had a firm, locally, but he used to come to the department quite often.

Mike Boskin, by the way, is somebody I taught tennis. He was a natural athlete, so he got better very quickly. The first year we played regularly, and I beat him every time, but in the second year he was already better than I was. Fred Nold was an excellent player and always beat me and Mike Boskin.

Can I talk about tennis some more?

I guess instead I was going to ask about the book *Advanced Econometrics* [44] first. I remember the book as the lecture notes. When did you know that you were writing a book?

Right at the beginning, as I started having lecture notes typed, I had intentions of eventually making a book. A long time before I actually published *Advanced Econometrics* in 1985, my lecture notes were circulated at Berkeley and MIT. I wrote an introductory textbook (*Introduction to Statistics and Econometrics* [55]) in 1994, also published by Harvard University Press, based on the lecture notes for the first-year graduate course. One of the theorems, that the plim of a continuous function is the continuous function of the plim, became a very famous theorem among graduate students. That's called "Theorem 6.1.3"; in a student skit, one student was asking another how to deal with a problem that had nothing to with econometrics, and another student shouts, "Try Theorem 6.1.3!"

We may have already covered this ground, but what was the most intellectually exciting time or place in your career?

Well, the first time I came to Stanford as assistant professor, Marc Nerlove was still here, and Wayne Fuller was visiting. There was a very exciting atmosphere; all the mathematical economists and econometricians had offices together in Serra House, which is very close to the present statistics department. Ken Arrow had an office there, and many of his students who later became famous, like Walter Heller and Steve Ross. It was a very exciting time and place. Next to that was the early 1970s, when I started doing research in limited dependent variables.

Your paper had an initial consistent estimator for the Tobit model ...

That was actually in the second Tobit paper [13], on simultaneous-equation Tobit models, also published by *Econometrica* one year after my initial Tobit paper [11]. That's where I discuss the initial consistent estimator, because the simultaneous-equation Tobit model is very complicated, and the full maximum likelihood estimation (MLE) is very difficult. I proved consistency of my initial consistent estimator, but somebody did a Monte Carlo study and showed that it doesn't converge until you get a sample size of one million. Any estimator based on moments for the Tobit model is very inefficient. I got the impression that a lot of information is contained in higher moments.

Do you have any anecdotes about your editorial work for *Econometrica*? You were co-editor for *Econometrica* for a year, and then you moved to a relatively new journal, the *Journal of Econometrics*.

I have all the old issues of the *Journal of Econometrics*, and do you know how much they are worth? An incredible amount. Many universities in Europe would pay something like $20,000 for the whole series.

Now it is online.

Really? Well, it started in 1973, by Dennis Aigner, Phoebus Dhrymes, and Arnold Zellner, and I joined in 1982 when Phoebus resigned. I already had published a few papers in the *Journal of Econometrics* by 1982. In fact, at that time I had several papers—"The Nonlinear Two-Stage Least Squares Estimator" [15] was published in the *Journal of Econometrics*. I liked that journal quite a lot, so I was quite happy to become an editor when they decided to ask me about it. The *Journal of Econometrics* has an "executive council," and I have no idea what that is, but I am a member.

Before I became editor of the *Journal of Econometrics*, I was an associate editor of the *Journal of the American Statistical Association*. At that time I was still an associate professor. When my daughter Naoko was born in 1970, I would help my wife change her diapers, so I called myself "associate professor, associate editor, and associate diaper changer."

https://doi.org/10.1017/S0266466607070065 Published online by Cambridge University Press

Turning to some more general questions now, what I learned from you was mostly estimation, less testing.

Testing I don't like. I don't think I have ever written any paper about testing, and I present a strong criticism of classical testing in my introductory textbook [55]. I much prefer Bayesian principles of testing, because classical testing, especially in testing composite against composite, is based on a very arbitrary principle.

So, in a sense, Bayesian procedures, which look at the whole likelihood function ...

Yes, then there is a posterior density over the set specified by the null and the set specified by the alternative. It is much better to look at the area under the density, but the classical testing principle essentially looks at where the maximum occurs. My introductory textbook [55] is written with a philosophy that is very sympathetic to Bayesian principles. Arnold Zellner likes my book.

