Oskar Morgenstern's Contribution to the Development of the Theory of Games

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The theory of games represents one of the few genuine social-scientific inventions of the twentieth century. Unlike many contributions to social science which rely on or are applications of some preexisting theory, game theory was developed by von Neumann and Morgenstern *ex nihilo*. Despite some similar work by Émile Borel, there are almost no precursors to the book *The Theory of Games and Economic Behavior* (hereafter called *The Theory*). To be assigned the task of measuring Oskar Morgenstern's contribution to this invention is like being a journalist asked to investigate James Crick's contribution to the discovery of the double helix after both Watson and Crick had died. This is a difficult task since intellectual imputation among scholars is very awkward, especially when an "imputee" is one of the twentieth century's greatest minds. Despite this difficulty, however, I will begin my essay by discussing the topic assigned to me in its narrow form, What was Oskar Morgenstern's contribution to the development of the theory of games? As a corollary I will discuss some subsidiary topics such as, How would game theory be different if someone else had collaborated with von Neumann? What future events in game theory and economic theory were anticipated by von Neumann and Morgenstern in 1944 and what developments would Morgenstern have been most excited about if he were still alive today?

Before I start, let me list my credentials for this assignment. First of all, *The Theory* was written in 1944, three years before I was born. Hence, it should be obvious that my insights come not at all from personal observation. I cannot claim to have been some invisible third person sitting in von Neumann's livingroom while he and Morgenstern

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wrote their famous book. In fact, quite the opposite is true. I knew Oskar Morgenstern at New York University after his retirement from Princeton and up until his death. I was in some sense his last student. But his lectures in the 1970s clearly had the same zeal and enthusiasms as they must have had in the 1950s (in fact I am quite sure they were the same lectures since we only studied *The Theory*). Hence, in some indirect way I feel that I too was there in the beginning and was privileged to insights into his relationships with von Neumann and others.

Oskar Morgenstern's Contribution to the Development of Game Theory

Contributing to *The Theory*

As Morgenstern states in his essay on collaborating with von Neumann (Morgenstern 1976) one of the questions most asked of him in the years after von Neumann's death was what exactly was his contribution to the theory of games and to their collaboration. Clearly, when one collaborates with a recognized genius one is assumed to be a junior partner until proven otherwise. In his essay, Morgenstern spells out the answer in great detail; I will discuss it shortly. Whenever I have been asked the question of Morgenstern's contribution, my response has been to ask the questioner what he or she thinks he or she would produce if he or she had access to John von Neumann for one year. Upon reflection, the response is usually silence since the questioner soon realizes that having access to von Neumann is no guarantee that one would be able to interest him in any problem, let alone produce something as grand as *The Theory*. Quite simply, Oskar Morgenstern was a visionary constantly on the lookout for the new and the unusual. Unlike many visionaries who ultimately are looked upon as kooks, Morgenstern was capable of transforming his vision into tangible and pathbreaking works. His freedom from the strictures of Anglo-American neoclassical training (he was a product of the Austrian School) allowed him to see a new path for mathematical economics, one freed from the physicslike maximizing models that Samuelson and others were creating. It was this visionary quality that combined so well with von Neumann's mathematical abilities to produce a new tool and a new field of inquiry.

In concrete terms it is very simple to list Morgenstern's contribution to *The Theory*. To begin, as the book's title explains, there are two parts to the 1944 classic—game theory and economics. As must be obvious, the pure game theory contained in these pages was clearly a creation of von Neumann's, although I think that his choice of topics, especially on the cooperative game side, was heavily influenced by Morgenstern. In terms of economics, however, *The Theory* was a natural outgrowth of several earlier ideas of Morgenstern's and must be appreciated as a milestone in the evolution of Austrian Economics.

