

Memoir

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I was born in Gary, Indiana, at the time, a major steel town on the southern shores of Lake Michigan, on February 9, 1943. Both of my parents were born within six miles of Gary, early in the century, and continued to live in the area until 1997. I sometimes thought that my perignations made up for their stability.

There must have been something in the air of Gary that led one into economics: the first Nobel Prize winner, Paul Samuelson, was also from Gary, as were several other distinguished economists. (Paul allegedly once wrote a letter of recommendation for me which summarized my accomplishments by saying that I was the best economist from Gary, Indiana.) Certainly, the poverty, the discrimination, the episodic unemployment could not but strike an inquiring youngster: why did these exist, and what could we do about them.

I grew up in a family in which political issues were often discussed, and debated intensely. My mother's family were New Deal Democrats – they worshipped FDR; and though my uncle was a highly successful lawyer and real estate entrepreneur, he was staunchly pro-labor. My father, on the other hand, was probably more aptly described as a Jeffersonian democrat; a small businessman (an independent insurance agent) himself, he repeatedly spoke of the virtues of self-employment, of being one's own boss, of self-reliance. He worried about big business, and valued our competition laws. I saw him, conservative by nature, buffeted by the marked changes in American society during the near-century of his life, and adapt to these changes. By the mid-seventies, he had become a strong advocate of civil rights. He had a deep sense of civic and moral responsibility. He was one of the few people I knew who insisted on paying social security contributions for household help – regardless of whether they wanted it or not; he knew they would need it when they were old. (This attitude served me well; in 1993, while many Clinton appointees faced problems in being vetted because of their failure to pay these taxes, I was spared these problems because I had followed his example.)

I went to public schools, and while Gary was, like most American cities, racially segregated, it was at least socially integrated – a cross section of children from families of all walks of life. The Gary public school system was designed to integrate the immigrants who constituted such a large fraction of its inhabitant; here, the melting pot rhetoric that is so important part of America's self-image was taken seriously. All of us had to learn, for instance, two trades (mine were printing and being an electrician). I had the good fortune of having dedicated

teachers, who in spite of relatively large classes, provided a high level of individual attention. My teachers helped guide and motivate me; but the responsibility of learning was left with me, an approach to learning which was later reinforced by my experiences at Amherst.

The extra curricular activity in which I was most engaged – debating – helped shape my interests in public policy. Every year, a national debating topic is chosen. (One year, it was the reform of the agricultural support programs, an issue which I had to grapple with almost forty years later; some of my colleagues in the Clinton Administration too had been debaters, but they got taken up by the sport. I was attracted more by the ideas.) In debate, one randomly was assigned to one side or the other. This had at least one virtue – it made one see that there was more than one side to these complex issues

The intellectually most formative experiences occurred during the three years 1960-1963 I spent at Amherst college, a small, new England college (at the time, a men's college with around 1000 students). I went to Amherst because my brother had gone there before me, and he went there because his guidance counselor thought that we would do better there than at a large university like Harvard. Amherst is a liberal arts college, committed to providing students with a broad education. (Today, I serve on its board of trustees.) The notion that every well educated person would have a mastery of at least the basic elements of the humanities, sciences, and social sciences is a far cry from the specialized education that most students today receive, particularly in the research universities. But what distinguished Amherst was not only what was taught, but how it was taught, and the close relationships we had with our teachers. The best teachers still taught in a Socratic style, asking questions, responding to the answers with still another question. And in all of our courses, we were taught that what mattered most was asking the right question – having posed the question well, answering the question was often a relatively easy matter.

I thrived on the atmosphere; while until late in my third year, I majored in physics, and enjoyed immensely the camaraderie of the physics students as we strove to solve the hard problems that were assigned to us, I took a smattering of courses in mathematics, history, English, philosophy, and the standard fare of introductory biology and chemistry. I still remember well the courses, and have frequently drawn upon this learning. For instance, the discussions of the encounters between different civilizations that was a major theme in our Freshman history class helped shape my thinking about globalization more than three decades later; I felt I was in a better position to think about the current episode from an historical perspective, and see it more through the eyes of the *other* side.

But while I loved all of these courses, there was an irresistible attraction of economics. My three teachers at Amherst showed me the range of the subject: Arnold Collery, later to be Dean of Columbia College, was a thoughtful and erudite scholar, from whom I studied both micro-economics and macro-economics. The style of teaching was exemplified by his choice of texts for the micro course. Rather than a standard textbook, he used Abba Lerner's *Economics of Control*, a book written as a theoretical contribution to our understanding of how markets work, an inquiry into whether planning provided an alternative. James Nelson, who taught me introductory economics, was a vivacious policy economist, who conveyed the sense of excitement that came from trying to shape economic policies. Finally, Ralph Beals was a young graduate of M.I.T., trained in mathematical techniques that were just then coming into vogue. It was not until late in the spring of my junior (third) year that I decided to major in economics; I thought it provided an opportunity for me to apply my interests and abilities in mathematics to important social problems, and somehow, I thought it would also enable me to combine my interest in history and in writing. I wanted it all, and economics seemed to have it all. When I advised my teachers of my decision, they advised me that I should go on to graduate school. What I would study during my senior year would be largely repeated in my first year of graduate school. They then arranged for me to go to MIT, and to receive the finance I required (I had been on full scholarship at Amherst; the modest last minute fellowship from M.I.T. entailed my living on a dollar a day beyond my rent – the number that today is taken as the threshold for absolute poverty.) The flexibility of M.I.T., and Amherst, – the deadlines for application were well past, the money for fellowships had largely already been dispensed – is a tribute to America's higher educational system, and one of the reasons that it continues to excel. I left Amherst for M.I.T. without a degree, or without any promise of one. It was before I had done my work on the economics of information, and I think I didn't grasp the information that might be conveyed by having a degree from Amherst. I simply wanted to learn as much as I could as quickly as I could – not from any sense of "getting ahead" but simply from an overwhelming sense that there was so much to learn, and one needed to get on with it. (Later, Amherst did give me a degree, and still later, in 1974, they gave me an honorary doctorate.) One of my teachers, and one of the world's greatest economists, Hirofumi Uzawa, when asked where he got his advanced degree, would say they he had no degree to speak of; in academic circles, there is a certain pride in simply having pursued one's studies on one's own, outside the confines of a regular program. If Amherst hadn't given me a degree, I could have given a similar response.)

