The Art and Practice of Economics Research: Lessons from Leading Minds, S. Bowmaker, Edward Elgar, 2012

Charles F. Manski (Northwestern University)

Charles F. Manski was born in 1948 in (Boston) and graduated with a BS and a PhD in economics from Massachusetts Institute of Technology in 1970 and 1973 respectively. Professor Manski has taught at Carnegie Mellon University, 1973-1980, the Hebrew University of Jerusalem, 1979-1983, the University of Wisconsin-Madison, 1983-98, and Northwestern University, where he currently serves as the Board of Trustees Professor of Economics.

Professor Manski's primary research interests are econometrics, judgment and decision, and the analysis of social policy. His most-cited articles include, 'The estimation of choice probabilities from choice based samples', *Econometrica* (1977), 'The structure of random utility models', *Theory and decision* (1977), 'Identification of endogenous social effects: The reflection problem', *Review of Economic Studies* (1993), 'Economic analysis of social interactions', *Journal of Economic Perspectives* (2000), and 'Measuring Expectations', *Econometrica* (2004). His books include, *College Choice in America*, co-authored with David Wise (Harvard University Press, 1983), *Analog Estimation Methods in Econometrics* (Chapman and Hall, 1988), *Identification Problems in the Social Sciences* (Harvard University Press, 1995), *Partial Identification of Probability Distributions* (Springer-Verlag, 2003), *Social Choice with Partial Knowledge of Treatment Response* (Princeton University Press), and *Identification for Prediction and Decision* (Harvard University Press, 2007).

Professor Manski was elected as a Fellow of the Econometric Society in 1984, a Fellow of the American Academy of Arts and Sciences in 1994, and a Member of the National Academy of Sciences in 2009.

I interviewed Charles Manski in an office at the Department of Economics of Arts and Sciences at New York University, where he was a visitor. It was the late afternoon of Tuesday, September 14, 2010.

BACKGROUND INFORMATION

What was your attraction to economics?

I was always interested in how people make decisions. I started out in physics, but that was not intuitive to me, so I switched over to economics.

As a student, which professors were most inspirational or influential?

As a first-year graduate student, I was very lucky that Dan McFadden visited MIT. I was trying to analyze the problem of how people choose where to go to college, using neo-classical consumer theory. But that didn't make any sense since it is oriented towards modeling how many units of something you will buy. When McFadden came through, he was starting his work on discrete choice analysis, and I saw immediately that that's exactly what I needed.

Why did you decide to pursue an academic career?

The only other thing I ever seriously considered was being a professional pilot. I love flying, and I've flown privately, but my eyes weren't good enough to fly professionally. And I did think about going to law school rather than doing a PhD in economics, and I would probably have wound up as a law professor, rather than a practicing lawyer. I guess I always wanted to do research…other than being a pilot (*laughs*).

As a researcher, which colleagues have been most influential and inspirational?

Dan McFadden. Throughout the '70's, he was incredibly helpful. Our relationship has changed over the years, of course, but we're still close friends.

I arrived at Carnegie Mellon in the fall of 1973. I was an assistant professor in the School of Urban and Public Affairs, where two people were very helpful. One was an urban historian named Joel Tarr. I had to co-teach with him a course on American communities. I knew nothing about the subject, but Joel helped me see beyond economics and the value of quantitative work. He was a good friend and mentor.

Al Blumstein was also in the school. Back in the '70's, he headed the first National Research Council committee that studied the deterrent effect of various punishments. He introduced me to how research affects public policy, so that was helpful.

I moved to Wisconsin in the early '80's. Art Goldberger became a very close friend. I was a full professor by that time, but he spent three or four years reading my papers line by line and making edits the way that an advisor would for a PhD student. He eventually stopped doing that, but I could talk to him about anything. He was just a very wise person, and I miss him quite a bit.

GENERAL THOUGHTS ON RESEARCH

What do you see as the value to society of "pure" versus "applied" research in economics?

I don't like the conventional distinction between pure and applied research. I think they should go together. I have a fairly practical orientation, and I don't see much value in pure research for the sake of it. If that's all there is to it, then economics departments should be financed by universities in the same way as esoteric art history departments. But economics departments are pretty central to most modern universities, and the reason is because they should try to say something about the real world.

Theory should be useful in doing applied work. I've always gone back and forth between econometric theory and applications in my own research, and that's what I like best. But there is an increasing problem in economics of bifurcation between theory and application. There are economists who focus on theory for the sake of it, and would consider their work to be lacking depth if they were able to find an application. That's something you find among pure mathematicians as well.

On the other hand, some economists appear to think that you can do applications without knowing very much theory. The ones that are most troubling to me are the labor economists and public economists at MIT, Harvard, and Princeton, where the paradigm of randomized experimentation has taken hold.

