George A. Akerlof

The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel 2001

Autobiography

Family background

I was born on June 17, 1940 in New Haven, Connecticut. My father was a chemist on the Yale faculty, my mother a housewife. They had met ten years earlier at a departmental picnic when my mother had been a chemistry graduate student at Yale. My brother, Carl, was two years older. My father, who was born in Sweden in 1898, had come to the United States on a fellowship to obtain a Ph.D. at the University of Pennsylvania. When his thesis adviser received an appointment at Yale in 1928, my father followed, and continued up the career path as instructor, assistant professor, and associate professor. His own roots were partly in Dalarna, which was the ancestral home of his mother's family, and partly in Stockholm, which was his father's home. My Swedish grandmother was the daughter of a dairy farmer who lived near Hedemora. My Swedish grandfather worked as a clerk for the Swedish railways in the Stockholm station. His avocation was painting, which absorbed more of his psychic energy than his career. At least some of the murals in the Stockholm station are a remnant of his handiwork. Beyond this my knowledge of my Swedish heritage is not expansive. Partly this reflects my father's move to America in an age when travel was both time-consuming and expensive and therefore I lack first-hand knowledge. But it also reflects his taciturnity and also his scorn for history in all forms, even at the family level. He considered himself to be beyond all else a scientist.

On my mother's side of the family, I know a great deal more, partly because my grandmother and some of her relatives were present in America at the time of my birth, but also because my mother made up for my father's taciturnity by her loquacity. She also believed in family history as a lesson to her children. Thus her accounts included the good parts, but omitted the bad parts. For example, I only heard late in life that in the late 19th century her grandmother's father had gone from San Francisco to Sacramento for a day trip on a steamboat, and never returned, his disappearance always a mystery. My mother came from an academic family. Her father and mother were of German Jewish descent. The practical implications of this ancestry for my grandfather was that he was denied tenure at Johns Hopkins, where he had established the first clinic in cardiology in the United States. A man of wide interests, he changed fields. He had a deep interest in the applications of chemistry to medicine and so he accepted the chairmanship of the department of pharmacology at the University of Minnesota medical school. My grandfather's German Jewish ancestry also had cultural implications. My greatgrandfather had been born in Oakland, California. He graduated from Berkeley in 1873, then returned to Germany for his medical education; he became a pharmacological chemist and was professor of medicine at Cooper Medical College in San Francisco, later the Stanford Medical School. The tradition of chemistry established by my great-grandfather was maintained for three generations. Like his father, my grandfather also attended Berkeley, graduating at age 18, and went on to Germany for his medical education. As I already mentioned he became a pharmacologist, as well as a cardiologist. In turn he passed the chemistry tradition on to his children. My mother, whose interest in chemistry was rather minimal, nevertheless went to graduate school in the subject, and married my father, for whom it was as important as life itself. My mother's brother became professor at the University of Wisconsin and was one of the best-known physical chemists of his day, having been the lead author of a massive green tome entitled: *The Molecular Theory of Gases*. Being a chemist or, at least some form of physical scientist, was thus a family ideal, for my brother and for myself. My brother Carl became a physicist; I became an economist.

My US grandmother is more peripheral to the story. She came from a previously wealthy family which had fallen into hard times. Her grandfather had advanced from peddler with a horse cart to being one of the richest men in the state of Maryland, a fortune which was divided among his 12 children and then lost by my great-grandfather. He died not in penury, but in debt, perhaps to the tune of $500,000. He had loaned considerable sums to brewers who went bankrupt with the innovation of pasturized beer. The appearance of my grandfather as suitor to my grandmother in such circumstances was thus particularly

