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From Parlor Games to Social Science: von Neumann, Morgenstern, and the Creation of Game Theory 1928–1944

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I have the impression that [economics] is not yet ripe . . . not yet fully enough understood . . . to be reduced to a small number of fundamental postulates—like geometry or mechanics
John von Neumann to Abraham Flexner, May 25, 1934

Economists simply don't know what science means. I am quite disgusted with all of this rubbish.—I am more and more of the opinion that Keynes is a scientific charlatan, and his followers not even that
Oskar Morgenstern, Diary, April-May, 1942

All these considerations illustrate once more what a complexity of theoretical forms must be expected in social theory. . . . The emphasis on mathematical methods seems to be shifted more towards combinatorics and set theory—and away from the algorithm of differential equations which dominate mathematical physics
von Neumann and Morgenstern, 1944, p. 45

I. *Introduction*

THE READERS of this journal will need little reminder of the extent to which game-theoretic methods have permeated economic analysis. The theory has been applied to a range of theoretical fields, has been the driving force behind the emergence of the theory of auctions and contracts, and has been essential to the rise of experimental economics. Beyond economics, the adoption of the "game" metaphor has changed the construction of theory in political science and evolu-

tionary biology (see William Riker and Peter Ordeshook 1973; John Maynard Smith 1982).

To the extent that this intellectual sea change may be traced to any particular event, the obvious landmark was the appearance, 50 years ago, of von Neumann and Morgenstern's *Theory of Games and Economic Behavior*. It was this book which shaped the subsequent work of Nash, Lloyd Shapley, Robert Aumann, and Reinhard Selten, to name just a few of those whose contributions have had an impact on contemporary economics, and in political science and evolutionary biology, respectively, the ideas of Riker and Maynard Smith evolved in response to this book. With the publication of the *Theory of Games*, it appears, something important began to unfold at a *pan-disciplinary* level, the effects of which are still working themselves through.

Although it is perhaps too early to treat that shift in its entirety, we are well placed, 50 years after the fundamental break of 1944, to consider the creation of game theory in historical and intellectual perspective. What was it that led a Hungarian mathematician and a Viennese economist to a wartime collaboration at Princeton? What was the relationship between the theory of games and contemporaneous work in economics, mathematics, and science? And between von Neumann and Morgenstern, exactly *who* contributed *what* to the creation of the theory? In what follows, we tackle all of these questions, concentrating on two broad themes.

The first concerns the details of intellectual biography. For von Neumann, the key contextual features were Berlin mathematical economics and related debate over socialism and capitalism; the crisis in the foundations of mathematics; and a desire to reform social theory in line with developments in the use of mathematics as the language of scientific models. Morgenstern began with a typically Austrian concern for issues of psychology, foresight, and equilibrium in economic theory, but, under the influence of his contemporaries in logic and mathematics, found himself playing the role of intellectual intermediary, precariously straddling the then-quite-separate areas of economics and philosophical logic. Drawing on Morgenstern's personal diaries and other unpublished material, we discuss in some detail their collaboration on the 1944 book and von Neumann's views on what game theory was intended to achieve. This brings us to the second theme: the authors' "radicalism." While the virtual transformation of the microeconomic canon would suggest that the economics profession has now learnt to take game theory for granted, it is important to remember that von Neumann and Morgenstern's ideas were conceived as quite a blistering *attack* on the Hicks-Samuelson variant of neoclassicism then dominant. In what follows, we bring to the surface the key elements of their criticism. A third subtheme, broached toward the end of the paper, concerns the place of game theory in the history of science: we contend that game theory, the application of set theory and combinatorial methods in economics, is best understood as part of the general emergence in the 20th century of *structuralist* analysis across the scientific spectrum. The emergence of the synchronic, formal, logical analysis of structure was shared by a range of disciplines from linguistics to anthropology to physics, and, in economics, game theory was the manifestation of this change.

In connection with this story, many readers will remember Morgenstern's own account, in this *Journal* in 1976, of his collaboration with von Neumann (see Morgenstern 1976). Understandably, but regrettably, that reminiscence sacrifices some of the historical complexity of the

run-up to 1944. In particular, Morgenstern opted to illustrate the extent to which his early scientific interests had constituted conceptual *anticipations* of game theory, and thus ended up portraying his early theoretical concerns as having a "center of gravity" located, not only several years into the unknown future in a mathematical area in which he had no training, but on the other side of the Atlantic with someone he did not then know. His interpretation highlights the truly immense difficulty of getting beyond the looming presence of the *Theory of Games* so as to view pre-1944 developments in context. This we try consciously to overcome in the account that follows.

II. *Form as Content: John von Neumann, Mathematician*

Born in Budapest, Hungary, in 1903, Jansci von Neumann showed signs of precociousness in mathematics in secondary school, and continued in that field, enrolling at the University of Budapest in 1921. Once there, he chose not to attend lectures, and instead spent 1921–23 at the University of Berlin, taking courses in physics, including statistical mechanics from Albert Einstein, and returning to Budapest only to take exams. During this period, he also made contact with David Hilbert, at Göttingen, the "mecca of German mathematics," and worked close to him on such topics as set theory and foundations of mathematics, Hilbert space theory, operator theory, and the mathematical foundations of quantum mechanics. From Berlin, he went to the Swiss Federal Institute of Technology at Zurich, where he took a degree in chemical engineering in 1925. He received his doctorate in mathematics from Budapest in 1926, and, having spent the following academic year as Rockefeller Fellow at Göttingen, took an appointment as *Privatdozent* in mathematics at the University of Berlin in 1927. Following a semester at the Princeton mathematics department in 1930, he moved permanently in 1933 to the nearby, and then recently formed, Institute for Advanced Study. Until his death in 1957, his involvements were wide, ranging from continued work on mathematical physics, and extensive military consulting on weapons deployment, including work on the atomic bomb at Los Alamos, to foundational research on digital computing and cellular automata (see William Aspray 1990; Paul Halmos 1973; Steve Heims 1980; Harold Kuhn and Albert Tucker 1958; Norman Macrae 1992).

II.1 *Homo Ludens*

That von Neumann turned his attention to the mathematical analysis of games, producing the minimax theorem in 1926, was not so much the result of a detached moment of inspiration, as a reflection of the fact that a number of his peers were doing similar work at the same time. In fact, in the first decades of this century, there existed among Hungarian and German mathematicians something of a "conversation" about the mathematics of games, something which must be seen in the context of the period's mathematical history. Two features of the latter are worth noting: first, the attempt to establish various areas of mathematics on a secure axiomatic basis, central to the so-called Hilbert program at Göttingen; and, second, an associated imperialistic drive to show how mathematical formalization could constitute a widely applicable tool of explanation, even in areas that were hitherto deemed unapproachable in mathematical terms.

The Hilbert program emerged at the turn of the century as a response to a perceived crisis in mathematics, a sense of drift and foundational uncertainty, which, for simplicity, we may trace to the

discovery of several paradoxes and antimonies in set theory by Georg Cantor (1845–1918) and Bertrand Russell (1872–1970).[1] The effect was to drive mathematicians to "clean up" Cantorian set theory, to establish it on a firm axiomatic basis, on the foundation of a limited number of postulates, and those primarily involved in axiomatic set theory were Ernest Zermelo (1871–1956) and Abraham Fraenkel (1891–1965), followed by Paul Bernays, Kurt Gödel, and von Neumann. In 1928 alone, the latter published three papers on set theory. Beyond set theory, the shift toward axiomatics was evident in Hilbert's 1899 construction of the axioms of Euclidean geometry, in which he showed which groups of basic postulates were necessary for the completeness and consistency of that system. Importantly, the relaxation of one of these axioms, the parallel postulate, was shown to give rise to another geometric system, complete and consistent, yet in contradiction with Euclid's, a world at odds with experience, yet perfectly acceptable from a mathematical point of view. As Hermann Weyl (1949, p. 628) points out, this marked an important shift in emphasis toward *abstraction* in mathematics: a movement away from intuitive content—in this case, our daily world of flat surfaces and straight lines—toward a situation in which mathematical terms were leached of direct "external" empirical content, and simply defined axiomatically within the context of the theory. With Hilbert, Weyl remarks, "the question of truth [was] then shifted into the question of consistency" (1949, p. 630): the era of mathematical formalism had arrived.

The second, imperialistic, element concerns the success of Hilbert and others in extending the axiomatic approach to a range of areas, the most obvious being physics—the mathematical foundations of the kinetic theory of gases and quantum mechanics—less obvious being such social activities as parlor games. In this context, the first significant step was that taken by Hilbert's Göttingen colleague and set theorist, Zermelo, who, in 1912, turned his attention to the mathematics of chess. In a lecture, "On the Application of Set Theory to the Theory of Chess," delivered to the International Congress of Mathematicians at Cambridge, he presented an inductive proof that the outcome of chess is strictly determined, i.e., either white can force a win, or black can force a win, or both sides can force at least a draw (Aumann 1989, p. 1). While offering the entire exercise in the context of contemporaneous work on set theory, Zermelo also presents it as part of the attempt to push mathematics into as many realms as possible and to show how other phenomena, be they psychological or physical, may ultimately be "explained" by rendering them accessible to mathematical interpretation. Of the analysis of chess, he says that

> it is not dealing with the practical method for games, but rather is simply giving an answer to the following question: can the value of a particular feasible position in a game for one of the players be mathematically and objectively decided, or can it at least be defined without resorting to more subjective psychological concepts? (1912, p. 501)

This paper gave rise to further work on chess by a number of mathematicians close to Zermelo, including the lesser-known Denes König and Laszlo Kalmar, and, of course, von Neumann, all of whom were familiar with each other's

[1] These invariably spring from the claims to inclusivity or exclusivity which are inherent in set theory. A popular one due to Russell is the following. In a certain village, the barber shaves *all* those men, and only those, who do not shave themselves. Who shaves the barber? Any answer given is contradictory. See Edna Kramer (1982, pp. 594ff).

work (see König 1927; Kalmar 1927–28). In short, chess became a well-defined topic in German-speaking mathematical circles, and it was this interest in the relationship between set theory and parlor games which formed the intellectual setting for von Neumann's work on the minimax theorem. This he first presented to the Göttingen Mathematical Society in December 1926.[2] The paper contains mainly a long and difficult existence proof, based on functional calculus and topology, of the "solution" for all two-person, zero-sum, games with a finite number of strategies. The mathematical concept of a game is completely axiomatized and two examples are offered of simple zero-sum games with solutions only in mixed strategies, "matching pennies" and "paper, stone, scissors." He also treats the three-person zero-sum game, showing how the possibility of coalition formation introduces into such games a measure of indeterminacy, or "struggle." In preliminary remarks on games with more than three players, he introduces 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. We have seen that this is true for $n = 2, 3$; for $n > 3$ a general proof has yet to be found." If this is the case, he concludes, then "we have brought all games of strategy into a natural and final normal form" (p. 41). As in the papers by Zermelo and the others, mathematics is seen as something capable of penetrating the psychology of gaming: "a later publication," he says in the conclusion, "will contain numerical calculations of some well-known two-person games (poker, though with certain schematical simplifications, baccarat)" in which

> The agreement of the results with the well-known rules of thumb of the games (e.g., proof of the necessity to "bluff" in poker) may be regarded as an empirical corroboration of the results of our theory. ([1928b] 1959, p. 42)

Significantly, von Neumann also drew an implicit parallel between the probabilistic nature of social interaction and probabilism in physics:

> Although . . . chance was eliminated from the games of strategy under consideration (by introducting expected values and eliminating "draws"), it has now made a spontaneous reappearance. Even if the rules of the game do not contain any elements of "hazard" (i.e., no draws from urns) . . . in specifying the rules of behavior for the players it becomes imperative to reconsider the element of "hazard." The dependence on chance (the "statistical" element) 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: even if the formal rules contain no trace of it, it still will assert itself. ([1928b] 1959, p. 26, emphasis added)

The implicit reference here is to his work on mathematical physics, which ranged from statistical mechanics to Werner Heisenberg's probabilistic interpretation of quantum activity. The latter had been constructed three years previously at Göttingen in 1925, just before the minimax idea was presented. A substantial part of von Neumann's work at the time, culminating in his 1932 *Mathematical Foundations of Quantum Mechanics*, involved constructing a mathematical basis for Heisenberg's theory. Now, with the minimax theorem, the prevailing probabilistic view of the world in physics was being reflected in von Neumann's theory of human interaction.