How would you describe the connection between pure and applied work at Northwestern?

I finished being department chair two weeks ago, so I'm freer to talk about this now. What's problematic at Northwestern is that I have colleagues in economic theory, whom I very much respect, but whose work has almost no connection with any application.

How would you describe the dialog between theory and empirics in economics?

I can describe how it should be. Dan McFadden has always been a great exemplar in this respect. He started his work on discrete choice analysis with basic principles of economics, such as revealed preference, and went very coherently from the theory to the econometric application. Many people would call him an econometric theorist, but he would often say that he was an applied economist, since the theory fed directly into the application, without any leap of faith.

How would you characterize your own research agenda and how has it changed through time?

I've always been motivated by public policy problems. As I mentioned before, my PhD dissertation focused on modeling college choice. That had a very specific application: the federal government of the United States had recently instituted a new scholarship program for undergraduates, and the question was how this would impact enrollments, particularly of lower-income students. To answer that question, I needed to know the kinds of data and theorizing about decision making that might be helpful. And so, early on in my career, methodological issues led me into doing econometric theory. I got some pleasure out of the theory per se, but I always wanted it to be useful for empirical work. That's still true today.

Do you think it is important to have broad research interests?

I think so. I'm constantly bouncing back and forth between specific technical questions and very applied issues. And I'm trying to see things from different perspectives. For example, I've published in sociology and statistical journals, and I've always found it helpful to read psychology. But there is a contrary view that people need to specialize, and I have friends with whom I've disagreed on this very issue. In the end, I've decided that not everyone works the same way as a researcher, and that there isn't just one path to the truth.

Do you think there is any difference in the types of work done by researchers at different stages of their careers based on tenure concerns, publication requirements or other pressures? Should there be a difference?

As I've gotten older, this has increasingly become a sore point with me. Without going into the details, I think I can fairly say that I took lots of risks early on in my own work. One reason is that I've always wanted to be able to look back and be proud of what I did for myself. But in recent years, I have had repeated conversations with assistant professors who tell me, "I would really like to work on this project, but it's too risky. I'm going to wait until after I get tenure because, in the meantime, I must get my papers published in the top five journals." It's true that the major purpose of tenure is to have a license to think broadly. But what I worry about considerably is that when you are risk-averse for 12 years – five years at graduate school plus seven years as an assistant professor – it becomes so ingrained that you lose your ability to be creative. I agree that there has to be compromise to some extent, but I think the balance has moved too far towards conservatism for too many young researchers.

In the end, do you think the profession has helped to bring out and shape your research for the best?

No. The prevalent view in my mind is I've been against the so-called mainstream in the profession and it's often taken a long time for people to see the value of my work. I feel like I'm respected, but it's begrudged. Some economists do something hot early in their careers and they're immediately rewarded by the profession. That's not the way my career path has gone.

IDEA GENERATION

Where do you get your research ideas?

It's a curious process. But there was an extraordinary turning point that I can pinpoint from 1987. Irv Piliavin, a friend from social work at Wisconsin, came to me with a very specific applied problem. He was conducting a longitudinal study of homelessness in the city of Minneapolis, and there was attrition in the sample; he was interviewing people in the winter, but was then unable to find them again in the summer. He knew he had this missing data problem and wanted to know if I had any thoughts on how to handle it. One traditional approach is to assume that the data are missing at random, which solves the identification problem. The other way is to have some kind of a selection model, which is based on very strong assumptions. He said, "I just don't like either of those approaches. I won't believe my empirical conclusions. Is there anything else I can do?" I had known about these missing data problems for many, many years, but I hadn't thought about them seriously in terms of my own work. I told him I'd look at it, and he got me some summer salary from the Poverty Institute of Wisconsin.

At first, I found myself trying to see whether I could build on the literature. But I decided that wouldn't be fruitful, and figured that I needed to go back to first principles. That meant asking, "What if I made no assumptions at all about this missing data process?" And so I wrote down the simplest formulation, which is just the Law of Total Probability (dividing people up into those you can observe and into those you can't). Then I realized that although I couldn't pinpoint the things Irv was trying to learn, I could get a bound on them, and that the width of the bound would depend on how much missing data there was. I saw that there was a very general principle behind the process. It wasn't a case of saying, "What assumptions do I need to get a point estimate?", but, rather, saying from the beginning, "What can I learn from the data?" It may not be enough to get a point estimate, but you can still learn something. And that's what led to my last 20 years of work on what is known as partial identification. I've applied the technique to an enormous variety of problems, including analysis of treatment response and evaluation of public policy programs.

Initially, people hated it. Some very famous economists would tell me, "You can't give bounds. We must have point estimates." When I would ask them why, they would say, "That's just the way it is." But this is where Goldberger, in particular, was very supportive. He encouraged me to keep working on it even though at first it was considered wacky. I'm very happy that he did, because it's grown into what I clearly view as my most important work. And it's a great example of where research came from a very practical question.

