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THE ET INTERVIEW: PROFESSOR CHENG HSIAO

Interviewed[1] *by In Choi*[2]
Sogang University
and
Chung-Ming Kuan[3]
National Taiwan University



Cheng Hsiao

Cheng Hsiao was born in Chongqing, China, on July 28, 1943. He and his family moved to Taiwan via Hong Kong in 1950, where he received his elementary, middle school, and college education. He earned his B.A. in economics from National Taiwan University in 1965. After serving a year in the military, he went to Oxford University, U.K., and received his Ph.B. degree in 1968. He then went to Stanford University and completed his Ph.D. in 1972 under the supervision of Takeshi Amemiya and T.W. Anderson. After academic appointments at the University of California, Berkeley, as an assistant professor (1972–76), at the

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National Bureau of Economic Research as a faculty research fellow (1976–77), and at the University of Toronto as an associate professor and then full professor, in 1985 he moved to the University of Southern California, with which he has been affiliated ever since, except for a brief stint at the University of California, Irvine.

Cheng Hsiao has mainly worked in integrating econometric theory with economic analysis. He has made extensive contributions in methodology and empirical analysis in the areas of panel data, time series, cross-sectional data, structural modeling, measurement errors, limited dependent variables, qualitative choice models, etc. He has published more than 120 articles in professional journals and books. He is the author of the landmark Econometric Society monographs, *Analysis of Panel Data*, first and second editions (Hsiao, 1986, 2004). He has been a co-editor of the *Journal of Econometrics* since 1991. He is a fellow of the Econometric Society, the *Journal of Econometrics*, and the Modelling and Simulation Society of Australia and New Zealand, and an academician of Academia Sinica, Taiwan. He is also the recipient of the Modelling and Simulation Society of Australia and New Zealand Biennial Award.

In recent years Cheng Hsiao has been promoting academic collaboration between academic communities in Asia and North America. This interview was conducted in Chiangmai, Thailand, on January 13, 2011, on the occasion of the Third Annual Meeting of the Thailand Econometric Society.

Cheng, thank you very much for agreeing to be interviewed. In and I are much honored to do this interview for *Econometric Theory*. One of the major reasons we wanted to do this interview is that you have a quite different educational background compared to many other econometricians. You moved from China to Taiwan, then to England and to the United States. You now hold important teaching positions and have been editing one of the leading econometrics journals for many years. So, I think your knowledge, your attitude toward research, your ideas about academic research should be very helpful for junior faculty and students. (K)

We usually begin from childhood and early education in *ET* interviews. Your family moved from mainland China to Taiwan. When and why did it happen? (C)

Thank you, In and Chung-Ming, for granting me this honor. It is quite unexpected to be interviewed, given that my contribution to econometrics is very limited in comparison to other leading econometricians. My family moved from mainland China to Taiwan in 1949 or 1950 because right after the Second World War, the Nationalists and the Communists were engaged in civil war. The Nationalists were defeated by the Communists. My father was a member of the Nationalist Party so we moved to Taiwan by way of Hong Kong. In a sense, we were refugees, but my parents looked after us very well. I didn't ever feel I was a refugee.

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And then you went to school in Taiwan. How was your childhood education? (C)

I think I had good primary and middle school education. The curriculum was quite rigorous. I was just an average student. But I was very lucky to have good fellow students like Hsinshin Chou, Ling-Fong Li, and Yuankang Wang to help me with mathematics and Lin Chongming, Yau Fengbei, Wang Yuchung, and Zeng Hsin with other subjects. I am really very grateful because without them, I might have fallen far behind. At the National Taiwan University, I had good teachers, but because they were cut off from the developments in the Western world due to the Sino-Japanese war from 1937 to 1945, and then the civil war afterwards, the materials they taught did not correspond to those of North America and Europe. But they were good professors, serious about teaching and research. I learned a lot from the examples they set.

We all know that the educational system in the East, in particular in China, Taiwan, maybe also Korea and Japan, is quite different from the Western system, in particular the U.S. system. How do you think that your elementary or middle school education affects the way you think or the way you do your research? (K)

It's very difficult to say. I simply followed the curriculum that the government had set up. For each subject there was only one type of textbook approved by the government. The emphasis was on mathematics, Chinese language, philosophy, history, geography, good citizenship, etc. I think it formulated my general view about Chinese society, and any student who followed the curriculum rigidly was able to get solid training in mathematics and languages. At the college level, we had more flexibility. However, because of the turmoil in China, many professors did not have the chance to keep up with developing scholarship. Professors essentially gave lectures on their own. Fortunately, at that time pirated copies of modern textbooks were cheap and easily accessible, which allowed students to learn by themselves. I was a beneficiary of this; I chose to study economics at college, not because I knew what economics was about or was interested in it, but because of the influence of the Nationalist Party's education emphasis on restoring the glory of China. As you know, China was humiliated by the Western powers and the Japanese in the last half of the 19th century and in the first half of the 20th century. After the civil war, both China and Taiwan were very poor, so I thought that studying economics might provide insight on how to boost China and Taiwan's economic growth. That's why I chose to study economics.

