Gary S. Becker

It is my pleasure to be here and to participate in the Nobel economists lecture series, a series that has had the great modern figures in economics speak, people such as Friedman, Samuelson, and Buchanan, among others. I'm supposed to talk about my evolution as an economist and the forces that influenced me. I will take that as my charge. Yet it is a difficult assignment. It is hard to assess the forces that have had a major influence on one's research. What I can do is to talk a bit about my development as an economist. I will emphasize the many instances where I was extremely lucky to come into contact with people—giants in the field—who had a great influence on me. I will also discuss instances where I started down research paths that at the time seemed to be rather natural and appropriate extensions of microeconomics; I was not fully aware that these would develop into more comprehensive, path-breaking, and often controversial contributions to economics and social science. Following very brief remarks about my days growing up, I will turn my attention to my student days at Princeton, where I first became serious about economics.

I was born in Pottsville, Pennsylvania in 1930, but grew up in Brooklyn, New York, where our family moved when I was a young child. My father was a businessman and my mother was a housewife. My parents were very intelligent, I see in retrospect. I didn't always see that when I was growing up—they were not highly educated; neither one of them went beyond the eighth grade. My father, growing up in Montreal, Canada, left school because he was eager to make money, despite his mother's insistence that he remain in school. My mother's lack of schooling is not surprising since at that time most young girls were not expected to continue with their education. My parents did realize the value of a good education. They did not insist that we pursue higher education, although both my two sisters and my brother and I did so. We could tell growing up that there was an appreciation for education, in spite of the fact that there were not many books in our house and neither one of my parents read a great deal.

I was an excellent student, in general, at least to judge by grades, but up to the age of 16 I was not what I would call a serious student. I was more interested in sports than in academic subjects. I did the amount of work necessary to get good grades. I was not intellectually inclined in high school, at least initially. For some reason, I cannot precisely state what the forces were, but at about the age of sixteen my interests began to shift. I date them dramatically to when I voluntarily gave up on the high school handball team of which I was a member (I probably could have been the number one player), but instead chose to become a member of the math team. They met during the same period, and I had to choose one rather than the other. I went through a little bit of uncertainty, but finally chose the math team. I am glad I did so, because my subsequent academic interests were centered on mathematics and science.

What I liked about the math team was that we actually had competitions. This was in New York City, and there were competitions among schools in which we had to solve problems, two tough problems at a time, and solve them within an intense time frame, usually ten minutes to do both problems. And while there was a lot of pressure and tension, particularly when you did not know the answers, it was a fun and collegial process, with five members on each team. The math competitions had an important influence on me, first in seeing other students who were quite good, and second in proving to myself that our team could do well against Bronx Science and Stuyvesant and the other specialized high schools in science and mathematics.

My senior year in high school I became concerned about doing something for society. Here I was interested in math and science and then, all of a sudden, my interests began to shift as I became more socially conscious. Like most young people at the time, I considered myself a kind of a socialist and felt that I should move toward politics or history or some field where I could make more of a contribution to society. I had a really mixed view, continuing to be strongly interested in math, but no longer really wanting to become a mathematician.

A Princeton Undergraduate

I went to Princeton when I was seventeen and had this conflict going on—a strong interest in math but the desire to make a contribution to society. As freshmen we had to choose some electives and were required to take a course in the social sciences. And for some reason (I have no idea why) I chose to take the principles of economics. And for me this turned out to be a very good, or basically lucky, decision. As you will see, this was just one of several lucky decisions that came at various times. We used as our textbook an early edition of Paul Samuelson's *Economics: An Introductory Analysis.* It certainly has been one of the best-selling textbooks in economics, or in any other field. It is still in use. What I liked about this book was the rather brief section on microeconomics—how prices operate in a market system. It appealed to me because of its more mathematical discussion; basically microeconomics seemed to have a very compact mathematical foundation. Given my previous interest in math, microeconomics had a natural attractiveness for me, while at the same time it was dealing with social problems. I was less taken by Samuelson's extensive discussion of macroeconomics, which seemed to me to be rather vague and not fully satisfactory. I still feel that way about it, although there has been some real progress in that area.

I graduated from Princeton in three years, but during that period I read as many articles and books on economics that I could manage—probably the most influential being Stigler's *The Theory of Competitive Price,* as it was then called, and Hicks's *Value and Capital.* I remember reading Hicks one summer. When I saw one of my teachers in the fall and he asked what I was doing, I told him, "I read *Value and Capital* this summer, but I found it very hard." He said, "Don't worry about that, none of the faculty understand it either." So I felt they didn't understand it and I felt I didn't understand it either, but at least I was close to understanding it (or so I thought at the time). So that gave me a little confidence—maybe I could get someplace in economics.

The paper I wrote for my junior year thesis (required at Princeton) was on classical monetary theory. I criticized some analysis of Leontief and others that had been in the literature and debated at the time. Several faculty members said that I was correct in my criticism, and I think, in retrospect, I was correct. Eventually I collaborated with a teacher, William Baumol; we published the analysis from my junior thesis, plus other material, in the *American Economic Review* in 1952. The paper was titled "The Classical Monetary Theory: The Outcome of the Discussion." My senior thesis was also published in the *American Economic Review* during 1952, in a paper titled "A Note on Multi-Country Trade."

So I was doing well in economics. During my senior year at Princeton, however, I was losing interest in economics and began to think that I should go into something else. Economics seemed excessively formal to me. I'm sure that cannot be true of anything you have been reading as students nowadays, but then that is how it seemed to me. Economics appeared incapable of helping me understand the issues in which I had an interest: inequality, class, race, prestige, and similar issues that were important for society. A sociology professor at Princeton suggested I look at Talcott Parsons's *Structure of Social Action.* Parsons, then the dominant figure in American sociology, started his career as an economist and believed that social theory included economics as a special case. I tried to read this book, but it contained an enormous quantity of jargon that did not lead anywhere, or at least anywhere that I could follow. I concluded that sociology was too hard, and returned, somewhat reluctantly, to economics. I remained unhappy—unhappy by what seemed to me a disconnect between what economists would talk about in textbooks and elsewhere and what I wanted to talk about.

I decided nevertheless to go on for a doctorate in economics. Adlai Stevenson once defined a graduate student as someone who didn't know when the party was over. Well, I wanted to continue in this party atmosphere, so I went for a doctorate. Most of the faculty wanted me to stay at Princeton, although I had already taken many graduate courses at Princeton; I felt, and some of my teachers agreed, that it would be better if I went someplace else. I was choosing between Harvard and Chicago and, for a variety of reasons, I decided to go to Chicago.

Chicago: The Early Years

From a professional point of view, the decision to go to Chicago was probably the most important decision I ever made. The atmosphere at

Chicago was enormously stimulating. Original work was going on in many different areas and by many economists there, most notably Milton Friedman. The Cowles Commission (which subsequently left) was very active in mathematical economics and econometrics.

Milton Friedman became the greatest influence on my development as an economist. Attending his graduate course in price theory was just exciting, and I would eagerly wait for that course to come twice a week. Some people would even ask my friends, "How can he be so excited about attending class?" A lot of the classes were boring, but that is nothing new for students. Friedman's class was different. Here I saw economics as a tool and not simply as a game played by clever academics, which is what had worried me most about economics. In Friedman's hands, economics was a powerful tool to understand a whole host of problems—in the class Friedman dealt with things such as birthrates, insurance and lotteries, personal and business responses to taxes, how labor markets functioned, and the effect of having unionized versus non-unionized markets. And, on and on, economics was used to understand business practices of all types. "Let's see," Friedman would have asked, "how can we understand what Microsoft is doing?" (had Microsoft existed at that time).

It was a great course that showed me what I thought was not possible. You can do economics and do it in a rigorous way and nevertheless talk about important problems. So my indebtedness to Milton Friedman, one of the greatest economists of the twentieth century, is unlimited.

There were other people in Chicago who were doing important, original work: Ted Schultz in human capital, Gregg Lewis in labor economics, Aaron Director in law and economics, L. J. Savage in statistics and probability. It was a wonderful intellectual environment, and as I developed within that environment, I no longer had this feeling that economics couldn't do it. Indeed I developed the Chicago chip-on-the-shoulder attitude that economics could unlock the mysteries of the real world. Right or wrong, it was a great feeling to have. Here I was being given this powerful tool and a belief that the mysteries of the social world could be unlocked if we applied this tool in some creative fashion. I began to believe this as a graduate student. I still believe that it is true.

I stayed at Chicago for six years, the first three as a graduate student. During the second year I was looking for a thesis topic and had already done some research on an economic approach to political democracy. My paper on this topic was almost published in the *Journal of Political Economy,* but one of my teachers, Frank Knight, was the referee, and he did not like it. I have kept his comments to this day. Knight was a great economist, but he looked at democracy with what I would characterize as a normative point of view. He defined democracy as government by discussion. I wanted to employ an approach to democracy that looked upon it as an institutional system operating in a particular way, for good or bad, which you could analyze using economic analysis. The approach I took in this paper was a very early work in what we now call public choice theory. The editor wanted to publish it in the *JPE,* but was persuaded not to by the referee. It eventually got published in 1956, in a shorter, toned-down version in the first issue of the *Journal of Law and Economics.*

I thought of developing this topic more broadly as one of my possible thesis subjects. But, finally, I hit on something in which I became more interested—an economic approach to the issue of discrimination against minorities, whether religious minorities, racial minorities, gender, or anything else. It was a topic that I had been interested in, of course, but never thought about systematically until I began to think in response to a question put to me by Friedman and Ted Schultz—how might we analyze the fact that there is discrimination in the economy? It occurred to me at the time, but again I am not sure exactly how, that we can associate with each person a taste for discrimination. This taste or prejudice would be measured based on how much income an employer is willing to forfeit in order to avoid hiring somebody who he didn't like from a group that he didn't like; or how much an employee is willing to forfeit to avoid working with a member of a group that he didn't like; or how much a consumer will pay to avoid a product that is served by or produced by a group that he didn't like. So my approach to discrimination was to look at the willingness to pay, or forfeit income, in order to exercise a prejudice. This is still the only right way that I can tell to look at discrimination in mortgage lending, searches for drugs in cars, and other issues that are of great contemporary interest right now. Once you took this approach, then you had to think about how to link the observed discrimination that we see in the marketplace to these personal preferences or prejudices.

To make the link between market discrimination (that is, market outcomes) and personal preferences for discrimination required that I provide a model and analysis. My Ph.D. thesis showed how the degree of market discrimination depended not only on the discriminatory preferences of employers, employees, and consumers but also on the degree of competition in product markets, production technologies, and many other economic variables.

As I was working on my dissertation, I was fortunate to get encouragement from my faculty advisors, but some members of the faculty were highly dubious whether this was an appropriate subject. Their attitude was: "What's a good economist doing working on discrimination?" They could not talk me out of this topic, however, so they insisted instead that a distinguished sociologist at Chicago, Everett Hughes, become a member of my thesis committee, just to make sure that I did not go off the deep end. I do not think Hughes was much interested in what I was doing. The nice part was that he did not object to anything I was doing either. I would go to see him once every nine months and he would say "okay" and that was it. That satisfied the faculty who were dubious whether this was a worthwhile project.

Prior to entering the academic job market, I went to Harvard to present some of my work and also to MIT to discuss this work with some people. I remember talking to a younger faculty member at MIT (a subsequent Nobel Prize winner who gave one of your lectures); he asked me what I was working on and I said "racial discrimination." He said, "I thought you were a neoclassical economist?" I said, "I *am* a neoclassical economist, but isn't this part of neoclassical economics?" But I could not convince him that discrimination was a legitimate subject for economists to work on.

And this was the general experience I had with my dissertation research on discrimination. Eventually a revised version was submitted to the University of Chicago Press. The Press received very negative reviews on my manuscript when it was sent out to readers. The editor did not want to publish it. The economics department finally "bribed" the press. You see, Chicago believes in the market system. So the department said we would put up part of the cost and would share any profits on the book. The press agreed to publish my book, but only because of that bribe.

It gave me great pleasure that about ten years after the Press published *The Economics of Discrimination,* the then editor of the press (who was also editor when my book came out) wrote an article, published in the *American Economic Review,* in which he admitted that my book was attracting sizeable interest. It was not selling a lot—but it was creating a great deal of interest.

The negative reaction to my work on discrimination, coupled with Frank Knight's hostility toward my article on democracy, made it clear to me that using economic analysis to discuss social and political issues was not going to be welcomed with open arms by most economists. I initially had expected economists to applaud attempts to widen the scope of their field. I was surprised that the main hostility toward my work, at least as it was explicitly stated, came from economists, not non-economists. I began to realize that my original view was naïve. All disciplines have a strong and probably justified degree of intellectual conservatism. You do not give up ideas and concepts you have held for a long time without a fight. It is necessary to fight to get new ideas accepted.

Even after I became aware of the extent of the hostility, I remained confident that the contribution of the economic approach to broader problems would eventually be recognized. This confidence in what I was doing helped me persist against sometimes considerable and vicious opposition. There were two reasons why I remained confident. First, it just seemed to me obvious that economics could contribute to these areas. Economics was not the whole story, it was not the final word on discrimination, but how could economists justify that prior to my book on discrimination, with two or three exceptions, there was virtually no work by economists on a topic as enormously important as discrimination in the marketplace? I mean, it's incredible! So it seemed obvious to me that there was a role here.

That was one factor. As important as this was, I do not think that this would have been sufficient to enable me to persist against continuing hostility. The other factor was that I was fortunate to have intellectually powerful people on my side. I gained strength from the support of senior economists I greatly respected. Support came from my teachers, like Milton Friedman and Gregg Lewis, George Stigler—who soon effectively became my teacher—and other friends I had met along the way such as Armen Alchian and Jack Hirshleifer.

In short, this was a period in which my research and intellectual discourse encountered an enormous amount of opposition. There was little demand to hire me from the major institutions. After three years as a graduate student, I accepted an assistant professorship at Chicago. And these six years, looking back on it now, were perhaps the most important and most exciting of my career. I formed the foundation of what I was going to do later on. And I was learning at a rapid rate as I absorbed so many new things that were coming from people at Chicago and from others who came through Chicago. Four of my teachers went on to win the Nobel Prize: Milton Friedman, Friedrich Hayek, who was then on the Committee for Social Thought (I attended his seminars), Tjalling Koopmans, and Ted Schultz. And in addition to that, the Chicago intellectual vitality had many others, some mentioned here, who did not win the Nobel Prize but have done very important work that has been continually recognized.