At what point does an idea become a project that you devote resources to?

It took me a year to decide to devote a lot of resources to the project that I've just described. And that was for a very curious reason: my early results were ridiculously simple in mathematical terms. I could explain them to a fifth grader. And so initially, I thought, "Gee, I can't write this up as a paper. It's too simple, and people value technical cranking." I had to keep asking myself, "Well, if it's so simple, how come it was missed? Or maybe a thousand people have had this idea and thought that it was not important?" Each morning, I had to persuade myself that just because a simple thing is not in the literature doesn't mean that it's a stupid idea. And once it was published, and people didn't like it, I then had to just keep at it.

In the first 10 years that I worked in the area – throughout the '90's – I had the whole field to myself and was able to do an incredible amount of work. It was only in the early 2000's that the broader profession began to catch on and make major contributions. In fact, somebody introduced me recently as, "originally a conventional econometrician, but then an iconoclast." I had to look up the exact definition of iconoclast to see whether I liked it or not, but it was okay *[laughs]*.

IDEA EXECUTION

What makes a good theoretical paper? Can you give an example?

Dan McFadden's paper on conditional logit analysis of qualitative choice behavior, for which he won the Nobel Prize, has always been an exemplary theoretical paper.[1] He began with a very broad econometric theory question of how you make inference on decisions from observing choice behavior, but he wanted to have a practical outcome, so he added layers and layers of assumptions to make it tractable for estimation.

In terms of economic theory, a paper written around the same time was (James) Mirrlees' Nobel Prize-winning work on optimal income taxation.[2] Clearly, that's a theoretical paper,

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[1] McFadden, Daniel L. (1974), 'Conditional Logit Analysis of Qualitative Choice Behavior', in *Frontiers in Econometrics*, P. Zarembka (ed.), pp. 105-142, Academic Press: New York.
[2] Mirrlees, J.A. (1971), 'An Exploration in the Theory of Optimal Income Taxation', *Review of Economic Studies*, Vol. 38, No. 114 (April), pp. 175-208.

but it also has a real problem behind it: the design of income tax schedules. Looking back, there are various things that could be done differently – and it's a difficult paper to read - but he set it out in a very coherent way with great abstractions. A so-called pure theorist would look at Mirrlees' paper and say that it was extraordinarily important to economic theory because, even if they didn't care about tax policy, it represented one of the important beginnings of the mechanism design literature. However, it wasn't something that was just going to stand there as an artistic exercise for theorists. In fact, the IFS in London, where my good friend Richard Blundell is involved, set up a Mirrlees Commission recently. And so, 40 years later in the UK, you can see some level of influence of Mirrlees' early work on the formation of tax policy. That's the way it should be.

What makes a good empirical paper? Can you give an example?

It's not as easy to pin down what makes a good empirical contribution. When you see a totally coherent piece of theoretical work, you can say that it is beautiful and elegant. But in empirical research, there are thousands of compromises that you have to make, and it's easy to beat it up on lots of details, and then it's a question of how much common sense there is in the way you do it. Empirical work also tends to accumulate bit by bit, which means it's tougher to identify papers that just lay things open.

But if I had to recommend an empirical paper, it would be one by Adeline Delavande, a former PhD student at Northwestern who, two years ago, published her dissertation work on women's contraceptive behavior. The paper was called, "Pill, Patch or Shot?" and it's in the *International Economic Review*.[3] Adeline was doing discrete choice analysis of the McFadden type, but this was decision-making under uncertainty. She went out and collected her own data from women around the Chicago area, eliciting, in particular, their

[3] Delavande, A. (2008), 'Pill, Patch, or Shot? Subjective Expectations and Birth Control Choice', *International Economic Review*, Vol. 49, No. 3 (August), pp. 999-1042.

expectations about the chances of becoming pregnant if they were to use different forms of contraception. She then inputted that into a discrete choice model and did a beautiful job. This is a paper that you've got to read page by page. There was not just extraordinary care in the analysis, but also extraordinary creativity in being willing to go out and interview people and collect the data, which is something that economists rarely do. It was very risky for a PhD student to do this kind of work as well, so I've always appreciated that aspect of her. When people ask me, "What's the value of collecting expectations data?", or "How would we use expectations data in econometric analysis?", I just say, "Read Adeline's paper. It's a model."

When you hit a "brick wall" on a project, do you continue to work on the problem or do you take a break and work on something else?

If we knew where research was going, then there wouldn't be anything new to do (*laughs*). It's about introspection. Sometimes when you hit a brick wall, you've just got to keep pushing because you know it's in there somewhere, and you'll break through. Other times, you decide that it's not going anywhere, and you put it aside.

