Spring 1990

My Life Philosophy

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Published In
The American Economist, 34, 1, 22-32.
If "life philosophy" suggests an articulated set of tastes or beliefs that I apply repeatedly to give direction to my life, I cannot set out a life philosophy. In fact, I don't believe that it is useful much of the time to describe, even after the fact, whether one's choices are the result of initial or acquired beliefs (or tastes) or the result of constraints. Clearly, both operate and both tastes and constraints change. Choices are observable; beliefs are not. Moreover, my beliefs are not static. They are shaped not only by where I came from but by what happened to me along the way.

The latter is important. Much of what happened to me, not only to me, is the result of chance rather than of plan, belief or philosophy. A different experience would have given me different beliefs and imposed different constraints. Discussion of my life philosophy is a narrative with four or five main events that seem to me to have been important for shaping my beliefs and my later decisions in one area. Events did not always work by building on the past; some modified the influence of past experience and caused me to accept beliefs that I had rejected earlier or widened my perspective by introducing new elements. I illustrate this process by highlighting the changes in my beliefs about the role of government.

**Chance Events**

The first of several chance events that had a major influence on my beliefs had a very unhappy beginning. My mother died just before I was six years old, so most of what I believe about her influence probably reflects what I was told later rather than what I learned from her. It is not the same. From six to ten, my sister and I lived with our grandmother, a stern disciplinarian and a strong personality. The first word I think of is matriarch, and she was that. But, she was much too wise to depend only on her power to influence. She exerted influence in the way that other people chew gum; she didn’t have to think about it. Looking back I believe she internalized a set of rules. She was strong-willed, but she had a sense of humor and, though she could be fierce when angry, she was usually thoughtful, perceptive and kindly.

I learned both by observing her in action and by listening to her. Itinerants and peddlers came regularly. None was refused. There were no lectures on the virtues of charity or social responsibility and no need for them. The example was far more powerful. I like to believe that, in this as in other ways, she greatly influenced my beliefs and attitude toward others.

Her most important influence on my career and my outlook was her strongly held belief that, in America (and only in America), there were no real limits other than ability to what one could achieve by personal effort. Prejudice and restrictions were real; one would have to work a bit harder or be a bit smarter to overcome them. She must have been disappointed when, at eight, I gave up the violin, but she accepted and perhaps secretly agreed with my conclusion that I had little talent for that instrument. Despite this bit of contrary evidence I grew up with a positive view of challenges and a desire to accept them. The moral was not that winning is good; it was to live up to your potential, expand your horizon and develop your skills.

Grandmother had been a successful businesswoman earlier. Looking back, I believe that it was her discussion of this experience, especially the emphasis on the satisfaction she got from producing and selling a good product, that strongly influenced me to look for satisfaction in the work one does. This should not suggest that rewards and recognition are unimportant, but they are not the sole or even the most important reason for choosing one’s activities. While I grumble as much as anyone about the pressures

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of deadlines and commitments, I enjoy much of what I do. At times, I cannot distinguish between “work” and “recreation”, as those terms are used conventionally.

I cannot begin to sort out and set down all that I learned in the four years spent in my grandmother’s house. One strong effect was on my early social views. She was a Franklin Roosevelt Democrat, as were all of her family, neighbors and friends, committed to the idea that the government was a big brother or sister with responsibility for helping people with their problems just as she helped people who came to her door.

Grandmother was not very active in the practice of religion, but she faithfully followed the Jewish laws and orthodox traditions. At six I was sent for what now seems a large dose of daily religious and cultural training. I did not object at the time, and I continued the training for several years after I left her household. By the mid-teens, I tired of the disputatious arguments over narrow points of doctrine that could not be resolved. Man’s fate could not depend on such small differences. Further, there was, for me, too much emphasis on rules and prohibitions relative to ends or objectives, on form rather than substantive outcomes, on authority instead of personal choice. There seemed to be a wide gap between professed beliefs and practices. Later, I became convinced that these problems were not unique to Judaism or for that matter to religion. My religious training heightened my interest in issues such as these. My interest in institutions, in the way they work and the gap between objectives and outcomes began with this experience.