Chicago wanted me to continue there after my three-year term was up, but I wanted to leave, even though Chicago offered me more money and a good chance of getting tenure. I felt it was more important for me to leave the nest and go out on my own. I had protection at Chicago with the likes of Friedman, Schultz, and Lewis, among others, and that was great, but I wanted to see if I could make it on my own. I said in effect, "I appreciate the offer, but I really don't want to stay at Chicago." So I looked around, went on the job market, and as a lot of other students have experienced, I did not find a overwhelming demand for my services. Major universities, like Harvard, MIT, Berkeley, and Yale showed no interest in me and did not interview me at any of the meetings. This was probably because I was a Chicago graduate and Chicago at that time was an "outlaw" department in the profession. Its students were treated as suspect by representatives of most of the major institutions. It is not that extreme now.

I had only two interviews, with Johns Hopkins and Columbia. Hopkins decided not to make me an offer. So I considered all my choices and decided to choose among them and went to Columbia, which I was happy to do. I also was offered a position at the National Bureau of Economic

Research, which was then in New York City, so I could combine the two. I spent a dozen highly productive years at both institutions.

Columbia and the National Bureau of Economic Research

I started my work on human capital at Columbia and the National Bureau of Economic Research. When I came to the bureau, the then director, Solomon Fabricant, asked me what I would like to work on. From my work on discrimination, I had seen that there were enormous gaps in earnings between workers with different levels of education, among both blacks and whites. I knew the work Ted Schultz had been doing on human capital, which I found to be of considerable interest. So I told Fabricant that I would like to do a study on the rates of return to education and training. This would be a new departure for the bureau, but he said, "I will see what I can do." He got a small grant for me to work on education and earnings, which I began in 1957.

I soon realized that much more was needed in the human capital field than to calculate rates of return. There were no foundations for the theory of investment in human capital. My study was intended to be empirical, but I set about trying to sketch out a small set of foundations to give the work theoretical content. As I was sketching out some basic theory, I had no vision at all of what this would lead to. Once again, here is an example of the role of luck. As I delved into the theory and tried to develop a basic foundation for human capital investment, it looked to me that the theory could explain the way earnings rise with age (a concave age-earnings profile), the effect of education on the distribution of earnings, externalities of human capital, and many other issues that continue to this day to be discussed and debated. I was amazed and then greatly excited when I began to realize that this framework could integrate scores of observations and regularities in individual earnings, occupational differences in earnings, and employment.

In 1959, I made the first public presentation of some of my results at a session of the annual meetings of the American Economic Association. I presented a short paper that compared rates of return to schooling and returns on physical capital in the United States. And the discussants, to my amazement, were absolutely outraged. Once again, I continued to be surprised by what I should have anticipated. What was it that so outraged my discussants? In retrospect it seems silly. They were outraged that I was treating education as an economic activity, believing that this assumption somehow denigrated the cultural or non-economic aspects of education. I replied with some fervor and bluntness to my critics. It was one of the more heated sessions of the meeting. I was taken aback, but truth be told, I did not lose any confidence about what I was doing because their comments seemed so silly to me. I could not really believe that senior economists—I was 29 years old at the time—were making such dumb comments on my paper.

I continued working on the economics of human capital and in 1962 published an article on it. It was in fact well received. Then, in 1964, I published a book called *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education.* The long subtitle is now forgotten—it is now called *Human Capital.* Actually, I debated a long time before I used the title *Human Capital* because I had been aware that people said that if you call it "capital" you are treating human beings as if they had no soul. Some people would make fun of it and call it "human cattle," suggesting that one is not treating humans as individuals. I knew that, and could have weaseled a little and called it "human resources," a phrase that was becoming common at the time. I decided to take the bull by the horns and title the book *Human Capital,* although it had this long subtitle to protect myself a little.

By the time I finished this research, I was indeed convinced that human capital was a crucial concept to understanding economic and social issues in many areas of life. Still, and this I will also confess, I was not prepared for the magnitude of its impact. Eventually, it would be referred to endlessly, and by that language—human capital—not only in academic writing but by politicians of both parties, journalists, even in ecclesiastical encyclicals. After a while some of the people who had resisted using this term began to think, "Well, look, if we call everything human capital and say we are investing in people, this can provide a good rationale for obtaining public monies." I remember the superintendent of the Chicago school system at that time, Benjamin Willis, inviting me to deliver an address to a meeting of superintendents. He told me, "I don't know why people dislike that word; it can be a great tool for us superintendents to get more money." So I think that this partly explains its success.

Everything is now called human capital, including some things that should not be so called.

The more fundamental reason why the term human capital has been so successful and has continued to grow rather than shrink in importance is that it integrates into one basically simple concept a lot of actions and behavior that affect the individual and economy. In essence, human capital analysis puts individuals at the center of attention in an economy, not machinery, plant and equipment, or other inanimate objects. It is people who move an economy, people who determine whether an economy is rich or poor; human capital is a major aspect of the productivity and well-being of people. And it is investment in human capital, by acquiring skills from one's parents, through going to school, or through training and knowledge in the workplace, that helps determine a person's and an economy's stock of human capital wealth. I think this explanation is the fundamental reason why the term human capital has proven appealing and durable. It integrates diverse aspects of human behavior, pushing people to the center of the economic stage, so to speak.

At the same time that I was doing research on human capital, I was also working on the economics of the family and the demand for children. I began this work by asking what determines how many children that families have. I gave my first paper on this topic in the late 1950s, around the same time that I presented my early work on education. At a conference on the economic analysis on fertility, I drew an analogy between the demand for children by parents and their demand for durable consumer goods. I used that language in my paper. Well, you can imagine the reaction from my audience—as soon as I used this language everyone started laughing. Well, not everyone. This was a mixed audience of economists and non-economists. One economist who was the discussant on my paper, a youngish economist at Harvard, was very negative about it, stating that this approach could not explain much about the demand for children. I had learned over time to expect a negative reaction from the audience. But, once again, I was a little surprised by just how much hostility (and this was verbally expressed in public comments) my work aroused among some eminent economists. And yet again, this was an instance in which I feel indebted to Milton Friedman. Friedman had been a participant at the conference and was attending my session. He got up and vigorously defended my paper during the discussion period. I felt, well, there are these fools on the one side criticizing me, but I have people like Friedman defending me. And that is all I needed. I had enormous respect for Friedman—I still do—so I thought, "Now look, if Friedman is defending my work, there must be something to what I am doing."

Another subject I was working on at this time was the theory of the allocation of time. As I worked on the economics of education, it soon became apparent, not only to me but to Schultz and others working in this area, that the major costs of going to school were generally the earnings students give up by not working full time. Even at private colleges and universities, where the tuition is high, the major cost is generally foregone earnings, or the opportunity cost of one's time. This seemed to me to be important, but apart from labor-leisure choice analysis used to understand labor supply, there was little formal role for the value of time or the allocation of time in microeconomic analysis. Once again, as I was working on one subject, I was led by the logic of the analysis into a different, albeit related, subject.

So I began at that time to try to generate a systematic analysis of time use. I eventually published an article called "The Theory of the Allocation of Time," (*Economic Journal,* 1965), which treated each household like a small-scale factory or enterprise that used time and goods to produce various commodities that they could not buy directly, like children, good health, a good meal, and things of that kind. Unlike the work on discrimination, human capital, and fertility, my work on time allocation was less controversial and more quickly and readily accepted within the economics profession.

As evident from the previous discussion, my time in New York was highly productive. At Columbia, Jacob Mincer and I jointly conducted a labor economics workshop that gained a large following among a new generation of labor economists. I greatly benefited from my colleagues at Columbia and the bureau; in addition to Mincer these included Victor Fuchs, Robert Willis, Finis Welch, and Sherwin Rosen. I had a remarkable group of students at Columbia, too numerous to mention here, that have gone on to make major contributions in the fields of human capital and labor economics, health, crime, and law and economics. So I was quite happy professionally at Columbia. I did not consider many other offers, partly because I did not get many. It made life easy.

In 1968 and 1969, student rebellions hit American universities. And there were few schools that were harder hit than Columbia. I was very disturbed by this, not so much by what the students were doing, but by the faculty's weakness and eventual unwillingness to stand up to disruptive and, in my opinion, intolerant student tactics. I was especially disappointed by the attitude of senior members of my department (not all, but most). They seemed to me to be weak and vacillating at a time when I thought there existed clear-cut and simple steps that could be taken to protect the intellectual integrity of the university.

I began to think of leaving Columbia at that time. The future to me looked dim, having lost a lot of confidence in my colleagues. I began to look around. I received an inquiry from the economics department at Harvard through an old friend of mine from undergraduate days, Otto Eckstein, who was a professor at Harvard. And he said: "Would I be interested in coming to Harvard? It would be a good change for me." I said I was interested, but then somebody else called me back and said Harvard had unanimously voted to give me a one-year visiting offer. On the spot I said, "I am not interested." To be unanimously voted a one-year visiting offer did not seem to me to be a display of great interest. Eckstein had been talking of my becoming a permanent member. It seemed obvious to me that a permanent offer had run into difficulty among some members of the department. So I turned the offer down. For years Chicago was the only major institution interested in me, and they again renewed their offer for me to come back. I told them, "I will come back for a one-year visit." I did what Harvard had asked me to do, but I knew that Chicago wanted me to stay for good.

Return to Chicago

Upon my return to Chicago, I found it to be at least as stimulating as when I was a graduate student. Friedman was still there in his prime, Stigler was doing very important work in industrial organization and political economy. They had Black, Fama, Miller, and Scholes working out rigorous approaches to finance and the evaluation of options. Coase, Posner, Landes, Demsetz, and others were doing law and economics, building on the pioneering work of Aaron Director. My work on the economics of crime fit in well with their interests. We had Bob Fogel doing path-breaking work in economic history—slavery at first and then other topics. It was a magical place!

During my year there, following a lot of soul searching (because I did like my setup in New York), I decided to stay in Chicago and accepted an offer there. I thought that a permanent return to Chicago would help renew my energies, and it did. I developed a very close friendship with George Stigler, who became my best friend and collaborator on a number of projects. I learned a great deal from George over the years. Unfortunately he died in his prime. Although he was 80 at the time, his mind was still very active.

While I was at Chicago, I decided to continue my work on the family. I remember sitting in a hotel room in New York and thinking about the question of marriage—who marries whom? Now why I started thinking about this at that time, I do not know. True, my first wife had died and I was unmarried at the time, so perhaps that had something to do with it. As I thought about the question of who marries whom, it seemed to me that there is something we might think of as a marriage market. Not a market à la Li'l Abner on Sadie Hawkins Day, if any of you remember Al Capp's comic strip *Li'l Abner*. It's not literally a market with explicit buying and selling, but you can think of the matching of partners as operating like a market—people make their choices, they date, and so on. I worked out some rules that would determine who would marry whom, threw in a couple of "theorems," and submitted a paper to the *Journal of Political Economy*. The same distinguished economist that could not accept my discrimination work ended up as a referee (George Stigler was the editor and told me this). This referee hated the paper, saying, "What is Becker doing wasting his time working on these questions." Stigler disagreed. He told me that he liked the paper, to take account of the referee comments as best I could, and that they would publish the article, which they did. I then submitted a paper a couple of years later on the economics of divorce. Another very good economist wrote back and said they should not accept this paper. And Stigler again overrode the referee recommendation and, following revision, published my paper. Each of the papers turned out to have a good market, leading to a considerable body of subsequent research.

At about that time I decided that I should try to weave all this together into a book on the economics of the family. The book was to combine and integrate the topics of fertility, divorce, marriage, investment in children, and the evolution of the family over time. I even dealt with some nonhuman species. I worked very hard on this book for four or five years. I would wake up in the middle of the night, many nights, and work on it. It was very intense and, finally, in 1981, I published *A Treatise on the Family*. I was exhausted by that time, and it took me roughly two years to regain my mental energy. A comprehensive treatment of the family is inherently such a difficult problem, with so much history, so many cultures, and the like. I found it very difficult. And to this day the book remains controversial. When the Nobel Prize committee awarded me the prize in 1992, their news release stated, "Gary Becker's analysis has often been controversial and hence, at the outset, met with scepticism and even distrust." Nowhere has this characterization been truer than with my work on the family.

Time does not permit me to discuss much of my work in recent years. But I would be remiss if I did not mention my rewarding collaboration with Kevin M. Murphy, a former student and now a brilliant young colleague at Chicago. Building on earlier work on preferences and addiction in a paper I wrote with Stigler ("De Gustibus Non Est Disputandum," *American Economic Review* 1977) and a Chicago dissertation by Laurence Iannoccone, Murphy and I have written several articles on "rational addiction," among other topics.

Writing for a Popular Audience

Throughout my career, I had worked on topics closely related to public policy—education, crime, the family, discrimination, addiction, and politics, for example—yet I remained aloof from debates over public policy. I had never given advice to any political figures. Prior to 1985, I had never written one single word in the popular media, not a word, be it a newspaper, magazine, or the like. By 1985 I was 55, so I had gone through roughly 35 years of doing economic research, and not one single popular work. Nowadays, some economists hit 30 and begin writing op-ed pieces. I always felt it was a good division of labor on my part to concentrate on my research.

You can imagine then that it was a great surprise to me when in 1985 I received a telephone call from someone at *Business Week,* a magazine I had criticized a couple of years earlier because they misquoted me on something. Their representative said, "Would you be willing to write a regular column for the magazine—a one-page column every four weeks?" My initial reaction was to reject their proposal. But what I said was, "It is not something I have done, it would take me away from my research, but I will think it over and give you an answer." The response back was, "Oh good, I thought maybe you would turn us down on the spot."

So I went back and told my wife, Guity, and she said, "You should do it." She prevailed on me to try it for a while. She argued that the columns would help spread my ideas and might even have a small influence on public policy. And if I did not like doing it, I could always stop. Guity also promised that she would read my early drafts and provide comments. I am happy to say that she gave me good advice. And I might add at this point that I have been very fortunate for almost thirty years to be getting excellent advice and encouragement from her, both professional and personal. She has had an enormous influence on me.

So I got back to *Business Week* and said, "Okay, I am willing to write a column on a trial basis." They said, "Don't worry, we are looking upon it as a trial as well." And the contract sent to me stated that "either party can terminate the agreement with one month's notice." In academia we are used to tenure; there was no tenure here.

