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The Collaboration Between Oskar Morgenstern and John von Neumann on the Theory of Games

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TIME AND AGAIN since the publication of *The Theory of Games and Economic Behavior* in 1944 the question has been asked how it came about that von Neumann, one of the greatest mathematicians of our age, and I met and worked together on what turned out to be a major piece in both our lives [20, (1944) 1953]. Recently I have been pressed by many to set down the history of the collaboration. And so I shall try to give a brief account of our mutual involvement. A fuller account with precise dates may follow some other time.

My first book, entitled *Wirtschaftsprognose*, was published in 1928 by Springer in Vienna, Austria [10, 1928]. In that book, written in 1926–27 while a Fellow of the Laura Spelman Rockefeller Memorial and an Honorary Research Fellow at Harvard University, the whole question of economic forecasting was examined epistemologically, and the difficulties and virtual impossibilities of prediction were studied to the best of my then-existing knowledge. In my general scientific outlook I was strongly influenced by the work of Hermann Weyl, Bertrand Russell, and others in the mathematical and physical sciences. I also struggled hard with Ludwig Wittgenstein's *Tractatus Logico-Philosophicus* of 1921 [26, (1921) 1955]. At

Harvard, I frequently participated in the private seminars held by the great philosopher-mathematician Alfred North Whitehead, who had just published his *Science and the Modern World* [25, 1925] in which, however, he began to veer more towards metaphysics than was to my then-developing taste.

When I became a Rockefeller Fellow, I was a product of the Austrian School of Economics, having obtained my doctorate in 1925 with a piece on marginal productivity. But I was constantly troubled by the fact that Böhm-Bawerk's theory of bargaining and of the "marginal pairs," while dealing with fundamentals, could not be considered completed. This also led me, while still in Vienna, to Edgeworth's contract curve in his *Mathematical Psychics* [2, 1881]. On my way to the United States in 1925 I visited the aged Edgeworth in Oxford. I expressed great pleasure at the publication of his collected papers, but urged him repeatedly to republish the *Mathematical Psychics*, then totally out of print. His death intervened before he could carry out my suggestion, which he had accepted.¹

¹ Today the contract-curve and the core play a great role as one manner of relating game theory to more conventional economic theory.

In my book² I showed among other things that one is confronted in economics with two kinds of variables, which I called “dead” and “live,” the former being those that do *not* reflect decisions by other economic subjects, the second, those that do. In that connection, even the word “game” occurs. I also showed that the mere increase in the size of an isolated “simple economy” (in the sense of the Austrian School, meaning a household isolated by itself) was a less complicating factor than complications encountered by a simple economy, no matter what size, when involved with others; the latter, and the latter alone, would have to deal also with “live” variables, as I called them, *i.e.*, with decisions made by others. This states exactly one of the basic tenets of game theory where one can maximize only in the first case when the variables of nature are “dead,” but one is confronted with a conceptually different matter in the second case, since the “live” variables represent other “wills,” other “economic acts,” which may interfere with, or enhance, one’s own plans, as I expressed the matter then.

One of the problems that naturally sprang to my attention was that of the influence of predictions on the predicted events, a typical case in the social sciences. I examined this area from many points of view. I analyzed several cases of predictions: the first a single prediction becoming known to, and being believed by, everyone, with their reactions influencing the predicted events; then the case of several differing predictions with different distributions of acceptance and consequently different influences upon the behavior of individuals and therefore upon the future events, *etc.* In the course of these studies I produced the example of the pursuit of Sherlock Holmes by Profes-

sor Moriarty [10, 1928, p. 98]. I showed in some detail in particular that the pursuit developing between these two could never be resolved on the basis of one of them out-thinking the other (“I think he thinks that I think! ! . . .”), but that a resolution could only be achieved by an “arbitrary decision,” and that it was a problem of *strategy*.³

