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Game Theory and Operations Research

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GAME THEORY AND OPERATIONS RESEARCH: SOME MUSINGS 50 YEARS LATER

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By its very nature, a discursive reminiscence has to be somewhat self-referential. Furthermore, it cannot offer an exhaustive survey of the many special topics to which it may refer. Rather than suffer "strangulation by footnotes and references," for brevity and equity I do not provide references to subjects such as auctions, the new industrial organization, or experimental gaming because there are more than adequate references and survey articles available.

When I first skimmed *The Theory of Games and Economic Behavior* in 1948, I did not really understand it, but I sensed that this was the way to go in the study of multiperson conscious strategic behavior. I had heard a little about operational research casually in 1944 in two applications: one concerning how to aim an anti-aircraft gun to take account of the plane's motions during the time it took to reach it after firing, and I had a vague idea that one could try to analyze the best ways for convoy defense by using some form of mathematics.

The visit to the main library at the University of Toronto to look randomly at the new books in economics led to my going to Princeton to study game theory. To this day, I have been struck with the thought that it is possible to not know precisely what one is looking for, but recognize immediately when one finds it.

At Princeton, there was some direct talk about operations research per se, and only a few of us were aware of the newly formed Operations Research Society. But in a few years around Fine Hall (and elsewhere), much of the mathematics relevant to its development was being developed. Among the visitors, students, and faculty were Bellman, Feller, Gomory, Karlin, Kemeny, Kuhn, McCarthy, Mills, Minsky, Nash, Scarf, Shapley, Tucker, and Tukey. Dynamic programming, linear programming, convex programming, integer programming, inventory theory, game theory, artificial intelligence, and applied probability were all being developed.

While still at Princeton, and then when at the Institute for Advanced Study in the Behavioral Sciences and at General Electric in the period between 1953 and 1958, I was convinced that the methods of game theory were going to have a broad impact, not only on operations research but on the behavioral sciences in general. I suggested the applicability of game theory methods to operations research (Shubik 1953a, 1958a), economics (Shubik 1953b, 1953c), political science (Shubik 1954), management science (Shubik 1955), law (Shubik 1956), simulation (Shubik 1958a) and the decision sciences (Shubik 1958b).

A basic problem that has beset the publication of applications of operations research from its inception (in both the United States and the United Kingdom) has been the intermix between academic theorizing and practice. The introduction of the journal *Mathematics of Operations Research* provided a way to relieve *Operations Research* and *Management Science* of an overburden of the body of mathematical theory developing to deal with specific subdisciplines in operations research such as inventory theory, linear, and integer programming. The creation of a special Practice Section in *Operations Research* in 1984 attempted to make sure that there would be some segment of the journal devoted to the "real world." Michael Rothkopf (1994), in a valuable survey article, discussed some of the reasons why practitioners may not write for the open literature. Recently Omerod and Kiossis (1997) carried out a comparison of the nature of the publications in both United States and United Kingdom publications and concluded that there were possibly even fewer publications of results in the United Kingdom than in the United States.

Because my main purpose is to cover the evolution of the relationship between game theory and operations research rather than to review all of operations research, I limit my broader comments on operations research with a few Panglossian remarks. It is my belief that operations research has been so successful that it may have put itself out of business, at least in its easy-to-recognize sense. It has succeeded to the extent that it is taught in a more or less routine and watered-down manner in every business school. Linear programming, queuing studies, and elementary competitive models go with the turf.

Consulting firms flourish. A variety of military operations research firms make a good living off weapons analysis; specialist firms such as Fair Isaac have found a niche in credit evaluation; McKinsey dispenses generalized operations research and management science under a variety of names. RAND, Stanford Research Institute,

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Los Alamos, and many others may not be in their heyday—as they were when operations research was young and 100% improvement in performance almost anywhere was to be expected—but they still produce. Small groups of academic consultants provide consulting services in the design of auctions or in the structuring of games to study market structure.

Our "flagship journals" are academic journals, and the incentive structure for most practitioners to publish their findings in them is minimal. Validation in application is probably better measured by a repeat order from a customer than a publication in *Operations Research*. The goals of academics in operations research and the goals of practitioners are basically different and can be appreciated only when placed in the context of the organizations for which they work, their reward systems, and their life styles.