Speaking of college education, do you recall any courses that you were particularly good at in college? Your major was economics. So, do you recall any particular course that attracted you? Anything that you found interesting in your college study, and perhaps also any extracurricular activities that you want to share with us? (K)

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I was an average student. I kept up with the progress of my classmates, and I wasn't doing particularly outstanding work in any field. At the National Taiwan University, mathematics was not emphasized for economics students. We had a course called "Mathematics for Economics" using R.G.D. Allen's (1960) textbook *Mathematical Analysis for Economists*. It is an elementary calculus book with emphasis on economic applications like the Slutsky equation. The course was offered in the first semester in college, before we got a chance to study intermediate microeconomics. So it was hard for us to appreciate the importance of the subject.

Who taught that course? (K)

His last name was Chen, but I don't recall his first name. He was, at that time, a relatively young instructor by Chinese standards. He knew we were not interested in mathematics. It was a required course, and students took the course but hardly learned anything. That's why I was particularly grateful to my high school friends, Yuankang Wang, Ling-Fong Li, and Hsinhsin Chou. They helped me get some background in mathematics that allowed me to study econometrics later.

Did you study statistics in college at that time? (K)

Yes, mostly descriptive statistics like mean and standard deviation. There was no mention of econometrics. The first time I heard the term *econometrics* was from Professor Henry Wan of Cornell. He had just gotten his degree from M.I.T. and, returned to Taiwan. It was just an accident. We were both waiting to see a dentist and we started chatting. That was the first time I had ever heard of econometrics.

After your college in Taiwan, you went to Oxford. How did it happen? Why did you go to the United Kingdom to study? (C)

Well, there were a number of reasons. In my generation, practically every college graduate in Taiwan wanted to go to the United States. I thought perhaps I could be a little different by pursuing a degree in Europe. Some Chinese scholars in the early 20th century went to Oxford and Cambridge and they wrote articles about their experiences. I thought it was fascinating. It was also by accident. I don't know whether you know the Nobel Laureate Friedrich von Hayek. He was famous in Taiwan because of his book *The Road to Serfdom* (Hayek, 1944). He visited Taiwan when I served in the Air Force after college. My father knew him and introduced me to him. He agreed to write a recommendation letter for me to Oxford. That was another reason I ended up studying in Oxford.

So, you got a letter from Hayek? That's really prestigious! What did you do in Oxford? What did you study there? (C)

I think being a student at Oxford or Cambridge could be a very pleasant experience. But because my English was poor, and because my father financed

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my education at Oxford, I felt pressure to get a degree all the time. I did not enjoy it as much as I might have. At Oxford the education was different from the U.S. system. The Bachelor of Philosophy degree was a two-year program. There were no exams in that two-year period. You had no idea how you were doing. Only at the end you took an exam. If you didn't pass, you got nothing. A fellow student from Ghana actually committed suicide after learning he had failed the exam. There were lectures scheduled for some subjects, but no formal instructions for any subject. You got some help from your tutors. But neither the instructors nor the tutors were necessarily the ones who set exam questions. Basically, the lectures and examinations were unrelated. The exam questions were also very vague; even the native students complained. The faculty's response was, "In the real world the questions you confront will never be so precise." I think that was, to some extent, true. Because I could hardly follow the lectures given in English, I had to study by myself. Had I been better prepared, I think I would have had a much more enjoyable life. But at that time I was always so nervous and I was very shy. My two years at Oxford were just consumed by studying, and I didn't take advantage of their college life. I wish I could live again and go to Oxford; but things in life, once you miss them, you can't get them back.

Even though you mentioned that Oxford requires highly self-motivated learning, do you still recall anything that had a significant impact on your study in the United States and your research career? Other than the vagueness of the questions, anything significant? (K)

Basic education in Taiwan is very rigid. You have to follow whatever the instructors say. At Oxford, you have to be more independent and you have to look at things that you think are important. Someone does not tell you, "This is important, that is not important." I think that free spirit was lacking in my education in Taiwan. While I was at Oxford, I was very shy and I didn't have much social contact with fellow students. I think that wasn't good. Friends could really be of great help. At Oxford people invite you to their rooms for afternoon tea or you reciprocally invite your friends and people to chat about anything. I feel that if you talk to Oxford or Cambridge graduates, they give you an impression that they have a much broader knowledge and perspective about the world.

After Oxford, you moved to Stanford. Was that for Ph.D. study? And what is the major thing that you can recall from your study at Stanford? Can you also talk about your thesis research and adviser? (K)

It was for a Ph.D. degree. I applied to Stanford because I read some of Marc Nerlove's papers at Oxford. They were very clear and very thought provoking. I also liked Gurley and Shaw's book, *Money in a Theory of Finance* (1960), and Chenery's development studies. They all had their affiliations at Stanford. So I thought Stanford could be a good place to study. Sometimes how life evolves

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FIGURE 1. Takeshi Amemiya, Ted Anderson, and Cheng Hsiao with their spouses.

really depends on the opportunity. At that time the Ford Foundation provided quite a few graduate fellowships to top universities in the United States, and Stanford had these fellowships to offer. So that was another reason I went to Stanford. When I arrived at Stanford, Nerlove, Chenery, Arrow, the scholars with whom I thought I wanted to study, had left. But fortunately, Takeshi Amemiya and Ted Anderson joined the Stanford faculty in 1968. They were the main reason I switched to econometrics. I have been learning from them ever since. If I had theoretical problems I could not solve, I would approach them and ask for help. They are so smart and knowledgeable. They have provided valuable advice even to this day.

