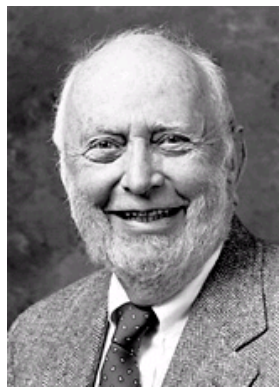




Douglass C. North

The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel 1993

Autobiography



I was born in Cambridge, Massachusetts - not because my family had any connection with higher education, but because my father was a manager at the Metropolitan Life Insurance Company in a nearby town and Cambridge was the nearest hospital - in 1920. In the ensuing years we moved a number of times as a result of my father's business. First Connecticut and then, when he became head of the Metropolitan's Canadian office, Ottawa. Because my mother believed in education broadly construed, we also lived in Europe and I went to school at the Lycée Jacquard in Lausanne, Switzerland, in 1929-30. My brother and sister are both older than I am and were born before my father went off to World War I.




I went to elementary school in Ottawa, and then to a private secondary school. When we moved back to the United States in 1933, I went to private schools in New York City and on Long Island, and then completed my high school education at the Choate School in Wallingford, Connecticut. While I was there I became deeply interested in photography, and indeed the most noteworthy event in my early life was winning first, third, fourth and seventh prizes in an international competition for college and high school students.

Our family life was certainly not intellectual. My father had not even completed high school when he started as an office boy working for the Metropolitan Life Insurance Company, and I am not sure that my mother completed high school. Nevertheless, she was an exciting person, intelligent, intellectually curious, and she played an important part in my intellectual development. My aunt and uncle were, and in the case of my aunt (Adelaide North) still is, a powerful influence. They introduced me to classical music and my aunt continues to be, to this day, a very special person in my life.

When it came time to go to college, I had been accepted for Harvard when my father was offered the position of head of the Metropolitan Life Insurance Company office on the west coast, and we moved to San Francisco. Because I did not want to be that far from home, I decided to go instead to the University of California at Berkeley. While I was there my life was completely changed by becoming a convinced Marxist and engaging in a variety of student liberal activities. I was opposed to World War II, and indeed on June 22, 1941 when Hitler invaded the Soviet Union I suddenly found myself the lone supporter of peace since everybody else had, because of their communist beliefs, shifted over to become supporters of the war. My record at the University of California as an undergraduate was mediocre to say the best. I had only slightly better than a "C" average, although I did have a triple major in political science, philosophy, and economics. I had hoped to go to law school, but the war started, and because of the strong feeling that I did not want to kill anybody, I joined the Merchant Marine when I graduated from Berkeley. We had been to sea only a short time when the Captain called me up on the bridge and asked me if I could learn to navigate since most of the officers had had only rudimentary education, and we needed to get from San Francisco to Australia. I became navigator and enjoyed it very much. We made repeated trips from San Francisco to Australia, and then to the front lines in New Guinea and the Solomon Islands.

What the war did was give me the opportunity of three years of continuous reading, and it was in the course of reading that I became convinced that I should become an economist. Then the last year of the war I taught celo-navigation at the Maritime Service Officers' School in Alameda, California; I took up photography again and had a difficult decision as to whether to become a photographer or go into economics. In the summer of 1941 I had worked with Dorothea Lange, head of the photographic division of the Farm Security Administration, travelling with and photographing migrants through the central valley of California. Now Dorothea tried to persuade me to become a photographer. Her husband, Paul Taylor, who was in the economics department at the University of California, tried to persuade me to become an economist. He won.

I went back to graduate school with the clear intention that what I wanted to do with my life was to improve societies, and the way to do that was to find out what made economies work the way they did or fail to work. I believed that once we had an understanding of

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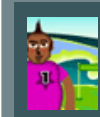
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what determined the performance of economies through time, and could we improve their performance. I have never lost sight of that objective.

I cannot say that I learned much formal economics as a graduate student in Berkeley. My most influential professors were Robert Brady; Leo Rogin, a Marxist and a very influential teacher of history of economic thought; and M. M. Knight (Frank Knight's brother) who certainly was agnostic, to say the least, about theory, but who had a wonderful knowledge of the facts and background in economic history. He became my mentor and my thesis advisor at Berkeley. But while I learned by rote most of the theory I was supposed to know, I did not acquire a real understanding of theory. It was not until I got my first job, at the University of Washington in Seattle, and began playing chess with Don Gordon, a brilliant young theorist, that I learned economic theory. In the three years of playing chess every day from noon to two, I may have beaten Don at chess, but he taught me economics; more important he taught me how to reason like an economist, and that skill is still perhaps the most important set of tools that I have acquired.