I've had some papers that I went back to 10 years after I'd hit a brick wall and all of a sudden I've had a flash of inspiration. For example, I published a paper in the *Journal of the American Statistical Association* in 1990 on the analysis of intentions data.[4] There had been a long-standing practice of asking people things like, "Do you expect to buy an automobile? Whom do you expect to vote for?" It had always bothered me that you shouldn't be able to give "yes" or "no" answers to these sorts of questions. A forecast is being made and there is uncertainty. The paper that I wrote led me to think that we needed to be eliciting subjective probabilistic responses to the questions. But Dan Nagin and I actually began this work at Carnegie Mellon in the late '70's and then it just stopped. I didn't think about it for

[4] Manski, C.F. (1990), 'The Use of Intentions Data to Predict Behavior: A Best-Case Analysis', *Journal of the American Statistical Association*, Vol. 85, No. 412 (December), pp. 934-940.

a long time, and somehow around 1988, it got back in my head and I started working on it again.

Another example relates to the first paper that I had published back in 1975.[5] It was on maximum score estimation, which was part of my dissertation, and represented a move away from standard parametric modeling as I was trying to weaken the assumptions of McFadden's conditional logit model. It was a good paper, but I didn't do any further work for some time. And then in 1983 – I can date it exactly – I went back to it because a new piece of research came along. Jim Powell had just done a dissertation on what he called 'censored least absolute deviations estimation.' It was about estimating median regressions rather than mean regressions when you have missing data. I read his work and I remembered that one aspect of my first paper had to do with making median assumptions. I thought, "Hey, there's some connection here", and so I went back to my earlier research. By this point, I knew a lot more mathematics and statistics than at the beginning of my career, and in 1985 I was able to publish a sequel to the original 1975 paper. I titled it, "Semiparametric analysis of discrete response', and that paper became fairly important in what was then becoming a whole new literature on semiparametric econometric analysis.[6]

What would you say has been the biggest change, in the course of your career, in how your research fields conduct research?

I think the level of mathematics, and its premium, have gone way up. In the early '70's, when I was in graduate school, doing econometrics meant cranking lots of extraordinarily boring linear algebra. And our formal statistical training was very weak. But you could be

[5] Manski, C.F. (1975), 'Maximum score estimation of the stochastic utility model of choice', *Journal of Econometrics*, Vol. 3, No. 3 (August), pp. 205-228.
[6] Manski, C.F. (1985), 'Semiparametric analysis of discrete response: asymptotic properties of the maximum score estimator', *Journal of Econometrics*, Vo. 27, No. 3 (March), pp. 313-333.

a great theorist with simple math, like Robert Solow who made enormous contributions. It's true that you need more math to do modern work. In fact, during the mid '70's, I went on leave to Berkeley and decided that I needed to tool up. Luckily, I didn't have to take exams, but I sat through the first-year PhD courses in the math department in measure theory, functional analysis, and topology, which is very rough. And nowadays, we seem to screen our admissions to PhD students primarily on math, and accept lots of students who have never had an economics course. That can be very negative because the technique of math gets valued per se, and the economics gets lost.

Sometimes I call myself a conceptual econometrician rather than a technical one. I've done a great deal of technical work, but I value simple insight, which is not always true for the rest of the profession. I've had a few PhD students whom I thought had extraordinarily good insights in their theses, but they told me, "I can't go on the job market with this paper - it's too simple. I'm not going to be able to show off how much math I know." They had worked on a totally obscure problem and figured out a simple way to view it. I know an econometric theorist who didn't get a job at certain places because they said her paper was too simple. And I know someone else who had two papers, one of which was a conceptual econometric paper that I liked, and the other being a technical paper that I thought was boring. He was advised to go on the market with the technical paper because that's how he was going to get a job.

This issue of the role of math feeds back into the growing estrangement of theory and empirical work. Learning all that math is an enormous investment, and some people are good at it or are willing to make that investment. And there are other economists who find they can go another route by, say, doing randomized experiments where you basically don't need to know any math at all, or even any economics. They put all of their energy, and I say this in a positive way, into trying to be very careful about data collection. And so we now have a situation where we have people who specialize in the math, but don't have the foggiest idea about data collection, and others who are very good at data analysis, but can't read an applied theory paper, never mind a straight theory one, because their math isn't up to it. It's very dangerous for the profession that we have these two groups of people who can't talk with each other.

THE WRITING PROCESS

Which aspect of the writing process do you find most difficult?

