Mathematical Conversations

Eric Maskin: Game Theory Master >>>

Eric Maskin

Interview of Eric Maskin by Y.K. Leong

Eric Maskin has made fundamental and groundbreaking contributions to numerous areas of game theory and economic theory, such as implementation theory, auction theory, the economics of incentives, and social choice theory. Among the wide range of topics he is currently studying are the design of auctions, comparison of different electoral rules, the pros and cons of political accountability, and the advantages and drawbacks of protecting intellectual property. His work, sometimes in collaboration with coauthors including Partha Dasgupta, John Riley, Jean Tirole, Jean-Jacques Laffont, Peter Diamond, and Drew Fudenberg, has been widely applied to industrial organization, finance, development, and other fields within economics and political science.

He has published over one hundred articles and book chapters, and has served on the editorial boards of leading economics journals such as the Quarterly Journal of Economics, Economics Letters, Social Choice and Welfare, Games and Economic Behavior, the Review of Economic Design, the Review of Economic Studies, and the Journal of Economic Perspectives.

He has been frequently invited to give named lectures, in particular, the Arrow Lectures, the Mckenzie Lecture, the Zeuthen Lectures, the Schwartz Lecture, the Marshall Lectures, and the Pareto Lecture. He is a Fellow of the American Academy of Arts and Sciences and of the Econometric Society, a Corresponding Fellow of the British Academy, and an Honorary Fellow of St. John's College, Cambridge. He was President of the Econometric Society in 2003.

He was a Research Fellow at Cambridge University (1976 – 77) and taught at MIT (1977 – 84) and Harvard University (1985 – 2000), where he was Louis Berkman Professor of Economics (1997 – 2000). He then moved to the Institute for Advanced Study at Princeton and is currently Albert O. Hirschman Professor of Social Science.

When he was at the Institute as an invited speaker of the program on uncertainty and information in economics, the Editor (Y.K. Leong) of *Imprints* interviewed him on 16 May 2005. The following is an edited account of the interview in which Maskin talked passionately about the revolutionary game-theoretic ideas that are changing economic theory and influencing the social and political sciences in practice.

Imprints: Your doctorate at Harvard was in applied mathematics. What kind of applied mathematics was it? Was it related to economics?

Eric Maskin: In those days, applied math at Harvard at the graduate level was a fairly free-form program. Each student designed his own program of study, and the only common requirement was that the dissertation had to have significant mathematical content. My own program included a fair amount of economics, and, in fact, my advisor was the economist Kenneth Arrow.

I: You were in the Mathematics Department?

M: No, I was in Applied Math. It wasn't a "department" *per se*, but an interdisciplinary committee, including some people from the math department, a few from the economics department (in particular, Arrow), and assorted others from statistics, engineering, and so on. I did quite a bit of mathematical economics. I also did some mathematics not related to economics simply because I was interested in it. My thesis was in game theory and social choice theory.

I: You went to Cambridge University immediately after your doctorate. Was there any specific reason for that?

M: While I was studying at Harvard, the Cambridge economist Frank Hahn – a close friend and collaborator of Arrow – visited for several months. Hahn encouraged me to spend some time in Cambridge in a postdoctoral position. The idea appealed to me: Hahn was a leading mathematical economist and so it made sense educationally; I also thought it would also be interesting culturally.

I: Did you do any joint work with Hahn?

M: I never wrote a joint paper with him, but talked to him a great deal about my own work—he was extremely generous about making time for that. I've remained personally very close to him since those days. In fact, I'm going to England at

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the end of this week to help celebrate his 80th birthday.

I: You taught at MIT for a short period after returning to the United States from England. Is it true that one of your most important works was done during this period? Could you tell us something about it?

M: Yes. Actually, I wasn't at MIT for such a short a period; it was 7 years. I worked on many things there, but probably the thing that I'm best known for now was a series of papers on implementation theory. In implementation theory, the idea is to construct a game or mechanism for attaining the goals that you as the mechanism designer wish to achieve. Suppose, for example, that there are some economic resources to be distributed among the agents in the economy and that you, the designer, have a particular set of criteria in mind for evaluating different possible distributions. Imagine however, that determining the best distribution according to these criteria calls for information that you don't have. Assume that the agents in the economy have this information, but you don't. Then, what you can try to do is design a game for the agents to play so that, when equilibrium is reached, the outcome is the same as the one you would have imposed had you had that information in the first place. In effect, the game itself "compiles" the agents' information, enabling the right outcome to emerge in spite of your own ignorance.

