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| Biographical | Becoming an economist: From early preparation to my new direction For decades, my research was driven by outstanding problems in macroeconomics: mainly growth theory and employment theory. Then, around 1990, my research turned to the study of economic systems and my development as an economist took on added dimensions. This biography will start with my student period and go on to my new direction.[1](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#1) The two periods seem to me to be linked and I will be drawing some of the connections. I will recount too the intervening period when macroeconomics was in revolution and I was one of the revolutionaries. So this biography gives my account of the battles that started in the 1960s between, on the one side, those economists who wanted macroeconomic models to have lifelike actors whose expectations and beliefs were causal forces and, on the other side, those who did not.  A taste of creativity in Hastings I am not sure that growing up give full weight to the role of the early education of economists in determining their direction and ambition. When my Prize was announced last October the town in which I grew up, Hastings-on-Hudson, N.Y., proudly claimed six Laureates! Newspaper writers asked whether there was “something in the water.”[2](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#2) Most of those interviewed thought it was something in the school system. It turned out that four of those Nobels had been won by scientists who were not sons of Hastings, merely commuters to universities and research hospitals in New York City. The other two Nobel winners were genuine products of Hastings and its public school – the economist [Robert Merton](https://www.nobelprize.org/nobel_prizes/economics/laureates/1997/index.html) (son of Robert Merton, the Columbia sociologist, who was a dear friend) and me. I entered the school in 1939 and graduated in 1951. The best part of the high school in Hastings must have been the Music Department. Its orchestra and concert band did well in county competitions and the dance band formed by its students was the best in the region. I played lead trumpet in all of them. They were a gateway to appreciating the best professional musicians of the time. Music was always a part of my life. (One Sunday, at little more than age 8, I had been listening to the New York Philharmonic broadcast when programming was interrupted with news of Japan’s attack on Pearl Harbor.) In the postwar years I marveled at the musical imagination in the big band hits and the outsized personality of the famous soloists. I wanted to play like Harry James. Later I discovered radio station WBAI and was awestruck by the innovativeness of the modern jazz giants Hawkins, Parker and Gillespie. At Carnegie Hall and the Lewisohn Stadium I listened to William Vacchiano’s trumpet seeming to set the tone for the New York Philharmonic.[3](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#3) I grew up, then, with towers of creativity around me. So I tried to be creative too. A musicology course, I remember, required us to harmonize a C major scale. I don’t suppose I did that very well but that I was able to do something interesting with it surprised me and made me believe that most of us sometimes find inspiration that we could not have imagined beforehand. The high school was also unusual in giving a course in “creative writing.” The assignments were my first experience at inventing a fictional world – a kind of modeling.  I reached the conclusion, though, that whatever my creativity in music or writing might have proved to be, it was not promising enough to warrant attempting a career as a performing artist or professional writer. My hunch was that I would be able to exercise the same or greater creativity in some other field. So I looked forward to the explorations of other fields in college.  Humanities and philosophy at Amherst I attended Amherst College from 1951 to 1955. The first two years were a revelation. There were innumerable exchanges with brilliant classmates, among them the playwright Ralph Allen, the classics scholar Robert Fagles, and the composer Michael Sahl. Above all, there were some first-year courses that left a lasting mark. Decades later I could feel the full imprint they left on my outlook – in political economy and in my own life.  In English 10, no doubt as eccentric a course as was ever taught, the question was, “What is Amherst?” referring to the town of Amherst, Massachusetts. The answer gradually dawned on us after numerous class hours of being baffled and feeling stupid. There is no unique and true reality, only diverse representations of the town. In the diaries of Emily Dickinson (who lived there) the town is one thing, in a map of Massachusetts it is another thing. (By coincidence, at the Nobel ceremony I heard in the Citation of [Orhan Pamuk](https://www.nobelprize.org/nobel_prizes/literature/laureates/2006/index.html) that in one of his books he asks, “What is Istanbul?” The city could be described in terms of its bricks and mortar or its business life; Pamuk argued that another view came closer to Istanbul’s distinctiveness.[4](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#4))  After so many weeks of building to the point, none of us would be likely to forget it. Certainly I kept in mind that many among a diverse set of perspectives on some mechanism or system may capture some aspect of the truth that the others miss. A nation’s economy is more than its markets, tastes, technologies and property rights. Capitalism is a system for grinding out GDP, yet it is simultaneously something totally different.  The first-year humanities course was enthralling. We read the Greek epics and dramas of ancient times, Cellini’s *Autobiography* from the Renaissance, Chaucer and, if I remember well, Cervantes. Why was Cellini there? Why were any of them there? It has only been in recent years that I have become conscious of what the intent must have been. We were being instilled with the philosophy called *vitalism*. Cellini’s Autobiography was there because of its open celebration of his success – his ambitions and his “making it.” We were being shown the value of a life in which sights are set high and determinedly pursued. Yale’s celebrant of vitalism Harold Bloom was not yet around then but Columbia’s Jacques Barzun was sending that message in his books.  Two other courses, one a first-year introduction to philosophy and a second- year course in American philosophy made a deep impression on me. In the first course we read just three philosophers – but what an inspired choice. There was Plato’s *Republic*, where the “shadows on the cave wall” suggested the inevitable incompleteness of human understanding, and some of his *Dialogues*, including the one on social responsibility. Next came Hume insisting on the role of the “passions,” not mere reason, and the importance of “imagination” as against some historical determinism. Last was the brilliant [Henri Bergson](https://www.nobelprize.org/nobel_prizes/literature/laureates/1927/index.html). His upholding a life of “becoming” over that of mere “being” and his championing of “free will” over “determinism” made him the outstanding 20th century interpreter of vitalism. (He won a Nobel Prize in 1925.) In the second course the star was William James. His embrace of new problems and perspectives and his energy in expounding his ideas conveyed a vital way of living as well as a philosophy of vitalism.  To be sure, the many courses I took as part of my “major” in economics were important too but in a different way. Two courses with James Nelson and three with Arnold Collery were crucial to my decision to pursue economics further in graduate school. Both also provided me with information of use in deciding where to do my graduate work.  In autumn 1954 of my senior year, Nelson and Collery prevailed on [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economics/laureates/1970/index.html) to pay a visit to Amherst to meet and possibly recruit to MIT the four outstanding majors in economics that year. I had enjoyed enormously the brilliant style and the frequent humor of his textbook *Economics: An Introductory Analysis* and had read some of his articles too. His lecture was the most forceful I had heard – on Austrian business cycle theory, by the way. Of course I saw my interview with him as a privilege. It was the beginning of a long-lasting friendship. Among the economists I knew I admired Paul the most – admired him for his amazing breadth, unsurpassed clarity and penetrating insight – and loved him for his scrupulousness, seriousness and fairness. (When I asked him once why he alone in the world always referred to the natural rate as the “Phelps-Friedman natural rate” he replied, “because that’s the way it was.”) So I nearly burst with pride when in a packed hall with some eight Nobel Prize winners Paul gave the keynote address to the Festschrift conference for me in 2001. Later, I had the great pleasure of seeing a paper of mine in tribute to him appear in the 2006 Festschrift volume in his honor. Yet, as fate would have it, much of my work – the work after what Paul once called my “blue period” – has at times moved me far from his kind of economic model, most clearly my recent line of research on capitalism.  In the end I decided on graduate school at Yale rather than MIT. It was all I hoped for and more. Yet I will never know what inspiration I might have drawn from Paul and from [Robert Solow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1987/index.html), who was then bursting on the scene.  Two curents in graduate school at Yale My courses and interactions with Yale’s greats in the last half of the 1950s – William Fellner, Jacob Marschak, [Tjalling Koopmans](https://www.nobelprize.org/nobel_prizes/economics/laureates/1975/index.html), [Gerard Debreu](https://www.nobelprize.org/nobel_prizes/economics/laureates/1983/index.html), Robert Triffin, Henry Wallich, [James Tobin](https://www.nobelprize.org/nobel_prizes/economics/laureates/1981/index.html) and [Thomas Schelling](https://www.nobelprize.org/nobel_prizes/economics/laureates/2005/index.html) – led me to deep parts of existing economic theory and resolved whatever doubts I had over whether economics was for me. Part of the graduate school experience is one’s fellow students, of course, and I recall interactions with Robert Aliber, Paul MacAvoy, and, from other classes, Duncan Foley and Sidney Winter.  At Yale, it is fair to say, the current of Keynesian economics ran very strongly even in the 1950s, when Yale was full of intellectual diversity. What I learned from the leader of the American Keynesians James Tobin, who very generously gave me an individual reading course, and, in my last year, from the young Arthur Okun became an important part of my toolkit – something I was to build on and to modify. Tobin was stunningly intelligent. After one of the more impenetrable-seeming talks given in the Tuesday Cowles Foundation seminar there would be a long silence until Jim quietly asked a question that invariably showed the rest of us what the argument had been or ought to have been. He was so lucid that if there was a weakness in his argument you had a chance of spotting it. When he did not understand something he would modestly ask whether somebody could “make sense” of it for him. And he was self-depreciating to a fault – not an ideal role model for those of us who would be joining the theory wars. Yet I think those who admired Jim greatly, which I did, also could not help noticing an unreasoning conviction in some of Jim’s beliefs about the economy’s workings.  Another current of economic thought at Yale appears to have had an even more enduring influence on the character of my work. Yale, as I noted in the [Banquet speech](https://www.nobelprize.org/nobel_prizes/economics/laureates/2006/phelps-speech.html) concluding the Nobel ceremonies, had as many Europeans among its economics professors as Americans (or so it seemed) and as many non-Keynesians and Keynesians. Fellner, Marschak and Wallich made Yale a hotbed of the ideas that germinated in *Mitteleuropa* in the interwar years. I saw Schelling with Carnap on his desk and young Alan Manne was quoting Russell. From Schelling I grasped why it may be crucial to “abandon symmetry” in modeling decisions in circumstances of imperfect information where it is necessary to make conjectures.[5](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#5) (I suspect that my 1983 paper on the prospect for a successful result from the central bank’s announced intention to bring about disinflation owed something to Tom’s pioneering paper on “surprise attack.”[6](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#6)) From Fellner I caught his fascination with reshaping probability theory to deal with decision making under Knightian uncertainty.[7](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#7) And it was Fellner who imported to Yale the Austro-Hungarian idea, familiar to Mises, [Hayek](https://www.nobelprize.org/nobel_prizes/economics/laureates/1974/index.html), Lerner and others, that came to be called the “natural” unemployment rate.[8](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#8) At a 2000 celebration of 50 years of economics at Yale I saw that few of those present knew of Fellner’s influence: I had to tell them about the view of the world he taught and his contributions to economics.  My 1995 autobiography credited both the Amherst and the Yale economists for the stimulation and preparation they provided me. Yet it slighted the influence of Amherst’s education in the humanities and philosophy and Yale’s unique education in the real character of an enterprising economy on the view I took in my study of capitalism over the past 15 years. I am trying to rectify that in this autobiography.  Efforts at Yale to broaden growth economics My very first job after the Ph.D. was at RAND. It was understood that one might work on one’s own projects in the time left after the company projects and I took advantage of the remarkable personnel there to talk at one time or another with Richard Bellman, Richard Nelson, Albert Madansky, John McCall, Alain Enthoven, Bart McGuire, Harvey Wagner and others. After lunch I usually headed for the library where I would read in the areas of operations research and dynamic programming. The Naval Logistics Quarterly was a special favorite. My RAND project involved a two-dimensional dynamic programming problem, which I managed to solve. ([Kenneth Arrow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1972/index.html) remembered it a couple of years later, but too late to include it in a volume he was editing.) I was to benefit from this background a half-dozen years later when I began modeling the hiring and wage setting decisions of a decentralized firm. Nevertheless, I soon reentered the academic job market and left RAND in summer 1960, after a little more than a year there.  My academic position was right back at Yale, where I remained, apart from leaves, to the end of academic year 1965–66. My closest pals on the faculty were probably Gustav Ranis, Bela Balassa, David Cass and Arthur Okun. I had a goodly number of outstanding graduate students, including Mordecai Kurz and Seong Yawng Park, who wrote dissertations with me, and in the classroom Guillermo Calvo, Susan Rose Ackerman and Uwe Rheinhart.  My appointment was in the Cowles Foundation, so I had reduced teaching – a course load of 2 and 1, the same as now. A word about my teaching might be in order. In the first few years I worked as hard as needed to attain what I thought was a level of acceptability. I was amazed to hear Paul Steiger, the managing editor of the Wall Street Journal for the past couple of decades, say that I was the best teacher he had in Yale economics and was the reason he changed his major to economics. I may have exceeded my optimum level of effort. I was also surprised when Uwe Reinhart, the Princeton public policy economist, told me he keeps his notes from a graduate course I taught at Yale. My teaching was to decline in the future until I finally created a course to which I became increasingly devoted over the 1990s: a course on the political economy issues that drove many developments in the twentieth century.  In my first three years or so at Cowles, including a year away at MIT in 1962–63, I worked in the classical mode. Robert Solow along with Trevor Swan and some others were producing a fast-growing literature on economic growth – mainly from a policy point of view. Several papers of mine fit perfectly into that literature, most notably, the one on the golden rule and the one on accumulation of capital with a known probability distribution of returns. Some of these made me known in the profession at an early age. (Many explored the golden rule, including Solow, and Samuelson expanded the risky capital model, which sparked a wave of papers on intertemporal portfolio choice.) I took considerable pleasure in crafting them too.  My first step outside this box was a paper on technology formation. The Solow model supposed that new technologies rained down on the world economy like manna from heaven. I set out to model technological progress as produced, thus endogenous: a production function linked technology increase in the world to the volume of research. The function I posited had Isaac Newton’s “standing on the shoulders of giants,” later made famous by Bob Merton, which other modelers had not included. This led to a golden rule of research alongside the golden rule of accumulation. More strikingly, the model had the lesson that the world’s population growth must be credited with making possible much of the technological progress in the past two centuries.  I became restless, though, with the classical character of this growth economics as it had developed at MIT and Yale, with its postulates of perfect information and complete knowledge. Each new technology was instantly and costlessly implemented by producers and consumers, as if there was no need for managers to cope with change. There were not even any decentralized savers or workers in the theory. It was almost like classical physics. I was also guilty. I liked to say, evoking the two laws of thermodynamics, that Bob Solow’s result – an increase in the saving rate would increase output per worker – was the First Law of Economic Dynamics; and my golden rule result – it would increase consumption per worker only up to a point and thereafter decrease consumption – was the Second Law. Robert Summers (father of Larry Summers) told a story in his class at Yale of a puzzled student who, after seeing the equations of an economic model, asks the professor, “sir, where are the people in the model?” When a journalist asked me minutes after the announcement of my Nobel what had been the thrust of my work, I remembered that story and answered that I had tried to “put people” into macroeconomics. That was a bit much, since Solow too and others including David Cass had sooner or later put in some people, some consumers at any rate. We were all aware we did not live in a centralized economy. However, the mainstream growth model continued to make the classical postulates of perfect information and complete knowledge.  My move away from the classical began with a couple of works in the mid-1960s. A paper Richard Nelson and I published in 1966 recognizes that in a market economy any new technology introduced by an entrepreneur would not be immediately adopted by all managers or consumers, contrary to the costless and instantaneous implementation supposed by the standard growth model. Of course, entrepreneurs who fear such costs and lags may be scared off from developing innovations that they would otherwise see as profitable. Nelson and I went on to argue that a liberal arts education serves to enhance the capabilities of managers to evaluate new methods, thus to speed up diffusion of new technologies and possibly to encourage development of new innovations. This paper seemed to have a delayed fuse timed to go off in the 1990s, when it was picked up in books and papers by Robert Barro and Xavier Sala-i-Martin, by Philippe Aghion and Peter Howitt, and by Amar Bhidé.  An early exploration in a less classical vein was my book *Fiscal Neutrality toward Economic Growth*. It questioned the presumption that competitive markets do well from the point of view of intertemporal allocation, owing to the difficulty households must have in estimating their true wealth net of fiscal burdens. A neutral fiscal policy would undo the disequilibrating effects of such misestimation: it would gear people’s expectations of over-life tax burdens to what will be required to pay for public programs; thus it would tend to tax-finance public outlays other than for projects that will charge user fees. Running a budgetary deficit is apt to cause overconsumption and under-supply of labor. The book was a first try at thinking about mis-expectations – about economic disequilibrium and its correction. Whatever its importance may be, for me it was significant as a breaking away from some of the implicit assumptions of competitive equilibrium theory.  There was a story about that work. Shortly before publication I gave a Cowles seminar on my new theme: a good fiscal policy would collect just enough tax revenue so as to cause households to feel as rich as they really were – that is the presumption, at any rate. Jim Tobin was visibly annoyed. He insisted that we cannot understand the effects of fiscal policy without allowing for the role of money in the economy. I probed to see the basis for that belief but did not find any basis, at least none I perceived. (Jan Tumlir, a young colleague at the time, said it was the most scintillating exchange he had heard at Yale.) That seemed odd to me and somewhat worrying. Such resistance was to break out in full force in the 1970s when several neo-Keynesians railed against the natural unemployment rate. Later I thought I detected a similar spirit in some proponents of New Classical economics. This may be inevitable. In any field some tenets are very stubbornly held.[9](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#9)  Later, in a paper with Robert Pollak written at Penn and published in 1968, the subject was what households are to expect about the consumption of future descendants – or, possibly, their future selves. The assumption that “they” will consume as “we” in the present would like them to is not at all general: they might have a selfish streak, just as may do, and thus save less than we would like. But how much saving would that be? Pollak and I were able to work out a sort of game-theory solution – a step back to intertemporal equilibrium, in which future people save as they are expected to do. But it is not a good competitive equilibrium, as there is (in a well-defined sense) under-saving by every generation.  Bringing expectations to employment models at Penn By the middle of the 1960s, I began to be aware that neither I nor anyone else was addressing what I felt ever since college was the most important challenge in economics: to integrate microeconomics and macroeconomics. Finally I decided to try to do it myself! I had already concluded that textbook micro, which was based on the classical model of perfect competition, truly was irreconcilable with anything like the prevailing macro. I suspected that any macro that we might find recognizable would have to be based on some *different kind* of micro – a micro in which, say, individual firms with their employees or customers were in imperfect communication with other firms and, more generally, the rest of the economy. (Later I was to think also of widely separated islands of workers, each island reachable from the others only by a slow boat.) I immersed myself, with a few breaks to do other things, in a project to rewrite the Keynesian economics of employment determination from the first half of 1966, when I started work on the project at London School of Economics (LSE) and Cambridge during a sabbatical leave from Yale, through 1971. The conference I organized on the subject took place in Philadelphia in January 1969, financed out of a threatening surplus in my National Science Foundation (NSF) research account. The conference volume *Microeconomic Foundations* – later known as “the Phelps volume” – was published in 1970. A monograph of mine that was a sort of sequel, *Inflation Policy and Unemployment Theory*, was written largely over 1970 and appeared in 1972. (Most of my papers in this area were published in my collection *Studies in Macroeconomic Theory*, Vol. 1, 1979.)  For the great bulk of this period, specifically from 1966–67 to 1970–71, I was a professor at the University of Pennsylvania. The advantage of Penn relative to my preceding home and maybe to my future home as well, was that there were not any established macroeconomists on the faculty and the younger ones there were not focused on employment or inflation, so I was not challenged to defend my thinking against the views of colleagues. This was also the disadvantage, of course, since there were few around who might alert me to mistakes. Still there were plenty of first class minds. I had friends in Karl Shell, Robert Pollak, Edwin Burmeister and [Edward Prescott](https://www.nobelprize.org/nobel_prizes/economics/laureates/2004/index.html) (from oldest to youngest). The first three all wrote a paper with me – Karl one about the incidence of public debt and Edwin one about which state agency to assign inflation control. (The paper with Bob was discussed above.) Ed Prescott gave me expert advice on my 1972 paper on what I dubbed “statistical discrimination” – the idea, introduced in my 1972 book on inflation policy and unemployment theory, that a person may be judged by the labor market in part by the group or groups in which he or she is categorized. I also engaged Ed in a discussion of that book’s digression on “routine stabilization.” At Penn there was also a brilliant undergrad, Steven Salop, who worked for several months on my 1968 paper on money-wage dynamics.  Confusion abounded at that time on the subject of employment determination. Keynes had said that, in general, one can expect “involuntary unemployment,” by which he (though no one before or since) meant a sort of under-employment that results from overestimates of prevailing labor market conditions causing the population to hold back some labor; the limiting case was called “full” employment, it being supposed that there could not be anything that might be called “over-employment.” If we departed from Keynes by allowing the existence of over-employment, resulting from underestimates of labor market conditions causing the population to offer supra-full amounts of labor, then the economy could be seen as ambling between underemployment and over-employment. Would there be a tendency toward recovery toward the “full,” or equilibrium level? He didn’t say yes, though he didn’t say no. (Nor did he say whether this equilibrium level might itself tend to be too low.) Keynes himself was not sanguine about the central bank’s ability to narrow employment swings around the equilibrium level, which might itself be subject to swings and shifts, of course. But in a time when employment and the price level had sunk to very depressed levels Keynes viewed interest rate cuts by the central bank as a means to pull up employment – and the price level with it.  Unfortunately, A. W. Phillips came upon an empirical relationship between the measured unemployment rate and the inflation rate – a negatively sloped curve, which was dubbed the Phillips Curve. There soon developed a neo-Keynesian aggregative model based on the postulate of such a “longrun Phillips Curve.” This model implied that a step-increase in the stock of money or its velocity would increase output and jobs at first but, as the price level rose, business activity would be driven back to equilibrium. But this same model implied the feasibility of a *permanent* increase of employment through successive increases in the money supply to accommodate successive increases of the price level – a vision beyond what Keynes saw to be the promise of demand management. This seemingly rock-solid Phillips Curve put the American Keynesians into a sort of euphoria, as if they had discovered atomic power. They, unlike Keynes himself, had long harbored the faith that the central bank was able to dial the average level around which employment would fluctuate – a claim the Continental monetary theorists Mises and Hayek had denied, arguing that the inflation rate would spiral upward if the central bank set too low an unemployment rate and spiral downward if the bank set too high an unemployment rate. With the establishment of an empirical Phillips Curve that was breathtakingly stable, there seemed to be no reason for the central bank to hold back from supporting low unemployment.  In the view I took in papers written in 1966 and 1967 (published in 1967 and 1968, respectively), it is a mistake to posit any such long-run negatively sloped Phillips Curve – to posit a long-run relationship running from long-run inflation rate to long-run unemployment. So a monetary policy shift that decreases the target inflation rate – the sort of event studied in my 1967 paper – should not be supposed to aggravate the level to which the unemployment rate tends in the long run; symmetrically, an increased tolerance for inflation should not be supposed to usher in a long-run decrease of unemployment. Even if there is a short-run Phillips Curve, such a curve takes the *expected* rate of inflation as a *given*; this *expectations-augmented* Phillips Curve will lie higher the greater is the given expected inflation rate. I then argued that a central bank policy to establish and maintain (un)employment at a level that would always represent, say, *over*-employment, so that the inflation rate would always exceed the expected inflation rate, would sooner or later cause the expected inflation rate to be *rising* and thus cause the economy to suffer an ever-higher Phillips Curve. This state of affairs could not be sustained.  The 1968 message, then, was that the central bank could not permanently maintain over-employment. The 1967 message was that it could engineer reduced inflation expectations and thus reduce actual inflation without causing *permanent* underemployment – but not without causing a *temporary* bulge of unemployment.  