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THE ET INTERVIEW: PROFESSOR PETER SCHMIDT

Interviewed by Robin C. Sickles, Rice University

Peter Above the Bosporus, 2020.

Peter Schmidt was born in Port Washington, Wisconsin, on August 8, 1947. He grew up and attended grade school and high school in Port Washington before leaving for East Lansing and Michigan State University in 1965. He obtained his B.A. (Economics) in 1969, his M.A. (Economics) in 1970, and his Ph.D. (Economics) in 1970. His Ph.D. thesis was directed by Jan Kmenta. He began his academic career as an Assistant Professor at Wayne State University in 1971, then moved to Chapel Hill where he became an Assistant Professor at the University of North Carolina in 1971 and then an Associate Professor in 1974. In 1977, he became Professor at Michigan State University, replacing James Ramsey. Peter has remained there since and in 1997 became University Distinguished Professor. He also has held many visiting positions in universities across the world.

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His research has been wide-ranging in its topics and approaches. He has made major contributions to the literature on panel data econometrics, efficiency and productivity analysis, time-series econometrics, limited dependent variables, and the economics of crime.

Peter is a Fellow of the Econometric Society and a Founding Fellow of the International Association of Applied Econometrics. He is a Principle Founder of the International Society for Efficiency and Productivity Analysis, a Fellow of the American Statistical Association, a Journal of Econometrics Fellow, a Fellow of Econometric Reviews, and received the first Lifetime Achievement Award given at the European Workshop on Efficiency and Productivity Analysis in 2017. Three special volumes have been dedicated to him. One is in progress (Kumbhakar, Sickles, and Wang, 2022). The other two are Festschrift in Honor of Peter Schmidt: Econometric Methods and Applications (2014) and Special Issue of Econometric Reviews in Honor of Peter Schmidt (2017).

Peter has published more than 170 refereed articles and 4 books. His early work on limited dependent variables, starting with Robert Strauss, was path-breaking. This was followed shortly thereafter by the introduction of the stochastic frontier model (Aigner, Lovell, and Schmidt, 1977; hereafter ALS). The ALS paper was singled out by a foundational figure in econometrics (Amemiya, 2009) as one of the most influential papers ever published in the Journal of Econometrics. Peter then refocused his attention on time-series econometrics. The term "refocused" was chosen carefully as his dissertation at Michigan State University (MSU) was a study of second-order autoregressive properties of time series. His time-series work on testing for stationarity led to another iconic contribution, the so-called KPSS test (Kwiatkowski et al., 1992). This is a test that every major computing platform in econometrics uses to screen time-series data for non-stationary behavior. At roughly the same time as his work in stochastic frontiers and time-series econometrics, Peter also contributed highly regarded papers to the panel data literature. His work on time-varying panel estimators for stochastic frontiers (Cornwell, Schmidt, and Sickles, 1990) was one of the Journal of Econometrics' All-Star Papers for 1989–1999.

Might we start out with a bit of the early experiences that put you on the remarkable path you have followed in your career and accomplishments? Would you like to say anything about your upbringing or high school experiences?

I grew up in a lovely small town on the shores of Lake Michigan, Port Washington, Wisconsin, a town of about 5,000 people. It was a working-class town with a power plant, two companies that made power equipment like riding lawn mowers and snowblowers, a chair factory, a shoe factory, that kind of thing. My father worked at the power plant.

Peter Schmidt and His Older Siblings Mary, Phillip, Paul, and John, 1948.

Of the 5,000 people, probably 4,000 were Catholics and a large fraction were children. The town was dominated in many senses by St. Mary's Church, which is on the biggest hill, overlooking the downtown. And I went to St. Mary's elementary school. The students there had names like Schmidt, of course, and also Bichler, Wiskerchen, Dickman, Poklasny, Pujanauski, and so forth. Then later, I went to the public high school, and there I met some Protestant kids with whom I became friends. The Protestant kids tended to have names like Allen, Miller, and Wilson—rather different. I discovered that our education had also been somewhat different. In the Catholic school, we learned how to diagram sentences, for example, and also how to read music, especially Gregorian chant, whereas they learned things like that a potato turns purple when you put iodine on it.

In high school, I was a nerd. I did tennis, basically because nobody else did tennis. I did debate. These were definitely not fashionable things like football or basketball or track. However, I'm very proud to tell everybody that I scored 42 points in basketball versus Fredonia, and I generally fail to mention that that was only a church league game. The person I was most envious of was Doug DeGroot, who was a nice guy and smart, but also a multisport athlete, and who had a very cute girlfriend.

