Game Theory at Princeton, 1949–1955: A Personal Reminiscence

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The report given here is clearly part of a Rashomon scenario. It presents a somewhat informal view of what was happening in game theory at Princeton during some of its early years seen through the eyes (and ego?) of a junior participant at the time. The view is impressionistic and undoubtedly biased in spite of my best intentions. The first draft of this essay was written quickly, from memory. This version has benefited from a reading of my diaries and from discussions with Lloyd Shapley and Herbert Scarf which enabled me to correct some outright errors and to more fully appreciate the Rashomon aspects of "eyewitness" reports.

I arrived in Princeton in the fall of 1949 with the express intention of studying game theory. I had sat in the library of the University of Toronto and attempted to read *The Theory of Games and Economic Behavior* in 1948 and was convinced that even though I scarcely understood the details of the mathematics, this was the right way to start to mathematize much of economics, political science, and sociology. The key feature was that it provided a language to describe precisely many of the key concepts of strategic analysis.

When I arrived in Princeton I found that my enthusiasm for the potentialities of the theory of games was not shared by the members of the economics department. Even the Princeton University Press, which as an academic publisher was meant to take reasonable risks with new scholarly enterprises, had required an outside subsidy of $4,000 before it would risk publication.[1]

There was Professor Morgenstern and his project with a few students, and there was the rest of the department. Although Morgenstern did give a graduate seminar in game theory, if one wished more than a single seminar it was necessary to become involved with the mathematicians

1. In defense of the press I must admit that the manuscript was extremely large.

and the activity at Fine Hall where a major research seminar was given and where there was a large group interested in game theory and its development.

The seminar given by Morgenstern that I attended had four students. They were Goran Nyblen, a brilliant young Swedish economist who eventually became paranoid and hung himself, but who did complete an interesting book on game theory and macroeconomics (Nyblen 1951). The second, William Young, went into industry immediately after graduate school, and unfortunately was dangerously alcoholic and died at an early age. The third, Djhangir Boushehri, decided to leave the graduate program before completing his degree and went on to a distinguished career as an applied economist (using more gamesmanship than game theory) at the International Monetary Fund. I was the fourth.

At Morgenstern's project were Maurice Peston, Tom Whitin, and Edward Zabel who were somewhat interested in game theory and operations research. Beyond that, game theory apparently had little impact on the economics department.

William Baumol raised questions about the value of the measurable utility assumption used in much game theory work at that time; outside of Princeton Karl Kaysen had questioned the worth of game theory in economics. The view was that in spite of the favorable reviews of Leonid Hurwicz and others this new mathematical bag of tricks was of little relevance to economics.

This view was put forward in particular by Jacob Viner whose favorite comment on the subject was that if game theory could not even solve the game of chess, how could it be of use in the study of economic life, which is considerably more complex than chess.

The graduate students and faculty in the mathematics department interested in game theory were both blissfully unaware of the attitude in the economics department, and even if they had known of it, they would not have cared. They were far too busy developing the subject and considering an avalanche of new and interesting problems.

Von Neumann was at the Institute for Advanced Study and Morgenstern was in the economics department. Then there was Albert Tucker in Mathematics who was actively interested. A host of junior faculty, visitors, and graduate students who in one form or the other were involved in some aspects of game theory, included Richard Bellman, Hugh Everett (recursive games), David Gale, John Isbell (absolute games), Sam Karlin, John Kemeny, Harold Kuhn, John Mayberry, John McCarthy, Harlan Mills, William Mills (four-person solution theory), Marvin Minsky, John Nash, Lloyd Shapley, Norman Shapiro, Laurie Snell, Gerald Thompson, and David Yarmish. Somewhat younger and arriving somewhat later were Ralph Gomory, Herbert Scarf, and William Lucas. John Milnor was an undergraduate and then a graduate student. Robert Aumann and many others came still later, after many of the earlier group had left.

The contrast of attitudes between the economics department and the mathematics department was stamped on my mind soon after arriving at Princeton. The former projected an atmosphere of dull business-as-usual conservatism of a middle league conventional Ph.D. factory; there were some stars but no sense of excitement or challenge. The latter was electric with ideas and the sheer joy of the hunt. Psychologically they dwelt on different planets. If a stray ten-year-old with bare feet, no tie, torn blue jeans, and an interesting theorem had walked into Fine Hall at tea time, someone would have listened. When von Neumann gave his seminar on his growth model, with a few exceptions, the serried ranks of Princeton Economics could scarce forbear to yawn.

