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Creating a Context for Game Theory

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Introduction

After decades of hesitation, the theory of games now plays a central role in economic theory. New textbooks on microeconomics no longer relegate Nash equilibrium to a section on "other topics," but place the formalization of interaction at the theory's very heart (see Kreps 1990). Nor has this been confined to the theoretical core: the "new" industrial organization, for example, differs from the older "structure-conduct-performance" paradigm in its emphasis on the game-theoretic aspect of firms' decisions (see Jacquemin 1987, Tirole 1988). Regardless of how one views the worth of these developments—and in this regard there is considerable debate (see Fisher 1989)—the ideas of game theory have permeated economics in a circuitous manner. While the stylized historical précis locates the seminal ideas with von Neumann and Morgenstern (1944) and their purification and refinement by Nash, Shapley, Aumann, and others in the intervening period, all cumulatively contributing to the position in which we find ourselves today, the adoption of the game paradigm by economic theorists has not been a smooth process. Indeed the "new" ideas of 1944 were primarily appropriated and developed not by economists but by mathematicians. The latter found theoretical games interesting because of their links with existing fields in mathematics and statistics and because the new ideas constituted a particularly rich bed of mathematical ore: the opportunity for constructing new theo-

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rems seemed great. They received support for such work because of the perception, in certain quarters, that game theory had potential military application. What is particularly interesting is that the mathematical work following von Neuman and Morgenstern 1944 concentrated not on those aspects of games given most attention in that book, but on earlier work by both von Neumann and some French mathematicians whom he seems to have ignored. Game theory's initial development rested, ironically, not on the material in the *Theory of Games*, but in the disparate papers that preceded it. In part 1 of this article, I discuss the disconnected contributions to mathematical games of the pre-1944 period, showing how, in the absence of any discursive mathematical community, game "theory" remains something of a misnomer. Part 2 examines the social, institutional, and mathematical transformations brought about by World War II, which created a context in which game theory became significant. After all the initial noise died down, these mathematicians took to exploring primarily pre-Morgenstern game theory, i.e., two-person noncooperative games. Furthermore, this postwar work was done with von Neumann's sanction and encouragement, even though he alone had developed the mathematics of cooperative games, had devoted the bulk of the *Theory of Games* to that topic, and saw the analysis of n-person games as the theory's crowning achievement.

The history of game theory has only recently begun to receive significant academic attention. Rives 1975 provides a useful introduction but does not explore any particular aspect in great detail. Mirowski 1991 treats some of the subject matter of this article, arguing that the military influence on games was not only significant, but retarding, in that it prevented the theory's proper or "logical" development. This article explores in considerable detail the seminal work by European mathematicians in the first half of this century, and then links it to the developments following the appearance of von Neumann and Morgenstern 1944. The latter work is shown to be significant not in that it *itself* became the object of direct mathematical attention, but in that it focused the attention of mathematicians and their patrons on an inchoate body of applied mathematics which World War II had made relevant.

1. Exploration: The Theory of Games to 1945

In 1953 there appeared in the columns of *Econometrica* (see Fréchet 1953) a restrained but firm debate on the legitimacy of John von Neumann's position as "initiator" of game theory. The challenge, posed by French mathematician Maurice Fréchet, implied that due credit had not been paid to his senior colleague Émile Borel. The latter, Fréchet pointed out, had written on game theory in the early 1920s prior to von Neumann, and while he had not proved the central minimax theorem, he had raised the question of its validity and had speculated on the ultimate application of such ideas to economic and military problems. Von Neumann wrote a stiff rejoinder claiming that until his 1928 proof of the theorem, "there was nothing worth publishing" (125). Fréchet remained committed to his claim, countering that Borel's early speculations provided "an open door," through which von Neumann could walk.

I take this debate as the point of entry into the history of game theory for the following reasons. First, it is a sign that something of theoretical significance had happened. There had emerged a set of ideas in which several parties had different, often conflicting, interests: there was something worth arguing about. Second, to the extent that it indicates what historical aspects the participants themselves found interesting, it helps us cast an interpretative net back over the period in question. Part 1 begins with an exploration of the period referred to by Fréchet. In particular, I examine the relevant work of Borel, von Neumann, and also the Polish mathematician Hugo Steinhaus. I also carry the inquiry through the 1930s, covering the passage of game theory to the United States, up to the publication of von Neumann and Morgenstern 1944. My concern, it must be emphasized, is not to reopen the priority debate. Rather, I illustrate with equal emphasis that, for the most part, there was negligible interaction between the individual mathematicians concerned. To the extent that a mathematical theory is given life by a discursive community arguing and contributing to the set of ideas in question, the mathematical analysis of games before World War II was a particularly lifeless affair. Not only was the number of interested people involved very small but, for all intents and purposes, they remained incommunicado. Only with the appearance of von Neumann and Morgenstern 1944 did this state of affairs begin to change. Throughout, to the greatest extent practicable, I describe the intellectual context of the ideas in question.

Borel's Wager

Like so many of the protagonists in our short historical excursion, Borel led a life that was, to put it mildly, fuller than that of many of his con-

temporaries (see Collingwood 1959, Fréchet 1965). His brilliance was recognized early, when at age eighteen, he won first prize in the Concours Général and achieved first place in the admission lists for both the École Normale and the École Polytechnique. For the rest of his career, he was prodigiously active in the spheres of both academics and politics.

Borel was directly influenced by mathematician Gaston Darboux in his decision to become a *Normalien* and pursue a career in research. His 1894 doctoral thesis on the theory of functions stemmed from a theme derived from Darboux and contained many of the seminal ideas that Borel would soon develop. During the period to 1905, he made several contributions of great significance, including the theory of measure, later developed by Lebesgue, the theory of divergent series, and an elementary proof of Picard's Theorem, which mathematicians had sought for over seventeen years. The culmination of this period was the beginning of a voluminous series on the theory of functions, edited and directed by Borel, to which he himself contributed five volumes. Under his directorship, fifty volumes of these "Borel Tracts" appeared.

His interests broadened during the first decade of this century to encompass probability theory, where he introduced the notion of enumerable probability[1] and his strong law of large numbers. In addition, with money he had received in academic awards, in 1906 he founded the *Revue du Mois*, a popular monthly, to which he contributed articles of scientific, philosophical, and sociological interest. He also undertook the editorship of a series of books intended to popularize scientific ideas, to which he himself contributed (with Painlevé) *l'Aviation* (1910) and *le Hazard* (1914). After World War I he entered public life, becoming a member of Parliament for twelve years, occasionally holding various positions of higher office, and all the while writing prolifically in mathematics. In 1921, having exchanged his chair of Theory of Functions at the École Normale for that of Theory of Probability and Mathematical Physics, formerly held by Poincaré, he began to edit and contribute to the monumental *Traité du Calcul des Probabilités et de ses Applications*, a series of monographs intended to "organize and expound the whole mathematical theory of probability and its applications as it

1. The issue of enumerable probability is concerned with the probability of events that depend upon the totality of an infinite sequence of random variables. While Bernoulli and others had examined this problem using the asymptotic properties of the probabilities, Borel was the first to consider the totality. His theorem showed that the probability for the totality depended on the convergence or divergence of the sum of probability sequences (see Collingwood 1959).

had developed up to that time" (Collingwood 1959, 488). This undertaking occupied the next fifteen years, till World War II, after which the seventy-four-year-old Borel began to write another fifty notes, papers, and books, mainly presenting mathematical and physical ideas to nonspecialist readers.

His first published work on the mathematics of games dates from 1921, by which time he had become absorbed in probability theory. In a series of notes written throughout the 1920s, Borel gives a reasonably systematic, if ultimately speculative, treatment of what would later be known as two-person symmetric zero-sum games. He clearly considered this work important enough for presentation to the august Académie des Sciences, to which he had been elected in 1921: three of the five notes appear in the academy's proceedings, the *Comptes Rendus*. As some of his observations are repeated from one paper to the next, I shall confine my discussion to the three most important (1921, 1924, 1927) in an attempt to capture both the letter and spirit of Borel's inquiry. To render this work tractable, I make a distinction between Borel's specific speculation on the possibility of a deeper mathematical theorem, which I examine first, and his general thoughts, by which is intended his illustration of concrete examples of games and his musing on the broader applicability of the general framework.

Borel 1921 defines certain concepts and creates the framework upon which the subsequent notes would build, albeit with frequent changes in notation.[2] First, Borel suggests that we consider a game "in which the winnings depend both on chance and the skill of the players," unlike such games as dice where skill does not influence the outcome. Defining a "method of play" as "a code that determines for every possible circumstance . . . what the person should do," Borel asks "whether it is possible to determine a method of play better than all others" (Fréchet 1953, 97).

Consider a game with two players A and B, who choose strategy ("method") C_i and C_k, respectively. Each have the same set of n strategies available. Given the strategies chosen, the entries in the matrix represent A's probability of winning the game: no winnings are transferred between players.[3] The numbers α_{ik} and α_{ki} are contained between

2. As far as possible, I use consistent notation. I also use matrices to a greater extent than Borel, in order to portray his ideas more easily.

3. In his later work, Borel uses payoffs in the matrix rather than probabilities, in the now conventional manner.

$-1/2$ and $+1/2$, and satisfy $\alpha_{ik} + \alpha_{ki} = 0$. Also, $\alpha_{ii} = 0$. The game is symmetric and fair.

Player B

		C_1	C_2		C_n
	C_1	$1/2 + \alpha_{11}$	$1/2 + \alpha_{12}$	$\cdots$	$1/2 + \alpha_{1n}$
Player A	C_2	$1/2 + \alpha_{21}$	$1/2 + \alpha_{22}$	$\cdots$	$1/2 + \alpha_{2n}$
	$\cdots$	$\cdots$	$\cdots$	$\cdots$	$\cdots$
	C_n	$1/2 + \alpha_{n1}$	$1/2 + \alpha_{n2}$	$\cdots$	$1/2 + \alpha_{nn}$

Players are assumed to automatically cast aside "bad" strategies, for example, methods of play that guarantee a probability of winning of less than half.[4] Having done this, the question is how the remaining strategies might be employed in the best manner possible. Borel suggests that a player can act "in an advantageous manner by varying his play," i.e., play a mixed strategy. C_k is played with probability x_k by A and y_k by B, where

$$\sum_1^n x_k = 1 = \sum_1^n y_k.$$

Given this, A's expected probability of winning is

$$\sum_1^n \sum_1^n (1/2 + \alpha_{ik}) x_i y_k = 1/2 + \alpha$$

$$\text{where } \alpha = \sum_1^n \sum_1^n \alpha_{ik} x_i y_k$$

and B's probability of winning is thus $1/2 - \alpha$.

Borel now considers the case where $n = 3$, given by the following matrix:

Player B

		C_1	C_2	C_3
	C_1	$1/2 + \alpha_{11}$	$1/2 + \alpha_{12}$	$1/2 + \alpha_{13}$
Player A	C_2	$1/2 + \alpha_{21}$	$1/2 + \alpha_{22}$	$1/2 + \alpha_{23}$
	C_3	$1/2 + \alpha_{31}$	$1/2 + \alpha_{32}$	$1/2 + \alpha_{33}$

Since no strategy is "bad," it must be the case that α_{23}, α_{31}, and α_{12} are all the same sign. He asks, is there any way that A can choose x_i

4. Borel suggests that strategies offering a probability of only 1/2 should also be rejected, but it is difficult to see why a player would do this and then proceed to seek a guarantee of 1/2 with the remaining strategies.

in such a manner as to ensure $\alpha \geq 0$, for any vector y_k? Alternatively put, if A knows the probabilities that B is going to employ, can he or she find a mixed strategy that will ensure an even chance of winning? Such positive numbers, x_1, x_2, x_3, Borel claims, are always easy to find: in a two-person symmetric game with three strategies available to each player, each player can ensure an even chance of victory.

