Daniel McFadden: Choice Models, Maximal Preferences >>>

Daniel McFadden

Interview of Daniel McFadden by Y.K. Leong

Daniel McFadden made fundamental and important contributions to behavioral economics in general and to choice theory in particular. He is an active proponent and exponent of the use of mathematics and statistics in solving problems of economic measurement and analysis arising in applied economics. The econometric models that he developed in choice theory have been widely used in economics and other social sciences; for example, to practical problems concerning transportation, choice of occupation, brand of automobile purchase, and decisions on marriage and number of children. He has developed scientific methods for conducting and interpreting surveys on social and economic issues. His numerous publications cover a wide range of areas in economics and econometrics. For his contributions to the development of theory and methods for analyzing discrete choice, he was awarded the 2000 Nobel Prize in Economics, which he shared with Jim Heckman.

Originally trained in physics and having made some innovative hardware contributions in the study of cosmic ray physics while still an undergraduate at Minnesota, McFadden switched to behavioral economics for his graduate studies. After a year at the University of Pittsburgh, he joined the University of California at Berkeley and then joined MIT where he was Professor of Economics, held the James R. Killian Chair and was Director of the Statistics Research Center. He returned to Berkeley in 1991 to establish the Econometrics Laboratory which is devoted to providing and improving computational techniques for applications in economics and of which he has been (except for one year) its director since then. He is currently the E. Morris Cox Professor of Economics at Berkeley.

McFadden has received numerous awards, prizes and honors from scholarly and professional bodies for his research work, among them the John Bates Clark Medal, Frisch Medal, Nemmers Prize, Richard Stone Prize, and, of course, the Nobel Prize . He has been invited to give distinguished lectures such as the Fischer-Schultz Lecture and the Hooker, Smith and Jahnsson Foundation Lectures. He is an elected Fellow of the Econometrics Society and a member of the American Academy of Arts and Sciences and of the National Academy of Science. He has served on the editorial boards of leading journals such as Journal of Statistical Physics, American Economic Review, Journal of Mathematical Economics and Journal of Econometrics. He has contributed his expertise and advice to many professional committees, advisory boards and public bodies. He was President of the Econometric Society and of the American Economic Association.

He was interviewed by Y.K. Leong on behalf of *Imprints* at the Swissotel, Singapore on 20 March 2005 when he was at the Institute to give an invited lecture during the program on semi-parametric methods. The following is an edited but unvetted transcript of the interview which gives us an insight into a creative mind of wide versatility and a glimpse of new interdisciplinary vistas that are opening up in economics.

Imprints: Could you tell us why and how you moved from physics to economics?

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Daniel McFadden: A little background on how I had very broad interests as a student in many subjects. I studied mathematics, psychology and physics, but I chose to take my degree in physics. I could have taken it in mathematics or psychology. In physics I was working in an experimental laboratory as an undergraduate and I continued that as a graduate student and started teaching physics right away. But I was only 19 years old. I still didn't know exactly what to do with my life. When an opportunity came to go into very a broad program in behavioral science, I just switched. I didn't think of that as a big change because it's all science and uses mathematical tools. I already had many courses in all these subjects. So it was an easy transition. I moved to psychology really to do psychology, not to do economics. So I began work in this behavioral science program with the intention of getting a PhD in psychology, but I was also very interested in mathematical modeling. I found that mathematical modeling was somewhat at the fringe of psychology. I found that the people in the economics department of my university were closer to my interests. I moved to economics primarily to do psychology using mathematical modeling. This was at the University of Minnesota. It was only after I had done that that I had to take the special economics requirement to write a PhD thesis in economics, which I did, and I thought that economics was very interesting. It was rather an accident that I came to economics, I went through it very quickly – I did all my coursework in one year and I wrote a thesis in my second year. I was still not very knowledgeable about economics when I got my PhD in economics. So that's the background. I don't view it as a big change in career and I think that in the things I do, I would probably have been a successful physicist or psychologist.

I: You actually did some research in physics?

M: I did, I designed an X-ray telescope and it was used in a first demonstration that the aurora borealis was an X-ray discharge. I designed some of the computers that were used in the van Allen satellites. In those days I was very much into the engineering and experimental side of physics. One of the reasons that I made the transition was that I was very interested in psychology and I was also very interested in theory. I thought that I was a better theorist than I was as an experimentalist. So in a way I was more attracted to behavioral science than I was to experimental physics.

I: Did your original training in physics influence the way you look at problems in economics?

M: Very definitely. I learned a great deal about how to be an empirical scientist and I learned a great deal about the interactions between theory and measurements and about testing hypotheses about your theory and keeping the integration of your theory into your empirical work. I've made that one of the themes in my work in economics which is to try to bind theory and measurement close together.