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Early years

Although the early pictures of my youth show me as happy and smiling, my mother assures me otherwise. I was considered to be a sickly child. Perhaps it was true. School began in Pittsburgh, Pennsylvania. My father had not received tenure at Yale; during the war he worked on the Manhattan Project in Dayton, Ohio; afterwards he transferred to the Mellon Institute in Pittsburgh where he was supported by the Koppers Corporation. Thus my first years of schooling were in Pittsburgh at the Shady Side Academy, a fine private school. I was, however, dismissed from kindergarten, not for misbehavior, or for academic failure, but rather for throwing up in school. My brother was an exceptional student, and I think that this may have caused the school to take some pity on me, since they allowed me to return to first grade, only with a special place at lunch away from the other students, a precaution which was unnecessary since by this time my stomach problems had subsided. I was not only the physical runt of the litter, but also the intellectual inferior of my brother. I always knew this from experience, and it was scientifically affirmed for me when I was 7 by an IQ score that was three points higher. In Pittsburgh, my father made a habit of taking my brother to his laboratory in the Mellon Institute on Saturdays. I remember going there a few times, but the last time I went there I burned myself blowing glass. I did not want to confess my foolishness, but I also did not want to return. To the best of my knowledge I never returned to his laboratory as a participant; only as the outside child observer. But my brother and father continued their weekly visits, and as time passed my brother became the scientist. Relative to this two-some I was an outsider who failed to understand the mysteries of the world; no doubt they were right. This left for me the task of finding an identity for myself. I thought about things that did not interest them. I was interested in social things: history and, if children can have such interests, economics. My family, not knowing what such people could do for a living decided that I was going to be a lawyer. Yes, I would go to Law School. That was their view of what a non-scientist might do that would permit some modicum of self-respect. I also found another way to establish a niche for myself. My brother did well in school, but I found that I could do better. After I was re-admitted to Shady Side I discovered that I could be first in my class, which gave me an identity in school as well as at home. These early years then were partly defining, but they were comfortable and happy. I liked my school, and also my mother would migrate from Pittsburgh to New Hampshire to escape the heat of the summer taking along me and my brother and my grandmother (my grandfather having died in 1942). For the first six years we rented cottages on Squam Lake in New Hampshire; then my family built one of our own. There were very few other children, so I had my brother as a sole companion. We did the types of things that brothers tend to do when they spend the summer at a lake: use of the motor boat, swimming, some badminton, and following along in the rounds of my mother and grandmother as from farm to farm they searched for the best tomato in the state of New Hampshire. I remember enjoying reading books about animals that talk, such as the Freddy books about Freddy the Pig, and Thornton Burgess' animal stories. I also remember being terminally bored when there was no such book available and my brother was engaged in his multifarious projects. I participated as the person who would look for the hammer when it was needed (and also who invariably would not find it, so he had to go for it anyway).

The Pittsburgh part of this life ended when I was ten. Koppers Chemical had decided to terminate my father's contract at the Mellon Institute, and so he was sent packing. With the support of the Office of Naval Research, he relocated to the Naval Powder Factory at Indian Head, Maryland, about 40 miles south of Washington. My mother felt that the schools in Indian Head were not of sufficient quality for her boys, and therefore a compromise was reached. My father would work at Indian Head during the week, and the rest of the family would live in a rented house north of Washington. My brother and I were sent to Sidwell Friends School in Washington. This sojourn did not last long. In the spring of 1950, my father showed us some titanium rings that he had produced by his own method of electro-discharge chemistry. And Princeton University, which was opening up the Forrestal Research Laboratories, hired him.

And so my family moved again, this time to Princeton. The University gave us one half of a huge colonial house. My brother and I went to a small private school, about a mile away, the Princeton Country Day School. The school was old fashioned: it was all male, even the teachers, and had an emphasis on classical education. Since my brother would have been quite behind in Latin, it was imperative that he be absolved of this requirement. My mother and father thus entertained the principal at dinner, with unusually strong martinis, and afterwards successfully pleaded my brother's case. My father felt no regret that Carl would not be learning the language of Roman Civilization. Science was not emphasized in the school. When it was introduced, partly at my family's urging, for eighth graders, the math teacher, who had majored in French at Princeton, made the students memorize stars in constellations. The conflict between my brother and this math teacher regarding what one considered astrology and the other considered astronomy had some fallout even on me, as I learned when he told me that he had been reading books of handwriting analysis and he was certain that I had the handwriting of a murderer, a prediction which has so far proven incorrect. Aside from that I had relatively little conflict with teachers. I belonged to a small group of students, who in today's terminology would be called nerds. Bikes gave us a great deal of freedom, not only as transport back and forth to school, but also to get together with friends who lived at considerable distance from home, and also for such entertainment as touch football and the movies. I formed a close friendship with Robert Fernholz, who later received his Ph.D. in math. School was out early on Wednesday afternoons and we would often rent a canoe to explore Lake Carnegie. We had our own games of touch football, our own movie group, and went out for tennis rather than

baseball in the spring. We were differentiated, for the most part, from those who were richer and also more athletically inclined.