I: It sounds a bit psychic.

M: Well, it's what economic systems are supposed to do. An important reason why market economies have historically worked better than planned economies is that typically economic planners don't have enough information to allocate resources appropriately, even if their intentions are benevolent. Markets, by contrast, provide a remarkably good way of aggregating or gathering information. Implementation theory can be thought of as a generalization of the sort of things markets do.

I: Is implementation theory part of game theory?

M: Yes, it is part of game theory. Implementation theory actually intersects a number of areas of economics, but because it presumes that people act strategically, it is certainly game-theoretic. Still, it differs from much other game theory in an important respect. Usually in game theory, we start with the game to be analyzed and then try to predict what will happen, i.e., what the outcome will be. In implementation theory, the process is reversed. We start with the outcome that we want and then consider how we might construct a game that achieves that outcome. So implementation theory can be thought of as the "reverse engineering" part of game theory.

I: You went back to teach at Harvard after MIT. Was it some kind of loyalty that made you return to Harvard?

M: It was partly loyalty. Also I was intellectually close to a number of people at Harvard, which had been my first academic home. Still, MIT was a terrific place to be. I enjoyed and profited from my time there enormously.

I: You once mentioned that game theory has revolutionized many fields, especially economics. Could you give us some examples of this in economics?

M: Yes. Before game theory, most of economics dealt with models of large markets–so–called "perfectly competitive" models, where there are lots of economic agents: lots of buyers, lots of sellers. Paradoxical as it may seem, the large numbers actually made the analysis easier, because the agents in those models didn't have to act strategically. If you are only one seller among many, you are not going to have much influence on the other sellers' outcomes, and so won't affect their behavior much. This means that you don't have to take into account how they are going to respond when you decide what price to set or how much to sell; your decision is relatively straightforward. Game theory provided economists with the tools for analyzing the small numbers case, which is the relevant case for many industries. In the U.S. automobile industry, for instance, each of General Motors, Ford, and Chrysler is big enough so that, whenever it acts, it has to calculate how that action is going to affect its competitors and how those competitors are going to react. For economists interested in the automobile industry, calculating what is going to happen is therefore harder than predicting what would happen in an industry where there are lots and lots of sellers. Game theory helps us to make those calculations. It's been developed precisely to deal with the case where each player has a significant influence on the payoffs of the other players.

I: Someone once said that every economist should know auction theory. How much do you agree with it?

M: These days auctions are highly visible economic institutions. They are used in settings ranging from online sales of everyday items to the privatization of major public assets. So, one reason economists should know some auction theory is that auctions constitute an ubiquitous practical way of allocating resources. They also provide us with an explicit mechanism of price determination. How prices actually get set is an issue that is left out of many economic models. Auction theory gives economists the foundation for a theory of price formation.

I: Does auction theory apply to any number of participants?

M: Oh yes, auction theory is all-inclusive in that sense.

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But the theory is probably most interesting when there are relatively few participants because that is when the game-theoretic aspects come to the fore.

I: Is auction theory a species of game theory?

M: Auction theory can definitely be thought of as part of game theory; an auction is a game in which the bidders are the competing players. If I'm a bidder in an auction and considering what bid to make, I have to take into account how the other bidders might behave. Similarly, they will be thinking about what I'm going to do. So there is a strategic interaction that calls for game-theoretic analysis.

I: Have you personally organized any auctions?

M: Well, I tend not to get too involved in practical consulting, but I have made a few exceptions. One was a couple of years ago. The British government was interested in creating an auction to help reduce carbon dioxide emissions by British companies. The problem raised some interesting theoretical questions, and so I agreed to help design the auction.

I: Did they raise more money than they expected to?

M: They weren't actually trying to raise money in this case; they were trying to spend it effectively. The government budgeted about 300 million pounds (or about 500 million U.S. dollars) that they were prepared to spend to induce British firms to decrease their CO_2 output. The question they put to us auction designers was how to construct an auction in which companies would be awarded money in exchange for pledging reductions so that this budget would stretch as far as possible, that is, the biggest reduction in greenhouse-gas pollution would result.

