ROBERT E. LUCAS, Jr.

I was born in 1937, in Yakima, Washington, the oldest child of Robert Emerson Lucas and Jane Templeton Lucas. My sister Jenepher was born in 1939 and my brother Peter in 1940. My parents had moved to Yakima from Seattle, to open a small restaurant, The Lucas Ice Creamery. The restaurant was a casualty of the 1937-38 downturn, and during World War II our family moved to Seattle, where my father found work as a steamfitter in the shipyards and my mother resumed her earlier career as a fashion artist. My brother Daniel was born in Seattle in 1948.

My parents were admirers of President Roosevelt and the New Deal. Their parents and most of our relatives and neighbors were Republicans, so they were self conscious in their liberalism and took it as emblematic of their ability to think for themselves. The idea that one could decide for oneself what kind of person to be, and that one ought to think about these decisions, was not limited to politics. I remember discussions, with my mother especially, of religion (she was a liberal protestant), of decor (she favored hardwood floors and oriental rugs), even on how to choose what kind of cigarette to smoke.

After the war, my father found a job as a welder at a commercial refrigeration company, Lewis Refrigeration. He became a craftsman, then a sales engineer, then sales manager, and eventually president of the company. He had no college degree and no engineering training, and learned the engineering he needed from the people he worked with and from handbooks. I remember many technical and managerial discussions with him, as well as our ongoing political arguments. When I took calculus in high school, he enlisted my help on a refrigeration design problem he was working on-and actually used my calculations! It was my first taste of real applied mathematics, and an exciting one.

I attended Seattle Public Schools, graduating from Roosevelt High School (where my parents had graduated in 1927) in 1955. I was good at math and science, and it was expected that I would attend the University of Washington in Seattle and become an engineer. But by the time I was seventeen I was ready to leave home, a decision my parents agreed to support if I could obtain a scholarship. MIT did not grant me one but the University of Chicago did. Since Chicago did not have an engineering school, this ended my engineering career. But when I began the 44 hour train trip "back east" to Chicago, I was pretty sure something interesting would turn up.

What to do instead? I took some mathematics at Chicago, but lost interest soon after my courses got past the material I had half learned in high school. I did not have the nerve to major in Physics, which is what you did at Chicago in those days if you thought you could make it. The real excitement for me was in the liberal arts core of the Chicago College, courses from the Hutchins era with names like History of Western Civilization, and Organization, Methods, and Principles of Knowledge. Everything in these courses was new to me. All of them began with readings from Plato and Aristotle, and I wanted to learn all I could about the Greeks. I took a sequence in Ancient History, and became a history major. Though I had no real idea what a professional historian does, I had learned that one can make a living by pursuing one's intellectual interests and writing about them. I began to think about an academic career.

I obtained a Woodrow Wilson Doctoral Fellowship, and entered the graduate program in History at the University of California. With no Greek or French and minimal Latin and German, I was in no position to pursue my classical interests, so I began work at Berkeley with little more than an open mind. The most exciting modern historian I had read at Chicago had been the Belgian historian Henri Pirenne, whose account of the end of the Roman era stressed the continuity of economic life in the face of major political disruptions. For me, Pirenne's shift of focus away from emperors and dreary Merovingian kings and on to the daily lives of private citizens was novel and exciting, and fit my sense of what was important. At Berkeley, I took courses in Economic History and audited an economic theory course. I liked economics at once, but it was obvious that to apply it with any confidence I would need to know much more than I could pick up on the side as a history student. I decided to move into economics and, since there appeared to be no hope of financial support from Berkeley's Economics Department, I returned to Chicago. During the rest of that academic year I took some undergraduate economics at Chicago and one or two graduate courses, to prepare for my real start as a graduate student the next fall.