[2] See von Neumann ([1928b] 1959, p. 13, n. 1). The paper was received by *Mathematische Annalen* on July 24, 1927. Von Neumann's (1928a) note to the French *Académie des Sciences* reveals his knowledge of the related work by Emile Borel in the 1920s (see Leonard 1992).

This is our first hint of the extent to which von Neumann's view of the application of mathematics to the social domain was conditioned by the philosophy and standards of physical science, a matter we take up further below.

We might also note that, at this stage, the prime focus was the analysis of *parlor games*, not economics. Admittedly, von Neumann suggests that the formalism is tapping the deep structure of more than just simple parlor games, when he says that "any event—given the external conditions and the participants in the situation (provided that latter are acting of their own free will)—may be regarded as a game of strategy if one looks at the effect it has on the participants" (p. 13). And, in a footnote, he even says that "[this] is the principal problem of classical economics: how is the absolutely selfish 'homo economicus' going to act under given external circumstances?" (p. 13). But we would be mistaken to overemphasize the suggested economic orientation of this work: it was written for an audience of mathematicians and its purpose was to render abstract games amenable to mathematical treatment. It was not primarily concerned with economics: those links, as we shall see, came later. Zermelo had begun with the game of chess; von Neumann was now, with characteristic audacity, trying to do the same for all zero-sum games, two-person, three-person, etc., groping for a completely general theory. It was part of a general effort to push mathematics to the limit, to show how pure, abstract, mathematics of the formal kind could constitute an "explanatory" device, of not only the "natural," as for example in statistical and quantum mechanics, but also the "social" where bluffing, outguessing, and cooperation among humans were involved (see Mirowski 1992, p. 121). If the Hilbert era had any defining feature, it was this supreme faith in the explanatory powers of mathematical formalism.

II.2 *Homo Economicus*

The evolution of von Neumann's interest in economic theory has been the subject of some recent discussion, principally involving debate over his sources of theoretical inspiration and, related to this, his pedigree: was he a classical or neoclassical economist? (See Weintraub 1985, 1989; Lionello Punzo 1991; Heinz Kurz and Neri Salvadori 1993.) First, according to fellow Hungarian Nicholas Kaldor, von Neumann expressed an early interest in economics in 1927, not long after his Berlin appointment, and, at Kaldor's suggestion, read Knut Wicksell's *Value, Capital and Rent*, which provided an introduction to Walras and utilized Bohm-Bawerk's capital theory. In this, he immediately criticized the Walrasian system, observing that it permitted negative prices (see Mohammed Dore et al. 1989). Then, in 1932, during the period in which he "commuted" between Berlin and the U.S., he presented "an economic model of his own devising at a colloquium in Princeton," his linear growth model, which he later published in 1937 in the proceedings of Karl Menger's own Vienna colloquium (Kuhn and Tucker 1958, p. 109). As Menger (1973, p. 55) subsequently recalled:

> Wald's [1935] paper on the equations concerning production greatly interested von Neumann, as he told me when passing through Vienna soon after its publication. It reminded him of equations he had formulated and solved in 1932 and now offered to present to our Colloquium.

Several features of this model, coupled with the evolution of the links between von Neumann and the "Viennese" economists in the 1930s, have given the impression that he may be viewed as part of the "neoclassical" general equilibrium group associated with the Menger Collo-

quium, principally Karl Schlesinger and Abraham Wald, who essentially created modern general equilibrium theory through their modification of the Walras-Cassel systems of equations (see Weintraub 1983, 1985). A feature central to both Cassel's and von Neumann's model was the equilibrium of a uniformly expanding economy, and von Neumann shares with the Viennese economists the use of inequalities rather than equations—the complementary slackness conditions of free disposal and zero price for goods in excess supply—and an emphasis on long-run equilibrium without profits. Furthermore, the Brouwer fixed point theorem, generalized by von Neumann in his growth model, subsequently became a central element in the theoretical baggage of neoclassical economics. All in all, viewed retrospectively, von Neumann becomes a very respectable early-modern neoclassical. In contradistinction, Kurz and Salvadori (1993) maintain that von Neumann is better seen as falling into the classical tradition, pointing out that, unlike the Walras-Cassel approach where factor services are transformed into final goods, von Neumann production involves time: it is the creation of commodities by means of commodities. Kurz and Salvadori even suggest that many features of von Neumann's model may be found, in one form or another, in earlier classical contributions, ranging from William Petty to Ladislaus von Bortkiewicz. In short, "early use of a fixed point theorem does not a neoclassical make."

In making the case for this interpretation, Kurz and Salvadori, drawing on von Waldemar Wittman (1967), shed very suggestive light on von Neumann's economics of the late 1920s. In particular, they link him to one Robert Remak, former mathematics student of Ferdinand Froebenius, and *Privatdozent* in mathematics at the University of Berlin from 1927 to 1929, when von Neumann was there. Remak wrote two papers on mathematical economics (1929, 1933), which were discussed by the Berlin mathematical economists and very probably came to the attention of von Neumann. The first paper, "Can Economics become an Exact Science?" is concerned with the existence of a vector of "superposed prices": an administered set of non-market, calculable, cost-covering, "reasonable" prices. Its context was thus the Berlin part of what would later grow into the socialist calculation debate, with Remak, at this stage, clearly taking the "socialist side." Kurz and Salvadori plausibly link von Neumann and Remak, highlighting the various similarities and oppositions in their work which seem to tie them together and locate both in the context of Berlin mathematical economics (see Kurz and Salvadori 1993, p. 149).

This interpretation of von Neumann's early economics is quite persuasive, and is consistent with further material, recently come to light. Following his 1930 visit to Princeton, and during the period he would have encountered Remak, von Neumann continued to correspond with Abraham Flexner, director of the Institute for Advanced Study at Princeton. In February 1934, Flexner sent von Neumann a copy of a book entitled *L'Economique Rationelle*, published in 1932 by one Georges Guillaume, a budding French theorist. "I have no idea whether or not it will be of any interest to you or your associates. I send it to you for your information," he wrote.[3] In this book, Guillaume sets out to construct a

[3] Flexner to von Neumann, Feb. 6, 1934; Faculty Files, John von Neumann, Folder 1933–35, Archives, Historical Studies—Social Science Library, Institute for Advanced Study, Princeton, New Jersey (hereafter VNIAS). The book by Georges Guillaume was written with the "mathematical assistance" of one Edouard Guillaume. All translation here is by the author.

mathematical economics in the manner of Walras and Pareto, treating not just exchange but also global production, and, in particular, draws explicit parallels between economics and physics. He criticizes the business cycle theories of William Jevons, Henry L. Moore, Holbrook Working, and Warren Persons which "do not constitute the experimental verification of theories themselves satisfactory" (p. 37), do not provide a vision of the *whole* process, and do not allow for possible relational shifts. Just as, in thermodynamics, the relation between pressure and volume will depend on temperature, there may be similar analogs in economics, causing curves to shift. The "Lausanne mathematical school" of Walras, he says, has come closest to creating a science of economics comparable to the exact sciences, but even this has failed because, being based on purely subjective concepts such as taste, want, and utility, it does not produce numerical data which can be subjected to tests.

Presenting his theoretical alternative, Guillaume says that every exact science was developed only when several exact principles were brought to light, and a theory built on these foundations. "It is curious . . . that in Political Economy, we have not yet succeeded in establishing very general laws, analogous to the fundamental principles which form the basis of the physico-chemical sciences." In physics and chemistry, principles such as Hans Mayer's conservation of energy were crucial. Similarly, just as Niels Bohr has succeeded in representing the atom as "a veritable solar system in miniature," so should economic theory aim for the same. Only then "will [we] be able, when we have formulated the axiomatic bases, to imagine, as in the physico-mechanical sciences, a 'little model' of economic life, a little model which we can easily master through thought" (Guillaume 1932, pp. 62–63).

Equating labor with energy, Guillaume constructs a model essentially based on a labor theory of value. Labor values are reflected in accounting prices, "*prix de revient comptable*," and the principle of the conservation of value (energy) is reflected in the universal interdependence of these prices. In economic equilibrium, accounting prices and market prices are equal; when they differ, the result is adjustment in the form of resources moving from one activity to another, and economic prediction involves studying this adjustment process. Following a series of variants of increasing complexity (pure barter; with money as a medium of exchange; with a money market, etc.), Guillaume concludes with some numerical applications.

Von Neumann, having read the book, quickly replied to Flexner from Budapest: "I think that in spite of some good and sound ideas on methodology, the mathematical technique of the authors is not good enough, to carry all the theoretical and statistical structures, which they want to build on it. I am giving some more details of my opinion on a separate leaf." We quote at length:

> I think that the basic intention of the authors, to analyze the economic world, by constructing an analogical fictitious "model," which is sufficiently simplified, so as to allow an absolutely mathematical treatment, is—although not new—sound, and in the spirit of exact sciences. I do not think, however, that the authors have a sufficient amount of mathematical routine and technique, to carry this program out.
>
> I have the impression that the subject is not yet ripe (I mean that it is not yet fully enough understood, which of its features are the essential ones) to be reduced to a small number of fundamental postulates—like geometry, or mechanics (cf. pp. 77–78). The analogies with thermodynamics are probably misleading (cf. pp. 69, 85). The authors think, that the "amortization" is the analogon [sic] to "en-

tropy." It seems to me, that if this analogy can be worked out at all, the analogon of "entropy" must be sought in the direction of "liquidity." To be more specific: if the analogon of "energy" is the "value" of the estate of an economical subject, then analogon of its thermodynamical "free energy" should be its "cash available."