The problems touched in that book never left me, in spite of my involvement in business cycle theory and statistics. In 1935 I published a paper in the *Zeitschrift für Nationalökonomie* “Vollkommene Voraussicht und Wirtschaftliches Gleichgewicht” in which the same illustration of Sherlock Holmes and Moriarty was used once more, but the whole matter of prediction and foresight was put into a wider frame [12, 1935]. That paper, incidentally, intrigued Frank H. Knight so much that he himself translated it and used it in his lectures.⁴ I showed that the assumption of perfect foresight leads to paradoxes and is inadmissible for general equilibrium theory, which was thus found critically wanting. After this paper was published, I was invited by Professor Moritz Schlick, the famous philosopher and leader of the so-called “Vienna Circle,” to give a discussion of the problems treated in that paper. This I did in a rather lengthy session, and the matter was discussed by many of those present in great detail. To the Vienna Circle belonged, in one way or another, Carnap, Feigl, Frank, Gödel, Hahn, Menger, Popper, Waismann, *etc.* Not all of them were present at this occasion. I went frequently to these meetings as well as to Karl Menger’s Colloquium, though I was not a formal member of either group.

I repeated this talk, at Menger’s request, in his Colloquium and after the meeting broke up, a mathematician named Eduard Čech came up to me and said that the questions I had raised were identical with

² Extensive summaries in English are contained in the two review articles by Arthur W. Marget [5, 1929] and Eveline M. Burns [1, 1929].

³ It is analyzed as such in our book [20, 1953].

⁴ This translation is published in [22, 1976].

those dealt with by John von Neumann in a paper on the Theory of Games published in 1928 [18], the same year that I had published my book on economic forecasting [10, 1928]. Čech, then already a promising mathematician, outlined to me its principal ideas and results and was very eager that I should study this particular work. I intended to do so, but the great burden of work I was carrying at that time as director of the Institute of Business Cycle Research during civil war conditions, with the Nazis threatening, with frequent trips to the League of Nations in Geneva, to Paris and London, *etc.*, made this impossible. Nevertheless, even during those years in the 1930's in Vienna, I managed to read a lot of logic and set theory, *e.g.*, Hilbert-Ackermann, Fraenkel, Hilbert-Bernays, Hahn, Hausdorff, *etc.* I also attempted to approach Kurt Gödel's great work on undecidability, helped and guided by my friend Karl Menger. At the same time Abraham Wald, to whom I had been able to offer a position as statistician in my Institute, gave me special instruction in various fields of mathematics. In those years fall not only the exciting work of Wald on the Walrasian equations but also the publication of Menger's great papers on the theory of returns [9, 1936] and the St. Petersburg Paradox [7, (1934) 1967]. Furthermore, in 1934 Menger's book on the logic of ethics was published [8, (1934) 1974], which I took as the appropriate occasion for a discussion of the role of modern logic for the social sciences [13, 1936].

A little later Karl Menger urged me to attend a meeting of his Colloquium in which John von Neumann, on one of his trips from America to Europe, would speak and present a theory of the expanding economy, which he did in 1937. Unfortunately, precisely during those days when he was in Vienna, I had to be in Geneva to attend the League of Nations committee meetings and so we did not meet.

In January 1938, I left for the United States, invited by the Carnegie Endowment for International Peace as a Visiting Professor at four American universities. I had hopes that I might have a chance to go to Princeton to meet von Neumann, then Professor at the Institute for Advanced Study. I did go to Princeton, but saw only the economists Frank Fetter and Frank Graham and did not encounter von Neumann. The Nazis took over in Vienna in March 1938. I was dismissed as "politically unbearable" from the University as well as from my Institute, which I had left in the hands of my deputy who emerged as a Nazi. The Institute was taken over by him and Wagemann, then the head of the Berlin Institute for Business Cycle Research. While in the United States, I received a number of calls from various American universities to join their faculties. I accepted the invitation from Princeton University for a three-year appointment as Class of 1913 Lecturer in Political Economy. Half my salary for these first three years was paid by the Rockefeller Foundation, which had for a long time supported my Institute in Vienna. The principal reason for my wanting to go to Princeton was the possibility that I might become acquainted with von Neumann and the hope that this would be a great stimulus for my future work.

Von Neumann and I met soon after the University opened. It is curious that years later neither of us could ever remember where we met the first time, but we did remember where we met the second time: I gave an after-luncheon talk on the 1st of February 1939 on business cycles at the Nassau Club, and he was there with Niels Bohr, Oswald Veblen, and others. Both he and Bohr invited me that afternoon for tea at Fine Hall, and we sat several hours talking about games and experiments. This was the first time that we had a talk on games, and the occasion was heightened by Bohr's presence. The dis-

turbance of experiments by the observer was, of course, one of the famous problems raised by Niels Bohr for quantum mechanics. These talks were taken up again with both at Weyl's house—he had also entered my life. This circle still widened when I encountered Einstein for the first time at dinner with Bohr at von Neumann's house, and I recall vividly Einstein's discussion of the priority of theory over experiment, the preeminence of conceptualization, and the deep puzzle of intuition. In many later meetings he would often return to these and related issues.