But you do have a reputation for "living in asymptotia," as some Bayesians would say.

That is because I am not very good at integration, so I cannot become a full-fledged Bayesian.

Do you have any advice for prospective students of econometrics? If you were to do it all over again, would you do econometric theory?

No, I would do Greek philosophy.

Again, it is hard to do the counterfactual. If you had started in Greek philosophy, would you have switched later to econometrics?

It is difficult to do econometrics at an old age. Many people start out in mathematical fields and then at a later age turn to more literary or historical subjects. Ted Anderson is an exception.

Do you think econometric theory is important?

No, certainly not a fancy kind. But nothing is really important in life anyway.

I was once asked to become a consultant for the Army Corps of Engineers, which was trying to estimate, quite a long time ago, the demand for port facilities in San Francisco in the year 2000. I have been specializing in estimating things to the year 2000, but now it is 2004, so I have to pick some future date. The reason the Army Corps of Engineers wanted to use me as a consultant was because they wanted me to use some very fancy tools to come up with a conclusion which they liked. What they liked is that demand will increase, so the Army Corps of Engineers would have many things to do.

I told them, "Don't try any fancy econometric tools; just draw a graph and look at how the graph changes." I never got the consulting job after that. I said,

https://doi.org/10.1017/S0266466607070065 Published online by Cambridge University Press

"If you look at this graph, you will see that the demand will not increase at a fast rate." Essentially I predicted a low demand for facilities, and I was quite surprised, a few weeks later, that there was an article in the *San Francisco Chronicle* saying, "Professor Amemiya of Stanford University is predicting a big demand for port facilities in San Francisco." That's the conclusion they already had.

So then, if fancy econometric theory is not important, that doesn't mean that applied work is always going to be the relevant alternative either, correct?

Well, yes. Maybe we shouldn't include this, especially in an interview for *Econometric Theory*! That sounds like a rather dangerous thing to say.

It is certainly provocative, but I don't know if it is dangerous. There must be something useful about econometrics, even if it is not immediately applied but just as basic research. Economists have data problems that you don't get in experimental fields.

One thing I can say, and I don't mind putting it in print, is that I don't believe in trying to prove theorems in econometric theory using the most general assumptions. I would much rather start with simple assumptions; that way you really understand what is going on. When you are too involved with the minute details of proofs, you really tend to lose the whole picture. In all the papers I have written, I have always tried to make the presentation very simple.

When students come to my office and ask about research they are doing, they also tend to explain what they are working on in very general terms. I always tell them to consider a more simple, specific example.

Is there anything else you want to say about, say, the place of econometrics in statistics or mathematics or social sciences? We could talk about that or about other things in life.

Once, long ago, there was a department tennis tournament, and I won the doubles championship with my student Tom McCoy, who was about the third Ph.D. student I supervised. He became an assistant professor at Williams College, but his father owned a big wheat farm in Oregon. When his father retired he wanted Tom to come back to the farm, so he quit his academic job, and now he manages this wheat farm with 3,000 acres. I visited that wheat farm several years ago, and I still keep up with him. He was a good tennis player, so he and I became doubles partners. What I did was try to run away from the ball whenever it came to my side and let him hit it; that way we became doubles champions.

Who did you beat? Who else was playing against you?

The final was against Bert Hickman and an associate professor who moved to Chapel Hill, North Carolina—I've forgotten his name—but we won quite deci-

https://doi.org/10.1017/S0266466607070065 Published online by Cambridge University Press

sively. Actually, I was the runner-up in the singles championship. Another student won; Tom McCoy did not compete.

Much later, when Keunkuan Ryu was about to finish his dissertation, I said I would sign the thesis if he beat me at tennis, and he beat me 6-4, 6-3. That's how he got his Ph.D.; he tells this story.

I started playing regularly with Ron McKinnon as soon as I came here in 1964. I think we've played more than 1,000 games, and I still play with him—I think I am playing with him this Thursday. We are pretty evenly matched.

Can you think of any questions I should have asked?