To begin, in his early work on economic forecasting, *Wirtschaftsprognose* (1928) and "Volkommene Voraussicht und Wirtschaftliches Gleichgewicht" (1935) (translated by Frank Knight as "Perfect Foresight and Economic Equilibrium" in Schotter 1976), Morgenstern clearly saw the problem of strategic interaction among economic agents as the central problem and the individual maximizing model of neoclassical economics as an inadequate representation of it. For him, economics consisted of both "dead" and "live" variables in which the dead variables' values were determined by nature while the live ones' were determined by their actions and the actions of others. In order for an agent to decide how to behave rationally in such circumstances, that agent must know how others are expected to behave, but these actions involve a similar expectation on the part of others. Hence, Morgenstern saw the perfect foresight assumption, so critical to the general equilibrium theory of the time, as a contradiction. If people had perfect foresight into others' actions, then unless all of these expected actions actually formed an equilibrium, someone would deviate and not behave as expected to. This idea was to resurface later under the banner of the Rational Expectations Equilibrium, and I will return to that point later on. The work of Morgenstern is probably the first statement of the rational expectations problem. Such a situation seemed circular to Morgenstern but was clearly the essence of "interesting" social science. This circularity was expressed most clearly by Morgenstern in his example of Sherlock Holmes and Professor Moriarty, presenting probably the first instance of a game with a mixed strategy equilibrium to appear in an economic article.

> Sherlock Holmes, pursued by his opponent, Moriarty, leaves for Dover. The train stops at a station on the way, and he alights there rather than travelling on to Dover. He has seen Moriarty at the railway station, recognizes that he is very clever, and expects that Moriarty will take a special faster train in order to catch him at Dover. Holmes' anticipation turns out to be correct. But what if Moriarty had been still more clever, had estimated Holmes' mental abilities better and had foreseen his actions accordingly? Then obviously he would have

> travelled to the intermediate station. Holmes, again, would have had to calculate that, and he himself would have decided to go on to Dover. Whereupon Moriarty would have "reacted" differently. Because of so much thinking they might not have been able to act at all or the intellectually weaker of the two would have surrendered to the other in the Victoria Station, since the whole flight would have become unnecessary. Examples of this kind can be drawn from everywhere. However, chess, strategy, etc. presuppose expert knowledge, which encumbers the example unnecessarily. (Morgenstern 1935, reprinted in Schotter 1976, 174)

For someone with these interests, the theory of games was a natural end product and, in fact, it was precisely this work that first stimulated the collaboration between von Neumann and Morgenstern.

Morgenstern was a visionary, and his vision can be seen most clearly in the introduction to *The Theory*. In this introduction three major things are accomplished. First the problem for economic science is shifted from a neoclassical world composed of myriad individual Robinson Crusoes existing in isolation and facing fixed parameters against which to maximize, to one of a society of many individuals, each of whose decisions matter. The problem is not how Robinson Crusoe acts when he is shipwrecked, but rather how he acts once Friday arrives. This change of metaphor was a totally new departure for economics, one not appreciated for many years. Second, the entire issue of cardinal utility is discussed, a problem whose importance to economics was clearly known to Morgenstern. While the proof of the existence of a cardinal utility was most certainly left to von Neumann, who thought the whole thing quite obvious, the axiomatization was obviously influenced by Morgenstern. An attempt is made to keep the axioms as close as possible to those needed to prove the existence of an ordinal utility function under certainty—a concern that Morgenstern must have felt more strongly than von Neumann. Finally, the entire process of modeling exchange as an n-person cooperative game and searching for a "solution" is described. Here one is struck by how Austrian was the economics used in *The Theory*. The presentation is clearly motivated by Böhm-Bawerk's example of a horse market—a market in which the "marginal pairs" determine equilibrium price. While the neoclassical theory of price formation was calculus-based and relied on first-order conditions to define equilibrium, game theory, especially cooperative game theory, relied more on solving systems of inequalities (such as the inequalities determining the core). While the neoclassical solution would often be included within the set of cooperative solutions (and would often prove to be the limiting solution as the number of agents in the market approached infinity), the theory of games offered new and appealing *other* solutions to the problem of exchange. This non-neoclassical emphasis, I feel, is another of Morgenstern's contributions to the development of game theory.

Finally, it is clear that Morgenstern saw game theory, or at least the von Neumann–Morgenstern (stable set) solution, as a formalization of a revived "neo-institutional" economics. Such an institutional emphasis was, of course, seen in earlier work by Menger (1883) where he investigates the organic or unplanned creation of social institutions. For Morgenstern the institutional question arises because he saw game theory as a tool to allow social scientists to define the set of possible, mutually exclusive, institutional arrangements that could emerge from a given social situation. If you substitute the words "institutional arrangements" for "orders of society" in the quotation below you will see the point being made.