Von Neumann and I had many other very animated and wide-ranging discussions. There was an instantaneous meeting of minds and a spontaneous empathy between us. I mentioned to him that I was greatly interested in studying both his papers, the one on game theory and the one he had delivered in Vienna on the expanding economy. We quickly exchanged reprints, I giving him in particular my work on perfect foresight. Von Neumann told me that he had done no work on game theory since 1928 or on the expanding economy model. He may have thought one way or the other about it, but never in any systematic way, nor had he put down anything on paper.

I now began to study his paper on game theory seriously. This was no easy matter because parts of the mathematics were new to me, in particular the entire topic of the fixed point theorem. The paper on the expanding economy was also difficult for the same reason. This quickly led to many conversations with Johnny, as I shall from now on call him. I remember vividly my great intellectual excitement, in fact also the emotional involvement with the theory of games, which he had developed in 1928. I saw what was meant and what tremendous possibilities there existed.

So I decided that I would write a paper showing economists the essence and significance of game theory as it then existed,

and I set out to produce such a paper. Many further talks with Johnny took place. Our contacts were frequent and, for the rest of our lives, always of the most friendly and intellectually exciting nature. When my paper was well under way, he offered to read the manuscript. He did so and then remarked that it was too short and therefore would not be intelligible to those who had not studied the theory to the extent that I had done. Besides, he and I had already begun to discuss many further possibilities and developments of the theory. So I expanded the paper. When he saw that already greatly enlarged new version, on a memorable day in my bachelor quarters at 12 Nassau Street, which was then the Princeton Bank and Trust Building, he suggested: "Why don't we write this paper together?" I was quite overcome by this suggestion. Our many meetings had already opened up an entire new world toward which I had reached out for years, and here was Johnny wanting to work with me, both of us pushing into a vast new field, never doubting its challenge, difficulty, and promise. Years later I read what Hilbert said when, while in Göttingen, he encountered Minkovsky, who became his closest friend: "Er war mir ein Geschenk des Himmels!" Here was my gift from Heaven.

This was in the fall of 1940. As our paper progressed, we thought—always sitting together and writing jointly—that perhaps the paper might have to be still a little longer. Johnny said it might have to be published in two parts, one piece being too long for a scientific journal. I said that wouldn't matter to me at all; on the contrary, we should be as detailed as the subject matter required. So we began to work and as our work progressed, Johnny said: "You know, this will hardly do as a paper, not even in two parts. Perhaps we should make a small pamphlet out of it, and it could be published in the *Annals of Mathematical Studies*, which Marston Morse is

editing.” He, Johnny, could probably persuade Marston to accept it, and it might run up to perhaps 100 pages.

As we continued to work, Johnny said: “Why don’t we go to the Princeton University Press and ask them whether they would be interested in such a pamphlet?” Datus Smith, then director of The Press, was very accommodating and an agreement was quickly made on the basis of which we committed ourselves to hand over an appropriate manuscript in due course, but I do not recall whether any specific date was set for this. At any rate, a pamphlet of about 100 pages was envisaged. After we had signed this statement, we felt quite relieved and cheered up in a funny sense. Now we really started to write. We completely forgot about any restriction to 100 pages and worked, thought, and discussed endlessly.

Often we went on long walks during which we talked over various games and the whole developing theory. We drove occasionally to the seashore and walked up and down the boardwalk at Sea Girt, in particular, discussing matters. At Christmas in 1940 we were in New Orleans where I had to give a paper on unemployment at a session of the American Economic Association. Later I was vacationing in Biloxi with Johnny and his wife Klari. Again, day after day was spent in discussing the theory. Among the things that came up very early was that we were in need of a number for the pay-off matrices. We had the choice of merely putting in a number, calling it money, and making money equal for both participants and unrestrictedly transferable. I was not very happy about this, knowing the importance of the utility concept, and I insisted that we do more. At first, we were intending merely to postulate a numerical utility, but then I said that, as I knew my fellow economists, they would find this impossible to accept and old-fashioned, in view of the predominance of indifference

curve analysis, which neither of us liked.

