

A LIFE OF LEARNING

Robert William Fogel

Charles Homer Haskins
Lecture for 1996



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A Life of Learning

by

Robert William Fogel

When I was in junior high school, I had four passions: literature, history, sculpting, and science. Literary passion probably came first. Both my mother and my father were fine storytellers, often recounting their youth and the trials of their parents in czarist Russia. My mother was also an avid reader who sat for hours with Tolstoy's *War and Peace* and *Anna Karenina*, but her favorite writer was Thomas Wolfe and she gave me long reviews of *Look Homeward, Angel* and *You Can't Go Home Again*.

It was my father who first encouraged my interest in science. I remember the awe with which he spoke about his mathematics teacher in gymnasium and how he relished his brief career as an engineering student at the Kharkov Technology Institute, cut short by the outbreak of the Russian Revolution. When he, my mother, and my brother, then aged two, fled to the United States in 1922, it was with the intention of completing his education in engineering. Their route passed through Constantinople, where typhus nearly took my father's life and exhausted the money that was supposed to pay for transportation to

the United States and for tuition at Columbia University. A loan from the Hebrew Immigrant Aid Society (HIAS) purchased steerage passage to New York, but my father had to borrow a few dollars from a fellow passenger to get from Ellis Island to the home of an older cousin, who took the family in.

My father's first regular job was with the Eagle Pencil Company, at what would today be called "below the minimum wage," but it was enough to permit the family to move into a small apartment on the top floor of a tenement house without electric lights, and kept dark by the shadow of the Delancey Street Bridge, on Manhattan's Lower East Side. After a few months, my father found a better job and soon saved enough to buy a half-interest in a neighborhood grocery and meat store, but not enough to pay tuition or to support our family while he completed an engineering degree. But he did pass on his unrequited love for engineering and science to me. Fervor for science was in the air during the late 1930s and early 1940s, despite fears that unwelcome advances in technology contributed to the massive unemployment of the Great Depression. The movies celebrated the lives of Louis Pasteur, Paul Ehrlich, and Marie and Pierre Curie. Biomedical sciences were held in particular esteem. Paul De Kruif's *Microbe Hunters*, a best-seller that told the stories of Pasteur, Robert Koch, Ehrlich, Walter Reed, and other bacteriologists, was discussed in several of my classes in junior high school. It was there that I began making scientific exhibits, such as a model of the earth rotating on its axis in a box with a light bulb that showed how each part of the earth passed from day to night.

My passion for art can be traced to teachers in elementary and junior high school who introduced me to the history of art. When I was 12 years old, I discovered the recently established Museum of Modern Art, and for most of 1940 and 1941 I spent practically every Saturday in its halls. It was in fifth or sixth grade that I began sculpting in soap and I graduated to clay in junior high school. In one of my art classes I made a statue of a street urchin, which my teacher submitted to a citywide contest sponsored by the New York City Board of Education. A picture of that statue became the frontispiece for the book containing the winning selections. On the advice of my art

teacher, I enrolled in a class in sculpting at the 92nd Street Young Men's Hebrew Association (YMHA), taught by a young sculptor of some note, Yonny Segal. It was there that I learned to sculpt from models and moved from clay to wood.

Another notable memory of my art career in my youth pertains to December 7, 1941, a good part of which I spent in the Museum of Modern Art. I ran into Yonny Segal in front of Jacob Epstein's "Mother and Child" and began talking about the statue when he interrupted to ask whether I had heard that the Japanese had bomb Pearl Harbor. His mood was somber and I wondered what it all meant. I soon found out when my brother, who had been married a year earlier, entered the Army.

Six years older and nine years ahead of me in education, my brother Ephim was the main intellectual influence on me before he joined the Army in 1942. After the war he completed his Ph.D. in English, writing a dissertation on Sir Philip Sidney. He taught renaissance literature at Cornell University from 1949 until 1990, and was recognized for his work on the use of internal evidence. A poet and translator of poets, his most widely anthologized works dealt with the Holocaust. I imbibed Eph's love for poetry and literature and his concerns about social politics. I still remember an intense discussion between my brother (then 17) and some of his college classmates about ways to solve the Depression that I overheard one night in 1938 as I lay in my bed, supposedly asleep, in the next room.