Like many teenagers, I was eager to leave home. I applied to colleges that were out of Boston and chose Duke. If there was a reason for the choice from among schools that took me away from home I do not recall it. At Duke, I learned some of what it meant to be in a segregated society. The Duke campus was completely segregated. I was appalled by the racial laws and by segregation. I joined various activist groups concerned about equality and in 1948, though not yet old enough to vote, helped to organize the Progressive Party of North Carolina. As a delegate to the party’s national convention in Philadelphia, I voted to nominate Henry Wallace and Glen Taylor for President and Vice President and to pass resolutions, many of them obscure items in the leftist political program of that day. No one explained the issues; we were told how we ought to vote. Only later did I learn what some parts of the platform meant. Others were so obscure, I could not find out what they meant and learned only that many delegates were as ignorant as I. I found much in the experience that reminded me of the religious orthodoxy I had rejected earlier.

The convention was in July. In August I completed my undergraduate work and moved to Los Angeles where my family had relocated. I went to work for the family business and, despite some concerns about my experience at the convention, continued working in the Wallace campaign. To my regret and surprise, the Los Angeles branch of the campaign had a very narrow view of the issues. They were not much interested in segregation and civil rights. Their focus was on defeating the Marshall Plan, which they denounced as an imperialist effort by the United States to establish hegemony in Europe. Although the number of active workers was not very large, my efforts to change the focus were futile. Somewhat disillusioned, I left the campaign by October.

The experience did not fit well with the view of government and government’s role acquired in my grandmother’s house. My associates were not much interested in working to achieve equality of opportunity and what I thought was self evidently a better world. They not only had their own agenda, but they dismissed the benefits the Marshall Plan would have for the Europeans and for trade and development.

The experience was disconcerting for a believer in the goodwill theory of government according to which a responsive government acts to improve society’s performance and increase justice. It was not that I recognized then how much is hidden by the words “improve society’s performance and increase justice.” Here were “good people” interested in pursuing their own objectives in much the same way as the people in power. I believed, then, that justice was a state or condition. Although one might not be able to describe the state in full detail, it was possible to describe changes as movements toward or away from the ideal of equal justice. The problem was that self-interested people used government for personal
ends. "Good people" were not supposed to do that.

Writing this after forty years, it seems totally naive. Yet, it is close to what I believed at the time. I do not think that these ideas were mine alone. They are common in the press, in journals of opinions and elsewhere, and they reappear in discussions with many academic colleagues.

By all the usual standards I had been a good undergraduate student of economics, but I did not begin to think like an economist or think about institutions, incentives and their roles in society until I met Karl Brunner. Brunner has been my teacher, collaborator and lifelong friend. We have worked together, and talked about so many things that it is not surprising that we often think alike. What is surprising is that we often think differently. The benefits of collaboration come in no small measure from ability to use what we agree upon to resolve many of our disagreements. We have also learned to use, and rely on, each other's comparative advantages to gain from exchange.

When I first met Brunner, he was in his second year as an assistant professor at UCLA. I came to UCLA by chance. I was married and living in Los Angeles. I don't recall that my wife and I discussed moving, and I know that I did not reopen the graduate admissions to larger and older schools that had been offered when I completed my bachelor's degree, nearly five years earlier.

At the time, UCLA's Ph.D. program was small and relatively new. I learned, or relearned, price theory as a teaching assistant in classes for William Allen and Warren Scoville and by talking with fellow graduate students, especially Walter Oi and Ted Balbach, after taking Armen Alchian's course. Alchian later became an outstanding classroom teacher but, at the time, he showed little interest in teaching graduate students. He did manage to stimulate us with his questions, but at the time, he showed little interest in teaching graduate students. He did manage to stimulate us with his questions, but he did little to help us learn to think about the answers. Perhaps that was his method. His questions drove the student toward fundamental issues about the price system and institutional arrangements. Like most graduate students, we were more interested in analytic technique. We did not know until later that he relied on Milton Friedman's (then unpublished) notes on price theory. All we knew was that there was little relation between his lectures and the assigned readings, so it fell to the graduate students to try to understand his lectures by pooling our very incomplete knowledge of economic analysis. That was how we learned. It may not have been efficient but learning rarely is.

Brunner was the very opposite of Alchian. He would stop by the room where all the teaching assistants had desks and invite us to join him for coffee. These invariably became sessions devoted to some question to which we applied economic analysis. One had to make the analysis explicit. These sessions and the coursework taught me to analyze first and apply value judgments or preferences later. It sounds simple, but it is rare and requires discipline. Much easier and far more common is to find arguments that support a conclusion one likes for other reasons.

