CONTENTS

CHAPTER III
ZERO-SUM TWO-PERSON GAMES: THEORY

CHAPTER IV

ZERO-SUM TWO-PERSON GAMES: EXAMPLES

CHAPTER VII

ZERO-SUM FOUR-PERSON GAMES

CHAPTER X
SIMPLE GAMES

CHAPTER XI
GENERAL NON-ZERO-SUM GAMES

Introduction

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Although John von Neumann was without doubt "the father of game theory," the birth took place after a number of miscarriages. From an isolated and amazing minimax solution of a zero-sum two-person game in 1713 [1] to sporadic considerations by E. Zermelo [2], E. Borel [3], and H. Steinhaus [4], nothing matches the path-breaking paper of von Neumann, published in 1928 [5].

This paper, elegant though it is, might have remained a footnote to the history of mathematics were it not for collaboration of von Neumann with Oskar Morgenstern in the early '40s. Their joint efforts led to the publication by the Princeton University Press (with a $4,000 subvention from a source that has been variously identified as being the Carnegie Foundation or the Institute for Advanced Study) of the 616-page *Theory of Games and Economic Behavior* (TGEB).

I will not discuss here the relative contributions of the two authors of this work. Oskar Morgenstern has written his own account [6] of their collaboration, which is reprinted in this volume; I would recommend to the reader the scholarly piece [7] by Robert J. Leonard, who has noted that Morgenstern's "reminiscence sacrifices some of the historical complexity of the run-up to 1944" and has given a superb and historically complete account of the two authors' activities in the relevant period. On balance, I agree with Leonard that "had von Neumann and Morgenstern never met, it seems unlikely that game theory would have been developed." If von Neumann played both father and mother to the theory in an extraordinary act of parthenogenesis, then Morgenstern was the midwife.

In writing this introduction, I have several goals in mind. First, I would like to give the reader a sense of the initial reaction to the publication of this radically new approach to economic theory. Then, we shall survey the subsequent development of the theory of games, attempting to explain the apparent dissonance between the tenor of the book reviews and the response by the communities of economists and mathematicians. As a participant in this response (from the summer of 1948), my account is necessarily colored by subjective and selective recollections; this is a fair warning to the reader.

The book reviews that greeted the publication of TGEB were extraordinary, both in quantity and quality; any author would kill for such reviews. Consider the following partial list of the reviews, paying special attention to

the length of these reviews, the quality of the journals, and the prominence of the reviewers:

H. A. Simon, *American Journal of Sociology* (1945) 3 pages*
A. H. Copeland, *Bulletin of the American Mathematical Society* (1945) 7 pages*
L. Hurwicz, *The American Economic Review* (1945) 17 pages*
J. Marschak, *Journal of Political Economy* (1946) 18 pages
T. Barna, *Economica* (1946) 3 pages*
C. Kaysen, *Review of Economic Studies* (1946) 15 pages
D. Hawkins, *Philosophy of Science* (1946) 7 pages
J.R.N. Stone, *Economic Journal* (1948) 16 pages
E. Ruist, *Economisk Tidskrift* (1948) 5 pages
G. Th. Guilbaud, *Economie Appliquée* (1949) 45 pages
E. Justman, *Revue d'Economie Politique* (1949) 18 pages
K. G. Chacko, *Indian Journal of Economics* (1950) 17 pages

The quotes from these reviews are a publisher's dream. Thus:

Simon encouraged "every social scientist who is convinced of the necessity for mathematizing social theory—as well as those unconverted souls who are still open to persuasion on this point—to undertake the task of mastering the Theory of Games."

Copeland asserted: "Posterity may regard this book as one of the major scientific achievements of the first half of the twentieth century."

Hurwicz signaled that "the techniques applied by the authors in tackling economic problems are of sufficient generality to be valid in political science, sociology, or even military strategy" and concluded "the appearance of a book of the caliber of the *Theory of Games* is indeed a rare event."