Yes, about the game of Go. My mathematician brother was one of the best amateur Go players in Japan. Every time I would go back to Japan I would play with him. I put three stones, and then we were evenly matched. I am a reasonably good Go player, but after I retire I want to spend more time playing it.

You also mentioned to me earlier that you would like to get back on some ships and cruise and that you found getting on a cruise ship was much more relaxing than attending a faculty meeting.

Just about everything is more relaxing than attending a faculty meeting!

Another hobby of mine is writing Chinese poetry. It is my latest hobby; I started doing it about June of last year. I studied several books about how to compose Chinese poetry. Chinese poems are very interesting. You have to have a rhyme, just like English poems. In addition, every Chinese character can be classified into two groups, called "plain" and "not plain"; you have to arrange those two types of characters in a very specific way, according to fairly rigid rules. So writing Chinese poetry is somewhat like solving a crossword puzzle. Starting in June last year, I have composed thirteen poems, but then I got busy in September, and since then I haven't composed a single poem, but I want to get back to it soon.

I visited China for the first time three years ago. I visited Beijing, and I gave a seminar there. At the beginning of a lecture I would write a Chinese poem on the blackboard, but at that time I wasn't composing, so I wrote famous poems by Chinese poets, to break the ice, so to speak.

Are they long or short, like a haiku?

Much longer than a haiku. Mainly I write two types (there are more than two types). One type is seven characters and four lines—we write them vertically. Another is five characters and eight lines. I wrote one of this latter kind for my daughter's wedding.

Well, thank you for speaking with me. To conclude, is there anything you would say is important in life?

When I was a child in Japan—and this is still generally true in Japan—I seldom saw my father. He was working very hard and would not come home until

https://doi.org/10.1017/S0266466607070065 Published online by Cambridge University Press

With family at Maui, Hawaii, 2002.

ten or eleven. Now that the Japanese economy is going down, maybe children see more of their fathers. While the Japanese economy was booming, many men worked very late hours, so child rearing was left completely to mothers, which is a really bad thing. I decided to spend a lot of time with my children, by playing with them a lot.

So, among all things, which are unimportant, my family is the thing which is least unimportant.

## *NOTE*

1. The interviewer is at the Department of Economics, University of California at Berkeley.

## *REFERENCES*

Benedict, R. (1946) *The Chrysanthemum and the Sword*. Houghton Mifflin.

Dodds, E. (2004) *The Greeks and the Irrational*. Univeristy of California Press.

Kendall, M. & A. Stuart (1944) *The Advanced Theory of Statistics* (in 3 vols.). Charles Griffin & Co.

Musgrave, R. (1958) *The Theory of Public Finance*. McGraw-Hill.

Nold, F. & M. Boskin (1975) A Markov model of turnover in aid to families with dependent children. *The Journal of Human Resources* 10, 467–481.

Stein, C. (1956) Efficient nonparametric testing and estimation. In J. Neyman (ed.), *Proceedings of the Third Berkeley Symposium in Mathematical Statistics and Probability*, pp. 187–195. Univeristy of California Press.

Tobin, J. (1958) Estimation of relationships for limited dependent variables. *Econometrica* 26, 24–36.

## PUBLICATIONS OF TAKESHI AMEMIYA


### 1966

1. Specification analysis in the estimation of parameters of a simultaneous equation model with autoregressive residuals. *Econometrica* 34, 283–306.
2. On the use of principal components of independent variables in two-stage least squares estimation. *International Economic Review* 7, 283–303.

### 1967

3. With W. Fuller. A comparative study of alternative estimators in a distributed-lag model. *Econometrica* 35, 509–529.

### 1968

4. The correlation study of the residuals in a multivariate regression model. *Economic Review* 19, 125–132.

### 1969

5. Methodology of econometric prediction. In Isamu Yamada (ed.), *The Structural Change and Prediction of the Japanese Economy*. Shunju-sha (in Japanese).

### 1971

6. The estimation of the variances in a variance-components model. *International Economic Review* 12, 1–13.
7. The effect of aggregation on prediction in the autoregressive model. *Proceedings of the IEEE Conference on Decision and Control*, pp. 537–539.
8. With R.Y. Wu. The effect of aggregation on prediction in the autoregressive model. *Journal of the American Statistical Association* 67, 628–632.