I: I understand that you have been recently applying game theory to the study of electoral procedures within a democratic system. Are there any clear-cut answers?

M: In fact, that work, which is joint with Partha Dasgupta, is not entirely game-theoretic. The issue we examine is whether there exist voting procedures that satisfy certain basic desirable properties. A famous theorem obtained by my thesis advisor Kenneth Arrow over 50 years ago establishes that there is no voting procedure that satisfies all these properties all the time. Thus, the natural question to ask is: Which voting procedures satisfy the properties in the largest class of cases? It turns out that there is indeed a sharp answer.

Specifically, the procedure called "simple majority rule" (or "Condorcet's method," after the 18th-century scholar, the Marquis de Condorcet) is the voting rule that satisfies these properties more often than any other. Of course, I have to tell you what these basic properties are. One, called the "Pareto (or consensus) principle" says that if everybody in society prefers candidate X to candidate Y, then candidate Y should not be elected. Another property requires that all voters should count equally. It's sometimes called the "anonymity" or "one-person, one-vote" principle. A third property, "neutrality," has two components. The first is symmetry, which means that the electoral rules should not favor one candidate over another. The second requires that the voters' choice between candidates X and Y should not depend on their views about some third candidate Z. The final property, called "transitivity," demands that if candidate X is chosen over Y, and Y is chosen over Z, then X should be chosen over Z. From Arrow's theorem, there is no voting procedure that satisfies these four principles all the time. But simple majority rule satisfies them more often than any other rule.

Simple majority rule compares candidates pairwise. If there are three candidates X, Y, and Z running, we should elect candidate X provided that X would defeat Y in a head-to-head contest and also beat Z in such contest. Of course, in most elections, you can't vote for more than one candidate at a time, which means that it may be impossible to tell from the returns alone whether X would beat both Y and Z in this way. Therefore, Dasgupta and I recommend that voters should be allowed to express their rankings of all three candidates. Rather than simply voting for candidate X, you might, for example, write X first, then Z, then Y. When all voters supply these rankings, one can make the pairwise comparisons that I was just talking about.

I: Is it possible to get a situation where there is no clear-cut winner, that is, no candidate who will beat the other two?

M: That is possible. In fact, that possibility was discovered by Condorcet himself. In such a case, one would have to use some tie-breaking rule to determine the winner. Still, even though simple majority rule doesn't always work as it is supposed to, it works, as I was suggesting, more often than any other voting rule. The cases in which there is no clear-cut winner turn out to be less numerous than the instances in which other voting rules run into trouble. Thus, there is a sense in which the problem you point out is less serious than those that beset other voting rules.

I: Has this method been tried in out in practice?

M: I don't believe it has been used in a large-scale election, say on the national level. But it has certainly been used in smaller elections, e.g., those for committee officers. Now that it's so easy to vote and count votes electronically, I think the time has come to try the method out on a bigger scale. The current way of determining winners in Congressional and Parliamentary elections in the United States and the

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United Kingdom—the winner is the candidate with the most votes even if that total is short of a majority—is, in my view, highly objectionable. With more than two candidates, it's quite possible for the winner to be a "minority" candidate in the sense that a majority of voters prefer some other candidate. I would like to see simple majority rule used instead in these elections.

I: You have also done some work on social choice theory. Could you tell us briefly what this is?

M: In a sense, we have already been talking about social choice theory: it includes implementation theory and voting theory. Social choice theory examines the question: How do we go from individual preferences to social preferences? Of course, we do that when we vote, but the theory goes well beyond voting. Almost any public policy question involves passing from individual to social preferences. When we decide how much public education to provide, how much to spend on national defense, or whether to redistribute income from rich to poor, we have to answer that question. Thus, all these issues belong to social choice theory.

I: These are very practical issues.

M: They certainly are. The theory itself is generally worked out at a fairly abstract level, but it often has strong implications for practical issues.

I: Is it easy to convince the politicians?

M: I never try *[laughs]*. I wouldn't know how to begin to convince politicians.

I: In some of your papers, you drew some analogies between economic behavior and animal behavior with respect to evolutionary biology. Could we interpret this to mean that animals are also "economic" creatures?