The technique of the authors to set up and deal with equations is rather primitive, the way f.i. [i.e., for instance] in which they discuss the fundamental equations (1), (2) on pp. 81–85 is incomplete, as they omit to prove that 1: the resulting prices are all positive (or zero), 2: that there is only one such solution. A correct treatment of this particular question, however, exists in the literature.

Various other technical details in the setting up of their equations and in their interpretations could be criticized, too.

I do not think that their discussion of the "stability of the solutions," which is the only satisfactory way to build up a mathematical theory of economic cycles and of crises, is mathematically satisfactory.

The emphasis the authors put on the possibility of states of equilibrium in economics (cf. pp. 68, 69) seems to me entirely justified. I think that the importance of this point has not always been duly acknowledged.

I cannot judge the value of their statistical methods, as they are given in the last part of the book, for practical purposes. Their aim is to diagnose the present status of economics, and to lead to forecasts. But I think that the theoretical deduction, which leads to them is weak and incomplete. (von Neumann to Flexner, May 25, 1934, Faculty Files, John von Neumann, Folder 1933–35, VNIAS)

Several aspects of the above interchange help complete our picture. First, the book was sent by Flexner with the suggestion that it might interest von Neumann or his "associates": quite plausibly von Neumann's colleagues with an interest in mathematical economics at Berlin. Second, von Neumann's claim that a discussion of "stability of the solutions" is "the only satisfactory way to build up a mathematical theory of economics cycles and of crises" seems to fit into the context of Berlin discussions of the mathematical economics of capitalism versus socialism. Third, the link to Remak is made even more plausible by his emphasis on "states of equilibrium in economics": Remak's "reasonable" prices are not the outcome of any equilibrium-based process. Fourth, there is von Neumann's somewhat cryptic remark concerning the existence of a semi-positive and unique solution price vector, "a correct treatment of [which] . . . already exists in the literature." It appears that this must be Wald (1934), the first existence proof demonstrating uniqueness and non-negativity. As this was *presented* to the Menger Colloquium only two months previously in March 1934, it appears that von Neumann had quickly become aware of Wald's work. Finally, we note von Neumann's insistence that the physical metaphor adopted by Guillaume is wrong: recall that the latter is trying to construct an economics which is *rational* in the sense used in physical models. Von Neumann contends that it is not simply a question of mistakes in interpreting the transfer of various elements from the physical to economic domain: his fundamental point is that mathematics of this sort are unsuited to the modeling of economic behavior, inadequate to the task of rendering economic theory "scientific." The problem, he says, is that the economic domain itself "is not yet ripe," is "not yet fully enough understood," a view which was, as we shall see, central to his subsequent work on games.

II.3 *To the New World*

Von Neumann continued to travel to Europe every summer until 1938, "when the political situation eventually soured him on life there" (Aspray 1990, p. 13).[4] And it appears that he never returned to

[4] As early as March 1933, a sense of mutual despondency with political developments is evident in his correspondence with Flexner, with von Neumann speaking ominously of developments in Berlin, and Flexner condemning the destruction of Göttingen as a research center by Hitler's Education Ministry.

Germany after 1934, going thereafter mainly to Hungary, with visits to Austria, England, Italy, and France. During these years, he worked principally on the spectral theory of Hilbert space, ergodic theory, rings of operators, and Haar measure (see Aspray 1990, p. 256, n. 35; p. 13). However, the 1930s were not devoid of work on games for, early in 1937, he gave a talk at Princeton in which, according to the *Science News Letter*, he presented some ideas on what was for him "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" (p. 216). Also reported, parsimoniously, are his comments on the probabilities of making particular plays in both dice and a simplified version of poker. Further evidence of his interest is revealed in his plans for a summer visit in 1940 to the University of Washington, Seattle, where he indicated that he would there give three evening lectures on games: "The general problem. The case of chess; The notion of the 'best strategy'; Problems in games of three or more players. General remarks."[5] What should be noted here is that the format of his intended talks follows *exactly* the development of game theory up to 1928, beginning with Zermelo on chess, and culminating with von Neumann groping for a theory for three and more players: game theory was still *the mathematics of parlor games*.[6] It was not until von Neumann met another émigré, Oskar Morgenstern, that a link was forged between the theory of games and economics. They first met in the fall of 1938, by which time they were both at Princeton, having left Europe for good. But before passing to their rather intriguing collaboration, let us consider the Austrian element, beginning with Vienna of the 1920s. (See, for example, Carl Schorske 1981; Allan Janik and Steven Toulmin 1973; Karl Popper's 1974 autobiography; George Clare 1980; and Earlene Craver 1986.)

III. *In Search of a Method: Oskar Morgenstern, Economist*

Born in Silesia, Germany, in 1902, Morgenstern moved at 12 years of age with his family to Vienna. In 1925, he obtained his *Dr. Rer. Pol.* at the University of Vienna with a thesis on marginal productivity. Then, having spent most of the next three years abroad, on a Laura Spelman Rockefeller Fellowship, he became *Privatdozent* in economics at the same university in 1928. Succeeding Friedrich Hayek in 1931 as director of Vienna's Institute for Business Cycle Research, he held that position until Germany's Anschluss of Austria in 1938. At that point, Morgenstern was visiting the U.S., and, finding it more propitious to remain in America, obtained a temporary position at Princeton. He soon became a permanent faculty member, and, following his retirement from Princeton in 1970, moved to New York University, where he remained until his death in 1976 (see Martin Shubik 1979). The work of his Austrian years was concerned mainly with business cycles and with methodological critique, centering par-

[5] Von Neumann to Prof. Carpenter, March 29, 1940, Container 4, File 3, Personal Correspondence 1939–40, Von Neumann Papers, Library of Congress (hereafter VNLC).

[6] This is not to ignore the fact that the minimax formalism was shared by both the 1928 theorem on the two-person, zero-sum game and the 1932 growth model. As von Neumann pointed out: "The question of whether our problem has a solution is oddly connected with that of a problem occurring in the Theory of Games dealt with elsewhere" (1945, p. 5, n. 1). But the operative word here is "oddly": the two shared the formalism, but their contexts and the motivation behind them were quite different.

ticularly on such issues as the relationship of time and foresight to general equilibrium theory. Following the publication of the *Theory of Games*, he coauthored work based on game theory and on von Neumann's growth model, and wrote several books, of which his *On the Accuracy of Economic Observations* (1950) is perhaps the most well known.

III.1 *"Vienna Circles"*

The emergence of the various cliques in Viennese economics, beginning in the early part of the decade, has been artfully described in Craver (1986). A key feature was institutional fragmentation, with research activity and discussion shifting away from the university toward the private seminars and research institutes. In this intellectual swirl, the figures of greatest initial importance to Morgenstern were Othmar Spann and Hans Mayer. The former was noted for his dissent from the politically liberal economics of his teacher, Carl Menger, and, under the influence of German Romanticist Adam Müller, had developed what he labeled Universalism, a conservative, organicist approach which rejected methodological individualism. By the early 1920s, Spann's *volkisch* views were a passionate mix of theory and German nationalist ideology, and he regarded the classroom as preaching ground. As one commentator writes:

> Spann's students . . . were carried away by the rhetorical boldness of the slight, handsome professor with the burning eyes of the true believer. . . . [Dreams] of Utopias—even arch-reactionary Utopias—were infinitely more pleasant than a world of defeat, inflation, and gnawing hunger. (John Haag 1976/77, p. 237)

Morgenstern was, for a time, swept along by this anti-liberal, anti-Semitic tide, and his diary entries in these years reveal the extent to which Spann had a hold over him. More important to our story is the fact that Morgenstern, given his mentor's views, spent a good deal of time reading idealistic philosophy—Fichte, Hegel, and Schelling—at the cost of undertaking other study, for example in mathematics.[7]

Internal chicanery, however, soon led Morgenstern to break with Spann and enter the circle surrounding Friedrich von Wieser's former assistant and now heir-apparent, marginalist Mayer, who had begun lecturing at Vienna in 1923. An Austrian theorist in the tradition of Wieser, Mayer emphasized the centrality of the imputation problem in economic theory. Like Spann, of whom he was jealous and hateful, he assembled a group around him and ran a seminar, focusing on imputation and the difficulties of incorporating time into equilibrium theory (see Craver 1986, p. 10–11).

In September 1925, Morgenstern left Vienna on his Rockefeller Fellowship, visiting England, the United States, France, and Italy, over the course of the next three years. In England, he met both Arthur Bowley and Francis Y. Edgeworth; he left there in early 1926 for the U.S. where, working mainly on business cycles, he had close contact with Wesley Mitchell and Moore at Columbia, and F. A. Fetter at Princeton.

[7] In his diary, the young Morgenstern writes: "Looking at my papers, work and reading, one would think I were a student of philosophy and not of economics. But that is no disgrace . . . . . my philosophical knowledge is very fragmentary in comparison to my knowledge of economics" (Diary, March 18, 1923, Oskar Morgenstern Papers, Special Collections Department, Perkins Library, Duke University, Durham, North Carolina (hereafter OMDU), quoted in Urs Rellstab 1991, p.12). In later years, working with von Neumann at Princeton, "philosophy gained" had become equated with "mathematics lost," and Morgenstern wrote "I was an idiot not to have studied math. even as a sideline at the university in Vienna, instead of this silly philosophy, which took so much of my time and of which so little is left" (OMDU, Aug. 7, 1941).

Following that, he spent several months at Harvard, where he met with Allyn Young and attended a philosophy seminar by Alfred North Whitehead. The culmination of his sojourn abroad was his first book, *Wirtschaftsprognose* (Economic Prediction; 1928).[8] While he would subsequently refer to it as the place he introduced the Holmes-Moriarty outguessing problem, the book is as remarkable for its pessimism as it is for any qualities of theoretical innovation. In it, Morgenstern sets out to show the impossibility of making any complete forecast of the state of the economy at any time, given the complexity of the mechanisms which shape economic events. Unlike astronomy or medicine, he says, the social sciences have the peculiarity of being able to affect their object of study. The prediction of the astronomer can have no effect on the subsequent movement of the stars, but that of the economist *can* change economic events. He says that static economic theory assumes complete subjective rationality: the rationality of economic acts is so perfect that there are no more acts of choice or decisions, "because everything stands still" (p. 7). However, in the real economy, individuals have a system of orientation points (*Orientierungspunkte*), which includes not only their knowledge of the laws of nature, but also their knowledge and beliefs about the *other* economic subjects:

> every action influences the other actions, and each is reflected in the other. The set of all different actors is similar to a cupola, in which every stone supports the others, and vice versa, and none can stand freely on its own. (pp. 92–93)

[8] What follows is a selective, not encyclopaedic, coverage of Morgenstern's work in this period, the papers chosen being principally those dealing with theory and methodology. Full bibliographies for Morgenstern are given in both Shubik, ed. (1967) and the volume edited by Andrew Schotter (Morgenstern 1976a).

