History

Not so long ago, the scoffer could say that econometrics and game theory were like Japan and Argentina. In the late 1940s both disciplines and both economies were full of promise, poised for rapid growth, and ready to make a profound impact on the world. We all know what happened to the economies of Japan and Argentina. Of the disciplines, econometrics became an inseparable part of economics, while game theory languished as a subdiscipline, interesting to its specialists but ignored by the profession as a whole. The specialists in game theory were generally mathematicians, who cared about definitions and proofs rather than applying the methods to economic problems. Game theorists took pride in the diversity of disciplines to which their theory could be applied, but in none had it become indispensable.

In the 1970s, the analogy with Argentina broke down. At the same time that Argentina was inviting back Juan Peron, economists were beginning to discover what they could achieve by combining game theory with the structure of complex economic situations. Innovation in theory and application was especially useful for situations with asymmetric information and a temporal sequence of actions, the two major themes of this book. During the 1980s, game theory became dramatically more important to mainstream economics. Indeed, it seemed to be swallowing up microeconomics just as econometrics had swallowed up empirical economics.

Game theory is generally considered to have begun with the publication of von Neumann & Morgenstern's *The Theory of Games and Economic Behaviour* in 1944. Although very little of the game theory in that thick volume is relevant to the present book, it introduced the idea that conflict could be mathematically analyzed and provided the terminology with which to do it. The development of the "Prisoner's Dilemma" (Tucker [unpub]) and Nash's papers on the definition and existence of equilibrium (Nash [1950b, 1951]) laid the foundations for modern noncooperative game theory. At the same time, cooperative game theory reached important results in papers by Nash (1950a) and Shapley (1953b) on bargaining games and Gillies (1953) and Shapley (1953a) on the core.

By 1953 virtually all the game theory that was to be used by economists for the next 20 years had been developed. Until the mid-1970s, game theory remained an autonomous field with little relevance to mainstream economics, important exceptions being Schelling's 1960 book, *The Strategy of Conflict*, which introduced the focal point, and a series of papers (of which Debreu & Scarf [1963] is typical) that showed the relationship of the core of a game to the general equilibrium of an economy.

In the 1970s, information became the focus of many models as economists started to put emphasis on individuals who act rationally but with limited information. When attention was given to individual agents, the time ordering in which they carried out actions began to be explicitly incorporated. With this addition, games had enough structure to reach interesting and nonobvious results. Important "toolbox" references include the earlier but long unapplied articles of Selten (1965) (on perfectness) and Harsanyi (1967) (on incomplete information), the papers by Selten (1975) and Kreps & Wilson (1982) extending perfectness, and the article by Kreps, Milgrom, Roberts, & Wilson (1982) on incomplete information in repeated games. Most of the applications in the present book were developed after 1975, and the flow of research shows no sign of diminishing.

Game Theory's Method

Game theory has been successful in recent years because it fits so well into the new methodology of economics. In the past, macroeconomists started with broad behavioral relationships like the consumption function, and microeconomists often started with precise but irrational behavioral assumptions such as sales maximization. Now all economists start with primitive assumptions about the utility functions, production functions, and endowments of the actors in the models (to which must often be added the available information). The reason is that it is usually easier to judge whether primitive assumptions are sensible than to evaluate high-level assumptions about behavior. Having accepted the primitive assumptions, the modeller figures out what happens when the actors maximize their utility subject to the constraints imposed by their information, endowments, and production functions. This is exactly the paradigm of game theory: the modeller assigns payoff functions and strategy sets to his players and sees what happens when they pick strategies to maximize their payoffs. The approach is a combination of the "Maximization Subject to Constraints" of MIT and the "No Free Lunch" of Chicago. We shall see, however, that game theory relies only on the spirit of these two approaches: it has moved away from maximization by calculus, and inefficient allocations are common. The players act rationally, but the consequences are often bizarre, which makes application to a world of intelligent men and ludicrous outcomes appropriate.

Exemplifying Theory

Along with the trend towards primitive assumptions and maximizing behavior has been a trend towards simplicity. I called this "no-fat modelling" in the first edition, but the term "exemplifying theory" from Fisher (1989) is more apt. This has also been called "modelling by example" or "MIT-style theory." A more smoothly flowing name, but immodest in its double meaning, is "exemplary theory." The heart of the approach is to discover the simplest assumptions needed to generate an interesting conclusion – the starkest, barest model that has the desired result. This desired result is the answer to some relatively narrow question.