I am reminded of one event that happened to me at General Electric and another that happened to George Feeney at Stanford Research Institute. I was complaining to one of the vice-presidents that in spite of the fact that General Electric in the 1950s had hired a first-class group of operations researchers, the management, except for Harold Smiddy (Smiddy and Naum 1954 is still worth rereading), did not appreciate us. Jack McKitterick replied that the trouble with the executives at General Electric was that they had not understood the basic motivation of the group they had hired. He said if they had to do it over again they would have paid us half as much but would have hired a special manager to stroke us and go around telling each of us how smart we were. George Feeney's experience at Stanford Research involved explaining what operations research was to one of their vice-presidents, who reacted immediately. "I see," he said, "operations research involves utilizing big minds to work on small problems." Both of these observations may be regarded as flippant, but both contain just enough truth to be worth considering. Kirby (2000, p. 666), in a recent article, has noted that part of the crisis in OR from the 1970s to 1990s was that it had failed to capitalize on its wartime strategic profile. "To the extent that most peacetime work had been 'tactical' it had been consistent with a gradual slippage of status of OR groups in relation to senior management."

This article is being written some 14 years after an address given at The Institute of Management Sciences meeting in New Orleans in 1987 (Shubik 1988). I realize that even then my views of the relevance of operations research in general and game theory in particular had changed when compared to my advocacy in the 1950s. In the 1950s, especially in economics (less so in operations research), game theory was looked upon as a curiosum not to be taken seriously by any behavioral scientist. By the late 1980s, game theory in the new industrial organization had taken over. The floodgates had broken as streams of specialized papers poured out and subindustries in auction theory, agency theory, bargaining, the new industrial organization, voting theory, and competitive mechanism design proliferated.

The third millennium has arrived; game theory proved its successes in many disciplines. The big gains were made. There is much in the way of valuable special results still to be obtained. But much in the same way as one can regard the limited views and models of classical economics, and the one-person conscious optimization problems that characterized much of operations research as calling forth the development of game theory, the limitations of game theory have indicated that many of the big problems for which it was designed need to be answered in a manner that can best be described as "post-game theory."

The new game theory in operations research applications lies in the study of organizations and in systems that involve individuals, networks, and institutions. The success of game theory in supplying the language for the study of information and providing the basic concept of strategy has led to our understanding the limitations implicit in the model of the fully informed rational individual decision maker. The vistas opened up by the formalization of the concepts of player, information set, strategy space, and extensive form led us to gaming, simulation, and artificial intelligence. The stress will be on individuals with limited capacity, optimizing locally in many special contexts where expertise and learning count.

Economic man, operations research man, and the game theory player were all gross simplifications invented for conceptual simplicity and computational convenience in models loaded with implicit or explicit assumptions of symmetry, continuity, and fungibility to allow us (especially in a precomputer world) to utilize the methods of calculus and analysis. Reality was placed on its bed of Procrustes to enable us to utilize the mathematical techniques available. Fortunately, there were many important problems in military OR and mass economies that fitted comfortably into this picture. Cooperative game theory utilizes combinatorics, but once one considers games with more than 5 or 10 differentiated players, the calculations involving all coalitions quickly become unwieldy.

After my stay at the Institute for Advanced Study in the Behavioral Sciences, rather than return to academia I decided to join General Electric, where an operations research group was being formed explicitly to provide internal operations research consulting. There were active operations researchers at Columbia, among them David Hertz and Sebastian Littauer, and in Princeton, the group at Mathematica, where game theory thinking was considerably in the fore.

It was my experience as a consultant at General Electric that led me to reformulate my thoughts along the lines indicated in my 1984 talk and paper. The key questions seemed to me to be: "What is an application?" and "When is theory a waste of time?" To some extent, I felt that the distinction between management science and operations research was at best fuzzy, but if there was one it was one of emphasis and professional view. The management scientist had to be more "management friendly." This boiled down to paying considerable attention to context and to some extent, handholding. What were the real problems faced by the manager, and which were the big problems? Furthermore, did the management recognize the importance of the problems in the same way as the consultant, or was the consultant merely a Cassandra who might take some pride at a later date in his ability to predict disaster without any ability to prevent it?

The operations researcher appeared to be more concerned with applying the appropriate solution techniques to pre-posed problems with little regard to becoming involved in the judgment of how important they were. In the OR applications in World War II, this division of labor was clearly not there. The great power of the military operations research at that time (as is shown in Morse and Kimball 1970, Kirby 2000, Blackett 1962, and others) was that the operations researchers were closely concerned with formulating the problems and evaluating how important they were. Similar observations, combined with questions concerning the obtaining of data within an organization, have been raised in the discussion of the CONDOR report (Committee on the Next Decade in Operations Research). The discussion of Wagner et al. (1989) notes the change in the interface between OR practitioners and the organizations. In "the old time operations research," problem formulation, understanding how good the data were, and where the data came from were critical responsibilities of the practitioner in the formulation and clarification of ill-perceived problems.