After praising the "careful and rigorous spirit of the book," Jacob Marschak concludes: "Ten more such books and the progress of economics is assured."

If the quantity of reviews and the quality of the journals in which they were published are impressive, the choice of reviewers and their positions in the social sciences are equally impressive. Two of the reviewers, H. A. Simon and J.R.N. Stone, were awarded Nobel Memorial Prizes in Economics.

The first review to appear was that of Herbert Simon. By his own account [8], he "spent most of [his] 1944 Christmas vacation (days and some nights) reading [the TGEB]." Simon knew of von Neumann's earlier work and was concerned that the TGEB might anticipate results in a book that he was preparing for publication.

*Starred reviews are included in the book.

The first review that was directed at mathematicians was that of A. H. Copeland, a specialist in probability theory and professor at the University of Michigan. Copeland's only significant work in social science is the so-called "Copeland method" for resolving voting problems: simply, it scores 1 for each pairwise win and −1 for each pairwise loss, and declares the alternative with the highest score the winner. His review gave the mathematical community an extremely complete account of the contents of the TGEB. As is typical of almost all of the reviewers, although Copeland pointed to the research challenges opened by the TGEB, he never engaged in research in game theory as such. The only paper in his prolific output that is marginally related to game theory is a joint paper on a one-player game which must be categorized as a game of chance. Copeland's principal contribution to game theory consists in the fact that he was Howard Raiffa's thesis adviser; the book *Games and Decisions*, written by Raiffa with R. Duncan Luce (published by Wiley in 1957 and reprinted by Dover Publications in 1989) was the first nonmathematical exposition that made the theory of games accessible to the broad community of social scientists.

Another reviewer, David Hawkins, is permanently linked to H. A. Simon for their joint discovery of the "Hawkins-Simon conditions," a result that every graduate student in economics must study. Hawkins was a young instructor at the University of California at Berkeley when his friend, J. Robert Oppenheimer, picked him as the "official historian" and "liaison to the military" at Los Alamos, where the first atomic bomb was produced. Hawkins later had a distinguished career at the University of Colorado, where he was chosen in the first class of MacArthur "genius" scholars in 1986. Hawkins did no research in game theory.

The pattern of extravagant praise and no subsequent research is repeated with more significance in the cases of Jacob Marschak and Leonid Hurwicz. Marschak was head of the Cowles Commission at the University of Chicago when he reviewed the TGEB. He had survived a tumultuous early life that took him from Russia, where he was raised, to Berlin, where he trained as an economist, to the United States, where he ran an influential econometric seminar at the New School for Social Research. Leonid Hurwicz preceded Marschak on the staff of the Cowles Commission and continued as a consultant after Tjalling C. Koopmans became director and the commission moved from the University of Chicago to Yale University. Both Marschak and Hurwicz were in a position to influence the research done at the Cowles Commission, but it is an astounding fact that the extensive research output of the commission did not encompass game theory until Martin Shubik joined the Yale faculty in 1963. Eight years after reviewing the TGEB, Hurwicz posed the

question: What has happened to the theory of games? His answer [9], published in *The American Economic Review*, contains conclusions that are echoed in this introduction.

Among the reviews and reviewers, the review of G. Th. Guilbaud is surely unique. Occupying 45 pages in the journal, *Economie Appliquée*, it contained not only an account of the main themes of the TGEB, but also went further into consideration of the difficulties that the theory then faced. Guilbaud himself was unique in that he was the only reviewer who has contributed to the theory; his book *Eléments de la Theorie des Jeux* was published by Dunod in Paris in 1968. However, he failed to convince the economic community in France to join him. Guilbaud's seminar in Paris in 1950–51 was attended by such mathematical economists as Allais, Malinvaud, Boiteux, and myself, but none of the French engaged in research in game theory. I am pleased to report that Guilbaud, a very private person, is still with us at 91 years of age, living in St. Germaine-en-Laye. It was he who discovered the minimax solution of 1713 [1], when he purchased the treatise on probability written by Montmort from one of the booksellers whose stalls line the river Seine in Paris.