The idyllic life in Princeton in the large colonial house was, however, broken after one and a half years. My family would continue to live in Princeton, but in at least subtly different circumstances. At Indian Head, and after coming to Princeton, my father had never been able to reproduce his previous feat of the titanium rings. After the initial success the equipment had broken, and Humpty Dumpty could never be put back together again. So my family had to move. They decided to stay in Princeton, where a consortium of Princeton scientists set him up in his own research laboratory. It was supported by government research grants. It was at this time, when I was 11 or 12, that I remember one of my first significant thoughts about economics. If my father lost his job, and my family stopped spending their money, then another father (it was typically fathers rather than mothers who worked at the time) would lose his job, and so on. The economy would spiral downward. Well, as I have told it my father got re-employed, so the system was not put to a test. Although slightly wrong, I had understood the foundation of Keynesian economics. The exploration of the reasons for unemployment and the defense of Keynesian macroeconomics have dominated my work as an economist. It is thus no coincidence, perhaps, either that I had made this observation, or that I now remember it.

The Princeton Country Day School ended at grade nine. At that point most of my classmates dispersed among different New England prep schools. Both for financial reasons and also because they preferred that I stay at home, my family sent me down the road to the Lawrenceville School. Indeed the house my parents built after my father left Princeton was on the Princeton-Lawrenceville Road, so that my brother and I could hitch hike easily to school and back. Again this was an excellent school. Almost all classes were in sections of 10 to 15; sometimes advanced sections with low demand were smaller. Students and faculty arrayed themselves around large oval tables. By skipping a year of math and a year of French, I entered college with advanced standing in English, history, math, Latin and French. This advanced standing was very useful in my college career, especially the advanced standing in math. The teaching was of the highest caliber. My French teacher in tenth and twelfth grades had won awards for his excellence in teaching. One of my English teachers was a leading scholar renowned for his work on Emily Dickinson. Socially, I was a misfit. I failed to understand why my classmates spent the typical free afternoon watching *American Bandstand*, a TV program of teenagers dancing. Nor did it help that I was the lone day-boy from the Princeton area. As a scholarship boy, I delivered the intercampus mail half the afternoons thereby avoiding the PT (physical training) program that was the athletic dumping ground for those who were on neither varsity nor junior varsity teams. The other afternoons I looked for other ways out: such as visits to the infirmary, use of cum laude privileges to skip class or athletics, etc. I also knew that in a short period of time I would be off to college.