Conflicting claims on my intellectual passions were temporarily resolved in favor of science when I was accepted by Stuyvesant, one of the three New York high schools specializing in science. Mathematics, chemistry, and physics were all fascinating, made so by skillful teachers who clearly loved their disciplines. I do not remember how I reached the decision, but when it came time to apply to college I told my parents I wanted to go into theoretical physical chemistry to study with Robert A. Millikan of the California Institute of Technology (CalTech), who had won the Nobel Prize in 1923.

Despite that decision, my interest in literature continued to grow dur

high school. Stuyvesant's English teachers were first-rate. Some of them carefully read the poetry I wrote, tutored me in the art, and encouraged me to write more. I wrote over a score of poems, some which were published in Stuyvesant's literary magazine, *The Calipe* and I served as the poetry editor of the magazine during my junior and senior years.

Although I had studied some history in elementary and junior high school, it was at Stuyvesant that I received my first systematic exposure to the subject. Once again I had delightful teachers with a zest for the field, who made history a gripping story. But, until the end of my high school career, I thought of history more as an avocation than as a possible vocation. The choice that perplexed me was whether to pursue literature or science.

My mother would not hear of my going to CalTech at a time when the train was still the principal means of long-distance travel. Fearing she would have little or no contact with me if I were studying in California she proposed Columbia or City College, where my brother obtained his bachelor's degree. My brother, the great arbitrator, proposed Cornell University: close enough so that I could return home in less than a day and still far enough away to give me the independence I was striving for. My father advised me to enroll as an electrical engineer which he described as "like physics but more practical." And so I did.

The first year of electrical engineering at Cornell included descriptive geometry, shop courses such as welding, and introductory calculus, physics, and chemistry, which retraced much of what I had already learned at Stuyvesant. Descriptive geometry, which provided a set of drafting tools that I still use in making diagrams, was okay but not compelling. By the end of the year, I had had enough of engineering and began to think of switching to either physics or English, but before I made that decision, I veered in another direction. During my freshman year I also enrolled in the introductory sequence in modern European history, taught by Carl Stephenson, whose lectures were so enthralling that I later applied for permission to take his advanced seminar in medieval history. It was in that course that I first read Charles Homer Haskin's *The Renaissance of the Twelfth Century* as well as

Ferdinand Lot, Henri Pirenne, and other leading medievalists.

As the euphoria over the end of World War II abated, the pending problems of the peace became more urgent and pessimism about the ability to sustain full employment was rife. Forecasts of an imminent return of the depression came not only from the left but also from such staunch defenders of capitalism as Senator Robert A. Taft, often referred to as "Mr. Republican." His pessimism became evident during the congressional debate over the Full Employment Act of 1946, which set up the Council of Economic Advisors to the President and charged it with developing an economic policy that would maintain full employment and stable prices. Everyone, said Taft, knew that it was impossible to maintain full employment under a capitalist system. Such debates turned my attention to economics and history. I hoped that studying economics would explain why it was so difficult to maintain employment during peacetime. I hoped that history would put recent economic experience into a longer-range perspective. Early in my junior year I switched to history as my major and to economics as a minor, but I took nearly equal numbers of courses in both fields during my junior and senior years.

I graduated from Cornell in 1948 and worked for a while in my father's meat processing business and then became involved in the presidential campaign of Henry A. Wallace. It was a disappointing experience--Wallace received hardly a million votes--except for one thing: I met and soon married the head of Harlem Youth for Wallace, Enid Cassandra Morgan, my wife, advisor, and friend for more than 47 years. She has been both my most unflagging supporter and my keenest critic, often sharpening my focus on key issues, especially when I become too preoccupied with technical details. During my graduate work, she spent numerous hours assisting in my research. Later on, at critical junctures, she subordinated her own career as an academic counselor and as a dean of students (at Rochester, Harvard, and Chicago) so that I could accept exceptional appointments.