At the time Brunner was more or less a Keynesian, and his graduate class developed versions of that model and taught us to squeeze out implications and compare them to the facts that we could assemble. It was not so much what he taught as his dedication, enthusiasm and the repeated demonstrations that economic analysis is applicable to real world problems that attracted me. When some students became interested in working through Paul Samuelson’s *Foundations of Economic Analysis*, Karl ran a night seminar to help us. With his remarkable and indefatigable wife, Rosemarie, he welcomed the students and encouraged them to think analytically about a wide range of problems. History, logic and philosophy were of particular interest to him, but Rosemarie’s interests included dance, art, religious beliefs, psychology and much more. To this day, he remains open to new ideas, willing to reexamine old ones and thoroughly committed to economics as a discipline and as a policy science. I learned much more from him than monetary theory and macroeconomics.

Karl urged me to begin systematic study of mathematics, and he laid out a program that would have kept me studying at UCLA for years. That was not my plan or program. I saw a

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1 Karl Brunner died in May 1989, long after this was written. I have not altered the text to reflect this sad fact.
Ph.D. degree as a license to learn on one’s own. What other way can one learn? In 2½ years I completed the economics courses, language and minor requirements and comprehensive examinations, applied for and received a Fulbright scholarship and a grant from the Social Science Research Council and left for France with wife, infant son, a pile of books and a thesis topic.

Brunner had become interested in the theory of the supply of money, and I had a strong desire to spend a year in France, so we combined the two into a thesis topic on the determinants of the stock of money during the wartime and postwar inflation in France. I had no idea whether I liked research and was somewhat vague about how to go about it. Research began only after one completed coursework, an odd and inefficient way to train researchers. Later, at Carnegie, I helped to develop a different model in which training for research begins early. This way students have a better idea about what an academic career entails and faculty have some information about whether a student has an aptitude for research.

Chance worked for me again. I enjoyed both the problem solving aspect of the thesis, the search for data, and seeing the hypothesis prove to be fruitful. By choosing monetary theory and developing an interest in inflation, I had chosen a specialty that soon became an important research topic and a major policy issue. My interest in money and inflation, both as areas for thesis research, affected my views about government and about the uses of discretion. Early postwar French prices were controlled, and wages were indexed to prices. On occasion the government of the day would remove items from the price index or make large imports of items that rose in price to reduce the increase in prices and hence in wages. Payments for the imports were financed by printing money. To my surprise socialist ministers not only did not object to these schemes, often they proposed or implemented them. (Years later, the Brazilian government made a similar effort to control wages by controlling the reported price index instead of choosing policies to control inflation. Perhaps this practice is more common than I know.)

This experience, and several others, reopened issues about institutions and incentives. I did not have a compact way of thinking about issues of this kind, but I believed they were important and I was intrigued by them. I began to look for ways to structure problems of this kind. Among the books I read, much later, Hayek’s *Constitution of Liberty* and Popper’s *The Open Society and Its Enemies* helped me most to appreciate the importance of institutions or rules that restrict arbitrary actions of government. I came to believe that the large difference between rhetoric and reality which I had encountered in the Wallace campaign, in France and elsewhere was not accidental and, unfortunately, not a rare occurrence.

An early consequence of this way of thinking was that I did not develop much interest in that part of economics where the market is the only institution. I recognize the value in formal analysis of an Arrow-Debreu economy or other efforts to develop axioms. Standard price theory produces implications that are useful, indeed indispensable, for predicting observable outcomes. Yet, neglecting institutions must have some cost.

The University of Pennsylvania offered me an entry level job while I was in France. The Penn economics department was then in the Wharton school, but the department’s mission did not include macroeconomics. That course or subject belonged to the finance department. I was hired to teach nine hours a week of principles and money and banking. To supplement my modest salary, I was given a night course to teach.

The Penn department was an unexciting place. Most of the faculty had graduated from the school, and several had never been anywhere else. The department was long on pretensions about being part of the so-called Ivy League and, with a few exceptions, short on research or a desire to excel. My experience at Penn convinced me that it is a good rule to diversify by restricting the hiring of your own students at least until they have established their independence.

Grandmother’s lessons and an extremely supportive wife helped me through the year. I worked hard to finish my thesis, grinding out regressions every night on a home calculator. The computer had been developed at Penn, but it was not yet widely used to process data, so I
and everyone else) inverted matrices and computed coefficients by hand. The department had two hand calculators, mainly used by the chairman and his assistants. I rented my own and worked at home whenever possible, a habit I have retained ever since. When my thesis was far enough along to be described as "almost finished", I wrote letters to other schools offering my services.

This time good fortune came in the form of a telephone call from Lee Bach, then the Dean of the Graduate School of Industrial Administration (G.S.I.A.) at (then) Carnegie Tech. By the end of summer 1957, I had defended my thesis and moved to Pittsburgh.