Could education be just a signal of ability? Why might bid-ask spreads exist? Is predatory pricing ever rational?

The modeller starts with a vague idea such as "People go to college to show they're smart." He then models the idea formally in a simple way. The idea might survive intact; it might be found formally meaningless; it might survive with qualifications; or its opposite might turn out to be true. The modeller then uses the model to come up with precise propositions, whose proofs may tell him still more about the idea. After the proofs, he goes back to thinking in words, trying to understand more than whether the proofs are mathematically correct.

Good theory of any kind uses Occam's razor, which cuts out superfluous explanations, and the *ceteris paribus* assumption, which restricts attention to one issue at a time. Exemplifying theory goes a step further by providing, in the theory, only a narrow answer to the question. As Fisher says, "Exemplifying theory does not tell us what *must* happen. Rather it tells us what *can* happen."

In the same vein, at Chicago I have heard the style called "Stories That Might Be True." This is not destructive criticism if the modeller is modest, since there are also a great many "Stories That Can't Be True," which are often used as the basis for decisions in business and government. Just as the modeller should feel he has done a good day's work if he has eliminated most outcomes as equilibria in his model, even if multiple equilibria remain, so he should feel useful if he has ruled out certain explanations for how the world works, even if multiple plausible models remain. The aim should be to come up with one or more stories that might apply to a particular situation and then try to sort out which story gives the best explanation. In this, economics combines the deductive reasoning of mathematics with the analogical reasoning of law.

A critic of the mathematical approach in biology has compared it to an hourglass (Slatkin [1980]). First, a broad and important problem is introduced. Second, it is reduced to a very special but tractable model that hopes to capture its essence. Finally, in the most perilous part of the process, the results are expanded to apply to the original problem. Exemplifying theory does the same thing.

The process is one of setting up "If-Then" statements, whether in words or symbols. To apply such statements, their premises and conclusions need to be verified, either by casual or careful empiricism. If the required assumptions seem contrived or the assumptions and implications contradict reality, the idea should be discarded. If "reality" is not immediately obvious and data is available, econometric tests may help show whether the model is valid. Predictions can be made about future events, but that is not usually the primary motivation: most of us are more interested in explaining and understanding than predicting.

The method just described is close to how, according to Lakatos (1976), mathematical theorems are developed. It contrasts sharply with the common view that the researcher starts with a hypothesis and proves or disproves it. Instead, the process of proof helps show how the hypothesis should be formulated.

An important part of exemplifying theory is what Kreps & Spence (1985) have called "blackboxing": treating unimportant subcomponents of a model in a cursory way. The game "Entry for Buyout" (Rasmusen [1988a]), for example, asks whether a new entrant would be bought out by the industry's incumbent producer, something that depends on duopoly pricing and bargaining. Both pricing and bargaining are complicated games in themselves, but if the modeller does not wish to deflect attention to those topics he can use the simple Nash and Cournot solutions to those games and go on to analyze buyout. If the focus of the model were duopoly pricing, then using the Cournot solution would be open to attack, but as a simplifying assumption, rather than one that "drives" the model, it is acceptable.

Despite the style's drive towards simplicity, a certain amount of formalism and mathematics is required to pin down the modeller's thoughts. Exemplifying theory treads a middle path between mathematical generality and nonmathematical vagueness. Advocates of both alternatives will complain that exemplifying theory is too narrow. But beware of calls for more "rich," "complex," or "textured" descriptions; these often lead to theory either too incoherent or too incomprehensible to be applied to real situations. Richness in a model tends to make it flabby.

Some readers will think that exemplifying theory uses too little mathematical technique, but others, especially noneconomists, will think it uses too much. Intelligent laymen have objected to the amount of mathematics in economics since at least the 1880s, when George Bernard Shaw said that as a boy he (1) let someone assume that $a = b$, (2) permitted several steps of algebra, and (3) found he had accepted a proof that $1 = 2$. Forever after, Shaw distrusted assumptions and algebra. Despite the effort to achieve simplicity (or perhaps because of it), mathematics is essential to exemplifying theory. The conclusions can be retranslated into words, but rarely can they be found by verbal reasoning. The economist Wicksteed put this nicely in his reply to Shaw's criticism:

