ON THE TRAIL OF A GREAT WHITE WHALE

Martin Shubik

I have written a formal paper, and anyone who wants to know the many details that I have no intention of covering here may find them in the papers that I gave to Kai. As I intend to have my discourse essentially fairly informal, I will burble on (a lovely word from *Alice in Wonderland*), I will burble on until my time runs out. Having said that, I note that having the script for the talk is really just a crutch to use in case the memory fails midway in the talk. Anyhow, here is the tale of the ancient game theorist.

I actually started my game theory at a very, very early age. In 1926, March the 24th, I was born. As any *Alice in Wonderland* buff knows, March 24 happens to be the day Mad Hatter went mad. I was born in the fair city of Manhattan. Now, the reason why I say game theory or at least gaming begins at this point, is because I was, at the age of three months, after living some glorious time in the Roaring Twenties on Riverside Drive, transported to London; and in 1940 I was transported from London to Toronto, Ontario. However, for all of my life, people listen to my accent and they say, "Where are you from?" I happily reply, "Manhattan." Pro forma this seems to be accurate, but it is one of these marvelous examples where pro forma truth conceals reality. I continue my tale. At the age of about eleven, I drove my schoolmasters crazy in an establishment called University College School, which is in Hampstead in England and was associated with the University College. I drove them crazy because I managed to achieve two improbable grades in the end-of-term examination. In my algebra exam I managed to achieve around a hundred, and in my geometry exam I managed to achieve around two. The mathematics teachers of the school descended upon me and informed me that this was impossible. It may or may not have been impossible, but the evidence was there. Sometime later at the University of Toronto, where I actually took mathematics and physics, the same sort of thing happened. I puzzled almost all of the faculty. I was obviously bad at mathematics, but I appeared to be good at something, but they couldn't figure it out and for that matter, neither could I. I still have some of my actual

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grade books, which say something to the effect of "probably an intelligent boy if he could only get organized."

After University College School, I was shipped to a village called Canford Magna, where Canford, a minor English public school, was located. It had the standing of being the prime riding school in England. This was rather quaint and it connected me with old-style gentry. I felt as if I had been a recruit for the Sudan wars because once we signed up in Canford, it turned out that our riding master, who was an ancient retired sergeant from the Horse Guards, treated us as if he had been recruiting for the cavalry rather than training the young masters to ride. I found that voluntary riding was compulsory at my school. We were thrown onto a horse's bare back prior to graduating to saddles. We rode over the moors behind Poole, which was the British submarine base. Among the other pieces of wisdom I picked up at the time when we were galloping across the moors was from our cavalry instructor, who took a look at some tanks on maneuvers on the moors and said, "They will never work. They didn't work in World War I, they need to go back to the cavalry."

Looking back on what seems to be centuries ago, I see that my proper public school was loaded with middle gentry, many of whose families had their sons decimated in World War I. This new crop raised from the survivors who were left over from World War I managed to get themselves massacred in World War II.

In 1940 I left London and went to Toronto. As I was still of high school age, I was shipped to a Quaker school in a place called Newmarket, Ontario, which is now a suburb of Toronto. In those days it was thirty-five miles north of Toronto with essentially subsistence farmers in the intervening distance. There I discovered a truly ambitious mathematics master whose only concern in teaching mathematics was to make sure that he shone in all of the measures of who was the greatest mathematics teacher in the province. I took my mathematics from him, and I was thrown out of geometry because he figured that I wasn't going to do well. Basically that's one of the ways a bureaucrat can sweeten his record. He gets rid of the poorer students first. That infuriated me to the extent that I studied geometry by myself, and I found that I could write the provincial exams without having to be recommended by him. I wrote the provincial exams and got a B+ out of sheer infuriation.

Mysterious academic interests really began at the end of high school. I was seated in the South Ontario countryside and decided to go to the University of Toronto. This was the end of 1942, the war was still on, and I first tried to enlist in a U.S. V-12 program, but never received a reply to my letter. Canada had an officers' training program whereby you could go to the University of Toronto or other universities. I volunteered for the naval reserve. This had the rather bizarre feature that I was at college all through the academic year and then found myself as a stoker second class on a mine sweeper around Halifax for part of the summer.