It was hard for me to learn how to write a popular column. The hardest part about writing is writing something short. I think it was Oscar Wilde who wrote to someone saying that he was sorry his letter was so long, but he did not have time to make it short. Writing short requires far more effort than writing long. I had to write roughly 800 words—one *Business Week* page comprising a certain number of *Business Week* lines—and to write in simple and nontechnical language.

And it was hard. But again, I was lucky. I do not know why they asked me, to tell the truth, but the experience has been great for me. It has taught me how to express economic ideas in a simple and nontechnical way. I will make the assertion that every single important economic idea can be stated simply. To develop an idea, you may have to use some apparatus to do it in a systematic way, of course, but you can state the essence of any idea simply. At least I have never heard one that could not be stated simply. And when people say that an idea is too complicated to state simply, it usually means they do not know how to state it simply, sometimes because they do not fully understand it.

Writing for a popular audience, I have addressed a wide range of topics. My interests in economics are broad and my column has resulted in my interests becoming even broader. I have written about baseball, the unfairness of the NCAA in not allowing student athletes to be paid, marriage contracts and religion, immigration, education, and the policies of different presidents or political candidates. Once again, this was one of several lucky events that ended up having a positive and important effect on me.

The Prize

The final thing I will discuss is the Nobel Prize. When I entered economics there was no Nobel Prize in economics; it is a recent award, having begun in 1969. There were two prizes awarded by the American Economic Association, the John Bates Clark medal for economists under the age of 40 (I was honored to receive this award in 1967) and the Francis Walker Prize for senior economists. The AEA abolished the latter once the Nobel Prize began.

By 1980 I began to be mentioned as a serious candidate for the Nobel Prize. I realized then that there were many older, highly deserving economists in the Nobel queue, mainly because the prize had not existed for very long. I realized that if I were ever to get the prize, and this was not assured, it would have to come later.

By the latter part of the 1980s, however, I felt pressure mounting on me because my name was so often mentioned as a leading candidate. A betting pool organized by some American economists had me listed as their favorite (i.e., the lowest odds person) for three or four years running before I got the prize. And so individuals and reporters had begun asking me with some regularity "When will you get the prize?" or, once the prize was announced each year, "Why didn't you get it this year?" Of course this bothered me.

I admit that I wanted to be awarded the prize, and for several reasons. Clearly the prestige and financial rewards were important. But there was another reason as well. I had a lot of students and others who had been pursuing research along the lines indicated by my work, often working outside the more narrow range of traditional economics topics. These economists were often given a tough time in the profession, and several had difficulty getting good jobs. One of my top students years ago, who worked on religion, generated almost no job market interest. Now he is considered a preeminent economist working in religion, perhaps the preeminent person in any field doing research on religion. But he had a difficult time getting a job. People would ask, "Religion, what kind of topic is that for an economist?" I wanted myself and others to get the validation that the Nobel Prize would provide—that the economic approach to human behavior is acceptable work and that we are doing real economics. Yet in 1992, my work continued to be controversial, especially in Western Europe, and I began to wonder whether I would ever receive the prize.

I thought there was no chance I would receive the prize in 1992. Economists from Chicago were among the three winners in 1990, and another Chicagoan, Ronald Coase, won in 1991. I concluded that the committee would never choose three Chicagoans in a row. In the fall of 1992, I had a terrible flu with a very high fever. Doctors wanted to put me in the hospital, but my wife resisted that move. For both these reasons, the last thing on my mind was the Nobel Prize. I had no idea when the announcement was coming. I had not been into school for a week and I was in bed at 5:30 on the morning of October 13, sleeping soundly for the first night in about a week. My wife, who had been up grading papers, answered the phone when it rang that morning, worried that it might interfere with my sleep. She was a bit nasty, she said to me later, but the caller said this was an important phone call for Professor Becker. My wife did not think it was the Nobel Prize, at least not for me. She went and woke me up and I kept saying, "I want to sleep, I haven't slept so well for a long time." "No, it's a call from Sweden," she said, and that was the magic word. A call from Sweden! I did not know that the prize was announced. But when I heard "a call from Sweden," I figured, "well, maybe" and picked up the phone. My wife subsequently said that she was sitting there as I was saying, "yes, yes" with no expression on my face, and she figured that they had called for my input on somebody else who was being considered. Finally, she hears me say "Thank you very much; tell the committee what a great honor it is that you have conferred on me." This of course was the call telling me I had been awarded the prize. The first thing Guity did was to let out a yell and the first thing I said was "I'm glad that monkey is off my back." We called our four children and then we took the phone off the receiver to have breakfast in some peace. Before 6:30 or so, the reporters, the *New York Times,* the university people, and so on, had found us.

So the long years of fighting attacks were largely over, although the official Nobel announcement called my work controversial. Still, they awarded me the prize. I did not realize at the time that there were some protests in Sweden about my getting the prize. Some Swedish feminist groups in particular complained about my receiving the award, charging that my work on the family was anti-feminist (I do not believe it is) and they had discussions about whether to picket my Nobel lecture. Perhaps that helped publicize the lecture—the hall was packed. People were standing all over the place, but they caused no disruptions, everything went very smoothly, and it was a great week.

The prize recognized my research in four broad areas: investments in human capital; behavior of the family (or household), including distribution of work and allocation of time in the family; crime and punishment; and discrimination in the markets for labor and goods. In private I was told that some members of the committee did not want to award me the prize. But because in the last couple of years I had been nominated the most often by the economists asked to identify potential recipients, the committee felt they had to give me the prize. Well, better than not. And so, it was a great week and a great period of recognition.

Some people have studied Nobel laureates and discovered that they did a whole lot less work after receiving the prize than before. I was aware of that work, done by several sociologists, and there is no doubt that there are many demands on your time as a result of getting the prize. I resolved to continue to do research and not change my life drastically. Since I have received the prize I have published three books. Two of the three books include new research; the other published book, *The Economics of Life,* is a collection of my *Business Week* columns. I hope that in my case the iron law of the negative effect of the Nobel Prize on productivity has been overcome.

To the best of my ability, I have tried to assess the factors that were most important in facilitating whatever accomplishments I have had. I have to give many thanks to my parents, my wife Guity, my children, my sisters, my brother, and certainly my teachers and colleagues. A series of lucky events over my life have put me into contact with enormously outstanding people and led me down paths whose rewards I could not have anticipated. I'm not suggesting that the Nobel Prize, or whatever is one's accomplishments in life, is simply a matter of luck. I am suggesting that success requires a number of fortuitous events to occur, many of which one cannot readily imagine, plan for, or determine. I suspect that there are a lot of equally able people out there, and it is only those who are particularly lucky or fortunate who end up receiving a Nobel Prize or some other noteworthy accomplishment. I have been fortunate.

Awarded Nobel Prize in 1992. Lecture presented April 13, 2000.

Date of Birth

December 2, 1930

Academic Degrees

B.A. Princeton University, 1951

M.A. University of Chicago, 1953

Ph.D. University of Chicago, 1955

Academic Affiliations

Assistant Professor of Economics, University of Chicago, 1954–1957

Assistant Professor of Economics, Columbia University, 1957–1958

Associate Professor of Economics, Columbia University, 1958–1960

Professor of Economics, Columbia University, 1960–1968

Arthur Lehman Professor of Economics, Columbia University, 1968–1969

Ford Foundation Visiting Professor of Economics, University of Chicago, 1969–1970

University Professor of Economics, University of Chicago, 1970–1983

University Professor of Economics and Sociology, University of Chicago, 1983–present

Senior Research Fellow, Hoover Institute (Stanford), 1990–present

Selected Books

The Economics of Discrimination, 1957, 1971

Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education, 1964, 1993

The Economic Approach to Human Behavior, 1976

A Treatise on the Family, 1981, 1991

Accounting for Tastes, 1996

The Economics of Life, 1997 (with Guity Nashat Becker)

Social Economics: Market Behavior in a Social Environment, 2000 (with K. Murphy)

James M. Buchanan

Born-Again Economist

I have been tempted to expand my title to "Born-Again Economist, with a Prophet but No God." Both parts of this expanded title are descriptive. I was specifically asked to discuss my evolution as an economist, an assignment that I cannot fulfill. I am not a "natural economist" as some of my colleagues are, and I did not "evolve" into an economist.[1] Instead I sprang full blown upon intellectual conversion, after I "saw the light." I shall review this experience below, and I shall defend the implied definition and classification of who qualifies as an economist.

The second part of my expanded title is related to the first. It is my own play on the University of Chicago saying of the 1940s that "there is no god, but Frank Knight is his prophet." I was indeed converted by Frank Knight, but he almost single-mindedly conveyed the message that there exists no god whose pronouncements deserve elevation to the sacrosanct, whether god within or without the scientific academy. Everything, everyone, anywhere, anytime—all is open to challenge and criticism. There is a moral obligation to reach one's own conclusions, even if this sometimes means exposing the prophet whom you have elevated to intellectual guruship.

In an earlier autobiographical essay, "Better Than Plowing,"[2] I identified two persons who were dominant intellectual influences on my own methodology, selection of subject matter, attitude toward scholarship, positive analysis, and normative position. One of these, Knut Wicksell, influenced me exclusively through his ideas. I used the occasion of my Nobel Prize lecture to trace the relationship between Wicksell's precursory foundations and later developments in the theory of public choice, notably its constitutional economics component with which I have been most closely associated. By comparison and contrast this paper offers me the opportunity, even if indirectly, to explore more fully the influence of the second person identified, Frank H. Knight, an influence that was exerted both through his ideas and through a personal friendship that extended over a full quarter century.

The paper is organized as follows. In the following section I try as best I can to describe my state of mind, intellectually and emotionally, before I enrolled in the University of Chicago in 1946. Then I offer a retrospective description of my Chicago experience, with an emphasis on my exposure to the teachings of Frank Knight along with a subconscious conversion to a catallactic methodological perspective on the discipline. The next section briefly traces the catallactic roots of my contributions to public choice theory. After that I discuss the remembered events and persons who were important in giving me the self-confidence that was surely necessary for any career success. Frank Knight was important but by no means unique in this evolution (and in this respect the word "evolution" can be properly applied). I then discuss the influence of Knight's principle of the "relatively absolute absolute" upon my own stance, as moral philosopher, as constitutionalist, and as economic analyst. Finally, I defend my use of the title of my earlier autobiographical essay, "Better Than Plowing," which has been questioned by colleagues and critics. Here I try to examine the motivations that, consciously or unconsciously, may have driven me throughout the course of a long academic career. Why did I do what I did? It may be helpful to explore, even if briefly, this most subjective of questions.

Pre-Chicago: Standards without Coherence

From 1940 I called myself an "economist," as my military records will indicate. I did so because after graduating from Middle Tennessee State Teachers College in June 1940, I was awarded a graduate fellowship in economics at the University of Tennessee for the academic year 1940–41, and I earned a master's degree in 1941. By the academic counters, I took courses labeled "economics," and I made good grades. But as

I have noted, however, I learned little or no economics in my preferred definition during that Knoxville year. I surveyed the workings and structures of the institutions of Roosevelt's New Deal; I came to understand central banking theory and policy; I learned something about taxation and budgeting processes; I learned a bit of elementary statistics, especially in practice. But neither in these courses nor in my prior undergraduate experience did I have proper exposure to the central principle of market organization. I remained blissfully ignorant of the coordinating properties of a decentralized market process, an ignorance that made me vulnerable to quasi-Marxist arguments and explanations about economic history and economic reality but also guaranteed that my mind was an open slate when I finally gained the exposure in question.

During the Knoxville year I did learn to appreciate the dedication of the research scholar though my association with Charles P. White, whose course in research methods was the intellectual high point. White instilled in me the moral standards of the research process. My experience with him, as both graduate student and research assistant, gave me something that seems so often absent in the training of the economists of the postwar decades, whose technology so often outdistances their norms for behavior.

By subject matter, by terminology, and with a bit of technique I left Knoxville as an "economist," but I lacked the coherence of vision of the economic process that I should now make the sine qua non of anyone who proposes to use this label. I have often wondered whether or not I was relatively alone in my ignorance, or whether something akin to my experience has been shared by others who purport to pass as professional economists without the foggiest notion of what they are about.

Chicago, 1946

I enrolled in the University of Chicago for the winter quarter, 1946. I had chosen the University of Chicago without much knowledge about its faculty in economics. I was influenced almost exclusively by an undergraduate teacher in political science, C. C. Sims, who had earned a doctorate at Chicago in the late 1930s. Sims impressed on me the intellectual ferment of the university, the importance of ideas, and the genuine life of the mind that was present at the institution. His near-idyllic sketch appealed to me, and I made the plunge into serious study for the first time in my life. In retrospect I could not have made a better selection. Sims was precisely on target in conveying the intellectual excitement of the University of Chicago, an excitement that remains, to this day, unmatched anywhere else in the world.

During the first quarter I took courses with Frank Knight, T. W. Schultz, and Simeon Leland. I was among the very first group of graduate students to return to the academy after discharge from military service during World War II. We swelled the ranks of the graduate classes at Chicago and elsewhere.

Within a few short weeks, perhaps by mid-February 1946, I had undergone a conversion in my understanding of how an economy operates. For the first time I was able to think in terms of the ordering principle of a market economy. The stylized model for the working of the competitive structure gave me the benchmark for constructive criticism of the economy to be observed. For the first time I was indeed an economist.

I attribute this conversion directly to Frank Knight's teachings, which perhaps raises more new questions than it answers. Knight was not a systematic instructor. More important, he remained ambiguous in his own interpretation of what economics is all about. He was never able to shed the allocating-maximizing paradigm, which tends to distract attention from the coordination paradigm that I have long deemed central to the discipline.[3] But Knight's economics was a curious amalgam of these partially conflicting visions. And for me the organizational emphasis was sufficient to relegate the allocative thrust to a place of secondary relevance. In this respect I was fortunate in my ignorance. Had I received "better" pre-Chicago training in economics, as widely interpreted, I would have scarcely been able to elevate the coordination principle to the central place it has occupied in my thinking throughout my research career. Like so many of my peers, aside from the few who were exposed early to Austrian theory, I might have remained basically an allocationist.

