The Achievements and the Future of Game Theory: A User's Perspective[*]

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The last fifty years, and especially the last thirty, have brought a revolution in the way economists think. Game theory has become the dominant mode of analysis in economics, supplementing and in many cases replacing the traditional Marshallian and Walrasian equilibrium frameworks. Game theory similarly dominates evolutionary biology. Its use in political science has accelerated rapidly in the last decade. It is also expanding in the analysis of law, and in other social sciences. To do justice to my (self-selected) theme would therefore require a book, and probably a year's full-time effort. In a short paper and presentation I can only offer a summary list of topics, and some more or less organized thoughts and reflections on the items in the list. Most of the uses I consider will be in economics.

The promise of game theory for economics was first seen in von Neumann and Morgenstern's book. It was heralded by its reviewers like Hurwicz (1945), and Marschak (1946) who prophesied that "[t]en more such books and the progress of economics is assured." It took a while for economists to transcend the equilibrium framework, but the promise has now been amply fulfilled. This is in no small measure due to the wonderful exposition of Luce and Raiffa. I am only one of thousands of economics graduate students who first learned game theory from this book.

Of course it took much further work to construct the framework we use today. Ideas and methods developed by Nash, Schelling, Harsanyi, Selten, Shapley and many others were essential for economic applications, and conversely, new game-theoretic solution concepts arose from needs of economic applications. In fact the progress of theory and applications was an interactive process, and there is no clear partition between theorists and users.

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[*] This is a very preliminary version of a paper written for the conference "Luce and Raiffa, What is next?" at the University of California, Irvine, January 25-27, 2008. Comments, especially to point out omissions in coverage and errors of interpretation, are most welcome. I have omitted bibliographic references when it was clear that most people at the conference would know them; I will add them later.

Despite the great progress we have made, there is plenty to do over the next 50 years. Important conceptual problems await good solutions, sometimes even good formulations.

In this paper I organize my overview of the achievements and unfulfilled promises of game theory in economics in a very traditional way, namely field by field.

Microeconomic Theory

Microeconomics had theorized about competition with small numbers of sellers long before game theory came along, and the equilibrium concepts of Cournot and Bertrand were nothing other than Nash equilibria in their specific contexts. But the adoption of a more general game-theoretic perspective has made a lot of difference to the way we think about and do microeconomics. This is most important in the field of industrial organization, so I will discuss it there. But other issues in microeconomics have also progressed thanks to game theory.

Probably the most important instance of this is the establishment of the link between competition and large numbers of buyers and sellers. Although Edgeworth provided the idea a century ago, it was the relation to the core that made it not only rigorous but also clearer. The Debreu-Scarf paper gave me one of my first "wow" moments in my student days. It is also worth mentioning because economics papers that use cooperative game theory are in the minority.

Another early and somewhat implicit use of game theory in economics was Vickrey's work on auctions. The game-theoretic basis of auctions was made explicit by Wilson in a remarkable series of papers in the 1960s and 70s. There he also established a three-way connection between market mechanisms, bidding mechanisms, and the core. The work on auctions has flourished in the last decade or so, with powerful motivation and much data coming from large real-world auctions.

We now regard information economics and game theory as tightly linked. This is natural, because when players in a game have private information, inferring information from actions, and the manipulating such inferences by choosing (or requiring the other player to choose) actions is itself a game, which is arguably more important than the

game that will subsequently be played. But early ideas of information economics emerged without a rigorous game-theoretic basis. Arrow gave us the basic concepts of moral hazard and adverse selection in a largely verbal discussion of health economics and insurance. Akerlof's lemons model was built in a traditional supply-demand framework. Spence constructed his signaling model in an ad hoc manner, as did Rothschild and Stiglitz for their model of competitive screening in the insurance market. The game-theoretic foundations for these equilibria came later from Kreps, Judd and others. This is an example of the interactive development of theory and application that I emphasized earlier.