I was hardly in a position to judge broadly at the time, but in retrospect, although some of us were primarily interested in game theory, the mathematics department as a whole had no special concern in the development of game theory *per se*. It was, to some extent, lumped with the budding developments in linear programming. Furthermore, topology, number theory, probability, differential equations, and many other domains of mathematics were actively being developed at Princeton at that time. The general attitude around Fine Hall was that no one really cared who you were or what part of mathematics you worked on as long as you could find some senior member of the faculty and make a case to him that it was interesting and that you did it well.

Although I did not appreciate it at the time, the book of von Neumann and Morgenstern could be regarded as four important separate pieces of work. They were (1) the theory of measurable utility; (2) the language and description of decision-making encompassing the extensive form and game tree with information sets, and then the reduction of the game tree to the strategic form of the game; (3) the theory of the two-person zero-sum game;[2] (4) the coalitional (or characteristic function) form of a game and the stable-set solution. Furthermore, with great care

2. More generally, constant-sum game.

von Neumann and Morgenstern had spelled out their attitude toward the relationship between game theory and economics and between dynamics and statics and the nature of what should constitute a solution.

Concerning economics they specified, "It will then become apparent that there is not only nothing artificial in establishing this relationship but on the contrary this theory of games of strategy is the proper instrument with which to develop a theory of economic behavior" (von Neumann and Morgenstern 1947, 2). Concerning the relationship between dynamics and statics, they observed,

> The next subject to be mentioned concerns the static or dynamic nature of the theory. We repeat most emphatically that our theory is thoroughly static. A dynamic theory would unquestionably be more complete and therefore preferable. But there is ample evidence from other branches of science that it is futile to try to build one as long as the static side is not thoroughly understood. On the other hand, the reader may object to some definitely dynamic arguments which were made during the course of our discussions. This applies particularly to all considerations concerning the interplay of various imputations under the influence of "domination." . . . We think that this is perfectly legitimate. A static theory deals with equilibria. The essential characteristic of an equilibrium is that it has no tendency to change, i.e. that it is not conducive to dynamic developments. An analysis of this feature is, of course, inconceivable without the use of certain rudimentary dynamic concepts. The important point is that they are rudimentary. In other words: For the real dynamics which investigates precise motions usually faraway from equilibria, a much deeper knowledge of these dynamic phenomena is required.
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> A dynamic theory—when one is found—will probably describe the changes in terms of simpler concepts: of a single imputation—valid at the moment under consideration—or something similar. This indicates that the formal structure of this part of the theory—the relationship between statics and dynamics—may be generically different from that of classical physical theories. . . . 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. 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 1947, 44–45)

It is my belief that von Neumann was even more committed than Morgenstern to the idea of a solution as a set of imputations. He felt that it was premature to consider solutions which picked out a single point and he did not like noncooperative equilibrium solutions.[3] In a personal conversation with von Neumann (on the train from New York to Princeton in 1952),[4] I recall suggesting that I thought that Nash's noncooperative equilibrium solution theory might be of considerable value in applications to economics. He indicated that he did not particularly like the Nash solution and that a cooperative theory made more social sense. Professor Albert Tucker, in a personal conversation, informed me that in his conversations with von Neumann, von Neumann had displayed somewhat the same attitude to the single point solution, the value, proposed by Lloyd Shapley.

The von Neumann–Morgenstern stable-set solution is a sophisticated and sociologically oriented concept of stability. The authors (1947, 42) noted that they did not have a general proof of the existence problem and that if existence failed this would certainly call for a fundamental change in the theory. The attitude around Fine Hall was that if von Neumann conjectured that a stable-set solution always exists, the betting odds were that it did. Shapley and D. B. Gillies were looking for proofs or counterexamples, but although in the course of the next few years they were able to produce pathological stable sets (such as a solution in which you could append your signature as part of the stable set) an actual counterexample was not constructed until much later. William Lucas had worked on the von Neumann conjecture since Princeton and finally published his counterexample in 1968 (Lucas 1968).

