Introduction

HAROLD W. KUHN

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.

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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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THEORY OF GAMES AND ECONOMIC BEHAVIOR

By JOHN VON NEUMANN, and

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CONTENTS

CHAPTER III
ZERO-SUM TWO-PERSON GAMES: THEORY

CHAPTER VII

ZERO-SUM FOUR-PERSON GAMES

CHAPTER XI
GENERAL NON-ZERO-SUM GAMES

PREFACE TO FIRST EDITION

This book contains an exposition and various applications of a mathematical theory of games. The theory has been developed by one of us since 1928 and is now published for the first time in its entirety. The applications are of two kinds: On the one hand to games in the proper sense, on the other hand to economic and sociological problems which, as we hope to show, are best approached from this direction.

The applications which we shall make to games serve at least as much to corroborate the theory as to investigate these games. The nature of this reciprocal relationship will become clear as the investigation proceeds. Our major interest is, of course, in the economic and sociological direction. Here we can approach only the simplest questions. However, these questions are of a fundamental character. Furthermore, our aim is primarily to show that there is a rigorous approach to these subjects, involving, as they do, questions of parallel or opposite interest, perfect or imperfect information, free rational decision or chance influences.

John von Neumann
Oskar Morgenstern.

Princeton, N. J.
January, 1943.

PREFACE TO SECOND EDITION

The second edition differs from the first in some minor respects only. We have carried out as complete an elimination of misprints as possible, and wish to thank several readers who have helped us in that respect. We have added an Appendix containing an axiomatic derivation of numerical utility. This subject was discussed in considerable detail, but in the main qualitatively, in Section 3. A publication of this proof in a periodical was promised in the first edition, but we found it more convenient to add it as an Appendix. Various Appendices on applications to the theory of location of industries and on questions of the four and five person games were also planned, but had to be abandoned because of the pressure of other work.

Since publication of the first edition several papers dealing with the subject matter of this book have appeared.

The attention of the mathematically interested reader may be drawn to the following: *A. Wald* developed a new theory of the foundations of statistical estimation which is closely related to, and draws on, the theory of the zero-sum two-person game ("Statistical Decision Functions Which Minimize the Maximum Risk," Annals of Mathematics, Vol. 46 (1945) pp. 265–280). He also extended the main theorem of the zero-sum two-person games (cf. 17.6.) to certain continuous-infinite-cases, ("Generalization of a Theorem by von Neumann Concerning Zero-Sum Two-Person Games," Annals of Mathematics, Vol. 46 (1945), pp. 281–286.) A new, very simple and elementary proof of this theorem (which covers also the more general theorem referred to in footnote 1 on page 154) was given by *L. H. Loomis*, ("On a Theorem of von Neumann," Proc. Nat. Acad., Vol. 32 (1946) pp. 213–215). Further, interesting results concerning the role of pure and of mixed strategies in the zero-sum two-person game were obtained by *I. Kaplanski*, ("A Contribution to von Neumann's Theory of Games," Annals of Mathematics, Vol. 46 (1945), pp. 474–479). We also intend to come back to various mathematical aspects of this problem. The group theoretical problem mentioned in footnote 1 on page 258 was solved by *C. Chevalley*.

The economically interested reader may find an easier approach to the problems of this book in the expositions of *L. Hurwicz*, ("The Theory of Economic Behavior," American Economic Review, Vol. 35 (1945), pp. 909–925) and of *J. Marschak* ("Neumann's and Morgenstern's New Approach to Static Economics," Journal of Political Economy, Vol. 54, (1946), pp. 97–115).

John von Neumann
Oskar Morgenstern

Princeton, N. J.
September, 1946.

PREFACE TO THIRD EDITION

The Third Edition differs from the Second Edition only in the elimination of such further misprints as have come to our attention in the meantime, and we wish to thank several readers who have helped us in that respect.

Since the publication of the Second Edition, the literature on this subject has increased very considerably. A complete bibliography at this writing includes several hundred titles. We are therefore not attempting to give one here. We will only list the following books on this subject:

(1) *H. W. Kuhn* and *A. W. Tucker* (eds.), "Contributions to the Theory of Games, I," Annals of Mathematics Studies, No. 24, Princeton (1950), containing fifteen articles by thirteen authors.

(2) *H. W. Kuhn* and *A. W. Tucker* (eds.), "Contributions to the Theory of Games, II," Annals of Mathematics Studies, No. 28, Princeton (1953), containing twenty-one articles by twenty-two authors.

(3) *J. McDonald,* Strategy in Poker, Business and War, New York (1950).

(4) *J. C. C. McKinsey,* Introduction to the Theory of Games, New York (1952).

(5) *A. Wald,* Statistical Decision Functions, New York (1950).

(6) *J. Williams,* The Compleat Strategyst, Being a Primer on the Theory of Games of Strategy, New York (1953).

Bibliographies on the subject are found in all of the above books except (6). Extensive work in this field has been done during the last years by the staff of the RAND Corporation, Santa Monica, California. A bibliography of this work can be found in the RAND publication RM-950.