Were both Ted Anderson and Takeshi Amemiya your advisers? (K)

Yes, I got help from both. My principal thesis adviser was Takeshi Amemiya. But because the two worked closely, I also got a lot of help from Ted. In the end I wrote two joint papers with Ted. Both men had a significant influence on me.

Can you talk a little bit about your dissertation? Your topic, how you formulated ideas and subsequent research? (K)

Stanford also had a free atmosphere. Basically, students chose dissertation topics by themselves. I started wanting to work on technological progress and investment. I read a great deal, but it was leading nowhere. I wasn't satisfied

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with the articles being published in that area, but I wasn't able to do any better myself. At that time, Stanford only promised four years of financial aid. I finished my course work in two years. A year of fanatic reading led nowhere. At the end of my third year, I said, "Jesus, I only have a year left! Better finish up!" I remembered reading Edwin Kuh's book, *Capital Stock Growth* (1963), in which he discussed the subtleties of using panel data for empirical analysis, and then I read Swamy's (1970) random-coefficient model paper. I thought, "Maybe I can generalize this and finish up my dissertation in this area." So I wrote a dissertation about issues in panel data analysis. My dissertation consisted of three parts. The first part was on the reconciliation between cross section and time series estimates. The second part generalized Swamy's random-coefficient model, which had only cross-sectional random variation, to both cross section and time series dimensions. The third part was on the aggregation issue, the difference between aggregated and micro data. So basically, my dissertation consisted of three separate topics related to panel data issues. I managed to submit my dissertation partly because my classmate, Fred Nold, volunteered to help polish my English. I think I am very lucky. I have a great family, great professors, and great friends.

Were they all about methodology, or was there any empirical work? (K)

I had very simple computation results just to illustrate my arguments. At that time software was not well developed. We had TSP, Time Series Process, to run regressions, a simple autocorrelation test or something else. I myself was not good at computer programming. So, my application was fairly straightforward and confined to using TSP. For the part on the random-coefficient model, I didn't have any application, because for that part I couldn't use TSP.

After graduating from Stanford, you went to Berkeley as an assistant professor. What was Berkeley like at that time? And what was your research when you started your career? (C)

Berkeley, at that time, was really exciting. Gerard Debreu, Daniel McFadden, Roy Radner, Tom Rothenberg, George Akerlof were there and very friendly to junior faculty. They were all great scholars. They had a vision for the subject, not just techniques. I talked to them, and I also attended some of their classes. Their lectures focused on general perspectives and didn't bother with technical details. I think having been at Berkeley really helped me shape my view of the subject, and I was very grateful to people like McFadden. He was my idol. He is so smart and such a great person. On the other hand, I had a secret inferiority complex. They were working at such a high level. If you ask me what is my greatest regret from my time at Berkeley, I would say I didn't fully take advantage of being their colleague, because I thought I was not at a level to speak to them.

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How many years were you there? (C)

Officially I was there for five years, but de facto, four. In my fifth year McFadden recommended me for a National Bureau of Economic Research (NBER) junior fellowship to do research at NBER with no teaching. At that time NBER also had a computer research center with Edwin Kuh in charge. So I went on leave from Berkeley to spend a year at NBER in Cambridge. While I was at NBER, the University of Toronto made me an offer with tenure. Since I did not feel I belonged to the same league as McFadden, Radner, etc. and I was not optimistic about getting tenure at Berkeley—and also since I liked Toronto as a city and the work of Dale Poirier, Tom Wilson, Mel Fuss, etc.—I decided to take Toronto's offer.

Did you develop any research with McFadden or Rothenberg? (K)

No, I was just a student learning from them. In my third year in graduate studies, I was interested in technological progress, but, as I said, I was not able to do anything with it. Later I learned that McFadden and his students like Mel Fuss did beautiful work on ex-ante, ex-post technological choice, which I found very impressive. However, at that time I had already moved in a different direction.

You basically kept your panel data research along the same line? (K)

To be frank, when I was in college I never thought I would become a professor and do research. In the 1950s and 60s people in Taiwan expected you to go to the United States for an advanced degree after graduation from college. I was just following the same trend and my family's expectations. I got a degree for the family, and then just intended to find a job somewhere. I was lucky to get a job at Berkeley, and the environment forced me to keep on working. I still remember Professor Amemiya telling me, "True research and learning start after you get the Ph.D." That is what my experience has been, and it has kept my interest on panel data since then.

After you spent a few years at Toronto, you moved to USC. And you have stayed there ever since, right? How many years at USC? (C)

I was officially in Toronto between 1977 and 1985, but from 1980 to 1982 I visited Princeton and Bell Labs. I moved to USC in 1985. In the early 1990s I also spent a few years at UC, Irvine.