I had written my dissertation on the history of life insurance in the United States and had had a Social Science Research Council Fellowship to go to the east coast and do the spade work. That turned out to be a very productive year. I not only sat in on Robert Merton's seminars in sociology at Columbia, but also became deeply involved in the Entrepreneurial school of Arthur Cole at Harvard. The result was that Joseph Schumpeter had a strong influence upon me. My early work and publications centered around expanding on the analysis of life insurance in my dissertation and its relationship to investment banking.

I next turned to developing an analytical framework to look at regional economic growth and this led to my first article in the *Journal of Political Economy*, entitled "Location Theory and Regional Economic Growth". That work eventually led me to developing a staple theory of economic growth.

I was very fortunate that at a meeting of the Economic History Association I came to know Solomon Fabricant, who was then director of research at the National Bureau of Economic Research; and in 1956-57 I was invited to spend the year at the Bureau as a research associate. That was an enormously important year in my life. I not only became acquainted with most of the leading economists who passed through the bureau, but spent one day a week in Baltimore with [Simon Kuznets](#) and did the empirical work that led to my early major quantitative study of the balance of payments of the United States from 1790 to 1860.

I married for the first time in 1944. During my graduate training my wife taught school, providing our major source of support. We had three sons, Douglass, Christopher, and Malcolm, born between 1951 and 1957. After the boys were in school my wife became a successful politician in the Washington State legislature.

Between my year at the National Bureau and 1966-67, when I went off to Geneva as a Ford Faculty Fellow, I did my major work in American economic history, which led to my first book, *The Economic Growth of the United States from 1790 to 1860*. It was a straightforward analysis of how markets work in the context of an export staple model of growth.

By this time (1960) there was a substantial stirring to try to change and transform economic history. The year that I was at NBER, the Bureau and the Economic History Association had the first joint quantitative program on the growth of the American economy, a conference that was held at Williamstown, Massachusetts, in the late spring of 1957. This meeting was really the beginning of the new economic history, but the program coalesced when Jon Hughes and Lance Davis, two former students of mine who had become faculty members at Purdue, called the first conference of economic historians interested in trying to develop and apply economic theory and quantitative methods to history. The first meeting was held in February of 1960. This program was highly successful and the reception that it received amongst economists was certainly enthusiastic. Economics departments very quickly became interested in having new economic historians, or, as we came to call ourselves, cliometricians (Clio being the muse of history). Therefore, as I developed a graduate program jointly with my colleague Morris David Morris at the University of Washington we attracted some of the best students to do work in economic history, and during the 1960s and early 70s the job market was very responsive and our students were easily placed throughout the country.

In 1966-67 I decided that I should switch from American to European economic history, and therefore, when I received the above-mentioned grant to live in Geneva for a year, I decided to re-tool. Re-tooling turned out to change my life radically, since I quickly became convinced that the tools of neo-classical economic theory were not up to the task of explaining the kind of fundamental societal change that had characterized European economies from medieval times onward. We needed new tools, but they simply did not exist. It was in the long search for a framework that would provide new tools of analysis that my interest and concern with the new institutional economics evolved. The result was two initial books, one with Lance Davis, *Institutional Change and American Economic Growth*, and the other with Robert Thomas, *The Rise of the Western World: A New Economic History*.

Both books were early tentative attempts to develop some tools of institutional analysis and apply them to economic history. Both were still predicated on neo-classical economic theory, and there were too many loose ends that did not make sense: such as the notion that institutions were efficient (however defined). Perhaps more serious, it was not possible to explain long-run poor economic performance in a neo-classical framework. So I began to explore what was wrong. Individual beliefs were obviously important to the

choices people make, and only the extreme myopia of economists prevented them from understanding that ideas, ideologies, and prejudices mattered. Once you recognize that, you are forced to examine the rationality postulate critically.

The long road towards developing a new analytical framework involved taking all of these considerations seriously: to develop a view of institutions that would account for why institutions produced results that in the long run did not manage to produce economic growth; develop a model of political economy in order to be able to handle and explain the underlying source of institutions. Finally, one had to come to grips with why people had the ideologies and ideas that determined the choices they made.