The move was a good one. At the time, G.S.I.A. had a lively and stimulating atmosphere and I believe it has remained that way. I have visited schools where faculty do not exchange ideas until they are ready to be published. G.S.I.A. was not (and is not) one of them. There are schools where faculty have to have the "right" view to be promoted. G.S.I.A. has not been one of them. Sometimes junior faculty have to apprentice to senior faculty in their field. G.S.I.A. has largely avoided both fields and apprentices. When I came, the atmosphere was open; you were expected to be enthusiastic about what you were doing and to be able to explain it to others. With rare exceptions, seminars were not trials by combat or efforts to show off. They were intended to be—and often were—efforts to help the author improve his paper. Several young faculty, who later became distinguished professionals, have told me that it was this atmosphere that attracted them to G.S.I.A. when they were interviewed at the start of their careers. I know it helped me to develop my ideas.

The original conditions owed much to the young Herb Simon and Bill Cooper. Their direct influence waned long ago, but the tradition continues. The remarkable record of achievement by faculty and students who have come, and in many cases gone elsewhere, speaks to the stimulus that others have found in the G.S.I.A. environment and that they, in turn, helped to foster. The small size of the school and the school's commitment to interaction across disciplines probably contributed to the stimulation. Some rather unique and time consuming procedures for faculty review and promotion helped to maintain high quality. Now that the school is larger, and cross-disciplinary research has declined in relative importance, the valuable local public good—the outstanding research environment—may not survive as in the past. The tradition is well established, however, so some of the tradition is likely to remain for a long time.

The contrast between G.S.I.A. and Penn was so marked that I can recall after more than thirty years my heightened sense of excitement. The school was relatively new—less than ten years old—when I arrived and very conscious of its "Mission". By reciting achievements of the school and the faculty, Lee Bach communicated that the aim was excellence, that the small faculty of 25 or 30 was to be innovative in education for business and economics and in research, that the tiny school in a small and not very distinguished technical university was going to change management education. With some help from the Ford Foundation, he managed to do just that. It fitted well with some of grandmother's teachings and my personal objectives.

Some of the excitement vanished long ago, perhaps the inevitable result of the professionalization of management education that the school set out to achieve, perhaps the result of increased size and the influence of professional disciplines, as Herb Simon insisted in the early days of the school. In my experience as a visiting faculty member at Harvard, Chicago and other universities, I have never found an atmosphere that matched my early years at G.S.I.A.

A Research Program

Research, like other types of learning, is mainly an individual activity. Perhaps that is why the author is often the one who learns most from research. Even collaborative research, of which I have done a lot, requires that someone write out the results and someone revise what is written. Often this is the time when I understand most fully the implications of what I (or we) have done and the price paid to get the result.

When I was a graduate student, I wondered about how people decided what was a researchable topic. This concern must have stayed with me early in my career because I would write
down and file potential topics. I don’t recall ever looking in the topics file, and soon I stopped adding new material. Years ago, I threw away the contents.

Much of the appeal of academic life to me comes from the large amount of freedom to work on subjects that interest me. There is no apparent limit to what people can convince themselves to believe. I enjoy finding out whether data support my hypotheses or whether a conclusion follows from established hypotheses without too many restrictions or auxiliary assumptions. Mostly, I am interested in work that has some bearing on the way the world works. I am, unashamedly, part of the generation that was attracted to economics as a discipline that can be used to improve welfare. Over the years, experience and research—my own and others’—has altered my view about the role that economists can play, but I have never changed my belief that economics is a policy science, not a branch of applied mathematics. Economics will be poorer if it does not include institutions and the incentives embodied in the rules, institutions or arrangements that we call society. Economics as a policy science includes both formal models and the analysis of institutions and their effects.

Much of what passes for research and fills the journals, even the quality journals, seems remote from the development of economics as a policy science. One person gets a result. Another finds that it does not hold under some change in assumption. Neither may have much to say about the world we inhabit. It is a poor excuse, though probably correct, to point out that much of what appeared in journals in the past did not contribute to the development of economics as a policy science. Economists are not the group to defend inefficiency.

Concern for policy does not mean that every piece should carry its own message. Foundations are necessary in any discipline. Economists have done much good work to strengthen the foundations of the discipline by working out the conditions under which standard propositions hold. Part of being an expert is knowing more than others about the qualifications and restrictions on the realm of application. At one time, economists often included in their papers statements about what happened when one “relaxed the assumptions” used to obtain sharp implications, but the practice has gone out of fashion. It is a pity; “relaxing the assumptions” should include taking into account institutions and the types of incentives that a policy proposal generates or neglects, and the effects of assuming that it is costly to acquire information.