College and graduate school

Regarding college, I had no choice. My brother had gone to Yale. Even if my brother's choice were not over-riding for my decision, I would probably have heeded the assistant principal of Lawrenceville, who admonished me that I should not wreck my life by even thinking about going to Harvard instead. My first two years at Yale were mainly spent in taking liberal arts courses and working on *The Yale Daily News*. My last two years were spent learning economics, and then math. When I went to Yale, I was convinced that I wanted to be either an economist or an historian. Really, for me it was a distinction without a difference. If I was going to be an historian, then I would be an economic historian. And if I was to be an economist I would consider history as the basis for my economics. This interest in history informed my academic program. In my Freshman year I signed up for a rather fuzzy course called Directed Studies, which was said to cover Western Civilization from many different aspects: history, philosophy, art, literature, etc. I also separately signed up for math and economics. The concern with history led to another decision regarding extracurricular activities. If I were going to be an historian I thought that it would also be useful to see how the news is made: the user of documents should also be aware of how the truth is distorted in the making. And so I decided to "heel," to go out for, *The News*. I may also have lied to myself. I may also have gone out for *The News* because I knew that I would enjoy it. Until Thanksgiving of junior year *The News* dominated my life. I wanted to do two things with the newspaper. First, I found it too much of an official organ. A typical prime assignment was for a leading reporter to interview the President (of Yale University) and to report his views. I wanted the newspaper to do something different. I wanted it to have more stories about student issues, and also more features of human interest. I wanted it to be less solemn and more serious. Surprisingly, just one individual reporter could make a difference. For example, I wrote a story protesting Yale's policy of keeping students in Directed Studies for sophomore year if they wanted to get out at the end of freshman year (I myself had been denied). I also wrote many articles which tabulated a questionnaire on the feelings of scholarship students who were forced to work in the dining halls in freshman year. Regarding stories of more general interest, in my sophomore year a friend of mine and a photographer went South at the time of the first sit-ins and covered that for *The News*. We talked to Black and White leaders throughout the South. I also covered the Nixon and Kennedy campaigns. Despite this record, in the beginning of junior year I was denied election to the news board. This was probably the best thing that ever happened to me. I would never have been a good reporter because I am not accurate regarding facts (probably the reporter's worst sin and the probable reason for my denial). Also the time I would have spent in junior and senior year at *The News* would have seriously impaired my education in economics. In some sense my career in economics has paralleled my vision for *The News*. Relative both to the economics of the 1960s and perhaps also to the dominant strand of economics today, I have sought to develop a theory that is similarly more serious and less solemn. I want a

theory that is more closely linked to substantial policy issues and less tied to the official (competitive general equilibrium) model and its assumptions.

At the time of this decision at the *News* I was taking my first course in abstract mathematics, as well as four courses in economics. Because I had gotten a jump ahead in math at prep school, I had somehow avoided any course in which proof was required. I thus found myself failing to understand, and literally flunking, my course in *Modern Algebra*. It was only through the intervention of my family that I survived. My mother's best friend's husband was a leading mathematician at Princeton. He gave me an hour's tutoring; he diagnosed my problem and showed me where my thinking was lacking. After that I was able to make headway in the course, and later got a perfect score on the final. In the first two years at Yale I mainly worked on *The News*; in my last two years I was entirely a student. In junior year I took almost all economics, except for modern algebra; in my last year almost all my courses were in math. This school work would then be the background for the next phase of my career, which was graduate school.

I entered MIT in the fall of 1962, and I was surprised that my background in both math and economics was better than that of almost everyone else in my class. I was surprised because previously I had not been impressed by the economics that I learned at Yale. Indeed my all-math senior year had been as much determined by the pull of the math courses as the push of the economics courses. My good undergraduate background then left me time to pursue interests outside economics. I spent most of my intellectual and psychic energy in my first year of graduate school on a course in algebraic topology taught by Raoul Bott at Harvard. Bott not only taught the details of algebraic topology, but also, much more deeply, how mathematicians truly think. He taught how to divide the meat of a proof from the detail. In this course I learned to respect the variety of mathematical structure that can be used to describe a problem. It bolstered my suspicion that many of the results of the economic theory of the time were due to economists' lack of (mathematical) knowledge rather than to the truth of their arguments. As a crude example, consider the cartoonist Edward Koren's furry animals. In contrast to the traditional clear-lined cartoon, Koren's characters are like fibre bundles, characterized by hairs everywhere. This distinction is relevant to economic theory: the standard economic model is mathematically represented by simple clear surfaces, but alternative models in the spirit of Koren's cartoons are also possible. Solow demonstrated two models of this sort in the course he taught us in growth theory. In the first model the output of labor depends upon the vintage of capital with which it is combined; in the second, capital is substitutable for labor before the capital is produced, but is no longer substitutable thereafter. Solow's models converged with what I was learning in algebraic topology. Together they suggested that standard economic theory was based on mathematics that failed to capture a good portion of economic reality. Richer structure would give a more realistic picture.

Socially, MIT was also a great deal of fun. I made many friends there, including Joe Stiglitz, Bill Nordhaus, Giorgio La Malfa, Joe Mooney, Eva Colorni, Mrinal Datta Chaudhuri, Vahid Nowshirvani, Tom Weisskopf, Steve Marglin, Marcelle Arak, Karl Shell, Mike Rothschild, Dick Auster, Les Aspin, Eytan Sheshinski, and many others.