In 1924, Borel extends this analysis, slightly modified, to the case of $n = 5$, where each player can choose from five strategies. Here, the entries in the matrix are the payoffs to player A. The same assumptions hold:

$$\alpha_{ij} = -\alpha_{ji} \text{ and } \alpha_{ij} = 0, \text{ if } i = j.$$

Player B

		C_1	C_2	C_3	C_4	C_5
	C_1	0	α_{12}	α_{13}	α_{14}	α_{15}
	C_2	$-\alpha_{12}$	0	α_{23}	α_{24}	α_{25}
Player A	C_3	$-\alpha_{13}$	$-\alpha_{23}$	0	α_{34}	α_{35}
	C_4	$-\alpha_{14}$	$-\alpha_{24}$	$-\alpha_{34}$	0	α_{45}
	C_5	$-\alpha_{15}$	$-\alpha_{25}$	$-\alpha_{35}$	$-\alpha_{45}$	0

Now A's expectation becomes

$$\sum_1^5 \sum_1^5 x_i y_k \alpha_{ik} = \sum_1^5 x_i Y_i$$

where

$$Y_i = \sum_1^5 y_k \alpha_{ik}.$$

Taking, for analytical convenience, the skew-symmetric matrix of the payoffs to be the following:

Player B

	0	1	a_1	$-a_4$	-1
	-1	0	1	a_2	$-a_5$
Player A	$-a_1$	-1	0	1	a_3
	a_4	$-a_2$	-1	0	1
	1	a_5	$-a_3$	-1	0

the Y_i, player B's expected payoff given that A plays strategy i, can be written:

$$Y_1 = y_2 + a_1y_3 - a_4y_4 - y_5$$
$$Y_2 = -y_1 + y_3 + a_2y_4 - a_5y_5$$
$$Y_3 = -a_1y_1 - y_2 + y_4 + a_3y_5$$
$$Y_4 = -a_4y_1 - a_2y_2 - y_2 + y_5$$
$$Y_5 = y_1 + a_5y_5 - a_3y_3 - y_5$$

Borel now asks, Is it possible for player B to choose the vector of y_i such that each of the Y_i is no less than zero? He then devotes the rest of the paper to showing that appropriate probabilities y_i can always be found. Depending on the values of the a_i in the payoff matrix, B can keep all the Y_i to zero, thus ensuring no advantage to A. Borel concludes that with $n = 5$, "nothing essentially new happens compared to the case where there are three manners of playing" ([1924] 1953, 114), i.e., each player can ensure an expected payoff of zero. But speculating as to whether this is likely to hold for n arbitrarily large, Borel is pessimistic and suggests that it will not always hold. However, three years later, in another note (Borel [1927] 1953) presented to the Académie, he reports an extension of his analysis giving rise to greater optimism: what has held for three and five strategies seems also to hold for seven, and it would thus "be interesting either to demonstrate that it is unsolvable in general or to give a particular solution" (117).[5]

While Borel may have concluded his search for a deeper theorem on a speculative note, his work of the 1920s is notable in certain other respects, especially in the provision of general concepts and examples. First, introducing the infinite game, where strategies are drawn from a continuum, he shows how the continuous analogue of player A's expected payoff is expressible as a Stieltjes integral:

$$\alpha = \int_{-\infty}^{\infty}\int_{-\infty}^{\infty} f(C_A, C_B)\,d\phi_A(C_A)\,d\phi_B(C_B)$$

where $f(C_A, C_B)$ is the function relating A's payoff to the strategies chosen, and $\phi_A(C_A)$ and $\phi_B(C_B)$ are A's and B's respective cumulative distribution functions over strategy space. The example offered of such a game is what later was to become known as a game on the unit square: in this case, each player chooses three real numbers summing to 1, the

5. Borel's analysis is confined to games with odd numbers of strategies because of his use of determinants of skew-symmetric matrices to calculate the optimal mixed strategy.

winner being the one with two choices of greater value than the opponent's. While Borel simply describes the game here, a decade or so later, in 1938, his student Jean Ville would extend the minimax theorem to such games.

Second, Borel looks at specific examples of finite games such as "paper, scissors, stone" ([1924] 1953, 102) and shows in detail how the calculation of the optimal mixed strategies depends on relative payoffs. For example, if the payoff to A for a particular strategy is relatively large, then the probability attached to it in the optimal mixed strategy will be correspondingly low: otherwise, B could gain by anticipating A's emphasis on the favored strategy.

Third, he considers the broader application of these ideas to the nonmathematical realm: "The problems of probability and analysis that one might raise concerning the art of war or of economic and financial speculation are not without analogy to the problems concerning games" ([1921] 1953, 100). He cautions restraint in this regard, however, saying that such matters are highly complex and that mathematical calculation can at best be a supplement to strategic cunning. As we shall see below, this trace of scepticism in Borel would grow even stronger with time.

In his final related communication to the Académie, in May 1928, Borel supplied the answer to the question that had occupied him in the notes discussed above, that is, concerning the existence of a "best" way to play. However, this clarification came not as a result of further work by Borel himself, but in the shape of a note from somebody who claimed to have proved the minimax theorem in Göttingen two years previously. This, of course, was John von Neumann, then in his mid-twenties and thirty years Borel's junior (see von Neumann 1928a). Before turning to von Neumann, however, I shall consider the related contemporaneous work of Hugo Steinhaus at Lvov in Poland. While Borel and von Neumann were at least aware of each other's work and common interest, Steinhaus, it appears, labored in total isolation.

A Pole Apart

The period between the two world wars was one during which mathematics flourished in Poland (see Kuratowski 1980). Under the tutelage of such figures as Zaremba and Sierpinski, there emerged from the universities in Lvov and Wroclaw a number of capable young mathematicians including Banach, Ulam, and Steinhaus. Born in 1887 in Jaslo, Poland,

Steinhaus spent one year studying at the university in Lvov and then took off to Göttingen, where he completed his doctorate in 1911 under David Hilbert. Following this, he lived as an independent scholar in Jaslo, until the outbreak of war, when he joined the Legions. In 1916 he joined the faculty at Lvov, became full professor by 1923, and stayed until again interrupted by war in 1941. In 1945 he moved to the scientific center at Wroclaw, where he remained until his death in 1972. In both Lvov and Wroclaw, Steinhaus was a leader among the groups engaged in mathematical research (see Kac 1985).

Steinhaus's mathematical interests were wide-ranging in both theoretical and applied areas. In the former, he was active in the theory of trigonometrical series, functional analysis, orthogonal series, the theory of real functions, and, perhaps most famously, in sequences of linear operations, for which he is remembered as a collaborator of Banach. In applied areas, he published on probability theory and on the application of mathematics to questions in medicine, electricity, biology, geology, and anthropology. It was, no doubt, this latter taste for the mathematics of applied problems that led Steinhaus to games.

The work in question is a single article, "Definitions for a Theory of Games and Pursuit," which appeared in 1925 in the first issue of "an ephemeral pamphlet called 'Mysl Akademicka' " (Steinhaus 1960, 108). A short-lived periodical edited by Lvov students, its first issue was also its second to the last! Here, Steinhaus claims that he is concerned with the construction of mathematical definitions for a group of problems that lie "beyond the strict boundaries of mathematics" (106). The problems discussed are chess, naval pursuit, and card-playing, and the thread that binds them together is their use by Steinhaus to motivate the notion of minimax play (without actually terming it such). He introduces the concept of a "mode of play" which connotes, for either player, "a list of all possible circumstances with a preferred move for each" (106). Considering chess, he modifies the game by placing a limit, known to both players, on the total number of moves permitted. Should White not win before this limit is reached, Black wins. Black's aim, therefore, is to adopt the strategy that prolongs his defense, while White's is to keep the length of play as short as possible by winning as quickly as possible. Given that Black chooses a strategy to maximize the duration, White chooses the strategy that keeps this maximum to a minimum. In exactly the same manner, Steinhaus discusses two ships in pursuit: one chasing, the other fleeing, both at a given speed. The

pursuer's aim is to close the angle between its line of steering and line of sight, thereby minimizing the time in pursuit. The evader's aim is the opposite. Each ship's strategy is a function of both ships' coordinates. At any moment, given that the evader has chosen the time-maximizing strategy, the hunter will respond by attempting to minimize it. Finally, Steinhaus notes that in card games a similar tussle is involved as each player tries to reduce the expected gain of his or her opponent.

While conceding that only definitions have been provided, Steinhaus notes that these are essential for the next stage, the calculation of best play. However, actually finding the "best move," "best pursuit," or "best way of playing" involves "enormous difficulties." Pursuit, for example, would require us "to use the calculus of variations on a very difficult problem of mathematical analysis," while even the simplest card games "lead to very involved combinatorial calculations" (107). Had Steinhaus learned the following year that the mathematical consideration of games was the subject of discussion among the Göttingen group, centered on his doctoral supervisor Hilbert, his paper might not have been forgotten. As it was, it gathered dust for decades.[6]

New Boy at Göttingen

Born to a wealthy Jewish banking family in Budapest in 1903, "Jansci" Neumann was the eldest of three brothers.[7] Among these he was "the most aggressive one, the least sentimental, the most thoughtful," staying away from childish games, preferring to read, study, or calculate or, during World War I, play "elaborate battles with toy soldiers" (Heims 1980, 41). The young von Neumann was precocious and, shortly after entering the Lutheran Gymnasium at age ten, was recognized by his mathematics teacher as a child prodigy. At the teacher's suggestion, the

6. Steinhaus's paper was not translated into English until 1960, when it was published in the *Naval Research Logistics Quarterly*. The paper is introduced there by Harold Kuhn who explains that the Polish version was secured by Stan Ulam. An early reference to the paper appears, however, in a letter from Oskar Morgenstern to Olaf Helmer at RAND in October 1952, in which he states that a friend was writing to Steinhaus for a copy of the paper (see OMPD, Box 14, File RAND). In a letter accompanying the published paper, Steinhaus offers some background to his search for the paper. Not until 1957 did he retrieve the paper, and then only through a colleague who secured a copy of the journal in Lvov, which was by then part of the Soviet Union.

7. The family name Neumann became von Neumann when John's father, Max, was ennobled by Emperor Franz Joseph in 1913 "for his contributions to the economic development of Hungary" (see Aspray 1990, 254 n. 4; Heims 1980, 29–32).

child's father arranged additional tutoring from a mathematics lecturer at the University of Budapest. By the time he left secondary school, he had won the nationwide Eotvos prize for "excellence in mathematics and scientific reasoning" (44).