As a game theorist with a special interest in the economics of oligopoly, I sailed into General Electric with a whole tool kit of strategic interaction models inherited from a peculiar set of Jesuitical exercises much enjoyed by young mathematical economists teething on conjectural variations. There were Cournot reaction dynamics, Bertrand dynamics, Stackelberg variations, Chamberlin embellishments, and then all the embroidery one could add if one had been taught enough about difference and differential equations. But they were all cut out of the whole cloth, and their connection to economic reality was tenuous at best. Possibly one of my best teachers and friends was Harlan Mills, who had a wonderful feeling for the appropriate abstraction of reality. It was from Harlan that I heard the golf expression, "Drive for show, but putt for dough." Translated into English, this was a way of saying that details mattered and the ability to understand the precise aspects of the problem at hand was critical to its solution.

General Electric had switched to five-year planning and as a gung-ho young game theorist, I could see no reason why not 10 or 15 years. It took me many moons to finally understand that long-range planning takes place now and that there is a constant updating as information comes in. Furthermore, as new information comes in there is a considerable feedback between the information and the updating of the plans. Even more important, because the contingencies proliferate so fast as we go out in time, there have to be methods for pruning the branches to be followed. Few long-range plans can afford the luxury of working out more than a few alternative paths. Once even the most staid of firms goes out more than a few years into the future, the planning becomes more of an exercise that provides a broad definition of intentions and moral imperatives than an exercise in operations research.

I enjoyed my years at General Electric, and the exercise of looking carefully at many different plants in a variety of industries vastly increased my respect for those who knew their business without necessarily knowing any of what passed for theory. From there I went to the IBM research laboratories where a fine group of mathematicians and operations researchers were located. At IBM, several items concerning both my perceptions of the role of long-range planning and competitive analysis, and about my own professional interests and desires, became clear. My colleagues were a pleasure to be with, but unlike at General Electric, we were no longer mixed in with the "troops in the front line." We were neither managers nor consultants. I knew I did not want to be a manager and possibly did not have the appropriate talents to be one even if I had wanted to go that way. I started to work on a purely abstract problem that was of great fascination to me. This was on the development of a basic theory of money. It appeared to me that there was a great gap in economic thought on the treatment of money and that, although I did not know how, I suspected the techniques of game theory could be applied. While at IBM, I made no visible progress on this topic.

My other interests at IBM were in experimental, teaching, and operational gaming and in developing a theory of bidding. It seemed to me that gaming provided a useful way to teach the insights of game theory and to provide a way to organize concepts and data on the structure of markets. I believe to this day that eventually a good planning department of any major corporation should utilize a simulation of the corporation and its market as a device to organize perceptions and data, to help formulate questions, and to facilitate communication among practitioners.

Bidding and auctions appeared to several of us in the early 1960s as a natural subject for the application of game theory techniques. Since that time, the literature has proliferated, and a case can be made that it has been of value in OR application.

My decision to accept an attractive offer to re-enter academia was based on two features. I believed that my comparative advantage was in seeking out fundamental problems and chasing basic conjectures, hopefully encouraging others to develop the mathematics required to establish the conjectures or to exploit the models. This occupation does not have an immediate corporate or military payoff. Furthermore, the environment at Yale offered far greater freedom than either IBM or GE for my purposes. In making the move, I followed my own precepts and observations concerning the uses of long-range planning and game theory—good major decision-making calls for the understanding of environment (both physical and human). At best we find ourselves facing a partially controlled stochastic process where nature or plain luck accounts for a large part of the variation and other individuals account for a segment that may or may not be as large as that of nature. The basic problem faced by the individual or corporation is: does he, she, or it have a clear picture of what he, she, or it wants to do locally in the next few years? Individuals frequently complain that they never experience good luck. One hypothesis is that they are unable to recognize good fortune even when it stares them in the face.

I came to Yale permanently in 1963 and am still here. I am more and more appreciative of the amazing contribution of game theory to the behavioral sciences in general and to OR in particular. But I am more sensitized to understanding that A's big problem may be B's small problem and furthermore that B's big problem may be regarded by A as a quaint academic irrelevance. So it is with game theory and its applications. A brief look at the last 10 years of *Operations Research* and *Mathematics of Operations Research* shows a little more than 20 articles that can be classified as game theory in operations research. They are in search theory, some military OR, and the rest in the new industrial economics and some on joint costs, assignment, and bargaining. During the same period, there were around 40 game theory papers in *Mathematics of Operations Research*, essentially all of which were technical and for the theorists. But before we draw conclusions, we need to ask what we care about and why.

Some years ago, it was suggested that game theory could be categorized in at least three forms. They are:

High church game theory. (Publications in *Mathematics of Operations Research* fit here.)

Low church game theory. (The new industrial organization qualifies here.)