There were a few intellectuals in the town, even a few intellectual teachers. One of them I specifically remember was Clarence DeGroot, Doug's father, and he was not only a good math teacher, but also a bit of a philosopher. I can still remember him saying, "Time is irretrievable. A minute, once lost, can never be regained." And of course, I was too young to appreciate that at the time.

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You had an unusual path to obtaining your Ph.D. Explain the circumstances. You attended MSU for all of your degrees. You presumably had other options. Why did you make that decision?

Well, although Port Washington was a lovely town, I found it very small and very dull. So I wanted to get out of town, so to speak. Most of my friends went to the University of Wisconsin at Oshkosh, not because they couldn't get into Madison, but because they basically wanted to continue high school, as much as they could. I did not want to continue high school. I wanted to get out of town. I would have gone to Madison. But MSU offered me a full scholarship, and full academic scholarships at that time were rare, so I took it.

When I got to MSU, I was a chemistry major. In my first term there, I took the usual stuff that a chemistry major would take, like chemistry and math and freshman English, but I had one more course I needed to take. The ideal course would fit my schedule by meeting Monday, Tuesday, Wednesday, and Friday, not Thursday, at 10:20. And so I looked and the only course I could find that would fit that schedule exactly was econ one. So I took econ one, and I liked it better than chemistry, and eventually I became an econ major. And for the rest of my time as an undergrad, I took economics courses, math courses, and so forth.

Eventually, I graduated. I had a National Science Foundation graduate fellowship, so I was going to be admitted almost anywhere I wanted to go. I applied to and was admitted to Wisconsin, Yale, MIT, and of course MSU. I would have gone to Yale, I'm not sure why, but the draft raised its ugly head, and I didn't want to go there just to be drafted, so to speak, so, in Year One, I started grad school at MSU. Then, in Year Two, I stayed because MSU would let me finish early, and they could get me out of the draft by making me an Instructor. That is (or was) a professorial rank generally given to people who don't have their Ph.D. done, but, in my case, it was given to me to keep me out of the draft. And the only instructions were that I was not allowed to go to faculty meetings.

A story that I think is funny is, when I was admitted to all these other places, the Graduate Director of Yale called me up, to sort of nudge me into coming to Yale. He said, you know, I'd like to welcome you to Yale, and other things of that sort. And I said, I was sorry, but I wasn't going to be able to attend Yale. And he said, honest to God, I do hope you're not going to Harvard. And I said, no, actually I'm going to stay at Michigan State. Then, there was a very long pause, and he said, "*Well I hope you'll be very happy there*." And I was.

Oh, that's great. That's a great story.

When at MSU and living in East Lansing were there students whom you worked with whom you have continued to communicate with since?

With respect to my undergraduate friends, not really. I do know where some of them ended up. One of them is (or was) at Yale in a nonfaculty role. Another one is a chaired professor of biochemistry at Illinois. Another one started a software company. But I kept only very loose track of them. Again, with respect to friends that I had when I was a grad student, not really. I wasn't a grad student very long, and I didn't get to know the other grad students all that well. But a lot of them were quite successful. Of course, after I came back as a faculty member, yes, I've worked with lots of Ph.D. students after they got their Ph.D. Actually, I counted, and it's about 15 such students, so they're the people I've kept track of and in touch with and done things with.

Being rather young when you finished your Ph.D., and given the draft status of teachers at the time, you had real incentives to teach. However, you completed your Ph.D. under the direction of Jan Kmenta when you were only 23. What was it like to be competing with freshly minted Ph.D.'s who were substantially older than you when you went on the job market?

Obviously, on the job market, it was a disadvantage being a student from MSU as opposed to, say, Yale or MIT. And, honestly, I didn't know as much, having been at MSU for 2 years as I would have if I had been at Yale or MIT for 4 years. But I didn't really have any age-related issues that I know of.