I: Is behavioral economics a science? How does it differ from traditional or classical economics?

M: I say that behavioral economics is a science if by science you mean that there is some theory and there is a measurement method and you use scientific methods to test your theory using the measurements. I think that traditional and behavioral economics have this scientific core, but classical economics is more like mathematics, it is more axiomatic. It takes the principles as self-evident axioms and makes logical derivations from them and then applies them to economic problems. The difference between that and behavioral economics is really the use of experimental evidence in closely tying the axioms you accept to the experimental data.

I: How old or how young is behavioral economics?

M: I think it's actually quite old but it's only in the last decade that there are now enough coherent measurement techniques available that are actually useful. Before, it was recognized that there was a need to do empirical testing of economic theory but the problem was that the classical theory was itself, in some ways, too accommodating. It was too easy to explain data without really getting the scientist to test. In earlier periods, people would talk about the need to look at behavior, and some important work was done by Herbert Simon, for example, to recognize and take a serious look at the limitations of fact and theory. But it was not enough of an engine for developing hypotheses to replace the classical theories. But now I think that due to new measurement tools, it is a very effective device.

I: Is it getting more mathematical?

M: In some ways, it's getting less mathematical and more experimental. I think that in the end there will be a wave of new experimental results and then, at some point this will be followed by a wave of new theory, a new mathematization of the subject, to regularize the wave of new results.

I: Do you introduce ideas and methodology from psychology into your research?

M: I do although I would say that I don't do it in a deliberate way. What I do is that I draw on all the subjects that I know. I find that having a rather broad background gives me access to psychology and other areas in behavioral science. I also studied anthropology, political science, sociology, and, of course, mathematics, statistics. I draw very freely from other subjects that seem appropriate.

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I: How do you perform controlled experiments in behavioral economics?

M: I will give you some examples from current research that I am doing. What we do is to interview people on the Internet and in this particular application we were interviewing elderly people regarding their plans about savings and finance for their retirement and medical expenses. There were also questions about what they have done and what positions they have made. In those surveys we design an experimental treatment into the survey questions so that a given subject will get a randomized treatment. In the design of the experiments, the intention is study things like impact on response behavior of question order and question framing. We find, for example, that in asking people a preliminary question about the probability of having to live in assisted living when they are old, you influence their response to a key question later on when they are asked what positions they have actually made. Based on how you frame a few questions earlier on, you can change people's report about their actual behavior. That's really classic experimental design, and in this case, we find that the questions will make a big difference. Our aim here is to try to improve survey techniques for economic surveys and discover first what are the biases that can recur and secondly, to try to build experimental treatment into surveys in an essential way within a questionnaire.

I: Has any of your models on choice behavior ever been applied on a big scale by large business or national organizations? Are the results as predicted by theory?

M: The answer to that is that they are widely applied. The model that I am best known for is the multinomial logit model, which is, in fact, not really original to me. I think there were some other things almost contemporaneous in the literature. The reason my particular version of that model became popular, initially among the economists, is that I showed how you could use the estimation of that model to derive preference maximization. You could draw inferences about people's tastes from the empirical model. That made it popular among economists. But that model is a pretty elementary model. What I did in the 1960s was that I also wrote software to estimate it. In those days there was no good software for statistical analysis. One of the reasons that the model became popular is that I provided a way to actually use it to test the estimator. But now what happens is that that particular kind of model is almost as common as linear regression. And like linear regression, it is sometimes used very badly and produces some very bad results. But sometimes it's quite useful for forecasting purposes. When I first developed it, one of the first applications I made was to transportation planning, predicting demand for transportation alternatives.

I: Were you commissioned to do the modeling or did you do it on your own?

M: I did that on my own, but I took advantage of the fact that there was research money available in the area because there was a new system under development. I was able to use the existence of that new system to get funding for a large project. Well, that original application in transportation has continued. Some of the most common uses of my methods continue to be in transportation. For example, in Paris and Hong Kong, it is used systematically, as I understand it, as an operational management tool to do things like real-time traffic management. I think it is also used for traveling planning. It's just a physical model – they are not using any deep theory. Like generic statistical tools, I don't have a single way of using it – people use it as they wish.

I: If you have patented those methods, it could have brought in some money.

M: Some people have told me that if I had patented it, I could have become wealthy. But I have a different philosophy. In everything that I do, I make it a point of giving it away freely. I'm a member of the open software philosophy, so all my software is openly available to everybody.

I: Are your methods part of the standard material in books?

M: Certainly in econometrics books, yes. They are standard procedures within most statistical packages.