There are subtle but important differences between the allocationist-maximization and the catallactic-coordination paradigm in terms of the implications for normative evaluation of institutions. In particular the evaluation of the market order may depend critically on which of these partially conflicting paradigms remains dominant in one's stylized vision. To the allocationist the market is efficient *if it works*. His test of the market becomes the comparison with the abstract ideal defined in his logic. To the catallactist the market coordinates the separate activities of self-seeking persons *without the necessity of detailed political direction*. The test of the market is the comparison with its institutional alternative, politicized decision making.

There is of course no necessary implication of the differing paradigms for identifying the normative stance of practicing economists. Many modern economists remain firm supporters of the market order while at the same time remaining within the maximizing paradigm. I submit here, however, that there are relatively few economists whose vision is dominated by the catallactic perspective on market order who are predominantly critics of such an order. Once the relevant comparison becomes that between the workings of the market, however imperfect this may seem, and the workings of its political alternative, there must indeed be very strong offsetting sources of evaluation present.

The apparent digression of the preceding paragraphs is important for my narrative and for an understanding of how my conversion by Frank Knight influenced my research career after Chicago. Those of us who entered graduate school in the immediate postwar years were all socialists of one sort or another. Some of us were what I have elsewhere called "libertarian socialists," who placed a high residual value on individual liberty but simply did not understand the principle of market coordination. We were always libertarians first, socialists second. And we tended to be grossly naive in our thinking about political alternatives. To us, the idealized attractions of populist democracy seemed preferable to those of the establishment-controlled economy. It was this sort of young socialist in particular who was especially ready for immediate conversion upon exposure to teachings that transmitted the principle of market coordination.[4]

An understanding of this principle enabled us to concentrate our long-held anti-establishment evaluative norms on politics and governance and open up the prospect that economic interaction, at least in the limit, need not embody the exercise of man's power over man. By our libertarian standards, politics had always been deemed to fail. Now by these same standards market may, just may, not involve exploitation.

An important element in Knight's economics was his emphasis on the organizational structure of markets, and it was this emphasis that elevated the coordination principle to center stage despite his continued obeisance to economizing-maximizing. Once attention is drawn to a structure, to process, and away from resources, goods, and services, many of the technical trappings of orthodox economic theory fall away. Here Knight's approach became institutional, in the proper meaning of this term.

It is useful at this point to recall that Frank Knight's career shared a temporal dimensionality with the seminal American institutionalists Clarence Ayres, John R. Commons, and Thorstein Veblen. He treated their technical economics with derision, but he shared with them an interest in the structure of social and economic interaction. Knight did not extend his institutional inquiry much beyond the seminal work on human wants that exposed some of the shallow presuppositions of economic orthodoxy. He did not, save in a few passing references, examine the structure of politics, considered the only alternative to markets.

Public Choice and the Catallactic Paradigm

Public choice is the inclusive term that describes the extension of analysis to the political alternatives to markets. It seems highly unlikely that this extension could have been effectively made by economists who viewed the market merely as an allocative mechanism, quite independently of its political role in reducing the range and scope of politicized activity. I can of course speak here only of my own experience, but it seems doubtful if I could have even recognized the Wicksellian message had not Knight's preparatory teachings of the coordination principle paved the way.

The point may be illustrated by the related, but yet quite distinct, strands of modern inquiry summarized under the two rubrics "social choice" and "public choice." I have elsewhere identified the two central elements in public choice theory as (1) the conceptualization of politics as exchange, and (2) the model of *Homo economicus*.[5] The second of these elements is shared with social choice theory, which seeks to ground social choices on the values of utility-maximizing individuals. Where social choice theory and public choice theory differ—and dramatically—lies in the first element noted. Social choice theory does not conceptualize politics as complex exchange; rather politics is implicitly or explicitly modeled in the age-old conception that there must exist some unique and hence discoverable "best" result. This element in social choice theory, from Arrow on, stems directly for the allocative paradigm in orthodox economics, and the maximization of the social welfare function becomes little more than the extension of the standard efficiency calculus to the aggregative economy.

By contrast, the extension of the catallactic paradigm—the emphasis on the theory of exchange rather than allocation—to politics immediately calls attention to the institutional structure of political decision making. Without Frank Knight as teacher and as role model, would Knut Wicksell's great work have been discovered by the fledgling economist that I was in 1948? I have strong reasons for doubt on this score.

The Evolution of Confidence

When I reflect on my own experiences over a tolerably long academic career, I come back again and again to identifiable events and persons that built up or bolstered my confidence, that made me, always an outsider, feel potentially competent among my academic peers. The first such event came with the release of academic grade records at the end of my second year at Middle Tennessee State Teachers College in 1938. My name led all the rest. For the first time I realized that despite my rural origins, my day-student status, and my graduation from a tiny struggling high school, I could compete with the town students, the live-in students, and with all those whose earlier education was acknowledged to be superior to mine.

A second such event occurred in January 1942, when I finished a three-month stint as a midshipman and was commissioned an ensign in the United States Naval Reserve. Again, despite my Tennessee heritage and my mediocre academic experience at both Middle Tennessee and the University of Tennessee, I ranked sixth or seventh in a midshipman class of some six hundred college and university graduates from across the land. The Tennessee country boy could indeed hold his own.

After a successful, interesting, exciting, and easy four years on active military duty in the Pacific theater of war, which I spent for the most part on the staff of Admiral Nimitz at Pearl Harbor and at Guam, my confidence was once again put to the test when I entered graduate school at the University of Chicago in January 1946. Here the test was of a totally different dimension. I knew that I could compete successfully in terms of the ordinary criteria—academic grades, degrees, and honors. I do not recall ever entertaining the slightest doubt about my ability to finish doctoral requirements. What I did not know was whether I could go beyond these criteria and enter the narrowed ranks of producing scholars who could generate ideas worthy of the serious attention of their disciplinary peers.

At this point Frank H. Knight again enters my narrative. Had my Chicago exposure been limited to the likes of Jacob Viner and Milton Friedman, both of whom were also my teachers there, I doubt that I should have ever emerged from the familiarly large ranks of Ph.D.'s with no or few publications. Jacob Viner, the classically erudite scholar whose self-appointed task in life seemed to be that of destroying confidence in students, and Milton Friedman, whose dominating intellectual brilliance in argument and analysis relegated the student to the role of fourth-best imitation—these were not the persons who encouraged students to believe that they too might eventually have ideas worthy of merit.

Frank Knight was dramatically different. In the classroom he came across as a man engaged always in a search for ideas. He puzzled over principles, from the commonsensical to the esoteric, and he stood continuously dismayed at the arrogance of those who spouted forth the learned wisdom. Knight gave those of us who bothered to listen the abiding notion that all is up for intellectual grabs, that much of what paraded as truth was highly questionable, and that the hallmark of a scholar was his courage in cutting through the intellectual haze. The willingness to deny all gods, to hold nothing sacrosanct—these were the qualities of mind and character that best describe Frank Knight. And gods, as I use the term here, include the authorities in one's own discipline as well as those who claim domain over other dimensions of truth. Those of us who were so often confused in so many things were bolstered by this Knightian stance before all gods. Only gradually, and much later, did we come to realize that in these qualities it was Frank Knight, not his peers, who attained the rank of genius.

As he was the first to acknowledge, Frank Knight was not a clever or brilliant thinker. He was an inveterate puzzler; but his thought process probed depths that the scholars about him could not realize even to exist. To Knight, things were never so simple as they seemed, and he remained at base tolerant in the extreme because he sensed the elements of truth in all principles.

There were many graduate students, both in my own cohort and before and after my time, who could not take in or relate to the Knightian stance before the gods. To these "outsiders" Knight seemed a bumbling and confused teacher, whose writings mirrored his thought and whose primary attribute appeared to be intellectual incoherence. To a few of us, what seemed confusion to others came across as profundity, actual or potential, and despite the chasm that we acknowledged to exist between his mind and ours, Knight left us with the awful realization that if we did not have the simple courage to work out our own answers, we were vulnerable to victimization by false gods.

My own understanding, appreciation, and admiration for Frank Knight were aided and abetted by the development, early on, of a close personal relationship. Some three or four weeks after enrollment in his course I visited Knight's jumbled office. What was expected to be a five-minute talk stretched over two hours, to be matched several times during my two and a half years at Chicago, and beyond. He took an interest in me because we shared several dimensions of experience. Both of us were country boys, reared in agricultural poverty, well aware of the basic drudgery of rural existence but also appreciative of the independence of a life on the land. Knight left his native Illinois in his teens for rudimentary college instruction in my home state of Tennessee, and he enrolled in graduate studies at the University of Tennessee where I too had first commenced graduate work. These common threads of experience established for me a relationship that I shared with no other professor. We shared other interests, including an appreciation of the gloomy poetry of Thomas Hardy and the fun of the clever off-color joke.

Of course I was a one-way beneficiary of this relationship. Knight was the advisor who told me not to waste my time taking formal courses in philosophy, who corrected my dissertation grammar in great detail, and who became the role model that has never been replaced or even slightly dislodged over a long academic career. In trying to assess my own development, I find it impossible to imagine what I might have been and become without exposure to Frank Knight.

Let me return to confidence, lest I digress too much. Both T. W. Schultz and Earl J. Hamilton deserve inclusion in this narrative account. Schultz encouraged students by his expressed willingness to locate potential merit in arguments that must have often approached the absurd. I was never a formal student of Earl Hamilton. I did not enroll in his economic history courses at Chicago. Nonetheless during my last year at Chicago, 1948, Hamilton sought me out and took a direct personal interest in my prospects. As with Knight, the sharing of common experience in rural poverty created a personal bond, supplemented in this case by a passion for baseball, reflected by trips to both Cubs and White Sox home games. Hamilton enjoyed giving advice to those he singled out for possible achievement, and with me two separate imperatives stand out in recall: the potential payoff to hard work and the value of mastery of foreign languages.

Perhaps Earl J. Hamilton's most important influence on my career came after 1948, during his tenure as editor of the *Journal of Political Economy*. First of all he forced me to follow up on his recommendation about language skills by sending me French, German, and Italian books for review. Second, he handled my early article submissions with tolerance, understanding, and encouragement rather than with brutal or carping rejections that might have proved fatal to further effort. Hamilton was indeed a tough editor, and every article that I finally published during his tenure was laboriously transformed and dramatically pared down through a process of multiple revisions and resubmissions. Without Hamilton as an editor who cared, my writing style would never have attained the economy it possesses, and my willingness to venture into subject matter beyond the boundaries of the orthodox might have been squelched early. With Earl J. Hamilton as editor, by the mid-1950s I had several solid papers on the record—a number sufficient to enable me to accept the occasional rejection slip with equanimity rather than despair.

I noted earlier how Friedman's analytic brilliance exerted a negating effect on those he instructed. An event occurred early in my post-Chicago years that tended to erase this negative influence by placing Milton

Friedman too among the ranks of those who take intellectual tumbles. A relatively obscure scholar, Cecil G. Phipps, of the University of Florida, located and exposed a logical error in one of Friedman's papers,[6] an error that Friedman graciously acknowledged.[7] To this day I have never told Milton how this simple event contributed so massively to my self-confidence.

The Relatively Absolute Absolute

I have already discussed how Frank Knight's willingness to challenge all authority—intellectual, moral, or scientific—indirectly established confidence in those for whom he served as role model. Any account of such an influence would be seriously incomplete, and indeed erroneous, if the philosophical stance suggested is one of relativism-cum-nihilism against the claim of any and all authority. It is at precisely this point that Frank Knight directly taught me the philosophical principle that has served me so well over so many years and in so many applications. This principle is that of the *relatively absolute absolute,* which allows for a philosophical way station between the extremes of absolutism on the one hand and relativism on the other, both of which are to be rejected.

Acceptance of this principle necessarily requires that there exist a continuing tension between the forces that dictate adherence to and acceptance of authority and those very qualities that define freedom of thought and inquiry. Knight's expressed willingness to challenge all authority was embedded within a wisdom that also recognized the relevance of tradition in ideas, manners, and institutions. This wisdom dictates that for most purposes and most of the time prudent behavior consists of acting as if the authority that exists does indeed possess legitimacy. The principle of the relatively absolute absolute requires that we adhere to and accept the standards of established or conventional authority in our ordinary behavior, whether this be personal, scientific, or political, while at the same time and at still another (and "higher") level of consciousness we call all such standards into question, even to the extent of proposing change.

In relation to my own work this principle of the relatively absolute absolute is perhaps best exemplified in the critically important distinction between the postconstitutional and the constitutional levels of political interaction. More generally, the distinction is that between choosing among strategies of play in a game that is defined by a set of rules and choosing among alternative sets of rules. To the chooser of strategies under defined rules, the rules themselves are to be treated as relatively absolute absolutes, as constraints that are a part of the existential reality but at the same time may be subject to evaluation, modification, and change. In this extension and application of the Knightian principle to the political constitution—and particularly by way of analogy with the choices of strategies and rules of ordinary games—I was stimulated and encouraged by my colleague at the University of Virginia, Rutledge Vining, who had also been strongly influenced by the teachings of Frank Knight.

Why "Better Than Plowing"?

As noted, in 1986 I wrote an autobiographical essay called "Better Than Plowing," a title I borrowed directly from Frank Knight, who used it to describe his own attitude toward a career in the academy. To me the title seemed also descriptive, and it does, I think, convey my sense of comparative evaluation between "employment" in the academy and in the economy beyond. This title also suggests, even if somewhat vaguely, the sheer luck of those of us who served in the academy during the years of the baby-boom educational explosion, luck that was translated into rents of magnitudes beyond imaginable dreams.

To my surprise constructive critics have challenged the appropriateness of the "Better Than Plowing" title for my more general autobiographical essay. To these critics this title seems too casual, too much a throwaway phrase, too flippant a description of a research career that, objectively and externally considered, seems to have embodied central purpose or intent. This unexpected invitation to write a second autobiographical essay provides me with an opportunity to respond to these critics and at the same time to offer additional insights into my development as an economist.

The many books and papers that I have written and published between 1949 and 1987 make up an objective reality that is "there" for all to read and to interpret as they choose. These words and pages exist in some space analogous to the Popperian third world. There is a surprising coherence in this record that I can recognize as well or better than any interpretative critical biographer. As Robert Tollison and I have suggested in our analysis of autobiography,[8] the autobiographer possesses a record over and beyond that which is potentially available to any biographer. The person whose acts created the objective record lives with the subject record itself. And such a person, as autobiographer, would be immoral if he relied on the objective record to impute to his life's work a purpose-oriented coherence that had never emerged into consciousness.