I did not return to scholarly pursuits until the fall of 1956, when I enrolled in the graduate economics program at Columbia University with the intention of obtaining a Ph.D. in economic history. I believe

that by combining the study of history and economics I would quickly discover the fundamental forces that had determined technological and institutional changes over the ages and that such knowledge would point to solutions to the postwar problems of instability and inequity. As I became aware of how little was actually known about these large processes and their interconnections, I began to focus on more discrete issues: What did we really know about the impact of the factory system on economic and social institutions? What was the contribution of such new technologies as railroads or steel mills to economic growth? I concluded that, to answer such questions, much greater use had to be made of quantitative evidence, so I set out to master the most advanced analytical and statistical methods that were then taught in the economics department. It was only later that I discovered that the training program I had worked out for myself was unorthodox for an economic historian.

The teachers who influenced me the most during my year at Columbia were George J. Stigler, who taught the graduate microeconomics sequence, and Carter Goodrich, who taught the sequence in American economic history. Stigler, who won the Nobel Prize in economics in 1982, made microeconomic theory come alive. He emphasized not elegance but its applicability to a wide range of issues in economic policy. He continually moved between theory and evidence, carefully considering the empirical validity of the assumptions that theorists made about the slope or other aspects of the shape of key functions. He often considered when, with what model, and under what implicit assumptions one could validly draw a particular inference from a given body of data.

Goodrich impressed me not only with his knowledge of the literature of American economic history, but also with his willingness to identify gaps in the profession's collective knowledge of key issues. By the end of the course, one not only had a good grasp of what was known about the process of American economic growth, but also a list of potential projects. It was to Goodrich that I turned for advice on my master's thesis. He was then engaged in research for his book *Government Promotion of Canals and Railroads* and raised a number of issues that puzzled him about the financing, riskiness, and

benefits of the Union Pacific Railroad. These questions became the subject matter of my master's thesis, which was also my first published book. Although Goodrich did not himself make use of the new mathematical and statistical methods of economics, he encouraged me to do so. He also suggested that, given my substantial interests and quantitative approaches to economic history, Simon Kuznets at Johns Hopkins University was probably the best economist to guide my future training.

Among the teachers who taught me the most at Johns Hopkins, aside from Kuznets, were Abba Lerner and Fritz Machlup in microeconomic theory; Evsey Domar in macroeconomic theory and the theory of economic growth; T. C. Liu in mathematical economics; and two teachers of mathematical statistics and of sampling design in the School of Public Health.

Kuznets, who supervised my doctoral dissertation, was by far the most influential figure in my graduate training. Soft-spoken and of moderate stature, Kuznets was a towering intellect, and one did not have to be in his class very long to discover his erudition not only in economics, but also in history, demography, statistics, and the natural sciences. His course in economic growth covered the history of technological change during the modern era, demography and population theory, and the theory of national income aggregates for the comparative study of economic growth and of the size distribution of income. It was not until some years later that I realized the course presented the substance of the research that later appeared in a series of 10 supplements to *Economic Development and Cultural Change* and in his 1966 monograph, *Modern Economic Growth: Rate, Structure, and Spread*--the work for which he was awarded the third Nobel Memorial Prize in economics. Kuznets's course was valuable not only for the substance of the material but also for the way that he used the material to transmit the art of measurement. He repeatedly demonstrated that the central statistical problem in economics was not random error but systematic biases in the data, and he discussed a number of powerful approaches to coping with that problem, particularly emphasizing the role of sensitivity analysis.

The movement to bring modern statistical and analytical methods in the study of history (cliometrics) was very broadly based in the late 1950s. It was natural for Ph.D. students in economics to think of us the required tools of their discipline for analyzing not only contemporary economic problems but those of the past as well. The other fields of history that most obviously lent themselves to the new methods included demographic history, urban history, parliamentary history, and the history of popular voting behavior. Since quantitative methods were more effective in describing the characteristics of large groups of people than individual behavior, they were especially useful to historians who wanted to study the history of ordinary people. Methods of analysis that are appropriate for determining whether Thomas Jefferson's income declined during the postrevolutionary years are not appropriate for determining whether the income of American farmers, as a class, declined. Although quantitative methods are not only way of dealing with groups, they facilitate the exploitation of sources of information that could not otherwise be analyzed.