These two basic conclusions have apparently survived the controversies that arose over this analysis. The corollary was the policy message of the 1967 paper: the central bank’s main responsibility must be seen as controlling inflation – by means of a money-supply or interest rate policy aimed at managing inflation expectations. Inflation will still be capable of ups and downs but it cannot go far if the expected inflation rate is under control.  It is sometimes asked whether there was a Eureka moment in all this. The closest I came to that was when I realized early one morning in September 1966 that in the theoretical setting I was focused on, it is *wage* expectations – what employers and employees expect wages at other firms to be over the near future – not *price* expectations – their expectations of what the price level is going to be – that are crucial for firms’ wage setting and employees’ quit rates. (I took the poetic license of closing down workers’ price expectations by taking labor supply as perfectly inelastic.) The difference in setting and in the consequent expectations mechanism lay behind the contrast between my results and those of [Milton Friedman](https://www.nobelprize.org/nobel_prizes/economics/laureates/1976/index.html), also published in 1968. His analysis was in fact directed to the natural level of labor-force *participation* while mine was actually directed to *unemployment*. Yet there were strong parallels in our conclusions and it was inevitable that we were soon yoked together – except for that minority of journalists who referred always to “Friedman’s natural rate of unemployment”!  I was not the only economist beginning to model unemployment determination from a micro point of view. Work by several others began to appear in manuscript or preliminary form over 1968, of which a paper by [Dale Mortensen](https://www.nobelprize.org/nobel_prizes/economics/laureates/2010/) was closest in broad outline to what I was doing. There was also the paper on customer markets that Sidney Winter and I were producing that year. Thinking that there is strength in numbers I decided to try to organize a conference on the subject at Penn in January 1969, drawing on some unused funds in my (NSF) grant. A paper by Armen Alchian offered an overview of informational imperfections in product and labor markets. Focusing on the labor market were papers by Mortensen, Charles Holt, my 1968 paper (modified but not for the better) and, with a quite different model, the paper by [Robert Lucas](https://www.nobelprize.org/nobel_prizes/economics/laureates/1995/index.html) and Leonard Rapping. The Phelps-Winter paper and some others focused on the product market. This almost instant convergence of so many scholars on so new a subject was exciting to see and such openness to new ideas made me admire the profession. It was also something special to discuss questions about future directions with a group so remarkably talented. The exchanges with Bob Lucas, who was already mapping out his own journey, were especially interesting. We both expressed regret at one time or another that we did not have more conversations.  The conference volume *Microeconomic Foundations* appearing in March 1970 sent shock waves through the economics profession – certainly the younger generation. The blurb on the back of the book’s jacket by Robert Hall clearly suggested it marked a paradigm shift for macroeconomics. Right up to recent times I have heard economists tell how excited they were to have the book in their hands and to start poring through it. This publication was the biggest “high” in my scientific experience. A huge advance had been made but the war had really just begun!  There was stiff opposition from a great many “Keynesians.” It was more than I would have thought likely since, in my mind, my work was not radically opposed to Keynes. It clearly recognized, with Keynes, that the central bank has the responsibility of managing “effective demand” – to keep it under suitable control – and that doing so would help to foster employment stability. If a change in “effective demand” spontaneously erupted, an ideal monetary policy would act to close the gap. My work eventually pointed out that if a supply shock occurred, such as an overseas shock to the world price of energy, an ideal monetary policy might make some response. (Such a shock might wreak wage deflation, absent any response.) Though not a wanton interventionist, I did not presume that the right policy was always *laissez-faire* – known now as “free market” economics. I understood that every real-life market economy was neither efficient nor just – not even close to it – and there were some limited measures that the government might usefully do to improve its performance.  Some neo-Keynesians claimed my work could not be right, arguing that it implied “acceleration” of prices and wages whenever the economy was away from the “natural rate” while the data showed that a mounting rate of disinflation was not characteristic of the 1930s – the years of the Great Depression out of which Keynes’s theory was born. It is true that my 1967 paper implied mounting acceleration of the price level (i.e., an increasing inflation rate) if the central bank were to hold employment steady at a level above the “equilibrium” level – which was a constant in that first model, not the path it became in the 1968 paper. The expected inflation rate was rising, pushing up the actual inflation rate accordingly, because the actual inflation rate was always greater than the expected rate, owing to the postulate of “adaptive” expectations. That postulate was particularly reasonable in the context of that paper: if the central bank were to decide to try to *drive down* the expected inflation rate, credibility issues would require the bank to generate inflation rates that were steadily below expectations and thus to drive employment to a depressed level in order to convince an initially skeptical public of its determination to wring inflation expectations out of the economy. It was realistic to suppose expectations would be adaptive in such circumstances: that they would gradually recede in response to their constant disappointment. But adaptive expectations would *not* appear to be realistic in all other circumstances.  In a time of economic depression and resulting deflation it would not be realistic for the public *indefinitely* to expect the rate of deflation to be greater than it was in the last observation – certainly not once it was crystal clear that the central bank was not trying to increase the rate of deflation and thus had no desire to hold employment below its natural level. A point may very well have come in the decade, possibly around 1934, where the price level and employment too had sunk so far that people began to expect “reflation” and recovery. Then the price level might level off, the expectations of inflation just counterbalancing the deflationary force of the deficiency in effective demand. Yet that would be a *disequilibrium* condition, not a “low-level equilibrium” and in no way a disconfirmation of the hypothesis of the existence of a natural rate. (I remarked on this in a review article, “On Okun’s Micro-Macro System,” *Journal of Economic Literature*, September 1981.) However, the criticism by neo-Keynesians paid no attention to my 1968 paper in which, starting from a depressed level of employment, there would be an *equilibrium path* leading toward the medium-term natural rate. Along this path, despite massive joblessness, the actual inflation rate would not be below the expected inflation rate (nor above either). Thus the accompanying price level and general level of money wages might be *flat* or even rising.[10](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#10)  There was criticism too from the emerging New Classical School. This was directed not against my thesis of a natural rate, which the New Classicals embraced, nor against my insertion into macro modeling of wage or price expectations, which the New Classicals also embraced, but against the treatment of expectations behavior (by me and others) as adaptive. The argument, as I heard it, was this: if, whenever employment and prices or wages are pulled up into the high-employment region (above the natural level), the economy’s participants were to go on under-forecasting the inflation rate (beyond the inflation surprise that might well be caused by the initial shock driving up employment), that regularity would mean *either* the public was being obtuse in not soon grasping the understanding contained in the analyst’s model *or* the public has an understanding not given to the analyst – the public was using a model that differs in this regard from the model the analyst is using. Since it is impossible to believe that participants would commit the same forecasting error over and over again, it must be that it is the analyst’s model that is “wrong:” it has failed to pick up something that the participants’ model takes into account. *The implication was that I and the other microfoundationists were using ‘wrong’ models.* This was impressive. I appreciated the brilliance and the seriousness behind the argument. To the best of my recollection I never derided it or sought to defeat it by rhetoric alone.  The argument made my head spin. I understood where they wanted to go: they saw the immense analytical convenience for an analyst of imputing to the actors in their decision making a use of the very same model the analyst is using. (I had considered the “perfect foresight” case in my *Fiscal Neutrality* as had Robert Hall and others in the 1960s.) But I kept asking myself whether it followed that my “wrong” models are illegitimate or inferior to some other. How could a real-life analyst’s model *not* be wrong? I recalled that every model of Amherst has its limitations and many have their merits. A model abstracts from details and sometimes from complexities that are present in reality. So it may be common and almost inevitable that there are some enduring misalignments between what the analyst’s model says the participants would do if they knew and subscribed to that model and what they actually do. In some respects, some participants may very well be more knowledgeable than the analyst or the planner, as Friedrich Hayek taught.  A propos the issue of inflation expectations in particular, participants may suspect that, although employment has moved up into what might be supposed to be the zone above the natural level, the natural level may have moved *even higher*, so that employment, while perhaps historically high, is not actually higher (and may be even lower) than the natural level currently is. Such a structural shift might have preceded the demand shift or have occurred jointly and thus at the same time. So, on observing a movement of employment to historically high levels, people might reasonably give some probability-type weight to the possibility that the underlying shock, whatever it is, has pulled up the natural level of employment even more than the actual. (Papers I wrote with Hian Teck Hoon and with Gylfi Zoega between 2000 and 2003 showed how an investment boom, which surely increases effective demand, shifts up the nonmonetary equilibrium path too.)  A further point: I did not remember postulating a Cagan-Nerlove adaptive expectations equation as a general rule. (Such an equation is simply not in the model presented in my 1968 paper, which is very largely about *equilibrium* paths, not disequilibrium paths.) My thesis could not be fairly summarized as the idea that employment is slow to equilibrate because expectations are adaptive – as if this were a piece of *psychology* I was importing into macroeconomics. My thesis was that participants, *not observing* the simultaneous wage and price decisions being taken elsewhere, have to form *expectations*; and, with participants not knowing completely, if at all, the *structure* of the economy, the expectations formed are a *causal force* that have to be reckoned with when we *analyze movements* in (un)employment and movements in inflation.  I dwell on these two bodies of criticism because they manifested a schism in economists’ views of the economy. The pioneering figures of the modernist view – Knight, Keynes, Hayek, Morgenstern, Zeuthen and Fellner – did not take the economy’s structure, let alone the timepath of that structure over the future, to be completely known. The *neo*-Keynesians and the New Classicals were *both* on the other side of this schism, preferring mechanical models of economies of known structure, as Axel Leijonhufvud and Roman Frydman have long pointed out. There was therefore a deep affinity between them – an affinity at the level of methodology. Since this is my biography I have to say that this put me in a strange situation. I was close to them, having warm personal relations with Jim Tobin and Bob Solow as well as with Bob Lucas and Tom Sargent – relations that have survived our differences. But I belonged to neither school.  As I began my gradual departure from Penn I thought back more than once to 1968, a year in which I somehow published three seminal papers – “Money-Wage Dynamics,” Phelps-Pollak and “Population Increase” – and 1967 and 1969 were good too. I wondered whether this could last. It did not. But there were two more periods of unusual creativity ahead: my structuralist modeling from about 1986 to 1992, which depended on what I learned in the 1970s, and my work on capitalism theory from about 1998 to 2006, which depended on what I learned earlier in the 1990s.  From monetary macro to economic justice at Columbia I left my professorship in the Economics Department at Penn for one at Columbia, starting in summer 1971. Although New Haven and Philadelphia had not been very far away, it was exciting to be right in New York City, with Bernstein at the Philharmonic, Balanchine at the New York City Ballet, Levine at the Met, and plenty of Stoppard and [Pinter](https://www.nobelprize.org/nobel_prizes/literature/laureates/2005/index.html) on or off Broadway. The move brought a momentous change in my personal life. At Columbia in early 1972 I met Viviana Montdor, a strikingly attractive and very cultivated young woman from Buenos Aires. I found her beautiful, smart and very insightful. Her background, which was so different from mine, fascinated me. We were married in 1974. She has been hugely supportive of my career. Viviana brought two children, Monica and Eduardo, to our marriage. Now we have seven grandchildren. I feel extremely fortunate.  Let me say that at Columbia, where my appointment has been for 36 years now, I have been fortunate to have as colleagues [Bob Mundell](https://www.nobelprize.org/nobel_prizes/economics/laureates/1999/index.html) and, in the present decade, [Joe Stiglitz](https://www.nobelprize.org/nobel_prizes/economics/laureates/2001/index.html). We have had great fun together. In both cases, though, our friendship is based, I believe, on our mutual respect for each other’s work.  The immediate problem to be tackled at Columbia was that, after the Interwar period when it was right up with Chicago and Harvard, it did not have the breadth of first-rate talents that its tradition and location suggested it would have. The relative newcomers among us – the chairman Kelvin Lancaster, Phillip Cagan, Ronald Findlay and myself – set about to strengthen the Economics Department at Columbia. Within three years we recruited Robert Mundell and Carlos Rodriguez to international monetary economics, Guillermo Calvo and John Taylor to macroeconomics, and Phoebus Dhrymes to econometrics. In addition, there were Jacob Mincer and the young [James Heckman](https://www.nobelprize.org/nobel_prizes/economics/laureates/2000/index.html) manning labor economics and [William Vickrey](https://www.nobelprize.org/nobel_prizes/economics/laureates/1996/index.html) covering public finance. (Yet the *Wall Street Journal* wrote of a “decline” of Columbia’s economics in the 1970s!) That this renaissance happened at all is more important than the fact that it faded. Some left, including Heckman in mid-decade, Taylor at the decade’s end and Calvo in 1986. In the present decade three newcomers have worked with me: Max Amarante, Amar Bhidé and Richard Robb (the latter two outside the Department). In my Columbia years I have also been fortunate to have several extremely talented graduate students. Among those a part of whose dissertation was related to my work have been Janusz Ordover, Roman Frydman, Juan Carlos di Tata, Luigi Bonatti, Hian Teck Hoon and Gylfi Zoega. And I have basked in the friendship of Graciana del Castillo, Luis Campos Cunha, Alfredo Navarete and Agustin Garcia Lopez ever since their classes with me. Later, New York brought us our close friend Pentti Kouri, who has been a supporter of my career and a help in my work. With my move to Columbia I wanted to take up a new research theme. Every time I moved – from Yale’s graduate school to RAND, from RAND to Yale’s Cowles Foundation and from Yale to Penn – I took a new lease on life. Or, maybe, every time I needed a new lease I moved. When I arrived at Columbia in September 1971, with my book *Inflation Policy and Unemployment Theory* in final draft, I was itching to leave monetary economics and get back to “real” economics. But what exactly? I had a stroke of luck there. During 1969–70, when a visiting fellow at CASBS, my office was next door to that of John Rawls. My memories of our many conversations were still reverberating and these provided the inspiration for a new start once settled in at Columbia.  The research I began first, though, was not in Rawls’s field at all. Somehow I had come to the idea that, in an economy with imperfect information, there would be social benefits from private altruism and personal morality. (I had read Ibsen’s powerful play “The Enemy of the People” and its parable stayed with me.) It adds to enforcement costs if people hide taxable income, drive through red lights and generally conceal harms they are doing to others. It discourages experiment and change if sellers can be expected to misrepresent unfamiliar products and mislead potential buyers. I went to Eleanor Sheldon, then still at the Russell Sage Foundation, to ask her support for a conference on the subject. She had to fight for it, she later told me, but got the money. The conference ran two half-days. Samuelson, Kenneth Arrow, William Vickrey, Peter Hammond, [James Buchanan](https://www.nobelprize.org/nobel_prizes/economics/laureates/1986/index.html) and I were among the authors. (My paper extended the Phelps-Pollak model to continuous time, which changed some results.) [Amartya Sen](https://www.nobelprize.org/nobel_prizes/economics/laureates/1998/index.html), Thomas Nagel, Guido Calabresi and Karl Shell were among the commentators.  The conference volume, *Altruism, Morality and Economic Theory* became a cult hit. To this day the occasional sociology student comes from France to ask me about the conference’s origins and its reception. (They may have known that it was not widely read.) This conception of the market economy was anathema to Chicago at that time. So I knew I was sticking my head in the lion’s mouth when at the invitation of George Stigler I decided to present my draft introduction to the conference volume before Chicago’s Law and Economics seminar. Entering the hall and taking my place, I looked up at steeply raked, semicircular rows of professors peering down at me and seeming to have me surrounded. For what seemed like hours, Gary Becker, Robert Posner, Stigler and others pounded away at me. I held my own, flagging only at the end when I grew tired or fed up. “You were doing so well,” one professor complained to me in a thick Viennese accent, “so why did you make that concession?” In my letter to George with my expenses and thanks I said that “now I know how Dr. Freud felt facing Vienna’s College of Physicians.” (I was comparing the two lecture experiences, of course, not comparing my thoughts on altruism to Freud’s theory of the unconscious.)  I was soon thinking again about Rawls. In papers written over the 1970s I explored the tax mix for a Rawlsian optimum, the inflation tax, optimal Rawlsian public debt and private wealth, and Rawls’s concept of economic justice and consequent issues for welfare economics. (Most of this work and earlier work in the same or a related vein is contained in my collection *Studies in Macroeconomic Theory*, Vol. 2, 1980.) [James Mirrlees’s](https://www.nobelprize.org/nobel_prizes/economics/laureates/1996/index.html) seminal 1970 paper on optimal wage-income taxation paper provided a necessary tool and the inspiration for an analysis of another problem: the optimal shape of the (generally nonlinear) tax schedule for wage income, which I went on to study in more than one setting. In these settings, I took the Rawlsian “least advantaged” to be an earner whose productivity is bound to be negligible, whatever tax policy may be. Thus this earner’s reward from work is essentially the size of the wage subsidy at the low end of the labor force. So Rawlsian justice meant raising the maximum possible tax revenue. This, it turned out, required setting the marginal tax rate on the last dollar of wage income of the highest earner equal to zero! (Efraim Sadka found the same result in a utilitarian framework. [Joseph Stiglitz](https://www.nobelprize.org/nobel_prizes/economics/laureates/2001/index.html) then analyzed a more general model in which the marginal tax rate at the top is negative!!)  How did Rawls view this result? Did he think it was immaterial? Not at all. He insisted that what is just cannot be determined with finality until “the consequences” of what analysis has provisionally pointed to as just have been explored and determined. I was to have two other significant areas of interchange with Jack over the years. The most fundamental was on the very meaning of his “justice.” A long letter I wrote to him from Amsterdam in 1980 said that his work was being taken as support for a universal basic income – a “negative income tax”, or “demogrant” – while his book clearly states in its first pages that the concept of justice it is concerned with is not some general social justice but rather economic justice in the structure of rewards for active contribution to society’s production. The matter lay there for quite some time. Some movement occured in 1985 when, writing to me about my just published textbook, he said its exposition of his theory of justice was entirely accurate. But that was not a public statement. Finally, in his last publication, one in the form of an interview, he underlined for those who had missed it this feature of his concept of justice. (He said that the book did not apply to “beach bums.”)  I mentioned work on the tax mix. Janusz Ordover, a young economist from Warsaw asked me to advise him on the last chapter of his doctoral thesis. (I was grateful to Ron Findlay for having introduced us.) Together we worked out the answer to the question of the optimal linear tax structure – the configuration of flat tax rates on wage income and the income from wealth – under various possible constraints involving maintenance of a subsequent steady state. When both private saving and public saving are unconstrained the analysis yielded simpler results! In that case the optimum tax rate on income from wealth is indeed equal to zero, as Corlett and Hague would have guessed, though that would not generally hold if the government’s algebraic surplus were constrained to be zero. (The taxation of wealth has been a continual obsession, surfacing most recently in December 2005, always without noteworthy results.) Through the algebraic haze could be seen another result: there exists a level of public debt and associated tax rate to sustain it that maximizes tax revenue. Thus, our paper (*American Economic Review*, September 1975) could be said to have characterized what it is like to be at the top of the Laffer Curve. For Rawlsians like us, it was good for society to be at the top of that curve. (Certainly it was good for society’s least advantaged participants.) Supply-siders felt that would be bad for innovation. To this day the issues remain unresolved.  Over the decade I also found time for the issues of that time in macroeconomics. Taylor and I, later joined by Calvo, built a series of models of the New Keynesian type. These constituted a sort of answer to the New Classical models coming out of the Midwest. In my 1968 paper I had pointed out that, for a model that would fit quarterly or annual data at all well, it might be important to take into account that firms’ managers cannot afford to spend every waking moment monitoring the economy with an eye to revising their posted wage rates and prices. The Columbia models were a “marriage of convenience” of that important point to the New Classical idea of rational expectations. The purpose was only to show that recoveries would be gradual, or sticky, even if rational expectations prevailed. For me, the models did not signify that the rational expectations hypothesis was a good idea, only that it was an expedient in a first round of modeling.  In fact, new differences between my work and the literature using rational expectations cropped up in those years, notably in my 1977 article “Indexation Issues” and its appendix, coauthored with Guillermo Calvo, “Employment-Contingent Wage Contracts” (*Journal of Monetary Economics*, 1977). The starting point was the observation that in a capitalist economy, more generally, in any enterprising economy, an element of the so-called s*tate of the world* has to be the subjective and only indirectly measured “visions” of the entrepreneurs. In such an economy there *are* no “rational expectation” of what the entrepreneurs are thinking – there are only their “animal spirits,” as Keynes said, referring to Plato. It followed that the neoclassical theory of the wage contract would be simply inapplicable to such a setting: In that model, the employment contract would link the number of employees who have the misfortune to be called up for work on any particular day to the “state” of the economy; but for the entrepreneur the state is something that he or she feels but cannot communicate to the employees nor would he or she be believed, owing to the temptation to pretend the state requires more labor input than it would if the contract were followed. I was surprised that the sad state of contract theory did not provoke something of a scandal. But apparently no one really cared very much what its implications were and how well or badly they matched up to any rough impression of reality.  A conference that Roman Frydman and I organized in 1981 contained another paper of mine that was different from rational expectations. Rational expectations means “rational” (in a manner of speaking) *relative* to a model, namely the analyst’s. I pointed to the existence of a more general case – an economy in which there prevails a pluralism of models: some actors in the economy operate on the monetarists’ model, others on the Keynesian model, still others on the supply-side model, and so forth. Here, I suggested, it would be wrongheaded for any of these *actors* to pretend that all the other actors follow a model the same as theirs; and the same goes for each *analyst*. Presumably analysts will find no use in the concept of rational expectations equilibrium in this case and will instead try to model the economy – in a stylized way, of course – *as it is*: to take account of the prevailing pluralism of models. I proposed that analysts of every persuasion will recognize that the monetarists in their model take account of the presence of Keynesians (and other dissidents) in the economy, that the Keynesians in their model take account of the monetarist (and other dissidents) in their midst, and so forth. (This is probably the best interpretation of the term “model-theoretic expectations” that Frydman and I sometimes used.) In my paper for the conference, however, I took only a small step in that direction by studying the case in which, for the sake of simplicity, everyone has the same model of the fundamental workings of the economy but, crucially, they do not know it: everyone believes that everyone else is operating on a *different* model. The conference volume, Frydman and Phelps (eds), *Individual Forecasting and Aggregate Outcomes*, was published in 1983.  Writing political economy: from Buenos Aires to Stockholm When the 1980s arrived there was still an item left on my agenda: a contract with W. W. Norton signed in 1971 to produce a different kind of introductory textbook. Why did I do it? It is not unusual – at least it was not when I was young – that a textbook is written by someone who sees himself as an important voice of his times or who hopes that the textbook will make him one – Samuelson, Fisher, Marshall, … As a pioneer in the 1960s of what I called “modern economics” I felt a need to introduce college students to what this revolution was and to some of the insights to which it led: Here are two passages early in Part 6, the first of the textbook’s two modern parts:[11](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#11)  In the *modern* style of market analysis, … information and knowledge hold the spotlight. In this modern view, messages are expensive to send, hence information is costly to find, and secrets are cheap to conceal, hence knowledge is not completely shared in m*ost or all markets*. The resulting focus of this modern theory is on the r*oles of imperfect information and incomplete knowledge* in the performance of the market economy – on the difficulties the market players have, as consumer and as employer or employee, in acquiring information and knowledge, and the consequences of those difficulties for the functioning of markets …  With the classical magic not there – no costless ‘auctioneer’ …, no unseen ‘regulator’ – the transactors are driven to collect their own information and to make their own arrangements … Thus some aspects of primitive exchange … are brought back: communicating terms, perhaps bargaining, watching out for cheating and other ‘moral hazards,’ aiming for cheat-proof incentive-based agreements, and so forth.  