I recognize of course that my own research-publication record may be interpreted as the output of a methodological and normative individualist whose underlying purpose has always been to further philosophical support for individual liberty. In subjective recall, however, this motivational thrust has never informed my conscious work effort. I have throughout my career and with only a few exceptions sought to clarify ambiguities and confusions and clear up neglected pockets of analysis in the received arguments of fellow economists, social scientists, and philosophers. To the extent that conscious motivation has entered these efforts, it has always been the sheer enjoyment of working out ideas, of creating the reality that is reflected finally in the finished manuscript. Proof of my normative disinterest lies in my failure to be interested in what happens once a manuscript is a finished draft—a failure that accounts for my sometimes inattention to choice of publisher, to promotional details, and to the potential for either earnings or influence.

I look on myself as being much closer in spirit to the artist who creates on canvas or stone than to the scientist who discovers only that which he accepts to exist independently of his actions. And I should reject, and categorically, any affinity with the preacher who writes or speaks for the express and only purpose of persuading others to accept his prechosen set of values.

In all of this, once again, Frank Knight has served as my role model. His famous criticism of Pigou's road case is exemplary.[9] By introducing property rights, Knight enabled others to see the whole Pigovian analysis in a new light. Something was indeed created in the process. I like to think that perhaps some of my own works on public debt, opportunity cost, earmarked taxes, clubs, ordinary politics, and constitutional rules may have effected comparable shifts in perspective. The fact that these efforts have been commonly characterized by a reductionist thrust embodying an individualist methodology is explained, very simply, by my inability to look at the world through other than an individualist window.

It is as if the artist who has only red paints produces pictures that are only red of hue. Such an artist does not choose to paint red pictures and then, instrumentally, purchase red paints. Instead the artist uses the instruments at hand to do what he can and must do, while enjoying himself immensely in the process. The fact that others are able to secure new insights with the aid of his creations and that this in turn provides artist with a bit of bread—this gratuitous result enables the artist, too, to entitle his autobiographical essay "Better Than Plowing."

Awarded Nobel Prize in 1986. Lecture presented October 28, 1987.

Date of Birth

October 3, 1919

Academic Degrees

B.A. Middle Tennessee State College, 1940

M.S. University of Tennessee, 1941

Ph.D. University of Chicago, 1948

Academic Affiliations

Associate Professor, University of Tennessee, 1948–1950

Professor, University of Tennessee, 1950–1951

Professor, Department of Economics, Florida State University, 1954–1956

Professor, James Wilson Department of Economics, University of Virginia, 1956–1962

Paul G. McIntire Professor of Economics, University of Virginia, 1962–1968

Professor of Economics, University of California, Los Angeles, 1968–1969

University Distinguished Professor, Virginia Polytechnic Institute and State University, 1969–1983

Holbert L. Harris University Professor, George Mason University, 1983–present

Selected Books

Fiscal Theory and Political Economy, 1960

The Calculus of Consent, 1962 (with G. Tullock)

The Limits of Liberty, 1975

The Power to Tax, 1980 (with G. Brennan)

The Reason of Rules, 1985 (with G. Brennan)

Liberty, Market, and State, 1985

Economics: Between Predictive Science and Moral Philosophy, 1987

Explorations into Constitutional Economics, 1989

The Collected Works of James M. Buchanan, Vols. 1–20, 1999–2002

Kenneth J. Arrow

Studying oneself is not the most comfortable of enterprises. One is caught between the desire to show oneself in the best possible light and the fear of claiming more than one's due. I shall endeavor to follow the precept of that eminent seeker after truth, Sherlock Holmes, on perhaps the only occasion on which he was accused of excessive modesty. "'My dear Watson,' said he, 'I cannot agree with those who rank modesty among the virtues. To the logician all things should be seen exactly as they are, and to underestimate one's self is as much a departure from truth as to exaggerate one's own powers'" ("The Greek Interpreter").

An individual examining himself cannot claim omniscience. I cannot really claim to know all the forces impinging on my life, personal or intellectual. Indeed, as will be seen, there are some elements in the development of my ideas and interests that I cannot now reconstruct. On occasion, on rereading an old scholarly paper of mine, I have realized that my mental recollection was in some degree in error. In effect, the speakers in this series are asked to be historians and biographers of themselves; and like all historians and biographers, they can occasionally make mistakes. Recollection can be taken as reliable when it can be checked against the documentary record. Otherwise, it is imperfectly reliable evidence, though of a kind to which the speaker, such as myself, has unique access.

I have always had an interest in the history of economic thought. In the last few years I have been giving a course in this subject. One question I have been facing is the relative importance of different factors in the development of new ideas. One might suppose, for example, that the personal histories and class backgrounds of economists would be important factors. Yet that does not seem to be the case. Among the great economists of the nineteenth century, David Ricardo was a highly successful businessman, a stock-exchange speculator to be exact, while John Stuart Mill was brought up to be an intellectual by an exacting father. Yet their economic theories were very similar indeed. Of course, access to education is important in intellectual development, and far more so today as economics, like all the natural and social sciences, has become a profession. Further, individual talents and interests may well govern which particular aspects of economics are studied and what techniques are used. But there is no evidence that the personality of an economist plays any significant role in the new concepts that he or she introduces to the subject.

I will therefore be brief in sketching my biography. My family, on both sides, were immigrants who arrived in this country about 1900 and settled in New York. My parents were born abroad but came here as infants, so they were effectively first-generation Americans. My father's family was very poor, my mother's hardworking and moderately successful shopkeepers. Both were very intelligent. My mother completed high school, my father college. My father was unusually successful in business when young, and the first ten years of my life were spent in a household that was comfortable and, more important for me, had many good books. Later, my father lost everything in the Great Depression, and we were very poor for about ten years.

I was early regarded as having unusual intellectual capacity. I was an omnivorous reader, and I added to that a desire to systematize my understanding. As a result, history, for example, was not merely a set of dates and colorful stories; I could understand it as a sequence in which one event flowed out of another. This sense of order crystallized during my high-school and college years into a predominant interest in mathematics and mathematical logic.

My primary and secondary schools were on the whole good. When it came to college, my family's poverty constrained me to attend the City College, which was then a completely free college opportunity that New York City had offered since 1847. I was far from the only able student in the same economic position, so that the quality of the students was high. The faculty, which was generally competent and occasionally better, was stimulated to hold the students to high standards, and I learned a good deal. Fear of unemployment led me to supplement my abstract interests in mathematics and logic with preparation for several alternative practical pursuits, among them high-school teaching, actuarial work, and statistics. It was the study of statistics that turned out to shape my economics career in a decisive way.

By following obscure references in footnotes, I learned about the then rapidly developing field of mathematical statistics, which gave a theoretical foundation to statistical practice and led to profound changes in it. When at college graduation, in 1940, I found there were still no jobs available in high-school teaching, I decided to enter graduate studies in statistics. There were then no separate departments of statistics and few places where mathematical statistics was taught. I enrolled at Columbia University to study with a great statistician, Harold Hotelling. Hotelling had his official position in the department of economics and had written a small number of important papers in economic theory. When I took his course in mathematical economics, I realized I had found my niche.

I received strong moral support from Hotelling and indeed from the whole economics department, somewhat surprisingly, as apart from Hotelling, economic theory was not well regarded by them. The emphasis was almost entirely on empirical and institutional analyses. The department's support was expressed in the most tangible and necessary way—good scholarships. As a result I learned economic theory as I had learned so much else, by reading. In my case at least, I believe this self-education was much better than any lectures I could have attended anywhere. The use of mathematics in economics had had a long history, but it was then still confined to a small group. By reading selectively, I could choose my teachers, and I chose them well.

I was an excellent student, but I was doubtful that I was capable of genuine originality. This concern was concentrated on the choice of topic for a Ph.D. dissertation. There were many possibilities for an acceptable dissertation, but I felt that I had to justify the expectations of my teachers and for that matter of myself by doing something out of the ordinary. This responsibility was crushing rather than inspiring. Four years of military service, though interesting in itself, served further to delay my coming to grips with my aspirations. Finally, a series of abortive research ideas, each of which seemed to be more of a distraction than a help, culminated in my first major accomplishment, known as the theory of social choice.

I will go into some detail about the genesis of this contribution of mine because it displays the interaction between the state of economic thinking in general and my own special talents and background. It differs in one significant way from the other areas of my research, which I will discuss later. The question was essentially a new one, on which there had been virtually no previous analysis. The others had been discussed to some extent in the literature, and my role was to bring new analytic methods or new insights. In social choice theory, I was almost completely the creator of the questions as well as some answers.

It had been argued by the more advanced economic theorists that economic behavior in all contexts was essentially rational choice among a limited set of alternatives. The household chooses among collections of different kinds of goods. The collections available to it are those that it can afford to buy at the prevailing prices and with the income available to it. A firm chooses among alternative ways of producing a given output and also chooses among different output levels. To say that choice is rational was interpreted by these theorists, such as Hotelling, John Hicks, and Paul Samuelson, as meaning that the alternatives can be ranked or ordered by the chooser. From any given range of alternatives, say, the technically feasible production processes or the collections of commodities available to a household within its budget limits, the choosing agent selects the highest-ranking alternative available.

To say that alternatives are ordered in preference has a very definite meaning. First, it means that any two alternatives can be compared. The chooser prefers one or the other or possibly is indifferent between them. Second, and this is somewhat more subtle, there is a consistency in the ordering of alternatives. Let us imagine three alternatives labeled A, B, and C. If A is preferred to B and B to C, we would want to insist that A is preferred to C. This property is referred to as *transitivity*.

Though this formulation of choice had been originated for use in economic analysis, it was clearly applicable to choice in other domains. Hotelling, John von Neumann, Oskar Morgenstern, and Joseph Schumpeter had already suggested some applications to political choice, the choice of candidates for election or of legislative proposals. Voting could be regarded as a method by which individuals' preferences for candidates or legislative proposals could be combined or aggregated to make a social choice.

The question first came to me in an economic context. I had observed that large corporations were not individuals but were supposed (in theory, at least) to reflect the will of their many stockholders. To be sure, they all had a common aim, to maximize profits. But profits depend on the future, and the stockholders might well have different expectations as to future conditions. Suppose the corporation has to choose among alternative directions for investment. Each stockholder orders the different investment policies by the profit he or she expects. But because different stockholders have different expectations, they may well have different orderings of investment policies. My first thought was the obvious one suggested by the formal rules of corporate voting. If there are two investment policies, call them A and B, that one chosen is the one that commands a majority of the shares.

But in almost any real case, there are many more than two possible investment policies. For simplicity, suppose there are three, A, B, and C. The idea that seemed natural to me was to choose the one that would get a majority over each of the other two. To put it another way, since the policy is that of the corporation, we might want to say that the corporation can order all investment policies and choose the best. But since the corporation merely reflects its stockholders, the ordering by the corporation should be constructed from the orderings of the individual stockholders. We might say that the corporation prefers one policy to another if a majority of the shares are voted for the first as against the second.

But now I found an unpleasant surprise. It was perfectly possible that A has a majority against B, and B against C, but that C has a majority against A, not A against C. In other words, majority voting does not always have the property that I have just called transitivity.

To see how this can happen, let me take an election example. Suppose there are three candidates, Adams, Black, and Clark, and three voters. Voter 1 prefers Adams to Black and Black to Clark. We will suppose that each voter has a transitive ordering, so voter 1 will prefer Adams to Clark. Suppose voter 2 prefers Black to Clark and Clark to Adams, and therefore Black to Adams, while voter 3 prefers Clark to Adams and Adams to

Black. Then voters 1 and 3 prefer Adams to Black, so that Adams is chosen over Black by the group. Similarly, Black receives a majority over Clark through voters 1 and 2. Transitivity would require that Adams be chosen over Clark in the election. But in fact voters 2 and 3 prefer Clark to Adams. This intransitivity is sometimes called the paradox of voting. Of course, the intransitivity need not arise; it depends on what the voters' preferences are. The point is that the system of pairwise majority voting cannot be guaranteed to produce an ordering by society as a whole.

The observation struck me as one that must have been made by others, and indeed I wondered if I had heard it somewhere. I still don't know whether I did or not. In any case the effect was rather to cause me to drop the whole matter and study something else.

About a year later my thoughts recurred to the question of voting, without any intention on my part. I realized that under certain special but not totally unnatural conditions on the voters' preferences, the paradox I had found earlier could not occur. This I thought worth writing about. But when I started to do so, I picked up a journal and found the same idea in an article by an English economist, Duncan Black. The result that Black and I had found could have been thought of any time in the last one hundred and fifty years. That two of us came to it at virtually the same time is an occurrence for which I have no explanation.

Priority in discovery is the spur to science, and being anticipated was correspondingly frustrating. I again dropped the study of voting for what I took to be less fascinating but more significant topics, on which I made little progress. But a few months later I was asked a chance question that gave the problem sufficient significance to justify a reawakened interest. The then new theory of games was being applied to military and diplomatic conflict. In this application, nations were being regarded as rational actors. How could this be justified when nations are aggregated of individuals with different preference orderings? My earlier results, I realized, taught me that one could not always derive a preference ordering for a nation from the preference orderings of its citizens by using majority voting to compare one alternative with another.

This left open the possibility that there were other ways of aggregating individual preference orderings to form a social ordering, that is, a way of choosing among alternatives that has the property of transitivity. A few weeks of intensive thought made the answer clear.

Given any method of aggregating individual preference orderings to yield a social choice that satisfies a few very natural conditions, there will always be some individual preference orderings that will cause the social choice to be intransitive, as in the example given.

My studies in logic helped to formulate the question in a clear way, which stripped it of unnecessary complications. But I did not use the concepts of mathematical logic in any deep way.

This result quickly attracted attention. One by-product was that I learned from several correspondents of what previous literature there was. The paradox of majority voting had indeed been discovered before—in fact, by the French author the Marquis du Condorcet in *1785*! But there was not a continuous literature. There were some ingenious unpublished proposals for conducting certain elections at Oxford about 1860, based on the possibility of paradox. They were circulated by a mathematician named Charles L. Dodgson. Dodgson also wrote an adventure tale for the daughter of one of his colleagues, Alice Lidell, which he published under a pseudonym, Lewis Carroll. The only significant published paper on social choice had appeared in 1882 in an Australian journal, hardly everyday reading matter. I know few if any interesting research topics that have had such a spotty and intermittent history.