Cliometricians have worked out rules for the processing of quantitative evidence that are akin to the rules worked out a century and a half earlier by German historians of ancient and medieval societies for the study of languages (philology), writing (paleography), documents (diplomats), seals (sphragistics), and coins (numismatics), and for identification of medieval and ancient weights. Cliometric procedures pertain to such matters as the authentication and verification of both quantitative and testimonial evidence, the correction of incomplete or otherwise unrepresentative data sets, the use of sensitivity analysis for the determination of the effect of errors (in either the data or the analytical apparatus that has been imposed upon the data) on the conclusions drawn from the analyses, and the use of simulation models to evaluate the information content of patchy evidence. The four decades of experience with the application of these procedures provide ample testimony about their value in historical work.

The methods of family reconstitution developed in France by the demographer Louis Henri and in England by historians E. A. Wrigley and Roger Schofield made it possible to reconstruct the course of

mortality, fertility, and nuptiality going back to 1740 in France and 1541 in England. Analyses based on these data made it possible to trace the rise of fertility control, first by the regulation of the age of marriage and then by contraceptive methods within marriage. Cliometric techniques made it possible to trace the escape from hunger and high mortality in Europe and America between 1750 and the present, revealing the key roles played by the expansion of the food supply and the revolution in public sanitation. Cliometric reconstructions of fertility, mortality, and nutritional status are beginning to provide reliable answers to the long debate on how a millennium cycle, during which overrapid population growth was followed by chronic malnutrition and deadly epidemics, was brought to an end.

Quantitative methods are now so entrenched in economic, social, and political history that it is difficult to understand why the introduction of these methods in the 1950s and 1960s produced a strident cultural war. Some attribute that conflict to the aggressiveness of the cliometricians, who were youthful, inexperienced, and sometimes lacking in the etiquette of scholarly debates. The cliometricians were also charged with exaggerating their accomplishments and underestimating the lines of continuity between the new work and the old. In defense of my generation, I should point out that most of us recognized that we were adding to the tools of historical analysis rather than replacing long-tested traditional tools. Many of us also recognized that art is an important feature of historiography. For my own part, I always viewed the narrative as the central focus of historical writing and considered the highly technical, highly detailed analysis of evidence as an intermediate step necessary to put a readable narrative on a sound footing.

The cultural war was also partly a battle over turf. The early cliometricians with Ph.D.s from history departments found it difficult to get established in the discipline. The leading history journals, even in economic history, initially refused to accept articles with complex tables, and even after such articles began to be accepted, equations were forbidden. A statistical comparison of the rate of promotions of cliometricians with that of traditional historians confirmed that anticliometric discrimination extended to employment practices. An

those who received their Ph.D.s in history before 1960, it took cliometricians 2.5 years longer, on average, to receive tenure than practitioners of traditional history. Journals that catered to the new mode of research came into being during the 1960s and 1970s and have flourished: among them, *Historical Methods*, the *Journal of Interdisciplinary History*, the *Journal of Family History*, and *Social Science History*.

By the beginning of the 1980s, there were about 2,500 scholars in quantitative methods, and they accounted for about one-sixth of all professional historians in the country. In the midst of a terrible depression for new holders of history Ph.D.s that began during the 1970s, the demand for cliometricians was expansive, and they received the lion's share of the new jobs. Instead of being held back when it came to promotion, cliometricians who received their degrees during the 1970s were admitted to tenure with unusual speed, achieving that position nearly two years faster, on average, than traditional historians of the same Ph.D. cohort.

We could not have made these advances without the blessing of traditional historians who did not hesitate to express their appreciation of the contributions we were making to the profession. "We had always known that the archives contained mountains of unexploited documents in such forms as dowry contracts, manuscript schedules, censuses, baptismal and burial records, and probate records; but we did not know how to make use of them. We thought they were too massive to be tackled." The words were those of Frederick C. Lane, a venerable historian of late-medieval Venice, who was nearing 80 when we had this conversation in 1979. "While the boisterous claims of the cliometricians are regrettable," he concluded, "their techniques are making these neglected sources of evidence available to the whole profession."