In 1921, at seventeen, von Neumann enrolled at the University of Budapest but promptly left the country for Berlin, thereafter returning to Budapest to take the necessary university exams or visit his family.[8] Spending 1921–23 at the University of Berlin, he was influenced by David Hilbert's former student Erhard Schmidt and by Albert Einstein from whom he took lectures on statistical mechanics. He kept company with fellow Hungarian émigrés including Eugene Wigner, Leo Szilard, and Dennis Gabor, all part of the "Hungarian Phenomenon," which was later to have an enormous impact on American physics and mathematics. During this time, von Neumann also made contact with Hilbert at Göttingen, the "mecca of German mathematics," beginning a collaboration which would last several years and influence von Neumann for life. From Berlin, he went to the Swiss Federal Institute of Technology at Zurich where he took a degree in chemical engineering in 1925. There he fell in with George Polya, another Hungarian mathematician who would later come to Stanford University, and Hermann Weyl, a German mathematician who was to be one of the first residents at Princeton's Institute for Advanced Study (IAS). In 1926, von Neumann received his doctorate in mathematics from Budapest and spent the rest of the year in Göttingen; the following year he was appointed Privatdozent at the University of Berlin. Throughout this entire period, he maintained close contact with Hilbert.

Given the importance of von Neumann to our story, it is worth portraying the historical developments in mathematics in which his mentors at Göttingen had crucial roles.[9] Over the nearly fifty years from 1895 during which he reigned at Göttingen, Hilbert made mathematical contributions that arguably, among mathematicians, exalted him to the

8. In Fermi (1968), Eugene Wigner, Hungarian contemporary of von Neumann and later atomic physicist in the United States, comments on the tendency for young Hungarians to leave early: "[While] Hungarian high schools were excellent, . . . the universities were very poor. . . . It was symbolic of the bureaucratic influence . . . that professors at universities kept their libraries locked up and a student who needed to consult a book was often obliged to borrow the key from his professor" (54).

9. For a fine account of this period, the reader is referred to Constance Reid's 1970 biography of Hilbert.

rank of an Archimedes, Newton, or Gauss. By 1902, he had made his mark on, *inter alia*, invariant theory, the calculus of variations, and the foundations of geometry. In the last, his axiomatization of Euclidean geometry (*The Foundations of Geometry*, 1899) signaled the beginning of a lifelong preoccupation with the way in which proofs in mathematics are related to the axioms on which they are based. True rigor, for Hilbert, required that axioms be complete, in the sense that all theorems be derivable from them; independent, in that the removal of any axiom would make it impossible to prove at least some of the theorems; and consistent, so that no contradictory theorems could be established using such axioms. In this, he was further galvanized by Zermelo's and Russell's independent observations of a fundamental antimony, or paradox, in set theory and called for the mathematical investigation of proof itself.[10] This became known as metamathematics or proof theory. Just as Hilbert was concerned with the logical rigor of mathematics, so too was he disturbed by the apparent lack of order in the constructions of the physicists, which at this time were growing rapidly. The turn of the century saw Hertz's proof of the existence of electromagnetic waves; Roentgen's discovery of X rays; the Curies' radioactivity; J. J. Thomson's electron; Einstein's special theory of relativity; and Max Planck's quantum theory. Hilbert "perceived the pressing necessity for investigation to determine whether these diverse principles were compatible with one another and in what relation they stood" (quoted in Reid 1970, 127). Thus, beginning in 1912, he turned his attention to the mathematics underlying purely physical phenomena, beginning with kinetic gas theory and then elementary radiation theory, in each case constructing, from the axiomatic base up, a mathematical theory consistent with the physics.

After World War I, Hilbert's attention reverted to the exploration of foundations in response to the view of mathematics known as intuitionism, then being propagated by Dutch mathematician L. Brouwer. Briefly stated, this view rejected mathematical objects, the proof of whose existence depended on an infinite number of steps. Any existence proof that implicitly depended on a greater than finite number of steps could not be regarded as constructive since the existent, even in principle, was

10. Russell's Paradox was put forward in 1902. Some sets are not members of themselves, for instance, the set of all women. Other sets are members of themselves, such as the set of all things that are not women. If one considers the set consisting of all sets who are not members of themselves, one finds the paradox that if it is a member of itself, then it is not, and vice versa. Antinomies such as these gave rise to restrictions on the use of general properties to define sets.

unattainable.[11] Logically, this meant the rejection of much that classical mathematics took for granted, including the Principle of the Excluded Middle, Cantor's theory of infinite sets, and many existence proofs. To Hilbert, who considered such concepts central, the denial of all this to mathematics was akin to "prohibiting the boxer the use of his fists" (Reid, 149). By 1922, his agitation was heightened further by the degree to which intuitionism seemed to be taking hold among younger mathematicians, such as Weyl, and, turning from physics, he threw himself at his work on axiomatics with renewed vigor. Mathematics, if it were to remain intact, had to establish deductions with the same certitude that existed for the arithmetic of whole numbers "where contradictions and paradoxes arise only through our own carelessness" (quoted in Reid, 176).

Meanwhile, Hilbert's colleagues were contending with the proliferation in physics, which by 1926 had become even more notable. Heisenberg's new theory of quantum mechanics was shown to be explicable in terms of matrix methods by Max Born. Then Schrödinger, at Zurich, constructed his wave mechanics which, while it led to the same results as Heisenberg, proceeded from an entirely different base. The two were soon mathematically reconciled by Courant, using, to a great extent, Hilbert's earlier work on integral equations and infinitely many variables (Hilbert Space). These radical developments in physics challenged prevailing views in mathematics. The theory of relativity deeply questioned many concepts that were central to classical mechanics, such as absolute space and time, simultaneity, and so on. Quantum theory, more importantly, threw mechanistic determinism into disarray by demonstrating the impossibility of knowing simultaneously both the position and velocity of a particle, without which its future evolution could not be predicted. Such basic contradictions called for fundamental changes in the mathematics used, to the chagrin of classical mathematicians such as Poincaré and Volterra, who believed in the underlying continuity of physical events and the possibility of their representation by the infinitesimal calculus and the theory of differential equations. As Ingrao and Israel (1990) point out, the emphasis shifted from *mechanical* analogy to *mathematical* analogy. Mathematics was to be used for its form, to provide a language unifying theories, rather than to mechanically describe physical processes.

11. Reid (149) offers the following example: consider the statement A, "There is a member of the set S having the property P." If S is infinite, each member of the set cannot, even in principle, be examined to verify the statement. Brouwer rejected such existential statements for infinite sets. Now the Principle of the Excluded Middle says that if A holds then ~A does not hold, and vice versa: there is nothing in between. Brouwer argued as follows: if in S above one finds one element showing property P, then the first alternative is substantiated. If, however, one does not, then the second alternative is still not substantiated, nor has the middle been excluded.

If strong tremors, therefore, were being felt throughout the mathematical world at this time, the Göttingen to which von Neumann was first directly introduced in 1923 was at the epicenter. His contributions for the next few years reflected the concerns of the time and the place. Hilbert's twin concerns about the foundations of mathematics and the axiomatization of mathematical physics greatly influenced him (see Heims 1980, Goldstine 1972). Working with Lothar Nordheim, Hilbert's assistant in physics, von Neumann undertook the axiomatization of Heisenberg's work and then proceeded to develop further the notion of the Hilbert Space to provide a fuller mathematical basis for quantum mechanics. His seminal work on the axiomatization of physics appeared in the form of three articles in 1927, three in 1929, and their condensation into a book, *Mathematical Foundations of Quantum Mechanics* (1932). In 1927, influenced by Hilbert's concern with the foundations of mathematics, he also published a paper conjecturing that all analysis could be proved consistent. At some point in 1926, von Neumann produced his proof of the minimax theorem which, not surprisingly, was overshadowed by his contemporaneous work (see Heims 1980, 56). The source of his interest in games thus remains something of a mystery. However, his use of the axiomatic approach is entirely in keeping with the Hilbertian ethic with which he was fully imbued, and the notion of chance, made central through probabilistic play, is consonant with the indeterminism at the basis of quantum mechanics: "Chance . . . is such an intrinsic part of the game itself (*if not of the world*) that there is no need to introduce it artificially by way of the rules of the game: . . . it still will assert itself" (von Neumann 1928b, 26; emphasis added). As noted above, there are two publications by von Neumann dealing with the minimax proof (1928a, 1928b), the first of these being the communication to Borel presented by the latter to the Académie. In it, von Neumann refers to Borel's work in this vein since 1924 and claims he has solved the problem of the existence of a best way to play the two-person zero-sum game. He points out that he reached these results independently: "M'étant occupé indépendamment avec le même problème" (1928a, 1689), and that the full proof is forthcoming soon as "Zur Theorie der Gesellschaftsspiele" in

Mathematische Annalen, the Göttingen journal (which had accepted it for publication by this time).

The second, von Neumann 1928b, appeared later that year, primarily containing a long and difficult proof of the existence of an equilibrium value for the two-person, discrete game, based on functional calculus and topology. The paper reveals little about whose work, if anybody's, von Neumann draws upon and there is no reference to the past except in two footnotes. The first of these says that the paper had been presented in shorter form in December 1926 to the Göttingen Mathematical Society.[12] The second states that while finalizing the current paper he "learned of the note of E. Borel in the Comptes Rendus of Jan. 10, 1927" (25n). In the paper, the concept of a game is completely axiomatized and two examples are offered of zero-sum games with solutions only in mixed strategies.[13] Among the situations that can be regarded as games of strategy are roulette, chess, baccarat, bridge, and "the principal problem of classical economics: how is the absolutely selfish 'homo economicus' going to act under given external circumstances?" (13).[14] Von Neumann also treats the three-person zero-sum game, showing how the possibility of coalition formation introduces a measure of indeterminacy, or "struggle," into such games. In preliminary remarks on games with more than three players, he broaches the topic to which von Neumann and Morgenstern (1944) would later devote much space. Without calling it such, he introduces the characteristic function, a "system of constants" describing "the sum per play which [each] coalition of the players . . . is able to obtain from the coalition of the other players," and conjectures that "*the complex of valuations and coalitions in a game of strategy is determined by these . . . constants alone*" (40–41; emphasis added). If this is the case, then "we have brought all games of strategy into a natural and final normal form" (41). In conclusion, von Neumann adds that a later publication will contain numerical calculations of a simplified poker and baccarat, the results of which are corroborated by the well-known necessity to "bluff" in poker.

12. This was undoubtedly the Göttingen Mathematical Club, "the highpoint of the mathematical week . . . during the 1920's" (Reid 1970, 168). This was an informal gathering where faculty and students gave talks on their recent work.

13. The two games illustrated are matching pennies (here the payoffs are given but the name is not used) and morra (also called "paper, stone, scissors"). The latter is one of the games considered by Borel in his 1924 note.

14. This is the first indication of von Neumann's interest in economic matters. Nicholas Kaldor (in Dore 1989) recalls von Neumann expressing such an interest in 1927, not long after his Berlin appointment. They met while returning on holiday to Budapest as young scholars working abroad. Kaldor suggested he read Wicksell's *Value, Capital, and Rent*, which provided an introduction to Walras and utilized Böhm-Bawerk's capital theory. Von Neumann, on reading this, criticized the Walrasian system, saying that it permitted negative prices. Kaldor speculates that this may have prompted von Neumann to write his 1937 paper on economic growth, and that furthermore, von Neumann's interest in economics may well have grown out of his interest in game theory.