At MIT at the time everyone learned growth theory. That was the core and center of the curriculum. I learned growth theory not because I had any intrinsic interest in the subject, but because it was there. The best of it, like Solow's models, and Arrow's "learning by doing" model were very interesting; after that most papers were rather mediocre and there were rapidly diminishing returns. Growth theory was useful because we learned from it how to model issues that were much closer to the heart of economics, which is how markets worked. But that comes later in the story. The leading chapter in my thesis demonstrated the stability of a putty-clay model without technical change, which I had been told was one of the burning topics in economic growth. I used the techniques derived there for the "Market for 'Lemons.'" Another chapter made a very preliminary attempt at deriving a theory of unemployment; a third represented the leads and lags resulting from changes in monetary policy. These last two papers were only marginally publishable, but they were the beginnings of an attempt to base Keynesian economics on sound microeconomic foundations.

Berkeley and India

I graduated from MIT in 1966, which was one of the years of highest demand ever for graduating PhD's in economics. I was lucky enough to obtain an assistant professorship at Berkeley. In my first year at Berkeley I wrote the "Market for 'Lemons.'" which is the work for which I have been cited for the Nobel Prize. I was helped considerably both in choice of topic and in execution by Tom Rothenberg, who also came to Berkeley in the fall of 1966. Tom and I had dinner together most nights that first year. On one such occasion I listed the possible topics on which I might work. "Lemons" was on the list, and Tom guided me not only in choice of topic, but also in turning it into a paper. I shall always to be grateful to him for his help and kindness. At the same time I was continuing my work on Keynesian macroeconomics. In that first year after graduate school I also turned out the first model of staggered wage and price setting. This is the basis both for the Fischer and Taylor models, which have more complex monetary rules than my original model, and also rational, rather than adaptive, expectations.

In 1967-68 I took leave from Berkeley to spend a year at the Indian Statistical Institute in New Delhi, where Steve Marglin headed a group that was seeking to develop a program to allocate the waters of the Bhakra-Nangal dam in northern Punjab. He wanted to produce a timetable for the release of the water so that peasants planting the new varieties of wheat

could be assured that they would get the water they needed to make such an investment worthwhile. I was brought into the project as an extra. By joining it, I thought that I would gain a first-hand view why India was so poor. My role in the project very quickly came to an end, when I discovered problems with the basic assumption needed to make the project feasible. Because of unseasonal rain and glacial melt I was unable to predict winter in flow into the reservoir from the rainfall of the previous monsoon. Instead, I wrote a paper on Federal-State fiscal policy in India. Planning had been temporarily suspended in India because of the bad monsoons, and my paper gave principles for planning if it should be revived. I also revised "The Market for 'Lemons'" which had been rejected two or three times in the course of the year by editors who felt that the issues in the paper were too trivial to merit publication in a serious academic journal. I included examples of incomplete markets from my readings of Indian economic history.

The trip to India was important for my intellectual development. Especially, it confirmed for me that nonstandard analyses were needed to understand many economic transactions. As I have hinted earlier, the fundamental problem I wished to explore in economics, was the reason for unemployment. Unemployment involves, above all, a gap between supply and demand. In India, the caste system for centuries has interposed itself between supply and demand. The gaps between supply and demand in the Indian caste system were then potentially informative as to how similar gaps might exist in labor markets in Western countries. What I learned in India became the keystone for my later contributions to the development of an efficiency wage theory of unemployment in Western countries. This theory unfolded over the next twenty years. Curiously, Joe Stiglitz visited Kenya at about the same time and developed models embodying alternative efficiency wage theory based on his similar observations of the underdeveloped world.