Looking back, I cannot be certain when these ideas began to affect my research program. By the mid 1960s, Karl Brunner and I had written about the gap between economic analysis and policy actions and, by the end of the 1960s, we were thinking more formally about the role of institutions and costs of information. Some of these ideas eventually found their way into a paper on “The Uses of Money”.

That was not where I started, however. At first, I tried two different ways of doing empirical work combining institutions and economic theory without examining why the institutions developed their form. One proved to be more fruitful for me than the other, again partly a result of chance.

The less fruitful path, for me, was stimulated by the work at G.S.I.A. on the behavioral theory of the firm. With a graduate student, I took protocols—detailed, step by step notes—on how an experienced professional investor made his decisions. Then we simulated his decision by writing down his rules of thumb. Although we published a paper explaining the method and giving the results of our simulation, and my collaborator, Geoffrey Clarkson, later won an award for the work he expanded into a dissertation, I was not attracted either by the method, or its possibilities in finance, or by the subject matter.

The other approach started with an empirical study of the way trade credit changed the allocation of loans. Critics of monetary policy had used banking data to show that banks denied loans to smaller firms when credit contracted. I was skeptical. The banking data did not take account of the allocation by the borrowers. I found that lending by large firms to small, through the use of trade credit, was one important, though perhaps costly, means of reallocating the outstanding stock. This paper influenced my thinking about financial markets, institutional arrangements and the many, at times important, aspects that could not be captured in standard models such as IS-LM. The paper helped to persuade Eli Shapiro, then
working with Bert Fox to organize research at the Commission on Money and Credit, to support some additional work and, more importantly, to recommend me later to Senator Paul Douglas, then Chairman of the Joint Economic Committee, as the person to lead a study of the dealer market for government securities.

The study of the dealer market (1960) gave me and my colleague Gert von der Linde, whom I invited to join in the study, a valuable introduction to market institutions. We interviewed many of the dealers and learned a great deal about the market for government securities and the first steps in the execution of monetary policy and other Federal Reserve operations. The study attracted the attention of Robert Roosa, then a vice president at the New York Fed who soon after became Under Secretary of the Treasury for Monetary Affairs in the (new) Kennedy administration. Roosa invited me to be a Treasury assistant. It was my understanding that I would attend policy meetings to find topics on which research could be helpful to the Treasury. I had the foresight to ask him to agree that I would not answer letters or do other routine tasks.

When I arrived in June, full of enthusiasm, energy and anticipation, I found that either there was a misunderstanding about my job or a change in the (unwritten) contract. My first assignment was to respond to a letter from a Congressman. Roosa declined to invite me to policy meetings. I resigned at the end of two weeks. Lee Bach supported my decision, urged me to leave on good terms, and welcomed me back to G.S.I.A. The Treasury made me a consultant. For several months I commuted to Washington for a few days every other week to work on the taxation of municipal bond interest. David Ott was doing a complementary analysis at Brookings. We joined our two studies in a book published as part of the Brookings study of taxation. After a while, I stopped going to the Treasury.

My experience in government lasted long enough to raise the issue about why governments act as they do. The Kennedy Treasury had "positions" that it did not choose to examine, including the importance of defending the dollar. None of my superiors wanted to consider whether it was good policy to "defend the dollar" by raising interest rates during a recession. The U.S. had large reserves relative to its trade deficit. Why not let reserves flow out during the recession and return during the next expansion to maintain money growth and moderate the recession? Nor did they wish to analyze whether efforts to use debt management to raise short rates and lower long rates had any effect on either the rates or the economy. I cannot claim that I understood why these issues could not be analyzed or even discussed, but I recognized that the Treasury was a much more restricted environment than the staff of the Congressional Joint Economic Committee, where I had worked on the study of the dealer market in my previous experience in government. It was clear that "political constraints" restricted not only the choice of policies but even discussion. I tried to think about what the constraints were, where they came from and how they affected the choice of policies.

Congressman Wright Patman had been the force behind the study of the dealer market. Although our conclusions about the market did not confirm his prior beliefs, he invited me to undertake another study of the dealer market for the House Committee on Banking and Currency. I persuaded him that the problems that interested him lay not in the dealer market but in the Federal Reserve's method of conducting policy. He agreed that the study should concentrate on the Federal Reserve.