My love of politics first manifested itself in my days at Amherst. I served on the Student Council both in my freshman and sophomore years (there were three representatives from each class), and in junior year, got elected president of

the student council. My conviction that if one attains positions of “power” one should view them as opportunities for social change also manifested itself. I began a campaign to abolish fraternities (to which 90% of the students belonged), because they were socially divisive, and contrary to the spirit of a liberal arts school and community. It was a campaign that was not welcomed by many of my classmates, and it took years to come to fruition, but it did, and I believe that Amherst is the better for it. This was only one of the many issues that I raised in my “activist” presidency. I, like many members of my generation, was concerned with segregation and the repeated violation of civil rights. We were impatient with those (like President Kennedy) who took a cautious approach. How could we continue to countenance these injustices that had gone on so long. (The fact that so many people in the *establishment* seemed to do so – as they had accepted colonialism, slavery, and other forms of oppression – left a life-long mark. It reinforced a distrust of authority which I had had from childhood.) I marched on Washington – the march where Martin Luther King gave his “I have a dream” speech remains an indelible memory. I organized an exchange program with a small, African-American, southern school; I believed it was important for us to understand, as much as we could, what they were confronting. These were the years where many civil rights activists from the North were killed; but in our enthusiasm for doing what was right, these risks never crossed our minds.

Not surprisingly, there was considerable opposition to some of my initiatives, so much so that a recall referendum was initiated. It was also my first encounter with the power of the press and personal rivalries; the editor of the student paper took on the cause of removing me. But my friends and allies beat back the initiative, and I continued to use the platform of the presidency of the student council to promote social change.

Amherst was pivotal in my broad intellectual development; MIT in my development as a professional economist. I spent but two years at MIT as a student (I did my generals in a year and a half, and then began writing my thesis.) It was the heyday of MIT – first-rate professors (I had at least four Nobel Prize winners as professor: Samuelson (Nobel Laureate in 1970), Solow (Nobel Laureate in 1987), Modigliani (Nobel Laureate in 1985), and Arrow (Nobel Laureate in 1972)) teaching first-rate students. My first paper presented at an academic meeting, to the econometric society, was jointly co-authored with George Akerlof, with whom I shared this year’s prize. I had many other first rate classmates that were to make truly important contributions to economics.

The particular style of MIT economics suited me well – simple and concrete models, directed at answering important and relevant questions. I sometimes wonder what would have happened had I gone to one of the universities in which other styles of economics were taught, either the abstract general equilibrium models, for which Berkeley was then noted, or the simpler partial equilibrium models for which Chicago was famous. The politics of MIT also suited me well. My teachers were mostly establishment liberals, but there were a few that were more questioning. I wonder too how I would have fared had I gone to one of the schools, like Chicago, where there is a more conservative bent. Would I have changed? Or would I have just been unhappy?

But, as I comment in my Nobel lecture, there was an incongruity between many of the models that we were taught and the policy positions that our teachers (and we) believed in. The models seemed more consonant with free market prescriptions, though they were presented more as benchmarks rather than full characterizations.

The students and faculty at MIT were highly interactive. There was a group of friends (mostly from the year ahead of me, including George), which included a few young economists from Harvard, with whom I spent much of my time. We lived economics and politics. We debated about what was wrong with the models that we were being taught. We thought about how we could or would go about changing the models, and occasionally about how we could or would go about changing the world. One of our group was from India (Mrinal Datta-Chaudhuri) and we learned from him a host of stories concerning the colonial experience.

After my first year as a graduate student, I was offered a wonderful opportunity, editing Paul Samuelson's collected papers. I often took Paul as a role model, the expansiveness of his learning, the breadth of his work, its originality and penetration. He wrote forcefully and beautifully. For many years after leaving MIT, I was best known as Samuelson's editor, to which I did not appreciate, since I wanted to be known for my own work.

The summer after my second year as a graduate student was one of the most exciting. Hirofumi Uzawa had moved from Stanford to Chicago, and had received an NSF grant to bring around a dozen graduate students from around the country to work together on theory. Eytan Sheshinski and his wife Ruthie, George Akerlof, Mrinal Datta-Chaudhuri, Georgio LaMalfa (later to be head of the Republican party of Italy and a minister in several of that country's governments) and his wife, Eva drove off to Chicago. We stopped on the way at my home in Gary for a night, where my parents were delighted to have a chance to meet my friends. At Chicago, we were joined by some of Hiro's Chicago

students and by Frank Levy from Yale (who now teaches at MIT), among others. Growth theory was then all the rage, and we did growth theory, day in and day out. Many of us worked on technical change, on work which would be rediscovered, two decades later and popularized under the name of endogenous growth theory. (The fact that the work that was done in this period received so little attention in the subsequent revival of interest in growth theory two decades later has been a subject of some interest to me, as part of what may be thought of as the sociology of knowledge. Economists tend to move in particular circles, defined by their “school” and “subject.” Endogenous growth theory in the 80s grew out of the Chicago school, while the earlier work on growth theory was part of the MIT school – treating Uzawa, though a professor at Chicago, as an honorary member of the M.I.T. fraternity. I moved both across schools and subjects. This allowed me to learn from each, and the cross fertilization was highly productive. But it did pose problems. Not being a dues paying member of any particular school/sub discipline sometimes meant it was more difficult to get one’s ideas accepted, or even widely discussed. This was particularly the case in macro-economics, where in the 70s and 80s, the reigning paradigms were either rational expectations/representative agent models or fixed price new Keynesian models. The models that Greenwald and I formulated, focusing on imperfect capital markets, risk averse, credit constrained firms, in which concerns about bankruptcy often play an important role, only became widely accepted after similar ideas were picked up by the card carrying members of the macro-fraternity.)

While the group of us who went to Chicago to study under Uzawa was supposedly chosen for our prowess as students, we shared a broad *weltanschauung*. As the month of intensive work ended, leaving a lifelong impression on all of us, most of us went up to George’s family place on Lake Squam. I was working as Bob Solow’s research assistant, and so had to commute from Cambridge.