For some cliometricians these concessions seemed begrudging. Many took little comfort from the large share of available slots going to them and instead coveted the slots that were still occupied by traditionalists. "What is wrong with your crowd?" Bernard Bailyn of Harvard University said to me one evening at the end of the 1970s. "They ne

seem to be satisfied, no matter how many appointments they get." A similar point was made to me by the late G. R. Elton, Regius Professor of History at Cambridge University. Another of the peacemakers, I condemned as "rubbish" the "sectarian exclusiveness" of cliometricians. Their "cries for the burning of heretics and the public humiliation of dissenting parties," he wrote in December of 1979, "hides cracks of uncertainty in those who do the yelling." Elton was willing to allow that these noises were a reaction to the "ignorant contempt emanating from the traditionalists." But, he concluded, "cliometrics (or call it what you will) long since won that battle and should now realize that it is sitting quite comfortably in the drawing room, not any longer storming the outer bastions."

By the 1980s, the integration of cliometricians into a variety of honor organizations on both sides of the Atlantic and their assimilation into the leadership of the leading historical associations, as well as into the administrative offices of universities and research organizations was apparent. Further evidence that the cultural war was over came from a survey conducted in 1987 among historians and historically minded social scientists in departments that grant Ph.D.s in history. The great majority believed that historians ought to have a workable knowledge of statistical methods. As for the cliometricians, 72 percent of them stated that even when statistical evidence is relevant, it should be subordinated to the development of the narrative.

My research career, so far, has revolved around three main projects. From the late 1950s through the early 1970s, I was primarily concerned with the impact of technological change on economic growth, including the types of institutions that encouraged technological change and the impact of technological change on institutional arrangements. Public enthusiasm for technological change may have been at an all-time peak in the late 1950s and early 1960s. Not only had the advanced productive technology of the United States made possible to defeat the evil war machines of the Axis powers, but the new technologies of the postwar era touched off a period of exceptionally rapid economic growth that began around 1953 and continued unabated for two decades. As these years of prosperity rolled on, memories of the millions of the unemployed during the Great

Depression began to fade, and it was widely believed that we had entered into a golden age that W. W. Rostow of MIT called "the age of high mass consumption."

This was the age during which Herbert Hoover's failed promise of a chicken in every pot and a car in every garage was finally realized. Not only was the electrification of the United States completed, but refrigerators, deep freezers, and air-conditioning had become commonplace; television, a dream before World War II, was, by the mid-1950s, installed in 90 percent of households. Later, clothes washers and dryers, dishwashers, frozen food, microwave ovens, and numerous other new consumer products reduced drudgery, and millions of women streamed into the labor force, which expanded so rapidly that it not only passed Henry A. Wallace's wartime dream of 60-million peacetime jobs but rushed toward the 100-million mark. Under such circumstances it was easy to herald technological change as the savior of the economy. To many economists during the late 1950s and 1960s, technological change was transparently good and beneficial to virtually everyone. It was a rising tide that raised all ships. That was the climate of thought that led me to concentrate on the history of technological change.

When I began my doctoral studies at Johns Hopkins, railroads were universally agreed to have been the greatest technological innovation of the 19th century and the engine that pushed forward the whole of the American economy. Kuznets pointed out that, despite this consensus, we still knew little about the factors that stimulated the growth of railroads, or about which aspects of this technology were most important, or about how changes within the railroad industry affected technological change in industries that either served the railroads or were served by them. The problems that he posed to me were both analytical and quantitative. It was necessary first to identify which aspects of this innovation were most critical and then to measure the magnitude of the cost reductions they brought about. Ideas about what was analytically important abounded. The far more difficult problem was where to find the evidence needed to evaluate these hypotheses. It was this task that occupied me full time from 1958 through 1965, and then intermittently down to the late 1970s.