It was lucky for me that one of my undergraduate texts referred to Paul Samuelson's *Foundations of Economic Analysis* as "the most important book in economics since the war." Both the mathematics and the economics in Foundations were way over my head, but I was too ambitious to spend my summer on the second most important book in economics, and Samuelson's confident and engaging style kept me going. All my spare time that summer went in to working through the first four chapters, line by line, going back to my calculus books when I needed to. By the beginning of fall quarter I was as good an economic technician as anyone on the Chicago faculty. Even more important, I had internalized Samuelson's standards for when an economic question had been properly posed and when it had been answered, and was in a position to take charge of my own economic education.

In the fall of 1960, I began Milton Friedman's price theory sequence. I had been looking forward to this famous course all summer, but it was far more exciting than anything I had imagined. What made it so? Many Chicago students have tried to answer this question. Certainly Friedman's brilliance and intensity, and his willingness to follow his economic logic wherever it led all played a role. After every class, I tried to translate what Friedman had done into the mathematics I had learned from Samuelson. I knew I would never be able to think as fast as Friedman, but I also knew that if I developed a reliable, systematic way for approaching economic problems I would end up at the right place.

Friedman's course ended my long career as a conscientious, near-straight A student. Now if a course did not promise to be a life-changing experience, I lost interest and attended only sporadically. I accumulated many C's, but also a lot of time to pursue what I found interesting. I took my first rigorous analysis courses, and a statistics course using Volume I of Willam Feller's *An Introduction to Probability Theory and Its Applications*. I still pick up Feller's book from time to time, as I do Samuelson's, just for the pleasure of the author's company.

There was also plenty of interesting economics going on at Chicago. My interest in probability and statistics stemmed from an interest in econometrics, stimulated by courses of Zvi Griliches and Gregg Lewis. Donald Bear, a new Assistant Professor from Stanford, taught a valuable mathematical economics course, and gave valuable encouragement to technically inclined students. Arnold Harberger's sequence in public finance was a lasting influence on me too. My thesis, which used data from U.S. manufacturing to estimate elasticities of substitution between capital and labor, was written under Harberger and Lewis, and was part of a larger project of Harberger's analyzing the effects of various changes in the U.S. tax structure.

There was a terrific collection of students at Chicago in the early 1960s. My closest friends were Glen Cain, Neil Wallace, Sherwin Rosen, and G.S. Maddala, and there were many others who now have international reputations. For many of us, the shock wave of Friedman's libertarian-conservative ideas forced a rethinking of our whole social philosophy. Intense student discussions ranged far beyond technical economics. I tried to hold on to the New Deal politics I had grown up with, and remember voting for Kennedy in 1960. "Nixon? Bob, you couldn't," my sister had said, and she was right (for then!). But however we voted, Friedman's students came away with the sense that we had acquired a powerful apparatus for thinking about economic and political questions.

In 1963 Richard Cyert, the new Dean of the Graduate School of Industrial Administration at Carnegie Institute of Technology (nowCarnegie-Mellon University), offered me a faculty position. I had met Allan Meltzer and Leonard Rapping at my job seminar there, and I knew GSIA would be a stimulating and congenial place for me. GSIA's leading intellectual figure was Herbert Simon. Although Simon was no longer working in economics when I came to Carnegie, he was always ready to talk about economics (or any other area of social or management science) at lunch or coffee. He gave all of us at GSIA the feeling of being in the major leagues, and helped us to outgrow the sense that all the important work was going on at Chicago or Cambridge.

Once my thesis was finished, I began theoretical work on the decisions of business firms to invest in physical capital and in improved technology. Dale Jorgenson had served on my Chicago thesis committee, and his work on investment had stimulated me. I spent a lot of time in my first years at Carnegie Tech learning the mathematics of dynamical systems and optimization over time, and trying to see how these methods could best be applied to economic questions. Economists of my cohort all over the world were engaged in this enterprise in the 1960s, and I remember exciting conferences on this theme at Chicago and Yale, led by Hirofumi Uzawa.