Concerned explicitly with the *policy* implications of his analysis, it is this element of interdependence, says Morgenstern, which renders futile any attempt at economic prediction. To illustrate this, he gives the example of the effect of a "monopolistic-authoritative total prediction," a unique, comprehensive, authoritative forecast by the public authority, indicating, in this example, certain increases in the price level, interest rates, rate of stock accumulation, and industry orders, over a certain future period. Because economic agents will always incorporate any forecast into their economic plans, the prediction, he asserts, can *never* be realized. In the present case, "they will buy tomorrow, if possible, what they would have bought over a longer time given constant prices" (p. 94). The result, he says, will be to accelerate the whole movement, pushing up orders, interest rates, and wage demands more than forecasted. Because of the complexity of action and reaction, one can never hope to pinpoint where this process ends: any attempt at revising the prediction will simply lead to a revision of plans and a defeat of that prediction.

At this point, he introduces the Holmes and Moriarty anecdote to illustrate the bind into which the forecasting agency and the public are drawn if they try to "outguess" each other: one ends up with an infinite regress of prediction, reaction, and revised prediction, from which there is no escape. As with Holmes and Moriarty, either "from so much thinking, there would have come no action," or alternatively the less intelligent one should capitulate at the outset. And because, somewhat harshly, "every prediction must become true, otherwise it is totally worthless" (p. 95), the whole process of economic prediction, he says, is a waste of time. He goes on to consider other types of stylized predic-

tion situations, in each case reaching a similar conclusion about the impossibility of correct forecasting. For example, in the case of there being several total predictions, each being partially acknowledged by economic agents, the result would be chaos. As he says, "*Difficile est satiram non scribere*" (pp. 100–01; trans. "It is difficult not to regard this as a satire").

Condemning the use of statistical methods in economics for anything other than description, he says that the "entire modern methodology since Menger-Rickert-Max Weber contains all the arguments against attempts at prediction, but the diehards don't seem to be aware of this" (p. 117). The diehards he has in mind are those aiming to develop statistical methods for business cycle forecasting: statistical services such as Moody's, Babson, Brookmire, and Warren Persons and the Harvard Business Barometer. Can such institutions, he asks, "help to improve the rationality of the economic system?" (p. 122). No, their correct function is simply to collect statistics and make information available with as little interpretation as possible, especially as regards the future.[9]

On his return to Vienna in 1928, Morgenstern resumed his involvement with Mayer's and other groups, and took over from the former the editorship of *Zeitschrift für Nationalökonomie*. However, perhaps partially as a result of his earlier contact with Edgeworth and Whitehead, he was now drawn more than ever before to mathematical logic and its relationship to economic theory, something which began to show in his work beginning in the 1930s.

> In the evenings, I read Carnap [*The Logical Structure of the World*], which is very difficult, but from which I gain a lot. I am slowly learning to think, and by doing that I come more and more into a mathematical way of thinking. (Diary, Mar. 30, 1929, OMDU)

This gradual shift was facilitated by the fluidity of intellectual life in interwar Vienna, where discursive fragmentation was a sign of intellectual vigor and energy. Apart from the *Mayerkreis*, there existed a number of interlinked groups, with individuals often belonging to several circles, ranging from the formal gatherings of the Viennese Economic Society at the offices of the National Bankers Association, to the evening discussions at the Reichsrat Café. Preceding Morgenstern in their defection from the Spann circle, Hayek and Herbert Fürth had formed their own *Geistkreis* in 1921: their discussions were broad, ranging from economic and political philosophy, to literature, art, and music. Overlapping with this was the important *Privatseminar* of Ludwig von Mises, which, beginning in 1920, included Gottfried Haberler, Hayek, Gertrud Lovasy, Fritz Machlup, Morgenstern, Paul Rosenstein-Rodan, Schlesinger, and Alfred Schütz. More focused than the *Geistkreis*, the group nonetheless debated broadly in the social sciences, with sessions devoted to Max Weber, methodology, and the philosophy of science. Visiting speakers from abroad included Howard Ellis, Robbins, and Ragnar Nurkse. The group appears to have been tightly knit, with

[9] Reviews of *Wirtschaftsprognose* by Arthur W. Marget (1929) and Morgenstern's friend Eveline Burns (1929) were charitable, but also deft in responding to his nihilism. As Mary Morgan (1990, p. 236) points out, Marget disagreed with him on virtually every point except the general difficulty of applying probability techniques to economics, and rebuffed Morgenstern gently in his own words: "One feels that Dr. Morgenstern has here given us what, despite his declared intention, can be described only as a satire" (1929, p. 332). Burns suggested that his emphasis on complexity was tantamount to abandoning "all hope of creating a usable economic science, or indeed any science at all" (p. 161). Lionel Robbins (1938) also felt that Morgenstern was going too far, and that "to disown all power of diagnosis and of prediction is to exhibit a truly superfluous austerity" unmerited by, and damaging to, economics (translated and quoted in Denis O'Brien 1988, p. 175).

meetings held in Mises' office at the Chamber of Commerce, and post-seminar discussions continuing late at the Café Künstler and various other Viennese watering-holes. According to Craver, Morgenstern felt that the "Mises seminar was far more important in the thirties than anything that went on at the university" (1986, p. 14).

Less close to economic methodology, but interested in economics from a mathematical perspective, was the group centered on Karl Menger's Mathematical Colloquium, which included Franz Alt, Gödel, Wald, and, as an occasional visitor, Morgenstern. While the latter's Austrian theoretical inheritance of concern for time, foresight, and knowledge would remain with him, it was the *manner* in which he was to approach these issues that would ultimately distinguish him from the Austrian economists, and it is this, in part, that allows us to understand his later being drawn to von Neumann. In this regard, the influence of Karl Menger was particularly profound, for it was from Mayer, a nonmathematical economist, to Menger, a mathematician interested in economics, that Morgenstern gradually transferred his allegiance at the turn of the decade.

III.2 *Changing Orbits*

Morgenstern's particular appreciation for the use of mathematics in economic theory is evident as early as 1926 when, in a review of Gustav Cassel's 1918 *Theory of Social Economy*, he praises the author's analytical rigor and "inclination towards mathematics" (1926, pp. 1–2). This review, which was never published, reveals the principal tension characterizing Morgenstern's papers of the late 1920s and early 1930s: the upholding of Mayer's conceptual inheritance but, in opposition to Mayer, the promotion of logic and mathematics as the appropriate vehicle for so doing. Mayer's work of this period culminated in his "genetic-causal" approach to general equilibrium (1932), a nonmathematical attempt to "describe and explain the entire genetic path of the economic process in its various intermediate stages up to the attainment of the final result—the equilibrium" (Eraldo Fossati 1957, p. 43). It thus reintroduces time into general equilibrium theory, suggesting that the final outcome is not path-independent: depending on their experience at intermediate stages on the path to equilibrium, individuals may find their tastes changing, leading them to revise their original plans. While the functional approach to equilibrium described by Pareto leaves no room for shifting psychological influences and offers a well-determined system, Mayer seeks to account for these, even if the result is only to suggest the "premises of a possible future" (Fossati 1957, p. 44). These emphases are evident in Morgenstern's work, but to the extent that he promoted the use of formalism, he was already diverging from his mentor: by 1934, he was moving in the shadow of Karl Menger, and was even studying mathematics under him at the University.[10]

The works by Menger which directly influenced Morgenstern during this period were a limited number of writings on a relatively broad range of topics: an

[10] Morgenstern's break with Mayer also had a strong personal element to it, with Morgenstern gradually growing disillusioned with his mentor's sloth, disinterest, and lack of professionalism. (See Diary, Dec. 22, 1928; April 19, 1929; Nov. 10 and Dec. 20. 1935, OMDU). For similar views of Mayer, see Craver (1986) and von Mises (1978). Karl Menger, son of Carl, the economist, was born in Vienna in 1902, and completed a Ph.D in mathematics in 1924. Following two years with Brouwer at the University of Amsterdam, he returned to Vienna as associate professor of geometry. Apart from a year visiting Harvard and the Rice Institute, in 1930–31, he remained in Austria until 1937. At that point he emigrated to the U.S., taking a position at Notre Dame and then the Illinois Institute of Technology. He died in 1985.

article on the Petersburg paradox, two others on the foundations of mathematics, and a book on ethics and social organization, and a paper on the Law of Diminishing Returns (respectively 1934d, 1930, 1933, 1934e, and 1936). In the first of these, Menger outlines and criticizes existing "resolutions" of the Petersburg paradox, calling for greater precision in the handling of such problems. Only through a formal treatment can imprecise thinking be avoided, says Menger, at the same time as acknowledging the limits of formalization in dealing with complex human behavior such as gambling. In his discussion of the debates in the foundations of mathematics, Menger rails against extreme positions such as that of Brouwer's Intuitionism, and argues for a sharp distinction between the *logical* content of particular of bodies of mathematics and their *value* or merit. Of the latter, he says, mathematicians, qua mathematicians, can have nothing to say: such choices are a question of psychology or taste. Mathematicians should be concerned only with the logical construction of theories having explicitly stated the basic assumptions and rules of procedure. These ideas were presented in two related papers, "On Intuitionism" (1930) and "The New Logic" (1933).

It was this same relativism that Menger then brought to his 1934 book on ethics, *Morality, Decision, and Social Organization*, which is particularly relevant in our consideration of Morgenstern. Taking an incipiently *structuralist* approach to social theory, Menger abandons any traditional philosophical consideration of the foundations of morals, and instead develops a static, or synchronic, analysis of the *logical* consistency of various sets of norms. Given attitudinal differences among individuals to a set of hypothetical rules of behavior, what are the logical implications for the formation of socially compatible groups? This is the question explored by Menger, and he explicitly suggests that analysis of group formation of this kind would be relevant to economic theory. This book, and the above papers, were taken very seriously by Morgenstern, and Menger's influence is evident not only in the latter's personal reflections, but in his papers on time, foresight, and logic of the mid-1930s, and in his subsequent Princeton discussions with von Neumann in the early 1940s.[11]

In September, 1934, Morgenstern wrote to Frank Knight that he had just published "The Time Moment in Economic Theory," saying "I put much thought and work into that article and, what's more, much breadth results, the limits of which I cannot see at the moment."[12]

As in much of Morgenstern's work in this period, the writing is not always clear, and offers little by way of "concrete" detail, the salient points being largely criticisms of existing theory. He

[11] Menger is first mentioned in Morgenstern's diary in March 1933. Then, several months later, "Saturday I was to dinner at Menger's. He gave, in a manner of speaking, a lesson on curve and dimension theory. We talked about a math. course that he wants to give, which will probably be excellent. We plan to meet again in August; until then, he is going to read lots of books and articles which I have lent him, and we are going to construct an axiomatics of economics. It could be of importance" (Diary, July 11, 1933, OMDU). Morgenstern later refers to mathematical lessons from Menger, Alt, and Wald, to having read Menger's book on ethics (see, Nov. 4, 1934), and to having found Menger's Petersburg paper very useful in his teaching on risk theory (Nov. 29, 1934). Following Menger's presentation of his paper on the law of diminishing returns (1936), described by Schumpeter as a reading of "the logician's riot act" to economists (1954, p. 587), Morgenstern wrote that it "was an exemplary piece of work for the proof of the necessity of exact thinking in economics" (Diary, Dec. 31, 1935, OMDU). For a more detailed account of Menger's work in its intellectual and social context, see Leonard (1994b, forthcoming).