I won a small scholarship to the university in General Proficiency, where the General Proficiency was biased toward history and English. I looked at the university programs and I said to myself, "I'm vaguely interested in the history and the social sciences." But I did not really want to go into history, and I found the social sciences at that time to be relatively lightweight. One could basically read the books oneself. One did not have to jump through the various academic hoops. I said to myself, "What can I do that would be of any real use in my education while I am at the university? I might as well get a degree in mathematics and physics, even though I am a rotten mathematician." I had already known this, and I found out fairly soon I was an indifferent student of physics, because a good physics student really requires mathematical and laboratory discipline unless you are a pure theorist. Laboratory discipline for someone who's as fundamentally disorganized as I was did not come easily. I decided I'd go into math and physics with the following reasoning. I expected that I could just pass, and I could just pass because I had been told before that I wasn't an unintelligent fellow, just ill-disciplined. I figured that I would learn an appreciation of mathematics and mathematicians, and I did. At the University of Toronto when I was there, there were two absolutely first-class mathematicians: Richard Brauer, an algebraist from Berlin who then went on to Harvard once that was feasible, which was after the war; and a very strange Englishman by the name of Harold Coxeter, who specialized in perfect solids and could tell you everything about the icosahedron you wanted to know. I could see their talent, and going through my mind at the time was that it was a shame those two brilliant individuals had to waste their time on the sort of student I was. There were maybe only four or five students who merited this level of instruction and who could really appreciate it. I could appreciate the art form, but I couldn't produce the art. I slugged my way through mathematics and physics for four years and emerged with something that has been useful for the rest of my life: I could talk to good mathematicians; and with a certain amount of explanation, I could interest good mathematicians in collaborating on proving my conjectures based on my views of the economy.

I finished my degree and also obtained my commission in the Royal Canadian Naval Volunteer Reserve and sailed to such holiday spots as Greenland, Baffin Island, Hudson Bay, Alaska, and the Queen Charlotte Islands. In 1948 I had to consider what to do next. There were three alternatives. One was to go get a job, two was to go to graduate school, or three was to go to law school.

During my career at Toronto I had spent a great deal of time in politics and in debating. I had been taught how to distort facts and deflect questions. I knew how to invent facts, and it was a marvelous contrast with what I was being taught in mathematics and physics. Debating was also serious, but a different art form; in debates class you were formally taught how to lie, how to evade questions, how to shade the truth, and I became reasonably proficient. A next natural step would have been law school, if you were thinking about politics. At that time I had the possibility of getting a nomination in a riding (i.e., an electoral district). I had a shot at being a candidate for the liberal socialists, known as the CCF at the time. But when I actually got into the CCF and into the real politics in Toronto, I had to pay my dues. Your dues are teach "union school," which I did, and go cultivate ringing doorbells and other legwork. I did some of this, but in the union school I learned a couple of things. The union members were not like a wishy-washy pseudosocialist liberal from the university. They hated the guts of almost anybody at the university. They were good honest unionists who represented the obverse side of capitalism. They were looking out for themselves, and their fight was against their particular corporation, but they were most willing to collude with the corporations to let the public pay for both. This was an easy way out. I note one small story. The communists, who had substantial force in parts of Toronto at that time, had decided to try to take over the union, and the one really good political move to my credit was to prevent the communists from taking over the union. The way I prevented them was simplicity itself. Toronto drinking laws were such that the beer halls closed at eight. The communist party was about to take over the union given the simple fact that many of the stalwart members who were not communists would leave at around seven to get their hour of beer drinking in before going home, leaving just a quorum dominated by communists able to take care of the final votes. I managed to talk the administration into putting in a rule that no one could leave the meeting until a vote for adjournment had been taken. And so nobody could leave the hall until the majority voted an early adjournment, and the communists didn't make it in. The next piece in my political life was to consider one of the big platforms of the party. This was the electrification of northern Ontario. I looked at the population of northern Ontario. I looked at the cost of dynamos. I looked at the cost of high-transmission wires, and I concluded it was infinitely cheaper to buy every inhabitant of north Ontario a private generator than it was to start to string high-tension wires for hundreds of miles across semi-barren north Ontario. This did not sit well with my seniors at the CCF, and in spite of the fact that I'd learned how to color the facts quite well in debates, I could not lie at this point and pretend that this was good policy, and I stepped out of active politics.

I decided to go to graduate school. I decided to take a master's degree in political economy at the University of Toronto. There was one serious scholar by the name of Harold Adam Innis; he was a very dour but very serious scholar, and I learned a fair amount with Innis as well as where his limitations were. One of the topics where he was somewhat annoyed at me involved my work on his great thesis about language and communication. He argued that a written language was needed for the existence of empire. I was a graduate student who raised a question about the validity of his language and communication thesis by doing my thesis on the Inca Empire and *quipos* as a method of communication in the Andes. This was of course a numerical communication good for accounting and inventories, but not a written language.