The payoff from any new theory, of course, lies in how it expands our understanding … The modern theory of markets, largely developed in the past two decades, has illuminated a whole universe of observations: the prevalence of the long-lived ‘firm,’ the mark-up of price above unit cost – hence a pure profit called ‘good will,’ undue hazards in products and occupations, reputable firms and deceiving ones, ‘job rationing’ and unemployment (even in equilibrium), discriminatory stereotyping in hiring and lending, layoff and seniority rules, and much else – a world of phenomena not understandable by neoclassical theory. (pages 380–381; italics in the original)  The inner need to produce the textbook was one thing, getting down to it was another. (Evidently I was not one of those *Schumpeterian* entrepreneurs who “get it done.”) Yet pressures to get it done were mounting. I was grateful to Donald Lamm for bringing me to Norton in 1961, they had made me their advisor in economics and had given me a nice advance. The time had come to deliver. At the same time Viviana was making the case that I ought to get out more into the rest of the world. The solution was a series of visiting faculty appointments overseas in Latin America and Europe where I could work mostly on the textbook and learn more about overseas economies. That also served to break up the large writing project into separate periods in varied settings and to replace the earnings that might have been made in other ways. In these trips Viviana would join me with a lag, accompanied by Monica and our precious dog Shaggy.  The first trip was a longish one in the early months of 1980 to CEMA in Buenos Aires at the invitation of Carlos Rodriguez, who earlier had been at Columbia. I remember to this day working on the first page and a half for two weeks, if not longer, in January 1980. (Odder still, I took a break somewhere in the midst of this effort in order to write what would be my paper for the Frydman-Phelps conference of 1981 and volume in 1983. I may have needed that break to avoid going crazy over pages 1–2.) The rest of the book went faster. But never fast. The problem was that there were few sections, let alone chapters, where I could simply imitate other textbooks. That summer I worked a term in the University of Amsterdam at the invitation of Jan Kremer – signed by the Queen, he said. After a year, I had done only three chapters of a projected 26, though. I was seriously behind now, so I worked harder in mid-1981 at the Getulio Vargas Foundation in Rio, invited by Enrique Simonsen; then even harder in summer 1982 at the University of Mannheim, where we enjoyed being with our dear friends Marlies and Jürgen Schröder in the hamlet of Juhöhe in the Odenwald. Now working frantically hard, I visited the European University Institute in summer 1983 at the invitation of Marcello de Cecco, in 1984 the University of Munich, invited by Edwin von Böventer, and finally, late that summer, Stockholm’s Institute of International Economic Studies, invited by Assar Lindbeck. Viviana and I celebrated completion of the manuscript with a trip to Patagonia at the end of 1984. The book was published under the title *Political Economy: An Introductory Text* in March 1985.  The book can be said to have been a *success d’estime*. It had the warm admiration of Partha Dasgupta, who got it into the Economics Tripos Part I at Cambridge. Jim Tobin declared it the most original introductory textbook since Irving Fisher’s. It found adoptions in a few premier schools such as Erasmus in Rotterdam, the Stockholm School of Economics, and the Institut d’Etudes Politique de Paris (in a superb translation by the French economist Jacques Le Cacheux). But it was not a commercial success. It was called pretentious, mannered, too difficult and so rich it was hard to teach from. This was about as painful an experience as I had in my career. Over the summer I would lovingly read portions of it, perhaps as part of a mourning process.  During a conversation a year later with Amartya Sen it struck me that in that long hot summer of 1985 in Rome I was not at all sure that I would find a way back into research. So many years had gone by without much time to do research or even to read it. I did reenter, though. What drew me back was a mounting curiosity to understand the deep macroeconomic puzzle posed by the 1980s experience: Continental western Europe was in its second depression of the 20th century – worse than that in the 1930s – yet one accompanied by elevated real interest rates rather than the depressed rates of the 1930s. Mises and Hayek may have had the answer, but if they had a model they had taken it to the grave.  Structuralism in Paris and Florence With the extraordinary events in the Continental economies from the mid-1970s to the early 1980s on my mind, I began to question whether any explanation was provided by models of the New Classical type and those of the New Keynesian type, with their stationary stochastic structure and accompanying rational expectations. Was the massive slump on the Continent one great big disturbance term, to be followed by one big recovery? If so, is that an “explanation” of the “fluctuation”? I soon wondered whether any of the three monetary theories of employment (the New Classical, the New Keynesian, and the old neo-Keynesian) were up to explaining *any* of the big macroeconomic developments I had witnessed in the postwar decades right up to the mid-1980s. The emerging literature on Real Business Cycle theory, sparked by Frank Ramsey and David Cass and developed most of all by Edward Prescott, appeared to be of no help either. Was the rise of the world real interest rate originating in the U.S. a contractionary force in Europe, as suggested by RBC theory?  A change of scene may have served once again as the catalyst for starting a fresh page. A Columbia sabbatical enabled me to visit the Banca d’Italia in Rome for several months the summer of 1985, the think tank OFCE in Paris over December of that year, and the University Institute of Europe (IUE) in Fiesole (Florence) from January to August 1986. I knew just one economist in Rome, Luigi Spaventa, who had arranged for my position at the Bank. (The corporatist theoretician Enzo Tarantelli, whom I was looking forward to talking with, had been assassinated a few months earlier.) For a few weeks I treaded water, producing a modification of Tobin’s dynamic aggregative model in preparation for his Festschrift in autumn 1986. Finally, as my time at the Bank was dwindling, I became intrigued by the question: why in the early 1980s had Italy’s real interest rate risen so high and did that help to explain Italy’s slump? The Italian economists I talked to all thought that the high rates were “made in Italy” and I argued that the rise was imported from America. But I floundered about for a model with which to show that this rise had ill-effects on output and employment. This led in turn to conversations with Jean-Paul Fitoussi, whom I had met in previous years at IUE. He was Director of Research at the OFCE on his way to becoming President. We resolved to make this the next subject of our research.  Jean-Paul and I began modeling mechanisms from high imported real rates to decreased employment in December 1985 at the OFCE and largely completed this work at IUE the next March. This was a heartwarming collaboration and our friendship has only grown closer over the years. We gave our results in a paper at the BPEA meeting at Brookings in April and in a book, *The Slump in Europe*, published in 1988. It was exciting to have such a new set of ideas on offer. The thesis that real rates and real exchange rates impact on employment in unorthodox ways became the signature theme of Fitoussi-Phelps. There were plaudits from Jeffrey Sachs and Kenneth Rogoff and a skeptical assessment by Julio Rotemberg and Michael Woodford. In a debate with Michael in Capri around 1990 I argued that their analysis went amiss in testing for real interest rate while omitting real exchange rates.  Then, in 1988, I became intensely curious to see whether I could derive the same results and maybe more results from *non-monetary* models possessing some of the same mechanisms embodied in the models used in Fitoussi-Phelps. (To my later embarrassment I did not recall when I embarked on building a series of non-monetary models that a non-monetary version of my 1968 model of wage dynamics had been published by Steve Salop in 1979 and a non-monetary general-equilibrium version of the 1970 Phelps-Winter model of a customer market had been published by me and Calvo in 1983!) From 1988 to 1992, drawing on earlier papers with money from the 1960s, I managed to build a non-monetary general-equilibrium version of the *customer* model; I persuaded Hian Teck Hoon to collaborate with me in building a non-monetary version of the turnover-training model, or *employee* model – both published in 1992; and I built two sector models drawn from *The Slump in Europe*. Taken together, these models constituted what I dubbed the *structuralist* theory of unemployment. The distinctive character of the structuralist theory is not exactly to have rebased macro on non-monetary mechanisms; after all, RBC theory had done that. It is to have *endogenized* the (path of) the *natural unemployment rate*, to have *integrated* into the story some *micro elements* of business activity – acquiring customers and employees as well as plant and equipment – not usually at the center of models, and to have shown how *valuations* of each of these business assets are a key *driver* of the unemployment rate. In the resulting system, employment (or its growth) in an open economy is driven by the shadow price placed on the business asset, private wealth (or the income therefrom), the exchange rate, domestic productivity and the overseas real interest rate.  Hian Teck Hoon played a key role in the theoretical work. The phenomenal clarity he was able to bring to the turnover training model in the open economy and closed-economy cases made the two most crucial chapters transparent. Later he was to join me in clarifying the effect of technical progress on employment, a subject that had not been well treated initially, and in studying the effect of taxation on employment.  The work’s theoretical implications differed radically from both the Keynesian view and the RBC view of several causal forces: Contrary to the Keynesian view, *aggregate* demand is gone and the structure of demands is key: consumer demand and investment demand operate quite differently; private wealth in a (open) economy is unhealthy in having adverse effects not only on participation rates but also on employee conduct and loyalty. Contrary to the RBC theory, an increase in the overseas real interest rate, abstracting from any other overseas forces, is harmful for investment and employment, not beneficial. So is a decrease in domestic productivity growth, overseas growth being unchanged. We were later to extend our distinctive analysis to the effects of overseas forces causing a real exchange rate appreciation. (Subsequent papers in this project include ‘Growth, wealth and the natural rate: Is Europe’s jobs crisis a growth crisis?,’ with Hian Teck; ‘The rise and downward trend of the natural rate,’ with Gylfi; ‘Natural rate theory and OECD unemployment,’ with Gylfi; and ‘Lessons in natural-rate dynamics.’)  Gylfi Zoega, embarked on his dissertation at Columbia, had the idea of devising and performing statistical tests of this structuralist theory, taking advantage of the distinctiveness of several of its implications. This was no longer a fashionable activity so it took some getting used to. Nevertheless I got involved and at the OFCE in Paris we worked flat out over June 1992 to prepare the grand statistical regression, using OECD data on 18 nations, for a report of the results – whichever way they went. I remember my growing anxiety as the end of the month drew near. On our last day (a Friday if I am not mistaken) Gylfi pushed the button. I doubt I will ever forget my elation with the results. A few years passed until we produced, using a richer specification, another round of results. (As I write, we are beginning to prepare what will be the third round of statistical results.) It was a fantastic feeling to be in possession of truths that in the whole world only Hian Teck, Gylfi and I had. That was a source of confidence when in ensuing years it was necessary to go up against the conventional wisdom from other quarters. And how lucky I was to be able to share the experience with my great friends Gylfi and Hian Teck.  The three models and the statistical study were assembled over 1992–93 into a book published in 1994 under the title *Structural Slumps*. This was my second rewrite of “macro” and, for me, in many ways more satisfying than the rewrite of the Keynesian model that I had done in the second half of the 1960s. But what about the outside world? How important were the theory’s benefits and to what extent did the profession adopt its perspective?  At the level of historical understanding, this theoretical development served to underpin hypotheses linking the 1980s slump to a worldwide rise of real interest rates, the sharp transition to more moderate productivity growth in the European economies, and the growth of the welfare state to huge proportions, especially on the European continent. At a more general level, this work pointed to the crucial role for employment determination played by the values (also known as shadow prices) that firms place on the various sorts of business assets with which they operate: the employee with the needed firm-specific preparation, the customer, and nonhuman tangibles such as industrial plant and office facilities. This feature of the theory suggested that the prices of shares traded on organized stock exchanges might be serviceable as observable proxies for the mostly unobservable asset values, which opened up new statistical tests of the theory. (See the 2000 paper by Fitoussi, Jestaz, Phelps and Zoega, ‘Roots of the recent recoveries: Labor reforms or private-sector forces?’)  The structuralist models of unemployment movements turned out to be useful in coming to understand the inflationless booms in the late 1990s. In their thinking about the long wave of business expansions in the late 19th century, the German School under Spiethof and Cassel suggested that prospects of new industries or new methods requiring further capital, and this interpretation can be translated into an unexpected jump in the values that firms, looking to the new opportunities, place on one or more business assets. (An op-ed in the *Wall Street Journal* in April 2000 provides an introduction to this analysis. A trilogy of op-eds on the two great investment boom of the 20th century, with corrections and updating, appeared as a journal article under the title, “The Boom and the Slump: A Causal Account of the 1990s/2000s and the 1920s/1930s.”) I would add that I doubt that I would have been led to view the shadow prices that managers or owners place on the various business assets as independent, thus causal, had I not become increasingly accustomed to thinking about capitalist systems, with their visionary entrepreneurs and speculative financiers – which takes me to the last chapter of my research or at any rate the last chapter of this biography. I felt good to have experienced what I saw to be an unusually fruitful period of discovery running from age 55 (in 1988) to about 65. I was also proud that I was able to produce my magnum opus at a relatively late age. (I was 60 when *Structural Slumps* came out; but perhaps “60 is the new 50.”) At a presentation of what was essentially the OFCE report to a NBER meeting in Cambridge, Mass., in the summer of 1993, my discussant Julio Rotemberg was then very positive: “What more can you ask?” he said. The initial reception for the 1994 book was also enthusiastic. William Nordhaus, speaking as co-author of the Samuelson & Nordhaus textbook, told me “we loved your book.” The blurb Pentti Kouri wrote for the book’s jacket praised its radical break and pointed to its new results. The notices in *The Economist* and the *Wall Street Journal* were rave reviews (as one reviewer termed it himself) or close to it.  Yet the reviewers in the academic journals were noncommittal. Their reservation, as I see it, was that the methodology was novel; whatever it was, it was not exactly in the rational expectations mode. In my work, every shock, at least every important one, is *de novo* – totally unanticipated and unprecedented; business life is “one thing after another.” (It was only for simplicity that markets are supposed to put the economy on a (new) equilibrium path after each shock.) So the story might be called one of *punctuated equilibrium*. (A rational expectations approach would describe a stochastic stationary state in which, with known probabilities, random forces would establish which of the known potential “regimes” would be the next regime and how long it would prevail until giving way to one of the other potential regimes.) I knew that my method lacked the beauty of stochastic rational expectations equilibrium. But I liked its pragmatic and concrete qualities. I was to find this especially natural as I worked my way into capitalism theory. Furthermore, the structuralist framework appears to me to have had influence among practitioners – in discussions of the effects of Chinese saving on American asset prices, the effects of a weak exchange rate on the profit share increase, the effects of swollen government indebtedness on employment, and more.  “Inclusion” – 1990 to 2000 In the 1990s I became more and more concerned with the problem of “inclusion,” or “economic inclusion,” as it came to be called, in the United States and also in continental Europe. I got my start with it in 1990 when I had the opportunity to present a paper “Economic Justice to the Working Poor through a Wage Subsidy” at the Jerome Levy Economics Institute up the Hudson River at Bard College. After discussing my concept of economic justice and Rawls’s advocacy of maximizing the lowest pay rate, the paper discussed a *graduated subsidy* to employers aimed at pulling up wage rates at the low end of the distribution. There was not much more in the paper than that – no unemployment, no urban tensions, no social externalities, and the example given was simply sketched, not “costed out.” In my example, $3 an hour labor would then earn $7 an hour.  Later, in 1993, I was invited to be a visiting fellow in 1993–94 at the Russell Sage Foundation in mid-Manhattan, just 25 blocks from where I lived. This was the perfect place in which to carry out this project. Sage provided its visiting fellows with the opportunity to interact with Robert Merton, one of the very greatest social scientists of the century and a Columbia colleague with whom I had not had enough contact. I would send him my first drafts, which would come back with a profusion of comments, grave and minor, all in red ink. I could not ask for a better editor. He saw things about me that I had not seen. When I gave one of the weekly seminars, Eric Wanner was away, so Bob presided. At the end, everyone filed out except Bob who sat there musing until he finally spoke to me. I miss him to this day.  The book that finally came out of this was *Rewarding Work*, written between July 1995 and March 1996 and published in January 1997. It went “against the stream,” in a phrase [Myrdal](https://www.nobelprize.org/nobel_prizes/economics/laureates/1974/index.html) used, in three ways. Conventional thought held that jobs were a nearly unalloyed burden that is compensated only by the wages they bring in. In contrast, I saw jobs, as Marshall and Myrdal did, as the main source of most of their personal and certainly their intellectual development – maybe not in Soviet communism, where the rampant employee absenteeism was revealing, but certainly in highly enterprising economies, which well-functioning capitalist economies are. (“But surely there is not much mental stimulation and intellectual development provided by the jobs filled by *disadvantaged* people, is there?” I answer, “true, but for most people participating in the world of work, even in the bottom rungs, is more interesting and satisfying than the alternatives available if they do not get out of the house.”) I also saw jobs as providing people with the opportunity to do work in which they can take some pride (Veblen’s instinct of workmanship) and as providing whatever self-esteem may come from being self-supporting (Emerson’s self-reliance).  Conventional wisdom also held that, whatever the wages and the nonpecuniary rewards of work, free markets created the right amount of jobs. In contrast, I argued that many or most workers at the low end of the labor market find their pay so meager and perhaps demeaning (and possibly the non-pecuniary rewards too) that, if their economic situation is not dire, they may find it emotionally difficult to stay in the job for long. Or they may become so demoralized or distracted as to become inadequate in their jobs, so they are not kept on very long. In addition, statutory minimum wage legislation may make them unaffordable to law-abiding employers. In my criticism of the free market in low-end labor I also appealed to some old-fashioned values: the presence of many people unable to support themselves at normal standard tends to erode the place our culture gives to self support, engendering instead a culture of dependency. The presence of so many not engaged in the community’s business – the business of America is business, of course – tends to erode the place accorded to contribution and to engender instead an underground economy based on drug trade and violent crime. What will be the preparation of children for a fulfilling life if they grow up under such pathological conditions? Finally I did not shrink from adding that the social pathologies stemming from pay rates too small to provide a living and underemployment are Exhibit A in the populist attack on free enterprise, which is necessary for economic dynamism and helpful for the level of employment, thus prosperity, too. The upshot is that the members of society would be willing to *pay* something to raise low-end employment and pay.  Conventional thought also held that the right way for society, acting through its government, to address the poverty of the working age poor would be to offer money in the form of a universal income guarantee. It was explained that only social benefits of such a no-strings, work-unrelated, lump-sum nature would have the desired property of being *neutral* as between working and not working, joining and not joining. In scientific meetings in Europe and elsewhere I tried to explain how profoundly misguided such a position is. Instead, low-wage employment *subsidies*, or subventions, by imparting a *non-neutral* bias to participation, quitting and hiring decisions, would serve to raise employment and pay rates.  Of course I could not fail to bring in, despite the unpopularity of the very word injustice in some quarters, the self-respect member of society feel from seeing some of their tax money go to boosting the reward that the less fortunate receive from their part in the production of the nation’s output. When I was done I phoned Rawls to tell him about the book. I had to admit straightaway that I emphasized the n*egative social externalities* that derive from sub-standard pay and under-employment. I did not emphasize the Rawlsian *injustice* of leaving pay and employment at the low end so far below what they could be. “You *can’t* in today’s climate,” he responded. This was quite a relief. I had only to bear the objection from the right that the book had too much on “belonging” and the objection from the left that it had too much on crime and violence.  *Rewarding Work* and a shorter 1995 report in French have had some success in Europe. In the year of its publication it was quoted at some length by Clive Crook in *The Economist* and endorsed by Martin Wolf in the *Financial Times*. I had the honor to speak about the ideas in the French Senate. Both France and the Netherlands adopted an assortment of plans to boost employment and pay at the low end, one component of which was been subsidies to employers. Neither country, though, adopted such subsidies with anything like the scale and universality envisioned by my plan. Now, in France, there has been a remarkable revival of attention to “the value of work” and to “inclusion,” and some have credited my work for helping to bring these ideas to politicians and to the public.  In contrast, these ideas have not been implemented in the United States, leaving aside the narrowly confined and not very cost-effective program called EITC, which sends checks most exclusively to women with low earnings in the current year (without regard to wage rates). In a marvelous review appearing one Sunday in January 1998 in the *Washington Post* the columnist Matthew Miller rebutted the objection that my plan, at 100 billions dollars per year, would “cost too much”. The Congress, he said, will certainly step up its annual spending by that much or more in a few years. And the Congress did – notably with a lavish new program of free pills for the elderly. Miller’s subsequent book *The Two Percent Solution* made my plan the centerpiece of a bouquet of legislative programs he proposed to meet a range of social problems in the U.S.  *Rewarding Work* was also the prelude to quite a lot of research. Hian Teck Hoon and I worked like dogs to mathematicize in our turnover/training model the optimum graduation of the employment subsidy. In November 1998 a quartet of highly technical papers were heard at a Russell Sage conference on alternative methods of helping low-wage workers. Several of my closest colleagues in this area came to give papers, including James Heckman, Dale Mortensen, Christopher [Pissarides](https://www.nobelprize.org/nobel_prizes/economics/laureates/2010/), Dennis Snower and Michael Orszag, as well as Hian Teck and myself. For the conference volume, *Designing Inclusion*, I wrote an introduction that is regarded by some as my best statement on the subject.  “Dynamism” in London, Rome and back in New York In the early 1990s, with the Soviet Union in an economic crisis, I was drawn into thinking about the kind of economic system that would best replace the communist system. Going with Kenneth Arrow and Fitoussi on the 1991 mission to Moscow for the nascent European Bank for Reconstruction and Development and writing a report started me thinking.[12](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#12) A year as a consultant to the EBRD in 1992–93 acquainted me with most of the issues. Years later I was led to the more general question: In any country, what criteria must be used in determining the best sort of economic system for it? This was to be a year of stretching myself into a new area. So it was a godsend to have in London the friendship and support of Beatriz and Philippe Aghion, Francesca and Chris Pissarides and Judith and Dennis Snower at this time.  When starting out I had no idea whether I would be well-suited for this research. But as I got farther into it I began to believe that there were elements of my background that did suit me to it. I am thinking particularly of Bergson and James, also the classic figures in the humanities who had made a deep impression on me.  The economic system that Marx dubbed “capitalism” held a fascination for me. The little I knew about Soviet communism mostly came from eastern European émigrés: Jan Tumlir, Bela Balassa, Janusz Ordover and especially Roman Frydman (the last two former students of mine). At Amherst and Yale I had read chapters of Hayek but more his writings on business fluctuations than his work on capitalism theory: what differentiates it from market socialism and communism. With occasional consultations with Frydman, Andrzej Rapaczynski, Jan Winiecki and Leszek Balcerowicz, I studied the key sections of Hayek’s seminal essays from the mid-1930s to the mid-1940s.[13](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#13) This led me back to passages by Knight and Keynes and forward to Michael Polyáni. They seemed so modern and yet out of this time. I had never met any of them. My friend Emile Despres told me about a lunch he had with Keynes during the war. I missed Polyáni’s 1962 Yale lectures on personal knowledge. There was plenty of time to meet Hayek but I never took the considerable initiative it would have required. (Some other regrets in this category are not having talked to Marlene Dietrich when the plane we shared was forced by bad weather to land in Pittsburg and not having gone even once to Café Taci on singing nights in order to talk to Franco Corelli. But small talk was never my forte. And maybe not theirs.)  In my reading two core properties of capitalism struck me. First, capitalism, in allowing individuals to become sole proprietors of companies or at any rate shareowners (provided the stock market has real influence over a company’s management), opened a society to future directions that are *not coordinated* by anyone or any institution. And since there is no coordination, there will generally be a *heterogeneity of views* about how the economy works – about what the future consequences would be of any of a wide range of present actions, especially investment decisions. For this reason, our models of capitalism, if they are to be faithful to this fundamental and distinctive characteristic that the system has, must recognize that the system generates disequilibrium. They must be models of *disorder*, though it is a disorder that may be preferred to any “order” that might be imposed. (It seems to me that Hayek, who would have accepted this point in the 1960s, overlooked it when in the 1930s he referred to the “spontaneous order” in capitalist economies.)  Second, capitalism, in its tolerance of a diversity of views, allows more participants to venture into *novel* directions than would otherwise be possible: to pursue an original career, to adopt an untried portfolio strategy, to start a company to develop and market a new product, to pioneer the use of a product or method. This property makes well-functioning capitalist systems a wellspring, if not *the* wellspring, of *Hayekian innovation*: not the routine innovation of Schumpeterian entrepreneurs, who merely undertake “obvious” applications of known scientific advances, but *innovations stemming from the conceptual originality of people in their business life*. The prospects for the Hayekian innovations are clouded by their considerable *ambiguity*: no one can know beforehand with what probability they will be commercially successful. In a capitalist economy, therefore, present participants – at any rate those among them who hit upon successful innovations – *influence or even shape the future: future know-how and goods in existence*.  For me, this was heady stuff. I did not lose sight of capitalism’s faults. I appreciated that many economists in the history of the subject have expressed dislike of its headless directions and its doldrums. There was also the problem of inclusion, though that was not peculiar to capitalist systems: it was evident in Europe’s corporatist economies too. Little by little, I was drawn away from macroeconomics, where my research had been stuck for nearly two decades, to the economics – or political economy – of alternative economic systems: capitalism, market socialism and corporatism.  