The French Connection

I observed above that von Neumann's communication with Borel was minimal, peremptory, and more in the nature of a rebuff than an invitation to exchange ideas. Under these circumstances, it is interesting to inquire as to what subsequently happened in the 1930s to the latter's work on games. Did he, for example, incorporate von Neumann's result into his analysis? Or did he take up the framework suggested in the 1928 paper? However, even these questions suggest too smooth and seamless a conception of the history of ideas: what happened, briefly, was that Borel seems to have gradually lost interest; his student, Jean Ville, took up the cudgel, doing some further original work; and one of his contemporaries, René de Possel, brought the good news in popular form to the French intelligentsia. *And all this went by, it appears, unbeknownst to von Neumann.*

In 1936, mathematician René de Possel wrote what might be regarded as the first popularization of von Neumann's minimax theorem. This came as part of a series of monographs on original contributions to science, philosophy, literature, and art, produced by the Centre Universitaire Méditerranéen de Nice and edited by the poet Paul Valéry. These were clearly intended for the well educated but nonspecialist reading public. De Possel's booklet is a forty-page description of the analysis of popular games, in which von Neumann's theorem is presented as the culminating achievement to date. Following Borel 1924, games are divided into those based on pure strategy, those based solely on chance, and those involving "ruse," or bluff, where a player can gain an advantage by knowing the opponent's intentions. De Possel discusses various examples. For example, in the game of "batonnets" each player draws a number of matches from a pile until it is depleted. No more than a certain number may be drawn each time and the last person to draw wins the game. Based on the number of matches in the pile, an optimal strategy exists for at least one of the players, demonstrable by backward

induction. Various versions of this purely strategic game are discussed. Roulette is presented as a game of pure chance. In this context, he explains the idea of the martingale, or how best to spread one's bets across several rounds, given the capital at one's disposal and the unfolding pattern of wins and losses. The ubiquitous "Baccarat du Bagne," or "scissors, paper, stone," is presented as an example of a "social game," which combines strategy, chance and "ruse," and von Neumann 1928b is invoked to show how minimax play is optimal in that it eliminates the risk of the opponent guessing one's intentions. While the theorem itself is "too technical to be reproduced here" (39; author's translation), it is clearly the booklet's central feature, and von Neumann is honored as "the first to seek to penetrate the mechanism of play from such a general perspective" (5; author's translation).

However, no such credit is afforded von Neumann in Borel's work of the same period. In 1938, as part of his extended treatise on probability, Borel contributed a volume on "Applications to Games of Chance." This is a version of his course on the topic at the École Normale in 1936–37, written and edited by his student Jean Ville.[15] Following his by now standard approach, Borel initially examines at length dice and simple card games, where chance is the dominant feature and the analysis is confined to combinatorial probabilities. Then, in a chapter called "Games Where Psychology Plays a Fundamental Role," he synthesizes and extends his work of the 1920s. Again, both finite and infinite games are analyzed. In the case of the former, the usual suspects are featured, for example, "Heads-or-Tails" and "Baccarat du Bagne," and the determination of optimal mixed strategies discussed in detail. Remarkably, however, *no* mention is made of what had become, for de Possel at least, the "théorème fondamental," that of von Neumann (1928b). This can only be regarded as an act of deliberate omission by Borel. Indeed, he devotes most attention to continuous games. For example, in the game where two players must each choose a point on a circle, some fair criterion determining the winner,[16] simple integration is used to construct the distribution that each should optimally apply in selecting a point; the density is uniform so all points are afforded the same probability. This is extended to the choice of three points from a continuum by each player,

15. It was, and to some extent still is, quite common for some French professors to have a *protégé* take careful class notes for later publication.

16. On a fixed circle, a chosen point wins if the other choice lies within an arc of length p, in the counter-clockwise direction. Otherwise, by definition, the other choice wins.

and the same mathematical justification is provided for the intuition of random play. Once again, Borel considers situations to which these principles are connected, in both the military and economic arenas. For example, he suggests that the analysis might be applied to the allocation, by opposing armies, of their respective forces to a limited number of common strategic points, or to the problem of how two merchants, wishing to sell equal stocks of similar goods, should distribute their available discounts across the goods, given that they are competing for customers.[17] In a final chapter, a simplified poker is analyzed, showing the importance of bluffing. It is possible that this was prompted by von Neumann's earlier indication to Borel that he had done such work.

Following the above, and occupying but 9 of the book's 120 pages, is a note by Borel's student Jean Ville, which, in the purely mathematical sense, was to have a greater subsequent impact on the consolidation of the theory than the rest of the book. This, of course, is Ville's construction of the first elementary proof of the von Neumann theorem for finite games and his own extension of this to show that a simple infinite game also has a value. Ville's proof is partly topological and rests on a theorem on linear forms in nonnegative variables. Compared to von Neumann's earlier contortions, it is positively elegant.

Consider p linear forms in n variables:

$$f_j(x) = \sum a_{ij}x_i \quad (j = 1 \ldots p; i = 1 \ldots n)$$

with the property that, whatever the nonnegative values of x_i, there exists among the f_j at least one that is nonnegative. Then, there exists at least one system of nonnegative coefficients $Y_1 \ldots Y_p$, $(\sum Y = 1)$, such that $\sum Y_j f_j$ is nonnegative for all nonnegative values of variables x_i. Having proved this, Ville then establishes a corollary: in the same system, if, whatever the nonnegative values of x_i, there exists at least one f_j no less than φ, itself a linear form in the same variables, then there exists a linear combination,

$$\psi = Y_1 f_1 + Y_2 f_2 + \cdots + Y_p f_p \quad \text{with } Y_j \geq 0, \ \sum Y_j = 1,$$

such that

$$\psi \geq \varphi \quad \text{at every point } x \text{ (where } x_i \geq 0, i = 1 \ldots n).$$

17. It is this military example that would later feature as the Colonel Blotto game, first appearing in Morse and Kimball 1946 and then developed at RAND in the late 1940s, ostensibly to consider the global allocation by the United States and Soviet Union of their respective armaments. See Dresher 1961.

Having defined a game and shown each player's expectation, conditional on the opponent's strategy, to be a system of linear forms, Ville simply invokes the above theorem and corollary to show the existence of a value that represents at once an assured upper limit on the minimizing player's expectation and a lower bound on that of the maximizing player. He then, for the first time, draws the infinite game into the compass of the minimax concept. Considering a simple game where each player chooses a point from the unit interval, player A's expectation is given by

$$\int_0^1 \int_0^1 K(x,y)\,dF\,d\Phi,$$

$K(x,y)$ being A's payoff given choice of points x and y; F and Φ being the probability distribution functions applied to the strategies chosen by A and B, respectively. Ville shows that if $K(x,y)$ is continuous in x and y, in the closed domain $0 \leq x \leq 1, 0 \leq y \leq 1$, then this game too has a value. This he does by establishing this infinite game as a limiting case of a finite game with very many strategies.

Following this, Borel offers some observations on his student's contribution which unambiguously reveal his doubts about the entire theory. Having thanked Ville for illustrating von Neumann's "important theorem," he says, "It appears essential for me to indicate, however, to prevent all misunderstanding, that the practical applications of this theorem to the actual playing of games of chance is, for a long time, unlikely to become a reality" (Borel 1938, 115). Actual games are exceedingly complicated, he says, containing many coefficients and equations. Even if one could simplify a game sufficiently to the point where such calculations were possible, the advantage of playing according to the above theorem are only attainable on average, after a great number of rounds. And even taking account of the experienced recommendations of players to locate what might be regarded as reasonable strategies, "there still remains such a great number of variables that even the task of writing the equations, not to mention that of solving them, appears absolutely insurmountable" (115).

In Borel, one senses a respect for games, many of which are old and have been played for generations. In many such games, a consensus may emerge on what constitutes good play and thus what the novice should be taught. However, no sooner has such agreement been reached than the better players take advantage of it to introduce newer, more successful ways of playing. This is what makes games interesting. Indeed, it often goes full circle, with today's innovators reviving ways of playing that

were once considered revolutionary but then abandoned. Can it ever be hoped, he asks, that this natural evolution will approximate the impracticable solution of a system of equations that completely describes the game? "This, I must admit, seems highly doubtful to me, and, anyway, if it happened for a particular game, it is almost certain that the game would soon be abandoned for a more complicated one" (116). Furthermore, even where ideal play involves a probabilistic element, it is very difficult not to follow some regularity when actually playing. This is particularly important in bridge, for example, where probabilistic play intended to defeat one's opponents may well mislead one's partner also! "All these remarks . . . are obvious to anyone with some experience in games. Perhaps they will make clear, to those uninterested in games, how enjoyable games are as leisurely distraction, at the same time showing to those who would wish to turn games into an occupation, how futile is the search for a perfect formula which is forever likely to elude us" (117). These remarks effectively signal the end of Émile Borel's active contribution to game theory.[18] They both constitute the first criticisms of the minimax idea, and suggest his unwillingness to sacrifice the mystery and delight of games for an elegant but inapplicable mathematics. His is a refusal to take it all too seriously.

Von Neumann in the 1930s

After a one-semester visit to Princeton in 1929 to lecture on quantum theory at the invitation of geometer and topologist Oswald Veblen, von Neumann alternated between there and Berlin, continuing his work toward the *Mathematical Foundations of Quantum Mechanics* (1932). During this period, his interest in economics was further stimulated and, in fact, the minimax idea resurfaced in the context of his 1937 growth model. At Menger's seminar at the University of Vienna, a sequence of papers presented between 1932 and 1937 dealt with equilibrium and growth in Cassel-type models (see Weintraub 1985, 72–78). These were usually published the year following in the *Ergebnisse* (translated, *Proceedings*), edited by Menger. Karl Schlesinger (1932) offered a reformulation of Cassel using inequalities and set forth a model, without mathematical analysis. Following this, Wald, Menger's student, proved the existence of a solution to Schlesinger's model (the first existence

18. Subsequent publications merely reiterated his earlier work, for example, Borel 1950, Appendix 1.

proof) and modified this further in 1936. Von Neumann (1937) removed the distinction between primary factors and outputs: all goods are produced. Rather than emphasize production of single goods, he uses processes; one process may produce multiple outputs and each output may result from different processes. He then characterizes the equilibrium rate of growth as the saddle point of a function relating the input and output matrices, its existence proved using Brouwer's fixed point theorem.[19] Apparently, von Neumann had presented the paper at the Princeton Mathematical Society in 1932 and, unlike the other papers in the series, did not present it to the Vienna seminar.

The 1930s, therefore, saw the affirmation of von Neumann's interest in theoretical economics. Among economists, it is for his contribution on the growth model during this period that he is remembered. It is also true, however, that the "pure" mathematics of games was on von Neumann's mind during this time. In April 1937, in its mathematics section, *Science News Letter* reported a talk by von Neumann at Princeton on what for him was "a mere recreation," his analysis of games and gambling. Apparently, he spoke about "stone-paper-scissors," showing that by "making each play the same number of times, but at random, . . . your opponent will lose in the long run." Also parsimoniously reported are his observations on the probabilities of making particular plays in both dice and a simplified poker. Two-and-a-half years later, in November 1939, von Neumann was planning a visit to the University of Washington, where he was to spend part of the upcoming summer semester as Walker-Ames Professor in mathematics. In a letter to the department, he suggests possible topics for his lectures, including quantum theory, operator theory, groups, and the "Theory of Games." On the last he says, "I wrote a paper on this subject in the Mathematische Annalen 1928, and I have a lot of unpublished material on poker in particular. These lectures would give a general idea of the problem of defining a rational way of playing" (von Neumann Papers [VNP], Container 4, File 3, Personal Correspondence 1939–40).