I returned from India in September 1968. In the fall of 1969 I was voted tenure by the department of economics at Berkeley, which was uncontroversial because the one person opposed was away on sabbatical at the time. In 1973-74 I served as Senior Economist at the Council of Economic Advisers. I was an extraordinarily poor staff member (partly because I had never been a research assistant in graduate school and partly because I am very bad at writing good bureaucratese.) Nor did it help that I had no loyalty to the Republican incumbents, Richard Nixon, and, in the beginning of my tenure there, Spiro Agnew. My tenure at the Council may have had little payoff for the government, but I personally learned a great deal, largely from June O'Neil and Barry Chiswick, the senior staffers in charge of labor economics. From them I learned how to do empirical economics. Also, of yet more importance, a former graduate student from Berkeley, Judy Graves introduced me to Kay Leong, who was a friend of hers from undergraduate days at Cornell and, coincidentally also a native of Berkeley. Kay and I got married at the end of the year.

After returning to Berkeley, it was time to be promoted to full professor, but the department did not think I had published enough. This lack of productivity had several different causes. First, after returning from India I suffered from colitis. A doctor at UCSF had cured the colitis, but by the use of drugs whose side effect was severe depression. Second, I spent a year studying Hindi-Urdu. Third, I had spent a year at the Council of Economic Advisers. Also, I spent two years on a paper which might have been considered interesting before the introduction of the accelerationist Phillips Curve into macroeconomics, but which was obsolete afterwards.[1] The consequence of my failure to receive this promotion was that Kay discovered a new facet of my personality, a trait she had not previously seen. This was the extreme concentration I am able to devote to a problem, in this instance the erasure of the department's implicit censure. She disliked my monomaniacal focus on this issue. This propensity to perseverate is sometimes self-destructive, as when I am unable to stop practicing a single piece on the piano or to quit solitaire, but I also consider it a major asset as an economist. For example, before the computer made the notion of "draft" obsolete because of continual revision, I wrote at least 50 drafts of papers on the effects of target-threshold monitoring of bank accounts on the efficacy of monetary policy. Gradually from the crude ideas in my thesis, these drafts developed into papers that in my opinion are a significant precursor to the modern work on this topic by Ricardo Caballero. I began working on this idea in 1963; I stopped working on it twenty years later in 1983. Similarly, in some sense I began work on unemployment theory when I was 12. 50 years later I am still mulling over the same subject. When Kay married me she had not appreciated either my persistence or its side effects, which were greatly magnified in the promotion crisis. The result was twofold. Kay went off with another man and I, after not being promoted, and with little personal reason to remain in Berkeley, accepted a professorship at the London School of Economics.

The LSE and return to Berkeley

In between Berkeley and the LSE I spent a year at the Federal Reserve Board in Washington, D.C., where I met Janet Yellen. We liked each other immediately and decided to get married. Not only did our personalities mesh perfectly, but we have also always been in all but perfect agreement about macroeconomics. Our lone disagreement is that she is a bit more supportive of free trade than I. We decided to get married hastily, not only because we had so little doubt about each other, but also for practical reasons. I had already accepted a professorship at the LSE for the coming year and if we were to avoid being separated, Janet would also need to get a job in England too. Luckily, she also was given a tenure-track lectureship at the LSE. There seemed to be no question about her tenure since she had already published several distinguished articles on the economics of bundling and advertising. After a year in Washington, we left for England. We very much liked both the LSE and London, but both of us had problems of identity: we were American, not English. Luckily, Berkeley had never accepted my proffer of resignation when I left for the LSE, so I was still nominally on the faculty. And Janet got a tenure-track

job in the business school with a promise of early review for tenure. We had met at the Fed in the Fall of 1977, married in June 1978, and left for the LSE in September of that year. We came back to Berkeley in August 1980. Shortly thereafter, in June 1981, our son Robert was born.