> The question whether several stable "orders of society" based on the same physical background are possible or not, is highly controversial. There is little hope that it will be settled by the usual methods because of the enormous complexity of this problem among other reasons. But we shall give specific examples of games with three or four persons, where one game possesses several solutions in the sense of 4.5.3 [the stable-set solution]. And some of these examples will be seen to be the models for certain simple economic problems. (von Neumann and Morgenstern 1947, 43)

To give an example of exactly how Morgenstern saw the institutional aspect of game theory and how he envisioned its being different from the standard neoclassical analysis, consider the following example taken from an early draft of a paper by von Neumann and Morgenstern (the only paper I know of on which they collaborated after the *Theory of Games* was written) which was left unfinished, but completed almost thirty years later by Morgenstern and Schwödiauer (1976).[1]

Consider a simple partial-equilibrium analysis of a market in which there is one seller who has two units of a good to sell and three buyers, all

1. The analysis presented here is also described in Braunstein and Schotter 1978.

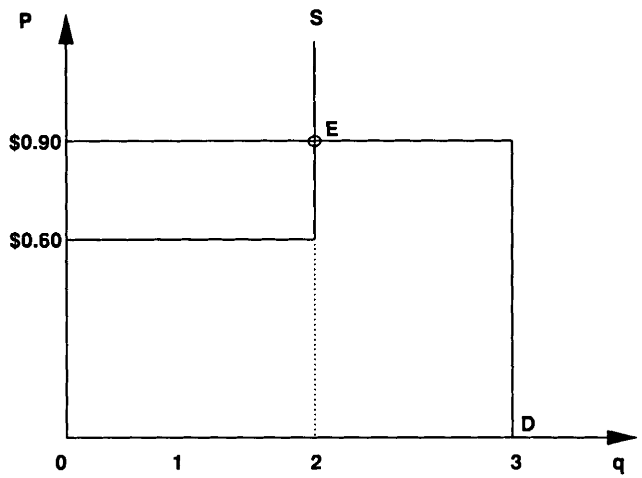

Figure 1

of whom want to buy one unit. Further assume the seller will sell any unit for a price of $0.60 or more, while each buyer is willing to buy any unit for a price up to, but not exceeding, $0.90. With this information, the conventional analysis of price formation would proceed by constructing the appropriate supply and demand curves and solving for that price at which supply equals demand (see figure 1).

In the neoclassical analysis the question is asked, What price or value relationships are consistent with the hypothesized institutional structure of perfect markets? The emphasis is on values given institutions. Consequently, since the buyers' side of the market is the long side, the price is supposedly bid up to the buyers' reservation price, yielding an imputation in which the seller receives $0.30 profit on each unit sold, while the buyers' consumer surplus is reduced to zero or an imputation of $x = (60, 0, 0, 0)$. The market price, $0.90, is associated with this imputation, and if the analysis is accepted as valid, an equilibrium institution-price relationship is established.

But is this really the relevant question? In other words, is the relevant question what the equilibrium price is, consistent with the assumption that perfect markets exist, or should we, as social scientists and economists, be asking which equilibrium institution-price pair will emerge as the stable pair, or set of pairs, in this situation of strategic and social interdependence. If it turns out that the only stable institution-price pair is the competitive market-competitive price pair (which, by the way, is a feature of Lucas's [1968] counter-example demonstrating the nonexistence of a stable-set solution to a ten-person game which Shapley and Shubik [1969] in turn demonstrate to be a market game) then we can say that the neoclassical analysis is not institutionally myopic in its assumptions. But if institutions other than competitive markets might emerge and define different equilibrium value relationships which may involve a set of personalized (hence not competitive) prices, then the neoclassical approach is myopic in not being able to define these equilibrium institutions and their associated pairs as logical possibilities.