I do have a couple of age-related stories. The first is that, when I was 25, I submitted an abstract for the Econometrics Society winter meetings, and they sent me a letter saying that my abstract had been rejected, because there was another similar paper and they wanted to give priority to younger authors. The second story is about an offer I got from the University of Maryland in the spring of 1975. They recruited me, I went up there and gave a job seminar, and they eventually said they wanted to make me an offer as a full professor. They told me the terms, and I intended to accept the offer. But they said I should come back up again, because they wanted to show me around the town more than they had, and also bureaucratically I had not met the Provost on my visit, and I had to meet the Provost before he would authorize the offer. So I went to see the chair, and the chair showed me the unsigned letter and said, you go downstairs and see the provost and, on your way back up here, he'll call me to authorize the offer and I'll sign the letter and give it to you. So I went and had a nice chat with the Provost, went back up to the chair's office, and he looked like something was seriously wrong. And indeed, there was: the Provost had vetoed the offer, because he had not realized I was so young, which was 27, and that didn't make me happy. In the end, they "compromised"; they said come in December instead of September, and by the way we're cutting the salary by $1,000. And I turned that down. So, but for that Provost, I could have been a terrapin. I can laugh about that now, but it's a good example of how a bad administrator can make mischief.

Second that, and, you know, better a ram or a Spartan than a turtle, right?

I might have gone into a shell.

The University of North Carolina (UNC) during the time you were there was quite a happening place. That said, given your prolific publication record you could have gone to some very highly ranked programs before ultimately taking the position your mentor Jan Kmenta had and moving to East Lansing where you *are now University Distinguished Professor. What were your reasons for making those career decisions?*

Well, when I was thinking of leaving UNC, I was recruited by some good places like Maryland, New York University (NYU), Cornell, Rochester, University of Southern California (USC), and Penn, but not really by top-10 places except maybe Penn. I told you what happened at Maryland. NYU, Rochester, and USC I did not find appealing, and probably they sensed that and there was no offer. Penn, however, is a more interesting story, and perhaps a fork in my professional path. I went there for a job seminar, and talked to various people. And I was in Albert Ando's office just before noon. We had a chat, and then he sort of leaned forward and softly said, what would it take to get you to Penn? Well, I had tenure at UNC and I said that I would have to have tenure. And he said, perhaps it is time for lunch. They hired Bobby Mariano instead.

*While you were at UNC you published the Marcel Dekker book Econometrics (*Schmidt*,* 1976*), referred to by many as the "Little Green Book," and one of the most influential texts for econometrics graduate students at the time. It was particularly helpful to students for their econometrics qualifiers. From MIT, to Cambridge, to Princeton, to UNC, and to many other graduate programs, the "Little Green Book" was essential and iconic. For all your UNC students and for many distinguished scholars who were in other graduate programs during 1970s and 1980s, your book was the bible for the linear model. Can you discuss a bit what prompted you to write such a book at this early time in your career, when text books per se are less likely to be a focus of one's scholarly pursuits?*

Two things. First, at the beginning, I was having trouble getting some of my papers published. And I thought, mistakenly, that a book would be easier to get published. I didn't understand the difference between solicited books where the publisher approaches a senior distinguished author, and unsolicited books like mine where someone just sends a book to publishers. They're not so easy to get published. The second thing is that I thought, correctly, that I would learn a lot, filling in gaps in my knowledge, by writing a book. That was successful. I never expected to make any money. And I didn't.

Let's stay with the "Little Green Book" for a moment and your perspective on what were then viewed as new and exciting developments in econometrics and what are now viewed in the same light. For example, Section 2.3 discusses multicollinearity and ridge regression. Now, we have shrinkage estimators and machine learning. Sections 3.3 and 4.6 cover IV, 2SLS, and simultaneous equations. Now, we have the credibility revolution and the local average treatment effect (LATE) theorem. Can you give us your take on what we've learned that is new or are we largely revisiting old ideas with more general theoretical underpinnings? Is it in part because the literature is so vast that little time is spent on rereading the masters?

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I think that most of what is new is new. Maybe not all of it is useful, but most of it is new. There are some exceptions, there is some recycling, for example, it used to be that we added a quadratic, and we called that a flexible form. Now, we call it a nonparametric sieve. Lots of important ideas were not available or at least not appreciated by econometricians back then. Think of ARCH, unit roots and cointegration, the bootstrap, non-parametrics, sparsity, weak identification, and partial identification. Some of these things originated with the real statisticians, but a lot of them originated with econometricians, and are "new."

What are your thoughts specifically about the relevance of big data and machine learning techniques in the field of econometrics, especially for causal analysis?

I think it's important to distinguish Big Data from Big Model. With respect to Big Data, more data can't hurt. With respect to Big Model, remember that you can have big models with little data—you just put in lots of terms.

I am not a fan of the sparsity assumption and I don't necessarily understand why it's better to let the data tell us that there are six variables with nonzero coefficients than to try to pick them out ourselves. It seems to me if you want to let the computer make these choices what we need is something akin to the short memory assumption in time series—that even if there was an infinite set of variables, the sum of their coefficients is finite. But I don't know how to formalize that.