You worked on econometric theory in relation to various areas, like time series, panel data, limited dependent model, and so on. What are your favorite theoretical contributions to econometrics? We know your very famous paper, Anderson and Hsiao (1981), about panel data. Besides that paper, you have made many contributions. Can you talk about your theoretical contributions? (C)

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I don't think I made any significant contribution. I work on different areas because I like to learn those subjects and the best way to learn is "learning by doing." However, I think the highest professional return would come from focusing on one subject with a good research agenda. You solve one problem and you go to the next level. So you become very focused and have a much deeper understanding of the subject. As for me, I like to learn different things to satisfy my desire for a broader perspective. What gets you most motivated is to try to solve some puzzles.

For example, what motivated me to learn about nonstationary time series analysis was that Peter Phillips and Steve Durlauf (1986) showed that the least squares estimator remains consistent even when regressors are correlated with the error of the equation as long as the regressors are I(1). That was different from what I learned from Ted Anderson about the simultaneous equation model. So I thought if I could relate those two subjects, I would be able to learn nonstationary time series. That helped me to learn new techniques.

Another reason I move from subject to subject is that I consider econometrics as a means of doing empirical analysis. I don't just want to be a pure theoretician. I wanted to look at how we can adapt the method we learned from theory to make it compatible with observed data. Different data sets raise different issues. For instance, suppose you have an underlying relation that is continuous, like y equals xb plus u and x is a continuous variable. However, the recorded x may not be continuous. For instance, income is often recorded in categories, say, between \$10,000 and \$15,000 but not an exact figure. So, if y is consumption, how do you estimate the marginal propensity to consume with income recorded in categorical intervals? Basically this is the sort of issue where I wanted to relate theory with the observed data. My research focus is mainly to satisfy my own curiosity. Then I just move from topic to topic following what interests me at the moment.

Which research strategy do you recommend to your students? Studying various things, or just focusing on one thing to have a high return? Having the highest return is not the sole purpose of research for some people. So, which one do you think is more appropriate for researchers? (C)

That really depends on one's goal in life. The famous Chinese mathematician, S.S. Chern of UC Berkeley, once said, "Those who do research have to be able to stand alone, without pursuing fame or money, no matter what others may think." The universe of unknowns is so huge that one cannot cover everything, no matter how smart one is. Anyone who wants to be a true scholar shouldn't be worried. I always like to say, "The universe of knowledge is unbounded. No matter how smart a person is, he cannot cover everything. As long as you are willing to study some subject in depth, you are the top one in that field. There is always success for people who are persistent." It's up to you to decide if you want to pursue your interests or to pursue fame.

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I learned another lesson from S.S. Chern, that is, whatever you are doing, be careful with your work. As I told you, I always want to know what other people are doing, and have an interest in learning this. But I also easily get bored about things I think I know. So if I thought I understood something, I would just move on, whether I truly understood it or not. Once my father and I went to Professor Chern's house for dinner. When we arrived at their house, he was still in his study. So we just sat in the living room, waiting for him. When he finally emerged, he apologized for keeping us waiting and said that he had had to review the galley proofs of a paper to make sure there were no mistakes. I was really impressed. This was such a famous scholar, yet he was so careful about his work. Who was I? I didn't even want to look again at those little things that I was working on. That really affected my attitude toward all stages of research.

You just mentioned your theoretical research on econometrics. Can you also talk about the empirical part of your research? (K)

I was more interested in history than economics, and as a result, I am interested in real-world issues like defense expenditures, economic growth, and all things that are related to the welfare of people, such as marijuana decriminalization. Since there will always be more topics than I have time and energy to pursue, whether I pick up a particular issue depends on whether I happen to stumble on a particular data set. For instance, I did some work on the issue of marijuana decriminalization (cf. Damrongplasit and Hsiao, 2009; Damrongplasit, Hsiao, and Zhao, 2010) because Vai Lam Mui invited me to visit Monash. While I was there, his colleague, Xueyan Zhao, talked to me about the data she had been collecting. I thought perhaps I could do some analysis with that data set. That's how I ended up doing this empirical analysis on marijuana decriminalization. Econometrics, after all, is supposed to analyze economic and social problems, not be just a purely abstract subject. The foundation of empirical analysis is data. However, people who collect and clean data do not get recognized. I'm also pragmatic. I don't want to spend time collecting data. When I do empirical analysis, in most cases it's because I happened to run into some data set and the data contain issues I'm interested in. I didn't spend time on collecting the data or anything like that.

You have talked about your research, both theoretical and empirical. Nowadays some econometricians start their careers focusing only on methodology, purely theoretical, and, perhaps in their whole lives, they never do any empirical research. Even when they do, those are small econometric exercises, not thorough and deep applications. What do you think of those researchers? Do you think it is just fine to focus on purely abstract mathematical pursuits? Or should people try, rather, thorough empirical applications? (K)

If people want to do econometrics, a solid background in math is a necessity. Rigor and specialization are for the advancement of any discipline. I advise young

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people to get a solid math background. It will put them at an advantage. I didn't have a good math background. It was tough to catch up. So I would advise them to study as much math as possible. Even though some topics may not seem directly relevant at the moment, it is still good training for developing rigorous logical reasoning.