To make this point more precisely, Morgenstern and Schwödiauer look at the cooperative game-theoretical analysis of the exchange situation described above. To do this they must define the characteristic function associated with this situation by defining the value of any coalition as the maximum sum of the consumers' and producers' surplus that can be achieved by any coalition of traders. Letting the seller have the index 1 and the buyers the indices 2, 3, and 4, and assuming transferable utilities and side payments, the characteristic function appears as

$$V(1) = V(2) = V(3) = V(4) = 0$$

$$V(12) = V(13) = V(14) = 30, V(23) = V(24) = V(34) = 0$$

$$V(123) = V(124) = V(134) = 60, V(234) = 0$$

$$V(1234) = 60$$

Now if buyers restrict their behavior to coalition-forming behavior or "blocking"-recontracting behavior, then the equilibrium imputation associated with this behavior is the unique core imputation $x = (60, 0, 0, 0)$ in which the seller sells both units for a price of \$0.90, thereby extracting all of the consumer surplus from the buyers. This outcome is then identical with the outcome attained by the neoclassical market. However, if one wanted to, in this case, one could say that this blocking or recontracting behavior defines a competitive market–competitive price pair as the equilibrium institution-price pair for this situation of exchange. But traders need not restrict themselves to this type of behavior. There may be other standards of behavior that lead to other stable institution-price or imputation pairs as defined by the von Neumann–Morgenstern solution, and these equilibrium institutions should not be ruled out a priori by the assumption that only markets of the competitive type exist in the economy under investigation. For instance, the buyers, realizing that blocking or recontracting will inevitably lead to the core, may form a cartel and refuse to bargain with the sellers except as a unit. If they do this, then there are several equilibrium-stable institution-price pairs that might emerge, as Morgenstern and Schwödiauer (1976) demonstrate. For instance, the following four sets of imputations collectively define the total symmetric sets of equilibrium institutional relationships. (Here x_2 is the buyer with the biggest imputation, x_3 the second biggest, and x_4 the smallest.)

The core-market: $X^1 = \{x \,|\, x = (60, 0, 0, 0)\}$. (1)
This is the unique core imputation yielding a price per unit of \$0.90. It is identical to the neoclassical market solution and hence we call it a market solution.

The two-trader symmetric cartel: $X^2 = \{x \,|\, x_1 = (60 - x_2 - x_3 - x_4, 15 \geq x_2 = x_3 \geq 0, x_4 = 0)\}$. (2)
Here two buyers form a cartel and exclude the third buyer. The set of imputations defined is one in which the price is reduced below its competitive market price of \$0.90 to any price between \$0.90 and \$0.75. All traders buy at the same price.

The three-trader symmetric cartel: $X^3 = \{(x \,|\, x_1 = 60 - x_2 - x_3 - x_4, 20 \geq x_2 = x_3 = x_4 \geq 16^{2/3})\}$. (3)
Here all three buyers form a cartel and bargain with the seller as a unit. Because of their collusion they are able to force the price into the interval between \$0.60 and \$0.65.

The three-trader asymmetric cartel with discriminated buyer: $X^4 = \{x \,|\, x_1 = \{(60 - x_2 - x_3 - x_4, 18^{1/3} \geq x_2 = x_3 = c \geq 15, 37^{1/3} - C \geq x_4 \geq 0, x_4 \in M(C))\}$. (4)
Here the three buyers form a cartel, bargain with the seller as a unit, but do not split the gains from collusion equally. Rather, one buyer is discriminated against, who we are assuming in this case is buyer 4, and is merely given a side payment for his cooperation.

Notice that all of these different standards of behavior or institutional arrangements are logical possibilities for what may emerge from the situation of primitive exchange described before, and that neoclassical theory misses the opportunity to predict the emergence of any institution other than the competitive market (in this case X^1), which it assumes to exist at the outset of the analysis. Hence, from an institutional point of view, the analysis must be considered myopic.

This emphasis on institutional analysis is distinctly Mengerian and most certainly a contribution by Morgenstern. It is probably no surprise that the work of Martin Shubik (Morgenstern's most famous student) has consistently been in what he calls "mathematical institutional economics." Nor is it surprising that he, in collaboration with his coauthors Lloyd Shapley and Pradeep Dubey, should have pursued models with very great institutional detail since that pursuit was clearly implied as the mission of game theory in *The Theory*. This connection of institutions and game theory has also been highlighted by Sugden (1986) and Schotter (1981) as well as by the more mathematical versions of Williamson's "new institutional economics."

In summation, *The Theory* was a natural outgrowth of the work of its two coauthors, one who pioneered the study of games of strategy and the other who cared greatly about the problem of strategic interaction in social and economic affairs.