I remember having an argument with Ed Leamer in the early 1980s about whether you should ever drop a variable. He said that no Bayesian would ever drop a variable. They would just downweight some of them, given the data. I told him that once you start with a finite set of variables, you have already dropped a lot of variables. There is always an infinity of things you could include. My fanciful example of something not to include was, in a time-series consumption function, the size of the polar ice cap, because it was collinear with the constant, even though it was potentially relevant because if it melted, all sorts of bad things would happen. At that time, that was intended as a joke. Of course, now it's not a joke.

You have been at MSU for the bulk of your academic life and you have had a major role in building its very strong econometrics group there. What is it about MSU that attracted you there from UNC in the first place and has anchored your professional life in East Lansing?

Well, I guess it feels like home. They've always treated me well. I suppose the initial attraction was at least largely money, and top-10 places never wanted to hire me. I turned down approaches from some top-20 places, but not top-10. If they had asked, I probably would have listened.

Following up on the UNC versus MSU traditions, were the pickup basketball games better at UNC or MSU? I am curious since I was a teammate of yours on the UNC pickup games.

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I guess they were better at UNC. At MSU, after the first few years, there just weren't enough American-born males to make up a game and we had to play with people from other departments. I do remember that we had economics faculty versus grad student games for a while. The faculty usually won. And then one year I ran into Myung Sup Kim, a tall, athletic Korean guy who treated me very respectfully except on the basketball court. I thought that it would be fine if he guarded me, because the Koreans never knew what they were doing on the basketball court—sort of like an American playing soccer. But it turned out to my horror that he knew what he was doing, and I asked him why. He said that during his stint in the Korean army, he was an MP guarding the U.S. army base and so he played a lot of hoops with the troops. I told him that was a great line and he should be sure to use it every place that he interviewed with. Supposedly, that was part of the reason he got his job at North Texas. They were asking him questions to see how familiar he was with American language and culture, and hoops with the troops resonated.

Most econometricians these days either work on cross section or time-series econometrics, but you have published extensively in both areas and of course in panel data as well. How would you classify yourself if you had to and why?

I would try not to classify myself to the extent possible. I have been able to work in more different fields of econometrics than is common these days.

I think it must probably have been more fun in the old days when people were less specialized. Think of Newton who knew everything. Keynes knew everything that there was to know about economics, math, statistics, that sort of thing. Samuelson was a little narrower but still knew a whole lot of different fields of economics. Then you get to Klein who was a little narrower but still knew lots of different subfields of economics, Goldberger who knew mostly econometrics but all parts of econometrics, and so forth. Now, people know everything about some tiny little topic. I do remember that, when I came out of graduate school, it was my ambition to be able to read intelligently every article in a single issue of *Econometrica*. I soon found out two things. One is that this would never happen. The other is that I was closer to that then than I ever would be again.

But if I had to classify myself I'm a micro econometrician at this point.

What was your most difficult area of research?

Long memory. I had some original ideas and I published a few papers, but I did not have the patience or maybe the ability to learn the math specific to that topic and so I gave it up.

You have made contributions in so many areas of econometrics. How do you decide what to work on?

Well, the short answer is, whatever I find interesting. The longer answer is that there's been a progression through my career, I guess. Early on, I wrote papers largely by reading *Econometrica* and asking what I would have done differently, or what you could do next. Those were rather derivative papers. They were easy to publish, but few of them had much impact. Later, I decided to write less articles, but better ones. And I half-succeeded in the sense that I succeeded in writing less articles, but I realized I really wasn't writing better ones by putting more time into the ones that I wrote. Still later, I tried to do things that were potentially useful. And those were not always easy to publish, but they were the papers that had the most impact.

Following up on your many contributions to the literature on panel data econometrics, how would you evaluate the recent focus on two-way-fixed-effects models, difference-in-difference, and treatment effect heterogeneity in light of the "old-school" approaches to panel data methods?

I haven't followed those topics all that closely, but I think they're not really so different from the old school methods. Some related topics are different. For example, regression discontinuity is different, because it is at least partially nonparametric. One sign of that is that asymptotically it uses zero percent of the data. And that was a concept that was foreign to our thought in the old school times.

In fact, probably the worst advice I ever gave anybody I gave to a junior colleague named Bill Quinn, who in the early 1980s came up with a non-parametric estimator for the Tobit model, and showed it to me. This was a bit ahead of its time, and later that would have been considered a hot topic, and a good paper, but I wasn't aware yet of the work of other people on the same topic and I said I thought he would have trouble selling a paper that used zero percent of the data. And of course, I was wrong.

