GAME THEORY AS A PART OF EMPIRICAL ECONOMICS

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There is something slightly madcap in agreeing to make a hundred year prophecy about a field of study less than fifty years old, particularly a field that has undergone considerable evolution in that time. Yet this is the situation of game theory. Although it has antecedents going back much further (e.g. in the work of Cournot, Edgeworth and Zeuthen), game theory did not become a coherent field until the publication in 1944 of von Neumann and Morgenstern's *Theory of Games and Economic Behavior*. And many of the extensions and reformulations that shaped modern game theory came only in the 1950s and 60s, in the work of Aumann, Harsanyi, Nash, Shapley, Selten, and others.

I will also speculate about the future of experimental economics, which is one of the tools – but by no means the only one – that I anticipate will play an important role in helping game theory bridge the gap between the study of ideally rational behaviour and the study of actual behaviour. Although it too has older antecedents, experimental economics is also a fairly new line of work, having originated more or less contemporaneously with game theory. Indeed, many of the earliest experimental economists are today known primarily as distinguished game theorists, and were drawn to experimentation by the chance to test game theoretic predictions, and observe unpredicted behaviour, in a controlled environment (see e.g. the experimental work in the 1950s and 60s of Maschler, Nash, Schelling, Shubik, and Selten).[1]

Since the safest part of a long term forecast is the far future, let me state at the outset that I am cautiously optimistic that, a hundred years from now, game theory will have become the backbone of a kind of micro-economic engineering that will have roughly the relation to the economic theory and laboratory experimentation of the time that chemical engineering has to chemical theory and bench chemistry. Game theory is, after all, the part of economic theory that focuses not merely on the strategic behaviour of individuals in economic environments, but also on other issues that will be critical in the design of economic institutions, such as how information is distributed (e.g. Harsanyi, 1967–68; Aumann, 1976), the influence of players' expectations and beliefs (e.g. Kreps and Wilson, 1982), and the tension between equilibrium and efficiency (e.g. Myerson and Satterthwaite, 1983). And game theory has already achieved important insights into issues such as the design of contracts and allocation mechanisms which take into account the sometimes counterintuitive ways in which individual incentives operate in environments having decision makers with different information and objectives.

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[1] E.g. Kalisch *et al.* (1954), Schelling (1957), Sauermann and Selten (1959) Shubik (1962), Maschler (1965).

However if we do *not* take steps in the direction of adding a solid empirical base to game theory, but instead continue to rely on game theory primarily for conceptual insights (deep and satisfying as these may be), then it is likely that long before a hundred years game theory will have experienced sharply diminishing returns. In this respect, I think the next hundred years will likely bring about a change in the way theoretical and empirical work are related in economics generally, and that, if not, then the entire discipline of economics may also fail to realise its potential.

The problem as I see it is that empirical work in economics has focused disproportionately on economically important questions. In case this does not seem like a heavy indictment, let me explain. While answering questions about important parts of the economy is a good thing for economists to try to do, it need not be the activity that best fosters the growth of theory, or fosters the growth of the best theory. And the relative neglect of empirical work directed primarily at testing and developing economic theory may therefore slow the growth of practical economic knowledge, since sound theory is of incalculable practical value.

Suppose, by analogy, that physical scientists had focused almost exclusively on important practical concerns like communication and illumination, to the detriment of more 'basic' science such as research on electricity and magnetism. We would likely have known much less today about radios and electric lights, which are not simply improvements on carrier pigeons and kerosene lamps. And, without the aid of experiments designed to elucidate basic phenomena far removed from immediate practical concerns, knowledge of electricity and magnetism would have accumulated much more slowly. Yet, in economics, 'basic science' is done disproportionately by theorists, who must rely for their empirical bearings on data collected for more immediately practical purposes.