After two years at MIT (supported in the second year by the National Science Foundation), I received a Fulbright fellowship to Cambridge for 1965-1966. At the time, there were three High Churches in the economics profession: Chicago on the right and Cambridge, U.K. on the left, with MIT being in the center. Cambridge was still basking in the reflected glory of Keynes, who had revolutionized economics some thirty years earlier. Lord Kahn, of the Kahn multiplier (which explained how a dollar of government expenditure had a multiple effect in increasing GDP), Joan Robinson, Nicky Kaldor, James Meade, David Champernowne, Piero Sraffa, these were among the gods that populated the colleges of Cambridge. I wanted to see as many views as I could, and I worried about coming too much under the influence of Samuelson and Solow.

Joan Robinson was assigned as my tutor. She had originally wanted me to redo my undergraduate degree – she thought it would take some time to undo the damage of my MIT education, but eventually she was prevailed upon instead to take on the responsibility of my reeducation. We had a tumultuous relationship. Evidently, she wasn't used to the kind of questioning stance of a brash American student, even a soft-spoken one from the mid-west, and after one term, I switched to Frank Hahn. He was flamboyant, and always intellectually provocative. Cambridge was in ferment. The quality of the students and the young lecturers matched that of the gray eminences: Jim Mirrlees (later to get the Nobel prize), Partha Dasgupta, Tony Atkinson; Geoff Heal, David Newbery and a host of others. There was a sense of excitement that was associated not just with the generation of new ideas, but with the belief that those ideas were important, and not just for economics, but for society more broadly. As Frank Hahn demonstrated the dynamic instability of the economy (a problem posed by the absence of futures markets going out infinitely far into the future; in technical terms, the absence of a transversality condition), he would excitedly exclaim that he had put another nail in the coffin of capitalism.

One evening I gave a seminar on a paper I was then completing, on the distribution of income among individuals (using the kinds of tools that had been used to describe the dynamics of growth to describe the dynamics of inequality). The discussion had been followed by a lively debate. The next morning, I received a twenty-page comment from James Meade (who received the Nobel Prize in 1977), suggesting elaborations and alternative interpretations. There was a sense of a community of scholars trying to understand some very important and complex problems.

My research in this period centered around growth, technical change, and income distribution, both how growth affected the distribution of income and how the distribution of income affected growth. The most important paper to emerge from my thesis, "The Distribution of Income and Wealth Among Individuals,"¹ received considerable attention at the time, but unfortunately, the topic has not been one which has received much attention from the economic profession, so that it has not generated as much follow-on research as I had hoped. But the subject of the causes and consequences of inequality has remained one of my abiding concerns, one which I pursued as I began to delve into the economics of information.²

My early research project in this area illustrated one feature of my research style which, while it may have contributed to the overall success of

¹ Stiglitz (1969a)

² See e.g. Stiglitz (1973a, 1975, 1976), Rothschild and Stiglitz (1973, 1982) and Braverman and Stiglitz (1989).

some of my research programs, was a source of unending frustration. Once I undertook the analysis of a problem, I often looked at it from a variety of perspectives. I approached the problem as a series of thought experiments – unlike many other sciences, we typically cannot do actual experiments. I would construct models changing one assumption or the other. Each would provide some insight into what drove the results. The whole was more than the sum of the parts; while each of the models was, by itself, of some interest, it was the collection of models, and how the results depended on the particular assumptions employed, which provided the greatest insight. My original work thus grew into a monograph of some hundred pages. Unfortunately, the preferred form of expression in the profession was narrowly defined articles, making a single point. I thus had to extract from the longer monograph a series of papers, a process which not only took a long time, but diminished (in my judgment) the insights provided. (This problem was even greater in the next two research projects, one exploring the behavior of the firm under uncertainty, and in particular, the consequences of risk with an incomplete set of risk markets; most (but not all) of that “paper” – an eight hour lecture I delivered in 1970 at Hakone, Japan, in another one of Hirofumi Uzawa’s workshop – was published as a series of articles over the next decade.³ The exploration of “Alternative theories of wage determination and unemployment in less developing countries,” completed while I was at the Institute of Development Studies at the University of Nairobi in the summer of 1969, was similarly published in a series of articles – the more recent of which was not published until 1982).⁴

Another project that I began in Cambridge concerned the interaction between the distribution of income and short run macro economic behavior. At the time, most macro economic models simply assumed that wages and prices were fixed. But of course during the great depression wages and prices had fallen considerably. The problem was not that they were absolutely fixed, but with the dynamics of adjustment. With Robert Solow (Solow and Stiglitz, 1968), I explored these dynamics, to explain the persistence of unemployment. With George Akerlof (see Akerlof and Stiglitz, 1969), I showed how such dynamics can give rise to cyclical behavior. Later work would attempt to provide stronger micro foundations for these adjustment dynamics.

I returned from Cambridge to take up a one-year appointment as an assistant professor at M.I.T., from which I went to Yale. My teaching at Yale seemingly warranted an indefinite deferment from the Vietnam War draft. During this period, I continued my work on economic dynamics, and began my

³ Including Stiglitz (1972a, 1972b, 1974a, 1989a)

⁴ See Stiglitz (1974b, 1982a). See also Stiglitz (1974c, 1992).

research on the economics of uncertainty, which in turn, quickly led to the work on the economics of information.