What do you consider the future of econometrics? On what topics and in what fields do you think young econometricians can still contribute?

It is hard to predict the future, other than to extrapolate the past. I guess that the most noticeable thing that has happened in the past and which I suppose will continue in the future is the convergence of econometrics and "pure" statistics. I don't know if that's a good thing or not. You might ask yourself why we have econometricians. It used to be that econometrics was different enough from pure statistics that we had econometricians, because they could talk to economists, and they could devise techniques that were motivated by economic problems. If you look at the Nobel prizes that have been given out, they have almost entirely been for developing new things specifically aimed at economic or financial issues, not just by picking up things out of statistical literature or extending the statistical literature.

I think there's no limit to what young econometricians can do, or if there is I don't know what it is. And my advice to them would be, don't expect that today's hot topic will be tomorrow's hot topic. I wrote a lot of papers on distributed leg models in the 1970s. If I was still working on distributed lag models, I would be a very obscure person indeed. You have to adapt.

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I remember writing one of those papers with you.

Probably your most cited work is on frontier production functions. Do you consider this body of work to be your most important contribution? And, in the context of answering that question, could you tell us about how you developed the idea of the stochastic frontier model?

As to your first question, yes, I do think that was my most important contribution. It certainly has had the most impact on applied work. I go to conferences with hundreds of people working on efficiency and productivity questions, and they look at me like I am George Washington, or at least John Adams. One guy actually said to me, something it's almost always a mistake to say, "I didn't know you were still alive." I used to try to limit my time spent on frontiers issues to no more than half my research time, because I wanted to keep up my standing with the theoretical econometricians. And it's only really in the last 10 years or so that I just said, "Who cares. I'm going to work on what is interesting to me and what people actually use." So almost all of my papers recently have been stochastic frontier papers.

As far as the history of the development of stochastic frontiers, Knox Lovell knows better than I do. What I know is the following. There was a student at UNC named Bob Dugger, who was a student of Sydney Afriat. They were working on the mathematical programming approach to efficiency analysis and an application of that to banking. When Sydney left UNC, Knox took over Dugger's thesis and I got put on the committee. That approach was non-parametric, and there was no statistics in it, just mathematical programming. (That's a problem that lasted about 20 years more until the work of Leopold Simar and his co-authors, especially Paul Wilson.) So I decided to be parametric and explicitly statistical and asked what kind of error should be in the production function. Knox and I agreed about that, and we wrote a paper. Then I ran into Dennis Aigner at the winter meetings and we got to chatting. And I found out that he had written more or less the same paper, and so we combined the two papers into a new paper, ALS. Only later did I find out about Meeusen and van den Broeck, who wrote another paper that was more or less the same paper and almost exactly contemporaneous. Obviously, it was an idea whose time had come.

Do you worry about identification in stochastic frontier models? Not in the technical sense but in the interpretational sense? When we estimate this unobservable we call it inefficiency, but is there an adequate degree of certainty or at least comfort that it is indeed a measure of inefficiency?

That's a fair question, but it has to be answered I think in the context of specific applications. You can't give a generic answer.

Let me give you an optimistic story, which regards the work done by Jim Seale, an Agricultural Economics student whose thesis I supervised, published in the *Journal of Applied Econometrics* (Seale, 1990). He went to Egypt, and he was going to analyze the technology of manufacturing concrete tiles and estimate the efficiency of individual tile-makers. They used a simple technology to take water, cement, and sand, mixed and pressed together, and left to dry. There was a short production span, so you can measure the inputs that actually go into the outputs of the same time period. I told him to rank the efficiency of the firms by eyeball on his first visit, which he did. Some of them had the machines setup reasonably, and some of them had broken open bags of sand and cracked tiles all over the place and other things obviously wrong. He ranked them by eyeball, and then he put the rankings away until he was done, and then he did the statistical analysis. When he did the rank correlation between initial and final rankings was almost 100%. So yes, sometimes at least what you're measuring is really inefficiency.

You have recently offered us a new family of Copulas, the Amsler, Prokhorov, Schmidt (APS) Copulas, tailored to the stochastic frontier model. There is a general feeling in some academic circles that copulas should become a major research focus in econometrics. This appears to relate to how we view dependence. Is the existence of dependence a risk that we cannot afford to ignore?