[12] Morgenstern to Knight, Sept. 12, 1934, (OMDU, Box 6 Corresp. 1928–1939, Knight).

begins by endorsing a claim made by Menger (1934b) that utility theory is not simply tautological, and, unlike the essentially analytical theorems of logic and mathematics, can be contradicted by observation: those who suggest otherwise are simply not "fully aware of the relationship between 'logic and economics'" (p. 152). His main concern, however, is the question of dynamics, and, inspired by Mayer, he criticizes the inadequacy of attempts to incorporate time into general equilibrium theory, such as that by Moore (1925). He condemns the Walrasian system for assuming infinitely fast reaction times, presumably of prices, and suggests having different reaction times for different prices. This, he says without further elaboration, "should give results," and referring to Mayer and his own 1928 *Wirtschaftsprognose* he stresses the importance of dealing with foresight and uncertainty. Lacking the formal tools, however, Morgenstern stops short of going for the jugular:

> it is more appropriate to refrain from any graphic description of these relationships since such an enquiry could easily fix itself on curves instead of leading to the relevant relationships and their logical connections. [It] may be pointed out that the theorems advanced here and those that will follow qualify very well for a quite rigorous presentation with mathematical tools. They might, for reasons not to be given here, make even better material for the application of mathematics to economic theory than was evident until now in an unfortunately large number of cases of mathematical economics. (p. 158)[13]

His crusade for exact thinking continues in his 1935 "Perfect Foresight and Economic Equilibrium," this time drawing praise—indeed, an offer to translate—from Knight. Here, Morgenstern shows evidence of his growing reading in logic and set theory, and the growing influence of Menger and his milieu. A variation on a now common theme, the paper is an attack on the assumption of perfect foresight, which, he says, leads to the destruction of the entire concept of general equilibrium. The whole issue has been treated sloppily, says Morgenstern, with Walras and Pareto failing to make explicit their assumptions about what subjects can foretell, and John Hicks (1933) assuming that perfect foresight is a precondition for equilibrium. Nobody has asked "the foresight of whom? of what kind of matters or events? for what local relationships? for what period of time?" (pp. 171–72).

As it stands, the assumption of complete foresight implies that individuals have complete insight into all economic processes concerning prices, production, and income. Given the interdependence and complexity of the economic system, this implies "incredible powers on the part of the economic agent," who must not only know exactly the influence of his own transactions on prices but also the influence of every other individual, and of his own future behavior on that of the others: persons of perfect foresight are not mortals, he says, but "demi-gods" (p. 173). In a world where mathematics was struggling to put its paradoxes back in their box, the economists were still content to entertain an assumption which was doubly paradoxical. Perfect foresight implies firstly that economics has posited the existence of an economic subject that perfectly knows economic science already! Secondly, it leads to a Holmes-Moriarty type bind, "out of which we cannot extract ourselves. . . . Unlimited foresight and economic equilibrium are thus irreconcilable with one

[13] Responding to the paper, Knight pulled no punches: "I read your article . . . and must say frankly that my reaction was not very enthusiastic. Not that I found anything to disagree with, but that it seemed to me the whole argument was rather in the domain of such a degree of refinement of conception and doctrine that I did not get the feeling of very great importance in the contribution" (Knight to Morgenstern, undated, OMDU, Box 6, Corresp.: 1928–1939, Knight).

another" (p. 174). And again he looks to mathematics for help:

> Up to the present time, the only examination of a *strictly* formal nature about social groups, even though it is carried out in another field and is limited to the co-existent individuals independent of one another, is a work by K. Menger, [*Morality, Decision and Social Organization*, 1934a] which it is hoped, will become known to economists and to sociologists because of its importance in laying the foundation for further work. (pp. 174–75)[14]

The final paper in the Viennese phase, his 1936 "Logistics and the Social Sciences," is essentially a rebroadcast to an economics audience of Menger's 1932 talk on "The New Logic." Recent developments in mathematical logic, he says, such as Russell and Whitehead's *Principia Mathematica*, have been ignored in the social sciences. If economics and its sister disciplines are to exhibit exact thinking, as the sciences have done, then it can only be through the adoption of mathematics and logic, both of which, he says, have the same structure. In fact, mathematics rests only on logic, so that the arguments claiming formalism to be appropriate only to the natural sciences can be dismissed:

> [The] potential use of mathematics in the social sciences means nothing else but that their problems can be formulated and treated in an exact manner. No "foreign" element is being introduced. (p. 390)

However, in contrast with his relatively mild assessment of same in his *Encyclopaedia of the Social Sciences* article of 1931, Morgenstern now says that the whole area of mathematical economics "is in a lamentable condition" (p. 393). Transformed by his contact with Menger, he now makes liberal references to all of the latter's work on economics, logic and ethics, discussed above, and to Hilbert's success in the axiomatic treatment of geometries and thermodynamics. He equates axiomatic rigor with putting an end to historicism, to debates over a priorism, and to the complex, phony opposition that the use of mathematics has encountered: only by adopting a formal language can one remove the "traps and contradictions of a sentential language" (p. 398). In Morgenstern's economics, in short, the spirit of Menger has become ubiquitous:

> It would obviously be important if one could formalize the achievement of a concrete economic policy measure in the same manner as this has been done for normative systems . . . Although space forbids [entering] upon the relation which Menger's book has to economic theory [it] may suffice to mention that his logic of wants or wishes would provide a

[14] Introducing his paper to Knight, Morgenstern said: "I have a certain interest to [sic] have this article appear in English because Mr. Keynes is preparing a book on the theory of money largely based on the element of expectation and anticipation" (Morgenstern to Knight, Dec. 18, 1935, OMDU, Box 6, Corresp.: 1928–1939, Knight). His opposition to Keynes' economics is a recurring theme in this period. In a letter to Burns in 1934, he claimed to have proven that, in Keynes' theory of money, "his equations completely don't hold up" (OMDU, Mar. 6, 1934). Writing to Haberler in 1936, on the *General Theory*, he said: "Keynes' book is simply horrible, to the extent that I have read it" (Morgenstern to Haberler, Apr. 4, 1936, OMDU, Box 5). Then, in the bibliographical appendix added to the 1937 translation of his 1934 *The Limits of Economics*, Morgenstern says that the *General Theory* "has given a prominent place to the rôle of expectations. But his analysis relating to this point is so vague that I think we shall have to wait for further elucidations from his pen before delivering final judgment on it. Obviously it is not sufficient merely to refer to expectations and anticipations. We need to know how they are determined, on what factors they depend and the ways in which they are mutually interdependent. Mr. Keynes gives no real analysis of these points" (p. 159). According to Morgenstern's diary, Menger liked the paper (see Diary, Sept. 11, 1935, OMDU). Knight, too, was this time more enthusiastic: "It seems to me that in your article on perfect anticipation you have done a major piece of work." He then went on to add that "the market for high grade economists in this country seems to be quite 'bullish' at the moment. Are you interested?" (Knight to Morgenstern, Mar. 12, 1936, OMDU, Box 6, Corresp.: Knight).

most useful model for a logically satisfactory economic theory of wants. (p. 404)

Not until two years later, in 1939, did Morgenstern explicitly turn to these issues again. Once more, he was talking with the mathematicians, but, this time, it was at Princeton, from where he wrote once more to Knight:

there are only a few people, if any, interested in methodological questions. Those with whom to discuss such problems are principally the mathematicians, of which we have some excellent ones in town. I have now been stimulated by these talks and proceeded to jot down notes on a further paper of what I called maxims of behavior. In this paper I shall endeavor to investigate a very curious relationship between the quantitative limits which maxims may have. I hope to be able to show you something of this in the not too distant future. (Morgenstern to Knight, Nov. 8, 1939, OMDU, Box 6, Corresp. 1928–1939, Knight)

IV. *Writing the* Theory of Games

The Princeton mathematicians referred to in the letter to Knight were those at the Institute for Advanced Study which, of course, drew Morgenstern like a magnet. Then in its heyday, it had Einstein, von Neumann, and Gödel as permanent faculty, and many of the period's more prominent figures in science and mathematics were regular visitors (see Edward Regis 1987). Morgenstern thus took up where he had left off in Vienna: listening to the scientists, talking to the mathematicians, and reflecting regretfully upon the dismal science. Of an early encounter with Bohr, for example, he writes:

He talked mostly about the influence of the observer on experiments, and how much more difficult the situation is in the social sciences. He complained that physics was taken too much as a model for our philosophy of science, physics being much too simple for that. The social sciences are so much more complicated (this incidentally is also what Planck thinks). (Diary, OMDU, Feb. 15, 1939)[15]

It was in this intellectual setting, dominated by the discussion of mathematics and mathematical physics, that he first encountered von Neumann, and that their conversation turned to economics and games.