Toward the end of my studies in my economics theory course, Professor Edwards requested that all students pick a new book or a famous old book and write a review of it for class presentation. This was 1948. I went to the library. I picked up *Theory of Games and Economic Behavior,* a 600-page book that taught a sort of mathematics that I'd never seen before. I could hardly read it, but I dived in and by the time I had spent about four or five hours on the book, I was hooked. I said to myself that this is the mathematical way into the social sciences. This is going to formalize the concept of information and it's going to formalize the concept of strategy and a lot of other things, and I want in on this. I finished my M.A. and decided I would try to go for a Ph.D. in game theory. I looked everywhere, and there were only three places I located that I could have a hope of getting a Ph.D. in game theory. They were Princeton, MIT, and Cal Tech. I decided Cal Tech was too far and too expensive. I just couldn't make it out to the West Coast, and so I wrote to MIT and to Princeton saying I wanted to do a Ph.D. in game theory. A couple of weeks later I received two letters, one with a lengthy form from MIT that seemed to say fill in every class you've taken since kindergarten, and the other a letter from Princeton from Oskar Morgenstern saying, "Are you really interested in this and why do you want to come? Write a two-page essay and send it to me." I then ditched the MIT application. I would have ditched the MIT application anyhow because it would have taken five days to fill in or I would have had to lie, and I didn't feel like lying. I received an acceptance and a small scholarship from Princeton, and I started there in the fall of 1949. I arrived to find that in certain areas it was the golden age, and I wound up because of my choice of game theory as a part of that golden age. Via Morgenstern I met von Neumann and got to know many of the physicists. When I lived near Einstein I had the joy of watching him walk to the institute in a ragged old sweater and wearing tennis shoes. On occasion as he walked to the institute, von Neumann would go by in a spanking new Cadillac in a three-piece blue business suit, which was von Neumann's standard dress for going to the institute. My impression was they disliked each other considerably.

Who your colleagues are as graduate students is a very major part of your education. As far as I was concerned I had hit the Klondike. The most depressing feature of the past few years has been the spending of my time writing obituaries of virtually all of my close friends and colleagues. When I arrived at the graduate college (known to the undergraduates as "Goon Hill"), I was stuck in one corner of the back quadrangle. In our suite there were three of us: Lloyd Shapley, John Nash, and myself. In the next room there was Bob Abelson in psychology, and a room or two over there was Marvin Minsky cooking up artificial intelligence. I took a look at the Economics department and there was very little talent there. There was some, in the form of a sharp-tongued gentleman by the name of Jacob Viner who really belonged at the University of Chicago from whence he had come. He was brilliant with brilliance turned essentially to purposes not congenial with my thoughts. Beyond that I soon found out that where I was going to learn the most was in Fine Hall, and so I became sort of an immigrant there. A key element in Fine Hall for educational purposes was tea at four o'clock. Tea at four o'clock was where you could see all the luminaries from the Math department at Fine Hall as well as some from the institute who often came. But one of the main occupations there was teaching each other and playing games; Go and Kriegsspiel (double-blind chess) abounded.

Minsky (later naming the field and working in artificial intelligence), John McCarthy (the creator of LISP), Nash, who was brilliant and was almost psychotic when he was there. He became fully psychotic a few years later. Many years later, in the

’60s, John miraculously emerged out of his psychotic stage, but he lost his genius and did no work since the ’50s. But he came back to humanity, and a woman writer wrote a book on Nash which was called *A Beautiful Mind*. The title was a paraphrasing of a comment by Lloyd Shapley, which was “Such a beautiful mind in such an obnoxious immature twerp.” Many years later I asked Nash what he thought about the book, and Nash said, “It really didn’t do me any harm because it had so little to do with my life.”

I absorbed what I could from the various people who were around me, primarily the mathematicians. I sat in on four courses in Fine Hall. One course was from Professor Solomon Lefschetz, who was an expert in differential equations. One of the items that impressed me about Lefschetz was that he had been an electrical engineer in France, and he had both hands burned off by an accident in a high-transmission line, so he had claws for both hands. On some occasions when anybody was particularly foolish in Lefschetz’s class, he would lift his claws and look at the audience and say, “Do you think I would be teaching you if I still had those hands so I could be an honest engineer?”