The subsequent record is very different. The literature has exploded. A recent survey, not intended to be complete, listed more than six hundred references. A journal devoted entirely to social choice theory and related issues has been started.

Social choice is a topic in which there was little direct relation to past work, although the connection with parallel developments in the theory of economic choice was important. I would like to discuss two further contributions of mine, which illustrate different relations to current economic theory and to the world of economic reality.

The first of the two is the study of what is known as general equilibrium theory. This is an elaboration of the simple but not easily understood point that in an economic system everything affects everything else. Let me illustrate. The price of oil became very low in the 1930s because of discoveries in Texas and the Persian Gulf area. Homeowners shifted in great numbers from coal to oil for home heating, thereby decreasing the demand for coal and employment in the coal mines. Refineries expanded, so more workers were employed there. There was as well a demand for refinery equipment, a complicated example of chemical processes. This in turn induced demands for skilled chemical engineers and for more steel. Gasoline was cheaper, so that more automobiles were bought and used. Tourist areas accessible by road but not by railroad began to flourish, while railroads decayed. Each of these changes in turn induced other changes, and some of these in turn reacted on the demand for and supply of oil.

The economic lesson of this story is that the demand for any one product depends on the prices of all products, including the prices of labor and capital services, which we usually call wages and profits. Similarly, the supply of any product or of labor or capital depends on the prices of all commodities. What determines what prices will prevail? The usual hypothesis in economics is that of equilibrium. The prices are those that cause supply to equal demand in every market. This hypothesis, like many others in economics and indeed in the natural sciences, is certainly not precisely true. But it is a useful approximation, and those who disregard it completely are much further from the truth than are those who exaggerate the prevalence of equilibrium.

The general equilibrium theory, or perhaps vision, of the economy was first stated in full-fledged form by a French economist, Leon Walras, in 1874. But it was hard to use as a tool of analysis and too difficult for economists with little mathematical training to understand. Only in the 1930s did interest revive, especially through the masterful exposition and development by John Hicks, with whom I had the honor of sharing the Nobel Prize in 1972.

But there was an unresolved analytic issue, recognized by at least some. General equilibrium theory asserts that the prices of all commodities are determined as the solution of a large number of equations, those that state the equality of supply and demand on each market. Did these equations necessarily have a solution at all? If not, the general equilibrium theory could not always be true. Indeed, some work by German economists about 1932 suggested the possibility that the equations need not have a meaningful solution. A Viennese banker named Karl Schlesinger, who had studied economics in the university and continued to follow developments in the subject, recognized that the apparent difficulties rested on a subtle misunderstanding and felt that the existence of general equilibrium could be demonstrated. He hired a young mathematician, Abraham Wald, to work on the problem. Wald came up with a proof of existence under certain conditions not easy to interpret; indeed, in light of later work, they were much too stringent. Even so, the proof was difficult.

The heavy tread of history breaks in on the story. Schlesinger would not believe that Austria could fall to Hitler; when it did, he committed suicide. Wald did succeed in leaving and came to the United States, where he shifted his interests to mathematical statistics. He was one of my teachers at Columbia. I came to learn, I do not know how, of the unsolved or only partially solved problem of the existence of general equilibrium. But when I asked Wald about his work on the question, he merely said that it was a very difficult problem. Coming from him, whose mathematical powers were certainly greater than mine, the statement was discouraging.

As frequently happens in the history of science, however, help came from developments in other fields. The theory of games was in a process of rapid development. One theorem, proved by a mathematician named John Nash, struck me as being parallel in many ways to the existence problem for competitive equlibrium. By borrowing and adapting the mathematical tools used by Nash, I was able to state very generally the conditions under which the equations defining general equilibrium had a solution.

There was more than mathematics involved, though. It was necessary to state the general equilibrium system much more explicitly. As Schlesinger had already shown in part, the exact assumptions that were made needed clarification, and much was learned in the process.

As you may see from this account, the existence proof was based on general theoretical progress in economics and in mathematics, and I was certainly not the only one with access to it. Indeed, while writing up my results, I learned that Gerard Debreu, the Nobel laureate in economic science for 1983, had independently come to essentially the same results. We decided to publish the results jointly. Just before our paper appeared, there was one by a third economist, Lionel McKenzie, along similar though not identical lines.

Multiple discoveries are in fact very common in science and for much the same reason. Developments in related fields with different motivation help one to understand a difficult problem better. Since these developments are public knowledge, many scholars can take advantage of them.

It is pleasant to the ego to be first or among the first with a new discovery. However, in this case at least, the evidence is clear that the development of general equilibrium theory would have gone on quite as it did without me.

I may add that, despite their abstract and mathematical sound, existence theorems in general equlibrium theory have turned out to be very useful. They certainly stimulated many more applications of general equilibrium theory to particular economic problems. They gave a greater understanding of what may be termed "general equilibrium thinking," that is, recognizing that a particular economic change will have remote repercussions that may be more significant than the initial change. More directly, Herbert Scarf showed that the method of proof could be adapted to find a way of actually calculating the solutions to general equilibrium systems. The method has been used to study a variety of policy problems: tariffs, corporate income taxation, changes in welfare measures, and economic development in a number of developing countries.

The third contribution I would like to discuss is drawing the economic implications of differences in information among economic agents. My sustained interest arose from considering a practical problem, the organization of medical care, but the ground had been prepared by my studies in mathematical statistics, some of my earlier theoretical work on the economics of risk bearing, and some developments by others of these topics. My contribution here, unlike the first two examples, has been not so much a specific and well-defined technical accomplishment as a point of view that has served to reorient economic theory.

The general equilibrium theory, like most economic theory up to about 1950, assumed that the economic agents operated under certainty. That is, the households, firms, investors, and so forth knew correctly the consequences of their actions or, in some versions, at least acted as if they did. Thus, producers were assumed to know what outputs they would get for given inputs. Investors would know what prices would prevail in the future for the goods they were planning to sell.

I don't mean to imply that economists were so foolish as not to recognize that the economic world was uncertain or that economic agents didn't realize that this was the case. Indeed, some literature clearly showed that much economic behavior could only be explained by assuming that economic agents were well aware of uncertainty; for example, investors held diversified portfolios and bought insurance. However, a general formulation that would permit integration with standard economic theory and in particular with general equilibrium theory was lacking. I was able to work out such a formulation, which introduced the concept of contingent contracts, contracts for delivery of goods or money contingent on the occurrence of any possible state of affairs. In effect, I postulated the existence of insurance against all conceivable risks. My rather sketchy paper was greatly enriched and extended by Gerard Debreu. The idea was simplicity itself and yet novel.

It has become a standard tool of analysis, in this case rather more than I intended. I considered the theory of contingent contracts as a sketch of an ideal system to which the methods of risk bearing and risk shifting in the real world were to be compared. It was clear enough empirically that the world did not have nearly as many possibilities for trading risks as my model would have predicted. I did not, however, have at first a particularly good explanation for the discrepancy.

A considerable insight came a few years later. I was asked by the Ford Foundation to take a theorist's view of the economics of medical care. I first surveyed the empirical literature on the subject. My theoretical perspective suggested that there was inadequate insurance against the very large financial risks. Indeed, insurance coverage, both governmental and private, has expanded greatly since then. But I soon realized that there were obstacles to the achievement of full insurance. Insurance against health expenditures creates an incentive to spend more freely than is desirable.

Was there a general theoretical principle behind this? The concept of insurance against uncertainties did not fully reflect the actual situation, namely, that different individuals may have different uncertainties. The person insured knows more about his or her state of health than the insurer. The fact that individuals have informational differences is a key element in any economic system, not just in health insurance.

To take a very different example, consider tenant farming. If the landlord hired someone to work his or her farm, the farm worker would have limited incentives to work to full capacity, since the worker's income is assured. The owner could indeed direct the worker if fully informed about what the worker was doing. But his information can be obtained only by costly supervision. In its absence, the two parties will have different information, and production will be inefficient. The other extreme alternative is to rent the farm at a fixed fee. Then indeed incentives to the worker (or, in this case, tenant) are very strong. But farming is a risky business, and poor farmers at least may not be able to bear the uncertainty. Hence, the compromise of sharecropping arose. It dulls incentives, but not completely, and it shares risks, but not completely. Similarly, most health insurance policies have a coinsurance feature, so that risks are partially shared, while the patient still has some incentive to economize.

The theme may be stated without elaboration. Informational differences pervade the economy and have given rise to both inefficiencies and contractual arrangements and informal understandings to protect the less informed. My own contributions here were conceptual rather than technical, and the present theory is the result of many hands.

I have tried to present, as clearly as I can, the genesis of some of my researches. They have all been related to the present state of thinking by others. The field of science, indeed, the whole world of human society, is a cooperative one. At each moment, we are competing, whether for academic honors or business success. But the background, and what makes society an engine of progress, is a whole set of successes and even failures from which we all have learned.

Awarded Nobel Prize in 1972. Lecture presented November 5, 1984.

Date of Birth

August 23, 1921

Academic Degrees

B.S. City College, New York, 1940

M.A. Columbia University, 1941

Ph.D. Columbia University, 1951

Academic Affiliations

Assistant Professor of Economics, University of Chicago, 1948–1949

Acting Assistant Professor of Economics and Statistics, Stanford University, 1949–1950

Associate Professor of Economics and Statistics, Stanford University, 1950–1953

Executive Head, Department of Economics, Stanford University, 1953–1956

Professor of Economics, Statistics, and Operations Research, Stanford University, 1953–1968

Visiting Professor of Economics, Massachusetts Institute of Technology, fall 1966

Fellow, Churchill College (Cambridge), 1963–1964, 1970, 1973

Professor of Economics, Harvard University, 1968–1974

James Bryant Conant University Professor, Harvard University, 1974–1979

Joan Kenney Professor of Economics and Professor of Operations Research, Stanford University, 1979–1991

Joan Kenney Professor of Economics and Professor of Operations Research, Emeritus, Stanford University, 1991–present

Selected Books

Social Choice and Individual Values, 1951, 1963

Studies in the Mathematical Theory of Inventory and Production, 1958 (with S. Karlin and H. Scarf)

Public Investment, the Rate of Return, and Optimal Fiscal Policy, 1970 (with M. Kurz)

Essays in the Theory of Risk-Bearing, 1971

The Limits of Organization, 1974

Collected Papers of Kenneth J. Arrow, Volumes 1–6, 1983–1985

Ronald H. Coase

After accepting Professor Breit's invitation to give a lecture in the series, "Lives of the Laureates," I read the book containing the previous lectures and found that the subject of my lecture was to be "My Evolution as an Economist." This led me to consider in what ways my ideas can be said to have evolved. The notion of an evolution in someone's ideas suggests a move from the simpler and cruder to something more complicated and more refined, brought about by a thought process which gradually improves the analysis. Lars Werin, speaking for the Royal Swedish Academy of Sciences, in introducing me at the Nobel Prize award ceremony, after referring to my article, "The Nature of the Firm," published in 1937, in which I explained, as I thought, why firms exist, said that I "gradually added blocks to [my] theoretical construction and had eventually—in the early 1960s—set forth the principles for answering all the questions," that is, the *principles* for answering all the questions relating to the institutional structure of the economic system. His statement about the final result is, I believe, substantially correct. But if his words are interpreted to mean that I started with a relatively simple theory and gradually, purposefully added building blocks until I had accumulated all that were needed to construct a theory of the institutional structure, it would give a misleading view of the development of my ideas. I never had a clear goal until quite recently. I came to realize where I had been going only after I arrived. The emergence of my ideas at each stage was not part of some grand scheme. In the end I found myself with a collection of blocks which, by some miracle, fit together to form, not a complete theory, but, as Lars Werin indicated, the foundation for such a theory.

The development of my ideas seems to me to have been more like a biological evolution in which the changes are brought about by chance events. How all this happened will be the subject of this lecture. It will, I think, throw some light on what Professor Breit calls the major rationale for this lecture series, learning about "the process by which original ideas are germinated and eventually accepted by one's peers." But if the occasion for the emergence of my ideas was provided by chance events, my response to them was no doubt influenced by the spirit of the age. Virginia Woolf has asserted that "on or about December 1910 human character changed" leading to "a change in religion, conduct, politics and literature."[1] If it is true that this date marks a turning point in human affairs, one would hardly expect that my approach in economics would be exactly the same as that of those who preceded me.

As you will by now have guessed, I was born in December, 1910. To be precise, I was born on December 29th at 3:25 P.M. The place was Willesden, a suburb of London. I was to be the only child of my parents. My father was a telegraphist in the post office where my mother had also been employed until her marriage. Although both my parents had left school at the age of 12, they were completely literate. However, they had no understanding of, or interest in, academic scholarship. My interests were always academic. But I grew up with no idea of what scholarship involved, had no guidance in my reading, and was unable to distinguish the charlatan from the serious scholar. But in two respects I am greatly indebted to my parents. They may not have shared my interests but they always supported me in what I wanted to do. And my mother taught me to be honest and truthful. Frank Knight has said: "The basic principle of science—truth or objectivity—is essentially a moral principle."[2] My endeavors to follow my mother's precepts have, I believe, been important in my work. My aim has always been to understand the working of the economic system, to get to the truth, rather than to support some position. And in criticizing others, I have always tried to understand what their position was and not to misrepresent it. I have never been interested in cheap victories.

When young I had a weakness in my legs which led to my wearing irons. I went to the local school for physical defectives. It was run by the same department of the council that ran the school for mental defectives, and I suspect that there may have been an overlap in the curriculum. I have no clear recollection of what I was taught there. All I can now remember is having been taught, at one stage, basket-weaving, a useful skill that I am afraid I failed to master.