Nash, Shapley, and I roomed close to each other at the Graduate College at Princeton and there was considerable interaction between us. In particular we all believed that a problem of importance was the characterization of the concept of threat in a two-person game and the incorporation of the use of threat in determining the influence of the employment of threat in a bargaining situation. We all worked on this problem, but Nash managed to formulate a model of the two-person bargain utilizing threat moves to start with. This was published in *Econometrica* (Nash 1953).

Prior to this work Nash had already done his important work on equilibrium points in n-person games in strategic form (Nash 1950). As I had

3. Even though the noncooperative equilibrium is frequently not unique.

4. I cannot verify the specific date.

read Cournot's work, I recognized that this was a great generalization of a concept that already existed in economics, the Cournot equilibrium point. Somewhat later, after Nash had completed the cooperative game model, he and I and John Mayberry collaborated in applying both the noncooperative and the cooperative models to duopoly with quantity strategies, such as Cournot models (Mayberry, Nash, and Shubik 1953). Later, with help from Shapley I extended the analysis to the Bertrand-Edgeworth models (Shubik 1955) and decided to do my thesis primarily utilizing the noncooperative solution applied to oligopoly problems.

There was considerable work going on on zero-sum game theory. My firsthand knowledge on this is less detailed and less accurate than on the other work because I felt that zero-sum games were not that interesting in application to economics.

Kuhn had studied two-handed poker and Nash and Shapley had considered three-handed poker. An expository article written by John MacDonald and John Tukey appeared in *Fortune Magazine* pointing out (among other things) that the concept of a bluff in poker was by no means merely psychological. They noted that even if one assumed totally passionless, bloodless individuals, they would bluff some percentage of the time in playing an optimal mixed strategy.

My own interests were directed primarily toward non-zero-sum games and both cooperative[5] and noncooperative theories. I will now move on to the cooperative theories.

The properties of stable sets including their intersection were originally considered by Gillies (1953) in his thesis. I believe that Shapley named the set of undominated imputations, the core of an n-person game. I was under the impression until I talked to Shapley that it was he who suggested considering it as a solution concept by itself. He pointed out to me that the idea of the core as a solution concept in its own right came up in our conversations when (as I was the only one in the group of us who was meant to know some economics), I observed that, in essence, the idea of the set of undominated imputations was already in

5. At Princeton I tried in vain to consider cooperative models that illustrated the role of money in an economy, feeling that it might have something to do with side payments and transferable utility. I made essentially no progress until about twenty years later when I worked on strategic market games rather than market games, the basic difference being that the former are process-oriented and the nature of money and financial instruments is part of the control system in an economic process. This is difficult to capture in the totally static and equilibrium-oriented analysis of cooperative games.

Edgeworth (1881) in his treatment of the contract curve, along with the idea of the replication of all players in order to study convergence.[6] In our conversations we originally were talking about stable-set solutions, but for the two-person Edgeworth bilateral monopoly the stable set and the core are identical. When we looked at the four-person game (two on each side) the distinction between the core and stable set eventually became clear. Sometime between 1952 and 1959 as we began to better understand what we were saying to each other and how the game theory compared with the work of Edgeworth, we understood the core as a separate solution concept.

As I was (and still am) mathematically weak, even though I recognized that the treatment in Edgeworth was of a game without transferable utility (currently referred to as NTU), as it was much easier to consider the game in side payment or transferable utility (TU) form, I first formulated it in that manner and eventually (Shubik 1959) was able to publish a simple proof of the convergence of the core of what I called "the Edgeworth game" to the competitive price.[7] Some years later, on a walk from Columbia University to downtown New York after Herbert Scarf had given a paper on an economy with a single dynamically unstable equilibrium point, I suggested to him that the core could be regarded as a combinatoric test for stability and I conjectured that the convergence of the core was probably true for NTU games. Scarf obtained a proof which he presented at a game theory conference in Princeton. It was published in the proceedings (which are difficult to locate). Somewhat later Debreu and Scarf (1963) obtained a somewhat more general proof of the existence and convergence of the core which was published in a readily available journal.

A fairly standard criticism of any attempt to interest the community of economists in cooperative game theory was that the representation of a game by a characteristic function entailed the implicit or explicit assumption of the existence of a magic substance or "utility pill" with a constant marginal utility to all traders. This assumption is called the TU assumption. The prevailing attitude of economists in the 1950s appeared to be that this assumption was so damaging as to make the application of cooperative game theory virtually useless.