Coming up with the title and then writing the introduction *[laughs]*. I can spend a couple of weeks on those things because it's forming your whole orientation towards the work. As well as being absolutely critical, it's also very painful, and one part of the writing process that has probably been difficult for hundreds of years. But the rest of the process has changed extraordinarily because of word processing. I typed my thesis by hand, which meant that you had to keep an enormous amount in your head, write it down in long hand, and then at the very end, put it on the typewriter. Thankfully, that became a lost art when word processors were introduced. Now, I compose my work on the screen in front of me, and I can end up with 500 versions of a paper, because I'm constantly iterating it and molding it. I think it's changed the way that most people work, and it should make the quality of writing higher than in the past. I try to put a lot of care into my writing, both in terms of general organization and line-by-line specific choice of words.

What steps have you taken to improve the quality of your writing during your career?

I am embarrassed by a couple of my early papers. It was the classic graduate student/assistant professor problem of wanting to show how abstract and formal you can be, and making the paper much denser than it needs to be. My writing became far better in the late '80's when I was Director of the Institute for Research on Poverty at Wisconsin, and talking to sociologists, political scientists, and historians. And I was going to Washington and talking with people in the government, occasionally even those in Congress, and I had to learn how to write verbally, which I find much harder than writing mathematically. I also had two editors who went through my work, and I learned from them and became a much better writer. Of course, it was also partly due to maturity, since your ability to do math gets lower with age, but your ability to write coherently should improve with age.

COLLABORATION

When you work with co-authors, how do you decide whom to work with?

The most important thing is you've got to find someone who, temperamentally, you can deal with. I've been fortunate. I don't think I've ever had a case where I really regretted working with someone, although that's not to say that it's always been easy.

Just as an aside, last week, I received some silly questionnaire that someone wanted me to fill out on how you do research. I was bored. I was sitting in the hotel here in New York, so I took 15 minutes and did it. It turned out it came from lab scientists for the National Institute of Health, where everything is very hierarchal. There's someone who brings in the funding, another person who sets up the experiment, and then someone else who does the data analysis. But that's not how it works in economics. Every collaboration is distinct.

When you do work with co-authors from outside your university, how do you prefer to interact with them (e-mail, phone, or face-to-face meetings)?

I don't work well face-to-face. I've got to able to think for, say, an hour, go for a walk, and then come back to it. I don't like the idea of a forced, very intensive situation.

A very important thing is who's going to do the drafting. Usually, I think that I'm a better writer than my co-authors - or maybe it's just that I'm territorial – and so I prefer to do the first draft. On the other hand, there are people whom I trust and they can do the first draft and I can then edit. But there has to be enough shared commonality on that issue, because the first draft is an enormous amount of work and someone can get possessive about it. You can't go in afterwards and say, "Nah, I think we've got to dump this whole thing and start all over again." You have to learn how to work with each other.

RESEARCH ASSISTANCE AND FUNDING

How do you use undergraduate and graduate research assistants?

I never use undergraduates. They would be useless. Often, you need a research assistant to do computational work that you used to be able to do or don't want to do. It could also be something technical. You get better at defining what's an interesting problem, but worse at cranking out proofs. And so you can have a symbiotic relationship with research assistants because they need to know how to do computations and technical work. Of course, you're also paying them.

The most rewarding experiences are when the research assistant puts enough into it such that you say at some point, "Look, you really should be a co-author." Then you become more collegial, and they make their own contributions.

How important is funding for getting your work done?

I don't tend to do work that needs lots of funding. For the small amounts that I need, I'm fortunate that I have an endowed chair and receive a certain amount of money per year, plus NSF or NIH grants that'll pay for the RA's salary. In the cases where funding has mattered, for example, when I've done original data collection on surveys for my measurement of expectations work, it's still been fairly small scale. I have made a conscious decision to stay away from large-scale empirical projects, because the effort involved in managing them is enormous.

Do you have any advice for a young scholar on the funding process?

The people who need large amounts of money these days are development economists. I've seen this as a department chair. They are doing large field experiments, which is the new wave in that research area, and they spend time overseas in Africa or South Asia. This means that when you make them an offer as an assistant professor, you have to provide packages for them. It's very similar to putting together a lab for a biological scientist. But theorists just need a computer and a pencil!

SEMINAR PARTICIPATION AND NETWORKING

What are the benefits to attending a seminar that is closely related to your work versus one that is not closely related?

You have to go to the ones that are closely related to your work, whether you like it or not. You can pick and choose the ones outside your field, based on whether you think it'll be interesting. Sometimes you'll learn something, and sometimes you won't.

How important is professional networking to success in research?

It's extremely important. Going to the large meetings, like the American Economic Association meeting or the Econometric Society World Congress, is a waste of time. You might network in the sense of meeting people in the hallway and having random conversations, but that's usually not very fruitful. What you need to do is give talks elsewhere, have one-on-one meetings, attend small conferences, and make short visits. I'm sitting here at NYU for a week and, similarly, people come through Northwestern all the time.