I missed taking the entrance examination for the local secondary school at the usual age of 11 (perhaps because I was at the school for physical defectives). But, through the efforts of my parents, I was allowed to take the examination at the age of 12, as a result of which I was awarded a scholarship to go to the local secondary school, the Kilburn Grammar School. The teaching there was good, and I received a solid education in the usual school subjects. I passed the matriculation examination in 1927, with distinction in history and chemistry. It was then possible to spend the next two years at the Kilburn Grammar School studying for the intermediate examination of the University of London. This covered the subjects that would have been taken during the first year at the University. I had to decide what degree to take. My inclination had been to take a degree in history but I found that to do this, at least for the degree I wanted to take, it was necessary to know Latin, and having arrived at Kilburn Grammar School one year later than usual, boys of my age who had chosen to study Latin had already done so for a year. I had therefore been assigned to the science side of the school. This meant that I would not be able to take a degree in history. So I turned to the other subject in which I had secured distinction and started to study for a science degree, specializing in chemistry. However, I found I did not like mathematics, essential for a science degree, and decided to switch to the only other degree for which it was possible to study at the Kilburn Grammar School, one in commerce. Thinking back over this episode, I have concluded that the reason I disliked mathematics was that we learned formulas and mathematical operations without understanding the sense of what we were doing. Had I come across Silvanus Thompson's *Calculus Made Easy,* which explained the sense of these mathematical operations, or if the teaching at the Kilburn Grammar School had adopted a similar approach, it is very likely that I would have continued with my science degree. It is good that I did not as I would have made a mediocre mathematician and would never have become a first rate scientist. As it was, I studied at school for the intermediate examination of the Bachelor of

Commerce of the University of London (apart from accounting, not taught at the Kilburn Grammar School and which I studied by means of a correspondence course). Although I had only a rudimentary knowledge of the subjects, I managed to pass the examinations. And in 1929, at the age of eighteen, I went to the London School of Economics (LSE) to continue my studies for a B.Com. I passed part I of the final examination in 1930. For part II, I decided to take the Industry Group, supposedly intended for those who wanted to be works managers, but what universities say about their courses is not always to be taken seriously. However, although I could not have known this, I had made a fateful decision, one that would change my whole life.

Arnold Plant was appointed Professor of Commerce (with special reference to business administration) at the London School of Economics in 1930, having held a similar position at the University of Cape Town in South Africa. He took charge of the Industry Group. I therefore studied for the Industry Group in the very year that Plant took it over. In 1931, some five months before I completed my studies, I attended Plant's seminar. It was a revelation. He introduced me to Adam Smith's "invisible hand." You should remember that I had not taken a course in economics at LSE although some of the courses had economic content. The result was that my notions on economics were extremely wooly. What Plant did was to make me aware that producers compete, with the result that they supply what consumers value most. He explained that the economic system was coordinated by the pricing system. I was a socialist at the time, and all this was news to me. I passed the B.Com., part II, final examination in 1931. However, as I had taken the first year of university work while still at the Kilburn Grammar School and three years of residence at LSE were required before a degree could be granted, I had to decide what to do during this third year. The course that I had found most interesting in my studies for part II was industrial law, and my tentative decision was to use this third year to study for the B.Sc. (economics) degree, specializing in industrial law. Had I done so, I would undoubtedly have ended up as a lawyer. But this was not to be. No doubt as a result of Plant's influence, I was awarded a Sir Ernest Cassel Traveling Scholarship by the University of London for the year 1931–32. I was to work under the direction of Plant, and the year would be counted as a year's residence at LSE. This is how it happened that I took the road that would lead to my becoming an economist and *not* a basket-weaver, a historian, a chemist, a works manager, or a lawyer. "There is a divinity that shapes our ends, rough hew them though we may."

When I had completed my studies for the B.Com. degree, I knew a little about accounting, statistics, and law. Although I had never taken a course in economics at LSE, I had also picked up a little economics. Acting on hints in Plant's seminar, I had discussed economic problems with my friend Ronald Fowler, who was also taking the Industry Group. And LSE was a relatively small institution at that time. I knew students who were economics specialists and had discussions with them, particularly with Vera Smith (later Vera Lutz), Abba Lerner, and Victor Edelberg. That I had come to economics without any formal training was to prove a great advantage. I had never been trained what to think and therefore what not to think, and this gave me a lot of freedom in dealing with economic questions.

I proposed to use my Cassel Traveling Scholarship to go to the United States and to study vertical and lateral integration in industry. Plant had discussed in his lectures the various ways in which industries were organized, but we seemed to lack any theory that would explain why there were these differences. I set out to find this theory. There were two other problems that seemed in my mind to be connected to my main project. Plant had spoken in his seminar about the economic system being coordinated by the pricing system and had been critical of government schemes for the rationalization of industry—particularly those for coordinating the various means of transport. And yet, in his lectures on business administration, Plant spoke of management as coordinating the factors of production used in a firm. How could these two views be reconciled? Why did we need management if all the coordination necessary was already provided by the market? What was essentially the same puzzle presented itself to me in another form. The Russian Revolution had taken place in 1917. But we knew very little about how a communist system would operate. How could we? The first five-year plan was not adopted until 1928. Lenin had said that under communism the economic system would be run as one big factory. Some western economists were arguing that this could not be done. Yet there were factories in the western world and some of them were very large. Why couldn't the Russian economy be run as one big factory?

These were the puzzles with which I went to the United States. I visited universities but in the main I carried out my project by visiting businesses and industrial plants. I talked with everyone I met and read trade periodicals and the reports of the Federal Trade Commission. At the end of the year there was much about the organization of industry that I felt I did not understand. But I believed that I had solved part of the puzzle. Economists talked about the economic system as being coordinated by the pricing mechanism (or the market) but had ignored the fact that using the market involved costs. From this it followed that means of coordination other than through use of the market could not be ruled out as inefficient—it all depended on what they cost as compared with the cost of using the market. I realized that this way of looking at things could affect one's views on centralized planning. But, and this was what really mattered to me, it also meant that we could understand why there were firms in which the employment of the factors of production was coordinated by the management of the firm while at the same time there was also coordination conducted through the market. Whether a transaction would be organized within a firm or whether it would be carried out on the market depended on a comparison of the costs of organizing such a transaction within the firm with the costs of a market transaction that would accomplish the same result. All this is very simple and obvious. But it took me a year to realize it—and many economists seem unaware of it (or its significance) to this day.

It was an extraordinary piece of luck that the last year of my studies for the B.Com. coincided with Arnold Plant's first year at LSE. It was another piece of luck that the next year I was able to secure a Cassel Traveling Scholarship. This was followed by a piece of luck even more extraordinary. I came on to the labor market in 1932, the worst year of the Great Depression. Unemployment was rife among LSE graduates. And yet I secured employment. It came about this way: in 1931, with the financial support of George Bonar, a prominent member of the jute industry, there had been established, with the advice of Sir William Beveridge and others at LSE, a School of Economics and Commerce in Dundee, to be administered by the Dundee Educational Authority. The purpose of the school was to train students for business. The senior appointments were made in 1931. The junior appointments were made in 1932, just when I needed a job. It is easy to see in retrospect that my qualifications, meager though they were, would have seemed more appropriate for this position than those of most graduates in economics. I was appointed an assistant lecturer at the Dundee School of Economics and Commerce in October, 1932. If the Dundee School had not been established in 1931, I don't know what I would have done. As it was, everything fell into place. I was to be an economist and could evolve.

My duties involved lecturing in three courses all of which started in October. How I did it I can't now imagine. Duncan Black, the other assistant lecturer, has described how I arrived in Dundee with my head full of my ideas on the firm. Fortunately, one of the courses was on "The Organization of the Business Unit." In a letter to my friend Ronald Fowler that has been preserved, I described the contents of my first lecture in that course. It was essentially the argument that was later to be published as "The Nature of the Firm" (one of the two articles cited by the Royal Swedish Academy of Sciences in 1991 as justification for the award of the Nobel Prize). I could never have imagined in 1932 that these ideas would come to be regarded as so significant. Of course, I liked the lecture. In my letter to Fowler, after describing the contents of the lecture, I expressed my great satisfaction with it: "As it was a new approach (I think) to this subject, I was quite pleased with myself. One thing I can say is that I made it all up myself." As I said in my Nobel Prize lecture, "I was then twenty-one and the sun never ceased to shine."

At Dundee I began to read the literature of economics—Adam Smith, Babbage, Jevons, Wicksteed, Knight. Writing of my days at Dundee, Duncan Black, in notes prepared for Kenneth Elzinga in connection with the article he was writing about me for the *International Encyclopedia of the Social Sciences,* commented that at this early date my attitude was "surprisingly definite." "He wanted an Economics that would both deal with the real world and do so in an exact manner. Most economists are content to achieve one or the other of these objectives and to my mind the distinguishing mark of Coase's work in Economics is that, in a fair measure, it achieves both objectives." Whether I succeeded or not, Black does describe what my aim in economics was, and is. I ascribe it to the fact that I started not with an academic study of economics but with an education in commerce and that when I began to study economics it was with a view to using it to understand what happened in the real world.

However, I was not immune to what was happening in the economics world. In 1933 Chamberlin's *Theory of Monopolistic Competition* and Joan Robinson's *Economics of Imperfect Competition* were published. These books created a great stir in economics, and I was swept up along with the others. While still at Dundee I wrote an article in which I used the analysis developed by Joan Robinson to examine the problems discussed by Chamberlin. This article was published in 1935. More illustrative of my general attitude was the work I did on expectations at that time.

While at Dundee I spent my vacations at LSE. Much of my time was taken up with discussions on economics with Ronald Fowler, who had been appointed an assistant lecturer at LSE. One question that interested us was the belief, held by many economists, that producers, in deciding on output, assumed that existing prices and costs would continue in the future. It had been shown that if producers acted in this way, it would result in fluctuations in prices and output (termed the "cobweb theorem" by Kaldor). An example of the cobweb theorem was thought to be provided by the pig-cycle in Britain. We undertook a statistical investigation that showed, as we thought, that pig producers in Britain did not assume that existing prices would continue unchanged in the future. When prices were unusually high they expected them to fall, and when they were unusually low they expected them to rise. As my correspondence shows, I intended to use the techniques we had developed in this study to investigate the formation of producer expectations in other areas—and Fowler had a similar intention. What I then had was a strong interest in measuring the concepts which were usually only treated theoretically by economists. In this I was greatly influenced by the work of Henry Schultz of the University of Chicago in deriving statistical demand schedules. Apart from my work on expectations I also started an investigation of the cost of capital and how it varied with the size of the issue, and the size and industry of the firm. None of this work was completed. Fowler did however complete a study of the elasticity of substitution between scrap and pig iron in the production of steel which was published in the *Quarterly Journal of Economics* in 1937.

At this time my own expectation was that my future research would be to engage in similar quantitative investigations. But this did not happen. It is easy to see why. In 1934 I was appointed an assistant lecturer in the University of Liverpool with the duty of lecturing on banking and finance, subjects on which I knew next to nothing. More important was that in 1935 I was appointed an assistant lecturer in economics at LSE. Here my duties were to lecture on the theory of monopoly (taking over a course that had previously been given by John Hicks who had gone to Cambridge), to assist Plant in the Department of Business Administration (the DBA) and to give the course on the economics of public utilities (previously given by Batson, who had gone to South Africa). The lectures on the theory of monopoly created no particular difficulty for me. We had Joan Robinson's book, and I had written on the theory of duopoly. In 1937, I published an article, "Notes on the Theory of Monopoly," which contained some of the ideas that came from these lectures. My work in the Department of Business Administration was more humdrum. I prepared some cases in the manner of the Harvard Business School and assisted in the teaching.

Ronald Edwards, whose field was accounting, had joined the DBA, and Fowler and I collaborated with him in the work of the Accounting Research Association. Among other things, we investigated how far the figures in the published accounts could be used for economic research. We showed that they could be so used, once the basis for the figures was understood, by publishing a study of the British iron and steel industry using the material in published balance sheets. I also published in *The Accountant* a series of articles on cost accounting, articles which have since been reprinted and much referred to, largely I think because they contain the only systematic account of the opportunity cost concept as it was taught at LSE in the 1930s. My main research activity was, however, in connection with my course on public utilities. I soon found that very little was known about public utilities in Britain, and I made a series of historical studies of the water, gas, and electricity supply industries, and particularly of the post office and broadcasting. Another publication should be noted. In 1934 while still at Dundee, I had written the draft of an article entitled "The Nature of the Firm," a systematic exposition of the ideas in my 1932 lecture. At LSE I revised this draft and submitted it to *Economica,* in which it was published in 1937. It created little interest. I have recounted how, on the day it was published, on the way to lunch the two professors of commerce congratulated me but never referred to the article again. Lionel Robbins, in whose department I was, never referred to the article ever. It was not an instant success.

In September, 1939, war was declared. What I have just described is the work on which I was engaged in the seven years from 1932 to 1939. In 1940 I was appointed head of the Statistical Division of the Forestry Commission (responsible at that time for timber production in the United Kingdom), and in 1941 I moved to the Central Statistical Office, one of the Offices of the War Cabinet. I ended up responsible for munitions statistics, those relating to guns, tanks, and ammunition. I did not return to LSE until 1946. My six years in government service played little part in my evolution as an economist, except perhaps to confirm my prejudices.

On my return to LSE I became responsible for the course on the principles of economics, a conventional exposition of mainstream economics. In 1946 I published an article, "Monopoly Pricing with Interrelated Costs and Demands," based on material in my prewar monopoly course. Another article published the same year, "The Marginal Cost Controversy," should also be mentioned because it illustrates the way in which my approach to economic policy differed from that of most of my contemporaries. Towards the end of the war, the economists in the Economics Section of the Offices of the War Cabinet began to consider the problems of postwar Britain. James Meade and John Fleming, in the Economics Section, wrote a paper on the pricing policies of state enterprises in which they advocated marginal cost pricing. Keynes, who was an advisor to the Treasury, saw the paper, was enthusiastic about it, and reprinted it in the *Economic Journal,* of which he was editor. I also saw the paper as did Tom Wilson (also in the Economics Section), and we did not like it. I published a short critical note in the *Economic Journal,* and after the war I wrote "The Marginal Cost Controversy." I was already familiar with the case for marginal cost pricing before I saw the Meade–Fleming piece. Abba Lerner had been an enthusiastic advocate and an able expositor of marginal cost pricing at LSE, and it was undoubtedly pondering over Lerner's argument that gave me my view. I maintained that a general policy of marginal cost pricing would lead to waste on a massive scale.