Contributions after *The Theory*

What influence did Oskar Morgenstern have on the development of game theory after *The Theory* was written? By influence I mean only that influence created by his published work. As anyone familiar with Morgenstern will know, his influence in the field at the time came from many sources, not the least of which was his role as a mentor and catalyst for others. His Econometric Research Program at Princeton in the 1950s was a great meeting place where game theorists congregated and where game theory flourished. When a new field is created out of thin air, one of the ingredients necessary for the field to grow is a strong leader who will encourage others and arrange for opportunities for the best people to find positions and obtain research funding. Morgenstern had a long history as such an entrepreneur extending back to his days in Vienna where he ran the Institute of Business Cycle Research and, among other things, arranged for Abraham Wald to come to America. His leadership at Princeton is well known to those who passed through but will not be the focus of my survey here. I will concentrate on his published work only.

In this connection it is unfortunate, but in some sense not surprising, that Morgenstern did very little in the way of game-theoretical application in the years after the publication of *The Theory*. In fact, aside from some surveys and his work on the von Neumann model of an expanding economy with Thompson, Kemeney, and others, I cannot think of another article than the one mentioned above written with Schwödiauer, that can be considered either an application or an extension of the theory. Rather, Morgenstern went on to write in a wide variety of fields including the predictability of stock market prices, the von Neumann model of an expanding economy, the use and abuse of economic statistics, matters of national security, and many others. Hence Morgenstern's written influence on the development of game theory stopped with the publication of *The Theory*. Still, the agenda for the application of game theory to economics in the fifties, sixties, and seventies was skewed substantially toward the application of cooperative games. I feel this agenda was heavily influenced, at least indirectly, by Morgenstern through the work of Martin Shubik and Lloyd Shapley, the first and most influential authors to pick up the call to arms offered in *The Theory* in a manner consistent with its intent. While the noncooperative theory has become the main tool used today in economics, such an emphasis might have occurred earlier had not attention been focused on the core solution concept in the period up until the late 1970s. Further, although the book is entitled *The Theory of Games and Economic Behavior*, it was clearly conceived as a general theory for all social science. As Professor Riker discusses elsewhere in this volume, cooperative game theory was the major game-theoretic tool used by political scientists for at least forty years after *The Theory* was written. In fact, the analysis of simple games in *The Theory* already contained the central element for the study of voting games developed by Riker, Ordeshook, McKelvey, and others in the 1970s. It always was on Morgenstern's agenda to give social science a new, and more flexible, mathematics with which to study social phenomena, and the cooperative theory clearly seemed well suited for that purpose. In fact Morgenstern notes that von Neumann was always amazed at the primitive state of mathematics in economics; he commented in the 1940s that if all economics texts were buried and dug up one hundred years later, people would think that the economics they were reading was written in the time of Newton. This emphasis on cooperative game theory and the belief that the proceeds of bargaining, threatening, and bluffing are of central empirical importance to social and economic life stem directly from Oskar Morgenstern, and I think strongly bear his imprint.

Later Events Anticipated by *The Theory*

In this section I will attempt to outline the various later developments in the theory of games and its economic application that were anticipated by Morgenstern and von Neumann in their original work. Let me say, however, that my definition of the word "anticipated" here is rather broad since I include all work that is consistent, at least in spirit, with *The Theory*. Finally, in addition to outlining what was anticipated, I will also indicate what I feel were developments that could not be considered natural outgrowths of the work contained in *The Theory*.

Anticipated Materials

Clearly the work in the 1960s and 1970s on the core of an exchange economy is the most natural outgrowth of the analysis investigated in *The Theory*. Von Neumann and Morgenstern had already defined the characteristic function for analysis of markets, although they had clearly pinned their hopes on the stable-set solution. The idea of the core, a subset of every stable-set solution, was not foreign to them. It was, of course, left to others such as Martin Shubik, Herbert Scarf, Gerard Debreu, Lloyd Shapley, and Robert Aumann to demonstrate the relationship between the core and the conventional Edgeworth contract-curve analysis. Despite the elegance of this literature, the limit theorems on the core might still be considered a disappointment to Morgenstern. While he clearly wanted a theory that could include the competitive analysis as a special case, I think that he had hoped that something more than the competitive equilibrium in the limit would exist. The idea that as societies grow the only stable "orders of society" or institutional arrangements are competitive markets is rather inconsistent with the spirit of the stable-set analysis.