6. The idea of replication is also in Cournot's treatment of duopoly being replaced by more competitors (1897).

7. I was helped by discussions with both Lloyd Shapley and, later, Howard Raiffa.

Von Neumann and Morgenstern had made this assumption not because it was a logical necessity but because it yielded a great simplification in the representation of an n-person game and enabled considerable calculation to be done which would have been far too complex in an NTU formulation. Shapley and Shubik (1953) pointed out that the assumption of TU was not only not needed, but that one could well define cooperative solutions to games where the preferences of individuals were represented only ordinally. A full development of this possibility did not take place until considerably later.

Shapley was concerned with developing a one-point solution for n-person games in coalitional form. He developed a set of simple but persuasive axioms which led to the selection of a value or a priori worth for each player. An interpretation of the Shapley value which eventually led me to suggest its application to the allocation of joint costs (Shubik 1952) was as a sociologically neutral expected combinatoric averaging over the marginal worth of an individual in all possible employments.

An immediate application of the value solution was to problems in voting, and Shapley and I (1954) collaborated in utilizing the value applied to a voting game to provide an index to measure the a priori voting power of an individual. At the time I had a few friends in the political science department who seemed to me to be more receptive to new ideas than members of the economics department.[8] William Ebenstein and Richard Snyder[9] encouraged us to consider sending this nonconventional approach to the *American Political Science Review* and much to my surprise it was accepted within a few months.

The Shapley value has been one of the most fruitful solution concepts in game theory. It generalizes the concept of marginal value and it, together with the Nash work on bargaining and the Harsanyi value, has done much in the last thirty years to illuminate the problems of power and fair division dealing with side payments and no side payments, fixed threats and variable threats, two individuals and many individuals.

Another informal activity at Fine Hall, although not immediately concerned with the mathematics of game theory, was of relevance. This was the many sessions (often at tea time) devoted to playing games (such as

8. I had the amusing experience of receiving from Friedrich Lutz a failing grade for a term paper in economic theory at the same time it was accepted for publication in *Econometrica* (Shubik 1952).

9. Richard Snyder invited me to edit a small book entitled *Readings in Game Theory and Political Behavior*. This, I believe, was the first booklet published explicitly on the theory of games as applied to political science.

go, chess, and kriegspiel) and to talking informally about paradoxical or pathological properties of games and the possibility of inventing games that illustrated these properties. Hausner, McCarthy, Nash, Shapley, and I (1964) invented an elementary game called "so long, sucker" where it is necessary to form coalitions to win, but this alone is not sufficient. One also has to double-cross one's partner at some point. In playing this game we found that it was fraught with psychological tensions. On one occasion Nash double-crossed McCarthy who was furious to the point that he used his few remaining moves to punish Nash. Nash objected and argued with McCarthy that he had no reason to be annoyed because a little easy calculation would have indicated to McCarthy that it would be in Nash's self-interest to double-cross him. We dubbed McCarthy's action as "McCarthy's revenge rule." If you are prevented from winning by a double-crosser, try to take the double-crosser with you.[10] "So long, sucker" still has not been fully analyzed, and the relationship between revenge and rational behavior still remains to be explored.

Some years later I formalized the rules for the dollar auction game (Shubik 1971) and considered its noncooperative and cooperative game solutions. The ideas for this illustration of a game with escalation or addiction in all likelihood may have had its origins (like the folk theorem concerning noncooperative equilibrium points in infinite horizon repeated games) in the informal sessions devoted to dreaming up paradoxical playable games.

To the best of my knowledge none of us at that time had formally considered experimental gaming in economics or political science, but the idea of experimentation was beginning to filter in from Mosteller and Nogee (1951) and later the book of Thrall, Coombs, and Davis (1954).

Unknown to me at the time was the breadth of the activity going on in linear programming. By 1947 von Neumann had conjectured the relationship between the linear programming problem and its dual and the solution of zero-sum two-person games. Gale, Kuhn, and Tucker started to investigate this more formally by 1948 and published their results in 1951. Kuhn and Tucker were also active in the development of nonlinear programming. The seminar at Fine Hall lumped the newly developing mathematics of game theory and programming together.