During my years there, Carnegie-Mellon had a remarkable group of economists interested in dynamics and the formation of expectations. Foremost, of course, was John Muth, my colleague in my first three years there. Morton Kamien and Nancy Schwartz had come from Purdue about the time I came from Chicago. Dick Roll, a student of Eugene Fama's at Chicago, brought the ideas of efficient market theory to GSIA. Thomas Sargent came to Carnegie-Mellon from Harvard in the middle of writing his thesis, and I remember the discussions he and Roll had about interest rates (that none of the rest of us could follow). Morris DeGroot taught a course in statistical decision theory that influenced Edward Prescott, and through Ed, me. John Bossons and later Michael Lovell studied direct evidence on expectations. It would be hard to think of a better group of colleagues, given my interests in economic dynamics.

At Carnegie I became involved in two collaborations, both of which bore immediate fruit and also influenced my thinking for years afterward. One of these was a project with Leonard Rapping, my closest friend and colleague at that time, in which we undertook to provide a neoclassical account of the behavior of U.S. wages and employment from 1929 to 1958. The paper was a bolder step into new territory than I would have taken then on my own, and the project never would have been undertaken or completed without Leonard's confidence and his expertise in labor economics.

Edward Prescott had come to GSIA as a doctoral student in the same year I joined the faculty, and we were immediate friends. A few years later, when Ed had become a faculty member at Penn, I enlisted his help on a theoretical project I had begun on the dynamics of an imperfectly competitive industry. That problem defeated us, but in the course of failing to solve it we found ourselves talking and corresponding about everything in economic dynamics. In a couple of years we learned large chunks of modern general equilibrium theory, functional analysis, and probability theory, and wrote a paper, "Investment under Uncertainty," that reformulated John Muth's idea of rational expectations in a useful way . During this brief period my whole point of view of economic dynamics took form (along with Ed's), in a way that has served me well ever since.

David Cass, who came to Carnegie-Mellon in 1971, had earlier aroused my interest in Samuelson's overlapping generations model of a monetary economy. At about the same time, Edmund Phelps convinced me that Rapping's and my model of labor supply needed to be situated in a general equilibrium context. These influences, combined with much that I had learned working with Prescott, came together in my paper, "Expectations and the Neutrality of Money," which was completed in 1970 and published in 1972. The role of this paper, certainly the most influential of my writings, is one of the subjects of my Nobel lecture. In May, 1995, Rao Aiyagari organized a 25th Anniversary Conference for this paper, sponsored by the Federal Reserve Bank of Minneapolis. This occasion ranks high among the professional pleasures and honors I have received.

In 1974 I returned to Chicago as a faculty member. In 1980 I became the John Dewey Distinguished Service Professor at Chicago, the position I hold today. Chicago has been a marvellous place for me, as I knew it would be from my student experiences, and I have been stimulated by colleagues and graduate teaching into research on monetary theory, international-trade, fiscal policy, and economic growth: all the basic topics in macroeconomics. But the main features of one's approach to science, like the main features of one's personality more generally, are set early on. For me, the influences of my parents, my undergraduate and graduate years at Chicago, and my years at Carnegie Mellon were critical, so it is these influences I have focused on here.

I have had a rewarding personal life, intertwined with the intellectual life that I have described in these notes. Rita Cohen, also an undergraduate at Chicago, and I were married in New York in August, 1959, just before I began graduate studies at Berkeley. Our son Stephen was born in Chicago in September, 1960. Our son Joseph was born in Pittsburgh in January, 1966. Steve is now a securities trader at the Chemical Bank in New York. Joe is a graduate student in History at Boston University, and his wife Tanya is a resident at Beth Israel Hospital in Boston. Rita and I were separated in 1982, and divorced several years later.

Since 1982 I have lived with Nancy Stokey, who is now a colleague of mine at Chicago. We have collaborated in papers on growth theory, public finance, and monetary theory. Our monograph, Recursive Methods in Economic Dynamics, was published in 1989. Since then, our collaboration has been a domestic one only . We have an apartment on Chicago's north side, and a summer house on Lake Michigan, in Door County, Wisconsin.