IV.1 *The Collaboration*

The collaborative process which culminated in the manuscript of the *Theory of Games* being sent to press in April 1943 was neither simple, mechanical, nor linear, and the ideas expressed in the book should not be seen as the *inevitable* outcome of a conversation begun three years previously. Just as it would never have been written had von Neumann and Morgenstern never met, the form it took was a reflection of the contingencies of their collaboration, and the latter must be seen as a *process*. In what follows, we try to recover some of that contingency, and show how "order," in the form of the book, emerged from the relative "chaos" of intellectual interaction. Using Morgenstern's diary as a window on the pe-

[15] Morgenstern quietly longed to be at the Institute. He had contacted Flexner from Vienna as early as June 1937 (see Morgenstern to Flexner, June 2, 1937, General File, Archives, Historical Studies-Social Science Library, VNIAS), and later, when at Princeton, wrote: "If only I had a position . . . Perhaps in time something can be done. I am on good terms with Walt Stewart, amongst others. Now also with Weyl, John von Neumann, Lowe etc. But that doesn't mean that I will be able to get something" (Diary, Feb. 15, 1939, OMDU; see also Oct. 12, 1941). His longing to be with the scientists was complemented by the disdain he felt for the Princeton Economics Department, of which his diary is replete with criticism, e.g., "There is a spark missing in the department. It's too provincial" (OMDU, Oct. 26, 1940). Or, "I am dissatisfied with the department. There are only 4 or 5 graduate courses, no seminars yet, no discussions. The students don't like it either. [X] is totally unsuitable: the mathematicians have weekly colloquia, as do the physicists, psychologists and chemists. We have to have somebody with a truly scientific spirit. . . . . something must happen" (OMDU, Oct. 7, 1941).

riod, let us enter at neither the beginning nor the end but in the middle, when in July 1941 he wrote:

> The Hicks essay has been published, 33 pages. The Maxims are still lying around and have to be worked on. In the meantime, have started a treatise with Johnny about games, minimax, bilateral monopoly, and duopoly. What fun. We will probably get it finished before September, and the treatise will surely be of farreaching significance, especially because it touches upon the foundation of the subjective theory of value (i.e. Robinson [Crusoe] = max[imum] problem; Individual in social science = Min Max problem!) I want to publish that in the J.P.E. After close study of his manuscript, I am rewriting the Introduction. (Diary, OMDU, July 12, 1941)

This entry contains many of the elements important to our account: the Hicks essay, the Maxims paper, von Neumann's "manuscript." What were they? How did they relate to each other, and to the emergence of the *Theory of Games*?

Morgenstern completed his review of Hicks's 1939 *Value and Capital* in mid-1941, when he had already been several months under von Neumann's spell, and his harsh critique—he called it one of "the most unreadable works . . . on economic theory" (p. 364)—bears all the hallmarks of his recent conversations with the latter. Even if it is less than consistently clear in content, the review is the first published document indicating von Neumann's impact on Morgenstern, and his views on general equilibrium theory are delivered clearly and confidently. Hicks is taken to task for continuing to assume that the counting of equations and unknowns is sufficient to guarantee determinateness of a linear system, and is further criticized for ignoring Wald's (1934) and von Neumann's (1937) existence proofs, the only treatments which have transcended this difficulty. Echoing his conversations with von Neumann, who directly influenced his writing of the review, Morgenstern suggests that the whole notion of equilibrium needs careful attention:

> the indiscriminate use of the word "equilibrium" is also objectionable, if not often entirely misleading. . . . If the respective equilibrium is not qualified further as being either stable, labile or indifferent, the whole statement hangs in the air, adding to the vagueness of the usual procedure. . . . Some of these equilibrium conditions need not at all conform to the ordinary simple maximums or minimums. They are more likely of the so-called "minimax" type, the analysis of which requires instruments of great subtlety. (1941a, pp. 374–75, footnote)

Criticizing Hicks for ignoring his own "Perfect Foresight" and "Timemoment" papers, and rebuking Keynes for his "casual use of notions about expectations" (pp. 381–82), Morgenstern condemns the unclear use of the concept of "consistency of plans" as a condition of equilibrium. Hicks, he says, uses it in a purely definitional sense, without giving the concept of "consistency" any concrete meaning:

> It is obvious that . . . it has not been decided whether there exists only one single grouping of plans which is compatible with equilibrium or whether there are many possible ones, each of which would be 'consistent'. In order to decide a problem of this kind it is, naturally, necessary to be more specific about the character of the plans or, in other words, to define them more specifically. The problems involved are of quite exceptional difficulty and resemble closely those of the theory of games. (1941a, p. 380)

That the discussion of the equilibrium compatibility of groupings of plans seems evocative of Menger's 1934 work on the compatibility of ethical decisions should not be surprising: Morgenstern, at this stage, was actively trying to reconcile his own theoretical concerns with Menger's contributions to ethics and economics, with von Neumann on general equilibrium, and now with these new ideas on games. Indeed, it could well be argued that, at this point, Mor-

genstern was intellectually poised between Menger and von Neumann, just as, a decade previously, he had passed from Mayer to Menger.[16]

Read in the light of what we now know, the review of Hicks thus becomes an important transitional text, which says as much about Morgenstern himself as it does about Hicks's economics. It shows how his thinking was still dominated by a familiar range of concerns: interdependence, knowledge, foresight, and dynamics, and, as in his related Viennese papers, its lack of unity is suggestive of his own intellectual flux. Reinforced by his encounter with von Neumann, Morgenstern's grip on the Viennese questions has tightened, yet the links have not been worked out. What von Neumann is saying seems to be important, yet what it means exactly for foresight and dynamics is unclear. And it is all as difficult as it is fascinating:

> Yesterday at Johnny's, who gave me a long lecture about quantum logic. Quick as always, often assuming more than I could contribute, but still to such a point that I now know what is happening. And that disturbs me extraordinarily, because it means that far-reaching epistemological conclusions must be drawn. The beautiful, comfortable division of the sciences into logical and empirical sciences falls. . . . Everything comes from quantum mechanics, and it again leads back to the foundations debate . . . . Since I had read [A. d'Abro's *Decline of Mechanism*] I was in a better position to follow Johnny yesterday . . . I have the suspicion that it would also be necessary to introduce new thought forms in economics. For example, a logic of wishing (it leads back to Menger). The bad thing for me is that I see all this, and feel that it is necessary, but suspect darkly that it escapes me. . . . All this probably because I never had the necessary wide mathematical training. Sad. (Diary, Jan. 22, 1941, OMDU)

With the Hicks critique completed, Morgenstern turned to the "maxims," to which he had referred in his diary since leaving Vienna. Written in May 1941, "Quantitative Implications of Maxims of Behavior" (1941b) is a fascinating document, for several reasons. Unlike virtually everything he had written up to then on the methodological issues preoccupying him, this paper actually moves *forward* from critique to construction, it is entirely nontechnical, it shows the direct influence of Menger's 1934 book on ethics, and it makes direct appeals to von Neumann for mathematical clarification: indeed, taking all this into account, I believe it to be, unlike the *Theory of Games*, truly representative of Morgenstern's own thinking.[17]

The issue, Morgenstern tells us at the outset, is the theory of *society*, economics being a branch of same. Although Menger is not credited explicitly with the overall structure, the appropriation is abundantly clear: the principles governing behavior are to be called "maxims," we are "concerned only with the formal aspects of these principles" and not the larger philosophical problems connected with them, and, as in Menger, Kant is invoked for having taken "one of the most famous steps in this direction . . . when he formulated his categorical imperative" (p. 1). Maxims may be classed

[16] And he was quite aware of the shift, writing privately: "It is interesting to compare him to Menger. Menger takes himself much more seriously. Neumann practically always excuses himself when he has to say that he has worked on something" (Diary, Sept. 1, 1940, OMDU).

[17] From his diary, however, it is clear that Morgenstern's maxims paper was influenced by von Neumann from an early stage. A year earlier, in April 1940, Morgenstern wrote: "We spent nearly four hours in discussion. Maxims of behavior, (I understand perfectly what it's about and how difficult this is) about games, and about foundational questions in mathematics" (Diary, April 5, 1940, OMDU). In August, when von Neumann returned from Seattle, Morgenstern wrote: "Neumann came for three hours and we had a discussion. First, my [maxims] essay, with which he was generally in agreement (I only gave him a general outline), and then 4-person games, where he has solved nearly all the questions. There is only one catch. These combinatorical things are complicated and not very clear" (OMDU, Aug. 20, 1940).

into two types: restricted and unrestricted. The former can be followed regardless of whether others do the same or not (e.g., "Thou shalt not steal"), whereas observing restricted maxims will depend on whether others do the same or not. For example, the maxim to "withdraw bank deposits when a danger threatens" is practicable only if some, and not all, follow it. Its feasibility will depend on quantitative limits such as knowing how others perceive, and will react to, the same "danger."

Connected with the concept of maxims is the Austrian notion of "subjective rationality": the pursuit of aims given the individual's knowledge of facts and the intelligence with which they interpret those facts. A certain minimum level of subjective rationality of individuals is assumed, but the degree is clearly subject to change. This notion matters in the context of restricted maxims:

> The individual will have to make an appraisal in his mind as to what the consequences of his acts will be and whether the consequences are still compatible with the aims which gave rise to the maxim he follows. In the case of unrestricted maxims the answer is simple because there the subjective rationality will not be impaired by the behavior of others according to the same behavior. (p. 7)

In this framework, many of the topics previously raised by Morgenstern come up naturally. The knowledge relevant to the adoption of maxims may be called "foresight": in the case of bank withdrawals, this would refer to the knowledge of where one stood in the "queue," so to speak, for the bank. This will depend on the behavior of others in respect of the maxim, and, in turn, on the particular individual's understanding of that behavior. Clearly,

> very great requirements are made as far as the intelligence of the acting individuals is concerned if they are supposed to understand precisely where the restrictions of maxims set in. (p. 8)

Time may also enter the picture, in turning unrestricted maxims into restricted ones, although this, he says, is not something that will be treated here. He then devotes a large proportion of the paper to considering the policy implications of it all. For example, regulation may be intended to *substitute* for subjective rationality, e.g., in the case of the bank withdrawal, the government may declare a moratorium and thereby bring about the protection of deposits which all individuals would have liked to secure voluntarily, had they had "sufficient information and assurance that their own action would be followed by appropriate behavior of the others concerned" (p. 16). This regulation concerning the upholding of restricted maxims suggests a type of *positive interventionism* "which is not exposed to any of the criticisms which are voiced against every intervention by the adherents of a purely laisser-faire attitude" for it "leaves the maxim by which people are moved entirely untouched": it is "merely a substitute for the corrective which superior information and intelligence would offer" (p. 17). In another example, individuals may refrain from pursuing a maxim because they see the smallness of their own contribution or because the collective result is foreseeable only in the distant future. Policy here, he suggests, may encounter insurmountable difficulties. Finally, he turns to the issue of the compatibility and coexistence of maxims of different kinds, again following Menger. For example, are there policies which would allow maximum fulfillment of a given set of maxims? The coexistence of unrestricted maxims, he says, requires

> very interesting methods of a mathematical nature: It is sufficient here to refer to the writings of Karl Menger [*Moral*, etc.] where a fairly exhaustive first treatment has been of-

> fered for the first time on the basis of exact methods. Space does not permit going into the matter here which also lies outside the lines of this paper which is concerned with somewhat wider aspects of the question of compatibility. (p. 20)

The key difference between Menger's and Morgenstern's analysis is that the former deals with ethical maxims which are unrestricted in Morgenstern's sense. Thus, Menger is concerned with the implications for compatibility *once the choices have been made*. Morgenstern, on the other hand, faces the added complication of considering choices which are a priori *interdependent*. One can choose to be polite regardless of what others do; one cannot make bank withdrawals regardless of others. The restrictiveness of certain maxims means that the individual's problem of choice is now much more complicated than traditionally suggested in economics.