So later we decided we would settle on thinking about a numerical utility. It did not take us long to construct the axioms on which the present theory is based that gave us a firm utility concept, that of an expected utility, numerical up to a linear transformation. We did not publish the proof of the existence of this number on the basis of our axioms in the first edition of our book, which appeared in 1944 (though we had that proof, of course). In Vienna I had given courses on risk, expectations, and the time element in value theory and also had published on some of these questions. Regarding risk, Karl Menger’s important paper of 1934 on the St. Petersburg Paradox [7, (1934) 1967] played a great role. Johnny also read my paper on the time element in value theory [11, 1934] and urged me to expand it into a book (which I never did). He said that the problems discussed there were both very important and would offer great mathematical difficulties. But the construction of axioms for our expected utility came quite naturally. I recall vividly how Johnny rose from our table when we had set down the axioms and called out in astonishment: “Ja hat denn das niemand gesehen?” (“But didn’t anyone see that?”) Incidentally, we always spoke German even while writing in English, which later caused a knowledgeable reader ironically to remark that the whole book is written “. . . in such nice professorial German.”

It was largely my doing that this utility theory was developed, and I am very content with what happened then and also later, although I still feel that the ultimate theory of utility is much more complicated than what we did. We were, of course, aware of the difficulty with the logical foundations of probability theory. We decided we would base our arguments on the classical frequency approach to probability, but we included a footnote saying that one could axiomatize utility and prob-

ability together and introduce a subjective notion of probability. This was done later by others.

Johnny's paper on the expanding economy [19, 1937], the one I had been unable to hear him deliver in Vienna, was another area of thought which occupied me. I now also studied this paper very carefully and was immediately convinced of its extraordinary importance. I persuaded Johnny to give a talk on it to the general economics seminar of the Department, which he did. This took place in the old Pyne Library where at that time we had a seminar room. The talk was fairly well attended, but I was dismayed at the total lack of response it found. There were then hardly any mathematical economists at Princeton, let alone persons who could have been receptive to the fundamentally new ideas he propounded.

Even while we were beginning to work on game theory, I also wrote my paper, "Professor Hicks on Value and Capital" published in *The Journal of Political Economy* [14, 1941; 22, 1976]. In that paper I referred to Johnny's model of the expanding economy, and I emphasized the fact that in economics one is confronted essentially with inequalities and not with equations. It is, as far as I know, the first mention of his paper anywhere, certainly in an economic periodical or book. Incidentally, in my paper there is also a strong reference to the fundamental work done by Abraham Wald (also in the 1930's at Menger's Colloquium) on the Walrasian system, a work again not taken up by Hicks. Johnny read my paper on Hicks carefully and was in full agreement with what I said. He even made one or two annotations to the manuscript. This paper on Hicks has been completely bypassed in the literature, perhaps due to the wartime conditions, perhaps due to its irreverent nature. Von Neumann's view of books on mathematical economics written up to that time (of course, excepting Wald and

Menger) and even somewhat later was: "You know, Oskar, if these books are unearthed sometime a few hundred years hence, people will not believe that they were written in our time. Rather they will think that they are about contemporary with Newton, so primitive is their mathematics. Economics is simply still a million miles away from the state in which an advanced science is, such as physics." He repeated this kind of remark even in later years and also with regard to the description of an economy or society.

The topic of the expanding economy had never left me, and Johnny and I talked many times about it. From the beginning I did not like one very restrictive assumption of his beautiful model, which is not its linearity, but the condition that every good produced must enter in the next phase into the production process of every other good produced, no matter in how small a quantity. This would only be true for highly aggregated conditions. In 1956 when he was already very sick with cancer, fortunately I could tell Johnny that J. G. Kemeny, Gerald L. Thompson, and I had succeeded in removing it, generalizing his model substantially, which pleased him very much. This was to become known as the "KMT model," which also established firmly the relationship of the model to game theory. In both, the fundamental minimax theorem is of the essence; KMT showed why this had to be the case and—unexpectedly—that game theory can also be used as a mathematical technique, as a calculus, in addition to being a true model. Many further generalizations were possible beyond the KMT model, an indication of the power of Johnny's original ideas. I also told Johnny that Thompson and I were planning more work along these lines. A series of papers containing further extensions and generalizations led to a book with Thompson, which I found gratifying and which is a major piece in our lives: *Mathematical*

Theory of Expanding and Contracting Economies [17, 1976].