A few months earlier, Karl Brunner had suggested that we begin to work together to write a textbook. I had published enough independently to be able to work as a colleague. We agreed that we would not write a textbook until we had done much more to fill in some of the empirical gaps in monetary economics. I began work on the demand for money and Brunner continued his work on money-supply. We never wrote the textbook.

Patman's invitation struck me as an exciting opportunity to learn more about the supply of money and the Federal Reserve's methods of controlling money and making decisions. I invited Brunner to join me, and he agreed immediately. He was still at UCLA, so many of the interviews with dealers, bankers and Federal Reserve officials in Washington and New York fell to me and to David Meiselman who had joined the staff of the Banking Committee. During the summer Brunner and I conducted
Typically, money growth rose (relative to trend) objectives of full employment without inflation. Policy actions were only loosely related to the used to control money was so inadequate that turning points in the economy, but the process procedures. The Federal Reserve staff detected made no effort to achieve better control money market to study the effects on economic activity and prices.

The study and what we learned in the process greatly influenced the work that Brunner and I did on targets and indicators, on monetary control and monetary policy, on money and intermediation and on the relation of money and debt to income and prices. In our view, research on money that would be useful for policy decisions had to be rich enough to be able to answer questions about institutional structure and regulation. We saw our work as fitting between the large-scale econometric models, with their emphasis on details and concern for aggregative effects of allocative patterns, and standard macro theory in which money was a given denoted M. I think we made the right choice, but I am convinced that we did not persuade most of the profession. Discussions of intermediation and monetary policy most often still use models with one asset or use a large scale econometric model to simulate the responses. Discussions of Federal Reserve actions typically assume that the Federal Reserve’s objective is to maximize social welfare.

Two of the main conclusions of our study were that the Federal Reserve relied on unreliable control procedures and that they had made no effort to achieve better control procedures. The Federal Reserve staff detected turning points in the economy, but the process used to control money was so inadequate that policy actions were only loosely related to the objectives of full employment without inflation. Typically, money growth rose (relative to trend) in periods of economic expansion and declined (relative to trend) in recessions. Monetary policy was procyclical, adding to problems of inflation and recession in the economy.

The study produced little response from the Federal Reserve. Internal memos commenting on the study did not refute the main conclusions; a seminar on the study at the Board drew a large audience but not much comment or criticism. Later, when the Federal Reserve changed procedures, it did nothing to eliminate procyclicality; the change was from control of free reserves, which we had criticized, to control of a short-term interest rate. A consequence of the short-term focus and reliance on interest rates as an indicator of the current policy stance is that the Federal Reserve produced the inflation of the 1960s and 1970s not by design but by misinterpreting the degree to which policy was expansive. They did not give up reliance on interest rates as an indicator of policy stance until 1979 when, under pressure from foreign governments, they finally adopted and maintained a policy to reduce inflation. They shifted to a measure very similar to free reserves and, in 1982, with inflation reduced, shifted back.

The Federal Reserve study and subsequent events had two lasting effects on my attitudes toward government. First, the Federal Reserve had made major contributions to two debacles—the Great Depression of the 1930s and the Great Inflation of the 1970s. Both were costly to the U.S. and the rest of the world. Yet, there was no public outcry. Neither the Congress nor successive administrations seemed concerned about the Federal Reserve’s role in these failures. Congress and the public did not consider alternative arrangements to reduce errors in the future. Second, my skepticism about discretionary policy increased. If an agency of government that I knew well performed as poorly as we had found, why would other agencies be different? Subsequent experience with regulation Q, the management of deposit insurance and housing programs increased my skepticism about activities with which I was most familiar and, of course, about other areas as well.

Governments often seemed to commit human and material resources without a clear long-term objective. When short-term objectives were not met, additional resources were often committed to avoid the appearance of failure. During my interviews on the West coast and began work on the study.

I was surprised at how little research the Federal Reserve had done on the monetary mechanism. Many of our questions were about basic issues such as what they controlled, how they decided to act, or how their actions affected economic activity and prices. Many of their answers came as analogies with different analogies favored by different Governors. A set of questions about operating procedures and regular policy actions required a system-wide committee to agree on the answers. In its first fifty years, the Federal Reserve appeared to have done little to learn about its mission and the effects of its actions. The focus was, or seemed, dominantly short-term and did not get beyond the money market to study the effects on economic activity and prices.
brief stay in the Treasury in 1961, a small incident left a deep impression. A businessman, serving as an intern in the public service, was studying the drug problem, part of the Treasury's responsibility at that time. He was amazed that there was no long-term program to achieve some set of objectives. He found neither the program nor the objectives. All planning was tied to the short-term budget cycle. Later, I realized that much the same could be said about the lack of objectives and of strategies to achieve objectives in actions to prevent or reduce inflation, the so-called war on poverty and many other activities including, apparently, waging war.