Sometimes a subject may not seem directly or immediately relevant. However, even if it is not relevant today, you can never say that it won't be relevant tomorrow. For instance, when I first encountered papers in *Annals of Probability* on random-matrix theory, I thought it was irrelevant to my research and had no intention of learning it. However, computing speed and storage capability have enabled researchers to collect, store, and analyze data sets of very high dimension. Then it becomes a very relevant subject because classical asymptotic theorems under the assumption that the dimension of data is fixed do not appear adequate to analyze issues arising from very high dimensional data. So random-matrix theory is no longer just an abstract topic.

For many of the developments in science, what appears very abstract today could become very relevant tomorrow. A scholar should have freedom to pursue whatever appears interesting and relevant. That's basically my view. On the other hand, after all, econometrics is a branch of social science, not pure statistics or pure math. Therefore, I would think people should have a good understanding of institutional and historical backgrounds, not just for doing empirical analysis. Such knowledge could also offer them a better perspective on what is important even in the study of abstract issues.

In a nutshell, there should be no boundary in our way of thinking about research. Whether it is purely theoretical or empirical, we just do it if we like it, right? And theoretical things may become very important in the future even though they are not regarded as important now. So there should not be any boundary in our thinking. That's your attitude. (C)

Yes, that's my view, and that's related to S.S. Chern's saying, "Doing research, one has to be able to stand loneliness." You research something independent of whether people pay attention to it or not. If you can achieve something, sooner or later you will be recognized.

Can you cite some examples in econometrics, in econometric theory, that were ignored in the beginning stage but later attracted attention from other researchers? You just mentioned random-matrix theory. But that's not from econometrics, that's from probability. So can we have some examples in econometric theory that were totally ignored in the beginning but later attracted researchers' attention? (C)

Econometrics is application oriented. Even if you do theory, you should have potential applications in mind. Empirical analysis requires synthesis and

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interpretation of institutional conditions and a good understanding of human behavior. It is different from mathematics. Mathematicians just look at the logic or internal beauty of a subject. Offhand, I can't think of any examples. If I think of some, I'll write to you.

I also think that's a tough question. In econometrics, some theories are born without any attention to applications and then they are totally ignored. (C)

I would think a pure math problem without a potential application probably will not generate much interest to econometricians. On the other hand, there are also some practical topics that did not appear to generate much attention among economists. For instance, Hans Theil (1954) published a book on linear aggregation in the 1950s in which he discussed the issue of heterogeneity among micro units in aggregate data. This issue has been studied off and on by such economists and econometricians as Jean-Michel Grandmont and Arthur Lewbel. However, it did not appear that the subject had become a mainstream research topic for economic theory or econometrics. I began to appreciate the importance of the subject only after I found that there were discrepancies about the Japanese money demand at the prefecture level and the national level (cf. Hsiao, Shen, and Fujiki, 2005) when I visited the Bank of Japan. I know too little about macro, but it seemed to me that one branch of macro focused on developing complicated behavioral rules from the individual intertemporal optimization framework. Then they say, "That's for everyone" and treat it as the behavioral rule at the macro level. But even with individuals following the same decision process, if there is heterogeneity among individual units, the observed relationships among the aggregate data could be very different from that of the micro data. If heterogeneity is indeed present at the micro level, then shall we build the model at the macro level or the micro level? After all, policymakers are supposed to make decisions based on the outcomes for the group, not a particular individual. Then what is the appropriate level of aggregation? I am not sure macroeconomists or econometricians have sufficient appreciation of the importance of heterogeneity in aggregation.

Can you talk about your famous book on panel data (Hsiao, 1986, 2004)? How did you start the book? (K) It has been cited more than 5,000 times, according to Google Scholar. (C)

In 1980 I was visiting Princeton University. I think at that time Cambridge University Press had just signed a contract with the Econometric Society to start the Econometric Society monographs series. The publisher, Colin Day, dropped by my office and asked me if I would write a book on identification because he had read my survey article (Hsiao, 1983) on the topic for the *Handbook of Econometrics* and liked the paper. Since I had just finished that paper, I was bored with the subject. So I asked, "Could I change the subject to panel data?" He agreed and

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gave me a contract. That is how it started. If he hadn't approached me, I probably would not have written the book. Since I was still in an early stage of my professional development, I needed publications. He offered me an opportunity, so I started writing the panel data book. It was purely accidental. The book caught the attention of the profession, partly because more panel data sets became available and partly because people had begun to realize that panel data offer richer information than pure cross-sectional and time series data could have offered. I was lucky to be in the right place at the right time.

These days, if you look at famous journals, most empirical papers are using panel data rather than cross-sectional or time series data. In this regard, your book must be very helpful to many researchers. (C)

That is for others to judge. I think the major reason there are more empirical papers using panel data is in the informational content of panel data. Panel data allow people to construct more sophisticated behavioral models. If you consider any model to be a simplification of reality, you want as simple a model as possible. If you include too many variables in the analysis, you might lose sight of the crucial relations among the important variables.

But real-world phenomena are complicated. Not all heterogeneity across individuals and over time can be captured by the included observed variables. If you have only cross-sectional data or time series data, you treat the effects of that unobserved heterogeneity as random variables. Panel data allow you to play some tricks to capture some of the impacts of unobserved heterogeneity. That, I think, probably contributes to the proliferation of empirical applications of panel data analysis.