The major concern in my research on dynamics was the stability of the market economy. The standard models assumed that there were future markets extending infinitely far into the future. Following work of Frank Hahn (1966), Karl Shell and I showed that a competitive economy with futures markets extending an arbitrarily large finite number of periods into the future would, in general, exhibit dynamic instabilities; that is, it would take off onto a path that *appeared* to be efficient and stable, with the inefficiency and instability only manifesting itself some distance into the future (Shell and Stiglitz, 1967). This theme was explored in a variety of different contexts. The subject was central to the on going debate concerning the efficiency of the capitalist economy. If stability and efficiency required that there existed markets that extended infinitely far into the future – and these markets clearly did not exist – what assurance do we have of the stability and efficiency of the capitalist system? In one important variant on this theme, I assumed that there were rational expectations. Simplistic representative agent models living infinitely long had been constructed, and, not surprisingly, in these models, the problems of instability and inefficiency did not arise. I assumed, on the contrary, that individuals were finitely lived; there were overlapping generations. In that case, there were an infinite number of paths consistent with rational expectations extending infinitely far into the future. (Stiglitz, 1973b)

This concern with *multiplicity* of equilibrium (both in the short run and the long) was to appear over and over again in my subsequent work, where under a wide variety of circumstances, the economy could be trapped in a “bad” equilibrium. In some cases, some individuals are better off in one equilibrium, some worse off, but in other cases, one equilibrium could Pareto dominate others.⁵

Much of my work in this period was concerned with exploring the *logic* of economic models, but also with attempting to reconcile the models with every day observation. Thus, in much of my earlier work I began by asking what would happen to the standard results if there were not the complete set of risk markets which Arrow and Debreu (Nobel Laureate in 1983) had postulated in their analysis of competitive equilibrium. This was a question which one could approach largely (though not entirely) deductively. (Stiglitz, 1972a, 1982b) But my research in this area quickly posed problems for which there was no obvious

⁵ For a more complete analysis of these multiple equilibria models, see Hoff and Stiglitz (2001). The first example of such multiplicity out of the growth context was my model on equilibrium in stock markets (Stiglitz, 1972a), where the riskiness of the projects chosen by one firm depends on those chosen by other firms. Other examples of multiple equilibria can be found in Stiglitz (1972b, 1974c, 1977, 1995)

answer: what should (or do) firms maximize? This early work exposed how sensitive not only were the *results* of the standard model to the (clearly unrealistic) assumptions posited, but even the reasonableness of the *assumed* behavior.⁶ As my work progressed, the discrepancies between the kind of behavior *implied* by the standard model and actual behavior also became increasingly clear. In the standard model, the only risk that firms should worry about was the correlation of the outcomes (profits) with the “market”; in practice, businesses seem to pay less attention to that than they do to “own” risk, the chance the project will succeed or fail. In the standard model, everyone agrees about what the firm should do; in practice, there are often heated disagreements. It seemed to me that any persuasive theory of the firm had to be consistent with these, and other, aspects of widely observed firm behavior. (Stiglitz, 1982c, 1989b)

Economists spend enormous energy providing refined testing to their models. Economists often seem to forget that some of the most important theories in physics are either verified or refuted by a single observation, or a limited number of observations (e.g. Einstein’s theory of relativity, or the theory of black holes. Thus, models which suggested that there was no such thing as unemployment, or that it was at most short lived, to my mind were suspect. Economists often like startling theorems, results which seem to run counter to conventional wisdom. Perhaps the most important result in the economics of uncertainty in the 1950s was that of Modigliani and Miller (Nobel Laureate in 1990), who argued that corporate financial structure – whether firms finance themselves with debt or equity – made no difference (other than as a result of taxes). What was interesting about the theory was that it was based on assumptions of rational behavior, and yet if it were true, there was ample evidence of market irrationality – the thousands of people on Wall Street and other financial centers who seemed to be worrying about corporate finance – and for reasons that had nothing to do with taxation. I began my analysis of corporate finance by demonstrating that the result was far more general than they had shown. (Stiglitz, 1969b) But there were two assumptions that they had ignored, and these turned out to be crucial: they had assumed no bankruptcy and perfect (or at least symmetric) information. Over the succeeding years, I was to explore the consequences of these (related) assumptions, not only for the theories of corporate finance, but also for corporate governance (including takeovers) and macro-economics. As I note in my Nobel lecture, the failure of the IMF to take on board fully the consequences of these assumptions played an important role in their policy failures almost three decades later.

⁶ Sanford Grossman and I pursued these ideas further in Grossman and Stiglitz (1977, 1980)

My work on the economics of uncertainty led naturally to the work on information asymmetries, and more generally, imperfect information. In the work on the economics of uncertainty, I explored the consequences, *given beliefs about probability distributions, say, of prices and outputs*, of economic behavior. The standard theory not only had assumed that there was a complete set of markets for these risks, but that beliefs about these probability distributions were exogenous, unaffected by any actions. But individuals and firms spend an enormous amount of resources acquiring information, which affects their beliefs; and actions of others too affect their beliefs.

As I approach the problems that are today referred to as the economics of information, I was greatly helped by the breadth of my education at Amherst and M.I.T. The problem of how people form their *beliefs* is, of course, the central question of statistics: making inferences on the basis of limited data. The first course for which I served as a teaching assistant was statistics (with Harold Freeman), and it was concerned with using probability theory to make statistical inferences (rather than “classical” statistics). I am sure that I was, at least subconsciously, affected too by the work going on in Cambridge in statistical decision theory, by people by Raiffa, and while I never took a course from him, he was active in the Harvard-MIT theory seminar, and was a presence at the dinners we often had afterwards.

Another set of central insights came from the work that I had been doing in public finance (at it was called at that time; with my 1984 textbook, I helped shift the sub discipline to focus more broadly on the *economics of the public sector*.) As I noted in my Nobel lecture, an early insight in my work on the economics of information concerned the problem of appropriability – the difficulty that those who pay for information have in getting returns. This is, of course, the central concern of *public goods*, one of the main subjects within the economics of the public sector. I recognized that information was, in many respects, like a public good, and it was this insight that made it clear to me that it was unlikely that the private market would provide efficient resource allocations whenever information was endogenous. (See, e.g. Stiglitz, 1987a) Much of the subsequent work was trying to define more precisely the nature of the market failures.

As I explain in my Nobel lecture, the time I spent in Kenya was pivotal in the development of my ideas on the economics of information. I have often wondered why. I think in part the reason is that seeing an economy that is, in many ways, quite different from the one grows up in, helps crystallize issues: one takes too much for granted, without asking why things are the way they are. As I studied development, I was forced to think everything through from first principles. Had I grown up in a world in which everyone was a sharecropper, I probably would have accepted this as the way things are. As it was,

sharecropping seemed like a peculiar institution, for it seemed to attenuate greatly the incentives workers had to work (since they typically had to give one out of two dollars that they earned to the landlord.)