That depends on the application. In lots of estimation problems, you can ignore dependence and then use a heteroscedastic and autocorrelation consistent (HAC) estimate for the variance matrix. But, in other problems, it's essential to take it into account. Examples would be forecasting, as opposed to parameter estimation, or cases where you have only weak exogeneity, and so forth. So I can't give a general answer.

Editorial work took up much of your time and efforts. Your long stints on the editorial boards of the best and most selective journals in economics and econometrics, such Econometrica, the Journal of Econometrics, Econometric Reviews, and the Journal of Productivity Analysis, are remarkable. Moreover, those official roles belie the substantial informal role that you have had in refereeing and commenting on students' and colleagues' research that has been instrumental in the successes of many of us who were your students and colleagues. Many in the profession have commented to me how influential you have been in their careers because you took the time and effort to provide academic criticism and professional advice. What instilled in you such an attitude that motivated you to give so much more than you got? MSU and the economics profession have been remarkably lucky to have benefitted from your giving nature and your reasoned judgement, perspective, and brilliance.

Interestingly, since this is an ET interview, I note that I have been an associate editor for almost all of the top econometric journals except ET. And I have published in virtually all of them except ET. Certainly, I am proud of my editorial service. It shows that lots of very smart people apparently trusted my judgement. I'm especially proud, frankly, to have been an associate editor of *Econometrica* for 18 years. Only Tinbergen served a longer term than that. But this was not all about philanthropy. It's not all one sided—I got a lot out of being an associate editor or referee, since it forced me to read and think about papers that I otherwise would not have read or thought about. And also, maybe it made editors more favorably inclined towards my papers.

The same is true of reading and commenting on papers by other people, especially students of course, but not just students, because it led to a lot of co-authored papers. An example would be my paper with Kajal Lahiri, published in *Econometrica* in Lahiri and Schmidt (1978), which came from me reading a paper of his and making an insightful comment on it. And I think also it made other people more likely to read and comment on my papers.

You came of age as a major player in econometrics at, and in, a very young age. In the early 1970s, when you began to publish your research at the age of 24, there was no Internet. Literature searches and the accessing of referenced articles in economics were not conducted online but on foot. Although the Internet and online search engines, such as Google, in the mid to late 1990s led to an explosion in accessible articles and thus in citations, for the first 25 years or so of your career, you were publishing in a different era. Even with this handicap, as last I checked your articles have been referenced almost 57,000 times, more than the combined faculty citations of many top graduate programs. Your top-five publications have been referenced not less than a startling 37,000 times. How did you do it?

Well, certainly, it used to be a different world. I remember, especially at UNC but also at MSU, I used to go to the math library, maybe once a month, and just see if there was something there of interest to me, and that did lead to some papers. I had little gray boxes for 3 × 5 index cards. And when I would read a paper, I would fill out a card, and put the author's name at the top. So they were indexed by author, and then the place that the paper appeared maybe a little blurb about it. And then if I wanted to look something up, I could find it, presuming that I could remember the name of the author corresponding to the paper or idea. I subscribed to more of the occasionally econometric journals like *Review of Economic Studies, Review of Economics and Statistics, International Economic Review, Journal of the American Statistical Association*, and so forth. Of course, now you have Google and JSTORE. But the really biggest difference is email. I remember in 1985 I was in Southampton, and Chris Cornwell was finishing up his thesis at MSU. In those days, airmail took 7 days to get there, and then his response took 7 days to get back. Long distance calls and fax were considered too expensive for anything but an emergency. And so you had to learn to juggle, to keep more balls in the air at once, since you couldn't just sit around for 14 days doing nothing, waiting for a reply.

As for lots of citations, I suppose the key is that I tried to do things that were useful and which were therefore used, and cited. And I also had some exceptional co-authors and that helped.

You have been recognized for these accomplishments with a University Distinguished Professorship at MSU, as a Fellow of the Econometric Society, a Founding Fellow of the International Association of Applied Econometrics, a Fellow of the American Statistical Association, a Journal of Econometrics Fellow, a Fellow of Econometric Reviews, a Principle Founder of the International Society for Effi-

ciency and Productivity Analysis, and you received the first Lifetime Achievement Award given at the European Workshop on Efficiency and Productivity Analysis in 2017. Three special volumes have been dedicated to you. One is in progress for Empirical Economics. The other two are Festschrift in Honor of Peter Schmidt: Econometric Methods and Applications (Springer-Verlag, 2014), and Special Issue of Econometric Reviews in Honor of Peter Schmidt (Taylor & Francis, 2016). The list of contributors is most impressive and reads like a Who's Who of accomplished and distinguished scholars.