From a methodological perspective, it's also very challenging. How do you model this unobserved heterogeneity? If your panel data cover only a short period of time, using the standard time-invariant individual-specific effect to capture unobserved heterogeneity may seem reasonable. But if the time series dimension is also large, is that still a reasonable assumption? That's questionable. Then how should you model it?

These days, some people assume that both N and T are infinite in panel data analysis. What do you think of that? (C)

Models with large N and T raise many challenging issues. When T is finite and N is large, basically it is a one-dimensional issue in terms of the application of large sample theories. When both N and T are large, it is multi-dimensional. The application of large sample theories in multiple dimensions can be complicated. However, even in this case, you can still have beautiful large sample results, but they may not offer much guidance for practitioners. Large sample results approximate the finite sample behavior of some estimators reasonably well in one dimension, but the same may not be said for multi-dimensional cases.

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Moreover, when T is 100 or 200, I am not sure if it really corresponds to the case when both N and T go to infinity and T over N is a nonzero constant. So I am not sure if large sample results can approximate the behavior of some estimators well in finite samples, even under the simplest standard formulation.

The other concern about the model with large N and T is how to model unobserved heterogeneity. Is the standard approach of assuming time-invariant individual-specific effects for large N and finite T still a good approximation? That's questionable. In the large N and T case, how do you model cross-sectional dependence? I mean, if you assume a factor model, when N becomes 5,000, how do you estimate a factor-loading matrix of dimension 5,000 by k? Is this an empirically feasible approach? But in many cases, you do have cross-sectional dependence because in a model specification some common variables could be omitted, such as a financial crisis affecting all different cross-sectional units, though their impacts may not be of the same degree for each cross-sectional unit. How do we capture this cross-sectional dependence to make the law of large numbers and the central limit results hold?

That's probably a future research agenda, right? We can model cross-sectional dependence with panel data. People have already started working on it. But that's a kind of topic we should give more attention in the future. (C)

Yes. For instance, Hashem Pesaran has a very beautiful idea of using cross-sectional means to wipe out cross-sectional dependence (cf. Pesaran, 2006). The method works for the linear static model. But what if you have a nonlinear model? I want to study more about it, but I haven't been able to.

What other future research agendas do you see for panel data? (C)

The future of research should be decided by the younger generation. It is up to them to decide what issues are important and relevant. I do not think I should affect their perspective. As for me, I am interested in such issues as how to model unobserved heterogeneity when both N and T are large, how to control their impacts in nonlinear models, and how to model cross-sectional dependence. Take the case of modeling cross-sectional dependence, really a tricky issue. The factor approach is flexible, but if no restriction is imposed, it implies strong cross-sectional dependence. If you have strong cross-sectional dependence, and if you don't capture it in your statistical analysis, then the standard large sample results will not hold. However, as I mentioned earlier, it's not really feasible to estimate the factor-loading matrix if N is large. On the other hand, if there is only weak cross-sectional dependence, I mean dependence only around units in the neighborhood of a given unit, you can ignore the cross-sectional dependence if N is large. The spatial approach assumes one knows the spatial weighting matrix. But how come one knows so much about the relative strength of the interaction between the

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units, yet the only thing unknown is the proportional factor? I'm always puzzled. Modeling cross-sectional dependence fascinates me.

So you are not satisfied with the way people model cross-sectional dependence. (C)

I'm not completely happy, but I don't have better alternatives. I think that's a fascinating subject to study. The other fascinating subject is nonlinear panel data models. We know that for a linear panel data model you can do various transformations to eliminate unobserved heterogeneity if you are assuming standard additive, fixed or random effects. But if your model is nonlinear, I don't think there is any general methodology you can apply if incidental parameters are present. You have to deal with it case by case. Even if a consistent estimator can be found, the conditions one has to place on the data could be very restrictive. For practitioners, it may not be very useful.

Let's take a simple binary choice model with state dependence, namely, current choice depends on the choice of the last period, as an example. The way Honore and Kyriazidou (2000) derive the consistent estimator is very clever, but the restrictions one has to place on the data involve at least four time series observations for each individual. However, only a subset of individuals that switch positions at the two intermediate periods can be used. Your sample is further reduced by the restrictions on the X variables. The X variables at the third and fourth periods should be identical, but to get a good estimate, you also need the X variables at the second and third periods to be very different. You will probably end up with a very small number of observations that meet these conditions.

Moreover, this method only works with the simple formulation of heterogeneity. You could have a more complicated structure of unobserved heterogeneity with nonlinear models. Say, the coefficients also vary across individuals. Heckman and others have given numerous examples showing that economic theory would imply coefficients vary across individuals. Furthermore, the variation of coefficients could be correlated with the regressors. I don't know any simple way to handle these problems.

Some scholars have suggested changing the focus to finding a bias-adjusted estimator instead of trying to find a consistent estimator. The trouble is that bias-adjusted estimators often depend on unknown true parameters to make the adjustment. When you use estimated parameter values, not the true ones, for the adjustment, the method may not work as well as expected. People have studied a simple linear dynamic model with both individual- and time-specific effects and derived the formula for bias-adjusted estimation. The correction depends on the true value, but you don't have the true value. You only have estimated parameter values. Some Monte Carlo studies done by Tahmiscioglu and me (cf. Hsiao and Tahmiscioglu, 2008) have shown that the bias-adjusted method did not appear to do well with small T because when T is finite, the estimated value is quite severely biased. I think nonlinear panel data models are very fascinating to study, but the progress so far is still limited. People have clever strategies, but if you

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want applied people to use them, they also have to be easy to implement. So far we have not achieved that.