This absence of long-term objectives and operating strategies characterizes many activities, private as well as public, civilian as well as military. The private sector has more effective corrective mechanisms and, therefore, greater incentive to correct errors. One reason is that more of the costs of private mistakes are borne privately by those who undertake the risk or those who make the mistakes. Can these costs be incorporated when comparing the two sectors?

At the time of the Federal Reserve study, many views about limiting discretion were not well formed. I knew about proposals for rules and had read some of the literature on rules and discretion. Although we were critical of the way discretion was used, I don't recall any discussion with Karl Brunner about a proposal for a monetary rule as part of our study.

The skepticism that grew out of my limited experience with government was reinforced by some main implications of the literature on public choice that was emerging at about the same time. Lingering early beliefs about the role of government withered. I did not become an anarchist; I recognized that there must be a role for collective action. What was that role? What should it be? The standard literature on public goods did not give satisfactory answers. Governments engaged in many activities that could not be described as public goods without removing all meaning from that term. Theories of optimal taxation seemed to be divorced from actual tax systems. Theories of optimal tariffs and regulation and much of welfare economics, whatever their other merits, do not explain either what modern governments do or how they choose policies. The gap between research and practice is much too wide.

In the social sciences, it is said, ideology or beliefs influence the problems people choose to study, or the way in which they proceed. This is at most a partial truth but difficult to reject since we observe decisions and can only speculate about motives, beliefs or philosophy. However, I don't believe that I began work on the size of government to support any normative view.

A number of different influences, in addition to those I have mentioned, combined to shift some of my interest from monetary theory and policy to forces determining the size of government. Data on the growth of public expenditure or tax revenues showed that these measures of the size of government had increased faster than broad measures of aggregate output in all developed countries during the postwar years. Why was this so? Discussions with political science colleagues suggested that there were only a few papers but no generally accepted answer. In the early 1970s, Brunner started the annual Interlaken Seminar on Analysis and Ideology where broad issues of this kind were discussed. In 1974, I was invited to give a lecture at a law school as part of a series on the role of government. My first effort was, in fact, an attempt to work out my own normative view. In the process of writing, I realized that there was nothing I would say that had not been said, probably better, many times, so I tore up the draft and tried a positive approach. Since transfers or redistributions have become the largest part of government spending, I concentrated on that.

The main idea in the explanation I offered is that benefits are concentrated and taxes diffused. My colleague Scott Richard later criticized that idea from a rational expectations view. It relies on myopia to a degree. Voters can know or learn that benefits will be concentrated and costs diffused even if they do not know the details of a particular program. What prevents them from learning that the taxes they pay are more or less than the value of the services they receive? What prevents them from learning that the difference between the costs and benefits to the individual of most spending programs rises with income?

I found the questions hard to answer. Together, we developed what we called "A Rational Theory of the Size of Government".
Voters are not myopic. The size of government is determined by a median voter who recognizes that taxes reduce effort. He compares the loss he experiences because taxes reduce his own and others' efforts to the benefits he gets from redistribution.

"A Rational Theory . . ." explained the size of government as a result of voter choice and the difference between the uniform distribution of votes and the skewed distribution of income. Earlier work often invoked special interests or institutional arrangements in particular countries. These explanations do not give a common explanation for the increased size of government common to many countries in the postwar. They do not explain the failure of conservative parties to reduce government spending relative to broad measures of output when they are in office or why voters do not learn about the redistributive pattern.

Research opens new issues to analysis. A puzzling issue of long standing is why the public chooses to have governments engage in providing services that can be produced competitively. I have never encountered a claim that governments produce health services, education, or other services more efficiently than a private market. The aim is nearly always redistribution of opportunity or cost. Redistribution of the cost of purchasing these services can usually be achieved without having government involved in production or distribution. Much the same can be said about housing. Yet, governments own and operate rental housing in most countries. Why do voters not oppose these programs? If voters choose to redistribute income in kind, why not give cash or vouchers to be used for payment? Richard and I extended our work to explain in-kind transfers. Later, Alex Cukierman and I extended the model to explain intergenerational redistribution and other commonly observed patterns.