Meanwhile the focus of my research had changed in a subtle way. Previously the main focus of my research had been to discern the consequences for macroeconomics of different microeconomic structures, such as staggered contracts, target-threshold monitoring, asymmetric information, and also the existence of "jobs." Now, increasingly, my research concerned the effects of different assumptions regarding fairness and social customs on unemployment. In my view, there were two leading problems in macroeconomics. The first was whether there could be such a thing as involuntary unemployment: why couldn't an unemployed person obtain a job by being willing to accept a less advantageous job? The second was whether with complete information monetary policy could have real economic impact. In my early papers I posited a wage established by social custom, in turn resulting in unemployment. The theory was based on my understanding of the institution of caste in India. But it somehow did not have the ring of truth. When I presented this anthropology-based paper at Yale, Tjalling Koopmans asked whether I had read any sociology. It turns out that I had not, and so in my first year in England I made amends, reading the sociology classics. I then wrote a paper on efficiency wages in which non-market clearing results from the lower morale and productivity of workers whose sense of fairness has been violated. This was a sociologically-based efficiency wage theory of unemployment. Other authors at the time were developing, or had developed, similar theories of unemployment which were variously based on training costs and on information. These theories answered the first key question regarding how involuntary unemployment could occur, but the second question remained. Subsequently, Janet and I, in response to probing by James Tobin, who was a visitor to Berkeley in 1982-83, devised a theory to explain the second phenomena: sticky wages and prices in an economy with monopolistic competition and efficiency wages would be near-rational. Firms that followed rules of thumb, causing them to change prices and wages slowly, would lose something, but not much. Such sticky prices and wages would explain why monetary policy would be effective: if the money supply increased, real balances, which determine real demand, would rise. Thus rules of thumb, whose individual losses were economically insignificant, could have a significant effect on the economy. Janet and I worked together on many papers for the ten years from 1984 to 1994. For the first part of that decade we focused on macroeconomic theory: near-rationality and efficiency wages. We later turned to working on poverty and policy issues, such as the economic strategy for East Germany after German unification and the causes of rising out-of- wedlock childbearing in the United States.

Washington and return to Berkeley

Our work together was interrupted when in 1994 Janet was named to the Board of Governors of the Federal Reserve System. Janet, Robby and I moved from Berkeley to Washington. The Brookings Institution named me a Senior Fellow and generously supported about one third of my salary for the next five years. For the first three years, while Janet was at the Fed, I commuted back to Berkeley in the Spring term to teach. When Janet later became Chair of the Council of Economic Advisers in 1997, Berkeley gave me full-time leave. When Janet was at the Fed, I supported her as much as possible by taking over household duties; later when she was at the White House my role in providing psychological support in the daily political storms was yet more important. I also continued to work on both macroeconomics and poverty. With Bill Dickens and George Perry, I wrote on the economics of low inflation. This work challenges the natural rate, accelerationist theory of the Phillips Curve. It shows that at low inflation there is likely to be a significant long run tradeoff between inflation and unemployment. This result has potentially important implications for monetary policy. With Rachel Kranton of the University of Maryland I also wrote papers that incorporate the concept of identity into economics. This work yields a theory of minority poverty in the United States and new views on the economics of gender and discrimination and the economics of education. The initial impetus for this work came from Rachel's understanding of the importance of identity in Middle Eastern Studies.

This takes us almost to the present. In 1999 Robby graduated from St. Albans School, the high school he attended in Washington, D.C., and he set off for college, at Yale. At the same time Janet left the Council of Economic Advisers, and we returned to Berkeley. Rachel Kranton and I are still working on identity, whose introduction into economic analysis, we believe, will help unify economic, with sociological, anthropological and psychological theory. We are excited about the range of economic analysis and policy implications for this approach.

Conclusion

In conclusion, I am very honored to have been named co-recipient of the Prize in Economic Sciences in Memory of Alfred Nobel. Economics is a far richer field with more interesting, realistic, and detailed models than when I first entered the profession. Asymmetric information is a good example of this evolution. In addition, there is now an increased willingness to base economics on findings in the other social sciences. Over the last thirty years we have been gradually evolving an economics that relies more on careful empirical observation, and less on questionable assumption regarding how rational people must behave. It has been a great pleasure to have been a contributor to this development. I hope, with the help of my co-authors, to continue to do so as long as time permits.

1 In the accelerationist view there is only one unemployment rate which would give constant inflation. In the pre-accelerationist view at any given rate of unemployment relative prices and wages would converge to particular steady-state values; lower unemployment caused higher steadystate inflation. This paper showed that in such a "quasi-equilibrium," standard cost-benefit analysis would apply: the amount of inflation resulting from any given expenditure would be proportional to the dollar expenditure on it. In consequence standard cost benefit analysis worked.

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