However my optimism that in the future we will see more empirical work pointedly directed at theoretical issues is based on the fact that work of this sort has already begun to thrive. To illustrate what I mean, I will briefly mention some of the areas in which such work has been done. And then I will try my hand at the riskier part of prophecy, namely forecasting what will be some of the most productive avenues of work in the near and intermediate term.[2]

CONFRONTING THEORY WITH EVIDENCE IN THE LAB AND IN THE FIELD

(A) *Laboratory Studies*

Expected utility theory, as formulated by von Neumann and Morgenstern, was one of the first subjects in economics to attract the sustained attention of experimenters. From the very beginning this effort has both provided indications of the extent to which the predictions of the theory are approximate guides to individual choice behaviour (e.g. Mosteller and Nogee, 1951), and

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[2] Twenty-first century readers should note that the omission of many of the most productive avenues of research that emerged during the years following this article is due to the severe space limitations under which the prophet laboured.

identified particular situations in which a significant proportion of subjects consistently violate the predictions of the theory (e.g. Allais, 1953). And this experimental work has fed back into the theoretical literature, giving rise to new theories of individual choice and to experimental tests of those theories (e.g. Loomes and Sugden, 1987). At the same time, experimental techniques have been developed which allow theories stated in terms of individuals' expected utility to be examined under controlled conditions. For example, Becker *et al.* (1964) described an experimental procedure for eliciting reservation prices from utility maximisers.[3] Using procedures of this kind, experimental methods allow investigators to measure some of the parameters on which the predictions of a theory may depend, and which would be unobservable in non-experimental situations.

For example, the classical game theoretic models of bargaining which date from the work of Nash were unusually resistant to tests with field data because their predictions depend on difficult to observe elements of the bargainers' preferences. But laboratory experimentation presents the opportunity to measure or control these factors, and thus permits bargaining to be observed in environments for which the prediction of these theories can be known, and therefore tested. And when examined in this way, the evidence supports some of the qualitative predictions of these models, for example concerning the effect of risk aversion on the outcome of bargaining, while contradicting others, concerning, for example, what constitutes 'complete' information about a bargaining problem (see e.g. Roth, 1987). And a variety of unpredicted regularities have been brought to light and subsequently observed in a wide range of experimental environments. Some of these regularities have been the subject of vigorous investigation and productive exchange among experimenters with different intuitions about the way in which existing theory may need to be modified to account for them (see e.g. Guth *et al.*, 1982; Binmore *et al.*, 1985; Neelin *et al.*, 1988; and Ochs and Roth, 1989). Part of what allows this kind of exchange among experimenters to be so productive is that experimenters do not have to rely on one anothers' data, but can generate their own data from experimental environments well suited to testing their hypotheses precisely. And so series of experiments allow the experimental community to build upon and critique one anothers' work in ways that are not as readily available to economists using non-experimental methods.[4]

Experimental data can also provide insights into field data. A good example is the extended series of experiments that John Kagel and his colleagues have conducted on auction behaviour. Kagel's particular interest has been in a question that arose among oil companies involved in auctions for offshore oil

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[3] And as alternative theories of individual choice have been developed, it has been noted that this procedure may not give the correct incentives to non-expected utility maximisers, and that alternative experimental procedures for eliciting reservation prices may be desirable for testing the predictions of these theories (see e.g. Safra *et al.*, 1990).

[4] I think that bargaining experiments have been particularly productive in this respect, with investigators showing an exemplary willingness to address each others positions. This is not yet uniformly the case in all areas of experimental economics, and in the nearest term, experimenters will have to learn more about how to conduct and report experiments so as to most efficiently conduct productive dialogues.

rights. In the trade journals people began talking about a phenomenon that has since been called the 'winner's curse'. The idea is that the winning bidder in an auction frequently finds out that he has bid too much, once he discovers how much oil is recoverable from the plot he has won the right to drill on. Now, (since oil prices do not hold still, and wells do not produce until years after the bidding) it has proved hard to judge from field data whether this is a real phenomenon, or just the self-interested talk of oil companies trying to convince each other not to bid too competitively. So this is the kind of phenomenon that naturally lends itself to experimental investigation. Kagel and his colleagues have shown in a series of experiments that, with inexperienced bidders, there is a clear winner's curse' that this tends to go away as they accumulate experience; but that the learning that they exhibit does not help them very much in adjusting to new environments, such as a different number of bidders.[5] And by observing in experimental environments that public information about the value of the object being auctioned effects the bid price in opposite directions depending on whether the winner's curse is present, Kagel and Levin (1986) suggest new ways to test for the winner's curse in field data, by comparing rates of return for wildcat tracts (on which no drilling data are available) and drainage tracts (for which drilling data from adjacent tracts are available).