> Mr Shaw arrived at the sapient conclusion that there "was a screw loose somewhere" – not in his own reasoning powers, but – "in the algebraic art"; and thenceforth renounced mathematical reasoning in favour of the literary method which enables a clever man to follow equally fallacious arguments to equally absurd conclusions *without seeing that they are absurd*. This is the exact difference between the mathematical and literary treatment of the pure theory of political economy. *(Wicksteed [1885] p. 732)*

In exemplifying theory, one can still rig a model to achieve a wide range of results, but it must be rigged by making strange primitive assumptions. Everyone familiar with the style knows that the place to look for the source of suspicious results is the description at the start of the model. If that description is not clear, the reader deduces that the model's counterintuitive results arise from bad assumptions concealed in poor writing. Clarity is therefore important, and the somewhat inelegant Players-Actions-Payoffs presentation used in this book is useful not only for helping the writer, but for persuading the reader.

This Book's Style

Substance and style are closely related. The difference between a good model and a bad one is not just whether the essence of the situation is captured, but also how much froth covers the essence. In this book, I have tried to make the games as simple as possible. They often, for example, allow each player a choice of only two actions. Our intuition works best with such models, and continuous actions are technically more troublesome. Other assumptions, such as zero production costs, rely on trained intuition. To the layman, the assumption that output is costless seems very strong, but a little experience with these models teaches that it is the constancy of the marginal cost that usually matters, not its level.

What matters more than what a model says is what we understand it to say. Just as an article written in Sanskrit is useless to me, so is one that is excessively mathematical or poorly written, no matter how rigorous it seems to the author. Such an article leaves me with some new belief about its subject, but that belief is not sharp, or precisely correct. Overprecision in sending a message creates imprecision when it is received, because precision is not clarity. The result of an attempt to be mathematically precise is sometimes to overwhelm the reader, in the same way that someone who requests the answer to a simple question in the discovery process of a lawsuit is overwhelmed when the other side responds with 70 boxes of tangentially related documents. The quality of the author's input should be judged not by some abstract standard but by the output in terms of reader processing cost and understanding.

In this spirit, I have tried to simplify the structure and notation of models while giving credit to their original authors, but I must ask pardon of anyone whose model has been oversimplified or distorted, or whose model I have inadvertently replicated without crediting them. In trying to be understandable, I have taken risks with respect to accuracy. My hope is that the impression left in the readers' minds will be more accurate than if a style more cautious and obscure had left them to devise their own errors. My major strength is in boiling down difficult models into simple games that still capture the essence of the models' ideas. My major weakness is in sliding past technical points, some unimportant, some important. I apologize in advance for the mistakes I am sure this book contains, but I hope that they are orthogonal to the mistakes of other books, and obvious enough not to lead the reader far astray.

Readers may be surprised to find occasional references to newspaper and magazine articles in this book. I hope these references will be reminders that models ought eventually to be applied to specific facts, and that a great many interesting situations are waiting for our analysis. The principal-agent problem is not found only in back issues of *Econometrica*: it can be found on the front page of today's *Wall Street Journal* if one knows what to look for.

I make the occasional joke here and there. Game theory is a subject intrinsically full of paradox and surprise. I want to emphasize, though, that I take game theory seriously, in the same way Chicago economists say that they take price theory seriously. It is not just an academic artform: people do choose actions deliberately and trade off one good against another, and game theory will help you understand how they do that. If it did not, I would not advise you to study such a difficult subject. There are much more elegant fields in mathematics from the aesthetic point of view. As it is, I think it is important that every educated person have some contact with the ideas in this book, just as they should have some contact with the basic principles of price theory.

I have been forced to exercise more discretion over definitions than I had hoped. Many concepts have been defined on an article-by-article basis in the literature, with no consistency and little attention to euphony or usefulness. Other concepts, such as "asymmetric information" and "incomplete information," have been considered so basic as to not need definition, and hence have been used in contradictory ways. I use existing terms whenever possible, and synonyms are listed.

I have often named the players Smith and Jones so that the reader's memory will be less taxed in remembering which is a player and which is a time period. I hope also to reinforce the idea that a model is a story made precise; we begin with Smith and Jones, even if we quickly descend to s and j. Keeping this in mind, the modeller is less likely to build mathematically correct models with absurd action sets, and his descriptions are more pleasant to read. In the same vein, labelling a curve "$U = 83$" sacrifices no generality: the phrase "$U = 83$ and $U = 66$" has virtually the same content as "$U = \alpha$ and $U = \beta$, where $\alpha > \beta$," but uses less short-term memory.