We have evidence that Morgenstern held such a view. In a later paper (Morgenstern 1949, published in Schotter 1976) he states,

> The economist's prime interest in the success of the extension of the number of players will naturally be whether the present basic belief that large numbers of buyers and sellers secure a free competition where each individual faces a pure maximization problem, finds any strengthening or not. As far as I, personally, can see there will be nothing of this kind, although some sort of asymptotic behavior of the solution may emerge. (298)

The Nash-Shapley-Harsanyi cooperative analysis of bargaining is another topic that naturally follows, at least in spirit, from *The Theory*. Bargaining is the prime mover of prices and allocations in the new game theory (in both the small- and large-numbers cases) and the cooperative theory of bargaining is consistent with this world view. In addition, the fact that early bargaining theories were axiomatized was, methodologically, a natural extension of the approach von Neumann and Morgenstern used in their utility analysis—an analysis that presented the first set of axioms in economics.

The axiomatization of fairness and its equivalence with the Shapley value was welcomed by Morgenstern. If bargaining was to be the prime mover of social interaction in the new theory of games, then it would have been natural for a corresponding moral theory to be developed that was based on the rationality of the players and on nothing else. As we will see later, to the extent that these notions serve a selection criterion of unique equilibria, I will show that such a consequence would not have been welcomed by Morgenstern.

Unanticipated Events

Little of what eventually happened to the field in the 1980s, especially the recent work in noncooperative game theory, was clearly envisioned in 1944, since *The Theory* did not pursue this analysis past its zero-sum minimax formulation. In fact, it is quite remarkable that the Nash equilibrium concept was not defined nor its existence proven in *The Theory* since at least Morgenstern was well aware of Cournot's work. I suspect the main reason for this omission is that both von Neumann and Morgenstern were looking for a way to break the circularity of the "I think he thinks that I think" logic of strategically interdependent situations. They wanted to provide a way for players to behave that was independent of their expectation of what their opponent intended to do. This is, in fact, exactly what the minimax strategy does since when one chooses a minimax strategy, one is *guaranteed* a certain return, no matter what one's opponent does. The Nash equilibrium does not provide this independence, since under such an equilibrium both parties must have correct conjectures about the actions of the other. Hence, it was not the type of solution they were looking for. Ultimately, however, they would have been driven to it since in most variable-sum games minimax strategies for players do not form an equilibrium. Von Neumann and Morgenstern also used the minimax formulation to define the characteristic function for a game, and did so for the reasons stated above. Their formulation ran into some conceptual problems that were later dealt with by the β and α definitions of the characteristic function and by Rosenthal's work on the effectiveness function forms for games.

Much of what we find interesting today in game theory and its application was not anticipated by (i.e., in the spirit of) *The Theory*. This is not to say that von Neumann and Morgenstern would not have found them interesting or important; it simply means that they were not directly in line with their thinking *at that time*. For example, the recent interest in refinements of Nash equilibrium, while probably exciting to Morgenstern personally, was not a natural outgrowth of *The Theory* given its lack of focus on the Nash concept. As noted before, the entire emphasis in *The Theory* was to find a decisive way to act that avoided any thought of what action one's opponent might take. Hence, elaborate analyses of beliefs off the equilibrium path were not consistent with this view.

Further, the extent to which refinements focus on choosing a unique outcome for each game is the extent to which that program is inconsistent with the world view expressed in *The Theory*. For Morgenstern, indeterminacy was not something to run from but rather to embrace.[2] The world is uncertain and social situations interesting only because they contain indeterminacies that many physical situations do not. To explain this belief, Morgenstern and Thompson (1976) present the following example.

> The great physicist W. Nerst, who established fundamental laws of thermodynamics, showed a long time ago the difference between physical mechanisms and social processes by the following devise: Assume a body to be in the position A upon which two forces 1 and 2 work as shown in the figure below [see figure 2]. Then it is well known that the body will move along the dotted resultant toward A′. But Nernst said, suppose the "body" is a dog and in (1) and (2) are two sausages, each exerting an influence on A. Clearly, the dog will not walk to Point A′ between (1) and (2). (Morgenstern and Thompson 1976, 9)

2. This view is not shared by John Harsanyi, who is a strong advocate of the proposition that social-scientific theories should have determinate predictions. Harsanyi (1976) states that the main reason why game theory had not, as of that date, found extensive application is that von Neumann and Morgenstern's approach "in general does not yield determinate solutions for two-person nonzero-sum games and for n-person games."