In the theory of n-person games, there have been some further developments in the direction of "non-cooperative" games. In this respect, particularly the work of J. F. Nash, "Non-cooperative Games," *Annals of Mathematics,* Vol. 54, (1951), pp. 286–295, must be mentioned. Further references to this work are found in (1), (2), and (4).

Of various developments in economics we mention in particular "linear programming" and the "assignment problem" which also appear to be increasingly connected with the theory of games. The reader will find indications of this again in (1), (2), and (4).

The theory of utility suggested in Section 1.3., and in the Appendix to the Second Edition has undergone considerable development, theoretically, as well as experimentally, and in various discussions. In this connection, the reader may consult in particular the following:

M. Friedman and *L. J. Savage,* "The Utility Analysis of Choices Involving Risk," Journal of Political Economy, Vol. 56, (1948), pp. 279–304.

J. Marschak, "Rational Behavior, Uncertain Prospects, and Measurable Utility," Econometrica, Vol. 18, (1950), pp. 111–141.

F. Mosteller and *P. Nogee*, "An Experimental Measurement of Utility," Journal of Political Economy, Vol. 59, (1951), pp. 371–404.

M. Friedman and *L. J. Savage*, "The Expected-Utility Hypothesis and the Measurability of Utility," Journal of Political Economy, Vol. 60, (1952), pp. 463–474.

See also the Symposium on Cardinal Utilities in Econometrica, Vol. 20, (1952):

H. Wold, "Ordinal Preferences or Cardinal Utility?"

A. S. Manne, "The Strong Independence Assumption—Gasoline Blends and Probability Mixtures."

P. A. Samuelson, "Probability, Utility, and the Independence Axiom."

E. Malinvaud, "Note on von Neumann-Morgenstern's Strong Independence Axiom."

In connection with the methodological critique exercised by some of the contributors to the last-mentioned symposium, we would like to mention that we applied the axiomatic method in the customary way with the customary precautions. Thus the strict, axiomatic treatment of the concept of utility (in Section 3.6. and in the Appendix) is complemented by an heuristic preparation (in Sections 3.1.–3.5.). The latter's function is to convey to the reader the viewpoints to evaluate and to circumscribe the validity of the subsequent axiomatic procedure. In particular our discussion and selection of "natural operations" in those sections covers what seems to us the relevant substrate of the Samuelson-Malinvaud "independence axiom."

John von Neumann
Oskar Morgenstern

Princeton, N. J.
January, 1953.

TECHNICAL NOTE

The nature of the problems investigated and the techniques employed in this book necessitate a procedure which in many instances is thoroughly mathematical. The mathematical devices used are elementary in the sense that no advanced algebra, or calculus, etc., occurs. (With two, rather unimportant, exceptions: Part of the discussion of an example in 19.7. et sequ. and a remark in A.3.3. make use of some simple integrals.) Concepts originating in set theory, linear geometry and group theory play an important role, but they are invariably taken from the early chapters of those disciplines and are moreover analyzed and explained in special expository sections. Nevertheless the book is not truly elementary because the mathematical deductions are frequently intricate and the logical possibilities are extensively exploited.

Thus no specific knowledge of any particular body of advanced mathematics is required. However, the reader who wants to acquaint himself more thoroughly with the subject expounded here, will have to familiarize himself with the mathematical way of reasoning definitely beyond its routine, primitive phases. The character of the procedures will be mostly that of mathematical logics, set theory and functional analysis.

We have attempted to present the subject in such a form that a reader who is moderately versed in mathematics can acquire the necessary practice in the course of this study. We hope that we have not entirely failed in this endeavour.

In accordance with this, the presentation is not what it would be in a strictly mathematical treatise. All definitions and deductions are considerably broader than they would be there. Besides, purely verbal discussions and analyses take up a considerable amount of space. We have in particular tried to give, whenever possible, a parallel verbal exposition for every major mathematical deduction. It is hoped that this procedure will elucidate in unmathematical language what the mathematical technique signifies—and will also show where it achieves more than can be done without it.

In this, as well as in our methodological stand, we are trying to follow the best examples of theoretical physics.

The reader who is not specifically interested in mathematics should at first omit those sections of the book which in his judgment are too mathematical. We prefer not to give a definite list of them, since this judgment must necessarily be subjective. However, those sections marked with an asterisk in the table of contents are most likely to occur to the average reader in this connection. At any rate he will find that these omissions will little interfere with the comprehension of the early parts, although the logical chain may in the rigorous sense have suffered an interruption. As he proceeds the omissions will gradually assume a more serious character and the lacunae in the deduction will become more and more significant. The reader is then advised to start again from the beginning since the greater familiarity acquired is likely to facilitate a better understanding.

ACKNOWLEDGMENT

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They are also greatly indebted to the Princeton University Press which has made every effort to publish this book in spite of wartime difficulties. The publisher has shown at all times the greatest understanding for the authors' wishes.