The incorporation of private information in games largely follows Harsanyi. In each application, a model begins by specifying the possible types of each player. One can never be absolutely sure that the specification is exhaustive, and equilibria of the model may not be robust to introducing the possibility of some other type, even with small probability. In evolutionary games, stable outcomes may similarly be sensitive to the specification of what kinds of mutants can possibly arise. The validity of our models of most applications may therefore be at risk of what Donald Rumsfeld called "unknown unknowns." This is a real worry, and some progress is being made on it at the level of basic theory: less than full common knowledge (Dekel and Gul 1997), global games, where players get different noisy signals about the game (Morris and Shin 2003), and games where players are imperfectly aware of the strategies of other players (or even of their existence) and of each other's awareness (Feinberg 2005). But this work is still at a foundational stage, concerned with developing the right language and theoretical structure for addressing the issues. I have not seen any satisfactory applications.

The role of game theory in microeconomics goes far beyond the obvious one for analyzing markets with small numbers of participants. Even when a market has a large number of participants ex ante, the transactions often entail some sunk costs that then lock pairs or small groups into strategic interactions ex post. These interactions require game-theoretic analysis, and the numerous market participants must look ahead to this second stage when they design the terms of their transactions at the first stage. This is the basis for Williamson's transaction cost economics. Once again his original ideas were

developed without a rigorous game-theoretic basis, but have been subsequently enriched by that framework.

Behavioral economics is a very active research area these days. It pertains to the "decisions" part of Luce and Raiffa just as much as to the "games" part. Luce and Raiffa gave us a definitive statement of expected utility theory that was the state of the art at that time. They only briefly mention Allais. We now know that preferences often depart from the assumptions of rationality that underlie expected utility theory: completeness, transitivity, the independence axiom, dependence only on final outcomes (invariance to status quo points and framing), and so on. Preferences also differ from the assumption of selfishness that economists often make. All of this in turn enters game theory; Rabin and others have shown how other-regarding preferences can affect game equilibria. Camerer's recent book has given us an excellent account of these developments.

A second aspect of behavioral economics relates even more directly to individual decisions and their interaction in games. This is the argument that people do not optimize their own objectives or payoff functions; they use various heuristics and rules of thumb.

The evidence for behavioral claims from laboratory experiments is impressive, although recent evidence from field experiments conducted by List and others (see Harrison and List, 2004) suggests that traditional microeconomics retains much validity in its own setting of market transactions among experienced traders. Similar contrast between the laboratory and the real world occurs in the use of mixed strategies: they are not observed in the laboratory but conform well to equilibrium predictions in professional sports. More generally, reality seems to be some mixture of orthodox and behavioral approaches, and the relative weight of the two differs in different contexts. Pinpointing the precise mix for each context promises to be an exciting research agenda for the next decade.

Social Choice Theory

Game-theoretic foundation of this field was originally implicit in the works of Arrow and his immediate followers like Sen. It became more explicit when strategic manipulation of social choice mechanisms came to the fore in works of Hurwicz, Gibbard

and Satterthwaite. Identifiable subfields now include aggregation schemes and voting theory (see Saari 2001), and implementation theory developed by Maskin and others.

The field also used cooperative game theory. The power indexes of Shapley-Shubik and Banzhaf are obvious examples.

Political Economy

Traditional economics starts with the Pareto optimality of general competitive equilibrium, and examines how reality differs from that ideal for various reasons: imperfect competition, asymmetric information, sunk costs, externalities and public goods, and so on. Political science takes a pessimum as its starting point, namely Arrow's impossibility theorem, and then looks for various escape routes from it: preference restrictions in voting theory, procedural rules in committees and legislatures, and so on.

In political science, decision-making in committees and legislatures involves relatively small numbers of players, and deals with agenda manipulation, strategic voting, repeated interactions and self-enforcing collusion, information acquisition, delegation, and so on. All of these are eminently game-theoretic.

Another noteworthy feature of this field is the availability of large amounts of data of good quality. This enables empirical work on estimation of preferences and testing of hypotheses concerning strategy choices and game equilibria.