Finalizing matters four months later, in March 1940, he indicated that he would give three evening lectures on games:

19. Despite apparently taking Cassel's work as a starting point, the paper contains no reference to the related work of Schlesinger or Wald. Wald and Menger, however, accepted the paper for publication without exercising their editorial prerogative. Arrow (1989) notes that "von Neumann's lack of references is in general a source of difficulty in reconstructing the evolution of ideas" (17).

1. The general problem. The case of chess.
2. The notion of the "best strategy."
3. Problems in games of three or more players.

General remarks (File 3).

The extent to which Seattle's mathematicians were stirred by von Neumann's still-quirky ideas is unknown. What is clear, however, is that just prior to this sojourn he had captured the imagination of one distinctly nonmathematical economist who had moved to Princeton from Vienna two years previously, Oskar Morgenstern.

Morgenstern's Early Career

Born in Silesia, Germany in 1902, Oskar Morgenstern moved at twelve years of age with his family to Vienna where, in 1925, he obtained his doctorate with a thesis focusing on marginal productivity.[20] Following three years visiting London, Columbia and Harvard Universities, Paris, and Rome as a Rockefeller Fellow, he was appointed Privatdozent at the University of Vienna in 1929. Morgenstern's habilitation thesis and first book, *Wirtschaftsprognose* (translated *Economic Prediction*, 1928), focused, in the Austrian tradition, on epistemological difficulties in economic forecasting: the difficulties of "knowing" when "other wills," other "economic acts" may "interfere with, or enhance, one's own plans" (Morgenstern 1976, 806). During the 1930s, his work concerned such issues as the business cycle, methodology, and the treatment of time in economic theory. He also had some work published in the United States and both George Stigler and Frank Knight were familiar with his work. His main professional activity was the directorship of the Vienna Institute of Business Cycle Research and he also acted as editor of the *Zeitschrift für Nationalökonomie*.

It is easy to look back at his work and pick out those elements that are most congruent with what later appeared in the *Theory of Games*. However, I believe his position as coauthor with von Neumann is better understood by focusing not on particular incipiently "game-theoretic" concepts he may have alluded to in his earlier career, but rather on his

20. For the fullest extant treatment of the life and work of Oskar Morgenstern, the reader is referred to Rellstab (1991, 1992), both of which are part of a larger project. In gaining an understanding of Morgenstern, I have benefited tremendously from conversations with, and the generosity of, Urs Rellstab.

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general position as an arbiter of ideas, an intermediary between theorists in disparate fields, and one ultimately most capable of giving his energy to penetrating criticism rather than alternative theoretical construction. First of all, Morgenstern was primarily an outsider: neither intellectually nor psychologically did he fit comfortably into any particular group or school of thought. He attended the meetings of the Vienna Circle and was much taken by the philosophical flux centered on Schlick and Carnap, but he was not a philosopher, and he discussed these developments at a distance. Neither was he a mathematician, and while he attended the Menger colloquia and debated issues in mathematics and their relationship to the social sciences, his relationship to mathematics remained akin to that of the impresario to the music.[21] Second, Morgenstern's intellectual interests, while varying in depth, were certainly broad, and he consistently attempted to relate disparate developments in philosophy and mathematics to economic theory. In "The Time Moment and Value Theory" (Morgenstern 1935b), the work of Karl Menger and Moritz Schlick in logic forms the background for his critique of the treatment of time in neoclassical economic theory. Similarly, in "Perfect Foresight and Economic Equilibrium" (1935a) and "Logistics and the Social Sciences" (1936), he contends that only by incorporating recent developments in logic can economics achieve what he saw as the necessary level of rigor. The need for mathematical rigor he emphasized incessantly in both his public remarks and personal reflections: "When I ask myself what I consider to be my main duty working on economic problems, it is the introduction of truly exact reasoning and truly exact methods" (OMPD, Diary, Box 13, 19 April 1936). Broadly familiar with the developments in mathematics of Russell, Hilbert, and their contemporaries, he felt that the implications for economic theory were enormous. Indeed, he sought to overcome his deficient training in mathematics by taking private tutorials with Abraham Wald, whom he appointed as researcher at his institute, but he remained personally incapable of taking the theoretical steps that he himself envisioned.[22] Indeed, if any irony characterizes Morgenstern's career, it is that, in his

21. I believe his authorship of the *Theory of Games* created a false impression about his capabilities in this regard. In presentations on the subject for years afterwards, Morgenstern felt obliged to preface his remarks with the observation that they would be nontechnical. Only by 1964, as the dusk of his career approached, did he say bluntly, "Mathematical intuition is a very important thing. I wish I had some" (quoted in Mensch 1966, 100).

22. Morgenstern hired Wald at the suggestion of Karl Menger, Wald's teacher.

continuous agitation for mathematical rigor in economics, he was ultimately calling for a theoretical approach in which thinkers of his own kind would have increasingly little place.

In 1938 he visited the United States with the support of the Carnegie Endowment for International Peace and soon found himself unable to return, having been dismissed by the Nazis in his absence. When offered a three-year appointment in political economy at Princeton, he accepted. His decision to stay in the United States was not one reached suddenly, however, for two years earlier he had written to Frank Knight,

> The idea of being a professor in a large and reputable American University appeals to me very much. You know very well, that I have a high regard for research in social sciences in the States and I have tried to transplant a good deal of the American method to Vienna. . . . Anyway, I wanted to make clear to you that my reaction to your question whether I would like to come to the States is possitive and I wish you would let me know if there are any further developments. (OMPD, Box 6, Correspondence 1928–39, Knight, F., 6 April 1936)

One should note, however, that even though he had been made aware of von Neumann's 1928 paper on games by the Czech mathematician Eduard Cech, he did not read the work until he met von Neumann in Princeton at the end of the 1930s.

Collaboration at Princeton

By 1938, therefore, both Morgenstern and von Neumann were at Princeton. One an economist, the other primarily a mathematician, they differed in many ways, but their common situation likely eclipsed differences that back in Vienna or Göttingen might have been more significant. The two always spoke German together, even when writing in English, and there were dinners and conversation with Einstein, Bohr, and Weyl (Morgenstern 1976). A glowing account of their collaboration on the *Theory of Games and Economic Behavior* (1944) is given in Morgenstern (1976). This reminiscence, while it may capture Morgenstern's nostalgia as he looked back at the high point of his career, is at odds with the version of events actually recorded in his personal journal at the time of his work with von Neumann during World War II.

My concern here is to understand how their book reflects the different interests of the two authors and how it relates to the explorations dis-

cussed above. As mentioned previously, von Neumann and Morgenstern met in the fall of 1938, shortly after Morgenstern arrived at Princeton. Despite several further meetings in the interim, not until April 1940 did they actually discuss games and economics, by which time, we have noted, von Neumann had already arranged his lectures on games for Seattle. During these discussions, von Neumann, we are told, read and praised Morgenstern's "Time Moment in Value Theory." While away, von Neumann did further work on games, moving on to consider four-person games. On his return, in the fall of 1940, von Neumann began to write a paper in two parts synthesizing his work on game theory to date (1940, 1941). This constitutes the theoretical framework of what became the *Theory of Games*. From the beginning of the first part, "General Foundations," the emphasis is clearly on games with more than two players: two-person theory is given short shrift, being quickly used to motivate the notion of the (characteristic) set function v(S). The concepts of strategic equivalence and (in)essentiality are defined, and the three-person game is illustrated in detail, showing how its solution is a system of possible "apportionments" (imputations). The stability evident in this case is considered in the general n-person game and a complete definition is given: no two valuations in the solution dominate each other and every external valuation is dominated by at least one solution member. This, of course, became known as the "Von Neumann–Morgenstern Stable Set" after 1944. In addition, the concept of (in)essentiality is discussed and a graphic illustration of the three-person game provided. All the aforementioned is seen as a prelude to "our ultimate goal, i.e., to find the solutions of all games with $n \geq 4$" (1940, 25). The second part extends the coverage to include non-zero-sum situations, showing how the analysis remains essentially the same, and, adopting a more formal set-theoretic notation, proves some simple theorems on stability and discusses decomposition of games (the properties of games when considered together versus separately). Throughout, the presentation is dense and rigorous and without discussion of economic or any other applications.

While von Neumann was writing this extended paper, however, he was listening to criticisms of conventional economic theory by Morgenstern, who was then preparing his scathing (1941b) critique of Hicks's (1939) *Value and Capital*. Provoked by this, in May 1941 he asked Morgenstern to write a paper illustrating his basic thoughts on economic theory. Within a month, this yielded "Quantitative Implications of Maxims of Behavior" (Morgenstern 1941).

If von Neumann's work above is impressive for its relentless mathematical rigor, the paper by Morgenstern is equally impressive for different reasons. Devoid of mathematics, it offers a skeletal social theory that is both methodologically individualistic and cognizant of the importance of social interaction. His concern, he states at the outset, is to construct a "theory of society," but we will limit ourselves to economics since "there alone the beginnings of a theory of social behavior are discernible" (1). To this extent, a "maxim" may be regarded as a complex plan. Morgenstern distinguishes between two types of maxim. Unrestricted are those whose success or failure is independent of their adoption by other individuals, while restricted are those who are so dependent. A successful economic theory, that is, one that enables prediction, will have to recognize that individuals make decisions whose outcome depends on whether or not similar decisions have been made by others. This is the core of Morgenstern's message.

In the process, Morgenstern conveys to the reader an idea of how he views knowledge, the individual, society, and economic theory. There exists an objective mechanism which underlies the evolution of economic phenomena. Individuals, having less than perfect foresight, have incomplete knowledge of "the facts," and thus act with subjective, rather than objective, rationality. In addition, because all decisions take place in time, and because time changes maxims from unrestricted into restricted ones, its introduction into any theory is "essential" (2). This is emphasized repeatedly by Morgenstern, for example, "Restricted maxims will be applied in succession and have therefore a dynamic character" (21a). The quantitative effects of other individuals' behavior on one's application of restricted maxims may be positive or negative. For example, one's decision to withdraw deposits in order to protect them from bank failure will depend on the number of others making a similar decision. In cases such as this, the rift between subjective and objective rationality may be very large indeed. This raises the importance of the "institutional setting" in which economic decisions are made: regulation may help overcome information deficiencies, thereby reducing these interaction effects. Interestingly, Morgenstern claims that the case for government intervention is made stronger when it serves to enhance inadequate private information, making easier the pursuit of restricted maxims. This, he says, is something not recognized by the laissez-faire doctrine. In general, however, "social progress [might] be presented as a gradual shift from restricted to unrestricted maxims" (6a).

The document is manifestly positivistic: the entire theory is predicated

on the gap between the true mechanism of the economic world and our knowledge of it, both of which are completely quantifiable. The only suggestive mathematics in existence is "the theory of games by J. von Neumann," referring to the paper discussed above, but, perhaps in an attempt to provoke the latter to further efforts, he adds that even this does "not take the problems into consideration which have been described above" (22).