### 1973

9. Regression analysis when the variance of the dependent variable is proportional to the square of its expectation. *Journal of the American Statistical Association* 68, 928–934.
10. Generalized least squares with an estimated autocovariance matrix. *Econometrica* 41, 723–732.
11. Regression analysis when the dependent variable is truncated normal. *Econometrica* 41, 997–1016.

### 1974

12. With M. Boskin. Regression analysis when the dependent variable is truncated lognormal: An application to the determinants of the duration of welfare dependency. *International Economic Review* 15, 485–496.
13. Multivariate regression and simultaneous equation models when the dependent variables are truncated normal. *Econometrica* 42, 999–1012.
14. A note on a Fair and Jaffee model. *Econometrica* 42, 759–762.
15. The nonlinear two stage least squares estimator. *Journal of Econometrics* 2, 255–257.
16. With K. Morimune. Selecting the optimal order of polynomial in the Almon distributive lag. *Review of Economics and Statistics* 56, 378–386.
17. Bivariate probit analysis: Minimum chi-square methods. *Journal of the American Statistical Association* 69, 940–944.

## 1975

18. With F. Nold. A modified logit model. *Review of Economics and Statistics* 57, 255–257.
19. Qualitative response models. *Annals of Economic and Social Measurement* 4, 363–372.
20. The nonlinear limited information maximum likelihood estimator and the modified nonlinear two stage least-squares estimator. *Journal of Econometrics* 3, 375–386.

## 1976

21. L'estimation des modèles à equations simultanées non linéaires. In *Cahiers du Séminaire d'Econometrie* 19, 23–33. Centre National de la Recherche Scientifique.
22. With D.J. Aigner & D.J. Poirier. On the estimation of production frontiers: Maximum likelihood estimation of the parameters of a discontinuous density function. *International Economic Review* 17, 377–396.
23. The maximum likelihood, the minimum chi-square, and the nonlinear weighted least squares estimator in the general qualitative response model. *Journal of the American Statistical Association* 71, 347–351.

## 1977

24. The maximum likelihood and the nonlinear three-stage least squares estimator in the general nonlinear simultaneous equation model. *Econometrica* 45, 955–968.
25. The modified second-round estimator in the general qualitative response model. *Journal of Econometrics* 5, 295–299.
26. Some theorems in the linear probability model. *International Economic Review* 8, 645–650.
27. A note on a heteroscedastic model. *Journal of Econometrics* 6, 365–370.

## 1978

28. On a two-step estimation of a multivariate logit model. *Journal of Econometrics* 8, 13–21.
29. A note on a random coefficients model. *International Economic Review* 19, 793–796.
30. The estimation of a simultaneous-equation generalized probit model. *Econometrica* 46, 1193–1205.

## 1979

31. The estimation of a simultaneous-equation Tobit model. *International Economic Review* 20, 169–181.

## 1980

32. Selection of regressors. *International Economic Review* 21, 331–354.
33. The $n^{-2}$-order mean squared errors of the maximum likelihood and the minimum logit chi-square estimator. *Annals of Statistics* 8, 488–505.

## 1981

34. With J.L. Powell. A comparison of the Box-Cox maximum likelihood estimator and the nonlinear two stage least squares estimator. *Journal of Econometrics* 17, 351–381.
35. Qualitative response models: A survey. *Journal of Economic Literature* 19, 1483–1536.

## 1982

36. The two-stage least absolute deviations estimators. *Econometrica* 50, 689–711.
37. Correction to a lemma. *Econometrica* 50, 1325–1328.


https://doi.org/10.1017/S0266466607070065 Published online by Cambridge University Press

## 1983

38. Nonlinear regression models. In Z. Griliches & M. Intrilligator (eds.), *Handbook of Econometrics*, pp. 334–389. North-Holland.
39. With J.L. Powell. A comparison of the logit model and normal discriminant analysis when the independent variables are binary. In S. Karlin, T. Amemiya, & L. Goodman (eds.), *Studies in Econometrics, Time Series, and Multivariate Statistics*, pp. 3–30. Academic Press.
40. Partially generalized least squares and two-stage least squares estimators. *Journal of Econometrics* 23, 275–283.
41. A comparison of the Amemiya GLS and the Lee-Maddala-Trost G2SLS in a simultaneous-equations Tobit model. *Journal of Econometrics* 23, 295–300.
42. Co-edited with S. Karlin & L.A. Goodman. *Studies in Econometrics, Time Series, and Multivariate Statistics*. Academic Press.