> [It] is not only a matter of making preferences between aims in a purely qualitative manner in order to establish a line of action of maximum subjective rationality, . . . the further problem of compatibility has to be solved by each single individual where such maxims occur. If the individuals are not aware of the existence of this problem then we clearly get a different kind of behavior than if they are aware of it and accordingly the structure of theory would have to be modified. It does not need many words to make clear that this opens up new fields of investigations. (p. 21)

He concludes with an explicit appeal to von Neumann for help with the formalization. Although the present treatment has been qualitative, he says,

> the character of the problem is essentially a quantitative one and . . . therefore, would require mathematical tools in order to be given a proper formulation, not to mention solution. Unfortunately, the character of the mathematical problems involved is such as to make it exceedingly difficult even when the use of the most advanced mathematics is envisaged; it may even be argued that the necessary mathematics do not, as yet, exist. Consequently, it will be necessary to leave it to the professional mathematicians to work out solutions and it is not exaggerated to think that they will have a very hard nut to crack. Reference should be made to the studies on the theory of games by J. von Neumann which was [sic] recently extended in a series of lectures. However, the cases treated there are as yet of a restricted nature and do not take the problems into consideration which have been described above. It is, even, not certain that they can be translated into the schemes devised in his latest researches. (p. 22)

Ensuring that von Neumann got the message, Morgenstern handed the text to him, and several days later wrote:

> [T]he Maxims are at Johnny's for eight days, but he has only read a part up to now. He questions whether one can make such a sharp division of the two classes. I believe so. Have checked up on everything: Kant, Menger, Schlick and M. Weber etc. and found nothing. If I'm right, then it is important. Even if the division cannot remain, the quantitative implications which are important to me stay. On Friday, Johnny wants to discuss it all with me. (OMDU, June 2, 1941)[18]

At the time Morgenstern was writing his Hicks review, von Neumann was completing the first part of his "manuscript," "Theory of Games I, General Foundations," the final element of our introduction to the collaboration. This he had begun during his summer sojourn in Seattle, where he lectured on game theory. The first part of the manuscript is dated October 1940, and the second part, "Theory of Games II," January 1941. Part I moves quickly from two-per-

[18] And so the subsequently inserted modifications to Morgenstern's Maxims paper reflect von Neumann's comments. For example, on the division of the two classes into restricted and unrestricted, a less plodding, more tightly argued two page insertion questions, following von Neumann, "whether the distinction is at all a clear cut one" (p. 6a). The Maxims paper remained unpublished. A note in Morgenstern's handwriting, jotted on the margin years later, reads "both JvN and Gödel urged me to work this out & publish it: (never done!)" (OMDU, p. 1).

son theory to the concept of the (characteristic) set function $v(S)$. Strategic equivalence and essentiality are defined, and the three-person game is analyzed in detail, showing how its solution is a system of possible "apportionments" (imputations). The notion of stability here is extended to the general n-person game: no two valuations in the solution dominate each other and every external valuation is dominated by at least one solution member. Morgenstern did not turn his full attention to this paper until July 1941, nine months after it was written, having presumably been occupied with the Hicks review in the interim.[19]

Turning to the manuscript, he wrote an introduction for economists to its first part: this was his "essay with Johnny," which he mentions often and immediately considered to be of great importance, and which von Neumann read at intervals, suggesting revisions.

> The essay with Johnny is preoccupying me a lot. I hope he is going to stick to it. There are going to be great representational difficulties, because the mathematics that is being used is very disparate (set theory, vector theory, higher algebra (bilinear forms etc.). I am absolutely convinced that it is a matter of great importance. It must be clearly shown what a minimax problem is. Johnny thinks one will elicit 99% stupid objections, but one just has to put up with it. The essay will probably only be a beginning. It's also going to start something in the whole discussion about duopoly etc.; and also as far as consistency of plans is concerned, but it will take a lot of time until the present, usual prattle about 'dynamic theory' stops. (Diary, July 12, 1941, OMDU)

Having initially considered sending it as a two-part article to the *Journal of Political Economy*, they agreed, by September, at Morgenstern's suggestion, that it should become a book for Princeton University Press:

> Will perhaps be 100 pages long and full of dynamite. The consequences are becoming clearer and clearer to me. I do not exaggerate when I say to myself that it is something that goes into the deepest depths of the theory, overturns many things and introduces many new and positive things. It will be a difficult but totally clear book. Johnny is a fantastic scientist! (Diary, Sept. 22, 1941, OMDU)

Von Neumann had finished "Theory of Games II," the second part of his "manuscript," in January 1941. Here, the theory is extended to cover non-zero-sum situations, and, adopting a more formal set-theoretic notation, he proves some simple theorems on stability and discusses decomposition of games. Throughout, the presentation is dense and rigorous, without any discussion of economic or any other applications. Morgenstern does not appear to have received this until October 1941, again nine months after it was written.[20]

The second part of von Neumann's manuscript was integrated after October 1941, and it quickly became clear that the book would be much larger than expected. Morgenstern was still hopeful that the theory would have something to say about dynamics, but as the collaboration wore on, it became evident that it would not. Throughout 1942, the analysis of a simplified poker was worked out,

[19] In July 1941, Morgenstern speaks of rewriting the introduction to the manuscript, having given it "close study" (OMDU, July 12). Of von Neumann's response, Morgenstern wrote: "[Johnny] wants elaborations. He is in agreement with all general comments but thinks a few things would be dealt with in greater detail because, while we would understand, others perhaps not. He also liked the presentation of game theory, and wants a few additions. I have already written a number of things, made a new comment (on the supposed inapplicability of physics and its methods as a model)" (OMDU, Aug. 14, 1941).

[20] Morgenstern wrote: "Thursday talked twice with Johnny at his place and he showed me the case (3,1) according the latest development. . . . Yesterday in the afternoon again, He gave me MS II (his) and he discussed it through with me yesterday. It will give me a lot of work but it is not insurmountable. It already covers the [non-zero sum] case" (Diary, Oct. 12, 1941, OMDU).

and Morgenstern delighted at the rapidity with which von Neumann constructed an axiomatization of measurable utility.[21] By December, they were discussing possible titles, and in January von Neumann headed off to England as consultant to the Navy. Following a delay imposed by his absence, the manuscript finally went to the publisher in April 1943.

IV.2 *von Neumann* and *Morgenstern*?

As we reflect upon the writing of the *Theory of Games*, several facts should be noted. Above all, while Morgenstern was completely absorbed by the project, for von Neumann, it was something of a secondary interest. In the years 1940–1943, when the book was being written, he was heavily involved in military consulting. Beginning in 1937, he was consultant to the Ballistic Research Laboratory at Aberdeen Proving Ground, Maryland, and he continued as such throughout the war. For a year from late 1941, he was involved with the National Defense Research Committee, dealing with the shaping of explosive charges, and from late 1942 to mid-1943, he consulted on operational research for the Navy's Section for Mine Warfare, spending the last part of 1942 in Washington, D.C., and the first half of 1943 in England.[22] It was during his British stay that he became interested in computing, something which dominated his interests thereafter. He also was heavily involved in the design of the atomic bomb at Los Alamos, again dealing with the direction of blast waves. However, time limitations notwithstanding, it is apparent from the evidence presented above that all the technical aspects of the theory may be credited to von Neumann: the formal development of the theory he appears to have carried out independently. What then was the contribution of Morgenstern?

By virtue of his training and natural abilities, Morgenstern was in no position to contribute to the technical game-theoretic details, whether it was the analysis of poker, or the construction of the stable set, the central solution concept of the *Theory of Games*. Time and time again, he bemoans his lack of mathematical and scientific training, and he was keenly aware of the intellectual gulf that separated him, like virtually everybody else, from von Neumann.[23] However, the two became very close friends and intellectual partners, and, in the division of intellectual labor, Morgenstern's role was crucial. What he did was to ask interesting, provocative, questions, and his ideas on economic plans, interdependence, and equilibrium were grist to von Neumann's mill. He focused the latter's attention, he acted as a spark:

> One of these days, I have to write down a few things about the story of the book (and my minimal share; but I seem to have acted as a kind of catalytic factor). (Diary, Jan.1, 1943, OMDU)

But Morgenstern brought more to it than a set of questions: certain features

[21] "Today at Johnny's: axiomatization of measurable utility together with the numbers. It developed slowly, more and more quickly, and at the end, after two hours (!) it was nearly completely finished. It gave me great satisfaction, and moved me so much that afterwards I could not think about anything else. . . . We . . . will not print the whole proof. There are still some serious questions there, especially the question of risk and pleasure of gambling. But that doesn't belong in the book; perhaps later" (Diary, Apr. 14, 1942, OMDU).

[22] This information is taken from a letter from von Neumann to Frank Aydelotte, October 11, 1945, Faculty Files, John von Neumann, Folder 1941–, VNIAS.

[23] Thus, "He is a mysterious man. The moment he touches something scientific, he is totally enthusiastic, clear, alive, then he sinks, dreams, talks superficially, in a strange mixture. His wife sees the big child in him" (Diary, Sept. 1, 1940, OMDU). Then a year later, "The more I get to know Johnny's mind, the more I must admire him. One is presented with the incomprehensible" (OMDU, Jan. 10, 1941).

of his personality were crucial to the whole enterprise. He was a malcontent, perpetually dissatisfied with virtually every aspect of economic theory he encountered. It is evident from his very earliest writings, where he emphasizes the impossibility of prediction and criticizes all attempts at doing so, through his writings of the 1930s, which are blistering critiques but offer little by way of alternative theory. His criticisms became even harsher once he started working with von Neumann.[24] This critical side, however, owes something to a genuinely intuitive, inspirational spark in Morgenstern, and he seems to have been driven to transform economic theory in a way of which he was personally incapable. Cursed by a combination of enormous intellectual ambition and limited theoretical ability, he turned to others: to Menger in Vienna, to von Neumann at Princeton. The image that emerges is that of a Morgenstern "never happy to join a club that would have accepted him," yet without these unique personality features, the *Theory of Games* would not have been written.