Toward the end of the 1960s, my attention shifted from railroads to iron and steel industry, which had been the symbol of modernity during my elementary and high school days and which continued to be celebrated during the postwar years. In 1967 Stanley Engerman and I began collaborating on what we expected to be a study of long-term trends in the growth of the American iron and steel industry and in the technological innovations that turned steel from a semiprecious metal into a common material that was cheap enough to be used for the manufacture of inexpensive children's toys. The first phase of the study focused on the growth of iron and steel prior to the Civil War. We attempted to evaluate the relative importance of technological innovation and tariff protection on the turbulent ups and downs of the industry, which sandwiched eras of spectacular expansion with years of recession so severe that bankruptcies abounded and a third or more of the labor force was discharged.

We were about three years into this project when our attention was distracted by the ongoing debates among cliometricians about the economics of slavery. Cliometrics announced its birth at a conference in Williamstown in 1956 jointly sponsored by the Economic History Association and the National Bureau of Economic Research (NBER). The high point of the conference was a presentation by two young Harvard econometricians with strong historical interests, Alfred H. Conrad and John R. Meyer, who stunned the conference with a paper entitled, "The Economics of Slavery in the Ante-Bellum South." Contradicting received wisdom, they argued that far from being an economic failure, the slave plantations of the antebellum South were highly profitable. Their econometric analysis revealed that the rates of return on slave plantations in all sections of the deep South were equal to, or exceeded, rates of return in northern manufacturing industries.

Although this highly technical article attracted little public notice, it touched off one of the longest and most passionate debates in the history of the field. I was a graduate student at Johns Hopkins in 1964 when the slavery paper was published. The debate in our department raged for weeks and embraced most of the graduate students and faculty in economics. Some were elated by this attempt to extend

"hard" scientific methods to a central question of American history. I found it difficult to believe that a system as reprehensible as slavery was so profitable and, along with others, searched for analytical errors that would overturn the finding. The only important error discovered in that effort, when corrected by Yasukichi Yasuba, now a distinguished development economist at Osaka Gakuin University, actually reinforced the conclusions of Conrad and Meyer by showing that the profitability of slavery was not only high, but increased rapidly during the last two decades of the antebellum era.

The vast outpouring of cliometric studies on slavery delved not only into the issue of profitability but also into the viability of slavery as an economic system and the impact of slavery on economic growth. Cliometric studies were also voluminous on such other topics as monetary and fiscal policy, investment in education, migration, urbanization, and public policy. Toward the end of the 1960s, Engerman and I decided that it would be useful to bring together a book that would represent 30 or so of the best cliometric contributions during the preceding decade. The book that we designed, and that we published in 1971 under the title *The Reinterpretation of American Economic History*, was divided into nine principal sections, one of which dealt with the economics of slavery. During the course of writing the introduction to this section, Engerman and I posed the question "What should the cliometricians studying slavery do next?" We suggested that they ought to measure the relative inefficiency of slave labor, and undertook "a back-of-the-envelope" computation that would illustrate our point. Much to our surprise, this rough calculation showed that slave agriculture was 6 percent more productive than free agriculture--a patently ridiculous result. We spent several months collecting better and more complete data and recalculated the index. To our astonishment, instead of going down, the relative advantage of slave agriculture rose to over 40 percent.

At that point we did what all economists do when they have a problem: we applied for a research grant from the National Science Foundation (NSF). This grant and several that followed it permitted us to undertake a thorough examination of all aspects of slavery as an economic, social, and political institution. The results of this work,

which involved scores of collaborators and extended from the early 1970s to 1992, resulted in the publication of six volumes that Engerman and I jointly wrote or supervised and numerous other books and papers. The controversy set off by these studies, which was painful at times, led to a far more detailed and subtle understanding of the nature of slavery as an economic and social system than any of us initially dreamed was possible.