By September 1941, the two decided that their papers (above) should be combined to form a small volume. In an October letter to Frank Aydelotte, director of the IAS, von Neumann wrote, "We hesitated for some time whether to publish it as a paper (in one of the economics periodicals) or as a book. We are now inclined to do the latter since this would free us of limitations in space which would be rather troublesome."[23] It ended up, three years later, as the 635-page tome with which we are now familiar, "a big book, because they wrote it twice, once in symbols for mathematicians and once in prose for economists" (David Blackwell quoted in Albers and Alexanderson 1985, 27). In the introductory chapter, "Formulation of the Economic Problem," Morgenstern offers both a critique of accepted neoclassical theory and a reworked version of the alternative conceptual approach broached in his "maxims" paper. While the wings of Morgenstern's imagination have been clipped this time by the need to have his theoretical hopes conform to the "actually existing mathematics," the grandiloquence remains. The importance of interdependence is argued: when we move from the Robinson Crusoe world to a social group, qualitatively new features enter the picture; these features require a mathematics better adapted than the traditional differential calculus. Throughout, physics is presented as a benchmark in scientific progress which economics should strive to attain. Gone is the emphasis on time and dynamics: "Our theory is thoroughly static" but, as the experience in physics has shown, "it is futile to try to build [a dynamic theory] as long as the static side is not thoroughly understood"

23. Dated 6 October 1941 in VNP, Container 32, File 90, Theory of Games and Economic Behavior. Aydelotte responded, "I was keenly interested in your suggestion that the mathematical theory of games might demand the development of new branches of mathematics running parallel to the impulse which physics gave to mathematics in the seventeenth century. If you can get the mathematicians to take as much interest in that side of your paper as the economists are sure to take in the economic aspects, you would indeed be starting something of first-rate importance" (9 October 1941). As we shall see below, for the next twelve years or so, game theory was a distinctly mathematical, rather than economic, affair.

(44). The rest of the text is a lengthy elaboration of the theory, centering largely on games of three or more persons, with Morgenstern's economic examples appearing in chapter 11.

How do von Neumann and Morgenstern situate their book with regard to the earlier work considered in this article? While Steinhaus, as we know, remained incognito, and therefore could not have been taken into account, even in principle, it is remarkable that none of Borel's work before 1938 is mentioned. The latter volume, furthermore, is briefly referred to twice. The first concerns poker, where Borel's related work is characterized in a footnote as "very instructive, but . . . without a systematic use of any underlying general theory of games" (186, n. 2). The second mentions the first elementary proof of the minimax theorem by Ville in the same volume. The proof used by von Neumann, based on the theory of convex sets, is offered as a further, simplifying step in this process of elementarization: in general, the text presents itself as a revolutionary one, whose relationship to earlier mathematical and economic ideas is one of contrast rather than continuity.

The Period to 1944: Overview

The period leading up to the publication of the *Theory of Games* displays historical features against which the subsequent development of game theory may be juxtaposed and without which it cannot be understood. If one term can possibly capture the twenty-five or so years considered above, it is "fragmentation." From 1921 until the collaboration of von Neumann and Morgenstern, the three interested parties worked in relative isolation. In Poland, Hugo Steinhaus seems to have remained disconnected from and unaware of the contemporaneous writings of both Borel and von Neumann.[24] The relationship between the latter two bears the hallmarks of a certain "standoffishness," if not outright academic antagonism. At no stage, despite their mutuality of interest, did either show

24. Although its broader significance for our story remains a matter of speculation, both Borel and Steinhaus did have one thing in common: a strong interest in naval matters. We have already noted Borel's involvement as Minister of the Navy. Commenting on his own 1925 paper, Steinhaus (1960) wrote, "I was especially interested in naval pursuit. After having found the concepts of minimax and maximin I was well aware that the minimax time of the pursuer is longer or equal to the maximin time of the pursued, but I did not know whether they are equal in all similar games. Consequently, I called 'closed' the games for which there is equality and 'open' the other ones. My pupils in Poland have adopted later this terminology (thus the pursuit of one ship by two is *closed*, and it is *open* when there are three pursuers)" (108).

a willingness to cooperate or exchange ideas. Nor is this particularly surprising. There is a natural spirit of competition felt by mathematicians with regard to their work. Priority debates, for example, reflect this sense of pride and the desire to be given credit for initiative and originality. In von Neumann's case, this natural tendency may have been magnified by two other features. First, as noted above, he was considerably younger than Borel and, while his genius was being recognized, he no doubt relished the prospect of outsmarting somebody of Borel's stature. Second, it reflected the longstanding rivalry in mathematical matters between Göttingen and Paris which, in turn, was in keeping with the volatile nature of Franco-German diplomatic relations.[25] In the work of both Borel and von Neumann, from 1928 onwards, references to the contribution of the other were scant or nonexistent. Despite the fact that each clearly drew on the other's work to some extent, whether in the choice of examples or the basic framework, relations between them remained distant and disconnected. At no stage could anybody have spoken of a game theory "community."

Even in their collaboration on the book during the early 1940s, von Neumann and Morgenstern worked in isolation. For example, neither seems to have been aware of the work in France: in December 1941, Morgenstern *accidentally* discovered Borel's volume (1938) containing the elementary minimax proof by Ville. Neither was von Neumann aware of this work (see Morgenstern 1976, 811; Rellstab 1992). This of course is not too surprising, given the political situation. Even at Princeton, however, they were quite isolated. Through 1941–42, von Neumann gave a few related lectures which were not well attended, and the economics department at Princeton, to Morgenstern's dismay, remained aloof. He speculates that this kind of work may have somehow been inappropriate to the conditions of war then obtaining.[26] That game theory was shaped at Princeton in virtual isolation from the mathematics and economics communities there is also borne out in other accounts. Ted Harris, later to be head of mathematics at RAND, received a Ph.D.

25. Reid's 1970 anecdotes endorse this. For example, the 1909 visit by Poincaré to Göttingen "was an unwelcome reminder that the mathematical world was not a sphere, with its center at Göttingen, but an ellipsoid" (120). Further, in 1917, when Hilbert wrote a memorial for the deceased Gaston Darboux, his house was besieged by a mob of angry students demanding that "the memorial to the 'enemy mathematician' be immediately repudiated by its author and all copies destroyed" (145).

26. Ironically, as we shall see, it was to be the consideration of war that would motivate the theory's early development.

in mathematics in 1947 and recalled that his first encounter with game theory was his chance discovery of the *Theory of Games* in Princeton's bookshop, just before leaving the university. Harris had been unaware of this development during his career at Princeton and had seen von Neumann speak only on ring operators and other topics (Harris 1990). Albert Tucker, student of Solomon Lefschetz and then professor of mathematics at Princeton, similarly did not become interested in game theory until 1948 when George Dantzig invited his participation in a trial project on linear programming being supported by the Office of Naval Research (ONR; Albers 1985, 343). Likewise, Sam Karlin, a fellow graduate with Harris, and soon to be a key game theorist at RAND, consulting from the California Institute of Technology (Cal Tech), had had no exposure to game theory at Princeton. The *Theory of Games*, he argues, appeared to have had little effect on a Princeton mathematics department oriented primarily toward topology and analysis; indeed, Karlin suggests that game theory came into existence there only after he *left* in 1947 (Karlin 1990).

Thus, the *Theory of Games and Economic Behavior*, when it emerged, was without a "natural" audience. Though directed in its rhetoric toward economic theorists, its central ideas were equally novel in the mathematical sense. While the authors hoped that the impact would be greatest in economic theory, they also surmised correctly that some time would pass before the "game" idea became common currency. The second part of this article illustrates the emergence of a game theory community. For the first time, there appeared an extended group, among whose members the exploration of the mathematics of strategic interaction became a *modus vivendi*. What is of particular interest to economists today is the curious role played in the whole affair by the *Theory of Games*. The book certainly signaled to the broader audience in mathematics and economics that a theoretical innovation had taken place. However, a community of "game theorists" emerged *not* through their exploration and elaboration of the "great book," but rather as a result of the role of the older minimax idea in World War II. Postwar mathematicians came to grips with game theory by extending the earlier work of von Neumann and Ville on two-person games, *not* the characteristic function touted as the theoretical centerpiece of the *Theory of Games*.

Part 2: Postwar Consolidation

The significance of wars in determining the direction of research and, therefore, the form of "discovered knowledge" is enormous. In this regard, the effect of World War II was unprecedented. The application of scientific methods to conflict not only destroyed lives and cities more "efficiently" than ever before, but had fundamental repercussions on the way science would be pursued when the conflict ended. And this applied to mathematics and economics as much as to the natural sciences. Shortly after the end of the war, interest in game theory had spread to other mathematicians besides von Neumann. It started to gain respectability as an area of applied mathematics, support for further research in the area was forthcoming, and a self-reproducing community appeared. The entire process represented the stabilization of the mathematics of games. This occurred through the demonstration of the links between games and what was already known, for example, in other areas in mathematics or what had recently been learned in the area of military strategy. It is somewhat ironic that while the grander ambition of the *Theory of Games*, particularly as expressed in Morgenstern's introductory chapter, was temporarily shelved in the process, von Neumann himself was a fundamental force in the entire movement.

I first look at the role of academics in wartime research and then concentrate on two loci of particular interest: the Operations Evaluation Group, attached to the Navy, and the Statistical Research Group, attached primarily to the Air Force and part of the Applied Mathematics Panel. It was in the former that game theory found its first military application, while the latter would form the core of a group of postwar mathematicians centered at Project RAND, devoted to research on games. The work at RAND reflected several influences: the continuation of research on military strategy as demanded at the end of the war; the exploration of games from the perspective of von Neumann's new interest, computation; the exploration of the theoretical links between games and linear programming on the one hand and statistical decision theory on the other. Out of this milieu came the first textbook, the first layman's version of game theory, and the personnel who would carry the theory back to campus mathematics departments, as they were revived by the ONR.

Academics and War

The German invasion of Poland in 1939 highlighted the fact that there were "grave shortcomings in the organisation of science for war" in the United States (Baxter 1946, 11). For "science" one might as well read "scholarship" in general, for the ensuing half decade saw the mobilization of scholars of all shades, physicists and humanists alike. For example, the National Defense Research Committee (NDRC) coordinated the contracting of federal research in the development of equipment and war techniques by university physicists, chemists, engineers, and mathematicians.[27] The Manhattan Project, developing the atomic bomb at Los Alamos, drew on physicists including Oppenheimer, Fermi, Teller, Wigner, and von Neumann. Nor was it all "hard science": without a centralized agency to collect and evaluate foreign intelligence, "the inadequacy of the American intelligence apparatus had become conspicuous and critical" (Katz 1989, 2). The response was the Office of Strategic Services (OSS), which drew on academic economists, historians, and sociologists and ultimately became the CIA.

The wartime mathematical activity of interest to us fell under the control of the NDRC. It was established in June 1940 by order of President Roosevelt. Its function was to "correlate governmental and civil research in the fields of military importance outside of aeronautics" and it was chaired by Vannevar Bush (Baxter 1946, 15).[28] The NDRC had five internal divisions catering, respectively, to perceived needs in armor and ordnance; bombs, fuels, and chemicals; communication and transportation; detection, controls, and instruments; patents and inventions. These proceeded by administering contracts with universities and other institutions for defense research. Because of the intense activity and the attendant sense of urgency, these university departments experienced considerable upheaval in the process. For example, the Radiation Laboratory at MIT at its peak employed over four thousand people from all over the country. Its key contribution was the development of shorter-wave radar which yielded better resolution on the radar screen. Other large laboratories, developing underwater sound, were operated in Cali-

27. Baxter 1946 is the official history of the NDRC. Its coverage of the work of the Applied Mathematics Panel is, however, negligible.