Similarly, growing up in Gary Indiana gave me, I think, a distinct advantage over many of my classmates who had grown up in affluent suburbs. They could read articles that argued that in competitive equilibrium, there could not be discrimination, so long as there are some non-discriminatory individuals or firms, since it would pay any such firm to hire the lower wage discriminated against individuals, and take them seriously. I *knew* that discrimination existed, even though there were many individuals who were not prejudicial. To me, the *theorem* simply proved that one or more of the assumptions that went into the theory was wrong; my task, as a theorist, was to figure out which assumptions were the critical ones.

A topic of abiding concern since I was in high school was *economic organization*. I grew up in the midst of the cold war. At the time, Communism *seemed* to be delivering faster economic growth, but at the expense of liberty. Much of the world seemed to be suffering under the yoke of colonialism, which neither delivered economic growth or democracy, and one which seemed to inconsistent with the principles in which I had been taught, and come to believe. The market economy seemed to be plagued by repeated periods of unemployment, and to leave large fractions of their populations in poverty. Yugoslavia's system of self-managed firms intrigued me. Economics seemed to provide the tools with which one could analyze these alternative economic systems. A central question was on how, and how well, alternative systems addressed the problems of gathering, analyzing, and disseminating information, and making decisions based on imperfect information. Understanding the limitations of the market – the so-called market failures – became one of the central foci of my research.

I recognized that the standard model was deficient not only in its assumptions about information, but also in ignoring technical change. The latter I thought particularly curious, given the importance that technical change clearly played in our economy. I joined the growing band of those who paid homage to Joseph Schumpeter because of his emphasis on technical change, a subject which was not even broached in the standard first year graduate economics course, let alone in undergraduate principles courses. (I tried to remedy the latter deficiency by introducing a chapter on the subject in my Principles book.) But while I thought that Schumpeter had asked the right question, I was not convinced he gave the right answer. The close links between the work that I had been doing on information and technical change allowed me to begin to formalize models of Schumpeterian competition, and I quickly realized that

several of the “accepted” results of Schumpeterian competition were not valid, e.g. that there would necessarily be a succession of short lived monopolies. (See, e.g. Dasgupta and Stiglitz, 1980a, 1980b, 1981, 1988) I showed that a monopoly, once established, could be persistent, that Schumpeterian competition was not, in general, “efficient,” and that in particular the incumbent could/would take actions which deterred entry, that potential competition would not in general suffice to ensure a rapid (efficient) pace of innovation. These ideas are, of course, of particular relevance in the “new economy,” which centers around innovation.

There was a rather different strand of literature (often associated with Hayek) which praised the virtues of the market economy, not the basis of the standard competitive (Arrow Debreu model), or one the basis of Schumpeterian competition, but rather on “evolutionary” grounds. In the early 70s, I had become fascinated with this alternative approach, and begun to subject it to scrutiny. At the time, there was little formal work on evolutionary modeling, and even later, most of the modeling focused around *describing* (often in simulation exercises) evolutionary processes. I was interested in *evaluating* evolutionary processes. What could one say about whether free markets, by themselves, led to “efficient” or “desirable” evolution? Were there interventions in the market which might “shape” evolution in ways which would lead to better outcomes? Hayek and his disciples had argued for free markets, but never really even addressed these questions. This remains a question that has still not been well investigated, but preliminary results (cited in my Nobel lecture) suggest strongly the limitations of unfettered free market evolution. (Part, but only part, of the problem lies with imperfections of capital markets.)

Later, with the collapse of the Soviet system, and the recognition of the problems of socialism more broadly, I rethought the lessons that might be gleaned from the failed experiment. In *Whither Socialism?* (See Stiglitz, 1994) I came to the conclusion that the failure of the socialist economies reinforced my belief in the inadequacy of the competitive equilibrium model. If that model had been correct, market socialism probably could have succeeded. The standard competitive market equilibrium model had failed to recognize the complexity of the information problem facing the economy – just as the socialists had. Their view of decentralization was similarly oversimplified – a point which I had earlier emphasized in my work with Raj Sah, where we had compared hierarchical and polyarchical decision making structures⁷. Here, our concern was not with asymmetries of information or incentives, but with how different economic organizational structures in effect *aggregated* the disparate and limited information of different individuals.

⁷ See e.g. Sah and Stiglitz (1985a, 1986).

As the former socialist economies decided to make the transition to a market economy, a host of fascinating problems was posed on how best to make that transition. China provided the first venue for looking at these questions, in a series of meetings in 1980 and 1981, and Russia and the other countries of the former Soviet Union and Eastern Europe provide a second. The debates were heated. Much was at stake. And underlying the debate were very different understandings of the fundamentals of a market economy – what was necessary to make it function. My views on the inadequacy of the standard model played a central role in my thinking. I emphasized the importance of competition, corporate governance, finance, and more broadly the institutional (including legal) infrastructure. I did not place much stress on privatization. I was part of a wider school, sometimes referred to as “gradualists,” as opposed to the shock therapists that focused on rapid transitions, with quick privatization. The strategy for transition that I advocated was markedly different from that pushed by the IMF and the shock therapists. The failures of so many countries to make a successful transition back to a market economy has provided new insights into what makes market economies function, one which I had occasion to explore during my years as the Chief Economist of the World Bank. There is now a wide consensus on the importance of the institutional infrastructure, and on the dangers of rapid privatization. (See the references cited in my Nobel lecture.)

I referred earlier to my work in the economics of the public sector.⁸ I was convinced that there was an important role for government to play. Given that,

⁸ My work in the economics of the public sector has gone through four stages. It began with extensive collaborations with Tony Atkinson and Partha Dasgupta. Diamond and Mirrlees had helped revive interest in Ramsey’s work in optimal taxation. They had extended Ramsey’s analysis to a general equilibrium context, and *seemed* to incorporate distributional concerns. This work also seemed one of the few positive results in the theory of the second best: even though government could not impose lump sum taxes, one could say something meaningful about what the government should do. But the conclusions were unpersuasive. They suggested, for instance, that the government should not impose taxes on corporations and should not impose tariffs, and Ramsey’s earlier analysis suggested that high tax rates ought to be imposed on commodities, like food, with low demand elasticities. Such taxes were regressive, and I could not believe that they were truly “optimal.” Atkinson and I (1972) formally incorporated distributional concerns in the design of tax policy, with results that were more in accord with our intuition. Similarly, Dasgupta and I took into account limitations on the ability of the government to impose taxes, and within this broader, and we would argue more realistic framework, tariffs and corporate income taxes did make sense. (Dasgupta and Stiglitz, 1971, 1972, 1974) Later, I began to think of the problem of taxation as an information problem—limited information imposed restrictions on the set of taxes that could be imposed; and asked what were the set of *pareto efficient* tax structures, that is, given the limitations on information, what were the set of tax structures such that no one could be made better off without making anyone worse off. (Stiglitz, 1998b) Within this framework, it became clear that Ramsey’s analysis of optimal commodity taxes made little sense; only if the government could not impose income taxes as well as commodity taxes (as was the case in some developing countries) was it of much relevance. (Atkinson and Stiglitz, 1976)