Industrial Organization

Competition among small numbers of sellers is the central theme of this field; therefore it was the obvious place to apply game theory. But this had to wait until the mid- and late 1970s. Before that, the field relied on an uncomfortable ad hoc set of tools: residual marginal revenue curves and the like. This led to a focus on price and quantities as the strategic variables; other choices made by firms, such as R&D and advertising, were regarded as mere shifters of cost and demand curves. Perhaps the most important conceptual contribution game theory has made to industrial organization is to broaden our vision of competition, by letting us recognize the full multidimensionality of strategies in

which firms compete. Perfect competition should no longer be thought of as "price-taking"; rather, it should be "utility-taking." This shift of viewpoint is most useful when we analyze markets with information asymmetries, for example when we prove constrained optimality of some equilibria involving moral hazard.

Early explicit applications of game theory to industrial organization focused on some specific problems. One was entry deterrence; Spence and Dixit clarified the role of commitment using the concepts of credibility from Schelling, and this was later made more rigorous in terms of Selten's concept of subgame perfectness. Milgrom and Roberts focused on the information aspect, and clarified its role in predation and limit pricing.

The other focus was on repeated interaction. Its role in resolving prisoner's dilemmas goes back to the 1950s, and Luce and Raiffa (pp. 97-102) give a clear account of this phase of the theory. It has come a long way since, contributing to the development of general solution concepts such as perfectness and renegotiation-proofness, and characterization of possible outcomes such as the folk theorem and work of Abreu-Pearce-Stacchetti and others.

Labor Economics

Bargaining between management and unions is an important topic in this field. For many years it used the Nash bargaining solution for modeling this. Nash's axiomatic approach may seem more appropriate for normative or prescriptive purposes than for positive descriptive ones. But Nash thought of the outcome of bargaining as resulting from some unspecified process of negotiation or strategizing by the individual bargainers each acting in his own interests; the cooperative solution was intended as a device to cut through the complex details of this process and be useful for predictive purposes. Game theorists subsequently considered different specific non-cooperative bargaining games, and these, most notably Rubinstein's model, have also proved useful for thinking in labor economics. In more general bargaining contexts, Raiffa's excellent book combines theory and empirical case studies and gives very valuable practical guidance.

Many labor markets have large numbers of suppliers and many firms on the demand side, so tools of traditional economics can be applied. But in many markets the

parties have heterogeneous preferences, so matching is an important consideration. The theoretical and empirical contributions by Roth, following in the steps of Gale and Shapley, have established this as a major application of game theory.

Search theory is an important topic in labor economics. This pertains to decision theory more than to game theory, but that is an equally important part of the Luce-Raiffa book, and therefore should be noted in our celebratory conference.

Macroeconomics

Macroeconomics is a peculiar game, with just one large player, namely the government or the central bank, and numerous small players, namely the private actors in the economy. But the conduct of monetary policy raises many game-theoretic issues, such as time-consistency, inferring information from actions, commitment by delegation, and so on. When a central bank is independent of the government, the conduct of monetary and fiscal policies becomes a game between two large players.

Public Economics

The optimal income tax model of Mirrlees was perhaps the earliest example of a mechanism for screening by self-selection, although not many people realized its full potential. Indeed, it implicitly develops and uses the revelation principle, later developed rigorously by Myerson, Dasgupta-Hammond-Maskin, and others. The applications to regulation by Baron-Myerson and Laffont-Tirole probably deserve the most credit for popularizing the method.

Dynamic optimal tax theory also brings time-consistency issues to the fore, and these have been studied to distinguish tax policies with and without commitment.

International Economics

The choice of tariffs and other trade restrictions in a world with large countries is a prisoner's dilemma. Early models of trade wars and trade negotiations, for example by

Johnson (1953), did not make explicit use of game theory. The relationships emerged later in subsequent research and surveys, for example Dixit (1987).

The topic of strategic trade policy, where governments put in place tariff and subsidy policies to give their national firms in the subsequent oligopolistic competition in world markets, was from the start explicitly grounded in game theory. The theory of multi-stage games and subgame perfectness had just emerged, and the taxonomy that clarified the gains from making commitments to be more aggressive or more passive developed simultaneously (Fudenberg and Tirole 1984). Most of the basic literature on strategic trade policy is surveyed in Helpman and Krugman (1989).