In my year at the EBRD, my job was to edit the first Annual Economic Outlook. At that time I had no original line, no new idea, of my own to impart. We – in my circle – thought that the “market economy” meant capitalism, no doubt buttressed by regulations and cushioned by welfare entitlements. In the chapter I drafted to introduce the volume I sought to clarify the theory of capitalism existing in the literature and to highlight ideas of Aghion, Kornai, Frydman and Rapaczynski that had surfaced at the EBRD.[14](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#114) Here is a passage of the chapter arguing the *long-termism* of classic capitalism: The entrenchment of a manager is the most serious difficulty …  The entrenched manager will be more willing to risk covert practices for his own benefit than would a manager that owners could easily dislodged … [I]t is best that the business strategy of an enterprise be decided by its owners: the chance of huge gains motivates them to think big, the fact that they are not wedded to the enterprise (they can invest elsewhere) motivates them to think fresh, and the opportunity as owners to derive immediate benefit from expectations of far-future payoffs motivates them to think far ahead. In contrast, entrenched managers favor strategies that ensure the firm’s survival so that they can hang on to what they have.[15](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#15)  When John Flemming, who was head of the economic section, sent the page proofs of the Outlook for approval, something unexpected happened. The Vice President demanded my introductory chapter be removed, saying that the EBRD must be *neutral* about the sort of market economy eastern Europe would best “transition” to. John was able to save the piece only by making it the Annex! The privatizations in Russia and the Czech Republic operated to entrench managers and employees in their positions. It appeared that many economists would accept “transition” to the *market socialism* found in eastern Europe and many more were quite content to see Russia and the others adopt the *corporatist system* of continental western Europe. Much of my research since then has aimed to show these judgments to be misguided. It was one of the thrills of my career to have taken up the study of capitalism at such an exciting time. But I hoped eventually to make some theoretical contribution to the understanding of capitalist systems. How is a capitalist economy to be described – to be modeled? What, that is, do such economies do and how do they do it?  My research project in this direction, still in its infancy but nevertheless in progress, dates from May 1977. Luigi Paganetto, whose Faculty of Economics and Law at University Tor Vergata in Rome I had been visiting since 1989, asked me whether I would be willing to be play the role of adviser in a research project at the Consiglio Nazionale delle Ricerche to study the weaknesses of the Italian economy as it faces the European Union and the global economy. I began work in May 1997 and presented at CNR my last semi-annual report in May 2000. A compilation of the six reports was published in 2002 under the title *Enterprise and Inclusion in Italy*. It did not give birth to a model. But it was a start. Not that I did it alone. I owed much of my knowledge of the Italian economy to Luigi, to Stefano Micossi and Luigi Spaventa. A group of young economists working in Rome was assembled, some friends for a decade or more. Giovanni Tria and Ernesto Felli, whom I had known since the mid-1980s, were mainstays. Luigi Bonatti, Francesco Nucci and Alberto Petrucci were also important contributors.  In this period I hit upon an idea I liked – so much that I reported it four times: *Financial Times* in 2000, *Economic Policy* in 2001, *Enterprise and Inclusion* in 2002 and Aghion *et al*. in 2003. It was not about the endogenous offer and selection of new ideas, which I later denoted by the term *dynamism*. It was about *vibrancy*. Spiethoff and Cassel, the leaders of the turn-of-the century German School, saw innovation as a frictionless and unfailing response to advances in science and navigation. Schumpeter modified that, arguing that there is always a scarcity of entrepreneurial types able and willing to dedicate themselves to the challenge of developing a new product or method. But that is not all, I suggested. Before going ahead an entrepreneur has to foresee an adequate presence of Nelson-Phelps managers and, similarly, Bhidé consumers with the education and venturesomeness required to weigh, judge and operate the new product or method. Entrepreneurs may have to get through a thicket of government licensing and regulatory requirements. A broad stock market with high share prices where venture capitalists can cash in their investments in fledgling firms may be important. It turns out that of the 12 largest economies in the OECD are ranked according to an index of these attributes circa 1990, the ranking predicts very well how those economies ranked with regard to their responsiveness to the internet revolution that broke out in 1995. (Aghion and Peter Howitt also built on Schumpeter, of course.)  But I hoped to move away from this narrow, nearly classical perspective. At the Festschrift conference for me in 2001, Samuelson and I chatted before he gave his keynote speech. “You know, Schumpeter was not an Austrian,” he said. “Yes,” I answered, “I know.” I knew that very well. In Schumpeter’s system, there is no “Knightian uncertainty” faced by the start-up entrepreneur launching an innovation, which was central to Hayek and also Knight (for whom just about everything was uncertain), no “personal knowledge” of the entrepreneur that a financier has to face, which was familiar to Hayek and Polyáni, and no “moral hazard” for the financier that the entrepreneur will find chances for self-dealing, which would have been understood by Mises.[16](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#16) Most fundamentally, in Hayek’s system, the entrepreneurs are people driven to implement unknown ideas of unknown promise; persons may have new commercial ideas that spring from the private knowledge and insights they acquire in the course of their business or job. As I saw it, advances in capitalism theory must model the economy as basically evaluating, selecting, developing, and spreading new commercial ideas. Every distinct good or method is the expression of a new idea.  The concept of a research center for developing capitalism theory began to emerge out of numerous conversations that Roman Frydman and I had in the late 1990s. Over the years I had greatly enjoyed our discussions as well as our warm friendship and this was one of our best ideas. We received strong encouragement from William McDonough in New York and David Lord Howell in London. At Columbia then president George Rupp and the provost Jonathan Cole gave me the green light. It would be inter-departmental, even inter-school, and allow members outside Columbia. Thus was born the Center on Capitalism and Society.  The project would not have been possible without a remarkable confluence of talent at the right place and the right time. Bruce Greenwald, Glenn Hubbard, Richard Nelson and Andrzej Rapaczynski were insiders from the beginning alongside Roman and myself. But we would have been too small a group. Our great luck was that Joseph Stiglitz had just arrived at Columbia and agreed to join the Center, Amar Bhidé, a world authority on innovation, had just come to the Business School, and Jeffrey Sachs came to Columbia a year later. Then I persuaded Janusz Ordover of NYU and [Robert Shiller](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2013/) of Yale to join the roster. This body has remained at 11 but the Center has grown. Jean-Paul Fitoussi has become a Foreign Member. Several more have become advisors or regular attendees: Pentti Kouri, Robert Mundell, Richard Robb and Jürgen Schröder. Philippe Aghion and David Jestaz have been near-regulars.  Our inaugural conference, held at Columbia in 2004, was supported entirely by Jeff Sachs’ Earth Institute, which we became a part of in 2002. When not a single foundation would provide support during the years of the depressed stock market, Jeff came through for us. The 2nd annual conference was held in Reykjavik in cooperation with Tryggvi Herbertsson and Gylfi Zoega of Iceland University. The 3rd annual conference was held in Venice in partnership with Hans-Werner Sinn of Munich’s CESifo.[17](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#17) The Center’s electronic journal *Capitalism and Society* was developed by Amar and launched in summer 2006.  In my efforts in recent years to contribute to capitalism theory these scholars have been a huge intellectual stimulus and also a great forum in which to try out my own ideas. Amar and Richard have been invaluable.  I would like to point to three papers of mine that strive to deepen the understanding of capitalism’s selection for development and for launch of new commercial ideas. These papers are all about *dynamism*. I use the term to refer to the *volume* of innovation of which the economy is capable (under given market conditions) weighted by the *desirability* of their direction and reach – the gifts of financial markets in deciding the innovations to back and of the product markets in deciding the innovations to adopt.[18](https://www.nobelprize.org/prizes/economic-sciences/2006/phelps/biographical/#18)  One of these papers sketches a model in which, every day, each person with a new idea for a startup firm gathers in some known place to make a presentation to every one of the potential financiers for such entrepreneurial projects. I visualized a line of entrepreneurs going around a circle of financiers. It was delightful when Richard Robb reported he had learned that such an institution exists in the United States. Richard verified that the entrepreneurs do in fact circle around the financiers. My interests, however, were economic. What characterizes the set of projects that “make the cut” – that receive capital from some financier? Does the “optimism” of *entrepreneurs* raise market value of startups or just reduce the external finance required? Of *financiers*? What interested me was that by being precise about the interaction between the two sides of the market one could begin to tease out various implications. Modeling pays. (This paper is to appear in a conference volume of the Kauffman Foundation and the Max Planck Institute.)  A paper I prepared with Gylfi Zoega for the April 2004 issue of the CESifo periodical Forum weighed evidence on the importance of certain *economic institutions* for the degree of dynamism in a country’s economy. We looked first at the policy parameters of neoclassical theory. As in our 1998 paper, we found that inter-country differences in the tax rate on labor had little power to explain the differences in various economic indicators (productivity, unemployment rate, etc.). Inter-country differences in the replacement ratio provided by unemployment benefits were also statistically insignificant. In fact, the explanatory power of a great many policy variables – government purchase of goods and services, public capital stock, corporate profits tax rate and so forth – has been slight, even if sometimes statistically significant.  If our conception of the advanced economies is one centered not around consumption and leisure but instead around the attractions and rewards of business life – problem-solving, the discovery and development of talents, and the achievements that may result – then it is not surprising that these policy parameters, though important in the neoclassical perspective generally adopted by supply-side analysts, do not make much of a dent on unemployment and participation – as long as they stay in the historical range at any rate. It becomes hard to see why the neoclassical preoccupations with work-leisure substitution should be center-stage. Reducing the calibrations of the welfare state or cutting government purchases or adding to capital stocks will not make jobs far more engaging and rewarding, hence make participation in the labor force far more attractive and unemployment far smaller. Only modest results can be reasonably hoped for.  What institutions appear to matter for inter-country differences in performance? Presumably there are economic institutions the presence and high development of which affect performance indicators through their impact on dynamism. It is reasonable to hypothesize that organized stock exchanges, company law, suitable bankruptcy provisions, corporate governance spurring corporate performance, and schools preparing the population for business life all foster dynamism … and that economic institutions whose presence and force obstruct or impede dynamism – corporatist institutions that invest company employees, labor unions, communities and other interest groups with veto power to block or limit entrepreneurial ventures and shifts in corporate operations may choke off valuable innovations – all dampen entrepreneurial spirits and thus decrease dynamism.  Fortunately, our investigations found statistical support for as many of those hypotheses as we could test. Yet, in view of other studies, it appears that results in this area are fragile. Something is missing, it seems.  The third paper is more radical. A part of an economy is its economic institutions. Another part consists of the elements of the country’s economic *culture*. Some cultural attributes in a country may have direct effects on performance on top of their *indirect* effects via the institutions they foster. Values and attitudes are analogous to institutions – some are impeding, others enabling. Clearly, any explanation of the poor performance on the Continent that omits that part of the system must be of unknown reliability.  Of course, people may at bottom all want the same things. Yet not all people may have the instinct to demand and seek the things that best serve their ultimate goals. In the University of Michigan “values surveys” there is evidence of systematic differences in *workplace* values between the Continent’s Big 3 (Germany, France and Italy) and the U.S. and Canada. Take the values that might impact on dynamism. Asked what they look for when seeking a job, relatively few persons in the Big 3 reported that they want jobs offering opportunities for achievement; relatively few that they want chances for initiative in the job; and few that they look for interesting work (!). Relatively few are keen on taking responsibility, or exercising freedom in decision making, and relatively few are happy about taking orders. I like to joke that a country in which no one wants either to give or receive orders will find it difficult to start a firm capable of doing the novel and creative work required in developing an innovation.  The paper goes on to show that inter-country differences in the endowment of these values are very commonly statistically significant in explaining differences in standard performance indicators – more significant than many of the institutional variables usually accepted as quite important for performance.  Now I realize that this perspective, which looks very wide, is not deep enough. Where do differences in the set of economic institutions – differences in the economic system – come from? The same question may be asked about differences in workplace values. I have a suspicion about where they come from. In earlier work I organized my thinking around some of the intellectual currents of reaction on the Continent to the Enlightenment and to capitalism in the 19th century: the *solidarism* of protecting the “social partners,” the *consensualism* of blocking business initiatives lacking consent of the “stakeholders,” the *anti-commercialism* that revived in the 19th century, the *conformism* that militates against sticking out from the group, and the scientism that encourages looking to the state for social advance. I call this ideology *corporatist* though we could just call it *Continental*. Now I am inclined to hypothesize that the rise of the corporatist ideology allowed the erection of institutions harmful to dynamism: if some of the ill-effects were foreseen, their cost was not seen as high enough to deter building those institutions. I hypothesize further that the corporatist ideology was the root cause of the decline of economic values helpful to dynamism: Continentals, finding over the course of the 20th century that there was little call and little room for the exercise of freedom and responsibility in the workplace, learned not to prize those values and thus to stop looking for the satisfaction of those values in the workplace.  I would just add that in June 2006 I had the great pleasure of speaking at the Institut d’Etudes Politiques in Paris on how capitalism might be morally justified. Let me refer readers to the last section of my [Prize Lecture](https://www.nobelprize.org/nobel_prizes/economics/laureates/2006/phelps-lecture.html) where I discuss my ideas on that interesting and, I think, important subject.  In closing this section I will just recount how receptivity to the subject of capitalism has evolved in France. In 2000 a lecture of mine at Sciences Po was announced on some merits of capitalism. The attendance at the lecture was 5 persons, counting the moderator, Jean-Paul, and my wife Viviana. In 2005 I spoke at the Sorbonne on much the same topic to an audience of nearly 500. (The same number or more appeared at the June 2006 event.) I expect to be staying with this subject for quite awhile.  An immensely gratifying period The 2000s have brought wondrous times. Long ago, in the early 1980s, when I was not yet 50, I was elected to the National Academy of Sciences (USA), to the American Academy of Arts and Sciences, and named to the relatively ancient McVickar chair at Columbia. That in the space of some two years. Then nothing of the sort happened again for nearly two decades. That was to change.  In January 2000 I was named a Distinguished Fellow of the American Economic Association. My fellow Fellow was Jack Hirschleifer, whose work I admired. This was a most relaxed event. The Chicago labor economist Sherwin Rosen read with glee the citation where it said “then the government tested the [natural rate] idea.”  In October 5–6, 2001, the Festschrift conference in my honor was held up at Columbia. It was an unforgettable event. As many said, it seemed that all the important macroeconomists in the world had come – nearly 140 in all – and this is in spite of the shocking and tragic events of 9/11 just weeks before. Roman Frydman was masterful as the main organizer while Philippe Aghion, Joe Stiglitz and Mike Woodford contributed in a range of important ways. Tobin, Lucas, Heckman and Solow each agreed to be a commentator at one of the four sections and Samuelson gave the keynote. Klein, Mirrlees and Mundell also came. (Sadly, Tobin as well as Merton and Rawls could not attend for reasons of health.) Many moments at this event took my breath away, almost literally. One of these came following my speech at the end of the first day. As I walked back to my seat to rejoin Viviana there was a deafening drone of applause that seemed endless. I felt a bond between me and each one of them, which was private but powerful.  The award of a Nobel Memorial Prize is a huge honor. To be the sole winner, as I was in 2006, is the greatest honor. My citation was, for me, a validation of the first half of my life’s work. What makes the Nobel so exceptional? For one thing, the Nobel award is so extraordinarily public – and on a global scale. With the press conferences and media coverage, a great many people come to know your name and think of you for a range of invitations. Thus the award may be life-changing, a watershed event. Nobel Week, with its ceremonies and festivities, represents a kind of elevation, which signals greater respect and correspondingly higher expectations. I have felt pressed to ramp up my performance in an effort to meet the elevated expectations.  It is a privilege to have all this stimulus and challenge. Toward the end of Nobel Week, at a gathering in the Nobel Foundation, Professor Gunnar Öquist, permanent secretary of the Royal Swedish Academy of Sciences and a man of dignity and seriousness, came over to say goodbye for the last time. As we shook hands he said: “Use it well.” I am trying to do that.   |  | | --- | | Endnotes | | Edmund S. Phelps is McVickar Professor of Economics, Columbia University, and Director, Center on Capitalism and Society, Earth Institute, Columbia University. | | 1. Three earlier memoirs are “A life in economics,” in Arnold Heertje, ed., *The Makers of Modern Economics*. Aldershot: Elgar, 1995, “The origins and further development of the natural rate of unemployment,” in Rod Cross (ed), *The Natural Rate of Unemployment: Reflections on 25 Years of the Hypothesis*, Cambridge: Cambridge University Press, 1995, and “Recollections of my past research in economics,” Acceptance Speech, Beijing Technology and Business University, June 2005. These and other data, including my CV, are at www.columbia.edu/~esp2 | | 2. *New York Daily News*, December 8, 2006. | | 3. A recent essay on the phases of the Boston Symphony remarked that Charles Munch in choosing Roger Voisin as principal trumpet “sealed the fate” of the orchestra. | | 4. That does not mean that economic descriptions are unimportant. Istanbul’s geography and access to water may be important to the city, even to its cultural character. Also, there may be a set of cities having a similar culture yet every one is distinctive in the character of its economy and the resulting rewards from work. | | 5. T. C. Schelling, “For the Abandonment of Symmetry in the Theory of Cooperative Games,” RAND , Santa Monica, 1958. Oddly, the bibliography of Schelling’s RAND papers that I found on the internet portrays the paper as showing that symmetry does not follow from “rational expectations” while the lesson I drew was that so-called rational expectations cannot be successfully defended by the argument that (in the model under study at any rate) it would be deducible if symmetry were present and were common knowledge. | | 6. T. C. Schelling, “The Reciprocal Fear of Surprise Attack,” RAND, Santa Monica, 1958. | | 7 W . J. Fellner, “Distortion of Subjective Probabilities as a Reaction to Uncertainty,” *Quarterly Journal of Economics*, 75, November 1961, 670-89. | | 8. W. J. Fellner, “Demand Inflation, Cost Inflation, and Collective Bargaining,” in P.D.Bradley (ed), T*he Public Stake in Union Power*. Charlottesville: University of Virginia Press, 1959. | | 9. We cannot test and confirm everything in our model; there have to be some untested premises in any test of the other hypotheses. Hence, when a model fails it is indeterminate whether the problem lies in the hypotheses being tested or in the accompanying premises on which the model is grounded. So we are reluctant to scrap our untested premises. (The point is made in my Ph.D. thesis “A Test for the Presence of Cost-Push Inflation,” *Yale Economic Essays*, 1961. It is a theme in James Heckman and Richard Robb, “Alternative Methods for Solving the Problem of Selection Bias in Evaluating the Impact of Treatments on Outcomes,” H. Wainer (ed), *Drawing Inferences from Self-Selected Samples*, 1986. That paper’s reception by John Tukey is told with dry humor by Heckman in his Prize Lecture.) Thus, rational choice, in the sense of Samuelson’s revealed preference axioms or, perhaps less aptly, Savage’s axioms, is a premise that most of us economists are loath to give up, even though we recognize that there may be contexts in which we would do well to depart from it. *But*, we do not treat it as an *absolute*, never to be deviated from. | | 10. Another objection of some radical Keynesians was that the proposed microfoundations apparently assumed that supra-natural unemployment and sub-natural unemployment too would not occur but for the *deficient response of the general price and money-wage level* to contractionary or expansionary shifts in effective demand; and in the belief of these Keynesians it does no good for the price level to fall following a contraction of demand (and no good for it to rise following an expansion of demand). They explained that when the price level fell, the real burden of indebtedness at businesses would be increased, adding to insolvencies and failures, and thus weakening the amount of output demanded, particularly output for investment purposes. To a close approximation, they said, effective demand was *perfectly inelastic* with respect to the price level. In their view, this made the natural rate idea beside the point. But there was never any econometric evidence adduced in favor of that extreme position. Furthermore, effective demand could not remain deficient for long without the central bank’s taking the falling inflation rate as a signal to cut expected short-term real interest rates and thus to move *by policy means* the economy back toward the natural rate of unemployment. (Exchanges in 2006 with N. Gregory Mankiw and with Michael Woodford led to the result that, if the central bank uses a Taylor rule but misgauges the natural real rate of interest at too high a level, the inflation rate tends to sink to the level that is below the central bank’s target by just enough to drive the target real interest rate below the natural real rate by just enough to bring the unemployment rate back to the natural unemployment rate.) | | 11. Edmund S. Phelps, *Political Economy: An Introductory Text*. New York: Norton, 1985. | | 12. Kenneth J. Arrow and Edmund S. Phelps, “Proposed Reforms of the Economic System of Information and Decision in the USSR: Commentary and Advice,” *Rivista di Politica Economica*, v.81, November 1991. | | 13. Hayek, C*ollectivist Economic Planning*. London: Routledge, 1935, *The Road to Serfdom*. London: Routledge, 1944 and *Individualism and Economic Order*. Chicago: University of Chicago Press, 1948. | | 14. Philippe Aghion argued the necessity of competition among firms; Janos Kornai the necessity for the state to break the habit of bailing out failing firms; Frydman and Rapaczynski the necessity of designing “voucher privatization” to create enterprise owners who would take control from powerful managers and employees. | | 15. ‘Arguments for private ownership,’ Annual Economic Outlook, EBRD Economic Review, September 1993. Quotation, p. 120. | | 16. William Baumol tells of Mises saying, “Vat is dee Austrian School bat me und Hayek?” | | 17. The website of the CCS is at www.earth.columbia.edu/ccs | | 18. Because a model capturing capitalism must be something different from the Spiethoff- Schumpeter model – something with some capabilities added (and some subtracted) – any such model requires added *concepts*, thus added *terms*. | |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0838=EP  [Unidentified] – Hello.  [Adam Smith] – Hello, may I speak to Professor Phelps please?  [Unidentified] – Who’s calling?  [AS] – I’m calling from the official website of the Nobel Foundation in Stockholm, and my name is Adam Smith.  [Unidentified] – Yes, ha, ha, ha.  [AS] – Yes, I know, it could be a hoax call with a name like that, I realize. It’s a terrible burden I carry.  [Unidentified] – Yes, would you hold on a moment please because he’s on with CBS at the moment.  [AS] – Certainly.  [Edmund Phelps] – Good morning.  [AS] – Professor Phelps?  [EP] – Yeah.  [AS] – Well, first of all, congratulations on being awarded the Prize in Economic Sciences.  [EP] – Thank you, thank you very much.  [AS] – What were you actually doing when …  [EP] – Honoured to have this prize.  [AS] – What were you doing when you received the call?  [EP] – Sleeping.  [AS] – And have you been able to do anything since hearing the news or has it been constant telephone calls.  [EP] – I have done nothing but take phone calls and have some nice interviews.  [AS] – I’m sure. You’ve been awarded the prize for your contributions to several different fields, and the common theme seems to be an increase in our understanding of goal conflicts and intertemporal tradeoffs. Am I right in summarizing one key finding as being the relationship between employment and inflation rates is less straightforward than was previously assumed?  [EP] – That’s right.  [AS] – And one real world implication of this is that long term unemployment rates can’t be adjusted by simply manipulating inflation rates?  [EP] – Exactly right, exactly right.  [AS] – Right. Are there any particularly striking examples where this sort of revision of thinking has been used for policymaking?  [EP] – Well, in the 1960’s, and stretching back to the 1930’s, it was felt by many economists that easy money is a reliable way to increase employment. And first the doctrine was that there would be no cost in terms of inflation, and then the revised argument was the Phillips curve, that a little bit of increased employment would have only a little bit of cost, in terms of a little bit higher rate of inflation. And now central banks don’t think that way at all, a lot of central banks never did think that way, but central banks are no longer under pressure to keep lowering interest rates in order to boost employment. It’s now widely understood in economies all over the world that the market will send …, that if employment is pushed too far then the rate of inflation will take off, which would be unacceptable. It was gradually learned that acceptance of a somewhat higher inflation rate would not really bring somewhat higher employment. That you’d have to keep on raising the inflation rate to higher and higher levels in order to try to keep employment elevated to the level that you thought you …  [AS] – … you were aiming for.  [EP] – … you could do it.  [AS] – And I would have thought one of the general problems with working on such intertemporal tradeoffs would be the length of time that it takes these scenarios to play out. The experiments one wishes to conduct must last for many years, or indeed decades?  [EP] – That’s absolutely true. When I first [real]ized that no good would come from increasing the inflation rate to a higher level there wasn’t any way of really backing that up with statistical analyses. That was only in 1966 really when I was first writing about that and we didn’t have a hundred years of data, or anything like that, with which to test statistically such ideas. So in a sense there was no way of convincing most economists on the point until there was virtually an experiment; in the 1970’s the Federal Reserve Bank in the United States embarked on policies that led to appreciably higher rates of inflation, with the thought that that was a way of escaping from the fall of employment that had occurred early in the 1970’s, and it seemed not to work. And that was probably the decisive moment for this theory. So people went from being skeptical about it to being converts.  [AS] – Yes, I suppose as theorists you’re always at the mercy of what governments and central banks choose to do, in waiting for your experiments to come to fruition.  [EP] – Right. Yeah it’s very, very frustrating to get governments to act on something. I mean I’ve been trying for ten years to get governments to adopt low wage employment subsidies to pull up the demand for low wage labour, and there are all sorts of reservations that governments have about that and I guess the only thing that will persuade them is when some government is willing to do the experiment on a large scale so there can be few doubts about interpreting the subsequent data.  [AS] – Another area where your work has been influential is the study of capital accumulation, and balancing short- and long-term objectives there. Again, I suppose the question would be are there real world examples that spring to mind where you think your findings have been put into practice?  [EP] – The subject of national saving is one I’ve come back to over and over again. It may be relevant to your question to say that right now in the United States and in several European countries, we’re facing a kind of demographic overhang of huge pension obligations that the government has committed itself to pay out between the years 2020 and 2050, and one would think that a future minded policy would be in order, and that would involve perhaps raising some tax rates in the present and perhaps cutting back expenditures in the present in order to have budgetary surpluses so as to accumulate a kind of nest egg for paying out those …, for meeting those government obligations in the future. But then the supply side economists have come along and said that’s not the way to go, it’s better to grow your way out of the problem, and it’s possible to do that by cutting tax rates. So, that’s an example of a controversy very much involving intertemporal tradeoffs with regard to saving and investment and consumption and so forth, and labour.  [AS] – Thank you, thank you. Of course last week there was the award of the Nobel Prizes for physical and life sciences, and in those fields there’s a constant interplay between basic and applied research. Do you think the same categories apply to research in economic sciences? And also, the sort of sequela of that, is where do you think your research fits in?  [EP] – Interesting questions. Well, I’m not very happy with those terms basic and applied. I do think from time to time that conceptual questions arise; what do we mean by equilibrium, what do we mean by this concept and that concept? And so to clarify matters we often need to work with a very [word inaudible] model, that has no particular connection with any particular country. So all kinds of economic research activities do from time to time raise questions about the concepts and there is sometimes a sense that the concepts are not clear, that the questions being asked are not clear, and so you have to dig deeper. So that kind of basic research has to always be a part of our portfolio of research activities. But at the same time applied research is necessary too because if we don’t do any applied research we may miss learning that some of our models have implications that appear to be contrary to the data, appear to be counter-factual, not empirically borne out. A healthy economics has got to have both conceptual, theoretical research and applied, empirical research. Well, I think of myself as a theorist, so I think of myself at the conceptual end, but I would like to say, modestly if possible, that I have paid pretty close attention to questions of whether the data are consistent with what my models say.  [AS] – Well I think that was made very clear in the announcement just a little while ago …  [EP] – Oh good, good.  [AS] – … the fact that your work has had profound real world implications. One question that we often like to ask people is whether there has been any one event or person in particular that led you to work on the problems you chose to study?  [EP] – I was always interested in macroeconomics, probably for a complex of reasons, and so a lot of my contacts with my mentors were about macroeconomics and I’d just like to mention that in graduate school two professors had a profound influence on me. One was William Fellner, and the other was [James Tobin](https://www.nobelprize.org/nobel_prizes/economics/laureates/1981/index.html), though I also got a lot from [Thomas Schelling](https://www.nobelprize.org/nobel_prizes/economics/laureates/2005/index.html) and [Tjalling Koopmans](https://www.nobelprize.org/nobel_prizes/economics/laureates/1975/index.html). I soaked up as much of their knowledge as I possibly could, but it’s probably correct to say I’ve gone in my own directions and my work is generally different, generally has a different thrust and character from theirs.  [AS] – Others are soaking up your influence now.  [EP] – Yes.  [AS] – Well, when you come to Stockholm in December we have a chance always to speak at greater length to the Laureates, so perhaps those influences on your early career we can explore in greater detail. But I suppose my last question would simply be to ask how you might plan to celebrate the award later today when you finally get off the phone with the journalists?  [EP] – Uh, celebrate later today. You mean like dinner?  [AS] – I don’t know, yes, any …  [EP] – The thing I’m looking forward to most at the moment is breakfast.  [AS] – I bet, yes … it’s got to be a two egg day, at least!  [EP] – I’ve been scared by cholesterol a long time ago, it’s seldom that I see an egg. Although I understand that the white of the egg is quite safe.  [AS] – That’s what they say, and this is a red letter day after all.  [EP] – Right, I’ll ask my wife whether, perchance, she has an egg in the house.  [AS] – OK.  [EP] – There will probably be an event at Columbia, and I don’t know when.  [AS] – I imagine they’re planning press conferences as we speak.  [EP] – Yeah, right.  [AS] – Well, it’s been very kind of you to spend time talking to us, thank you very much indeed, I’ve enjoyed it a lot.  [EP] – Well, thanks for calling. Bye.  [AS] – Have a good day, bye. |
| Interview |  |
| Q18 | If you had to describe it in your own words – there are several things that you’ve been awarded for – if you were to describe it in your own words, what do you think is the most meaningful thing that you’ve brought to the study of macroeconomics and to policy making? |
|  | I’ve worked in two or three different areas, but in the area of employment and unemployment and fluctuations, that kind of thing, what I think I did was I brought expectations into the analysis of price setting and wage setting which led to new light on cost inflation, the difficulties of disinflation. I also introduced into wage setting something called “incentive wages” – that’s what I called it – incentive wages. That proved to be quite important because the idea was that each firm is trying to pay a better wage than the other firms in order to reduce the quit rate among its employees, but as they all try to get an advantage over one another they just end up the industry standard pay scales so that labour becomes more expensive for all of them and as a result employment falls and unemployment is created. |
| Q18 | Where typically do you see these phenomena? Is it just industrialised countries or would you say that there is relevance for it all over the world? |
|  | It’s pretty relevant all over the world. Even in less developed countries you see zillions of people queued up at the post office for some jobs there or you see people waiting at the factory gates. They can’t knock on the door and bid down wage rates in order to get themselves one of the jobs. The firm is not interested in paying less wages; it likes wages high. |
| Q40 | In terms of arriving at your theories and doing your research, how much of impact have other Nobel Prize winners – specifically within your department at Columbia University – how much of how you think has been formed by what they think? How much of discussion is there? |
|  | Edmund Phelps: Actually we don’t see each other very much.  You don’t?  Edmund Phelps: No, we’re all at airports and in airplanes and teaching classes and running around, so there isn’t very much opportunity for interchange. Also in this age of the internet you’re emailing everybody all the time, including people down the hallway.  And here I was thinking that you were having coffee in the coffee room with Joseph Stiglitz …  Edmund Phelps: No, those days are gone.  But they used to be?  Edmund Phelps: Yes, it used to be the case. |
| Q39 | Perhaps we can touch on that, then, maybe that’s where we can lead into this thing that I’ve been wondering about as how educational institutions and how students as well have changed over the years since you were a young student. |
|  | When I was a student we entered graduate school knowing not only very little economics but we also had a dreadfully poor background in mathematics. Most of us had never studied history either – knew nothing of psychology. We were just practically clean slates as we entered. But I guess what we had going for us is some pretty deep curiosity about the subject and maybe there was some part of it that we wanted to pursue in depth. Maybe we were more seekers and discoverers than today’s graduate students are.  Because they’re so prepared?  Edmund Phelps: Yes, but they do arrive with a dozen math courses under their belt. They come extremely well equipped. I’m just not sure that they come with the same inspiration or the same drive that we had then. |
| Q3 | I’m sure your students would actually disagree… which brings me on to another subject that’s actually quite interesting to me and I think probably relevant to lots of students and even people in business, up and coming through business – is the role of mentorship. Who have been your mentors and what have they meant to you? |
|  | I think my earliest mentor was in high school. I used to play the trumpet and I was interested in music and the head of the music department at school took an interest in me and kind of motivated me to study music in various ways. He even created a course in musicology one semester for a group of us. He was quite an important mentor. Then in college I happened to have two brilliant teachers of economics. I was extremely fortunate to have them and they guided me I think to the right graduate school for me. Then there’s graduate school. I went to Yale and there was a wealth of very important, very interesting economists including William Fellner and [Tjalling Koopmans](https://www.nobelprize.org/prizes/economic-sciences/1975/koopmans/facts/) and Henry Wallich and Robert Triffen and Jacob Marshak. All of them were Europeans. And then there were also [James Tobin](https://www.nobelprize.org/prizes/economic-sciences/1981/tobin/facts/) and [Tom Schelling](https://www.nobelprize.org/prizes/economic-sciences/2005/schelling/facts/), and no doubt others that I may be forgetting. Both Tom Schelling and James Tobin got Nobel Prizes. Which ones were mentors? Well, James Tobin was a mentor, so was Tom Schelling, but also I think Willy Fellner, a Hungarian economist was probably the deepest influence on me in graduate school.  Can you tell me a little bit more about that influence, when you say it was the deepest influence?  Edmund Phelps: He was always quite interested in the murkier side of economics, of expectations, uncertainty. He had some instinctive perception that there are entrepreneurs out there in the market economy with any kind of luck and I think he conveyed a broader and deeper kind of economics than I was getting from most of my other teachers.  These are still the things that you talk about today of course.  Edmund Phelps: Yes.  I’m just thinking it was a very interesting fact, this interest in music that you had at high school. Why the jump from music into economics?  Edmund Phelps: It’s a staggeringly difficult question.  We have lots of time.  Edmund Phelps: I guess it’s true, it seems to me to be true, that if you’re a musician, a performing musician, you try to somehow recreate that music in an interesting way. You didn’t compose it, but you are bringing your own creativity to it. With regard to economics I think economics is a very mysterious, extremely challenging subject and I think it helps a great deal to be a creative person when you tackle economic problems, economic issues, because there’s no end of things that ought to be considered and thought about. The problem is just to think of them and organise them.  This is what you talk about as well, about bringing expectations into economics and it’s finding all the variables.  Edmund Phelps: That’s right, it’s throwing in things that are of central importance that somehow got neglected, overlooked. |
| Q27 | What are the things that you think today are being overlooked in terms of variables that are being looked at for economic policy? |
|  | I’d have to say creativity in business; the creativity of the entrepreneur. It just doesn’t exist in any formal economics course. It may be that in some business schools there are courses in entrepreneurship but what I mean is that in the way we teach let’s say macroeconomics – the economics of employment and inflation and growth and all that – as it’s taught in standard economics courses the word entrepreneur is never mentioned. The notion that there are entrepreneurs out there who have new ideas and who upset the applecart and who think outside the box and who actually change the economic model so to speak by dint of their innovations, all that is just alien to standard economics. So there’s a lot to think about and to bring in to the subject. |
| Q27 | This is something that we hear about continuously across Europe, and I know specifically in Sweden we’ve just recently had a change of government which wants to be more entrepreneur friendly because we are losing manufacturing jobs, like most other countries. But how do you really breed entrepreneurs and how do you really support them? In what way would you suggest that politicians actually do it? |
|  | That’s the 64 dollar question as we used to say, when I was a teenager. I imagine that the best strategy is to do 100 things and hope that some of them work – and if they don’t, do another 100 things. I don’t know how you breed entrepreneurs. If I wanted to be fanciful I would think about maybe changing high school programmes so that students are made to understand that you could be creative in business too. You don’t have to just go into dancing or something like that. There are all sorts of avenues for creativity. I do think – I’ve been writing about this a little bit and if time doesn’t run out maybe I’ll have a chance to say something about it at the end of my lecture on Friday – but I do think that here in the west (and this includes even the United States which is of course very west) the whole classical idea of the good life, a life of adventure, expanding your horizons, discovering your talent, that notion that’s in Aristotle and Cervantes and in Shakespeare and a whole bunch of other writers and thinkers, is just about lost. People today seem to be obsessed with job security and they seem to be asking the politicians to do everything possible to reduce uncertainty. But we can’t have an exciting life if we don’t accept uncertainty.  A certain amount of risk taking.  Edmund Phelps: Lots of it, yes.  A little bit of gambling.  Edmund Phelps: Yes. A shot in the dark.  Now and again.  Edmund Phelps: Right. |
| Q11 | Good one. I wanted to ask you a little bit – and we touched on this slightly before – but we talked a lot about successes and if I asked you which was the proudest moment of your career I’m pretty sure I know what the answer will be, but I’d like to also talk about failure because I think that shapes your route to success as well for people who hang in there and make it past a failure. Have you had any failures or anything that you would regard as a failure? |
|  | I’ve had all kinds of failures. I never got an offer from Harvard. I never got into the American Philosophical Society, which irritates me no end. I’m an American. I’m a sort of a philosopher, too.  But why were you not allowed?  Edmund Phelps: It’s not a matter of not being allowed, it’s just they haven’t voted for me yet.  Ah, OK, is that how it goes? OK.  Edmund Phelps: I remember once – it was December 1992 and I got three pieces of dreadful news in a row – and I was talking with my wife from London when she was in New York and we were both discussing this string of misfortunes. But she said “Gee, you’re so resilient. You sound pretty upbeat considering these disasters”. I guess it’s true. I’ve always had my work to fall back on, I’m fascinated by my work and so in the end it doesn’t matter. I didn’t go into my career just to collect prizes or accolades or even money. I don’t have much money. I went into it for the adventure of it, the mystery of it.  So as long as you are able to keep your focus on your adventure and pursuing it you should be able to surmount …  Edmund Phelps: Yes, weather the storms.  … even one or two failures along the way.  Edmund Phelps: Yes. I’ve had more than one or two.  If you reflect upon them, have the failures been as important as the successes – in breeding resilience I mean; in moving ahead?  Edmund Phelps: That’s a hard question. I don’t know what to say. I’m still capable of writing books that fail. I don’t seem to have learned how to avoid that. I’m also capable of writing books that succeed. I think the thing is about learning from your mistakes, of course that must be important, but at the same time I’m always changing and the environment’s always changing. I’m not going to make the same mistake twice but …  There will be new variables?  Edmund Phelps: … it’s hard to draw lessons from the past about what to avoid in the present. |
| Q10 | I guess I’m thinking about it in terms of students today – those people who will probably want to watch you and listen to you and ask you how did you make it – you must have had really, really bad days – how did you make it past your bad days? How do you keep focus? How do you move on? Is there any advice that you can give to people who want to hear that? |
|  | Edmund Phelps: No, just carry on.  Keep having fun.  Edmund Phelps: Yes. Keep doing what you like.  Keep doing what you like.  Edmund Phelps: Yes, I think that’s a good one.  That makes a great deal of sense actually.  Edmund Phelps: Right.  Since the announcement was made – you’re in Stockholm now, but there was some time in which you were still in the US as well – how did winning this prize actually change things for you at work, among your colleagues? What did your students say? What were the responses from people?  Edmund Phelps: I think my students were kind of thrilled by it really and very nice. My colleagues were also happy for me. It wasn’t the first Nobel Prize in economics around Columbia; it was the fourth one I think, and these came in the past 10 or 12 years. Of course it’s nice to get increased recognition in the university community and I have a little more clout now than I did before – maybe a lot more clout. So, from that point of view, that’s another dimension in which it’s very nice to have a Nobel Prize.  Are there greater expectations from you as well now?  Edmund Phelps: Yes. Now people sometimes interview me for television shows and ask impossibly difficult questions, so that’s a challenge that I could always avoid before but now …  But now you look forward to it?  Edmund Phelps: With mixed feelings. |
| Q49 | There have to be a few reservations. I was talking about your students. We talked a little bit about how students have changed over the years. What about educational institutions themselves? How have they changed, in your opinion, over the years? I know you can’t actually generalise across the US and Europe and the rest of the world. But in general what is your feeling about the way that education is going in the institutions? What are they focusing on? |
|  | I’m not sure that my experience is broad enough to qualify me to speak on that. As I said before, I was very lucky in graduate school at Yale to be in the middle of this hothouse of intellectuals, about half of them European. It was almost a unique time. When I was an undergraduate in college, at Amherst College, it was again a very special experience. We had wonderful courses in the humanities and in philosophy and I took several of those, also they had a wonderful economics department, really kind of inspiring. Now is that kind of thing around today? Maybe it is. It was rare then. If it is, it’s pretty rare now. Maybe I detect that there was a little more informality and a little more creativity in teaching and in shaping courses 50 years ago, 55 years ago, more creativity, more flexibility than there is now. Maybe that has to do with the fact that all the professors are so bent on their research that they don’t throw as much of their creativity and energy into teaching, and they put more of it into research than was the case 50 years ago. But then that’s nice for the students too, because then they see that their teachers are doing real stuff; they’re not just talking about the research of others. |
| Q49 | I increasingly get the sensation that when we talk about students, what people study at universities, Sweden is one of those countries, thanks to its social welfare system, where basically anyone who wants a higher education can have one free of charge with the result that a lot of people do study and do have a higher education. At the same time there seems to be a mismatch with this creative situation, where you have lots of highly qualified people but you don’t have the jobs in industry to accommodate them. We talk about growing a services sector here to replace manufacturing jobs and there are too many highly skilled people. Would you agree that this is something that’s happening, not just here but elsewhere as well – perhaps in other Scandinavian countries? |
|  | I wouldn’t be surprised. There’s a little debate going on in economics about how do you pep up the economies of Western Europe. Now I haven’t been thinking very much about the Scandinavian economies lately – in recent years I mean – but I know that on the continent it’s often proposed that what we need to do is step up the breadth and the quality of education. But if you don’t do something about the economy so that the economy creates jobs for more of these people who are highly educated then there’s going to be frustration and many of the newly created highly educated people will go off to Silicon Valley or something like that; they won’t stay at home. I’m sceptical about thinking that the solution for Europe’s economic problems is more education. I think it’s a better economy, and a better economy would be able to utilise more educated people. |
| Q73 | I’m going to go onto something else now and just ask you, just for the heck of it as Americans would say, if time and money were no obstacle what causes would you dedicate your time to? What are the things that you think are important to you that you would like to spend time on but perhaps don’t have the time today? |
|  | I would like to see the creation of better economies – let me say in the west because I’m not an expert about the economics of the Third World and the less developed countries. But I have the sense that there should be a lot of rethinking about business life, I think that there should be more recognition of the importance of intellectual stimulation in the workplace and problem-solving and discovery of talents. That’s one dimension in which I think that a great many present-day economies are woefully deficient in. The other dimension is the disadvantaged. Both in Europe and in the United States you have very high unemployment rates among the least advantaged people in the labour force and in the United States you have crushingly low rates of pay also for those people. That’s another dimension in which I’d like to see a major improvement in the performance of the economies of the west. I think this is possible. I think where you don’t have an exciting workplace – which is most of the economies of continental Western Europe – there’s a solution to that and the solution is more entrepreneurship; a lot more entrepreneurship and a quantum improvement in attitudes towards business and willingness to bear uncertainty and so forth.  So this is as much to do with business leaders leading companies as it is to do with politicians actually supporting a system that would allow them …  Edmund Phelps: I think on the regulatory front there are all sorts of things the governments can do to stop getting in the way of new firm creation and more entrepreneurial practice opportunity in established firm. Then with regard to the least advantaged there are lot of things the government could do there too. First of all a healthier economy I think would by itself create more jobs for the less advantaged and pull up pay rates of the less advantaged. But in addition the governments in Europe could do a lot more than they’re doing – and also in the United States – could do a lot more than they’re doing now to address the problem of low wages and low employment among the disadvantaged. |
| Q3 | I’m going jump back to something now and I’m going ask you to think of being even more magnanimous than you have been now, talking about the causes you would dedicate time to. Since 1999 the economics prize has been always a shared prize which makes you break a trend here that we’ve seen. I just wanted to ask you if you could be so magnanimous, if you could choose one person to share your prize, who would it be? |
|  | In the same subject?  Your choice.  Edmund Phelps: That’s an interesting question. You see I think the people that I used to feel myself in a friendly rivalry with in my own generation have pretty much all got the Nobel Prize now. I’m the caboose that came along. When you though the train was all done there comes the caboose there, so I don’t think there’s anybody … I’m not sure that in my generation there’s anybody my age that I can immediately think of. But I could jump to younger people: Philippe Aghion, a young French economist now at Harvard, is doing highly original work on innovation and productivity. I’d have been happy to share it with him. And I was half expecting that if I got the prize I would share it with two people with whom I worked in the 1970s at Columbia: Guillermo Calvo and John Taylor. We worked on something called the New Keynesian Economics. What it means is just that we built on the idea that firms don’t adjust their wage rates all together at a certain day of the year the way they do in Japan with the Spring Offensive. Firms adjust their wage rates maybe annually but at different times in the year. That led to quite a lot of significant work so I thought possibly I would share with Calvo and Taylor. Earlier in my career when I was working on economic growth I worked with Richard Nelson and Sidney Winter and I once or twice imagined that I’d share it with them. That would also have made me … I would have been perfectly content about that. But I have to say, Rupini, it’s great to win it alone. |
| ID | 0839 |