What do you think of nonstationary panel data analysis? (C)

Nonstationarity in the time dimension certainly exists, just like aggregate time series data. If T goes to infinity, I would assume that the results from the time series literature could be applied. If T is finite and if cross-sectional units are independent conditioning on the explanatory variables, then standard large sample results can be applied on the cross-sectional dimension and you do not need a long time series to distinguish whether the data on the time series dimension is stationary or nonstationary. That is one nice thing about having panel data. If observations are cross-sectionally correlated, it could raise new issues. That's why I like to say that panel data offer many interesting challenges for both theoretical and applied econometricians.

In your career, you have visited different institutions. Could you tell us how those institutions have affected your research? Or the other way around? (K)

I like to travel. I like to meet different people. Attending conferences, you are somewhere for just a short period of time. The conference could be very stimulating, but you go there, give a talk, and you leave the next day. You don't really get a chance to explore and see differences among different people. Visiting different places has really helped broaden my perspective and has also changed my view on a lot of things. I think that's important. When I was growing up, because of my education and family background, my mind was focused on restoring the glory of China. Now I think that kind of nationalist view was silly. Everywhere people are the same. They want good lives and they want peace. People can be friends with each other and shouldn't be treated like enemies because of race or historical rivalries. Personal contact can change the impressions textbooks gave you about other groups of people and their lives. Meeting others can also stimulate your research. I mentioned the reason why I did empirical analysis on marijuana decriminalization (cf. Damrongplasit and Hsiao, 2009; Damrongplasit et al., 2010). If I hadn't visited Monash, I probably would never have written a paper on that subject. My work on the paradox between aggregate and disaggregate data (cf. Hsiao et al., 2005) was stimulated by conversations with Dr. H. Fujiki at the Bank of Japan. All these interactions have really broadened my perspectives.

You also studied the effects of the Chinese takeover of Hong Kong on its economic growth (cf. Hsiao, 1999). (K)

That was again stimulated by conversations with scholars at the City University of Hong Kong, like Eden Yu, H.S. Ching, and Ken Chan. I encourage

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people to visit different places for extended periods. It is more beneficial than short visits. Because of my background, I like to come to Asian countries, and I have been back repeatedly. I can see the growth in academic collaboration over time. In the past, when I came to Asia I did not see much interaction locally. Neither did I see much interaction among scholars in different Asian countries or regions. But I have noticed significant changes in the last five years. Scholars in the Asian countries are cooperating and interacting with each other much more. I think the Symposium on Econometric Theory and Applications (SETA) initiated by Chung-Ming played an important role. You started by getting people from Korea, China, Hong Kong, and Singapore together and then followed up with In Choi running the SETA meeting in Hong Kong. I understand the next meeting will be in Australia. This trend of appreciating the benefits of interactions and mutual stimulations appears growing among Asian scholars. For instance, Songsak Sriboonchitta at the Thailand Econometric Society now works hard to get scholars from Taiwan, China, Korea, Japan, and even Europe and North America, to participate in their annual conference.

Following up on what you described about the recent developments in the Asian academic community, how do you see the future of development in Asia? What should we do here? (K)

Japan has always been at the forefront in the Asian community. Japanese econometricians like T. Sawa, K. Morimune, N. Kunitomo, F. Hayashi, H. Ichimura, and K. Tanaka have always been in close contact with scholars in North America and Western Europe, but the same may not be the case for scholars in other Pacific Rim countries. However, the local scholars' eagerness to learn and to catch up is indeed impressive. The improvements in academic fields in Asia over the years are quite remarkable. While I was a student at the National Taiwan University, I didn't even know what econometrics was about. Now we have first-rate econometricians in nearly every Pacific Rim country. In Choi, Yoon-Jae Whang, M.J. Lee, Simon Lee, Chang-Jin Kim, and Jae-Young Kim are now in Korea, and Joon Park also goes back to Korea regularly. Chung-Ming Kuan is in Taiwan, and Songnian Chen and Jun Yu are in Hong Kong and Singapore, etc. This is an excellent group of econometricians, comparable to those in the West. That's a noticeable difference. Some graduate training programs in Asia have also improved significantly and can match their counterparts in the West in certain fields. In the past, with the exception of Japan, locally trained Ph.D.s in Asia could not publish in international journals. Now, you find locally trained Ph.D.s publishing parts of their dissertations in international journals. In my visits to Asia, I ran into some of Chung-Ming's students in Taiwan, some of Songnian's students in Hong Kong, and some Chinese students trained by Yongmiao Hong and Qi Li in China. They have all managed to publish in international journals. This improvement is remarkable.