(B) Field Studies

Field studies, as opposed to laboratory studies, are what economists traditionally do, but the field studies I want to draw attention to here are non-traditional in the sense that the economic importance of the particular markets being studied plays rather less than its usual role in motivating them. Rather, a primary motivation is the opportunity to make observations that will help economists formulate and test important theory.

A good example of what I have in mind is the study by Ehrenberg and Bognanno (1990) of the performance of professional golfers at different stages of tournaments. Tournaments have been proposed as models of executive compensation and promotion by large corporations, where, for example, many vice presidents may compete for promotion to president. These models have implications about the incentives for working hard in environments in which the outcome is determined by chance as well as by effort. But studies of executive career paths and compensation offer little hope of testing these predictions, both because of the difficulty of gathering appropriate data, and because of the many non-tournament features of corporate employment. Ehrenberg and Bognanno proposed instead to test the theory of tournaments *per se* on a domain to which it clearly applied, and on which unambiguous data was available on incentives (the prize distribution in each tournament) and on output (players' scores). Controlling for player quality and course difficulty, they were therefore able to examine tournament incentives much more directly than would have been possible using labour market data.

[5] And these conclusions hold for construction industry executives as well as for student subjects (Dyer *et al.*, 1989).

Another set of field studies, in which I have been involved (in order to practice what I preach), is the study of various entry level labour markets. A large body of theory on two-sided matching markets has grown from Gale and Shapley's (1962) initial definition of *stability* for such markets, including a modern literature on the incentives and strategic choices facing agents in such markets (see Roth and Sotomayor, 1990). To initiate empirical tests of the theory, it has proved convenient to concentrate on markets which employ various kinds of centralised matching institutions, since in these markets the information about the 'rules of the game' required to test the theory is most readily available. For example, the market for new medical school graduates in the United States employs a centralised matching procedure which was developed in the early 1950s in response to a series of market failures in the decentralised markets that preceded it. In Roth (1984) it was shown that this centralised procedure yields stable outcomes. The performance of this procedure in the intervening years led to hypotheses about the role of stability in organising markets of this kind, and the role of instability in the earlier market failures and in recent difficulties caused by the growing number of two-doctor households in the market.

An opportunity to test these hypotheses arose in the United Kingdom, where similar centralised labour markets, inspired by similar market failures, were introduced in some regions of the National Health Service in the late 1960s and early 70s. Because different regions have used different procedures for organising the market, the United Kingdom presents a natural experiment that allows these procedures to be compared with each other. And because some of these centralised procedures have failed and been abandoned, whereas others have succeeded, this natural experiment also presents an opportunity to test the hypotheses about stability motivated by the U.S. market. (And the data supports the hypotheses, while suggesting some refinements. The stable market mechanisms – in Edinburgh and Cardiff – both perform comparably to the American market, while the mechanisms that have failed produced unstable outcomes (Roth, 1990 a, b)). And these hypotheses can be further tested on a different domain in the centralised 'markets' for new members run each year by sororities on American college campuses (Mongell and Roth, 1990).

My point about all these markets, from golf tournaments to physicians to sororities, is that their potential importance derives at least as much from the tests of theory they make possible as from their place in the world economy. And without direct tests of this sort, theorists are often forced to rely on indirect inferences from data which are ill suited for testing and refining theory, although they may concern very important parts of the economy.