I graduated from Princeton in 1953 with my Ph.D. In the meantime Shapley and I had written a little piece in *American Political Science Review* called “A Method for Evaluating the Distribution of Power in a Committee System.” The basic intuition is simply this: consider a vote, what do we think about votes, with one person, one vote? What about a weighted voting system where someone has ten votes, somebody has three, somebody has two, and four others all have one. How do you measure the power there? Immediate observation: if the first individual has ten votes, he has all of the power. This may mean that there is a nonlinear function that apportions the power. My good friend and colleague Lloyd, in a more general context than voting, was able to prove what that function had to be. This is now known as the Shapley value. As soon as Lloyd figured out the Shapley value, I pointed out to him that it could be applied directly to many problems in political science and in committee design. Items like measuring the power of the chairman who controls the agenda can be modeled. We wrote a joint paper aimed at the political scientists. We were two unknown graduate students, and much to my surprise, the *American Political Science Review,* which was the top journal at the time, took it immediately. I don’t know who or how or why I was sponsored, but I suspect that probably projected me into the Center for Advanced Study in the Behavioral Sciences, which was at Stanford. I had a splendid year there. There were two notable psychiatrists, Frieda Fromm-Reichmann and Franz Alexander, and I benefited from discussions with them. There was a great anthropologist, Alfred Kroeber, with many insights into the cultures of the American Indian tribes. Conversation with Kroeber was an absolute pleasure. I was one of the two youngest, and he was one of the two oldest fellows at the center. When Kroeber submitted his plan of action to the center, it contained about forty years of work he intended to do. He didn’t quite finish.

At the end of the year at the center I had to face what to do. I could go back to Princeton as a lowly research associate or I could go elsewhere. I applied for various jobs, and I had a very attractive offer from Northwestern. But then I got an out-of-the-blue telegram from General Electric. I paraphrase what it said: We have decided to form an operations research service, and we are gathering a series of names of top operations research people in the country. Are you interested in joining? I said to myself, there are a couple of possibilities. You are no good academically and you might as well go fight the good fight in the corporate world and end up a nonacademic but fairly well heeled. Or you could go as an assistant professor to Northwestern. I wrote two letters and posted them simultaneously. I posted one letter to Northwestern saying I'll come as an associate professor, and another to GE saying I'll come at a salary that was double the summer adjusted offer from Northwestern. Two days later I received a telegram from GE saying it is a done deal. Two weeks later I got a lengthy letter from Northwestern saying we would love to have you come, but you have to go to a different committee and it will take three months before we can get things going. I decided to risk going to General Electric because the way the General Electric offer was phrased, it was an absolutely glorious, first-class postdoctoral position for anybody who really wanted to study American industry.

I went to GE and found I had first-class tickets and limousine service to virtually every major industrial plant that they had. I spent my time as a consultant visiting Richland to check on the atomic energy generation, Phoenix to check on computers, Santa Barbara to check on consulting services, Cincinnati to check on aircraft engines, and so on. I spent much of my time flying and learning how things were done. What I learned was a fair number of them had the reverse causality that I had been taught in so-called economic theory. Concerning reverse causality, a quick example suffices. I was very curious about how advertising budgets were set; there is a whole literature about how you advertise more and get a larger market share. But the way the advertising budgets were set was on occasion the opposite. If the firm had a good year, they had a lot of cash, and they could afford to waste some of it on loosely accounted-for goodies for all. They doubled the budget of the advertising group, and sales meetings were held in Bermuda, and the causality was not what it seemed to be. I learned many applied features of oligopolistic competition, and at the end of three or four years I was threatened with a promotion in which they said, in effect, put away your childish toys and go run a plant. I was invited to the University Club on Fifth Avenue, and an avuncular arm was put around me by an executive vice president for whom I had considerable respect. He said, "We have been watching you, lad, and you may have the ability to go run a plant." I looked at myself and thought, "Do I want to spend part of my life running a plant in Louisville?" And the answer was no. Also, to be honest, could I manage a thousand people and enjoy doing it? The answer is probably no. At this point, it was up or out at GE, and I had to consider the next move. IBM research labs came through with a refuge. I went to IBM and had the pleasure to meet with Benoit Mandelbrot, who became a very close friend of mine. Benoit was in the group I was in. There were several others of note, such as Richard Karp, who has a reputation in computational complexity.

While I was in IBM research, I started to look for a serious important problem to work on. I finally had an idea and went down to Princeton to see Oskar Morgenstern somewhat before he died, and I said to Oskar, "I have finally picked my problem." He asked what it is. I said, "A decent clean scientific theory of money." Oskar said, "Lots of luck. This has been on the books in economic theory for two hundred years and the odds are against you being able to do it; but you might as well give it a shot."