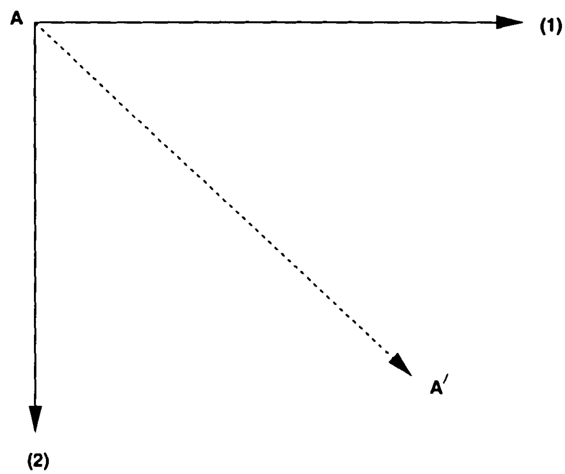

Figure 2

This is not to say that considerations of rationality should not be used to *narrow down* the set of possible equilibria that may emerge, but narrowing down is not the same as choosing one uniquely.

From an institutional view, the entire literature on mechanism design, while institutional in focus, concentrates more on the planned aspects of economic and social institutions and less on Menger's (and Morgenstern's) emphasis on unplanned orders of societies. I do not personally think that Morgenstern saw game theory as a tool for social planning and institutional engineering, although clearly this literature is some of the most exciting to be written in the past twenty years. The literature is too "un-Austrian" for Morgenstern.

Given his appreciation for the indeterminacy of social situations, I feel that Morgenstern also would have appreciated much of the work on repeated games (despite the fact that repetitions do nothing to change the analysis of zero-sum games). It is ironic, however, that he would have appreciated just that aspect of this literature that frustrates those who work in it—the large indeterminacy of the folk-theorem. For Morgenstern, it would not be surprising (or undesirable) that repetition of a super-game would lead to cooperation possibilities that were not available in one-shot play. For example, the emergence of cooperation in prisoners' dilemma games is clearly something that could be expected, particularly because one can view the play of repeated noncooperative games as a version of play of a cooperative one-shot bargaining game.

Finally, there is the modern literature on industrial organization, entry prevention, reputation building, signaling, and sequential equilibriums. One remarkable aspect of *The Theory* is its lack of attention to the noncooperative oligopoly problem despite, as mentioned earlier, Cournot's earlier work. In this sense one must place this literature in the unanticipated category (although the authors of *The Theory* would not have been surprised that their work was applied to the oligopoly problem. In fact this problem is mentioned on the very first page of the book, yet never presented anywhere as an example of the theory's potential applicability). Furthermore, many of these models are incomplete-information models in which the actions of incumbent firms are viewed as signals of their hidden costs. To the extent that bluffing possibilities exist in these models we see analyses that, in spirit, are very much like the bluffing analysis described by von Neumann and Morgenstern in the discussion of poker.

Most Appreciated Events

Since Morgenstern's study of strategic interaction was stimulated by his interest in the problem of perfect foresight and prediction, the recent work on rational expectations equilibria was a welcome event. This concept, in some sense, solves the problem that motivated Morgenstern's interest. Morgenstern's prediction problem is solved by Muth, Lucas, and others by assuming that all agents in the economy make their predictions using the same model of their situation. Hence, if all agents in an economy had the same "objectively correct model" and formed their forecasts of the future on this basis, the Rational Expectations Equilibrium forms a Nash Equilibrium in beliefs and actions. *At the equilibrium* the circularity of beliefs is settled and Morgenstern's original problem disappears. Still, the Austrian in Morgenstern would not have tolerated such a simplistic view of the world. For example, in the small-numbers case a Rational Expectations Equilibrium might be vulnerable to the problem of "theory absorption" discussed by Morgenstern and Schwödiauer (1976). Theory absorption asks whether a solution concept (or equilibrium notion) like the Rational Expectations Equilibrium or the core will be adhered to if people understand the theory behind it. For example, in a rational expectations model assume that I know that all agents are forecasting the price in the market using the same theory, and that it is common knowledge that all people are doing so and using these forecasts to guide their behavior. Further, assume that one of the parameters in the model is unknown and must be estimated using market data. If I am "large" enough I might try to manipulate the beliefs of others by deviating and acting as if I was forecasting prices using some other theory. This might then corrupt the data that others are observing and lead them to change their behavior to my benefit. Hence, knowledge of the rational expectations theory may, once absorbed, lead to a breakdown in the predicted equilibrium. When the number of agents in the economy is "large" such problems do not arise since no agent can corrupt the aggregate data by failing to adhere to the commonly used model. In fact, Morgenstern's original prediction problem must have been a small-numbers problem since even that problem would fail to exist with agents of measure zero. This same phenomenon of theory absorption was demonstrated to be serious for the core solution concept in an experiment done by Braunstein and Schotter (1978).