## 1984

43. Tobit models: A survey. *Journal of Econometrics* 24, 3–61.

## 1985

44. *Advanced Econometrics*. Harvard University Press.

## 1986

45. With T.E. MaCurdy. Instrumental-variable estimation of an error-components model. *Econometrica* 54, 869–880.

## 1987

46. With Q. Vuong. A comparison of two consistent estimators in the choice-based sampling qualitative response model. *Econometrica* 55, 699–702.
47. Discrete choice models. In J. Eatwell, M. Milgate, & P. Newman (eds.), *The New Palgrave Dictionary*, vol. 1, pp. 850–854. Macmillan.
48. Limited dependent variables. In J. Eatwell, M. Milgate, & P. Newman (eds.), *The New Palgrave Dictionary*, vol. 3, pp. 185–189. Macmillan.

## 1988

49. Two-stage least squares. In S. Kotz, C. Read, N. Balakrishnan, & B. Vidakovic (eds.), *Encyclopedia of Statistical Sciences*, pp. 373–375. Wiley.
50. With K. Shimono. An application of nested logit models to the labor supply of the elderly. *Economic Studies Quarterly* 40, 14–22.

## 1990

51. T.W. Anderson and the estimation of dynamic models using panel data. In George P.H. Styan (ed.), *The Collected Papers of T.W. Anderson: 1943–1985*, vol. 2, pp. 1577–1578. Wiley.

## 1991

52. Risan sentaku model (discrete choice model). In H. Hironaka (ed.), *Suri Kagaku Jiten (Encyclopaedia of Mathematical Sciences)*, pp. 489–494. Osaka Shoseki (in Japanese).

## 1993

53. With M. Saito & K. Shimono. A study of household investment patterns in Japan: An application of generalized Tobit models. *Economic Studies Quarterly* 44, 13–28.

## 1994

54. *Collected Essays of Takeshi Amemiya*. Edward Elgar.
55. *Introduction to Statistics and Econometrics*. Harvard University Press.

## 1995

56. With K. Asakura, K. Hayami, K. Tsujimura, T. Nakamura, & Y. Kurabayashi. Japanese economy and economics in the postwar fifty years. *Toyo Eiwa Journal of the Humanities and Social Sciences*, 5–68 (in Japanese).

## 1996

57. Structural duration models. *Journal of Statistical Planning and Inference*, 39–52.

## 1998

58. *Introduction to Aristotle's Ethics. Translation and Commentary on Aristotle's Ethics by J.O. Urmson*. Iwanami (in Japanese).

## 1999

59. A note on left censoring. In H. Pesaran, K. Lahiri, C. Hsiao, & L.-F. Lee (eds.), *Analysis of Panels Data and Limited Dependent Variable Models*, pp. 7–22. Cambridge University Press.

## 2001

60. Endogenous sampling in duration models. *Monetary and Economic Studies* 19, 77–96.

## 2002

61. With Dongseok Kim. A generalization of the nested logit model. In I. Klein & S. Mittnik (eds.), *Contributions to Modern Econometrics*, pp. 1–8. Kluwer Academic Publishers.

## 2004

62. Criticism of utilitarianism and efficiency. *Weekly Economist*, March 23, 52–55 (in Japanese).
63. The economic ideas of classical Athens. *Kyoto Economic Review* 73(2), 57–74.

## 2005

64. Comments on "Flow models for ancient economies" by John K. Davies. In I. Morris & J. Manning (eds.), *The Ancient Economy: Evidence and Models*, pp. 157–160. Stanford University Press.

## 2006

65. With Xinghua Yu. Endogenous sampling and matching method in duration models. *Monetary and Economic Studies* (forthcoming).