I then spent around ten years, butchering model after model after model, and I got nowhere until the very end of 1970. I had left IBM and moved to Yale. This is a little story against myself; I note it now and close my story after it. I was finishing some work with my old colleague Lloyd Shapley and went to RAND for a year. I left Yale with my then newly minted bride, and we drove at the end of 1970 across the United States in her dowry, which was her Volkswagen Beetle.

When we got to RAND I worked partially at home and partially in my office at RAND. I was then in my forties and had wasted ten years on trying for a decent theory of money, and I had not made any decent progress. I decided to face up to it. If I had nothing to show by this time, I should cut losses, abandon the problem, and look at my list of other problems. The next problem down called for embedding the Cournot model of oligopolistic competition into a general closed exchange model. One might be able to obtain a characterization of what happens if there are a large number of agents. In economics, a very large number of agents is 10^8, 10^9. In biology and in physics, those are small numbers. Shapley and I had shown that the competitive equilibrium had various other properties than merely decentralization of an economy, and we had been establishing convergence theories showing that there were different ways of looking at the different properties such as fairness, immunity to the power of coalitions, efficiency, and so on and so forth. You could axiomatize each one of these separately and prove that they all converge under certain circumstances to a competitive equilibrium. The one solution concept that was missing was the Nash noncooperative equilibrium. I looked at the Cournot-Nash approach again, and it finally dawned on me that the reason why it was missing was that Shapley and I had been looking at cooperative solutions only where all talk to each other, and they bargain limiting their agreements to optimal contracts. The assumption is that no outcomes waste possible surplus. The Nash noncooperative equilibrium being totally individualistic (like Nash), every individual ignores everybody else as a human being and regards them all as part of some unseen mechanism and optimizes against it; technically it is as though there are n parallel but linked one-person games against a mechanism.

I looked at this problem and considered that we needed to get a convergence process that came from below the efficient surface and went up to the surface. I built a little model which had n individuals, and each individual was a monopolist with one commodity. It turned out that when I tried to set this up in terms of bidding at an equilibrium, I was fine. In disequilibrium I was missing a degree of freedom. And I was baffled trying to figure out what to do. I noted that by adding one extra commodity and giving everybody a large supply of it, I could write a model that was intrinsically symmetric in every individual's state space but was not symmetric in disequilibrium in the whole space. Furthermore, I could use my convergence apparatus of adding more traders of every type. Much to my pleasant surprise, my example converged to the competitive equilibrium. I took it to Lloyd, who was the heavy mathematician in our joint work, and said, "Lloyd, we finally have our mass convergence for the Cournot-Nash solution that has been missing from our work."

It took me three weeks to realize that the solution for the convergence of the noncooperative game was also the solution to an appropriate model to study the theory of money. The n + first commodity was a money, and I did not recognize it because when I switched problems my context was completely different than previously. I was looking at a different problem. Once I saw it clearly, everything dropped into place. I was on the Santa Monica beach at the bottom of Sunset Boulevard when it dawned on me, and I said to Julie, "This is Alice through the looking glass. I have just got through the looking glass. The mechanisms and institutions are all going to come out like unwinding a sweater." As soon as the function of money is nailed down as the great simplifier and decoupler of trade, we may find that one can run out of money. Running out of money can be cured by inventing credit. Unfortunately, when credit exists a trader may be in a position that formal theories of equilibrium miss. A trader may be in a position where she has borrowed, but through blundering or for other reasons the borrower is unable to repay, at which point you have to invent bankruptcy laws and so on, and the logic for the invention of a new instrument appears. Some forty-five years ago on the beach, I said that the way into the theory of money is there, but now the key has been found the whole topic is breaking into many separate pieces that must be dealt with before there is a finished theory. The institutional richness of dynamics is there.

I estimated that each of the many pieces was going to require the collaboration of two or three mathematicians or mathematical economists, two or three probability theorists, and at least one physicist, and take a year or two. I did not think I would live long enough to finish, but in my opinion, I more or less finished about a year ago and am waiting for my book from the MIT Press entitled *The Guidance of an Enterprise Economy* to appear;[1] it was written with a young physicist and biologist, Eric D. Smith (who also just finished a thousand-page book with Harold Morowitz, on the origins of life, just before Harold died). The details on my putting together of the jigsaw are in the companion piece I gave to Kai and also in CFDP 2036.[2] I am in the process of writing a nontechnical version covering much of the work, and hopefully I will live long enough in working shape to finish this. After this has been done and has found a home, it may be time to retire and get some light reading done.

Notes

1 It was published in September 2016.

2 See http://cowles.yale.edu/sites/default/files/files/pub/d20/d2036.pdf.