Some of the implications of this work affected my beliefs and actions. If, as our work implies, the size of government depends mainly on the distribution of income and the voting rule—modified majority rule with universal suffrage—there is not much reason to believe that a few individuals would have much effect by serving in government. They may be able to change a few details, but major changes are unlikely. Politics is mainly about income distribution—who pays and who receives. One party will talk about equity and justice; in practice, they will seek more redistribution for their constituents and higher taxes on upper income groups. The other party will talk about incentives; in practice, they will favor less redistribution and lower taxes. An equilibrium that appeals to the median voter keeps the tax share fixed. Efforts to achieve a particular agenda by working in government will not have large effects unless there is a change in the distribution of income, the voting rule or some other relevant determinant of the process. I lost much of my early enthusiasm for trying to improve policy or increase efficiency by working in government.

My interest declined also in the normative question that excited me earlier: What should governments do? Countries have established institutions or arrangements to answer that question. I became more interested in research about how arrangements such as voting rules influence the choices that voters or officials make and how the institutions are chosen or evolve. Economic analysis applied to these problems seems far more powerful than the ideas I learned from Grandmother, or the goodwill theory of governments or the fiction that government is mainly engaged in supplying public goods. When we understand better the consequences of various arrangements, and why the arrangements persist, we may be able to address broad issues about optimal arrangements in economies with individuals who vote and spend.

Informing the Public and Improving Policy

Although my interest in normative questions has declined, it has not vanished. Academic research can contribute to the policy process by analyzing existing rules and procedures and proposing improvements. The improvements may not be the ideal point chosen in a free election by informed voters, but they may increase welfare.

Voters are not fully informed about the consequences of alternative rules or procedures. Neither are economists or officials. Acquiring information is costly. Every teacher who grades exams learns that there is a difference between providing and acquiring information. Surely, the
same problem arises in society where the incentives to acquire information about public policy are weak. Economists who have studied an issue can contribute information about alternatives.

There are times when policy changes can be made. Major changes in policy often occur in periods of “crisis”. A crisis, within government, often means little more than a strong belief that prevailing methods or rules do not work or have become costly. The public may have lost confidence in a program or a governing party, or Congress may complain about the side effects of a policy and want some change, so there is a perceived need for change.

The breakdown of Bretton Woods, public criticism and avoidance of the military draft, and rising inflation are three examples of perceived crises. In each case, previous research and discussion had made a case for an alternative. Objections and criticisms had been answered, if not completely, at least in a way that clarified the problem and increased understanding of the choice.

When the perceived crises came, the options were understood. Instead of exchange controls and trade restrictions, the U.S. and other countries adopted fluctuating rates. The volunteer army replaced the draft instead of a lottery or some other scheme. The Federal Reserve allowed interest rates to rise substantially and partially changed procedures. Thus, research influenced practice and the choice of rules. No one can be certain that the outcome would have been different without years of research and public discussion, but I believe the research and discussion changed the outcome for the better in these cases.

Some economists are critical of efforts at public education carried on by those of us who speak in public forums and write in the public press. I am not one of them. It is true that one must leave out qualifications and possible exceptions when addressing public issues in a public forum. This involves making a judgment about the main conclusion and the importance of the qualifications. Some feel exposed and uncomfortable making these judgments.

I respect the need for care and not overstating the case. The risk that anyone will persuade by overstatement seems small, however. Responsible officials seek not one, but many views. There is a competitive market in ideas and recommendations. Officials often use this market to delay or avoid change by finding “experts” on different sides, thus showing that opinion is divided or making a case for their policy or for their views.

Critics usually have the advantage of hindsight. With others who share some of my views about the role of experts, I have joined to organize groups like the Shadow Open Market Committee and the Shadow European Economic Policy committee to criticize policy actions and to propose alternative policies in advance. There is a discipline that comes from choosing policy actions after considering the facts, the uncertainty about what has happened and about what the future may bring. There is always much relevant information that we do not have, and cannot know, about the duration of shocks, the reasons for observed changes and their future consequences. I have learned that even the best forecasts of economic variables are subject to errors that are large and, on average, not very different from the average change. The relatively large errors of forecast, and the realization that the errors cannot be reduced using the methods or models have been more persuasive for me than any theoretical arguments about rules and discretion.

In the present state of knowledge, I have become convinced that activist, discretionary policies are more likely to increase than to reduce variability and uncertainty and lower welfare, so I have become a proponent of policy rules that do not rely on forecasts. My beliefs about the appropriate role of government evolved out of experience, in response to research findings and by looking at evidence. In this area, as in others, it is difficult to understand fully why I accepted the views I once held. The account I have given is almost certainly incomplete, but it may convey why I find it difficult to set out a “life philosophy.”