Well, of course, I'm very pleased and proud to get all these awards, but as you know you're not in this business for awards fundamentally. I'm more proud of the fact that so many graduate students chose to write with me. I believe there were 41, although there are some definitional issues. And, of course, there are some prominent and successful former grad students, and grandstudents, and even a student in law. (Hung-Jen Wang is married to my student Yi-Yi Chen.) I'm very pleased to have that extended family.

It is not possible to do justice to the 170 (plus) refereed articles and the 4 books you have published and keep this interview within the guidelines of ET's page limitations. I will, however, mention one in particular. Your 1977 introduction of the Stochastic Frontier Model, with Aigner and Lovell (the ALS model), was singled out by a foundational figure in econometrics, Takeshi Amemiya, in 2009 as one of the most influential papers ever published in the Journal of Econometrics. As one of the founders of the field of efficiency and productivity analysis, was this (recognizing that many other related and highly cited papers followed) one of your most satisfying publications?

Well, yes, certainly in retrospect. It's interesting that when I first wrote that paper I did not foresee how influential it would become. I guess I was thinking like a theoretical econometrician, and they have not been exceptionally enthusiastic about the topic. But empirical economists have been. The concept of X-inefficiency has been around since Leibenstein, 1966, but there really hadn't been a systematic attempt to quantify it, and that's what stochastic frontier models do. Of course, not all economists like to think in terms of inefficiency. I remember having a conversation with Ernie Berndt, for example, and he asked me in all seriousness, why would it pay for a firm to be inefficient? There are possible economic answers to that, like maybe it costs too much to be efficient. But I gave him instead a much more straightforward answer. I said, Ernie, do you live in the same world that I do? I see inefficiency all around me.

Well said, we know you of course are one of the founders of stochastic frontier analysis, by the ALS name, but your name has also been "acronymized" in the KPSS test for stationarity. How did this line of research emerge in the 1990s? Why did you then decide to move away from time-series econometrics?

Well, it's hard to explain to anyone who wasn't there how influential and interesting that line of research (unit roots) was in the early 1990s. Peter Phillips sort of erupted on the scene with this topic in the late 1980s. I remember going to a conference, I don't remember where, in probably 1990, and everybody was talking about cointegration, unit roots, and so forth. And so I decided that I had to learn something about this, and I did. I had a few ideas that fortunately Peter Phillips found interesting, and that led to two joint papers, one of which was KPSS. But I gradually moved away from it, and I think partly because it was not my comparative advantage. And also, partly to some extent because the whole profession moved away from time-series econometrics. The latter may have been partly a case of people having picked the low-hanging fruit, but more of it is due to the indifference or antagonism of the real business cycle macro-economists, and that is scientifically speaking unfortunate.

Yes, I agree with that.

Are there any research topics in which you may have had interest but not the time to pursue at the level that has characterized your other professional contributions? Are there topics you find interesting and follow but do not actively publish in and if so why do you find these topics of interest to you?

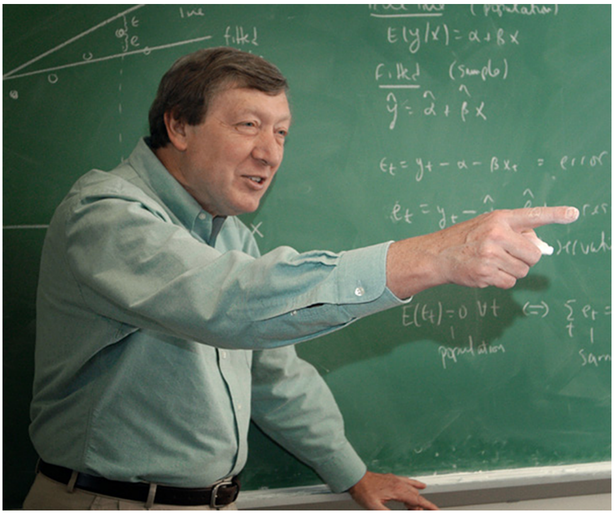
Well, some topics that I have interest in but not the time or maybe the ability to delve into would be long memory, bootstrapping and related issues, and weak identification.

The main thing that I follow but am not really actively involved in is the treatment effects literature. That's largely because of being associated with Jeff Wooldridge and being on the committees of his students. So I learned a lot about it and learned to appreciate it, but I've not really worked on the topic.