28. Aeronautics was in the domain of the National Advisory Committee for Aeronautics (NACA), established in 1938.

fornia and at Columbia, Harvard, and the Woods Hole Oceanographic Institute.

Under the umbrella of the NDRC, mathematicians were engaged in the application of mathematics and probability toward improving the effectiveness of weapons, either through better use of existing ones or the design of new types. They had shown an early keenness to become involved: the American Mathematical Society and the Mathematics Association of America had, in 1940, jointly appointed a War Preparedness Committee with subcommittees on research, preparation for war, and education for service.[29] The two NDRC groups of interest from the point of view of game theory are the Statistical Research Group (SRG) at Columbia, whose work was used primarily by the Army Air Force, and the Anti-Submarine Warfare Operations Research Group (ASWORG) located in Boston and attached to the Navy. The reader should bear in mind, however, that the bureaucratic divisions that seem so neat on paper were, in practice, much less clear: everybody knew everybody else and, to the extent permitted by secrecy, there was much exchange and interaction.

The Statistical Research Group

In late 1942, the NDRC was completely reorganized and the activities of mathematicians were grouped under the control of the Applied Mathematics Panel (AMP), headed by Warren Weaver, with technical assistant Mina Rees.[30] Policy decisions were guided by a committee including R. Courant, G. C. Evans, T. C. Fry, L. M. Graves, O. Veblen, Marston Morse, Sam Wilks, and Weaver. Courant, having left Göttingen in 1933, had established the Courant Institute at NYU. Veblen and Morse were both colleagues at the Institute for Advanced Study with von Neumann, who also served as advisor to the committee (Rees 1980, 609). The AMP set up contracts with eleven universities including Brown, Berkeley, Columbia, Harvard, NYU, Northwestern, and Princeton. Two broad categories of work can be identified among the projects undertaken: first,

29. Among the consultants to the research group were von Neumann (ballistics), Norbert Wiener (computation), and Samuel S. Wilks (probability and statistics) (see Morse 1941, 296).

30. Rees (Ph.D. Mathematics, Chicago 1931) taught at Hunter College until 1943. After the dissolution of the AMP in 1945, she became head of the Mathematics Branch of the Office for Naval Research, which subsequently supported much mathematical activity, including game theory, in universities all over the United States (see "Mina Rees" in Albers 1985).

fluid mechanics, classical dynamics, mechanics of deformable media, and air warfare; second, probability and statistics.

In the first category, the Applied Mathematics Groups operated at NYU, Brown, and Columbia. At NYU, a group under Courant worked on gas dynamics and, in particular, on the theory of explosions both in the air and underwater. This resulted in the *Shock Wave Manual* (1944) and its successor, *Supersonic Flow and Shock Waves* (1948). At Brown, the focus was classical dynamics and the mechanics of deformable media, under W. Praeger. The Columbia group was concerned with aerial warfare, in particular air-to-air gunnery, a departure from the classical applied mathematics of the first two above (Rees 1980, 612). Here the concerns included aeroballistics (the motion of a projectile from an airborne gun), the design of different types of weapon sights, and pursuit curve theory.

The principal work in the second category, probability and statistics, was done by Statistical Research Groups operating at Columbia, Princeton, and Berkeley. The first, the largest and most important of these, was run by Allen Wallis with Harold Hotelling as principal investigator.[31] Samuel Wilks, at Princeton, headed the second group, while Jerzy Neyman at Berkeley ran the third. Wallis's group occupied a portion of a building at West 118 Street, New York, which also had as tenants Columbia's Applied Mathematics Group, mentioned above, and the Strategic Bombing Section of Wilks's Princeton group, run by John D. Williams. Both Williams and Wilks, it appears, interacted very closely with Wallis. During this time, Wallis and Hotelling gathered around them an exceptionally capable group of mathematicians and statisticians including Abraham Wald, J. Wolfowitz, Milton Friedman, James Savage, Abraham Girschick, Frederick Mosteller, and George Stigler (see Wallis 1980). Wald had left Vienna in 1938 with an invitation to the Cowles Commission arranged by Oskar Morgenstern, and then moved to teach at Columbia (Freeman 1968). Wolfowitz, Friedman, and Girschick had all been Hotelling's students during the 1930s.[32] Savage, after

31. At the time that Weaver was still head of the Fire Control division of the NDRC, in 1941, Wallis had left Stanford for the Office of Price Administration (OPA) in Washington (Wallis 1980). At Hotelling's suggestion, Weaver approached Wallis in mid-1942, suggesting that he lead the Columbia SRG, which he did from then until its dissolution in 1945.

32. Girschick, in fact, only spent 1944–45 at the SRG, the remainder of 1939–46 being spent as principal statistician for the Bureau of Agricultural Economics. Affected by his work with Wald, however, after a brief stay at the Bureau of the Census, he went to RAND soon after its creation in 1946 (see *International Encyclopaedia of Statistics*, 398–99).

receiving a Ph.D. from Michigan in 1941, had gone first to the IAS as von Neumann's assistant and then to Cornell and Brown before working for Weaver (Wallis 1980).

As with their Applied Mathematics neighbors, the study of aerial combat was central to the work of the Columbia SRG. It is impossible to adequately describe the 572 reports, memoranda, and substantive letters that resulted, but a sample will give some idea. An early study on alternative ways of placing machine guns on a fighter aircraft involved studying the geometry and tactics of aerial combat. This, in turn, led to work on antiaircraft weapons, aircraft turret sights, and dispersion of aircraft machine guns. A second broad area was the design of the optimal lead angles of aircraft torpedo salvos. This involved the interpretation of photographs of Japanese destroyers to glean information on speed and turning radius. The third field concerned the development of inspection and testing procedures. Out of discussions on the sampling of equipment came the idea that perhaps testing could be stopped before the prescribed sample had all been used, if the information gained thus far somehow suggested that adequate testing had been done. The ratification and formalization of this idea became sequential analysis, developed by Wald.[33] Although Wald himself had already used the minimax idea in his 1939 paper on statistical decision theory, game theory, it appears, was not used as an analytical tool by the group.[34]

One area where the mathematics of games *did* find some use was at ASWORG. Headed by Philip Morse,[35] physicist at MIT, they performed, as their name suggests, operations research directed toward antisubmarine warfare (see Morse 1948, 1951).[36] Morse's group was constituted primarily in response to the presence of German U-boats along the north Atlantic convoy shipping routes. Analyzing data sent in from widespread naval bases, the group made recommendations about the use of equipment in the pursuit of submarines. For example, taking account of the detection range of equipment and the rate at which a plane or ship could patrol a given area, they calculated a "sighting likelihood curve," which related the probability of sighting to range, visibility, and other factors. Simple probability theory was used to recommend the best way, given the limited resources, to carry out radar and sonar searches of areas of the sea.

33. The Quartermaster Corps of the Navy apparently made significant economies in inspection using these procedures (see Wallis 1980).

34. Savage, too, was familiar with games, to some extent: "[At lunch one day], Wald discussed some of his ideas on decision theory and Savage . . . remarked that he knew a rather obscure paper that would interest Wald, namely, VonNeumann's 1928 paper on games. Wald laughed and said that some of his ideas were based on that paper" (Wallis 1980, 334).

35. Morse had already done some research work for the Radiation Laboratory and for the Army Air Force (see Tidman 1984, 34).

36. Morse's discussion appears in two sources. Morse 1948 is his Josiah Willard Gibbs lecture to the American Mathematical Society. Morse and Kimball 1951 is the declassified version of a report by the same authors written in 1946 (see Tidman 1984, 102–3).

ASWORG used game-theoretic analysis in two applications. The first of these was the barrier patrol in which a narrow seaway is patrolled regularly by aircraft in order to prevent the passage of submarines. For example, the Straits of Gibraltar were monitored to keep U-boats out of the Mediterranean, and the area between Brazil and Ascension Island was patrolled in order to catch German ships returning with tin and rubber from Japan and Malaya. The passing submarine, incapable of remaining submerged during its trip through the straits, must choose a point at which to surface. The patrolling airplane must choose a point at which to cross the sound in surveillance. The latter wishes to make contact while the former wishes to avoid it. In order not to have its move predicted, a mixed strategy is preferred; otherwise, the plane would simply choose the point at which it knew the U-boat was going to surface, or the submarine would simply avoid surfacing at the point at which it knew the plane was going to cross. The game is a continuous one: each player must choose a point on the strait of fixed, finite length and does so by applying a probability distribution to the spectrum of possible points. Morse shows how to find the minimax solution to the problem (see 1948, 613–19; 1951, 105–9). When applied, this solution ensures that the lowest probability of contact that the plane can ensure itself is the highest risk of contact that the submarine will have to face. The second application is in the allocation of forces into strategical and tactical components by two opposing armies: Blue and Red.[37] Without going into details, I again note that the minimax solution provides a "safe" option for both sides, given many simplifying assumptions about the relative effectiveness of opponents' forces against each other's production and each other's armies. Any side's choice of tactical force, relative to strategic force, will depend positively on both the opponent's total forces and the opponent's relative weakness in production (see 1951, 73–77).

37. Manichean mathematics! The reader should have little difficulty identifying who's who in this escapade!

Morse finishes with a rallying call,

> The difficulties of solving such problems are not ones of tedious detail, but often due to lack of fundamental techniques. Much more basic research must be carried out before many problems of practical importance can be solved. (1948, 619)

> The studies of von Neumann and Morgenstern show that there are solutions to each problem and show the general nature of these solutions. They do not show, however, the technique for obtaining a solution. . . . A great deal more work needs to be done in finding solutions to various examples before we can say that we know the subject thoroughly. . . . It is to be hoped that further mathematical research can be undertaken on this interesting and fruitful subject. (1951, 109)

The alert reader will observe that the above is largely an unwitting "resurrection" of the prewar work of both Borel and Ville. The Blue and Red force allocation problem is essentially the sort of application suggested, but not analyzed, by Borel in 1938. The barrier patrol is simply a game on the unit square, the solution of which is first offered by Ville, as discussed above. Von Neumann, of course, was familiar with all of this, having been shown it by Morgenstern in 1941 while they wrote their book (see above). He was also close to the applied mathematics being done during the war.[38] He was also completely capable, however, of independently seeing applications and finding solutions to problems presented by Morse and others.

As Morse probably knew, his 1948 talk simply endorsed an agenda for postwar research on games that was already in the making.[39] It was

38. Von Neumann began consulting to governmental organizations after he became a naturalized U.S. citizen in 1937. His first contact was with the Ballistics Research Laboratory (Army Ordnance Department) at Aberdeen, Maryland, to which he was probably introduced by Oswald Veblen. From September 1941 to September 1942 he was attached to Division 8 of the NDRC, working on detonation waves, or how to arrange explosive charges so as to direct and maximize the blast. From September 1942 to July 1943, he advised the Mine Warfare Section of the Navy Bureau of Ordnance on the operations research of mine warfare. This is the work that took him to Washington, D.C., during the last few months of 1942 and to England for the first half of 1943 while working on the *Theory of Games*. In late 1943 and early 1944, he was back at Aberdeen working with Theodore von Karman on aerodynamics and continued to advise the Navy on mines. Beginning in September 1943, he was consultant at Los Alamos on the "Bomb." (See Aspray 1990, 25–27.) This suggests that his work with Morgenstern was a "side interest." From 1943 onward, games would be of interest to von Neumann primarily to the extent that they related to computation.