| Biographical | Being awarded the Prize in Economic Sciences in Memory of Alfred Nobel is a beautiful, fairy-tale experience, from beginning to end. It is not, however, conducive to research, writing, teaching, or indeed to any ordinary academic work. Instead, one is besieged by hundreds – indeed thousands – of demands to appear and speak at events and congresses, many of which have nothing at all to do with science, to underwrite good (and less good) causes, to sign and send autographs, to be photographed, to be interviewed, etc., etc., etc.  The reader should not misunderstand. This is not a complaint; a Nobelist is glad to do these things, it’s worth it. Rather, it’s an apology for the biography that follows. This biography was due on February 1, 2006, but I simply did not get to it. After many futile attempts on the part of the Nobel Foundation to wrest the biography from me, I was told that if it were not in by May 15, it would simply not be included. This alarmed me, so finally I started seriously working on it. But being serious did not help me; the requests for interviews, appearances, speeches, photographs, etc., etc., kept pouring in. Somehow, though, I did manage to devote a few minutes a day to the biography. Unfortunately (or perhaps fortunately) the result is rather haphazard and disorganized, jumping from topic to topic; but perhaps it is better – more interesting – that way than a more straight-laced essay would be.  I was born in June of 1930 in Frankfurt-am-Main, Germany, to an orthodox Jewish family. My father was a wholesale textile merchant, financially comfortable, whose family had lived in Germany for centuries; he had fought in World War I for the Germans, and been decorated. My mother grew up in London, and obtained a B.A. at University College, London – a somewhat unusual feat for women in the early 20th century. The Nazi regime in the thirties made life very difficult for Jews in Germany, and my parents saw the handwriting on the wall – realized that far worse was in the offing. In 1938 we obtained American visas with some difficulty, and emigrated from Frankfurt to New York. In that passage my parents lost all their assets, and had to work very hard to make a living; nevertheless, they gave their two children – my brother and me – excellent educations, and we had wonderful childhoods. We attended Jewish parochial schools, and obtained bachelor’s degrees at the City College of New York.  In high school I had an extraordinary teacher of mathematics, Abraham Gansler, who taught me to love the subject. What attracted me most were the axioms, theorems, proofs, and constructions of Euclid ‘s geometry. So in City College, I decided to “major” in (emphasize) mathematics. I was mainly interested in classical mathematics: complex and real functions, Fourier series, differential geometry, and so on. Most of all, analytic and algebraic number theory, which I read voraciously, largely from the books of Edmund Landau. Number theory fascinated me because (i) the problems are very natural; (ii) they are simple to formulate, a schoolchild can understand them; (iii) the solutions are very difficult and deep, they require years of university study even to begin to fathom; and (iv) the whole subject was absolutely useless, had no practical applications, was a purely intellectual endeavor. The vogue of pure mathematics – the “purer,” the better – was at its height in the mid-twentieth century, and, young and impressionable, I was drawn into it.  As is customary in American higher education, in addition to mathematics, I studied many other subjects: physics, chemistry, biology, bacteriology, geology, philosophy, English and German literature, history, writing, art, music, public speaking, film, … There was even a course in economics, which baffled and bored me, and which I “dropped” after a few weeks. But many of the other courses left a deep impression. The best were those with a “hands-on” approach: the art course consisted almost exclusively of showing and analyzing (reproductions of) specific paintings and sculptures; the music course consisted almost exclusively of playing and analyzing specific musical compositions; the literature courses consisted almost exclusively of reading and analyzing specific literary works, mostly English poetry and German drama (an entire semester was devoted to Goethe’s *Faust*). An extreme example was the course in geology. For three weeks we did nothing but learn the names of various kinds of rock, pieces of which were provided in the classroom. At the end of that period there was an examination: we were provided with twenty-five or thirty pieces of rock – not pictures, but the rocks themselves – which we had to identify. After that, we never saw the inside of the classroom again; the course consisted *exclusively* of field trips, all inside New York City and its immediate surroundings, all accessible by public transportation. I will never forget what I learned there – why rivers meander, what makes rocks different, dikes, glaciation, U- and V-shaped valleys, etc., etc.; when hiking in Israel and all over the world, I teach these things to my children and grandchildren, who do not have the benefit of such a marvellously broad education.  After completing a B.S. at “City,” I entered the Massachusetts Institute of Technology (MIT) for graduate studies. At MIT I became interested in the more modern branches of mathematics, like algebraic topology, to which I was attracted by the excellent lectures of George W. Whitehead. I decided to do a Ph.D. with Whitehead in knot theory, a branch of algebraic topology that deals with the properties of knots (those tied in ropes). Like analytic number theory, knot theory deals with problems that (i) are very natural, have an immediacy that is even greater than that of the distribution of prime numbers or Fermat’s last theorem; (ii) are simple to formulate, a schoolchild can understand them; (iii) have solutions that are very difficult and deep; and (iv) the whole subject was absolutely useless.  Whitehead gave me a very difficult problem – one that had been attacked unsuccessfully for a quarter of a century – namely, to show that knots are “aspheric” (we won’t explain here what this means). I didn’t solve this problem, but did establish asphericity for knots of a special kind: those that are “alternating.” That means that when you draw the knot on a piece of paper, and follow along any component of the knot, then the undercrossings alternate with the overcrossings – as, for example, in the famous “Borromean Rings.” My thesis was published in the *Annals of Mathematics* in 1956.  After completing the Ph.D., I went to work for an Operations Research consulting outfit – the “Analytical Research Group” (ARG) – affiliated with Princeton University ‘s mathematics department, and located at the University’s Forrestal Center for Applied Research. ARG did highly practical consulting. One of the problems that I was assigned had to do with defending a city from attack by a squadron of aircraft, a few of which are carrying nuclear weapons, but most of which are decoys. At MIT I had met [John Nash](https://www.nobelprize.org/prizes/economic-sciences/1994/nash/facts/) – who in 1994 shared the Prize in Economic Sciences in Memory of Alfred Nobel with [John Harsanyi](https://www.nobelprize.org/prizes/economic-sciences/1994/harsanyi/facts/) and [Reinhard Selten](https://www.nobelprize.org/prizes/economic-sciences/1994/selten/facts/), and who came to MIT as a young instructor in the early fifties – and had heard a little about game theory from him. At the time it didn’t interest me very much, but when I was assigned the problem about the decoys, I remembered the conversations with Nash, and figured that game theory had to be the right tool to attack this problem. So I studied some game theory – just enough for this problem – and then the subject started attracting me in its own right. The rest is history, as the saying goes.  Jews have been yearning for the land of Israel, and for Jerusalem, for close to 2000 years – ever since the destruction of the Temple by the Romans in the year 70, and the ensuing exile of the Jewish people. In our central prayer, which we recite three times a day, we ask the Lord to “return to Jerusalem Your city in mercy, and rebuild it and dwell therein.” Jerusalem is mentioned many thousands of times in the scriptures, in our other prayers, in the Talmud, and indeed in all our sources. So when the state of Israel was established in 1948, my brother and I made a determination eventually to make our lives there. My brother fulfilled this ambition shortly thereafter, in 1950, but I decided first to complete my education. In 1953 I met an Israeli girl, Esther Schlesinger, who was visiting the United States; we were married in Brooklyn in April of 1955. In the fall of 1956 I took up a position as instructor of mathematics at the Hebrew University of Jerusalem, and have been there ever since. Esther and I had five beautiful children; the oldest, Shlomo, was killed in action in 1982, while serving in the Israeli Army in “Operation Peace for Galilee.” At this writing, I have nineteen grandchildren and two great-grandchildren. Esther died of ovarian cancer in October of 1998, after we had enjoyed forty-four truly magnificent years together. In late November of 2005, about a week before being awarded the Prize in Economic Sciences in Memory of Alfred Nobel, I married Esther’s widowed sister, Batya Cohn.  When the Prize was announced, the work of mine most prominently mentioned by the Committee was my 1959 paper “Acceptable Points in General Cooperative *n*-Person Games,” which is perhaps the first rigorous treatment of repeated games that has some generality. Briefly put, the finding of that paper is that the strong equilibrium payoffs of a repeated game coincide with the core (more precisely, β-core) payoffs in the one-shot game. Frankly, I don’t recall well the genesis of that paper. As mentioned above, I became interested in game theory while at Princeton in the years 1954–56. The 1957 Luce–Raiffa book *Games and Decisions,* which made a big impression on me, has an interesting – though inconclusive – discussion of repeated games, and this may have piqued my interest. I vividly recall working on “Acceptable Points” while at the Bureau of Standards in Washington in the summers of 1957 and 58; the yellow pads are still before my eyes. In the course of that work I became aware of what later became known as the “Folk Theorem” (see my 1981 “Survey of Repeated Games”), but it seemed to me at the time that it did not have sufficient mathematical depth to merit publication. That was a big mistake. Both “Acceptable Points” and the Folk Theorem are expressions of the relation between equilibrium behavior in the repeated game and cooperative behavior in the one-shot game; but while “Acceptable Points” is undoubtedly interesting, and much the deeper and more subtle, the Folk Theorem is by far the more fundamental and important.  At some point during the academic year 1959–60 I gave a colloquium lecture at the mathematics department of the Hebrew University; the colloquium is a weekly gathering of the entire department, at which a faculty member or guest talks about his own research or a related topic. I chose to discuss the von Neumann–Morgenstern “solutions” of cooperative *n*-person games, sometimes called *stable sets.* Historically, this is the first “solution concept” for cooperative games, and to this day it remains one of the most subtle and beautiful. Michael Maschler, an expert in the theory of functions of a complex variable, was in the audience; after the talk he asked a question. This question led to a lifelong scientific partnership with Maschler.  The specific question that Maschler asked led eventually to our 1964 joint paper on the *bargaining set* for coalitional (a.k.a. cooperative) games. In turn, this led to a very considerable literature, encompassing related concepts such as the *kernel* (Davis and Maschler 1965) and *nucleolus* (Schmeidler 1969). But I myself made only two additional contributions to this subject after 1964. One was a computation and tabulation of the kernels of several classes of games with up to five players, done jointly with Bezalel Peleg from the Hebrew University and Pinny Rabinowitz from the Weizmann Institute of Science; this work led to several conjectures on the structure of the kernel, which turned out to be very fruitful and led to important theoretical advances and a much better understanding of this structure. The other is the 1985 paper “Game-Theoretic Analysis of a Bankruptcy Problem from the Talmud,” also joint with Maschler; it is an explanation of a difficult passage in the Babylonian Talmud (Ketubot 93a), the key to which was Schmeidler’s nucleolus. This is undoubtedly the work of mine that is best known; not so much to the scientific public – though there, too, it is widely cited – but to the general public. I have lectured on it dozens – perhaps hundreds – of times, to scientific audiences as well as to high school students and teachers, synagogue groups, Talmudic academies, and so on. It has been quoted and explained and reworked by many different people, innumerable times; I cannot possibly keep track of it.  Another important joint work with Maschler is that on repeated games of incomplete information. This began as part of a project initiated by the United States Arms Control and Disarmament Agency (ACDA) in the mid1960s, to help shape United States policy in the arms control negotiations that were taking place with the Soviet Union at that time. Involved in this project, in addition to Maschler and me, were John Harsanyi, Reinhard Selten, and [Gerard Debreu](https://www.nobelprize.org/prizes/economic-sciences/1983/debreu/facts/) – all three of whom eventually became Nobelists; Dick Stearns – eventually awarded the Turing prize in theoretical computer science; Herb Scarf and Harold Kuhn – eventually awarded the John von Neumann Prize in operations research theory; Jim Mayberry, whom I had gotten to know at ARG; and others. Several times a year the members of this stellar group would converge on Washington to discuss arms control and disarmament with each other and with the personnel of the agency; between meetings, we would work alone or in small groups in our home environments. I doubt that this work did indeed have much practical influence on policy, though one can never know; a side discussion with an agency staff member at lunch or during a coffee break can sometimes leave a deep impression – conscious or subconscious – that may eventually profoundly affect policy.  Be that as it may, the work of Maschler and me on repeated games of incomplete information, in which Dick Stearns also played an important role, spawned a large scientific literature. For many years the original work was difficult to obtain; it was available only in the form of the original ACDA reports, and one had to scrounge around for it. Eventually, in 1995, the original reports were published in book form by the MIT Press, together with extensive notes describing much of the subsequent work (up to 1995, of course). This book was awarded the Lanchester Prize of the Operations Research (OR) Society of America for the best OR book in 1995.  Maschler is by no means my only collaborator. I have collaborated – and am collaborating – with more than thirty scientists; a good part of my prize is attributable to them. Prominent among them is Lloyd Shapley, with whom I coauthored the 1974 book *Values of Non-Atomic Games;* it concerns games with many players, who impinge significantly on the outcome only when they form large coalitions, but not as individuals. Examples are national elections or large economies or markets. Another collaboration with Shapley concerns perfect equilibria in repeated games; it is a sharpening of the Folk Theorem, and it, too, was cited by the Nobel Committee. At around the same time that Lloyd and I were looking into this at the Rand Corporation in Santa Monica, California, in 1976, Ariel Rubinstein was doing a master’s thesis – with Bezalel Peleg in Jerusalem – on precisely the same question, and he reached essentially the same conclusions that we did. Although it became widely known soon thereafter, this work was not published until 1994 – most appropriately, in a festschrift honoring Maschler.  Others with whom I’ve collaborated extensively – and intensively – throughout the years include Jacques Drèze, Mordecai Kurz, Sergiu Hart, Bezalel Peleg, Adam Brandenburger, Frank Anscombe, Abraham Neyman, Benjy Weiss, Micha Perles, Joe Kruskal, Roger Myerson, and others. At this writing I’m collaborating with Roberto Serrano on a project on which we’ve already been working for several years; with Sergiu Hart on another project; and with Hart and Motty Perry on yet another project.  Influence is not limited to joint papers. Innumerable individuals impinge on one, both in person and in what one reads and hears. Prominent among them are one’s students, but there are many, many others. Science is one huge cooperative venture. The Nobel Prizes focus attention on important scientific work by focusing on a small number of individuals; but really, any scientific work – including work that merits a Nobel Prize – is the product of many minds.  For the finale, we flash half a century back. Though writing it up took a little longer, my thesis was essentially complete in October of 1954; I remember standing in the shower and being hit with the mathematical idea that enabled its completion. Fifty years later – almost to the day – at 10:00 pm, the phone in my flat rings. My grandson Yakov Rosen, who is in the second year of medical school, is on the line. “Grandpa,” he says, “can I pick your brain? We are studying knots. I don’t understand the material, and think that our lecturer does not understand it either. For example, what, exactly, are ‘linking numbers’?” “Why are you studying knots?” I ask. “What do knots have to do with medicine?” “Well,” says Yakov, “sometimes the DNA in a cell gets knotted up. Depending on the characteristics of the knot, this may lead to cancer. So, we have to understand knots.”  I was completely bowled over. Fifty years later, the “absolutely useless” – the “purest of the pure” – is taught in the second year of medical school, and my grandson is studying it.  Science is exploration – exploration for the sake of exploration, and for nothing else. We must go where our curiosity leads us, we must go where we want to go. And eventually, it is sure to lead us to the beautiful, the important, and the useful.  For me, life has been – and still is – one tremendous joyride, one magnificent tapestry. There have been bad – very bad – times, like when my son Shlomo was killed and when my wife Esther died. But even these somehow integrate into the magnificent tapestry. In one of his beautiful letters, Shlomo wrote that there can be no good without bad. Both Shlomo and Esther led beautiful, meaningful lives, affected many people, each in his own way.  And there have been a *lot* of very good times. The excitement of research, of groping in the dark and then hitting the light. The satisfaction of teaching, of meeting someone at a party who tells you that the course in complex variables that he heard from you twenty-five years ago was the most beautiful that he *ever* heard. The exhilaration of climbing – seeking and finding foot- and handholds – on an almost vertical rock face. The beauty of a walk in the woods with a four-year-old grandchild, who spots and correctly identifies a tiny wild orchid about which you told him last week. Dancing with your wife at your child’s wedding. Unraveling an intricate passage in the Talmud with your eighteen-year-old granddaughter, or with a study partner with whom you have studied for thirty years. Slipping on a “black” (expert) ski slope, tumbling two hundred meters down, and then going back and doing the same slope again – this time without a slip. Cooking a meal and hearing from a guest that the soup was the best she *ever* tasted. Raising a beautiful family. Seeing the flag of Israel fluttering in the wind, right next to that of Sweden, from the roof of the Grand Hotel in Stockholm. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0839  – Hello?  – Yes, I’d like to speak to Robert Aumann, please.  – Yes, speaking.  – Hello. My name is Maria Ullsten. I am doing a recording for Nobelprize.org which is our official website.  – Okay.  – So I would like to wish you: Congratulations!  – Thank you.  – How did you find out that you had been awarded the Prize?  – Well, the Nobel Prize Foundation called me …  – And where were you?  – In my office; it’s just about an hour and a half ago. And they called me, and they told me. They told me … “Will you be in your office in fifteen minutes? We want to have a press conference.”  – Okay. During those fifteen minutes, what went through your mind?  – Well, I finished some letters that I was writing, and I thought … What went through my mind is that I was very glad.  – Did you call anyone?  – No.  – Did you tell anyone that you had won the Prize?  – Just a moment, there’s another … [cell-phone interference]  – I’ll wait.  – There’s another phone call.  – Okay.  [on other phone] /*Hello? Hello?* …/ Sorry. Yes?  – No problem. That’s all right. I realise you must have had a lot of people calling you today.  – Yes. I want to go home and change clothes and put on a suit, for this press conference that’s going to take place. But I don’t have the chance [cell phone signal], because every moment there’s a phone call.  – I understand.  – Here’s another phone …  – No problem. I’ll hold.  [on other phone] /*Hello?* …/ Hello.  – Yes. Hello!  – Yes, yes.  – I understand. You mentioned that you’d like to go home and put on a suit.  – Yes.  – Is there a ceremony or something planned?  – No. A press conference.  – A press conference.  – I really don’t know why I have to put on a suit for a press conference, but that’s what they told me.  – That’s what you were told. I understand. Have you had a chance to speak to your family yet?  – Just a moment. My boss wants to give me a kiss. Just a second. /…/ Hello?  – Yes, hello. Have you had a chance to speak to your family yet?  – Some of my family called me.  – So they know.  – Yes. Those who know, know. I didn’t tell anybody …  – Do you know how they found out that you got the Prize?  – I guess from the radio or the television, whatever.  – And did the Prize come as a complete surprise, or had it ever gone through your mind that you might get it one day?  – Has it ever gone through my mind that I might get the Prize? Well, yes it did.  – And have you been thinking about it at this time of year when the announcement is made?  – No, no, no, no no, no. No, no. Actually I gave up on it a long time ago.  – Okay. So who was the first person that you actually …?  – What? Excuse me?  – Who was the first person that you managed to tell about the Prize? Was it your …?  – I didn’t tell anybody. People came in, to me. I didn’t tell anybody.  – And have you had a chance to celebrate at all yet?  – No, no, no. It’s just the whole thing is an hour and a half old and people have been calling all the time.  – And in this hour and a half as a Prize-winner, how does it feel?  – It feels great.  – And could you also, in a few words, describe, for a young audience …? [cell phone signal]  – Could I tell …? Just a moment, please. /…/ Yes, excuse me?  – Yes, if you could describe, for a young audience, what exactly you have been awarded the Prize for.  – It’s for game theory, which is a … It’s a branch of science which studies situations where people interact and each person has a different goal. Each person wants to do the best that he can for himself, and … [cell phone signal]  [on other phone] /*Hello? Hello?* …/ Just a moment, please.  – Can I ask you – Thomas Schelling that you were awarded the Prize together with, have you had a chance to speak to him yet?  – No, I haven’t had a chance to speak to Tom, but … Certainly I’m delighted that he got it and I’m delighted to share it with him.  – And do you know each other very well, or …?  – Yes, we know each other fairly well.  – What will you …?  – We’ve known each other for many years.  – Getting this prize, what will it mean for your work? [cell phone signal]  – Just a moment, please. /…/ Yes.  – Yes. No problem. I understand it’s a hectic day. But, when you do get a chance, how will you celebrate the Prize?  – How will I celebrate?  – Yes.  – Oh, I don’t know. I guess we’ll go skiing.  – Okay. Where do you go skiing if you live in Israel?  – Just a moment, please.  [on other phone] /… *Shalom* …/ Hello?  – Yes. Hello. Are you still there?  – Just excuse me. /…/ I think we’d better … Because it’s impossible …  – I understand. I just really wanted to congratulate you on the Prize and we hope that we will see you in Stockholm then, in December.  – Thank you very, very much.  – Okay. Thank you.  – Okay. Bye!  – Bye-bye. |
| Interview |  |
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| ID | 0840 |
| Biographical | I was born April 14, 1921, in Oakland California, spent most of my boyhood in California, with three years in the east and two in the Panama Canal Zone, my father being a naval officer. I attended the University of California, Berkeley (with two years out in Chile), graduating in economics in 1944. After a year and a half as an analyst with the U.S. Bureau of the Budget I attended Harvard University, completing my Ph.D exams in June of 1948. Appointed a Junior Fellow of the Society of Fellows, I took leave to join the administration of the Marshall Plan, spending one year in Copenhagen and a year and a half in Paris, resigning my fellowship. In November, 1950, I joined the White House Staff of the foreign policy adviser to the President, which in 1951 became the Office of the Director for Mutual Security, the office that managed all foreign aid programs. I left in the fall of 1953 to join the faculty of Yale University.  My experience abroad and in Washington mostly involved negotiations. I was an active participant in negotiating the European Payments Union in 1950; in Washington my responsibilities related to aid negotiations with European governments, primarily in connection with those governments’ contributions to the new NATO defense establishment. I had, at Harvard, become interested in bargaining strategy, and my government experience gave me much of the background I needed when later I decided to make bargaining theory my primary theoretical interest.  At Yale I began publishing what I believe the Nobel selection committee considered my contribution to “understanding cooperation and conflict,” first an “Essay on Bargaining” in 1956, in the *American Economic Review,* and “Bargaining, Communication, and Limited War” in the inaugural issue of the *Journal of Conflict Resolution,* 1957. Interestingly, these two articles were completed before I had more than a smattering of acquaintance with formal game theory. In 1957 the book, *Games and Decisions* by Howard Raiffa and R. Duncan Luce was published; it was my professional introduction to game theory, and I spent at least a hundred, maybe two hundred, hours with it.  In the spring and summer of 1958 I took my family to London, where I pursued what I considered my concept of game theory in a manuscript – typed by the woman on Charing Cross Road who did all of Agatha Christie’s books and plays – and submitted it to the *Journal of Conflict Resolution.* It was so long that that Journal decided to make it a whole issue. I persuaded the editor that a smart way to publicize the new journal would be to give me, without charge, instead of reprints three hundred copies of the journal to send to everyone I could think of. I called my article, “Prospectus for a Reorientation of Game Theory.” I was trying to get game theorists to pay more attention to strategic activities, things like promises and threats, tacit bargaining, the role of communication, tactics of coordination, the design of enforceable contracts and rules, the use of agents, and all the tactics by which individuals or firms or governments committed themselves credibly. I don’t think I had any noticeable influence on game theorists, but I did reach sociologists, political scientists, and some economists.  While in London I had made the acquaintance of several scholars and former military officers who were interested in theories of deterrence and limited war. I began to appreciate that the most immediate and important application of the kind of “game theory” I was pursuing was in military foreign policy, especially nuclear weapons policy. I became a close friend of Alastair Buchan, who was just establishing the Institute for Strategic Studies in London, an institute that was to be hugely influential in drawing scholars from all over Western Europe, North America, and Japan to its annual meetings at Oxford, Cambridge, Bonn, and other sites.  I then became the guest of the RAND Corporation, in Santa Monica , California, for twelve months, before settling at Harvard University , which had offered me a position I’d share between the Department of Economics and the Center for International Affairs. At RAND I continued my theoretical work, and went to Washington with a small contingent attached to the staff preparing for a prospective Geneva Convention on Measures to Safeguard Against Surprise Attack. I did not go to Geneva ; I did, however, produce two papers. One, “Surprise Attack and Disarmament,” explored the concept that the problem of nuclear surprise attack was the problem of the advantage, in case of war, of being the side to start it. Arms control should be oriented toward measures that precluded either side’s acquiring a pre-emptive capability, a “first-strike” capability as it was called. And this objective, somewhat paradoxically, entailed arranging for the safety not of populations and industrial assets but of retaliatory nuclear weapons.  At RAND I also developed the idea of a “probabilistic threat,” and spelled it out under the title, “The Threat That Leaves Something To Chance.” I also, that year at RAND, began drawing on an idea that is sometimes referred to as a “Schelling point,” or “focal point,” to argue that the only viable convention regarding the use of nuclear weapons would be “no weapons,” not some quantitative or qualitative limits. (This idea became the germ of my Nobel Memorial Lecture, forty-five years later.)  Most of the work I have described appeared in 1960 as *“The Strategy of Conflict,”* Harvard University Press.  I then spent thirty-one years at Harvard University, first in the Department of Economics and the Center for International Affairs, then in the Department but also, beginning with its establishment in 1969, in the John F. Kennedy School of Government. For ten years the Center gave me freedom to write and to consult, and I spent much of my time, especially during the summer, doing advisory work for the government.  During my first year at Harvard the Center received a grant, together with the MIT Center for International Studies, to spend on some joint activity. A colleague at MIT and I decided we’d establish a Center for Arms Control, that would meet every three weeks at one or the other faculty clubs for dinner discussion. The summer of 1960 I spent, with a dozen or more colleagues from Harvard and MIT at a “summer study” of arms control, financed by the Twentieth Century Fund. I had arranged to host a young colleague from the Yale graduate school, Morton H. Halperin, for his dissertation work, and took the occasion to make him a rapporteur for the summer study. At the end of the summer, Halperin and I decided to write a book reflecting the consensus the group was developing. We took advantage of the Harvard MIT Center for Arms Control by submitting chapter after chapter as the texts for discussion. The book, *Strategy and Arms Control, 1961,* was finished and available within a couple of weeks of the Kennedy inauguration.  The timing was perfect. Kennedy appointed as his national security adviser a Harvard dean who had participated in the autumn discussions of arms control, and as his White House science adviser an MIT professor who had been one of the group; another member became Deputy Assistance Secretary of Defense for Arms Control, another General Counsel of the State Department. Because of these connections I was appointed chairman of several interagency committees concerned with nuclear weapons policy over the next several years. (One of them brought into being the “hotline” between the Kremlin and the U.S. Government, another initiated the process that led, after a hiatus caused by the Soviet invasion of Czechoslovakia, to the Anti-Ballistic Missile Treaty.)  Partly because I had “connections,” I devoted most of my research during the ‘60s to weapons policy, publishing *Arms and Influence,* Yale University Press, 1977. In the spring of 1970, upon the U.S. invasion of Cambodia, I led a group of Harvard faculty to meet with President Nixon’s national security adviser to declare our opposition to the invasion and break relations with the Administration. That ended my connection with the government.  During the seventies and eighties two subjects intrigued me. One resulted from my participation, for seven years, in a committee of the National Academy of Sciences on Substance Abuse and Addictive Behavior. I observed that people who had habits or addictions or delinquencies often attempted, sometimes successfully, sometimes not, to deal with themselves as they might deal with another’s misbehavior, attempting, in effect, to “commit” themselves to avoiding the bad behavior or performing the good. Several essays on this topic are in two books of mine, *Choice and Consequence,* 1984 and, just recently, *Strategies of Commitment,* 2006.  