SOME THOUGHTS ON THE NEAR AND INTERMEDIATE TERM

One of the most striking features of many of the experimental and field studies mentioned above is that the dynamics of economic processes when they are out of equilibrium appear to play a large role. (For example, agents had an incentive to circumvent the centralised matching procedures for new physicians

in Birmingham and Newcastle, in ways that magnified this incentive for those who continued to follow the official rules. And, after a few years of operation, these procedures collapsed under the weight of the accelerating number of circumventers (Roth, 1990 b).) So the development of useful theories of out-of-equilibrium adjustment seems likely to be a productive avenue of research. This is particularly so since, when multiple equilibria exist, out-of-equilibrium dynamics may play an important role in determining which one (if any) is reached, so that without a dynamic theory, current efforts at (static) equilibrium refinement may experience sharply diminishing returns.[6]

Another conclusion that is hard to escape after examining these experimental and field studies is that, even in situations designed or chosen to be particularly susceptible to game-theoretic analysis, it is hard to specify *precisely* what game is being played. In experiments this may be so because of uncontrolled aspects of the players' preferences or expectations, and in field studies it may be because no one knows the details of the game many moves off the equilibrium path. (For example, no one knows exactly what would happen if one year no graduating medical students sought employment in the Massachusetts General Hospital, one of the most prestigious in the United States. Since this has never happened, neither economists nor market participants can have any clear idea of the consequences if it should happen. Yet many kinds of game-theoretic analyses are sensitive to the modeller's specification of what would happen.) In general, when the rules of the game must be learned by observation, it may be impossible to know all of them, particularly when some formal rules turn out not to be binding while other, informal rules (e.g. social norms) may be decisive in some circumstances. So it will be productive to identify those aspects of strategic behaviour that are robust to changes in parts of the game that may not be observable. In this connection I anticipate that the distinction between 'cooperative' and 'non-cooperative' game theory will become much less important.[7]

TOWARDS A MICROECONOMIC ENGINEERING

In summary, I think the next step in the development of game theory as an integral part of economics, and a step we must take if game theory is to continue to thrive, is to bring to the fore the empirical questions associated with strategic environments. Accomplishing this will require some changes in the kinds of theory and empirical work we do, in order to regularly confront theory with evidence, and to use theory as a guide to what kinds of evidence we should collect.

[6] See Brandts and Holt (1990) for an experimental study that makes this point very forcefully. Some experimental studies of out of equilibrium dynamics which focus on coordination games are reported in Cooper *et al.* (1990), and Van Huyck *et al.* (1990), and some preliminary theoretical analyses of this process are contained in Crawford (1990) and Crawford and Haller (1990).

[7] A cynical observer might summarise the present situation by saying that the less detailed cooperative models, which try to represent a game without specifying all the rules, aspire to a spurious generality, while the non-cooperative, strategic models, which are analysed as if they represented *all* the potential moves in a game, offer a spurious specificity when the game in question is a model of some observable situation. It will be largely an empirical matter to determine which aspects of games need to be modelled in detail in order to confidently draw which kinds of conclusions.

I anticipate that experimental economics will play a growing role in this effort. There are many questions for which laboratory experimentation will be the most direct way to test theory, and to explore the effects of variables that are difficult to measure or control in any other way. This is not to say, of course, that experimentation in economics will come to play exactly the role it plays in any other science, or that there will not be many questions that are best addressed by field research, including new kinds of field research, which will pay particular attention to the details of economic environments, including both formal and informal 'rules of the game', and cultural and psychological constraints on individuals' actions.

In the long term, the real test of our success will be not merely how well we understand the general principles which govern economic interactions, but how well we can bring this knowledge to bear on practical questions of microeconomic engineering, to design appropriate mechanisms for price formation (as in different kinds of auction), dispute resolution, executive compensation, market organisation, etc. To do this we will need to learn more about the various kinds of frictions that enter economic environments as a function of size and complexity, about which properties of these environments are robust and which are fragile, and about which kinds of environments facilitate which kinds of learning.[8] Just as chemical engineers are called upon not merely to understand the principles which govern chemical plants, but to design them, and just as physicians aim not merely to understand the biological causes of disease, but its treatment and prevention, a measure of the success of microeconomics will be the extent to which it becomes the source of practical advice, solidly grounded in well tested theory, on designing the institutions through which we interact with one another.

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[8] And while field studies will be central to this effort, laboratory studies will likely play a role here as well. See e.g. Plott (1987) for a discussion of some experimental studies carried out with a view to giving guidance to policy makers.

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