We worked intensively through 1941–42. Johnny, upon my urging, had given some lectures at Princeton University on game theory, mostly dealing with two-person theory, although some of our new results on n -person game theory also began to appear in his lectures. These were not terribly well attended, I do not know for what reason but, after all, we were already in the midst of war, not an ideal time for work of our kind! At any rate, the lectures helped focus our thoughts and advanced to some extent our manuscript.

There were endless meetings either at my apartment over the bank or at 26 Westcott Road, where Johnny lived with his wife Klari and his daughter Marina (now Mrs. Marina von Neumann Whitman). We wrote virtually everything together and in the manuscript there are sometimes long passages written by one or the other and also passages in which the handwriting changes two or three times on the same page. We spent most afternoons together, consuming quantities of coffee, and Klari was often rather distressed by our perpetual collaboration and incessant conversations. She was at that time collecting elephants made of ivory, glass, and all sorts of other material. At one point she teased us by saying that she would have nothing more to do with the ominous book, which grew larger and larger and consumed more and more of our time if it didn't also have an elephant in it. So we promised we would happily put an elephant in the book: anyone who opens the pages can find a diagram showing an elephant if he knows that he should look for one.

At this point I note a curious incident that shows how chance can influence the direction of scientific work. At the time when we were about to write down a new proof for Johnny's famous minimax theorem, originally developed in 1928, I went

out for a walk on a brilliant, snowy cold winter day. I went towards the Institute for Advanced Study and since I was cold, I walked into the library, looking around idly. I picked up E. Borel's *Traité du Calcul des Probabilités*, and there I saw in it suddenly a paper by Jean Ville [23, 1938] dealing with Johnny's 1928 paper. There, in restating Johnny's minimax theory, instead of using Brouwer's fixed point theorem, he gave a more elementary proof (Johnny's two earlier proofs were definitely not elementary). I had not known of Ville's work; so I phoned Johnny to whom this also was news. We met immediately and quickly saw that the best approach was to proceed by considerations of convexity. Thus was developed the "theorem of the alternative for matrices," built upon the theorem of the supporting hyperplane. These were ideas that had occurred nowhere else in any piece on mathematical economics. From this date on (*i.e.*, from 1944 on) stems the introduction of methods of convex bodies into the modern literature of economics, in particular via linear programming (an offspring of game theory).⁵

It is curious to think how different much of our mathematical treatment of game theory would have been, had it not been for this winter day walk, spotting Borel's book, opening it, and expecting to find nothing there about games of strategy. Of course, eventually convexity considerations would have appeared somewhere in the literature, probably with a delay of years. Many writings in other fields would have been retarded. We were, of course, happy that we could proceed in a more elementary manner, although we had to explain even those underlying mathematics in quite some detail. We were both firmly convinced that one should always try to proceed in as elementary a manner

⁵ The notion of convexity is used in a fundamental manner in von Neumann's 1932 model of the expanding economy [19, 1937; 17, 1976].

as possible and not display advanced mathematics when not needed. (Fixed point theorems reappeared, however, in a fair number of later writings by mathematical economists, sometimes unnecessarily complicating their arguments.)

In 1942 Johnny moved to Washington, but our manuscript was already very far advanced. The war had broken out, and he went to a research office of the Navy. At the time I was teaching something like twelve or fourteen hours a week. I had not received any “teaching credits” while working on the manuscript. At that time, it was not customary, and I had not asked for it. Neither did we get any financial support of any sort while we were working. While Johnny was in Washington, I frequently went there, sometimes staying at his house, and on weekends we worked furiously in order to finish and to make sure that this big work would really be done before a trip to England. When he left for a brief trip, he was equipped, to Klari’s and my great amusement, with a heavy fur coat, a steel helmet, and a volume of the Cambridge Ancient History under his arm. (In spite of our heavy work load, we found time to read and exchange these volumes as well as to study Thucydides, books we discussed extensively when not working on “the next chapter.”) Occasionally he came to Princeton and our work continued there deep into the night. He came again at Christmas 1942, and at that time we actually managed to write the last few pages. There were things that we had in mind that we wanted to put into the book, in particular more about economic applications, but we omitted these because the manuscript had already grown enormously. We had said most of the things that were really important to us to say, and time was running out. At Christmas, we put the last touches to the manuscript and also wrote the preface dated January 1943, and it was in the very first days of January that we made the work final.