39. Ed Paxson recalls, "I bought my copy of TGEB [*Theory of Games*] in 1946. I was

in this context of national defense that the theory first gained broader recognition.

Project RAND

As the war ended in 1945, discussions in the War Department, Office for Scientific Research and Development, and branches of the military centered on the prospective return to the campus of the many academics that had been involved in military-oriented research. The links forged by war were about to be dissolved by the peace, and there was a concern that much accumulated experience, along with the possibility for future cooperation, would simply be lost. In March 1946, following a discussion of this type among "Hap" Arnold (Army Air Force Chief of Staff), Dr. Ed Bowles (scientific consultant to the War Department), and some engineers from the Douglas Aircraft Company, Arnold committed $10 million of research funds remaining from the war. Thus Project RAND was born.[40] Initially located under the Douglas roof and then moving to its own quarters in Santa Monica, this group of physicists, mathematicians, and engineers was to conduct a program of research on "intercontinental warfare, other than surface, with the object of advising the Army Air Forces on devices and techniques" (46). In fact the group had more flexibility than this suggests, and Arnold, initially to the disgruntlement of his own military minions, ensured that they were not tied to time or forced to work on immediately applicable ideas. A postwar venue was thus available in which research was easily pursued, was very well paid, and was moreover free of the usual academic duties such as teaching and administration. It was the institutional stabilization of the military-academia symbiosis that had begun during the war.

Reflecting their ostensible purpose, the mathematics group at RAND was labeled the section for the Evaluation of Military Worth (Harris 1990). Its leader was John Williams, who had headed the New York

working at the Naval Ordnance Test Station, China Lake, California. Immediately fascinated, I formulated what was later to be called a differential game. This was a real problem, a duel between a destroyer firing at a maneuvering submarine, with allowance for denial of sonar coverage in the destroyer's wake.

"In November of 1946, I spent a day in Princeton with Johnny von Neumann working on this problem. He sketched an approach, and even did some machine coding for his yet unborn machine" (quoted in Shubik 1982, 414).

40. The engineers had advised Bowles in a successful study of the B-29 bomber. Arnold had been supportive of such work and wished to see it continued (see Smith 1966, passim).

branch of Wilks's Princeton Statistical Research Group. Among the others to arrive early were Edwin Paxson (see note 39); Morris "Abe" Girschick, who had also been at the Columbia SRG; Olaf Helmer, who had worked for the AMP in New York; Melvin Dresher, who came from the Office of Price Administration, where Wallis had been before the war; and J. C. C. McKinsey, another mathematician trained at Columbia in the 1930s. Girschick, Helmer, and Dresher were also émigrés: from Russia (1932), Germany (1936), and Poland (1932) respectively (see Kaplan 1983). Exerting a strong influence from a distance were von Neumann, Weaver, and Wilks, all of whom became RAND consultants.[41] Wilks, back at Princeton, directed his best Ph.D. students, among them Ted Harris, toward Williams. Samuel Karlin arrived at the Cal Tech mathematics department from Princeton in 1947 and became consultant to RAND the following summer at the suggestion of his chairman, Henry Bohnenblust, himself a RAND consultant and veteran of wartime operations research in England. Present too were Lloyd Shapley, then with only a B.A. from Harvard and, part-time, David Blackwell of Howard University.[42]

RAND was in this period a center of intellectual ferment. In addition to a constant flow of consultants, lengthy summer sessions were held, to which anyone of note or ability who might have something to contribute was invited.[43] Until the mid-1950s, Santa Monica was the point of reference for those working on matters related to game theory at Princeton, Michigan, and in other military-sponsored research institutions. This decade saw the "stabilization" of the mathematics of games through the elaboration of its links with other areas in mathematics, with linear programming, and with statistics: the theory became one strut in the framework of ideas developed under immediate postwar military patronage. The concentration on two-person games reflected its relation to military conflict as revealed during the war.[44] As I discuss below, it also resonated with ideas in activity analysis (linear programming) and statistics, and with von Neumann's new interest in computation, all of which were eagerly sponsored by the military. I conclude part 2 by briefly illustrating this stabilization process. This is done to convey to the reader a sense of the broad agenda in which game theory became central; the finer detail must await further treatment.

41. At $200 per month for several hours' work, they were well compensated. Von Neumann in particular was much sought after by many groups: it wasn't flattery when Williams told him, "Paxson, Helmer and all the rest are going to be especially nice to me if I succeed in getting you on the team" (Letter 16 December 1947, VNP, Container 15, File RAND Corp. Contract Corresp.).

42. Karlin would later persuade the exceptionally talented Shapley to return to Princeton for a Ph.D. in mathematics, which he did toward the end of the 1940s (Karlin 1990). Blackwell had met Girschick in 1945 in Washington, D.C., where the latter had lectured on Wald's sequential analysis. There began a collaboration which resulted in, *inter alia*, *Theory of Games and Statistical Decisions*.

43. Beginning in 1948, months-long gatherings were held for the discussion of games and related matters. Those participating were either RAND staff, regular consultants, or visitors from outside. The 1951 guest list, for example, included M. Abramovitz, K. Arrow, E. Domar, J. Duesenberry, J. Marschak, O. Morgenstern, P. Morse, P. Samuelson, and R. Solow. These convocations were generally of the "Secret," if not "Top Secret," variety, and Air Force clearance was required for attendance (for example, see letter of K. E. Wells to O. Morgenstern, 15 June 1950, OMPD, Box 14, File RAND).

The Stabilization of Games

Beginning in 1946 with Loomis's completely algebraic proof of the minimax theorem, there issued forth a stream of further proofs, largely falling into two categories. The first of these rest on fixed point theorems, or iterative procedures, and the second on the theory of convex sets (see Kuhn 1952, 71ff.; Luce and Raiffa 1957, Appendix 2). Included in the former are Weyl (1950) and Gale, Kuhn, and Tucker (1950), while the latter includes Nash's (1950) proof of the existence of an equilibrium point for all n-person games, of which the minimax is a particular case.[45] Other work utilized the geometric properties of the minimax solution to derive inductive proofs (see Luce and Raiffa 1957, Appendixes 3 and 4). In Dresher et al. 1948, the first collective publication by the RAND mathematics group, the issue of the actual calculation of solutions by matrix methods is raised, and the properties of infinite games under various assumptions about the convexity and continuity of the payoff function are presented. The issue of calculating solutions was particularly important for von Neumann, given his new interest in computation, discussed below. The exploration of continuous games with

44. Mirowski (1991) argues that all of game theory in the immediate postwar period reflected the military influence. Making this case persuasively necessitates showing how all related pursuits such as linear programming, statistical decision theory, experimental games, and computation were part of the military design. One should also show how the work on cooperative games, in Nash 1950 and Thrall et al. 1954, was more the exception than the rule.

45. For a good discussion of various proofs, the reader is directed to Kuhn 1952, his lecture notes on games.

various types of payoff functions was related to hypothetical duels of the bomber-fighter type, and so held the promise of future military value. They also constituted, however, mathematically intriguing problems and offered endless theorem-solving possibilities to the period's most capable mathematicians.[46]

Directly related to the problem of proving the existence of an equilibrium is that of finding, or calculating, the solution. In Kuhn and Tucker (1950) this had become "the principal outstanding problem of zero-sum two-person games" (viii). Although it was not easy for the busy von Neumann to travel to Santa Monica,[47] he was particularly interested in the work linking games and computation being done there. Writing to Warren Weaver in 1948, he said,

> I was very glad to see your comments on RAND's work on the theory of two-person games. I have seen several of their reports, and I need not tell you that I am also very much interested in the fact that some of their attention is now going to this subject. . . . I have spent a good deal of time lately on trying to find numerical methods for determining "optimum strategies" for two-person games. I would like to get such methods which are usable on an electronic machine of the variety which we are planning, and I think that the procedures that I contemplate will work for games up to a few hundred strategies. (1 March 1948, VNP Box 32, File Correspondence W. Weaver)[48]

Indeed, it could well be argued that he remained actively interested in games only to the extent that they related to his work on computation: of his four subsequent papers on the subject, one is concerned with numerical methods for calculating the optimal strategy (1954) and another (Brown and von Neumann) offers a constructive proof intended for "utilization when actually computing the solutions of specific games"(73).[49]

46. The question of what "really motivated" those working on game theory during this period recurs periodically. Personally, I believe motives were mixed. Dresher's declassified version of an earlier RAND volume (1961) is virtually exclusively devoted to games capable, in principle, of military application. Williams (1954) is also particularly concerned with the value of games in strategic considerations. Others, however, such as McKinsey (1952) and Shapley, in general, appear much less "gung-ho" and concerned above all with the mathematics.

47. In October 1951, von Neumann's consulting fee was doubled so that he might pay more attention to RAND than he had hitherto done. (See letter from Alex Mood [RAND] to von Neumann, 1 October 1951, VNP Box 15, Folder RAND.)

48. For various requests for advice from McKinsey and Paxson to von Neumann, see Letters, VNP, Box 25, File RAND Corporation.

49. For an exploration of von Neumann's work in computation see Aspray 1990. In 1943, while on a research trip to Britain for the Navy, von Neumann wrote to Veblen of his newly developed "obscene interest in computational techniques" (quoted in Aspray 1990, 27). That game theory could be sustained despite his diminished interest was one sign of its maturation as a field of applied mathematics.

The theoretical links between games and other areas such as statistics and linear programming were soon established. Following Wald 1945, which showed the application of the minimax theorem in statistical decision theory, the decision process being characterized as a game against Nature, these ideas were further developed in Arrow, Blackwell, and Girschick 1949 and Blackwell and Girschick 1954. In Koopmans 1951, both Dantzig and Gale, Kuhn, and Tucker demonstrate the equivalence of the tasks of solving a game and a linear programming problem, thereby drawing on activity analysis to provide *constructive* existence proofs in game theory. At RAND, 1952 also saw the first work in experimentation on games (see Flood 1958). With regard to the question of military application, both Haywood 1951 and Caywood and Thomas 1955 offer simple, stylized examples. These illustrate better the *confidence* the military held in the mathematics as a tool rather than the direct *usefulness* of the ideas in this context. Finally, out of this milieu in the early 1950s came the first textbook on game theory, McKinsey 1952, and the first popularizations, McDonald 1950 and Williams 1954. While the latter books became immediately popular, McKinsey's book was perhaps a little too austere for the economics readership of the 1950s and was supplanted several years later by the more "user-friendly" Luce and Raiffa 1957.[50]

Conclusion

This paper supports a relatively simple claim about the evolution of what we now know as game theory. For the thirty-five years preceding the end of World War II, the thinking on games was limited, disparate, and disconnected. In the absence of anything that could be regarded as discourse, the "theory," if it could be called such, was of limited meaning to a limited number of people. What ultimately gave it life was not Morgenstern's hope for its transformation of economics, but the changed postwar environment for mathematical research, the strong military interest in the theory, and the attention given to it by the mathematicians under their patronage. The confluence of these various streams created the context in which the theory was first stabilized. There re-

50. I owe this observation about the early textbooks to William Riker.

mains to be written a historical account of the passage of game theory out of military research into various universities with the encouragement of the ONR, its assimilation into political and biological science, and, above all, the halting process by which it gradually transformed the canon in microeconomics.

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