A second set of issues to which I turned was project evaluation, and in particular the determination of shadow wages and discount rates. I argued that one could not calculate shadow wages without a model

it was natural for me to turn to the question of how it could play that role most effectively. (See, e.g. Stiglitz, 1991, 1997a) One of the main questions with which I was concerned was how to redistribute income in a way as to minimize the loss in efficiency that are inevitably associated with tax distortions. Economics of information had provided a framework within which this question could, for the first time, be addressed in a meaningful way, as I explain in my Nobel lecture.

Still another important strand of my research, only tangentially related to my work on the economics of information, concerned industrial organization. In one of my most cited papers, that with Avinash Dixit⁹, we constructed a model in which there are so many firms that each can ignore its impact on others' economic actions, but still, firms face downward sloping demand curves – there is monopolistic competition. This seemed to describe many of the markets in the economy far better than either the models of pure competition, pure monopoly, or oligopoly. (Markets in which information are imperfect are also likely to be characterized by monopolist competition) Little progress on the theory of monopolistic competition had been made in the more than forty years since Edwin Chamberlin first broached the idea. In particular, he had only formulated a partial equilibrium model. We were interested in constructing a general equilibrium model, within which one could assess how well the market functioned, in particular in making the tradeoffs between economies of scale and product diversity. We showed that there was a single borderline case – of

of the labor market, one which including a theory of wage determination and migration. Once that was done, one obtained results that were markedly different from the “standard” wisdom; for instance, the shadow wage on labor in some central cases was the market wage, *even though there was a high level of unemployment*. (Stiglitz, 1982d and Sah and Stiglitz, 1985b). On the other hand, I argued against the use of market interest rates for project evaluation. (Stiglitz 1982e, Arrow et al, 1996) When I went to the Council of Economic Advisers, many of these views on cost benefit analysis became incorporated in the guidelines issues by the Office of Management and Budget for project and regulatory evaluations.

A third quite distinct research project developed the theory of local public goods. Tiebout (1956) had put forward the conjecture that competition among local communities was like competition in markets, and would yield efficient outcomes. My doubts about market competition naturally led me to have doubts about competition in this arena, perspectives that were confirmed as our formalized the theory of local public goods. (Stiglitz, 1977) This project, in turn, led to a joint research project with Richard Arnott on the relationship between expenditures on public goods and land rents: was it possible to finance the optimal supply of public goods by a tax on land only (what I referred to as the Henry George theorem).

There was a quite different strand of work motivated in part by a request from the U.S. Treasury concerning capital gains taxation. I had done earlier work on the impact of capital gains taxation in the presence of uncertainty, which changed many of the long standing presumptions. (Stiglitz, 1969c) But more complicated issues were raised by the dynamics, and by the obvious use of capital gains as part of tax avoidance strategies. I showed that, were markets perfect, one could take advantage of the special treatment of capital gains taxes to avoid all taxation. (See Stiglitz, 1983a) Though a variety of provisions of the tax code have been introduced to try to circumscribe such tax avoidance behavior, they are imperfect. At a theoretical level, this led me to consider the general principles of tax avoidance (Stiglitz, 1985b), and had a great deal of influence on my thinking about the problems of tax reform, reflected both in my writing and the advice I gave both while at the Council of Economic Advisers and the World Bank. (See Stiglitz, 1997b, 1998a)

⁹ See Dixit and Stiglitz (1977).

immense simplicity – in which the market made that trade-off perfectly; but more generally, it did not.¹⁰

While my work on industrial organization and imperfect information undermined the confidence in the ability of unfettered markets to allocate resources efficiently, there was another strand of research in the economics profession which was trying to argue the contrary. In particular, there were those who argued that even with natural monopoly markets could be efficient; competition for the market could replace competition in the market; all that one required was potential competition. On the face of it, this idea seemed suspect. If it were true, there would be no monopoly rents. And indeed, my suspicions turned out to be true: I showed that even if there were arbitrarily small sunk costs (which there always are) then potential competition would not suffice to limit the abuses of monopoly.¹¹

The most important *systemic* failure associated with the market economy is the periodic episodes of underutilization of resources. Trying to understand why the labor market does not clear – why there is persistent unemployment – has been another abiding concern, one which I have tried to approach from a variety of angles. The work with Solow and with Akerlof cited above focused on the consequences of finite speeds of adjustment. Even if wages fall, if prices fall too, real wages may not adjust very quickly. Subsequent work with Greenwald tried to explain in a more coherent way these speeds of adjustment.¹² The efficiency wage theories (described in greater detail in my Nobel lecture) explain why it may pay firms to pay a wage higher than the market clearing wage: the increase in productivity more than offsets the increase in wages. The theory of equity rationing¹³ helped explain why more “flexible” contractual arrangements were not adopted; such arrangements (such as those where wages depend on firm profitability) in effect make the worker have an implied equity stake in the firm, and, given asymmetries of information, the value which workers are willing to assign to such contractual provisions is less than that which is acceptable to the firm.

The 1970s and 1980s represented decades during which the rational expectations/representative agent model was in ascendancy. This model suggested not only that, with rational expectations, government policy was ineffective, but that unemployment was not a serious problem. Neither of these conclusions made much sense to me; and with my former student, Peter Neary,

¹⁰ Subsequent work explored alternative versions of monopolistic competition. See Hoff and Stiglitz (1997), Salop and Stiglitz (1977) and Stiglitz (1979a,b, 1986, 1989a).