V. *Ars Combinatoria—von Neumann's Philosophy of Game Theory*

To understand what von Neumann hoped game theory to achieve, we must consider the theory's central solution concept in the light of our historical account above. In constructing the solution of an n-person game, von Neumann and Morgenstern introduce the dominance relation: one imputation, or outcome, x is said to dominate another y "when there exists a group of participants each one of which prefers his individual situation in x to that in y, and who are convinced that they are able, as a group—i.e., as an alliance—to enforce their preferences" (p. 38). As a solution to a n-person game, a set S has the characteristics that:

> No y contained in S is dominated by a x contained in S
>
> Every y not contained in S is dominated by some x contained in S

x and y being imputations. A solution is thus not a single imputation but a set of imputations, each set being stable in that none of its member imputations dominates any other, and every imputation outside the set is dominated by at least one imputation inside. Connecting the mathematical concept of "solution" with social phenomena, the intended breadth of the theory becomes evident. According to von Neumann and Morgenstern (1944), a "solution" may be correlated with a "standard of behavior" i.e. the particular set of rules, customs or institutions governing social organization at a particular time. To understand the analogy, they advise the reader to "temporarily forget the analogy with games and think entirely in terms of social organization" (p. 41, n. 1):

> Let the physical basis of a social economy be given,—or to take a broader view of the matter, of a society. According to all tradition and experience human beings have a characteristic way of adjusting themselves to such a background. This consists of not setting up one rigid system of apportionment, i.e. of imputation, but rather a variety of alternatives, which will probably express some general principles but nevertheless differ among themselves in many particular respects. This system of imputations describes the "established order of society" or "accepted standard of behavior." (p. 41)

The circular nature of the definition, based on dominance characteristics of the member imputations, lends each so-

[24] For example, "Last Thursday a report in the graduate course about Hayek's 'Pure Theory of Capital'. It is higher nonsense . . . They keep talking about 'Investment Function', but there is no question of stating the concept with any precision. He does not seem to know what a function really is. This type of 'economic theory' *must* vanish" (Diary, Mar, 15, 1942, OMDU).

lution a kind of inner stability. But a solution will be adopted only to the extent that the underlying standard of behavior commands general acceptance: the theory predicts neither which solution will be observed, nor which imputation within any solution will obtain. In contrast to other approaches, such as neoclassical general equilibrium or central planning, there is a huge range of indeterminism. Such other approaches, von Neumann and Morgenstern say, are invariably based on principles concerning distribution and other overall aims, e.g., profit and utility maximization, which are inevitably arbitrary and are usually supported by reference to arguments concerning inner stability or the desirability of the resulting distribution:

> Little can be said about the latter type of motivation. Our problem is not to determine what ought to happen in pursuance of any set of—necessarily arbitrary—a priori principles, but to investigate where the equilibrium of forces lies.
>
> As far as the first motivation is concerned, it has been our aim to give just those arguments precise and satisfactory form, concerning both global aims and individual apportionments. . . . *A theory which is consistent at this point cannot fail to give a precise account of the entire interplay of economic interest, influence and power.* (p. 43, emphasis added)

It thus becomes evident that the theory of games was intended to constitute a radical rupture with the Hicks-Samuelson variant of neoclassical economics. Von Neumann completely rejected the latter's underlying physical metaphor of classical mechanics and the associated mathematics based on the differential calculus. Echoing his remarks of a decade earlier on Guillaume (1932), he says that social phenomena clearly require theoretical categories and mathematical methods of a different kind:

> Our static analysis alone necessitated the creation of a conceptual and formal mechanism which is very different from anything used, for instance, in mathematical physics. Thus the conventional view of a solution as a uniquely defined number or aggregate of numbers was seen to be too narrow for our purposes, in spite of its success in other fields. (p. 45)

This break with mechanism is also reflective of those developments in quantum physics to which von Neumann had devoted his key years of the late 1920s. His *Mathematical Foundations of Quantum Mechanics* attempted to provide a basis for a fundamentally indeterministic quantum theory, which, once accepted, made impossible the existence of "hidden" causal variables (see Paul Feyerabend 1955; Trevor Pinch 1977; Léon van Hove 1958; Jonathan Powers 1982). It comes as no surprise that he should have regarded the causal social mechanics of neoclassicism as mathematically and philosophically antiquated.[25] In game theory, as in recent physics, any one-to-one correspondence between the theory and the world had been abandoned: in the spirit of Hilbertian mathematics, the theory bore only an abstract relation to social "reality":

> This definition should be viewed primarily in the spirit of the modern axiomatic method. We have even avoided giving names to the mathematical concepts introduced . . . in order to establish no correlation with any meaning which the verbal associations of names may suggest. In this absolute "purity" these concepts can then be the objects of an exact mathematical investigation. . . . *The application to intuitively given subjects follows afterwards, when the exact analysis has been completed. . . . The axiomatic models for intuitive systems are analogous to the mathematical*

[25] Thus, "[It] is to expected—or feared—that mathematical discoveries of a stature comparable to that of calculus will be needed in order to produce decisive success in [the mathematical analysis of social phenomena]. . . . *These observations should be remembered in connection with the current overemphasis on the use of calculus, differential equations, etc. as the main tools of mathematical economics*" (von Neumann and Morgenstern 1947, p. 6, emphasis added).

models for (equally intuitive) physical systems. (p. 74, emphasis added)[26]

The break with mechanism for the analysis of *structure*, evident in the theory of games, was a shift which characterized many disciplines in the early part of the 20th century, from physics to literary criticism (see Jean Piaget 1971; John Sturrock 1993). Writing shortly after the appearance of the *Theory of Games*, Weyl (1949, p. 237), mathematician and Institute colleague of von Neumann, says:

> Perhaps the philosophically most relevant feature of modern science is the emergence of abstract symbolic structures as the hard core of objectivity behind . . . the colorful tale of the subjective storyteller mind. . . . The [subject concerned] deals with some of the simplest structures imaginable, the combinatorics of aggregates and complexes. It is gratifying that this primitive piece of symbolic mathematics . . . accounts for some of the most fundamental phenomena in inorganic and organic nature. The same structural viewpoint will govern our account of the foundations of quantum mechanics . . . In a widely different field J. von Neumann's and O. Morgenstern's recent attempt to found economics on a theory of games is characteristic of the same trend.

Thus seen, the theory of games becomes more than a response to Morgenstern's earlier calls for a dynamic theory of interdependent economic decision. It becomes part of a general shift in science which involved, broadly speaking, the abandonment of determinism, continuity, calculus, and the metaphor of the "machine," to allow for indeterminism, probability, and discontinuous changes of state. The mechanical language of the calculus capitulated to what Weyl called "Ars Combinatoria," the use of set theory, the simple exploration of *structure*.

Taking this into account, we now see that what Menger's ethics and von Neumann's games share in common is much more *profound* than their collectively appealing to Morgenstern. For both Menger's analysis of ethics and compatibility groups, and von Neumann's theory of games and stable sets, were *part of the same shift* in the way mathematics was used as a tool in social theory. In the case of Menger, his approach to social theory—inextricably linked to his view of mathematics—was to examine the structural consequences, in the form of compatibility groups, of the adoption of particular ethical maxims. In the case of von Neumann, the formalist drive is even stronger. If mathematics is to serve any purpose in explaining social structures, a first step is the construction of an appropriate, modern mathematics, where both "appropriate" and "modern" have very particular meanings, both inseparably linked to the achievements of mathematics in post-mechanism physics: the way is clearly pointed to the analysis of social *structure*, ensembles of feasible social outcomes, stable sets, with the possibility of discontinuous passage from one to the other.[27]

[26] Referring to Hilbert's 1899 *Die Grundlagen der Geometrie* (trans. *Foundations of Geometry*), they discuss the completeness, freedom from contradiction, and independence of the game axioms, and essentially echo Zermelo's remarks on mathematics and psychology of 30 years earlier: "[It] is hoped that this may serve as an example of the truth of a much disputed proposition: That it is possible to describe and discuss mathematically human actions in which the main emphasis lies on the psychological side. In the present case the psychological element was brought in by the necessity of analyzing decisions, the information on the basis of which they are taken, and the interrelatedness of such sets of information (at the various moves) with each other. . . . There are of course many—and most important—aspects of psychology which we have never touched upon, but the fact remains that a primarily psychological group of phenomena has been axiomatized" (p. 77).

[27] Von Neumann continued to emphasize the stable set thereafter. In a 1953 letter to Harold Kuhn, he wrote that "over relatively long periods of time, one can meaningfully assert that the 'system' has not changed, while the positions of various participants within it may have changed many

VI. *Conclusion*

Fifty years ago, at the hands of von Neumann and Morgenstern, formal economic theory was given a certain orientation, a particular turn, which, like all such shifts in economics, displayed features of contingency, originality, and constraint. Had von Neumann and Morgenstern never met, it seems unlikely that game theory would have been developed: fortuitous historical accident played a role. Given that they did collaborate, however, their creative innovation was both highly original *and* guided by certain constraints operating on both of them. The latter were constituted by developments in the history of mathematics, changes in the use of mathematics in social and physical theory, and changed expectations of theory in a world where historical and descriptive economics were being increasingly deemed inadequate. Thus viewed, the creation of game theory takes its place as part of a broader intellectual shift, a movement which, beginning in the 1920s, affected a range of disciplines from science to literary studies, and became known as "Structuralism."[28]

Since von Neumann and Morgenstern, game theory in economics has developed in new and quite different directions. These include, inter alia, the linking of the core and competitive general equilibrium in the late 1950s and 1960s, and the important sequence of refinements of the Nash equilibrium following Selten (1975; see Aumann 1989). Related to the latter has been the increased emphasis on models of learning and constrained rationality, and on laboratory experimentation. The work has taken on a dynamic, behavioralist flavor, with a stronger focus on the exploration of individual rationality than was present in the static, structuralist, *Theory of Games*.[29] That research would unfold in this manner was beyond prediction in 1944, but the roots of these developments lie in the historical interlude described above.

times. This would seem to me to be the analogue of a single solution and and 'exploration' of the imputations that belong to it. After relatively long times, there occur discontinuous changes, 'revolutions' which produce a different 'system' (von Neumann to Kuhn, April 14, Container 24, File: Kuhn, H.W., VNLC). Later, at an informal conference on game theory held at Princeton in January 1955, "The discussion opened with a statement by von Neumann in justification of the enormous variety of solutions which may obtain for n-person games. He pointed out that this result was not surprising in view of the correspondingly enormous variety of observed stable social structures; many differing conventions can endure, existing today for not better reason than that they were here yesterday. It is, therefore, still of primary importance to settle the general question of the existence of a solution for any n-person game" (Philip Wolfe, ed. 1955, p. 25). (That not all n-person games necessarily have stable set solutions was subsequently shown in Stearns (1967) and Lucas (1968) for the cases of nontransferable and transferable utility, respectively). In this connection, it is interesting to consider the theoretical shift shared by Menger and von Neumann in the light of their mittel-European backgrounds: Menger's interest in ethics was sparked partially by social crisis (see 1974, p. 114), and von Neumann's personal history, too, left him with a strong sensibility to the contingencies of social structure. In both cases, scientific views and personal history fuse together to form a densely textured web (see Leonard 1993; 1994b, forthcoming).

[28] The formalist approach to general equilibrium theory of Gerard Debreu, which appeared later, may be seen as another manifestation of this structuralist turn via the influence of the Bourbaki school of mathematics (see Weintraub and Mirowski forthcoming).

[29] In this context, it is interesting to note both von Neumann's opposition to the Nash equilibrium and his apparent disinterest in experimentation. On the former, see von Neumann and Morgenstern ([1944] 1947, p. 15, p. 44) and Leonard (1994a). On the latter, see Leonard (1993, forthcoming).

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