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Quantitatively, you don't have as many econometricians in any one country as in North America, so the field coverage in each country or region is not as broad. That makes the interactions among Pacific Rim scholars even more important. However, because of geographic separation, it appears that the interactions among Asian scholars are not as frequent as their counterparts in North America and Western Europe. I hope I can see more interactions among scholars in the Pacific Rim countries. Perhaps someday you will also set up a joint graduate training program as the European countries have been doing. There is an economy of scale in academic research and teaching. If you have more people, you are more likely to cover different fields to match diverse interests. If you have fewer, you can't cover every field. I spent some years at the University of Toronto. It was a very happy experience because the department was very big. We had more than 50 faculty. I could find someone with expertise in practically any subject. I think size does matter.

You have been a co-editor of the *Journal of Econometrics* for almost two decades by now. What do you think you have done for the journal? What do you think of the future of the journal? (K)

I think the *Journal of Econometrics* has played a significant role in promoting and developing the study of econometrics. When I first came out of school, there were only three practical possibilities for any econometrics paper: *Econometrica, International Economic Review,* and the *Journal of the American Statistical Association*. These three journals are general-purpose journals, not specifically for econometrics. The *Journal of Econometrics* offered another possibility for people to get their work published. The journal has grown substantially in size and in the fields covered in the last four decades. The success of the journal has to be attributed to the founding editors, Arnold Zellner, who passed away last August, and Dennis Aigner. They set up a nice system and initiated the annals series, which provides venues for other scholars to participate in the editorial process as well. I just followed in their footsteps. That was a much easier job than founding a journal. The journal has a number of co-editors. It not only reduces the workload for each co-editor, it also allows broad coverage of different fields. The journal's associate editors and referees have also provided invaluable services to the growth of the journal. I have learned a lot from them. In this information age, maybe journals are less important. But in the early days, when you saw a published paper, the paper was already old, three or four or five years after the paper was initially written. Being an editor allows me to see papers much earlier than the general audience. That's another joy.

The journal is a privately owned academic journal. There are differences between privately funded and society-supported journals. A society-supported journal is much more assured in its success because members are behind it. With a privately supported journal, the publisher has the final say on the direction of

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the journal and the composition of the editorial board. If the publisher is willing to continue supporting the journal, and supporting the profession, and if the journal can maintain a good editorial board, I am confident about the future of the journal.

The Econometric Society started a new journal. Do you feel any pressure? (K)

There is a pressure, but I also believe that the only way to progress is through competition. I have always favored competition. One shouldn't try to monopolize things. The problem is not just that monopolists make extra profits. The greatest sin of monopoly is that it prevents progress. Having more journals puts competitive pressure on editorial boards. It is really helpful for the development of science. Moreover, we are human beings. Humans make errors. My judgment is not necessarily infallible. When I reject a paper, I may make mistakes. If there are other journals, they give authors another chance to get their papers published.

Do you have any hobbies in your personal life? (C)

I know you people all work very hard. My advice is to keep up your daily exercise. That is really important. When I was young, even in my middle age, I didn't do any exercise. I started probably 10 or 15 years ago. It really keeps both my body and my mind fresh. On weekdays I swim, and on weekends I play tennis with my wife. I don't have specific hobbies. That's why I say I can't retire. If I retire, I'll just sit there waiting for my death.

How about your family? I know you have four kids. You take very good care of your family. (C)

I try. But I don't think they like my constant nagging. I have a good family, and a good wife, and four lovely children from my perspective. It's very time-consuming raising children, but they have also brought so much pleasure and color in my life. I feel very fortunate.

I was told one of your daughters did quite well in school. Who was it? (K)

My elder daughter, Irene, is smart and a very hard-working person. She chose English literature as a field, and that's a very tough field. I ran into Jim Heckman at Chongqing in China. I told him that my daughter was going to Chicago to work on her Ph.D. in English literature. Jim opened his eyes widely and said, "There are no jobs!" I replied, "I knew, but I couldn't tell my daughter." When I went to Chicago in October 2010, for Arnold Zellner's memorial service, Jim

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FIGURE 2. Cheng Hsiao and his family.

was there. My daughter also went to the memorial service, because Arnold and his wife, Agnes, looked after her very well while she was a student at Chicago. I pulled my daughter to say hello to Jim and said, "This is the Nobel Laureate who said English literature has no jobs," and Jim said, "My daughter is now studying history."

Do you have any suggestions for young students who want to be economists in general, and econometricians in particular? How should they prepare themselves?

Try to learn as much math as possible. Also, keep in mind that econometrics is a branch of social science. The purpose of studying econometrics is to analyze social data. I shouldn't say which field to study and which directions to go, because the future of the field is up to the young people to develop. It's not for me to say, "This is a good field and that is not a good field." But I hope they keep in mind that the purpose of studying econometrics is to analyze data, not to treat it purely as a mathematical problem. Of course, that could be interesting and challenging, but that's not what econometrics is about.

All right, thank you very much for spending your time with us. It was very interesting and very informative. I learned a lot. I'm sure Chung-Ming also learned a lot from you. Thank you very much. (C)

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FIGURE 3. Chung-Ming Kuan, Cheng Hsiao, and In Choi.

NOTES

1. The questions asked by In Choi are marked by (C), and those by Chung-Ming Kuan by (K).

2. Address correspondence to In Choi at Department of Economics, Sogang University, Seoul, Korea; e-mail: inchoi@sogang.ac.kr; inchoi@gmail.com.

3. Department of Finance and Economics, National Taiwan University, Taipei, Taiwan.

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