A danger of this approach is that readers may not appreciate the complexity of some of the material. While journal articles make the material seem harder than it is, this approach makes it seem easier (a statement that can be true even if readers find this book difficult). The better the author does his job, the worse this problem becomes. Keynes (1933) says of Alfred Marshall's *Principles*,

> The lack of emphasis and of strong light and shade, the sedulous rubbing away of rough edges and salients and projections, until what is most novel can appear as trite, allows the reader to pass too easily through. Like a duck leaving water, he can escape from this douche of ideas with scarce a wetting. The difficulties are concealed; the most ticklish problems are solved in footnotes; a pregnant and original judgement is dressed up as a platitude.

This book may well be subject to the same criticism, but I have tried to face up to difficult points, and the problems at the end of each chapter will help to avoid making the reader's progress too easy. Only a certain amount of understanding can be expected from a book, however. The efficient way to learn how to do research is to start doing it, not to read about it, and after reading this book, if not before, many readers will want to build their own models. My purpose here is to show them the big picture, to help them understand the models intuitively, and give them a feel for the modelling process.

Notes

- Perhaps the most important contribution of von Neumann & Morgenstern (1944) is the theory of expected utility (see section 2.3). Although they developed the theory because they needed it to find the equilibria of games, it is today heavily used in all branches of economics. In game theory proper, they contributed the framework to describe games, and the concept of mixed strategies (see section 3.1). A good historical discussion is Shubik (1992) in the Weintraub volume mentioned in the next note.
- A number of good books on the history of game theory have appeared in recent years. Norman Macrae's *John von Neumann* and Sylvia Nasar's *A Beautiful Mind* (on John Nash) are extraordinarily good biographies of founding fathers, while *Eminent Economists: Their Life Philosophies* and *Passion and Craft: Economists at Work*, edited by Michael Szenberg, and *Toward a History of Game Theory*, edited by Roy Weintraub, contain autobiographical essays by many scholars who use game theory, including Shubik, Riker, Dixit, Varian, and Myerson. Dimand & Dimand's *A History of Game Theory*, the first volume of which appeared in 1996, is a more intensive look at the intellectual history of the field. See also Myerson (1999).
- For articles from the history of mathematical economics, see the collection by Baumol & Goldfeld (1968), Dimand & Dimand's 1997 *The Foundations of Game Theory* in three volumes, and Kuhn (1997). Collections of more recent articles include Rasmusen (2001), Binmore & Dasgupta (1986), Diamond & Rothschild (1978) on information economics, Klemperer (2000) on auctions, and the immense Rubinstein (1990).
- On method, see the dialogue by Lakatos (1976), or Davis, Marchisotto, & Hersh (1981), chapter 6 of which is a shorter dialogue in the same style. Friedman (1953) is the classic essay on a different methodology: evaluating a model by testing its predictions. Kreps & Spence (1984) is a discussion of exemplifying theory.

- The mathematician Robert J. Kleinhenz is widely said to have likened proving a theorem to "seeing the peak of a mountain and trying to climb to the top. One establishes a base camp and begins scaling the mountain's sheer face, encountering obstacles at every turn, often retracing one's steps, and struggling every foot of the journey. Finally when the top is reached, one stands examining the peak, taking in the view of the surrounding countryside and then noting the automobile road up the other side!" (see e.g., http://users.characterlink.net/The-Cookie-Jar/math_jokes_10.html).

  In the spirit of Lakatos, I would agree except that usually halfway up you discover that what you thought was the peak is a mirage or a lower crest, so you have to change direction.
- Because style and substance are so closely linked, how one writes is important. For advice on writing, see McCloskey (1985, 1987) (on economics), Bowersock (1985) (on footnotes), Fowler (1931), Fowler & Fowler (1949), Halmos (1970) (on mathematical writing), Rasmusen (2000), Strunk & White (1959), Weiner (1984), and Wydick (1978).
- **A fallacious proof that 1 = 2.** Suppose that $a = b$. Then $ab = b^2$ and $ab - b^2 = a^2 - b^2$. Factoring the last equation gives us $b(a - b) = (a + b)(a - b)$, which can be simplified to $b = a + b$. But then, using our initial assumption, $b = 2b$ and $1 = 2$. The fallacy is division by zero.