¹¹ Stiglitz (1987b)

¹² Greenwald and Stiglitz (1989, 1995)

¹³ Greenwald, Stiglitz, and Weiss (1984)

we sought to show that the results depended not on the rational expectations assumption, but on the assumptions concerning wage and price flexibility. We constructed a fixed wage/price model with rational expectations, and showed contrary to the suggestion of the rational expectations school, not only could unemployment be persistent, but that government policy was even more effective with rational expectations than without it (i.e. multipliers associated with government expenditures were larger.) The reason was simple: an increase in government expenditures today had some spill overs to future periods. Today's increased savings translated into tomorrow's increased income, and, with rational expectations, that increased income translated into higher consumption today. We also showed that there were *multiple rational expectations equilibria*: if everyone was pessimistic, then income would indeed be low today and tomorrow; but if everyone was optimistic, then both could be high.

Our work also emphasized that it was not just wage and price rigidities which could give rise to macro-economic problems. (This work could be thought of as a revival and formalization of Fisher's earlier work on debt deflation¹⁴) Incomplete contracts meant that unanticipated changes in wages and prices had large distributional effects, with correspondingly large consequences. While when we first put forward these ideas almost twenty years ago, they met with considerable resistance, they are now coming to be more widely accepted.

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While I spent most of my time teaching and doing research, I learned a great deal from the limited amount of consulting I did, and I thought it important to engage in issues of public policy. My first major consulting project was a direct outgrowth of work on imperfect information; it was concerned with the information externalities that arose in the process of oil exploration, externalities which played an important role in a heated dispute between the federal government and the states (which was eventually settled out of court for \$12 billion.) A variety of other consultations, typically associated either with antitrust violations or issues of corporate governance, gave me insights both into how real markets work as well as the behavior of firms.

In the 1980s, I was involved in two major public interest litigations, one concerning the treatment of Native Americans, the other with the exploitation of our natural resources. The first, involving the Seneca Indians in upstate New York, gave me further insights into the nature of America's past – and ongoing – exploitation of Native Americans. An unfair lease that had been imposed on the tribe was about to expire, and it insisted that it would renew only on more equitable terms. I helped calculate the magnitude of the amount by which the previous lease had "cheated" them – magnitudes in excess of a billion dollars in

¹⁴ See Fisher (1933).

present terms – and though the tribe was never compensated for these past injuries, the information I provided did, I think, contribute to a settlement which was far fairer than would otherwise have been the case.

The second suit was one against the federal government. In the 1980s, President Reagan tried to turn over as much of the offshore oil tracts to private companies as fast as he could – the fire sale was a give-away to the oil companies, depriving the American taxpayers of billions of dollars. Working with Jeffrey Leitzinger and a conservation minded NGO, NRDC, we tried to estimate this cost, and, unsuccessfully, to bloc the fire sales.

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I moved to Washington in March 1992 to join the Clinton Administration, first as a member, and then as Chairman, of the Council of Economic Advisers, in which capacity I also served as a member of the cabinet. The Council helps formulate economic policies for the Administration, and serves as a consultant for all the agencies in the government. Our span of responsibilities included not only macro-economics, but policies in almost every sphere, from trade to anti-trust, from environment to agriculture, from energy to transportation, from welfare to health, from social security to taxation, from affirmative action, to tort reform. It was a wonderful experience – I had to draw upon all of my previous research, all my connections, and go beyond. I became deeply involved in environmental issues, which included serving on the International Panel for Climate Control, and helping draft a new law (including a new legal framework) for toxic wastes (which unfortunately never got passed). I was pleased to see how ideas that I had helped formulate only a few years earlier, like adverse selection and moral hazard, were now part of the every day language of the policy debate in health care.¹⁵

Perhaps our most important contribution in this period was helping define a new economic philosophy, a “third way,” which recognized the important, but limited, role of government, that unfettered markets often did not work well, but that government was not always able to correct the limitations of markets. The research that I had been conducting over the preceding twenty five years provided the intellectual foundations for this “third way.”

Being on the Council was particularly exciting for me as a student of the economics of the public sector. I was a fly on the wall – but at the same time I could work to put into place some of the ideas that I had been developing.

¹⁵ See Stiglitz (1997b, 1998a) for brief descriptions of some of my views concerning these experiences.

I believe that institutions like the Council play an important role in our democracies. Work on information asymmetries emphasized the importance of incentives and the discrepancy between the incentives of government officials, and in particular professional politicians, and those who they are supposed to serve. As a citizen-bureaucrat, the members of the council, who are typically drawn from academia and return to academia, have markedly different incentives than those of a professional politician. Typically, though not always, the fact that our professional reputations as economists were at stake circumscribed what was said – we could not just be political hacks – and encouraged us to work for the adoption of economic policies that were consistent with economic principles.

When the President was re-elected, he asked me to continue to serve as Chairman of the Council of Economic Advisers for another term. But I had already been approached by the World Bank, to be its senior vice president for development policy and its chief economist. America's economic policy had been successfully redefined, and the economy was performing well. There were many problems yet to be addressed, such as pointing social security on a sound financial footing, but I was not optimistic about making progress on most of them in the coming years, given the Republican control of Congress. The challenges and the opportunities in the developing world seemed far greater. I had always wanted to return to the problems of development, and though I had had many visits to developing countries in the twenty five years since leaving Kenya, I had not really been immersed in their problems.

I had no strong agenda, other than doing what I could to promote the development of these countries, in ways which did as much as possible to eliminate poverty. But as I quickly became engrossed in the problems of development, a variety of issues surfaced, the most important of which was the intellectual framework with which development was to be pursued. In a recent article in *Atlantic Monthly*¹⁶ I described a trip to Ethiopia, where I saw the IMF advocate policies of financial market liberalization which made no sense, in which it argued that the country's budget was out of balance – when in my estimate that was clearly not the case – and in which it had suspended its program, in spite of that country's first rate macro economic performance. More broadly, the IMF was advocating a set of policies which is generally referred to as the Washington consensus, the neo-liberal doctrines, or market fundamentalism, based on an incorrect understanding of economic theory and (what I viewed) as an inadequate interpretation of the historical data. The IMF was using models that failed to incorporate the advances in economic theory of the past twenty five years, including the work on imperfect information and

¹⁶ See Stiglitz (2001a).

incomplete markets to which I had contributed. Most importantly, they had departed from the mission for which they had been founded, under the intellectual guidance of Keynes – they actually promoted contractionary fiscal policies for countries facing an economic downturn – and they advocated policies like capital market liberalization, for which there was little evidence that growth was promoted, while there was ample evidence that such policies generated instability.