During the whole period of our collaboration, each day after we met—and we wrote by longhand, of course—I would type two copies of what we had written, put in the formulas, and the next day, or whenever the next occasion arose, I would give one set to Johnny and keep the other; on that basis, we always had a somewhat orderly typed manuscript in front of us. This was a rather big chore, but I did it with pleasure and satisfaction. We had no secretarial or financial help. We did all on our own. Since I was a bachelor at that time, I had breakfast across the street at the Nassau Club, and Johnny, who usually got up early while his wife liked to sleep longer, came almost every day to join me at the Club. He was always wide awake in the morning, and even at the breakfast table we would start talking about what was to be done, if possible the same day in the afternoon. These breakfast meetings continued for many years, even after I was married in 1948, though less frequently.

After the manuscript had been completed, it was clear that we would have to go to Princeton University Press to explain that this was a somewhat bigger matter than the 100-page “pamphlet.” The people at The Press were quite overwhelmed seeing a manuscript of about 1200 typed pages full of graphs and uninhibited mathematical notations. They were generous and said they would try very hard to publish the book (during World War II!), but could some subsidy be obtained? First there was the need to produce a clean manuscript; everything had to be retyped and all formulas had to be put into the new copy. We finally managed to get the enormous grant of \$500 each from Princeton University and the Institute of Advanced Study for retyping. This was done and then a Japanese “enemy alien,” a young mathematician, put in all the formulas from the original manuscript. Johnny remarked in his usual manner that it is the fate of enemy aliens who are mathemati-

cians to be punished for being enemies by having to put other people's formulas into manuscripts.

The retyping and readying of the manuscript, with all the diagrams properly drawn—including the set-theoretical elephant!—done by a draftsman at the National Bureau of Economic Research, Mr. Forman, took quite some time. But the manuscript went to the printer in 1943. Then came the year-long process of typesetting, proofreading, *etc.*, which I shall not further describe here.

We had to settle on a title for our book. For a while we were thinking of calling it *General Theory of Rational Behavior*, but dropped this idea and similar ones quickly. They were not descriptive enough of our work, and we reverted to the title originally considered, *Theory of Games and Economic Behavior*, although, as mentioned, we knew perfectly well of the wider applicability of game theory to politics, sociology, *etc.* That settled, Johnny wanted to have our names listed alphabetically. I absolutely refused to entertain this proposal, and after some struggle he gave in.

The Press accepted the manuscript without any question. It never went to any referee, but they wanted some subsidy not only because of an increase in the cost of production, but also because they thought there was considerable risk involved. I went to a friend, a well-known American, who made an anonymous donation to the University, which was turned over to The University Press. It was not a very large amount, but it turned the tide, and The Press undertook publication of the book without further hesitation. It came out on the 18th of September 1944. They certainly lost no money.

What were our expectations regarding the fate of our book? Clearly, we were convinced that it represented first of all a fundamental break with conventional economics: we demonstrated that one is not confronted with ordinary maximum

or minimum problems (no matter what side conditions!), but with conceptually different situations. Though this becomes intuitively quite easily accessible for ordinary exchange, for monopoly, oligopoly, *etc.*, the phenomenon is all-pervading. The theory—which I shall not even begin to describe here—deals in a new manner even with such things as substitution, complementarity, superadditivity of value, exploitation, discrimination, social “stratification,” symmetry in organizations, power and privilege of players, *etc.* Thus the scope of the book extends far beyond economics, reaching into political science and sociology, but economics was of more immediate concern and interest to us. We also knew that there would be much resistance, both because of the basic orientation of the work and because of the unconventional mathematics that extend throughout the book. We took care to explain as much of the latter as feasible, but we knew that the book made great demands on the reader on both counts. Johnny said to me repeatedly that we ought to publish some further joint papers, and we certainly had ideas of what could and should be done. (One on symmetric solutions of general n -person games was distributed as a RAND Research Memorandum [21, 1961].) Otherwise, he said, the book would be “a dead duck.” Even so, he did not expect a rapid acceptance, rather we would have to wait for another generation. This view was shared by some of our friends, especially by Wolfgang Pauli and Hermann Weyl.