International agreements on trade liberalization and resolution of dilemmas and disputes must provide mechanisms for enforcement. Bilateral enforcement is often inadequate: if country A violates the agreement in a way that harms country B, for example if B exports to A and A raises its trade barriers, B may not be able to punish A because A may not export to B anything relevant or significant. Therefore effective punishments must be multilateral: some country C must punish A on B's behalf, even though C is not harmed by A's action. The need for, and potential of, such mechanisms has been modeled game-theoretically by Maggi (1999).

Conflicts also arise in international macroeconomics, in the conduct of monetary, exchange rate, and fiscal policies. Attainment and enforcement of cooperation raise similar issues. Explicit coordination, for example in the European common central bank, is a problem in political economy, namely decision-making in a committee. Such issues have been studied, sometimes starting with implicit game-theoretic basis, and gradually becoming more explicit in the use of game theory.

Institutional Economics

Institutional economics used to be purely historical and descriptive. In the last two decades it has acquired a better theoretical foundation. The basic idea is that institutions and organizations facilitate good outcomes in many social interactions. These interactions take the form of multi-person games that do not automatically have good equilibria. Coordination and assurance games have multiple equilibria, and a selection device such

as Schelling's focal point is needed. Prisoner's dilemmas arise in many transactions, and situations of the provision of public goods and prevention of public bads (especially the overexploitation of common property resources).

Schotter (1981) was probably the first to formulate the idea of institutions as equilibrium selection devices. Ostrom (1991, 1999) has studied numerous cases an interpreted them in rational and behavioral game-theoretic frameworks. Greif was a pioneer in interpreting institutions of trade in medieval times using game theory; his recent book (2006) gives a comprehensive statement. I have modeled non-governmental institutions that support trade by protecting property rights and enforcing contracts based on information dissemination and for-profit enforcement (see Dixit, 2004).

All of the formal work is about equilibria. There is evidence of how institutions evolve to cope with new and changing situations and to establish new equilibria (and sometimes how they fail to evolve and get locked in to equilibria that have become dysfunctional). But formal dynamics of this evolution is less well developed.

What Next?

This brief and selective overview is nonetheless enough to demonstrate that game theory has served economics very well. The solution concepts have been useful for rigorous modeling of basic economic issues of commitment, information, competition and cooperation. In return, the process of applying the formal theory to specific situations has been a useful testing ground, and has sometimes contributed to progress of the theory. But the overview also points out some areas and topics needing more attention in the future. Let me repeat some of these briefly.

The first such task calls for improving our understanding of the scope and limits of rational choice and behavioral approaches to decision theory and game theory. Much evidence has emerged from laboratory and field experiments, and from econometric analyses of non-experimental data. These have different merits and drawbacks, and present different tradeoffs in matters of scientific control or lack of it, practical relevance or artificiality of the situation, the participants' experience or lack of it in similar games, and the size of the stakes or incentives. No one empirical approach is perfect; therefore

we should not rely too much on any one of them, but instead should be willing to learn something from each. Our overall judgment should be formed by considering the findings of them all: laboratory experiments, field experiments, econometric studies, and even qualitative case studies.

My second point concerns credibility. Our models regard this as something binary: a promise or threat is either credible or it is not; an equilibrium is either subgame perfect or it is not. In reality we often think in terms of the degree of credibility. This is not hard to model at one level; it can be captured in the probability that the player is of the appropriate type. But this assumes knowledge of what other types he could be, and the equilibrium of the larger game with multiple types can be sensitive to the precise specification of all possible types.

This leads me to the bigger question of what knowledge the players in a game should be assumed to have in common. In many real games, one player does know all the strategies that are feasible for the other player; after observing an outcome the reaction often is “Who would have thought they could do that!” I mentioned some work that deal with such issues at an abstract theoretical level. Much remains to be done to relate it to applications, and improvement in the theories is likely to be more rapid if undertaken in interaction with applications. This promises to be an exciting area of research for the next decade.

Thus the good news is that a lot has been achieved in the first fifty years after Luce and Raiffa, and the better news is that there is scope to do even more in the next fifty. I hope that advances in bio-engineering will allow all of us to attend the hundredth anniversary conference on Luce and Raiffa and see many of these goals accomplished.

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