Given the extravagant praise of these reviewers, one might have expected a flood of research. If nowhere else, surely the Princeton economics department should have been a hotbed of activity. When Martin Shubik arrived in Princeton to do graduate work in economics in the fall of 1949, he expected to find just that. Instead, he found Professor Morgenstern in splendid isolation from the rest of the department, teaching a seminar with four students in attendance [10]. Morgenstern's research project consisted of himself assisted by Maurice Peston, Tom Whitin, and Ed Zabel, who concentrated on areas of operations research such as inventory theory, but did not work on game theory as such. If Shubik had come two years earlier, he would have found the situation in the mathematics department somewhat similar. Samuel Karlin (who received his Ph.D. at Princeton in mathematics in the spring of 1947 then took a faculty position at Cal Tech, and almost immediately started to consult at the RAND Corporation under the tutelage of Frederic Bohnenblust) has written that he never heard game theory mentioned during his graduate studies.

Nevertheless, many observers agree that in the following decade Princeton was one of the two centers in which game theory flourished, the other being the RAND Corporation in Santa Monica. The story of the RAND Corporation and its research sponsored by the Air Force has been told on several occasions (see [11], [12]). We shall concentrate on the activity in the mathematics department at Princeton, a story that illustrates the strong element of chance in human affairs.

The story starts with two visits by George Dantzig to visit John von Neumann in the fall of 1947 and the spring of 1948. In the first visit Dantzig described his new theory of "linear programming" only to be told dismissively by von Neumann that he had encountered similar problems in his study of zero-sum two-person games. In his second visit, Dantzig proposed an academic project to study the relationship between these two fields and asked von Neumann's advice about universities in which such a project might be pursued. Dantzig was driven to the train station for his trip back to Washington by A. W. Tucker (a topologist who was associate chairman of the mathematics department at that time). On the ride, Dantzig gave a quick exposition of his new discoveries, using the Transportation Problem [13] as a lively example. This recalled to Tucker his earlier work on electrical networks and Kirkhoff's Law and planted the idea that the project to study the relationship between linear programming and the theory of games might be established in the mathematics department at Princeton University.

In those halcyon days of no red tape, before a month had elapsed Tucker hired two graduate students, David Gale and myself, and the project was set up through Solomon Lefshetz's project on non-linear differential equations until a formal structure could be established through the Office of Naval Research's Logistics Branch. And so, in the summer of 1948, Gale, Kuhn, and Tucker taught each other the elements of game theory.

How did we do this? We divided up the chapters of the Bible, the TGEB, as handed down by von Neumann and Morgenstern, and lectured to each other in one of the seminar rooms of the old Fine Hall, then the home of the mathematics department at Princeton. By the end of the summer, we had established that, mathematically, linear programming and the theory of zero-sum two-person games are equivalent.

Enthused by the research potential of the subject we had just learned, we wanted to spread the gospel. We initiated a weekly seminar in the department centered on the subjects of game theory and linear programming. To understand the importance of this development, one must contrast the situations today and then. Today, the seminar lists of the university and the Institute of Advanced Studies contain over twenty weekly seminars in subjects such as number theory, topology, analysis, and statistical mechanics. In 1948, there was a weekly colloquium that met alternate weeks at the university and the institute. The topologists and statisticians had weekly seminars and my thesis advisor, Ralph Fox, ran a weekly seminar on knot theory; but that was that. So the addition of a new seminar was an event that raised the visibility of game theory considerably among the graduate students in the department and among the visitors to the institute.

The speakers included von Neumann and Morgenstern, visitors to the institute such as Irving Kaplansky, Ky Fan, and David Bourgin, as well as outside visitors such as Abraham Wald, the Columbia statistician who had made significant connections between game theory and statistical inference. (Wald had done the review of the TGEB for *Mathematical Reviews* and had tutored Morgenstern in mathematics in Vienna.)