I: In your research on behavior of choice, is there any finding that you consider to be counter-intuitive?

M: I would say that the most counter-intuitive thing that I have found (this is not my personal research but by a group of people in this area) is the finding that relatively high level economic decisions which seem to be a very complex cognitive activity involving a lot of learned activity and so forth seem to be tied to very primitive pathways in the brain – very direct connections to rewards in the brain. So you get the phenomenon that, for example, people will respond to play in lotteries or economic games in ways which seem to be very primitive, very fundamental in terms of their positions within the structure of the brain. That, to me, is very surprising. I would have thought that economic decisions would be very broad, high-level, dispersedly processed things but instead it is said that there are direct connections between economic decisions we make now and probably the earlier evolutionary development in humans of the pathways developed for reward or avoiding risk. It explains some of the strongest anomalies in economic behavior – the asymmetry between how people make judgments about

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gains and losses, the willingness of people to be altruistic and to trust other people. Some of these things are quite anomalous in terms of classical economics but when they study the pathways to reward in the brain, it corresponds exactly to economic behavior.

I: Could you tell us something about the Econometrics Laboratory of which you are the director?

M: My laboratory is primarily a service laboratory. My view of applied econometrics is that in the past, one of the limitations in economics has always been the difficulty of processing the large data sets collected. Economic data that is traditionally collected tends to be very large scale, like census data. Traditionally, economists are hindered in their ability to work with these; they had limited computers and limited skills. When I established this laboratory, my intention was really to provide a good background facility for economists to do large-scale empirical work and to use computationally intensive methods in econometric analysis. That's largely what we do. We have very large file servers, high powered computers and we service the large community of economists at Berkeley. Within the university, the laboratory is pretty open – certainly open to all members of the departments and students. When people not within the university need high powered computation, I try to accommodate them.

I: How often do you go back to your farm or ranch?

M: I have a little farm, about one hour's drive from Berkeley. I go there every weekend. When I'm there, I work very hard in my vineyard and garden. To me this is refreshing. I grew up on a farm. Before I left for college, I worked very intensely on the farm. When I was young, I thought I would – I didn't plan to be a farmer because the work was too hard – I thought I would be doing something related to farming, like being a county agent. I always said that. Now I enjoy being back on the land.

I: When you are back at the farm, do you still think about your scientific work?

M: I do, yes. My own experience is that if you actually sit at a desk and try to prove theorems, sometimes you just go slower and slower. It's very hard to be completely linear in developing mathematical results. Sometimes I find that if I put a problem down, go out and work hard physically, then either in the following working or when I wake up the following morning, the solution is there. I don't think it (farm work) slows me down at all; it probably helps me scientifically.

I: Can you say you have found some insights into your scientific work while you were working on the farm?

M: Definitely; not because of the farm work I'm doing, but simply because, at least for me, when I'm trying to prove a theorem that is difficult or challenging, I often have to do it almost subconsciously. I have to work very hard to prepare my brain and then to make the final connections, I almost have to walk away from the problem and then the pieces come together. And the farm is a good place for that.

I: Are you optimistic about the economic behavior of human beings?

M: I'm certainly optimistic about our ability to study behavior. Behavioral science has made great advances – a lot of good tools are available and computers allow us to build better models. We have learned a great deal about experimental techniques and from doing experiments. Game theory is becoming an important empirical tool, it used to be primarily a theoretical tool. Empirical game theory is becoming very useful, and there are now very strong interactions between economics and biology (brain science). It raises the possibility of doing experiments in which we use biological treatment (hormone treatment) as an experimental device to study behavior. I think this is a marvelous opportunity to learn how the mind works.

I: Has anybody actually tried to make connections between certain type of economic behavior with certain activity in the brain?

M: Yes, very definitely. It's not my own research, but I have followed with terrific interest the work of Ernst Fehr at the University of Zurich. He's doing experiments in which people are administered particular hormones and then asked to play an economic game, some kind of ultimatum game, which involves trust. It's a game where the Nash solution – you don't trust anyone – is expected to be played but, in fact, people do not play the Nash solution. What he finds is that by changing the level of hormones in different treatments, you can drastically change the way people play this game. It is a striking demonstration of a direct link between buffer brain chemistry and human behavior – altruistic behavior, trust, social behavior.

I: Is psychology becoming physiologically related?

M: Well, I think it's becoming so, very strongly. I think that's a powerful scientific advance because it gives you so many more possibilities for good experiments.

I: In some sense, it's also a bit pessimistic that you cannot run away from certain aspects of the brain's malfunction.

M: It does suggest that there is a lot of chemistry involved in our tastes and in our behavior, and to some extent, the

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