Pragmatically, assistant professors have to get their work known. This is part of the system that's essential. I can name cases of assistant professors in my department where, when they've come for midterm review after three years, we've had to say, "It looks like you're doing good work, but you really should go out and talk to more people, because you need to become better known. And maybe you'll get some ideas." We're also social beings. Why write anything up if not to communicate?

To what extent is the absence of departmental colleagues working in one's research area a major disadvantage?

For a senior person, it's not a big deal, because by that time your networks are established worldwide. I go to UCL every March and some of the people who are closest to me are there, like Richard Blundell and Andrew Chesher. If you're junior, and you don't have people in your own field, it's very hard. And when you come up for tenure, who's going to champion you? I have a former student who, until a few months ago, was here at NYU. He was doing very well, but NYU does not have a senior econometrician, and he just decided to move to Cornell with tenure, although NYU was ranked more highly than Cornell among economics departments. For him and what his interests are, it's a good move.

COMMUNICATION OF RESEARCH

How do you find the right balance between communicating your research at an early stage versus the close-to-finished stage?

There's always a trade off. I may tend to error on the side of communicating a little too early. The reason is you may get excited and want the feedback when it's going to be useful to you.

Some people are very closed. They worry about competition. But as I said earlier, I've been fortunate, because I've usually worked on things that nobody else is doing. And so I'm maybe not as neurotic about people stealing my ideas.

What are the unique challenges to giving a seminar and how do you overcome them?

It's crazy to think that you're going to do proofs in a seminar. No matter how smart you are, it's just impossible to assimilate a hard piece of work in 90 minutes. You've got to get across the main themes and then start working through the details to the extent that's appropriate. A classic problem with seminars is people feeling like they've got to go through *all* the details. I can read the paper for those. And so the hardest part is how to get some intuition for the result without having to go through the paper line by line. You may have been working on this research for months, but even if someone is in your own research area, you can't expect they can come in cold and get to the same point where you are.

I think I do okay at giving seminars. That's also something that gets better with experience.

PUBLICATION

How do you decide upon the appropriate journal to send your research to? Related, whom do you view as the readership of your research?

It's a complicated calculus of trying to forecast, substantively, what is the best place and then figuring out who is the editor and whether you are going to get fair referees. It's a horrible process, and it doesn't matter how senior you are. You get really stupid rejection letters, and you must have very thick skin.

I've never had a paper in the *American Economic Review*. I've had 10 papers rejected there. I've never had a paper in the *Journal of Political Economy*. And I wouldn't even think of sending a paper to the *Quarterly Journal of Economics*, given their tastes. And so I've got lots of '*Econometrica*s' and a bunch of 'ReStuds', but nothing in the generalist journals. They could be right, but maybe it's their loss.

How would you best describe your approach to dealing with a "revise and resubmit" request from a journal? How about an outright rejection?

What's the difference between a "revise and resubmit" and a rejection? Almost nothing is accepted in the first round. Sometimes you shrug and say, "It's a judgment call", but other times, you'll say, "These guys are such idiots." And there have been a few times when I've protested over the years. Usually, it's just to blow off steam, but I was successful a couple of times.

I once sent a paper to the *Journal of the American Statistical Association* that was then rejected. Initially, I thought that was a mistake, but I received a critical referee report that pointed out something that I had missed. I never learned who the referee was, but I was very thankful, because once I realized he or she was right, I spent a lot more time reworking it with my co-author. We sent it to *Econometrica*, where it was accepted, so that was a very good outcome in the end.

Do you think that the current structure of the publication process in economics facilitates or impedes scientific understanding and knowledge production?

I would say it's impeding those things, except I don't know how to improve it. It tends towards conservatism. Now that everything is electronic, I'd rather see more research coming out than less. Let it see the light of day, and let the research community decide whether it's worth something, rather than suppressing it through the refereeing process.

I write books, which is fairly unusual for an economist, and one good reason for doing that is to avoid referees. I find it an extraordinarily liberating experience. In terms of the generation of ideas, you don't really know where the research is going to take you when you embark on a book, because it's a several-year project rather than a delineated few months. I think the piece of work that I value most in my whole career is the short 1995 book, *Identification Problems in the Social Sciences*, which brought to a head my ideas on partial identification and allowed me to develop them further and get the big themes across. Then I iterated on that in my 2007 book, *Identification for Prediction and Decision*, which was a graduate textbook. It's much more than just a second edition, and the point is I could express myself in it. Senior people should write books more often.

What has been your best and worst experience during the publication process?