During the course of my slavery research, I became interested in demography, and in 1975 I decided to investigate the antecedents of the 20th-century decline in mortality. The evidence on secular trends in American mortality was so patchy in 1975 that experts in the field could not agree on whether the trend in mortality rates during the 18th and 19th centuries was increasing, decreasing, or flat. Engerman and I began to investigate whether data could be obtained for the United States that would permit us to develop time series on mortality rates and life expectation that extended back to the founding of the American colonies. During the year 1975-76, Marilyn Coopersmith, the associate director of the Center for Population Economics (CPE) at the University of Chicago, and I scoured documentary sources in the Public Record Office (PRO) in England that might bear on the course of mortality during the colonial period. At the same time, we began to investigate, with the help of Clayne L. Pope and Larry T. Wimmer of Brigham Young University (BYU), the resources of the Genealogical Library in Salt Lake City. By the end of 1976 we concluded that the data contained in the records of both the PRO and the Genealogical Library provided the basis for reconstructing the course of mortality in North America from the beginning of the 17th century.

We initiated a project, originally centered at the NBER but subsequently extended to the University of Chicago, BYU, and nine other institutions in the United States and Great Britain. The project, which was entitled "The Economics of Mortality in North America, 1650-1919," was originally funded by grants from the NBER, Harvard University, BYU, and the NSF, and later from the Charles R. Walgreen Foundation, the Exxon Foundation, and the CPE.

During the course of the work on mortality, we discovered that

anthropometric measures such as height, weight, and the body mass index or BMI (a measure of weight controlled for height) could provide rich insights into the nutritional status and health of a population. Such anthropometric information, particularly information on stature, proved to be so widely available that we established a second project, called "Secular Trends in Nutrition, Labor Welfare, and Labor Productivity" jointly sponsored by the NBER and the CPE. The project has gathered a score of data sets containing information on stature, mortality, and related variables on more than a half-million persons in the United States, Great Britain, the British West Indies, Sweden, Austria, and Hungary. Numerous other scholars working independently have become active in this branch of research and have provided information on many additional nations that has greatly illuminated long-term trends in the health and welfare of the poor in both industrialized and developing nations. Progress in this type of research has depended, to a large extent, on the spectacular reductions in the cost of data processing that have been made possible by the computer revolution.

In 1981 we began examining the possibility of creating life-cycle and intergenerational data sets that would permit us to study the impact of socioeconomic and biomedical stress early in life on the rate of onset of chronic diseases, on the capacity to work at middle and late ages, and on "waiting time" until death. The experiment, supported by grants from the National Institutes of Health (NIH) and the NSF, has focused on the pension records of the Union Army, which were available at the United States National Archives, and which contained detailed medical histories of veterans from childhood until death. This data set has made it possible to investigate changes in the process of aging since the beginning of the 20th century.

In 1995 we began the construction of two new longitudinal data sets. The first is based on a sample of births between 1910 and 1934 that is suitable for evaluating the impact of prenatal maternal health, fetal growth, and development during infancy on the odds of developing specific chronic diseases at middle and late ages. The second sample, which also focuses on babies born between 1910 and 1934, is designed to make use of family histories and other genealogical data.

order to measure the impact of intergenerational factors on the process of aging. The preliminary findings of these projects suggest that the secular decline in morbidity and mortality since 1700 stems from a synergism between technological and physiological improvements. This synergism has produced a form of human evolution--biological though not genetic; rapid, culturally transmitted, and not necessarily stable--which is still ongoing in both rich and developing countries.

Although my research during the last decade has turned increasingly toward my earlier interests in the natural sciences, I continue to maintain a strong interest in political and intellectual history. I am currently completing a volume entitled *The Fourth Great Awakening: The Political Realignment of the 1990s and the Fate of Egalitarianism* which is scheduled to be published by the University of Chicago Press in 1997. The book will present a theory of 100-year religious-political cycles and will examine the impact of these movements on the shape of economic policy.

When I was in high school, I made myself a promise that I would write a novel, a play, and a book of poems. I have often thought of that promise but in recent years the burdens of current projects have been so intense that it has been difficult to find the time to fulfill it. The invitation to be the Charles Homer Haskins Lecturer for 1996 has rekindled my literary aspirations and renewed my desire to try my hand at creative writing.

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