Conversational game theory. (This deals loosely with preformalized vague but strategic problems.)

All three have had impact. But it is the third that has made its way into the language of every consultant and has caught the imagination of the public. Von Neumann and Morgenstern did not invent cooperative game theory for consulting purposes. The two key solution concepts, the Shapley value and the Nash noncooperative equilibrium, were not constructed to solve any specific problem at hand. (To this day, the importance of the Shapley value is grossly underestimated. It is the natural combinatoric extension of the concept of marginal productivity.) At the time I, not being a mathematician but being concerned with applications in the social sciences, saw their potentials and had the good fortune to work with both Shapley and Nash in applying these solutions to the concept of voting power and to the economics of oligopolistic competition. A little later, I observed the possibility of applying the value to the assignment of joint costs and to accounting incentive problems.

When consulting at RAND, I read the notes of Tom Schelling, which led to his book on the strategy of conflict. I was deeply opposed to it at the time because it was (and still is) loaded with basic errors and a misunderstanding of elementary game theory. But what I failed to appreciate at that time was that it was the work of a social scientist willing to take the mindset of game theory seriously but not willing to accept the rules of the game as given. It is precisely the concern for context and the "games within the game," where the fuzzy meld of strategic—as well as tactical—modeling took place in "the old operations research." Strategic analysis in application has no neat and tidy rules to turn over to the boys writing the algorithms to solve everything.

While at RAND, Brewer and I, in collaboration with the General Accounting Office (Brewer and Shubik 1979), did a survey of military gaming and simulation. We had the resources to conduct a broad empirical study that enabled me later to more fully appreciate Roth's comment that one of the basic purposes of gaming is "whispering in the ears of princes." A good study is part of the conversation in activities such as weapons selection and evaluation and force structure. Specious accuracy and tidy database presentations are part of the Noh-play scenario developed in this use of conversational game theory dressed as high science engineering. My advocacy and predictions for the use of gaming and game theory in corporate planning groups (Shubik 1975) have not been fulfilled, in part because I grossly misestimated the organizational incentives, modeling, simulation, and data gathering problems.

At this time, certain forms of noncooperative game theory have captured many of the applications in the design of auctions and bidding, in industrial organization, in agency theory and the design of incentive systems, and in parts of the law. There has been a proliferation of experimental gaming in economics and industrial organization; operational gaming is still carried on by the military (but there is a danger that the improvements in computer technology have permitted war gaming models to become *more rather than less opaque*. In spite of the rumors of its demise, or a lack of public knowledge of its existence, cooperative game theory and its many applications is thriving. These applications include voting structure design, assignment problems such as medical school admissions, overhead costs, the pricing of pollution rights, aircraft landing pricing, and computer system time-sharing pricing.

The 1950s–1970s were the halcyon days in the development of operations research techniques in general and game theory in particular. They were also the days when the RAND Corporation, other not-for-profits, ONR, Bell Labs, the General Electric operations research group, and the T. J. Watson labs were in their prime. Today, the scene has changed. The universities have had to shoulder much more of the burden. Not only have the institutions changed, but so have the subjects for study.

The successes of game theory brought with them a deeper appreciation of the limits of the key models. A few straightforward combinatoric calculations concerning the proliferation of strategy sets tell us that individuals do not search strategy sets exhaustively; a little experience in long-range planning lets us know that 10 strategies is a big number—better stick to three or four, but show some taste in selection and construction.

The current questions in post or new game theory involve how we describe situations where there is not complete common knowledge about the rules of the game, where individuals have limited perceptual and computational capacities, where they do not know quite what they want, where their priors are not well defined, where they learn, where socio-psychological and even cultural factors matter—in short, where they are somewhat fleshed out human beings rather than computational powerful automata. The development of computer simulation techniques and the concern for dynamics and evolution are moving us beyond the basic tools that provided us with the language to understand the anatomy of information and much of the structure of decision making. The new technology of computation and communication has enabled us to see more clearly the importance of interlinked networks of individuals in contrast with the stress on the maximizing individual in isolation. Increasing concern with the rates of change in technology and the economy have stressed the need to study viability, flexibility, and feedback in locally optimizing organisms. New institutions are called for to succeed those of the 1950s and 1960s. It is not clear that institutions at the cutting edge of research can stay in their prime for more than 20 or 30 years. Currently, the Santa Fe Institute is attempting to move into new territory. How it will succeed and what others will join it are still open questions. But the time to move on is now.

Most of the individuals writing in this issue have much of their careers behind them. But many of them, though rich in experience, are still young in perception and thought. The purpose of musing and summary is not to bury a subject but to understand what has been learned and how to utilize the body of knowledge and understanding to start to explore the vistas it has opened up.

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