Actually, they go together *[laughs]*. I wrote a paper in the early '90's that was originally titled, "Simultaneity with downward-sloping demand." It related to the classical simultaneous equations in econometrics where you're trying to separate supply and demand with linear model assumptions. That's wonderful work from the '40's and '50's, but I wanted to disentangle it, and so I asked the question, "What can you learn from the assumption that the demand function is downward-sloping without any other assumptions?" It was a beautifully simple paper that I sent to *Econometrica*. David Card was the editor at the time, and he made an absolutely atrocious decision. He said that I should forget all this non-parametric work and instead do linear modeling. He is a complicated guy and he's gone to different extremes at different stages of his career. This was a time when he was totally against structural econometrics, because he had this Princeton background that I alluded to earlier. When he rejected the paper, I was so angry with him. Within a day, I faxed him a very, very nasty response. I had to get it off my chest, even though nothing happened. If David Card were to walk in here today, I would tell him he made a horrible mistake in rejecting that paper.

I sat on it for about two years and got a whole bunch of new results, so that it wasn't the same paper anymore. And then I resubmitted it to *Econometrica*, which, by that time, had a new co-editor, Peter Robinson, the British econometrician. The paper had a new title, "Monotone Treatment Response", and I told Peter that David Card had rejected a much earlier version, but that this was an entirely new version. I didn't want him to think that I was getting two shots. To his great credit, Peter sent it out to new referees, and the paper was accepted very easily. My good friend, Andrew Chesher, later told me that he was one of the referees, but we didn't know each other at the time. To this day, I still think it's one of the best papers that I've ever written.[7]

TIME MANAGEMENT

How do you divide up your working day, both in terms of quantity and timing of different kinds of work?

I tend to work at home as much as I can, at least in the morning, and then I'll go into the office in the afternoon and deal with the social aspects of seminars and talking with people. I have a wonderful study at home that looks over Lake Michigan, and we also have a second house, a farm in western Wisconsin, where there is nothing in my way when I need big blocks of time. There are too many distractions in the office.

How do you balance multiple research projects?

That's something that comes with experience. A PhD student writing a dissertation is totally single minded on writing that one piece of work and can't do multiple things. But over time, you learn to go back and forth. And when you hit these brick walls that you talked about before, it helps to have multiple projects.

On the other hand, there are periods when you just need to focus on one thing and have absolutely nothing else intervene. My wife won't like this, but she went off on a cruise with her mother to the Greek Islands a year ago, and I had an idea. I just sat at home and worked flat out for two weeks. I had nothing else on my mind for 24 hours a day.

[7] Manski, C.F. (1997), 'Monotone Treatment Response', *Econometrica*, Vol. 65, No. 6 (November), pp. 1311-1334.

What was the idea?

It's a piece of public economic theory that is published in a new journal called Q*uantitative Economics*. The title of the paper is rather curious: "When consensus choice dominates individualism: Jensen's inequality and collective decisions under uncertainty."[8] The idea came about as I was thinking about an entirely different area and realized there was a mathematical commonality between the two topics. And I was able to show that, under certain circumstances, a collective decision on provision of private goods Pareto-dominates standard individualistic private decision-making. If I had sent it to the *Journal of Economic Theory*, they would have laughed at it, because it's too simple. But I'm very proud of the paper.

As I say, I got the idea when I was thinking about an entirely different area, which was the old issue in econometrics of aggregating forecasts. James Surowiecki, the *New Yorker* columnist, wrote a book in 2004 called *The Wisdom of Crowds*. He describes walking through some street in Manhattan where people are counting jellybeans. You average their forecast for the jellybeans and they do better than the individual forecast. That turns out to be Jensen's inequality, but the concept dates back to Francis Galton's work in *Nature* back in 1907.[9] The damn thing is just algebra! There's no magic to it at all. I won't take the time now to explain it – you can read the article that came from a beautiful research experience.

How do you balance your research and non-research activities?

Being department chair was particularly hard. I had to be very disciplined, because I knew there were times of the year, particularly in the winter quarter and early spring, when I just could not get anything serious done. And so in the late spring and during the summer, I

[8] Manski, C.F. (2010), 'When consensus choice dominates individualism: Jensen's inequality and collective decisions under uncertainty', *Quantitative Economics*, Vol. 1, No. 1 (July), pp. 187-202.
[9] Galton, F. (1907), 'Vox populi', *Nature*, Vol. 75, pp. 450-451.

became extraordinarily jealous of my research time, and that's when I got work done. Looking back over the last three years, I was able to be more productive than I expected, but you still pay a price, because it's a rough job with not much personal reward. I'm glad it's over.

How do you balance your personal and your professional lives?

The distinction between 'professional' and 'personal' is not clear for a researcher, because we're basically working all the time. And what's work? To me, grading exams is work. I don't find any pleasure in it at all. And some other administrative aspects are work. But doing research is not work. Non-academics don't understand this, because they have this 9 to 5 mentality in which in you go home and leave your work behind. I am working all the time, but it's not work in the sense that I enjoy it.

REFLECTIONS AND THE FUTURE OF ECONOMICS

What have been the most important findings and contributions in your research fields during the course of your career?