Although there was a beautiful link between the mathematics for the

10. Game theory still does not have an adequate formalization of revenge, resolve, bravery, morale, or any of the many other features that distinguish actual conflict from this type of abstraction.

solution of two-person zero-sum games, to a certain extent this link may have hindered rather than helped the spread of game theory understanding as a whole. For many years operations research texts had a perfunctory chapter on game theory observing the link to linear programming and treating linear programming and game theory as though they were one. The economics texts had nothing or next to nothing on the topic.

The extensive form of the game was of concern to several individuals at that time. Kuhn (1950) and Thompson (1953) were concerned with the representation of information and with the concept of strategy. I probably did not appreciate it sufficiently at the time, but the development of the notation for the extensive form had a considerable impact on decision theory and psychology. The ability to represent and analyze different information structures was a breakthrough of the first magnitude.

The role of von Neumann as a mathematician in the development of the theory of games is clear. The role of Morgenstern is less clear, and in my opinion underrated. As a former student of Morgenstern it can be argued that I am not in a position to give an unbiased estimate. Yet I feel that it is important to reconsider the nature of basic contributions. In many instances individuals discover new things and do not know the significance of what they have discovered. They see, but do not comprehend. They look, but have no vision.

One of the great virtues of Oskar Morgenstern was that he understood the significance of the theory of games. He was not a mathematician and on some occasions may not have even understood some of the work he espoused. But he was clearly aware of many of the big problems in economics and was energetic enough and visionary enough that he tried to do something about them even if he could not solve them himself. Thus, in particular, he recognized "perfect foresight" as a *bête noire,* and much of his concern for the development of the theory of games was to get rid of the paradox of perfect foresight.

There is little doubt that much of his "value added" came not merely from his own work, much of which was provocative and relevant (such as his book on the accuracy of economic measurement), but from his dedication to a talent hunt and to getting individuals to work on problems he thought were important. His influence on Wald, von Neumann, and his many mathematician friends was considerable. He wanted to encourage as much talent as he could to work on the problems he deemed important. In particular Morgenstern felt that it was important to try to guide first-class mathematical talent toward reconsidering the basic models and assumptions of economics, and his collaboration with von Neumann on game theory must be viewed in this light.

Although I have tried to give a view of part of a long and exciting campaign as remembered by one of the then very young campaigners, now forty years on, a few more comments must be made to set matters adequately in context. My time of observation at Princeton was from the fall of 1949 to spring of 1955. Even then the development was not merely at one location. RAND at that time was probably at least as important as Princeton, and many of the individuals named above worked at RAND or consulted there. In particular the work on stochastic games [11] and duels was of note. Although few of us at Princeton appreciated it, there was considerable activity at Michigan at the time as well.

Various individuals who were relevant to the development of the theory of games had already left Princeton before I arrived. There are some individuals at Princeton whose work I may have missed in this somewhat impressionistic and eclectic survey. No slight is intended; my main concern has been to try to indicate that, at least for some of us, this was a period of considerable excitement and challenge. New developments were taking place and somehow they seemed to be important even if we did not quite know why. We were present at the creation not only of game theory, but programming in general; we saw the development of the computer at the institute [12] as well as the development of other branches of mathematics.

When I consider the history of mathematical economics and the treatment of Cournot's great book, I am impressed by the growth in influence of the theory of games—not how little and how slowly, but how much and how fast.

The contrast between the Department of Economics and the Department of Mathematics at Princeton at that time has some lessons to teach. Besides Morgenstern there were some fine scholars in economics such as Viner and Baumol, but there was no challenge or apparent interest in the frontier of the science. Morgenstern was to some extent an inconvenience. To me, the striking thing at that time was not that the mathematics department welcomed game theory with open arms—but that it was open to new ideas and new talent from any source, and it

11. I was aware of Shapley's seminal paper (1953) and it was this that led me to formulate a non-zero-sum version called games of economic survival (Shubik and Thompson 1959).

12. With the help of the good offices of Alan Hoffman I managed to get time on the Johnniac to solve a 17 × 17 matrix game representation of a price duopoly model for my thesis.

could convey to all a sense of challenge and a belief that much new and worthwhile was happening.

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