Beyond this, Morgenstern would probably have trouble agreeing with the assumption that all agents adhere to the same theory of the economy, a theory describing the objectively correct model of the economy. In fact, Morgenstern (1935) had already anticipated the rational expectations common-model solution and rejected it since economics even now is not yet a science for which we have a commonly accepted correct theory. He states, "Herein lies a contradiction which, in my opinion, can not be avoided for the aforementioned case, wherein full knowledge of the science, still not existing, has to be attributed to individuals because of full foresight" (Morgenstern 1935, in Schotter 1976, 176).

My feeling is that Morgenstern would have been more inclined to think of the agents in the world as adhering simultaneously to many theories and to think, in truly Austrian fashion, that many subjectively correct models of the real world exist, reality being determined, in part, by the different subjective models that people use. A sunspot model might even be closer to the type of analysis he might have envisioned. Hence, while in some sense the theory of rational expectation equilibrium would have been a very welcome event for Morgenstern since it dealt with precisely the problem that first aroused his interest in game theory, its treatment in the profession might ultimately have left him dissatisfied.

Although nothing in *The Theory* indicates this, the modern growth of experimental economics and experimental game theory was an "anticipated event." In a later paper (1954) Morgenstern commented on the already-published experiments of Chamberlain (1948) and clearly spelled out the possibility of reforming economic experiments. Since this topic is covered in depth by Vernon Smith, I will not pursue it further except to say that the model of physics experiments was one firmly planted in Morgenstern's mind, and as he explains in Morgenstern 1954 it is not a great leap of imagination to expect experiments to be done in economic and game theory.

Finally, since von Neumann was a pioneer in the development of computers and automaton theory and had influenced Morgenstern in this direction, one would think that the use of automatons as players in games would have been a welcome event. Clearly, on a personal level it would have been. The extent to which automatons represent less-than-fully-rational players with limited memories, however, is the extent to which the original spirit of *The Theory* may be violated. Still, it is hard for me to think that Morgenstern would not have welcomed the introduction of bounded rationality into games, perhaps along the lines of Radner 1987 or along the lines of Rubinstein 1986 and Neyman 1985. *The Theory* was simply too much of a first step to have taken things this far. In fact, the calculation requirements for the players in the types of games studied in *The Theory* is quite limited. Since they rarely get beyond four-person games, it is likely that when the combinatorial and calculating magnitudes of the problems got big, they too would have resorted to some type of bounded rationality premise to make sense of the situation.

Conclusion

Without Oskar Morgenstern we would not have the theory of games as we know it today. That does not mean that someone else would not have taken up work on von Neumann's problem, but it does mean that game theory would probably not have been introduced into the social sciences until many years later. Despite von Neumann's technical powers, the course of economics was changed by Morgenstern's focusing attention on a mode of analysis that has only recently come to be the dominant mode for all economics. The theory of games needed a non-neoclassical leader since it represented a fundamental break in economic thinking that would have been ruined if placed in the hands of a more conventional mind. The importation of Austrian imputation theory set the agenda for research in the 1960s and 1970s, and, whatever one thinks of the literature on the core, it is directly attributable to the change of emphasis initiated by Morgenstern in *The Theory*. I can think of no other economist at the time who could have walked into a room with John von Neumann and walked out later with a 600-page book on the theory of games complete with economic examples. That fact speaks for itself.

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