It would also bring about a redistribution of income and would lead to taxation that would introduce elsewhere divergencies between price and marginal cost that had not previously existed. Tom Wilson pointed out that the policy would lead to a substitution of state enterprise for private enterprise and of centralized for decentralized operations. What had happened is that, through concentrating on getting the right marginal adjustments, economists (for at that time belief in marginal cost pricing was the dominant view among academic economists) had completely ignored the effects their policy would have in other ways. They fiddled while Rome burned. I have called their way of proceeding "blackboard economics" since what they described could happen only on a blackboard. In the meantime at LSE I had been promoted to become a "reader in economics, with special reference to public utilities." My main research activity was the continuation of my historical studies of British public utilities. In 1950 I published a book, *British Broadcasting: A Study in Monopoly.*

In 1951 I migrated to the United States. What prompted me to take this step was a combination of a lack of faith in the future of socialist Britain, a liking for life in America (I had spent part of 1948 there studying the working of a commercial broadcasting system), and an admiration for American economics. Among the older economists it was Frank Knight that I most admired; among my contemporaries it was George Stigler. And I have already mentioned the influence of Henry Schultz. My first appointment in America was at the University of Buffalo, due to the presence there of John Sumner, a specialist on public utilities, who had visited LSE before the war. In 1958 I joined the faculty of the University of Virginia and in 1964 the faculty of the University of Chicago.

On coming to the United States I decided to make a study of the political economy of broadcasting, based on experience in Britain, Canada, and the United States. This was essentially a continuation of the kind of research I had been conducting at LSE. I collected a great deal of material for this project. I spent the year 1958–59 at the Center for Advanced Study in the Behavioral Sciences at Stanford. While there I wrote an article on "The Federal Communications Commission" which was published in the *Journal of Law and Economics.* This was to have far-reaching consequences.

In that article I examined the work of the Federal Communications Commission (the FCC) in allocating the use of the radio frequency spectrum. I suggested that this should be done by selling the right to use a frequency. The use of pricing for the allocation of resources was hardly a novel idea for an economist (and in any case the suggestion had already been advanced for the radio frequency spectrum by Leo Herzel). What was unusual in my paper was that I went on to discuss the nature of the rights that would be acquired. The main problem in the case of the radio frequency spectrum concerned interference between signals transmitted on the same or adjacent frequencies. I argued that if rights were well-defined and transferable, it did not matter what the initial rights were—they would be transferred and combined so as to bring about the optimal result. As I put it: "The ultimate result (which maximizes the value of production) is independent of the legal [position]."[3] This simple and, as I thought, obvious proposition, was disputed by the economists at the University of Chicago with whom I was in touch. It was even suggested that I should delete this passage from the article. However, I held my ground and later, after the article was published, a meeting was held at the home of Aaron Director at which I was able to convince the Chicago economists that I was right. I was then asked to write up my ideas for publication in the *Journal of Law and Economics.*

I took on this task with enthusiasm. I was a great admirer of what the *Journal of Law and Economics,* under the editorship of Aaron Director, had been accomplishing. In it were being published articles that examined actual business practices, the effects of different property rights systems, and the working of regulatory systems. I considered it essential if economics (and particularly that part called industrial organization) was to make progress, that articles such as these should be published, but they were articles that, at that time, would have found difficulty in being published in the normal economic journals. My article on the FCC was an example. However, I wanted to go beyond the passage in the FCC article to which objections had been made and to deal more generally with what may be termed the rationale of a property rights system. I had discussed the case of *Sturges v. Bridgman* in the FCC article, but I wanted to examine other nuisance cases (something I could do because of the familiarity I had acquired with the Law Reports in my student days at LSE). Also, I had long thought (again from my student days) that although Pigou's *Economics of Welfare* was a great book for the problems it tackled, Pigou was not very sure-footed in his economic analysis. I had made two passing references to Pigou in the FCC article but did not discuss his views since that article was wholly devoted to the problem of the allocation of the use of the radio frequency spectrum. However, my discussions at Chicago had made clear to me the strength of the hold that Pigou's approach had on the economics profession, and this led me to want to deal with it directly. I also wanted to discuss the influence of positive transaction costs on the analysis, something that I had only alluded to in a footnote in the FCC article. These were the various objectives or themes that I wanted to weave together and which I think I managed to do in "The Problem of Social Cost."

This article received considerable attention almost immediately. Articles were written attacking and defending it. It became one of the most cited articles in the economics literature. It contained ideas that I had long held at the back of my mind but had never articulated. It is a curious aspect of this story that had these Chicago economists not objected to the passage in the FCC article, "The Problem of Social Cost" would probably never have been written and these ideas would have remained in the back of my mind.

I wrote the article in the Summer of 1960 at LSE, where I had access to the Law Reports. I argued that Pigou had been looking at the problem of what is termed "externality" in the wrong way. It is a reciprocal problem, and it was Pigou's failure to recognize this (or at any rate to incorporate it in the analysis) that had prevented him (and the economics profession which had followed him) from developing the appropriate analysis. It was also true that Pigou's policy recommendations were unnecessary in a regime of zero transaction costs (which was implicitly his assumption) since in this case negotiations between the parties would bring about the optimal result. However, transaction costs are not zero and real world situations cannot be studied without introducing positive transaction costs. Once this was done, it became impossible to say what the appropriate policy recommendation should be without knowing what the transaction costs were and the factual situation of each case under consideration. What should be done could only be learned as a result of empirical studies. What I did in "The Problem of Social Cost" was to provide not a solution, but an approach. As I said in that article: "Satisfactory views on policy can only come from a patient study of how, in practice, the market, firms, and governments handle the problem of harmful effects. . . . It is my belief that economists and policymakers generally have tended to overestimate the advantages which come from governmental regulation. But this belief, even if justified, does not do more than suggest that governmental regulation should be curtailed. It does not tell us where the boundary line should be drawn. This, it seems to me, has to come from a detailed investigation of the actual results of handling the problem in different ways."[4]

A year or two after the appearance of "The Problem of Social Cost" I received an invitation to join the faculty of the University of Chicago. What attracted me to the position at Chicago was that part of my duties would be to edit the *Journal of Law and Economics.* I have already spoken of my admiration for the *Journal* and the articles it contained. I wanted to continue this work, and I went to Chicago to do it. I greatly enjoyed editing the *Journal.* Using the resources of the law and economics program at the University of Chicago Law School and the opportunity of publication in the *Journal,* I encouraged economists and lawyers (at Chicago and elsewhere) to undertake empirical studies of the kind advocated in "The Problem of Social Cost." As a result, many splendid articles were published. This was a very happy period for me. Every article was an event. In the 1970s and '80s, articles of a similar character began to appear in other journals, and there were many citations to the "Nature of the Firm" as well as to "The Problem of Social Cost." I felt the time had come to bring together my essays on the institutional structure of production and in 1988 published *The Firm, the Market and the Law,* which reprinted my chief articles on this topic. It included an introductory essay which explained my central message.

The next event to be noted as affecting the evolution of my ideas occurred in 1987 when Oliver Williamson and Sidney Winters organized a conference at Yale to celebrate the fiftieth anniversary of the publication of "The Nature of the Firm." This was probably the best conference that I have ever attended. The papers by eight distinguished economists were not designed to praise nor to bury "The Nature of the Firm" but to examine the issues it had raised and to extend, and—where they found error—to correct, what I had said. I contributed three lectures on the origin, meaning, and influence of the article. Attendance at this conference and preparation of my lectures had a great effect on my thinking. Writing "The Problem of Social Cost" and my later discussions with Steven Cheung in the 1960s had made me aware of the pervasive influence of transaction costs on the working of the economy, but I had not examined the problem in a systematic way. When Williamson, in his paper, ascribed the limited use of the thesis of "The Nature of the Firm" to the fact that it had not been made "operational," I had no doubt that he was essentially correct. What he had in mind was that the concept of transaction costs had not been incorporated into a general theory which could be checked and developed by empirical studies. However, this would be no easy task. The incorporation of transaction costs in an economic theory which assumed they were zero would involve a complete change in its structure. Even if we confined ourselves to the thesis of "The Nature of the Firm," narrowly conceived, there were formidable obstacles to making it "operational." Whether the coordination of the factors of production needed to produce a given result will be undertaken administratively within a firm or by means of pricing on the market depends on the relative costs of carrying out the coordination in these different ways, and whether it will be profitable depends on their absolute height. But what are the factors that determine these relative and absolute costs? Discovering them will not be at all easy. But there is an even more difficult problem. The analysis cannot be confined to what happens within a single firm. The costs of coordination within a firm and the level of transaction costs that it faces are affected by its ability to purchase inputs from other firms, and their ability to supply these inputs depends in part on their costs of coordination and the level of transaction costs that they face, which are similarly affected by what these are in still other firms. What we are dealing with is a complex interrelated structure.

The Yale conference rekindled my interest in the issues raised by "The Nature of the Firm" and led me to decide, once my existing commitments were out of the way, to devote myself to helping in the construction of a theory that would enable us to analyze the determinants of the institutional structure of production. For now I was not alone. As the papers at the conference had demonstrated, important work was being undertaken, aimed at the clarification and improvement of the theory while many empirical studies of high quality were being conducted which were providing data on the basis of which further advances could be expected to be made. We were beginning to see what needed to be explained and I felt confident that, although it would take many years of dedicated work by many economists to achieve this goal, we would ultimately be able to construct a comprehensive theory of the institutional structure of production. Although it is obvious that I will be able to travel only part of the way, I decided at Yale that this is what I should do in my few remaining years.

And then, in 1991, I was awarded the Alfred Nobel Memorial Prize in Economics. The two articles cited as justification for the award were "The Nature of the Firm," published over 50 years before and "The Problem of Social Cost," published 30 years before. The first article had been received with indifference, the second provoked controversy. Neither had commanded the assent of the economics profession and if, of which I am not sure, there is now general recognition of the importance of my work, it must have come very recently. Lars Werin at the awards ceremony in Stockholm, after saying that I had "remarkably improved our understanding of the way the economic system functions," added "although it took some time for the rest of us to realize it."

This lecture clearly provides grist to Professor Breit's mill in his quest to understand "the process by which original ideas are germinated and eventually accepted by one's peers." But what has my tale to contribute? It has often been remarked that original ideas commonly come from those who are young and/or have newly entered a field. This certainly fits my case. In 1932, when, in a lecture in Dundee, I introduced the concept of transaction costs into economic analysis, I was 21, and, if economics was my field, I had only just entered it. However, at first sight, it is not easy to understand why the inclusion of transaction costs in economic analysis was an "original" idea. The puzzle I took with me to America was there for all to see, and my solution was simple and obvious. The explanation for this failure to include transaction costs in the analysis is not that other economists were not smart enough but that, in their work, they did not concern themselves with the problems of the institutional structure of the economy and so never encountered my puzzle. This situation came about, as Demsetz has explained, because economists since Adam Smith have taken as a major task to formalize his view that an economic system could be coordinated by the pricing system. What has been produced is a theory of the working of an economic system of extreme decentralization. It has been a towering intellectual achievement and has enduring value, but it is an economics with blinkers and has had the unfortunate effect of diverting attention from some very important features of the economic system. This explains, among other things, why, when it first appeared, "The Nature of the Firm" excited so little interest.

But why did "The Problem of Social Cost" attract so much attention so soon? I have recounted the somewhat peculiar circumstances that led to its writing. This had the result that, when it appeared, it had the strong support of a powerful group of economists at the University of Chicago and especially of George Stigler. My argument that the allocation of resources in a regime of zero transaction costs would be independent of the legal position regarding liability was formalized by Stigler and named by him the "Coase Theorem." This attracted attention to my article, and many papers were published attacking and defending the "theorem." The fact that the "Coase Theorem" dealt with a regime of zero transaction costs was also helpful since this meant that economists felt quite at home discussing it, remote from the real world though it may have been. It does not seem to have been noticed that the "theorem" applies to a world of positive transaction costs for all exchanges that are actually made, providing that the transaction costs are not significantly affected by the change in the legal position regarding liability, which will commonly be the case. Strangely enough, I believe the fact that the discussion was not concerned with the real world of positive transaction costs did not diminish but actually increased the attention given to my article. Another circumstance that led to much discussion in the literature was that I criticized Pigou's analysis (accepted by most economists). As a result many articles were written by economists defending Pigou (and themselves). Another, and quite separate, circumstance was that this article, by discussing the rationale of a property rights system and the effect of the law on the working of the economic system, extended the economic analysis of the law beyond its previous connection with antitrust policy.

The article greatly interested lawyers and economists in American law schools, spawned an immense literature, and led to the emergence of the new subject of "law and economics." All these quite special circumstances combined to make this article an immediate success. But it would be wrong to conclude that for the thesis of an article to gain acceptance it is necessary to have the support of a prestigious group or the stir of controversy or involve some similar circumstance. After all, "The Nature of the Firm," received at first with indifference, has by now had a very considerable influence on the thinking of many economists. Without the kind of factor that affected the reception of "The Problem of Social Cost," it just takes longer for a good idea to secure acceptance. As Edwin Cannan, the teacher of my teacher, Arnold Plant, said: "However lucky Error may be for a time, Truth keeps the book, and wins in the long run."[5]

Given the broad acceptance of my analysis in "The Nature of the Firm" and "The Problem of Social Cost," what is the task ahead? The Nobel Committee said that I had provided the blocks for the construction of a theory of the institutional structure. We now have to discover how they fit together so that we can construct it. I hope to assist in this work. But, as is obvious, in a few years my evolution will come to an end. However, other able scholars will continue their work, and the outlines of a comprehensive theory should begin to emerge in the near future. No doubt some of these scholars will visit you to present a lecture in this series and to tell you about their evolution.

Awarded Nobel Prize in 1991. Lecture presented April 12, 1994.

Date of Birth

December 29, 1910

Academic Degrees

B.Com. University of London, 1932

D.Sc. (Economics) University of London, 1951

Academic Affiliations

Assistant Lecturer, Dundee School of Economics and Commerce, 1932–1934.

Assistant Lecturer, University of Liverpool, 1934–1935

Assistant Lecturer, London School of Economics, 1935–1938

Lecturer, London School of Economics, 1938–1947

Reader, London School of Economics, 1947–1951

Professor of Economics, University of Buffalo, 1951–1958

Professor of Economics, University of Virginia, 1958–1964

Professor of Economics, University of Chicago Law School, 1964–1970

Clifton R. Musser Professor of Economics, University of Chicago Law School, 1964–1981

Distinguished Professor (visiting) of Law and Economics, University of Kansas, 1991

Clifton R. Musser Professor Emeritus of Economics, and Senior Fellow in Law and Economics, University of Chicago Law School, 1982–present

Selected Books

British Broadcasting: A Study in Monopoly, 1950

The Firm, the Market, and the Law, 1988

Essays on Economics and Economists, 1994