Econometrics has changed much in the half-century you have been a prominent figure in the profession. What would you say are your specific research contributions that you feel have contributed most significantly to the current econometrics' paradigm, and those whose time may not yet have come?

I have already mentioned stochastic frontier models. In terms of more traditional theoretical econometric topics, I suppose: panel data, especially dynamic panel data models and factor models; aspects of GMM estimation, like redundancy of moment conditions; and KPSS and a few of my other time-series papers. Naturally like almost everyone I can think of lots of papers that didn't get the play that I think they should have. Specifically: my paper with Kyung So Im (Im and Schmidt, 2009), on more efficient estimation under non-normality when higher moments do not depend on the regressors; the Schmidt–Phillips unit root test (Schmidt and Phillips, 1992); and my Hamermesh and Schmidt (2003) paper with Dan Hamermesh on the determinants of ES fellows' elections. In the latter paper, we made the eminently reasonable suggestion that voters get to abstain.

All of your students are fascinated by your teaching. Your teaching is so very systematic and organized. Was your teaching so from the beginning or did you adapt to the students/audience? Were you born with the talent, or did you develop it somehow?



Peter in the Classroom, 1998.

I think that my teaching, whether good or bad, was always instinctive. I probably learned somewhat from experience, but mostly it was just instinctive.

Basically, I think to be a good teacher requires three things. First, you have to know what you're talking about, or at least think you know what you're talking about. Second, you need to be organized. And third, you need to be able to sense when and why students aren't getting it. If somebody asks a dumb question, for example, you need to comprehend what specific confusion led to that question. Repeating the original explanation seldom helps.

The number of graduate students who have chosen to write under you, and the number who have been professionally successful, is substantial. I know many of your former students and I have yet to hear anything but admiration and gratefulness for your mentoring and your continued interest in your students' careers and personal lives long after they finished their formal training under you. It must be a source of great pride and satisfaction.

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Peter and Robin Conferencing in Coral Gables, 2018.

Yes, it is. When all is said and done that is probably what I am most proud of.

You have had many former students from abroad along with American-born students. Is there any difference in guiding them?

Well, yes, to some extent, I don't want to over-generalize, but a familiar broad generalization with some basis in fact is that Asian students are more content to be told, do this, and do that, whereas American-born students are likely to be more independent, and sometimes require restraint from doing things that are dumb or unproductive. I remember one student who asked me if I could explain to him how to think originally. And you might guess it was an Asian student.

This is the last question, and it speaks to the theme of your student mentoring, which of course you indicated is your most satisfying legacy. In addition to the many graduate students with whom you have co-authored major works, you've also written with many, many others in the field. Intellectual compatibility and common interests must be one set of drivers for such joint work with both domestic and international colleagues, but there's so much more. How did you do it?

I like the feeling of being part of a worldwide intellectual community. And that's easier now that there's the Internet. But personal contact still counts. I have lived in eight different countries, and I've co-authored papers with people from almost all of them, plus with people from many other places that I've not lived in. Being a successful academic is a lot like being in the army, in the sense that you know people all over the world. Of course, it helps to like to experience different cultures,

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different foods, and so forth. And also, to have enough money to fly business class.

Seven Scholars at the Seven Scholars Restaurant in Taipei, 2010 (from left to right—Robin Sickles, Janet Meininger, Hal Fried, Subal Kumbhakar, Paul Wilson, Peter, and Christine Amsler).

Peter and Christine Amsler, Roche de Solutré, 2018.

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I will end with a vignette. Christine and I met Peter Phillips purely by coincidence, I think three times, in the Los Angeles airport, when we were on the same flight to New Zealand over Christmas break. We also twice met Peter Kennedy, who had a house in Akaroa, a lovely town on the South Island of New Zealand where we visited him a few times. But we didn't get to sit anywhere near him on the flight, because he was in tourist. He owned three houses and must have been a somewhat wealthy person, but with not as much income and so he was riding in economy class. Peter Kennedy died too young, and he should have sold a house and sprung for business class.

Peter, thank you so much for your most insightful answers to my questions. Before we finish this, I do want to mention that some of these questions were based on ones posed by former students and colleagues whom I contacted. Those include Chris Cornwell, David Guilkey, Jesse Levy, Chirok Han, Young Hoon Lee, Artem Prokhorov, Peter Phillips, and Alecos Papadopoulos. They may not have had their exact questions asked, as they were paraphrased in the course of my putting together these questions. Again, thank you so much, and at a personal level, thank you for being there for me. I know that I say the same thing that countless students and colleagues and people in the profession would say were they in my place.

You are welcome.

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