As an academic I was scandalized; as a former adviser to the President who had helped design a “third way” for the United States – a view of the role of government that was markedly different from that envisioned by the Washington consensus – I was particularly disturbed by the role of the US government (or more accurately, the US Treasury) in pushing these views.

If the IMF had only *pushed* its views – misrepresenting them as the lessons of economic orthodoxy, describing them as if they were Pareto dominant (that is, they were policies which would make everyone better off, so that there were no trade-offs), rather than the policies which reflected the perspectives and interests of particular groups within society – that would have been bad enough. But all too often they used their economic power effectively to *force* countries to adopt these policies, undermining democratic processes. As someone who had grown up in mid-America, strongly inculcated with democratic values, I found this hard to accept; and even more so because the IMF’s own governance was so dissonant with democratic principles (a single country has an effective veto; countries like China were long underrepresented, the “governors” of the IMF, those responsible for its decisions, finance ministers and the heads of the central banks, are hardly representative, and the heads of the central banks themselves are typically not directly democratically accountable).

With the East Asia crisis, my disagreements with the Fund came to a head. The Fund’s policies seemed neither to accord with an understanding of the crisis countries (several of which I had studied closely during my East Asia Miracle project) and what I viewed as basic economics, especially as it had come to incorporate concerns about asymmetries of information and bankruptcy, corporate governance and finance, with which I had long been concerned. I argued against their prescriptions, and those within the World Bank broadly agreed. But I made little headway with the Fund. There seemed to be no way out other than to bring the issues out into the public – and since as a democratic, I believed that there should be public discussion of such issues, I had few misgivings. I believe the public pressure that was generated did work; the counterproductive policies of excessive monetary and fiscal stringency were eased.

A third set of controversies was opened up as the World Bank began its ten year review of the transition of the former Communist countries to the market. The failures of the countries that had followed the IMF shock therapy policies – both in terms of the declines in GDP and increases in poverty – were even worse than the worst that most of its critics had envisioned at the onset of the transition. There were clear links between the dismal performances and the particular policies that the IMF had advocated, such as the voucher privatization schemes and excessive monetary stringency. Other failures were related to the inadequate attention given to issues of corporate governance (the importance of which had, for instance, been stressed in my earlier theoretical work (see Stiglitz, 1985a) Meanwhile, the success of a few countries that had followed quite different strategies suggested that there were alternatives that could have been followed. Again, while the IMF defended its previous policies, I believe that the clear lessons that were drawn from these experiences did have some impact on policy prescriptions going forward.

I left the World Bank in January 2000. The US Treasury had put enormous pressure on the World Bank to silence my criticisms of the policies which they and the IMF had pushed, and though the President of the World Bank agreed with the stances I took on most of the issues, he was, I think, less comfortable about open discourse of these issues. I had come to the World Bank under an agreement that I would be more than a corporate spokesperson, that I could speak out on the relevant issues, in a responsible way. I believed, in part, that the credence that would be given to what I said – and my ability to advance the development agenda – depended in part on the perception that I was expressing my views, not just repeating the institution's official views. Under Treasury pressure, it was impossible to maintain this kind of independence, which had been a hallmark of the World Bank's research division, at least from the time that it achieved international prominence under the leadership of Hollis Chenery. I was, in any case, ready to return to academia – when President Clinton had asked me to be his adviser, it had been my intention to come to Washington for only two years; I had stayed seven, and although I had managed in that period to carry out a moderate research program, I had had my fill of bureaucracy. Still, it was a great disappointment to me that my own government should have gone so much against the principles for which I believed it stood, including transparency and the importance of the role of government. (My conversations with the President convinced me that he himself supported both my stances and the values that underlay them, but that the U.S. Treasury often did not adequately inform him about the policies they were advocating, let alone ask for his approval.)

The experiences during the seven years in Washington have helped shape my activities since then. I helped found the Initiative for Policy Dialogue, with

support of the Ford, Rockefeller, McArthur, and Mott Foundations and the Canadian and Swedish government, to enhance democratic processes for decision making in developing countries, to ensure that a broader range of alternative are on the table and more stakeholders are at the table. This effort has enlisted the support of dozens of economics and other social scientists throughout the world, in a set of task forces that are intended to lay out alternative policy alternatives in a wide range of areas, and has conducted policy dialogues bringing together academics, government officials, NGO's, labor leaders, and the press in a number of countries, including Serbia, Nigeria, Viet Nam, and the Philippines. Both through the Initiative for Policy Dialogue and independently, I have continued to take an active role advising governments on a broad range of issues, from the role of monetary policy under dollarization (Ecuador) to the reform of social security systems and second and third generation reforms in China, to the lessons that can be drawn from the past failures and successes for privatization, to the design of macro-economic responses to an economic slowdown.

I have also continued to work actively to change the international economic arrangements, including the international institutions, to make them more transparent, to ensure that the policies that they have been pushing reflect the interests and concerns of the developing countries, and especially the poor within those countries, as well as the advances in economic science of the past quarter century. I have been pleased with the progress that has occurred: perspectives, such as greater reliance on bankruptcy and standstills, that I had long advocated have now either been adopted or are at the center of the policy debate. But much remains to be done, and I anticipate that pushing this agenda will occupy much of my time in the years ahead.

My research agenda too has been greatly affected by these experiences. While I have continued the research program on the economics of information—I have recently completed a book with my long time collaborator Bruce Greenwald which explores more fully the implications of information economics for macroeconomics, and monetary theory in particular¹⁷—I have turned more of my attention to an analysis of the role of information and incentives in political processes, as well as continuing my work on development more generally. (Stiglitz, 1999a) Another major area of research involves the continuing analysis of the appropriate role of the state in the economy; in particular, how to design policies which combine concerns for economic efficiency, social justice, individual responsibility, and liberal values.

¹⁷ See Greenwald and Stiglitz (1999).

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