Matters turned out in some ways quite differently. In 1945 and 1946 there appeared two very fine expositions of our efforts by Leonid Hurwicz [4, 1945] and Jacob Marschak [6, 1946] as well as a long review by Abraham Wald [24, 1947], who in 1945 had already laid a new theory of the foundations of statistical estimation based on the theory of the zero-sum two-person game.

Then, in March of 1946 a long and gen-

erally intelligently written article about our book appeared on the front page of a Sunday edition of *The New York Times*. This caused a minor sensation and as a consequence the book was sold out quickly, so that a second edition had to be issued, which appeared in 1947. We added a substantial appendix, giving the proof that our system of axioms for a numerical utility, set forth in the first edition, indeed gave the desired result. That theory has now penetrated into most advanced texts of economic theory and will surely in the long run completely replace conventional indifference curve analysis. We also added some further observations on utility, in particular regarding such topics as partial orderings, non-Archimedean orderings, and the question of a specific utility of gambling. In 1953 we published a third edition, enlarged only by a new preface.

As mentioned above we wrote only one of the planned joint papers. We also had other plans. For example, we were both convinced that the then current methods for time-series analysis of economic data were totally inadequate, that the widespread hostility towards Fourier analysis was unjustified, and that better methods based on Fourier series could be developed. But since we wanted to compute on a large scale, we postponed our work time and again until the electronic computer Johnny was then designing would be available. It never came to that: in 1955 Johnny was stricken by cancer, discovered too late, and after much suffering he died in Washington on 8 February 1957. (I did not give up this plan, but worked towards the development and application of spectral analysis; the fruit of this is my book with C. W. J. Granger, *Predictability of Stock Market Prices* [3, 1970], preceded by various papers.)

Johnny took an active interest also in some other work of mine, such as my investigations of the errors in economic statistics and the general problem of de-

scription. My paper "Demand Theory Reconsidered" [15, 1948], pleased him particularly, and he observed that it offered great mathematical difficulties and, as on other occasions, that it would take considerable time for my results to be accepted—as was, indeed, the case.

During the last years, Johnny worked not only on the computer design but was deeply concerned with a theory of automata. We discussed those problems intensively. On many walks through the streets of Princeton, often late at night, he would discuss in great detail especially the possibility of designing a self-reproducing automaton. One problem, in particular, came up time and again: what would the "mouth" of an automaton have to look like if it were confronted with parts of its own kind in order to recognize the parts of which it is made and how to put a duplicate of itself together. These discussions as well as those regarding the future of large scale computations were, of course, restricted by the fact that there existed at that time only bulky and energy-devouring vacuum tubes and as yet no transistors.

By now, our book on game theory has been translated into German, Japanese, and Russian, and translations into Spanish and Italian are in progress. Several international conferences have been held in various countries including the Soviet Union and an *International Journal of Game Theory* has been started, first appearing in 1971. A bibliography on game theory prepared in Vienna lists up to 1970 over 6200 publications, among them dozens of books in many languages.

It is clear that the account given above refers mainly to outward events connected with the writing of this book, and not all of them are here recorded. A great deal would have to be said about what actually went on intellectually between the two of us, but that is another story, which may be told at another time; this would also involve more specific references to

the theory itself. This period was, of course, the time of my most intensive intellectual activity ever. How could it have been otherwise: the closest possible lasting association in work and friendship with one of the truly great mathematicians of this century, a man whose genius communicated itself even to those who had only brief encounters with him.

We did an enormous amount of work in a very short time, but it was unceasing pleasure and never a time of drudgery. There was great excitement on both sides. There was the joy of discovery as we went along, with complete immersion in our task. Looking back, it seems strange that we could have found the time and energy, given our other duties and activities. Nor was it merely a time of work, since we also had intensive social contacts, with each other as well as with common friends. Never did a shadow fall on our relationship during this whole period, or in fact, for the rest of our lives. My brief obituary of Johnny in the *Economic Journal* [16, 1958] was written while very much still under the impression of the recency of his death. But there would be so much more to say.

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