More importantly it provided a forum for graduate students in mathematics who were working in this area to present new ideas. As Shubik has reminisced: "The general attitude around Fine Hall was that no one cared who you were or what part of mathematics you worked on as long as you could find some senior member of the faculty and make a case to him that it was interesting and that you did it well. . . . To me the striking thing at that time was not that the mathematics department welcomed game theory with open arms—but that it was open to new ideas and new talent from any source, and that it could convey a sense of challenge and a belief that much new and worthwhile was happening." He did not find that attitude in the economics department.

A crucial fact was that von Neumann's theory was too mathematical for the economists. To illustrate the attitude of a typical economics department of the period and later, more than fifteen years after the publication of TGEB the economists at Princeton voted against instituting a mathematics requirement for undergraduate majors, choosing to run two tracks for students, one which used the calculus and one which avoided it. Richard Lester, who alternated with Lester Chandler as chairman of the department, had carried on a running debate with Fritz Machlup over the validity of marginal product (a calculus notion) as a determinant of wages. Courses that used mathematical terms and which covered mathematical topics such as linear programming were concealed by titles such as "Managerial theory of the firm." Given such prevailing views, there was no incentive or opportunity for graduate students and junior faculty to study the theory of games.

As a consequence, the theory of games was developed almost exclusively by mathematicians in this period. To describe the spirit of the time as seen by another outside observer, we shall paraphrase a section of Robert J. Aumann's magnificent article on game theory from *The New Palgrave Dictionary of Economics* [14].

The period of the late '40s and early '50s was a period of excitement in game theory. The discipline had broken out of its cocoon and was testing its wings. Giants walked the earth. At Princeton, John Nash laid the groundwork for the general non-cooperative theory and for cooperative bargaining theory. Lloyd Shapley defined a value for coalitional games, initiated the theory of stochastic games, coinvented the core with D. B. Gillies, and together with John Milnor developed the first game

models with an infinite number of players. Harold Kuhn reformulated the extensive form and introduced the concepts of behavior strategies and perfect recall. A. W. Tucker invented the story of the Prisoner's Dilemma, which has entered popular culture as a crucial example of the interplay between competition and cooperation.

It is important to recognize that the results that Aumann enumerated did not respond to some suggestion of von Neumann; rather they were new ideas that ran counter to von Neumann's preferred version of the theory. In almost every instance, it was a repair of some inadequacy of the theory as presented in the TGEB. Indeed, von Neumann and Morgenstern criticized Nash's non-cooperative theory on a number of occasions. In the case of the extensive form, the book contains the claim that it was impossible to give a useful geometric formulation. Thus, game theory was very much a work in progress, in spite of von Neumann's opinion that the book contained a rather complete theory. Through the efforts at RAND and at Princeton University, many new directions of research had been opened and the way had been paved for the applications to come.

The TGEB was published with unparalleled accolades from the cream of the mathematical economists of the era, then ignored by the economists while mathematicians at the RAND Corporation and at Princeton quietly pushed the boundaries of the subject into new territory. It took nearly a quarter century before reality overcame the stereotypical view that it was merely a theory of zero-sum two-person games and that its usefulness was restricted to military problems. Once these myths were countered, applications came tumbling out and, by the time the Nobel Memorial Prize in Economics was awarded in 1994 to Nash, John Harsanyi, and Reinhard Selten, the theory of games had assumed a central position in academic economic theory. If Oskar Morgenstern had been alive in 1994, he would surely have said, "I told you so!"

In opening this new edition of the TGEB, you are given the opportunity to read for yourselves the revision of the economic theory that it contains and to decide whether it is "one of the major scientific achievements of the twentieth century." Although the subject has enjoyed a spectacular expansion in the sixty years since its publication, everything that followed is based on the foundation laid by von Neumann and Morgenstern in this book.

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Index