Going back from the beginning, Dan McFadden's extraordinary work on discrete choice analysis was path breaking, and influenced very heavily the first 10 or 15 years of my career. And so too did the development of nonparametric work in the early '80's, which contained some very simple conceptual ideas that were formalized by statistical theorists. In terms of my own work, it's clear that the research on partial identification is the largest, longest lasting contribution that I've made.

It's also very easy to forget, because it's not valued sufficiently in the profession, that empirical work requires data. And so the development of large micro data sets has been very important. This began in the late '60's in the United States with the *Panel Study of Income Dynamics* and the *National Longitudinal Survey of Labor Markets*, and has then moved on to the *Health and Retirement Study*. I think a Nobel Prize should have been given to the originators of some of those early data sets.

What are the main challenges facing your research fields?

The biggest challenge is to do useful work. Methodological developments should be things that contribute to solving real economic problems, whether they relate to understanding the way the economy works or forming public policy. I'm not looking for some great theoretical breakthrough at this point. It's not like a physicist saying we have to understand the theory of everything, or like a biologist in the '40's and '50's being oriented towards figuring out the genetic code. I don't see anything similar in economics right now, but maybe that's just my lack of imagination.

What are the strengths and weaknesses of your own research?

I'm very hard on myself, and that may be a weakness. I have a reputation as someone who is fairly conservative in drawing conclusions, and so someone who doesn't like my work would call me nihilistic. I don't think that's entirely fair, but it's true that I tend not to stick my neck out and say, "This is the answer." Tomorrow, I have to give a seminar here, which is going to be about those in policy analysis who act as if they know things. I'm constantly saying that we don't.

For example, the deterrent effect of various punishment, particularly the death penalty, is a classic problem. It's critical to forming criminal justice policy, but it's also very hard to study. Back in the early '70s', Isaac Ehrlich stuck his neck out and said in the *American*

Economic Review that one execution deters eight murders, which was later cited by the US Supreme Court. Al Blumstein, from the National Research Council, was called in as an adjudicator. I wasn't on the committee, but I went to some of their conferences and had some marginal participation. The NRC concluded that you can't learn anything credible about the deterrent effect of capital punishment. That was an extraordinary statement, and the research area got closed off for the next 20 years. But in the last 10 years, there has been a whole spat of new work on the deterrent effect of capital punishment, which has led to the creation of the NRC's Committee on Deterrence and the Death Penalty. I'm on the committee so I have to keep an open mind as to what we'll conclude about the new research. But if you have something as value laden as capital punishment, there's an issue of what the standard of proof should be. That's a very, very hard question.

As another example, Jim Heckman and I have enormously different views on sticking your neck out. He got a Nobel Prize for his work on parametric selection modeling, but I don't believe any of his assumptions. And now his fragile research on early childhood cognitive development is getting a lot of attention because he's pushing it so hard. Steve Levitt does this with his work too. Those guys really hate each other, but they actually share a lot in common.

Do you have any professional regrets?

Only one. I finished my PhD in three years, and I hated graduate school. We were spoon fed, told what's the orthodoxy, and I just wanted to get out of there. And so I went on the market much too early, and as a result, had a lot of trouble getting a first job. 24 years old was ridiculously young, and I was too naïve to realize that people wouldn't understand my work on discrete choice analysis. McFadden hadn't even published his first paper on that topic yet! I think if I had waited around another year, I would have got a much better job placement. On the other hand, where I did get a place was a very creative environment and so maybe I was better off being there than a mainstream department.

Do you have any professional ambitions?

Yes. I have this long line of work, starting with the partial identification research, which got transmuted into social planning under ambiguity. That is, how do you make public policy decisions with limited information? I think that is extraordinarily important, because making policy decisions with limited information is what we have to do. I can see 100 years of work to be done along these lines, but the main thing is to disseminate the ideas that I already have. And so I've signed another contract with Harvard University Press, with whom I've had a long relationship, to publish a book that's tentatively titled *Public Policy in an Uncertain World*. Sometimes I like to personify my audience, and in this case, I want Barack Obama to be able to read it. It's not for the newsstands in the airport; it's for the serious, intelligent, but non-technical, reader who is involved in policy-making.

I hate what goes on in the United States now, where everyone takes out an extreme position, like on macro policy. Either we've got to stimulate the economy or we have to worry about the deficit. No one knows what the right macro model is! I wish people would just face up to the fact that we have to make policy decisions with limited information. If I can somehow get that across so that it influences the way policymaking is done and can just get us out of this ridiculous extreme debating style that this country evolved into…I don't know what the chances of success are, but that's what I want to do.

How would you describe the state of economics today? Are you optimistic about its future?

It's hard for me to be optimistic about anything. Some would say that's part of my nihilistic personality (*laughs*).