M: If by "economic" creatures, you mean creatures that "optimize", I think they definitely are. In order to survive, an animal needs (i) food, (ii) shelter from the elements, and (iii) some way of dealing with other animals that might harm it. How an animal pursues these objectives is its "strategy". Evolution serves to select animals with successful strategies over those with strategies that don't work so well. The animals with the successful, "optimizing" strategies will survive. They will have the opportunity to reproduce, and their offspring will carry on the optimizing legacy. So, evolution forces animals to be optimizers in the same sense that consumers and producers in standard economic models are optimizers.

I: Have you tried to apply your ideas to evolutionary biology systems?

M: Absolutely. The paper I gave today at the conference—another project with my old friend Partha Dasgupta— is on this exact topic. We are trying to understand a certain kind of perplexing behavior that has been observed in certain bird species and also perhaps in humans. Humans are considerably more difficult to study because their behavior is so complicated; it's usually harder to do good laboratory experiments with human subjects than with pigeons. Anyway, the paper constructs an evolutionary model that attempts to account for the documented behavior.

I: A related question: does this mean that economic behavior may have genetic origins?

M: Undoubtedly, many aspects of our daily behavior do have genetic origins. The fact that we are impelled to eat when our stomachs are empty is programmed into our genes. But more complicated optimizing behavior (such as the way we invest in the stock market) is often so complex that usually one can't say how much of it, if any, is genetic in origin. That's why evolutionary psychology is such a controversial subject; in the case of humans, it is very difficult to disentangle what is the result of biology, what is the result of rational calculation, and what is the result of past experience.

I: It seems that economics is now so intertwined with so many other fields.

M: That's certainly true. In particular, the boundary with psychology is where some of the most exciting work in economics is being done today. But the boundaries with other social sciences—especially political science—are also very lively.

I: Is the urge to take risk genetically driven?

M: I don't know the answer to that. There are those who suggest that there may be a genetic disposition toward taking risks. It has been conjectured that males may be more disposed to taking certain kinds of risks than females. That's not a question I have looked into myself, but it's fascinating.

I: Economics is so different from what it was 20 years ago.

M: Yes, it's a subject that has evolved quite rapidly. That's one reason why it's stimulating to be an economist these days – the subject changes so quickly.

I: Do you do simulation in your type of work?

M: I don't personally use simulations often, not because I'm opposed to them but because I prefer, as a matter of

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taste, doing things the old-fashioned way by calculation. Simulation can be a useful first step for formulating and ruling out conjectures but, in my view, it should rarely be a last step. After you have done enough simulations to have a reasonably good intuition about what is true, then I think you should sit down and try to prove things analytically.

I: In auction theory, for example, there should be some scope for simulation.

M: Sure. In fact, often before a new sort of auction is used in practice, the designers will try to simulate bidding to make sure something won't happen that they didn't think of. Simulation can be a powerful short-cut. I just don't think it should take the place of theorem-proving.

I: Was there any specific reason why you moved from Harvard to the Institute for Advanced Study at Princeton?

M: Yes, there were a couple of reasons. Harvard was a fantastic place to be, and it has as good an economics department as one could hope for. But one advantage I have at the Institute is unparalleled freedom – freedom that a professorship in an economics department could never bring me. At the Institute I have few formal duties and the time to do pretty much whatever I want. The other thing I get there is interdisciplinary contact. In the School of Social Science (where I am), I bump up against anthropologists, political scientists, historians, etc. all the time. I find this interdisciplinary mix stimulating and interesting. I think one of the ironies about modern universities like Harvard is that one has an enormous variety of interesting people nearby, but little opportunity to talk to them. If you are an economist, you see the economists, of course, but you don't run across people in other fields nearly as often. One nice feature of the Institute is that you talk to non-economists every day, and so and it's not an extraordinary event to have a truly interdisciplinary conversation.

I: Are there other economists in the School of Social Science?

M: I'm the only economist on the faculty, but we always have economists visiting. At the moment there are 5 economists at the Institute for the year, and so I certainly don't feel lonely.

I: Do you have any students at the doctoral level?

M: I do, I have always had Ph.D. students. I think of that as an essential part of my professional life. It's not only fun supervising these students, but useful for my research. Advanced students, particularly those working in areas close to mine, often have stimulating new ideas, which cause me to rethink what I'm doing. In some cases, these students turn out to be collaborators. Some of my favorite coauthors are former students.