In *Structure and Change in Economic History* (1981) I abandoned the notion that institutions were efficient and attempted to explain why "inefficient" rules would tend to exist and be perpetuated. This was tied to a very simple and still neo-classical theory of the state which could explain why the state could produce rules that did not encourage economic growth. I was still dissatisfied with our understanding of the political process, and indeed searched for colleagues who were interested in developing political-economic models. This led me to leave the University of Washington in 1983 after being there for 33 years, and to move to Washington University in St. Louis, where there was an exciting group of young political scientists and economists who were attempting to develop new models of political economy. This proved to be a felicitous move. I created the Center in Political Economy, which continues to be a creative research center.

The development of a political-economic framework to explore long-run institutional change occupied me during all of the 1980s and led to the publication of *Institutions, Institutional Change and Economic Performance* in 1990. In that book I began to puzzle seriously about the rationality postulate. It is clear that we had to have an explanation for why people make the choices they do; why ideologies such as communism or Muslim fundamentalism can shape the choices people make and direct the way economies evolve through long periods of time. One simply cannot get at ideologies without digging deeply into cognitive science in attempting to understand the way in which the mind acquires learning and makes choices. Since 1990, my research has been directed toward dealing with this issue. I still have a long way to go, but I believe that an understanding of how people make choices; under what conditions the rationality postulate is a useful tool; and how individuals make choices under conditions of uncertainty and ambiguity are fundamental questions that we must address in order to make further progress in the social sciences.

In 1972 I married again, to Elisabeth Case; she continues to be wife, companion, critic and editor: a partner in the projects and programs that we undertake.

I would be remiss if I left the impression that my life has been totally preoccupied with scholarly research. True, it has been the fundamental focus of my life, but it has been intermingled with a variety of activities that have complemented that central preoccupation and enriched my life. I continue to be a photographer; I have enjoyed fishing and hunting with a close friend; and have owned two ranches, first in northern California and then in the state of Washington. I learned to fly an airplane, and had my own airplane during the 1960s. I have always taken seriously good food and wine. In addition, music has continued to be an important part of my life.

My wife and I now live in the summers in northern Michigan in an environment which is wonderfully conducive to research, and where most of my work in the last 15 years has been done. I work on research all morning. In the afternoons I hike with my dog, play tennis or go swimming. In the evening, as we are only 16 miles from the National Music Camp at Interlochen, we may listen to music two or three nights a week. It is a wonderful place for that mixture of research and leisure which has made my life such a rich experience.

From *Les Prix Nobel. The Nobel Prizes 1993*, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1994

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Addendum, May 2005

Since receiving the Nobel Prize in 1993 I have continued my research trying to develop an analytical framework that would make more sense out of long-run economic, social and political change. With that objective in mind, I have gone much more deeply into cognitive science and attempted to understand the way in which the mind and brain work and how that relates to the way in which people make choices and the belief systems that they have. Clearly these underlie institutional change and therefore are a necessary prerequisite to being able to develop a theory about institutional change. I have also attempted to integrate political, economic and social theory since, obviously, a useful theory of economic change cannot confine itself purely to economics but must try to integrate the social sciences and integrate them also with cognitive science. The result is a recently published book by Princeton University Press entitled *Understanding the Process of Economic Change*.

One result of these interests has been to establish jointly with Ronald Coase, who won the Nobel Prize in 1991, the International Society for the New Institutional Economics. Its first meeting was held in 1997 here in St. Louis, and subsequent to that it has become a thriving international organization with meetings all over the world. The new institutional economics has become such a significant addition to the social sciences that I have been asked to elaborate on it all over the world, particularly in China where there is much enthusiasm about the implications of the new institutional economics applied to solving problems of the Chinese political economic future. In 1995 the University of Beijing formally opened a research center in economics at which I gave the opening address. I also have served as adviser in applying the new institutional economics to economic development in Asia and Latin America and in Eastern Europe. One result of all of this was to establish here at Washington University in St. Louis a center for the new institutional social sciences which attempts to integrate, both at the level of teaching and in research, the social sciences.

In addition, because I feel very strongly that we must reorient the social sciences to attempt to confront these issues and to be more oriented toward policy problems, I held a meeting in the fall of 1994 of leading social scientists from political science, economics, and sociology to attempt to plan how the social sciences should evolve over time. This initial meeting was successful and successive meetings are planned for future years and at other universities to attempt to build on this development.

And finally, as a result of being asked to participate in the Copenhagen Consensus, which was an attempt to get a number of economists to confront leading issues around the world, I participated in what turned out to be a very interesting attempt to explore and resolve problems as varied as HIV and aids, malnutrition, clean water, etc., to come up with policy recommendations that would move towards solving such problems. I continue to be involved in all of those things at this time.

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