The second subject that occupied me in the seventies was the ways that individual behavioral choices could aggregate into social phenomena that were unintended or unexpected. One part of this work involved modeling spatial “segregation,” the ways that people who differ conspicuously in binary groups – e.g. blacks and whites, males and females, officers and enlisted personnel, francophones and anglophones – get separated spatially, in residence, in dining halls, at public events. Without knowing it I was pioneering a field of study that later became known as “agent-based computational modeling.” Much of this work was published in *Micromotives and Macrobehavior,* 1978.  In 1980 President Carter was to attend a “summit” in Venice. The Chancelor of Germany had submitted, for the agenda, the “carbon dioxide problem.” The White House asked the National Academy of Sciences for advice on what to do with that item. I was invited to chair a committee that would do a quick study and prepare advice; I confessed I knew virtually nothing of the subject and was told I could learn most of what was known in the four weeks before the committee would meet. I took the job, I had a superb committee and learned a lot, we did a satisfactory report, and I thought that was the end of an interesting experience.  A few months later the Congress appropriated funds for a longer, more substantial study, and, undoubtedly because I recently chaired a committee on the subject, I was asked to join the Carbon Dioxide Assessment Committee of the National Academy of Sciences. There I spent some fifty days over a two year period with a dozen scientists from the most pertinent disciplines and became an extremely well educated amateur. I wrote the chapter of our report on “policy and welfare implications of climate change.”  That subject remains a major interest. Its relevance to my Prize is that mobilizing to do something about prospective global warming and climate change is what I expect to be, during this century, what nuclear arms control was during the century just past, namely an immense challenge to “cooperation amid conflict.” My latest thoughts on the subject can be found in the 2006 book mentioned above.  In 1990 I retired from Harvard and accepted appointment as Distinguished University Professor at the University of Maryland, in the Department of Economics and the School of Public Policy. I continued my interest in nuclear weapons policy, climate change, commitment, and terrorism, the latter interest stimulated by another invitation to participate in a committee of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine, the Committee on Science and Engineering for Counterterrorism, two months after September 11, 2001.  As I reflect on my career I am struck with how much of what I am pleased to have accomplished was initiated by good luck and by the initiative of others. During the War, deemed unfit for military service by the Army and the Navy, I landed a superb job in the Bureau of the Budget; with that experience I was admitted to Harvard Graduate School with a teaching fellowship; I was invited to join the Marshall Plan and my boss took me to Copenhagen; from there to Paris and from there to the White House. RAND was by unsolicited invitation. The Kennedy Administration drew colleagues into influential positions and gave me access to senior officials. National Academy of Sciences committees approached me unexpected. I’ve had all the advantages. And now the Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel promises more opportunities. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0840  – Hello?  – Yes, hello. I’d like to speak to Thomas Schelling, please.  – This is he.  – Okay. My name is Maria Ullsten; I’m calling from Stockholm and I would like to do a recording for Nobelprize.org which is our official website. I would like to first congratulate you on the Prize.  – Thank you.  – How did you find out? Who called you?  – I was called by the secretary of the Committee; it’s just about thirty minutes ago.  – And where were you when you got the call? Were you asleep?  – I was asleep when the phone rang.  – Okay. And what was the first thing that went through your mind? What were you thinking?  – When he told me he was phoning from Stockholm?  – Yes.  – I was greatly surprised.  – And now, after your first half hour as a Prize-winner, how does it feel?  – Well, it feels busy. Yours is the third phone call.  – And it’s still not seven o’clock in the morning.  – Yes.  – Did you expect to get this Prize?  – No, I didn’t.  – Was it a complete surprise, or had it gone through your mind that you might get it one day?  – Well, for the last two years there have been rumours that I might be awarded the Prize. And, while I didn’t take it very seriously, naturally it was on my mind.  – Yes. Who was the first person that you told about the Prize?  – My wife.  – Yes, and what did she say?  – She was pleased.  – And have you had a chance to speak to Robert Aumann, whom you are sharing the Prize with?  – No, I haven’t.  -Have you been congratulated yet? You mentioned that you’d had a few phone calls.  – Well, my phone calls have been from … One from a correspondent in Bratislava and one from somebody in Columbia, and now you, and …  – That’s it so far?  – That’s it so far.  – Perhaps you could describe in a few words for a young audience what exactly it is you have been awarded the Prize for.  – Well, the language that I was given over the telephone is: for my “studies of conflict and co-operation”. And they weren’t any more specific than that.  – And how have your theories proved useful?  – Well, I spent a long time working on the subject of nuclear weapons control. And I have worked some on the economics of crime, and on studies of racial segregation, and I’m not sure just what it was that most attracted the Nobel Committee.  – It sounds very interesting. What will the Prize mean for your work? Will it change anything? Will you continue to work as usual?  – I don’t think it will change my work, no. I imagine I will get more invitations to go speak someplace, now that I’m a Nobel Laureate. But, otherwise, my … I’ll go on as I’ve been doing.  – And what will you do today? Will you have a chance to think about that?  – I have a hunch that I’ll be wanted to go out to the University of Maryland for some kind of ceremony, but it’s too early to know.  – Well, we hope to see you in Stockholm in December.  – It’s going to be what – December 10th?  – Yes. I think so. Have you had any chance to think about what you actually will do with the money?  – Oh, no. It’ll go into our bank account and we’ll live on it. Okay?  – Okay. And why did you start, if I may ask, to study economics? Was it a coincidence or a deliberate decision?  – To study economics?  – Yes. Why did you …?  – Well, you have to realise, I was brought up during the Great Depression and when I went to college I felt that the worst problem we had was the problem of depression and unemployment, so I majored in economics.  – I see. And what would you like to say to young people today? Why should anyone start to study economics today?  – I wouldn’t necessarily try to talk somebody into to becoming an economist; but I think everybody should have a basic understanding of how the economy works.  – Well, thank you so much for your time, and congratulations again.  – Thank you.  – Thank you very much. Bye-bye.  – Bye. |
| Interview |  |
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| ID | 0841 |
| Biographical | In the winter of 1968, Sten Thore, who was then a professor of economics at the Norwegian School of Economics and Business Administration (abbreviated *NHH* in Norwegian), where I was finishing my three undergraduate years, made me an offer that would change dramatically the path I was to take for the rest of my life. I had gone to the business school with the expectation that I would eventually become a business manager. But when Sten asked if I would like to be his research assistant (vitenskapelig assistent), I agreed without thinking about what would turn out to be one of the most crucial decisions of my life.  It is interesting that I would be employed by the Department of Economics (to which Sten belonged as a faculty member), as I had not shown any more interest in the economics classes than in business. Admittedly, as business schools go, the curriculum at NHH included a substantial focus on economics. The department housed several economists who were highly visible internationally.  During my studies, I had made a couple of wise (or perhaps just lucky!) decisions. The curriculum did not permit any flexibility, except in two ways. One was to choose two elective areas of concentration, which students were to pursue by taking one course for each area per semester during the first two years. These elective tracks could be selected among four foreign languages, economic geography, economic history, law, and mathematics. I chose mathematics as one of the two (German being the other). I even took two math courses beyond the four-course sequence required for the elective.  Another source of flexibility was that the curriculum called for three relatively advanced courses, to be selected from an extensive list. My second wise decision was to take, as one of the three, the one offered by Sten Thore. In this course, we read from Howard’s book on *Dynamic Programming and Markov Processes* and several rather mathematical articles from journals such as *Operations Research* and *Management Science*. I wrote my first computer program (in FORTRAN) doing dynamic programming, a tool I’ve used repeatedly ever since. After I had finished the course, Sten recommended to me a summer job at the local shipbuilding company to work on a computer program designed to determine a reasonable ship size for any particular route, given the available data on tonnage to be shipped and the per-unit time it took to load and unload it. Mathematically, it was an application of fractional programming – linear constraints and an objective function consisting of the ratio of two linear expressions.  As I said, Sten encouraged me to become a research assistant as I was approaching the end of my studies rather than take a job in industry. (He saved me from a boring life!) But then, after I had worked a few months as his research assistant, Sten informed me that he would be going on leave for a year to Carnegie-Mellon University, starting in January, and would I like to do my research-assistant duties there, and I agreed again. I postponed that move, however, until the summer, as Liv Kjellevold and I had married in August 1968, and Liv was finishing her last year as a nursing student. The second half of 1969 was when my fate was sealed – I would be an academic, and economics would be my field.  The early years There had been virtually no indication earlier in my life that such an outcome might even be a possibility. I grew up in Søyland (although born on the farm of my mother’s parents in the neighboring township of Bjerkreim), a small area of the township Gjesdal, about 40 km south of Stavanger. There were seven farms and us. One of the neighboring farms was my grandfather’s. My father, Martin, was the eldest son and therefore in line to take over. He decided, however, to buy a truck – the first in the area to do so. He would base his living largely on a milk route between Søyland and the nearest diary, in Ålgård, 15 km away, and also on hauling other goods (and, in the spring, sheep to better pastures, returning them in the autumn) for the farmers. Eventually, he expanded to two trucks. My mother, Johanna, worked at home until all the kids (six of us, of whom I’m the eldest) were grown. I’ve been told that my father did well in school, although neither of my parents tried to influence their children in terms of career paths. In fact, it came as a surprise to both when I ended up as an academic.  The elementary school, in which all the pupils, except for my siblings and me, were farmers’ kids, did seven years divided in three classes. We met twice a week in the first three years and three times a week during the remaining ones. The education wasn’t especially “active.” I was the only one in my class to go beyond elementary school. As a 15-year old, I went off to Bryne to attend Rogaland offentlege landsgymnas, the nearest high school. The need to rent a room was obvious, and probably half the pupils did so, as the distance to home made it infeasible for any of us otherwise to get to school in the morning in time for classes. Most pupils came from rural areas, as the cities typically had at least one high school.  This particular high school required an entrance exam, which I passed easily. As with all high schools in those days, one had to choose a concentration. This high school offered two, one emphasizing math and physics, and one placing a greater weight on foreign languages (although we all had English, French, and German for at least three years). Some high schools (but not this one) also had a concentration in topics oriented towards business and economics.  The education was exceptional. I’ve sometimes claimed that I knew more math at the end of high school than a typical American business or economics major, even at a university as highly ranked as Carnegie Mellon, know at the end of college. (As a consequence of my personal experience through my first 12 years of schooling, my bias has been to pooh-pooh the need for intensive education in elementary school, believing it is better to allow the pupils more time for play while they’re kids, and instead to emphasize the importance of great high-school education.) Whether because the student body was more select than at most other high schools or because the teaching was first rate (or both), this high school always ranked highly in terms of number of *preseterister* – those students who had the grade of *very good* or *excellent* (the latter almost impossible to achieve) in absolutely all subjects. Indeed, to achieve the status of *preseterist* was regarded as important enough all over Norway to be worthy of photos in the local newspaper. In my case, I missed that distinction because of my grade in one subject: Norwegian composition. But my point score was still high enough to offer me the choice of just about all the university majors (the only exception being, I thought at the time, theoretical physics).  My initial inclination was to apply to the university for engineering studies, not out of a deep interest in engineering, but more for the simple reason that I had had an easy time with math and thought that’s where that skill would be rewarded handily. Still, I had a nagging doubt about that decision. So to give me extra time to think about it, I applied for a one-year teaching position at the elementary school in Oltedal, the second-largest town in Gjesdal, and got it. In those days, the shortage of elementary-school teachers meant that such temporary hires straight out of high school were not uncommon. Thus, I spent a year teaching fifth and sixth graders in all subjects on Mondays through Fridays and second and third graders in Norwegian on Saturdays.  The year at Oltedal elementary school turned out to be important for my future. One of the other three teachers I’d see during lunch and other breaks was Harald Aarrestad who taught a junior-high-school class in the same school building. This program was nonaccredited, meaning that the students, at the end of the two years, would have to take their final exams in all fields at an accredited junior high school in a different town or city, but while they were in school, they could live at home. Aarrestad had taken the initiative to start this program and taught all the subjects. Further evidence of his energy and imagination was that he had started and was running two small businesses. I found him to be an extremely interesting person. Because his accountant was making lots of mistakes, he encouraged me to take a correspondence course in accounting and then promptly hired me as his accountant, a job I could easily do in my spare time. This experience gave me insight also in what it meant to run a business, a subject about which I had hitherto known nothing. Business hadn’t been among the fields I had even considered for study. But by the end of the year, I decided to apply to NHH.  I had thought that, with my high-school grades, theoretical physics was the only field unavailable to me. I was wrong – NHH rejected me! Business education in Norway was a relatively young field – NHH had been started in 1936. The class size was only 60 students, and these students came from all over Norway. I learnt later that NHH gave preference to students graduating from the business concentration (which wasn’t even offered at my high school). With that concentration, one could be admitted with a considerably lower high-school GPA than mine. Evidently, in those days, there was no appreciation for the notion that, in business and economics, mathematical ability could more than make up for lack of background in business subjects!  An option open to me was to study for a supplementary exam (in economics, law, business correspondence in English, German, and French, even in typewriting, which has served me well ever since!) to make my high-school education equivalent to that in the business orientation. This could be done through correspondence courses, which is what I decided to do. In the meantime, Aarrestad encouraged me to stay for another year in Oltedal, teach two subjects in his junior-high program, continue to do the accounting for him, and be fully in charge of running one of his two businesses (which imported tropical fish from Holland and distributed them to retail stores all over Norway) on a profit-sharing basis. As a result of being busier than expected, I was far from being finished with my correspondence courses at the end of the year, so I decided I might as well get my one-year mandatory military service out of the way and continue my correspondence courses while in the army. The following May, in 1965, I took the exams in Sortland, the location of the nearest high school with business concentration, during a two-week leave from the army and did well enough to be admitted to NHH starting that August. Four years later, I was off to the United States.  Doctoral student Liv and I arrived in the United States in July 1969, and I still remember vividly when Sten Thore first took me to the Graduate School of Industrial Administration (GSIA) at Carnegie-Mellon University. We entered the building through the back entrance and immediately, on the back steps, met two professors to whom Sten introduced me. One was [Herb Simon](https://www.nobelprize.org/prizes/economic-sciences/1978/simon/facts/).  My formal status at GSIA was visiting student. Although I was there to work half time for Sten, I still signed up for three core economics courses: macroeconomics by Martin Bronfenbrenner, econometrics by Marty Geisel, and general equilibrium theory by John Ledyard (intended for second-year students, but my mathematics elective at NHH, along with subsequent math courses there, made it eminently manageable for me). Moreover, I decided, probably with the encouragement of Sten, to take linear programming from Egon Balas. About a month into the semester (while still doing a moderate amount of work for Sten), I came to the realization that to make it in research I needed a doctoral degree. I applied, and was promptly accepted in the doctoral program.  In December, Sten and his family returned to Norway and we were on our own, although I was still supported financially by NHH. Nonlinear programming by Egon Balas was a useful course for me. So were statistical decision theory taught by Morris DeGroot and microeconomics by Mort Kamien. But the most unusual course that first spring was economic fluctuations by [Bob Lucas](https://www.nobelprize.org/prizes/economic-sciences/1995/lucas/facts/). He started out with basic mathematics, such as Kuhn-Tucker theory and functional equations, interspersed with economic applications. One day, sometime after the midpoint, Bob started setting up a model. In the following class he told us to scrap everything he had said the last time. He started over again, making a simplifying assumption or two, and then, over the course of the next couple of lectures, took the analysis to its conclusion. Later, we realized we had seen his paper “Expectations and the Neutrality of Money,” for which he was later to get the Nobel Prize, being developed right there in front of our eyes.  GSIA was (and is) unusual in at least two ways. One was the small class size, which promoted a co-operative environment among the students. Also, there was relatively little course work. Most of the material taught was foundational, with emphasis on tools to put the student right on the research frontier. Important requirements were the first- and second-year summer research papers. My first-year paper was entitled “Duality in Fractional Programming,” a topic that came to me partly because of the project for the shipbuilder in Bergen, partly because of all the mathematical programming I was taking. The paper ended up being my first serious publication, in *Naval Research Logistics Quarterly*, which my professors told me was ranked third in the operations research field. My main advisor was Bob Kaplan, later to become the dean of GSIA, whose specialty was dynamic programming (but who years later moved into accounting).  My second-year summer paper also involved dual prices, this time in hierarchical linear programs, and it was published in *Management Science*. By that time, I had become interested in an economic topic that went under the name of “the assignment problem.” It had generated a substantial literature. The idea was, within the system-of-equations framework dominating macroeconomics at the time, that fiscal policy was more effective at achieving certain goals and monetary policy was effective at others. If the right instruments were assigned to the right targets, the economy would function quite well, while if the wrong assignment was chosen, the economy would function poorly and could, in the worst case, even be unstable. My idea was to think of the monetary and fiscal policymakers as separate decision makers with different goals, such that all the target variables would enter each objective function, but with different relative weights. Thus, a dynamic game resulted. Moreover, because of the differences in policymaking process for the alternative instruments, it made sense to me to think of the fiscal policy maker as dominant in the sense that he went first in every period, with the monetary policy maker a follower. This set-up represented an alternative to the symmetric noncooperative ([Nash](https://www.nobelprize.org/prizes/economic-sciences/1994/nash/facts/)) solution.  In August 1971, I happened to run into a new professor who temporarily had been placed in a windowless office (presumably due to his arrival well before the start of classes), which later served for years as the mail room. He introduced himself to me as Ed Prescott and asked what I was working on. Evidently, he liked what I told him, and showed me some game-theoretic research he had done in an oligopoly context.  By the following spring, I had taken a course from Dave Cass who, like Ed Prescott, had arrived in GSIA in time for the 1971-72 academic year. I had had some conversations with him, and among other things told him about one of my findings, that even in the symmetric noncooperative case, the outcome was different when the solution was regarded as a policy rule in a recursive way as opposed to a *sequence* of decisions for the entire future. Dave asked me to prove it, as this finding went counter to the well-known property in single-player control theory that the solutions in what can be called policy space, on the one hand, and sequence space, on the other, give identical outcomes. Of course, once Dave saw the proof, he bought it right away. Moreover, I argued that the solution in policy space represented a more reasonable equilibrium from an economic standpoint.  That spring, I presented my thesis proposal to the faculty. The main element, motivated primarily by my version of the assignment problem, represented an application of dynamic game theory. Immediately after the faculty’s deliberations, Bob Kaplan, who had been my main advisor up to that point, came to my PhD carrel with the outcome. In addition to informing me that I had passed, he told me that Ed Prescott had insisted on becoming the chairman of my committee. All along I had thought it would be Dave Cass (who still was to be a member of the committee, as was Kaplan). Thus started in earnest years of productive and much-appreciated interaction with Prescott.  The 1977 paper … After four years of PhD work, I defended my dissertation in time to graduate in May 1973. It was time to return to Bergen. While Cass and Prescott had both suggested I would do well on the U.S. job market, I felt obliged to take a position offered me by NHH. They had provided full financial aid for my studies. But one thing remained to be done. In April, around the time the final draft of my thesis was turned in, Ed had shown me a new paper by Bob Lucas. Its title (at least in its final draft) was “Econometric Policy Evaluation: A Critique.” Ed had given it to me to consider whether one of the models Bob had used as examples of his critique could be banged into my dominant player framework, with a government objective added to Bob’s general set-up. In their *Econometrica* article on “Investment under Uncertainty” two years earlier, Bob and Ed had shown, using a dynamic industry model with capital accumulation, that the competitive equilibrium could be obtained quite simply by solving a particular stand-in consumer-surplus problem. So we thought why not do something similar, but then add that the industry was affected by cyclical investment-tax-credit policy? We submitted an abstract to the June Stochastic Economics and Control Conference, an annual conference that had recently started up at the initiative, I believe, of David Kendrick and Gregory Chow, and then we worked like crazy to get it in shape.  So while Liv and our one-year-old son Martin left for Norway at the end of May as planned, I stayed behind for an extra month to work on the paper and to attend the conference. I was pleased (as was Ed, I’m sure) with the attention the paper got.  The first year at NHH was frustrating as it was clear that modern macroeconomics was not the school’s strength. Soon I came up with the idea of inviting Ed to spend a year at NHH. He seemed interested, and I set the wheels in motion to drum up financial support for his stay. I succeeded, and Ed and his family showed up in time for the 1974-75 academic year. By that time, two significant things had happened. In my thesis, although describing three different solutions to the dominant-player game – the recursive without commitment, the commitment solution in policy space, and the commitment solution in sequence space – I had calculated examples of only the first, in part because that’s the one I argued was *the* candidate for an equilibrium. Also, the other two were much harder to calculate. During my first semester of teaching, I had identified an exceptional undergraduate, Nina Bjerkedal, whom I encouraged to become my research assistant. I gave her the task of writing a FORTRAN program to calculate the profits of a dominant firm on the assumption that it could commit to its optimal policy. It turned out these profits beat those of the otherwise time-consistent outcome by an astounding margin. Moreover, when Ed arrived, he had clearly warmed to the idea that the focus of our paper had to be a comparison with and without commitment. We expected the result that the time-consistent solution could represent quite an undesirable outcome.  Our progress initially was slow because Ed was busy with other matters and I was in the middle of getting out three papers based on my dissertation. One contained a description of the solutions that I argued, from an economic standpoint, represented dynamic equilibrium outcomes in symmetric noncooperative games and in dominant-player games (*International Economic Review* 1975), one was on the assignment problem (*Annals of Economic and Social Measurement* 1976), and one focused on dynamic dominant-player games (*Journal of Economic Theory* 1977).  When Ed and I finally got going on our paper in the spring, we first worried about two key issues. Our intuition was that the difference between the two solutions was greater when a lot of inherent dynamics was present in the model. Sticking to our stochastic model of capital accumulation in the face of government stabilization policy using the investment tax credit, we introduced “time-to-build” into the model *in combination with* the standard cost of adjustment. Secondly, we had realized, after we wrote our 1973 paper, that we needed to make sure the rest of the economy was treated explicitly as being inhabited by atomistic agents (not treated as one player). Ultimately, the appendix dealt with how to solve that issue (the “big K-little k” problem).  I had submitted my assignment-problem paper to a stochastic control conference to take place in Boston in May. At some point early in the conference, Gregory Chow announced a session for work in progress. I signed up to talk about Ed’s and my paper, and was told I could go first. All hell broke loose. Everyone was trying to locate the error. Admittedly, we had chosen a rather provocative title for our first draft: “On The Inapplicability of Optimal Control for Policy Making.” I was certain nothing was wrong. With all my experience in dynamic dominant-player games, I knew time inconsistency had to be an issue. I suppose at that point, after what happened at that presentation, I realized our findings could generate considerable attention. Moreover, as a consequence of the difficulty people had in understanding the time inconsistency, we decided to add, for expository reasons, a Phillips-curve example to our investment-tax-credit example before we resubmitted our revised version of the paper to the *Journal of Political Economy*. As I recall, it was motivated by a model in a recent paper by Phelps and Taylor. Of course, that example has turned out to be used a lot by subsequent writers.  With the rules-vs-discretion paper pretty much done, I dabbled in industrial organization for a while, especially pushing further the dominant-firm model from my dissertation. I had gone on leave for the academic year 1976-77 to the University of Minnesota (never to return, as it turned out, to Norway for any permanent position). While in Minneapolis, I was invited to visit Carnegie-Mellon University the following year. During that year, I was offered an associate-professor position, which I accepted.  … and the 1982 paper By that time, Prescott and I had started to work on business cycles. Some of the computer programs used in our 1977 article could be adapted quite easily to calculating dynamic equilibriums of business-cycle models. In the beginning, we considered models in which we made the technology linear and the representative household’s utility function quadratic. We included technology shocks, but at first, in large part because of Lucas’s 1972 article, we didn’t think we could do without monetary shocks. For an NBER conference in 1978, we wrote a paper that was somewhat schizophrenic. It contained a business cycle model, but also evaluated stabilization policy. The main idea behind the latter was that changes in taxes were costly as a way to balance the government budget over the cycle. Instead the “slack” should be picked up by fluctuations in government debt. In the end, we were asked to reduce the length of the paper for the resulting conference volume published by the NBER in 1980, and we had to leave out much of that material.  Instead, we wrote another paper on policy – a standard growth model with labor and capital taxes financing government purchases – and made the point that capital taxes should be low to maximize the representative consumer’s welfare. At the same time, lack of commitment was likely to lead to capital taxes that were much higher than optimal. That paper, published in the 1980 *Journal of Economic Dynamics and Control*, also dispelled a common misunderstanding at the time, namely that time inconsistency originates from the policy maker’s objective being different from those of the people. Our model economy is inhabited by millions of people who are all alike, and the government is assumed to have preferences that coincide with those of the representative household. Finally, the paper presents a way of determining the optimal policy (with commitment) through the use of a particular shadow price as a state variable. Variants of this basic method are still being used in many contexts.  A key breakthrough in Ed’s and my continuing business cycle research was the realization that we could start with the basic growth model, with both production and utility functions in exponential forms and, to the extent necessary, approximate in order to make the computations tractable. Suddenly, the number of parameters was much smaller and their calibration transparent. For example, first-order conditions for the steady state implied relations between the parameters and steady-state values of the model aggregates, the latter with counterparts in corresponding average values in the data. With those averages quantified from the data, one could then map, through those first-order conditions, to the parameter values that would reproduce the steady-state values corresponding to the data. This procedure is called calibration.  The paper published in the 1980 NBER conference volume (titled *Rational Expectations and Economic Policy* and edited by Stanley Fischer) was our first real-business-cycle paper. In the summer of 1979, we had written the first draft of another paper, later to be published by *Econometrica* in 1982, in which we put our vastly improved calibration procedure to use. My first presentation of it took place at Cornell University in the early autumn of 1980 (in a job talk; evidently, the paper did not impress them, as I was told later that no offer would be forthcoming).  Loose ends In our first two business-cycle papers Ed and I were surprised to find that including a role for money made very little difference to the model outcomes. Virtually all the “action” came from technology shocks and, moreover, they accounted surprisingly well for the data. In 1980, I wrote a first draft of a paper in which I focused on the role of money for the business cycle. I used two separate propagation mechanisms, one based on imperfect information in the spirit of Lucas’s paper on “Expectations and the Neutrality of Money,” and one in which there was a household trade-off between time and holdings of money. In the latter, the cyclical movement of the price level was in the right direction and of considerable magnitude, even when the central bank let the money stock grow steadily without fluctuations. The most interesting finding of the first part of the paper was that, while price shocks had the effect, as in Lucas 1972, of making people act in part as if these nominal shocks represented changes in real prices (because of the imperfect information), they also led people to react less to everything, including the real shocks, and the latter effect turned out to dominate, so that, for the calibrated economy, the larger is the variance of the price shocks resulting from central-bank behavior, the lower the business-cycle volatility. (Of course, this reduction in volatility is by no means welfare improving!)  While I gave this paper several times, and it was once on the program of the winter meetings of the Econometric Society, it continued to remain on the back burner in the sense that there always seemed to be projects with higher priority. In the end, I got the paper in shape to submit it to the *Journal of Monetary Economics* and got a favorable revise and resubmit. But then, in response to an invitation to a conference on the 100th anniversary of [Ragnar Frisch](https://www.nobelprize.org/prizes/economic-sciences/1969/frisch/facts/)‘s birth (whom I admire greatly and regard as having been well ahead of his time in the 1930s), I decided the paper fit well for that purpose and elected to use it there. The proceedings were to be published in a special volume of the *Econometric Society*. Unfortunately, the referee criticized the paper on issues related to the by then standard approximations (in my opinion a completely peripheral and inconsequential issue relative to the question addressed), and the editor, along with the conference organizer at the University of Oslo went along with the referee. At that point, three or four years after the *JME* referee report, I figured it was too late to resubmit it to them, so the paper remains unpublished (but often cited).  Much of my research immediately following upon the 1982 *Econometrica* article revolved around the labor market. The main anomaly relative to Ed’s and my business-cycle model was the high cyclical hours-of-work volatility in U.S. data. We were convinced that a large part of the discrepancy was attributable to the simplicity of the abstraction. All model people were assumed to be alike. Literally speaking, all of the model’s labor-input variation is in hours per worker, while, empirically, much more of it is in the form of changes in the number of workers (employment). This issue was dealt with beautifully by Gary Hansen in his 1985 *JPE* paper.  Another aspect of reality, not shared by our model, is the workforce’s great variety of skills for market production. When Allan Meltzer called me sometime in the autumn of 1982 (while I was visiting the Hoover Institution for a year as a National Fellow) and asked me to write a paper for the 10th anniversary Carnegie-Rochester Conference to be held a year later, suggesting that the organizers would like a paper on the importance of contract theory for the business cycle, I was so convinced of the much greater importance of heterogeneity of workers’ skills that I decided, without consulting Allan, that that was the labor-market topic I wanted to write about. So in the paper, entitled “Labor-Force Heterogeneity and the Business Cycle,” I document with the help of data from the Panel Study of Income Dynamics (PSID) the vast differences in cyclical hours behavior depending on skills, and then construct a model with two skills categories, calibrated to what was known about the workforce if it is divided in two, according to skills. One implication of the model indeed turns out to be substantially greater work-hour variability than in the basic equal-skills model.  Part of the problem was that, given the form of the 1982 model, comparing its labor-input implications with aggregate hours of work could be quite misleading. A better measure would quality-adjust the hours of each worker before adding them up. That’s what Ed Prescott and I did in a study a few years later. Based on data from the PSID, we found, for that sample, that the volatility of unweighted total hours was 40 percent greater than that of a weighted measure. This paper was published in 1993 in the Federal Reserve Bank of Cleveland’s *Economic Review*.  Finally, as it was clear while working on our 1982 paper that greater intertemporal substitution of labor was a way to produce more labor-input volatility in the model, as an alternative to our standard utility function we had also introduced one that was nonseparable over time in leisure. As later shown in my above-mentioned labor-force heterogeneity paper, this utility function can be regarded as a stand-in for a particular household-production formulation. Still, we were somewhat in the dark as to what were reasonable values of the two additional parameters characterizing the nonseparability. In a subsequent study with Joe Hotz and Guilherme Sedlacek, published in *Econometrica* 1988, those parameters were estimated based on data from the PSID.  A prevalent misunderstanding in the early 1980s was that Ed and I had put forward our 1982 model as a way to “fit” the data. As this misunderstanding was still evident from the general discussion following the presentation of my 1984 paper at the Carnegie-Rochester Conference session, I took the opportunity, in a rejoinder to the discussant’s comments, to state as precisely as I could what the question had been: If technology shocks were the only source of impulse to post-war U.S. business cycles, what portion of the cycle would remain? Our answer, based on that model, was over 50 percent. Importantly, this measurement was based on a calibrated model that was explicit about the dynamic decision problems faced by the model’s people and businesses.  My co-authors Throughout my academic careeer, I’ve had the great fortune to work with so many eminent researchers. I’ve already mentioned Hotz and Sedlacek. Prescott and I continued to do joint work off and on over the next decade. One focus was on variable capital utilization in the form of variation in the hours the capital is used. This general theme is reflected in our 1988 *Journal of Monetary Economics* paper and in our 1991 *Economic Theory* paper. As our business-cycle methodology received greater acceptance in the profession, we were also asked for methodological or expository contributions, which resulted, in particular, in our papers in the *Scandinavian Journal of Economics* 1991 and *Journal of Economic Perspectives* 1996.  The area of international macroeconomics has been ripe with puzzles and anomalies. An example is the fact that, between pairs of major countries, the cyclical consumption correlations are about the same as the corresponding output correlations. Another seeming anomaly is that, cyclically, the trade balance is the worst (that is, more negative) when one’s goods are the cheapest. Beginning with a one-year stay at the Federal Reserve Bank of Minneapolis in 1989, David Backus, Patrick Kehoe, and I wrote several papers on the general subject of international real business cycles. Indeed, our first paper, in the *Journal of Political Economy* 1992, is entitled exactly that. We find, among other things, that the “consumption anomaly” mentioned above is quite robust, even under serious impediments to trade. Another paper was published in the *American Economic Review* 1994. In this paper, we find that the seeming anomaly involving the trade balance and the terms of trade is precisely what a calibrated model says should happen.  In the early 1990s, Bill Gavin encouraged me to become a research associate at the Federal Reserve Bank of Cleveland, which entailed occasional visits there. Slowly but surely we started to work on questions having to do with the role of money for the business cycle. When, after some time, Bill moved to the Federal Reserve Bank of St. Louis, he talked me into continuing to visit him there. This working relationship has resulted in several papers. One in the *Review of Economic Dynamics* 1999, for example, documents differences in postwar co-movements of monetary aggregates with real GDP before and after 1980. The price level is still countercyclical in both subperiods, and the co-movements of various real aggregates with real GDP are very similar. Then we go on to show that a monetary model with fully flexible prices, but with differences in policy regimes, can account for the different episodes. It is well known in policy circles that an important change in monetary policy did indeed take place during and after the Volcker era at the central bank. The subsequent work with Bill has, to some extent, been joint with others at the St. Louis Fed, in particular Rob Dittmar and Mike Pakko.  After Gavin had left the Cleveland Fed, I was still encouraged to continue visiting there for a couple of weeks a year. Over time, I started to work with Peter Rupert, first on measuring quality-weighted labor input based on Current Population Survey data, which are monthly, more up-to-date, and cover many more workers than does the PSID. After a while, we, along with Paul Gomme, also became interested in a major anomaly in the literature on the interaction of household and business activity, a literature that had sprung up in the early 1990s. The anomaly had to do with the degree of contemporaneousness of cyclical movements in household and business investment, with the former leading the latter. Our most significant paper so far made headway on that anomaly and was published in the *Journal of Political Economy* 2001.  In the mid 1990s, I was hired by the University of Texas, supposedly to help them build up the macro group. Unfortunately, while everyone in my family enjoyed Austin very much, in the end the administration’s concept of keeping a promise was quite disappointing, and we unhappily left. But that stay had two longer-run consequences. Scott Freeman and I had talked about writing a paper about the interaction of money and output over the business cycle, using a novel model with both inside and outside money, and we finally completed it after I had left Texas. The paper ended up in the *American Economic Review* 2000. Subsequently, Scott and I wrote a follow-up paper for a conference at the Federal Reserve Bank of Cleveland, this time jointly also with Espen Henriksen, then a GSIA PhD student.  While in Austin, I was signed on as a research associate at the Federal Reserve Bank of Dallas, a relationship that has continued to this day. While there, I have done research with D’Ann Petersen, Mark Wynne, and Carlos Zarazaga. The work with D’Ann represented the beginnings of a labor-input measurement project. Mark and I (along with Alan Ahearne, a former student of mine at Carnegie Mellon, now at the Federal Reserve Board) decided to investigate the reasons for Ireland’s spectacular growth after 1990. Carlos and I embarked on a study of a much less successful nation – Argentina – and wrote a paper on its great depression in the 1980s, published in the *Review of Economic Dynamics* 2002.  Subsequently, Carlos and I decided to plug into our model – a standard version of the neoclassical growth model – the numbers for the period after 1990. To our great surprise, the model predicted that, while output from 1990 to 1998 had grown at a rapid pace by most standards, GDP and especially the capital stock should have grown much faster in light of the productivity growth that had taken place. This is a curious finding. Why should it be so? Did Argentina suffer from the “time-inconsistency disease” to such an extent that, even with the currency board instituted by former President Menem in 1990, the nation’s credibility among investors still was much too low? This research highlights the dire situation in which Argentina found itself after it experienced a second and much faster-progressing great depression after 1998. By 2003, measured capital per working-age person had fallen an astounding 20 percent relative to 1982, with pessimistic implications for the average real wage as well as for the income distribution.  In the early 1990s, Michael Bordo came to visit Carnegie Mellon for an extended period. Our interaction during his stay resulted in two papers in which we interpret the history of the gold standard in light of the time inconsistency of government policy and the need for a commitment mechanism to maintain credibility.  Dynamic macroeconomics, with the explicit analysis it entails of the decision problems of households and businesses, is not easy to teach and learn at the basic level. My impression over the past couple of decades is that the gulf between the research frontier and what’s in undergraduate textbooks has grown wider and wider. So ten years ago, I started to use the computational experiment as a teaching tool in part of my semester-long course in intermediate macroeconomics. At first, the students would simply use the executable version of a FORTRAN program, with its rather crude input and output formats. A few years ago, during my annual summer visit to Bergen and NHH as part of my adjunct professor position, I was able to interest Solveig Bjørnestad of the Institute for Information Science at the University of Bergen in the idea that she and her students, in co-operation with me, could design and develop a learning environment that would be user friendly, perhaps even fun, for the students, with the goal that it would enhance dramatically their understanding of dynamic macro. This co-operative effort still continues. A detailed description of the approach and of the contents so far is written up in our joint 2004 paper.  During all of this, I have helped to raise four children: Marty, Eirik, Camilla, and Kari. At times I’ve wondered if academic life took away too much time, which I otherwise could have spent with them. Some years ago, Liv and I parted ways. Liv claims that the extent of my travels took its toll. She has since remarried.  Almost two years ago, I had the incredible luck to stumble upon Tonya Schooler (now Kydland), a wonderful person, and we have been inseparable ever since. The complications of three academic careers (including her ex-husband’s) combined with co-raising her school-age children, Joel and Rachel, took us, in August 2004, to the beautiful city of Vancouver. I accepted the Jeffrey Henley Chair in Economics at the University of California at Santa Barbara. Admittedly, Santa Barbara is still a long commute from Vancouver, but one in which Tonya has been happy to share. In the spring of 2004, the University of British Columbia had made me a nice offer, but their rule of mandatory retirement at 65, from which their provost refused to make an exception for me, was inconsistent with my desire to remain an academic and continue my research until I drop. I believe my co-researchers are happy, I’m slowly but surely finding more time to get back into what I love to do. In my case, one of the benefits resulting from the Prize is the availability of funding to set up a major research institute at UCSB. Among other things, it will bring researchers from all over the world together for periods of time to brainstorm on important outstanding questions and anomalies in macroeconomics and finance. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0841 |
| Interview |  |
| Q1 | So how did all start? Why did you decide to become economists? Was it just a coincidence or was it something that you had planned for a long time? |
|  | I certainly didn’t plan for it when I was almost through with high school, I had done pretty well in mathematics and I thought I would become an engineer. But then through various coincidences I ended up studying at the business school, *Norges Handelshøyskole,* and still then I didn’t think I would become an economist. I assumed this was an education that would lead up to a management position somewhere, maybe become a director of some company. And I just happened to take an advanced seminar from a very exciting professor, Sten Thore, exciting and excitable. He had us read very interesting and advanced papers in management science, operations research and I wrote my first FORTRAN programme using dynamic programming.  Then just before I was done, he came and asked me to become his research assistant and I followed him to Carnegie Mellon where he went on leave for a year and decided I had to get a piece too to make it in this field. So I never applied anywhere other than Carnegie Mellon. And then that was another great fortune because Carnegie Mellon has such a great research training programme, rather than doing all kinds of courses they get you started on doing papers right away. And so I did a first year and second year research paper and got them both published, and that’s a great start.  Certainly. And you Professor Prescott, how did you start?  Edward C Prescott: Tend to be drifting in, into that field. When I went to college it was the Sputnik era, so everybody wanted to go into physics. I was in that programme at Swarthmore College and was in the honours programme in my junior year and I decided I didn’t like laboratories. I didn’t like sitting in there all day being very careful and meticulous. And so I said, Well, I’ll major in math. I did have a very good special teacher, one course and sort of engineering economics or engineering science, tied to management, Sam Carpenter.  And so I went to get a degree in operations research, which was a new field of applying mathematical tools and modelling tools to management problems. And then I went to Carnegie Mellon, it was Carnegie Tech then. It’s a multi-disciplinary programme, a minimal number of course requirements. I don’t know if they had any course requirements, but you had to take a certain number of exams in different areas. But then I got attracted to the people in Economics because it seemed to be where the action was. [Bob Lucas](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1995/lucas-facts.html) came as an assistant professor the same year I came as a graduate student.  It must’ve been a very interesting period there at Carnegie Mellon at the end of the 60’s.  Edward C Prescott: Yes. Actually I was thinking about going into artificial intelligence because that was an exciting field there, too. And took some of Alan Newell and Herb Simon’s courses in that field. So then I ended up in economics. |
| Q8 | I know of course that you were Finn’s advisor, but I’m really very curious about this time inconsistency, or consistency result, however you like to put it. How did you start with this in the first place? I know you produced the thing in Bergen in 1975, but I guess you had some idea, why you just picked this problem and you got together in Bergen in ‘75? |
|  | There was a change. We recognised that thinking in terms of using these old system of equation models, waiting to say What should you do now? was not a question in the language of economics. But you could evaluate rules. So the obvious question is what’s the best rule? And so we set out to find that. And the finding was that there was no best rule because you’re dealing with people who anticipate and think. Initially we were a little disappointed because we didn’t find what we were searching for, but then we thought, this is a neat result.  Do you have any comment on that?  Finn E Kydland: And then I had already encountered the time inconsistency issue in my thesis. I had looked around for a thesis topic, at that time the people were worried about something called the assignment problem and it had to do with does monetary policy and fiscal policy, what do they target? But there seemed to be no coherence to that literature in my opinion and so I decided to formulate the problem as a dynamic game between monetary and fiscal policy makers, because of the way decisions are made it seemed that the fiscal policy maker would be, what I would call, the dumber player. And right away it was a model in which the optimal policy for the fiscal policy maker was time inconsistent. In the long run I didn’t regard that particular application as that interesting. I dabbled around also with oligopoly theory with a dominant player, there’s literature on dominant player industries. And again the issue was very much prominent. The dominant player’s optimal plan was not time consistent.  But it became a much more interesting issue once we thought about having the government as a whole as the dominant player, sort of playing against the rest of the economy. And there were followers. It was not without challenges because once you do so and you want to do it right, you want to make sure that you don’t treat the rest of the economy as one player, they’ve got to be viewed as atomistic, so we struggled for a little bit with that issue, but it became a methodology that was very useful both for that particular paper and what was to come later. |
| Q6 | When you submitted the paper did you then, at that time, know that this is really a discovery? |
|  | Finn E Kydland: I was pretty sure it was an interesting discovery. We had written the first draft of the paper. I’d gone back to Norway and I arranged for Ed to come and visit for a year and we wrote that paper. And then I decided to talk about it at a conference in Cambridge, Massachusetts, in May of 1975. And this was a conference with quite prominent people in the field of economics and control theory, which was quite popular in those days. It’s true the title was kind of inflammatory, initially it was something about the inapplicability of control theory to policy making. So maybe that set them off. But anyway, everyone in the audience thought the result was wrong and a huge discussion ensued and I of course knew I was correct. But the fact that problem people such as Dan Fisher, Gregory Chow and others, David Kendrick, thought it was wrong, that just made it clear this had to be pretty big. |
| Q18 | How important do you think that your time consistency result has been for the subsequent central bank reforms? |
|  | Edward C Prescott: Not sure, but they always use it as justification. And if we help in a little way bringing about these reforms, we’re proud of what we’ve contributed. They have moved in that direction and really what the people say, including the head of the Bank of England for example and /- – -/, at the Central Bank here in Sweden, I never heard Greenspan say anything specific about it but many people on the board of governors articulate these views strongly and push in this direction of a good rule and along with the independents, then you can follow it.  Do you want to add anything?  Finn E Kydland: I have done a little travelling in South America and to me, I’ve seen the contrast between countries in which they haven’t worried too much yet about following your rule or trying to make policy credible, and one sees how much a nation can be hurt by not doing so. And so that’s very depressing. Now exactly what to do about the ones you have lost your credibility, it’s not so easy. But at the same time it provides a contrast where I guess we are quite convinced that in nations where they use transparent polices where one has a good idea about what’s going to happen in the next 5 or 10 years, those nations will be much better off. And if this theory has served to lay the foundation for making them convinced of that, that’s rather better. |
| Q73 | The second part of your prize concerns business cycle theory. How do you think that this branch of economics has developed since your pioneering article? |
|  | Edward C Prescott: I think in the pioneering article we developed a methodology. I’d look at what was done just a couple of years before, a statement from, for example, Lucas and Sergeant, who were attuned to the modern dynamic economic theory with the rational expectations. Just bringing that macro with the growth things just clicked and in the way we sort of matched things up with the national accounts and the growth facts and looking at the same set of statistics. And in the process a huge amount has been learned and there’s a multitude of studies within the framework.  We happened to focus jointly on the consequences of total factor productivity shocks, but Finn and others have focused on monetary factors, bringing them into that same analytic framework using it in the same tools and techniques. And in the process there’s discipline, theory tells you the answer. You have guesses, you have conjectures, and sometimes they’re right, sometimes they’re wrong. You don’t know until you quantitatively, it’s a big word, quantitatively, work things out. And we developed a way to do that. I think that was a key contribution.  Finn E Kydland: Yes, initially of course the theory was relatively simple, although we put in a few bells and whistles that we thought could be important for other business cycle questions we were studying. Since then economists have made great progress in expanding the set of questions you can ask. For example we made the assumption for attract ability and it turned out there was an assumption that didn’t matter for the answer that everyone is alike and is immortal.  Since then computing capability has expanded, knowledge of theories, the theoretical framework has been expanded so that it’s easy to bring to bear what we know about life cycle behaviour, for questions where we know that’s important. And they seem to go about these studies and using the same methodology. It’s just that the framework has been expanded. |
| Q3 | Swedes have a problem now, we have only two Laureates in Economics. We have three in Norway in now and well, today we can’t do very much about that. But I would like to know, did you know [Frisch](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1969/frisch-facts.html) or [Haavelmo](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1989/haavelmo-facts.html), have they inspired you or have you heard them lecture? |
|  | Finn E Kydland: I once heard Frisch lecture. When I was an undergraduate at the Norwegian School of Economics and Business Administration, Frisch came to give a guest lecture and I and a lot of the undergraduates went to hear him. The auditorium A, as it was called, was filled to capacity and I still remember Frisch coming in, in a dark blue suit and jogging shoes. And he sounded very excited about what he did. But I have to admit that at that time I didn’t know much about what he was famous for.  Since then I have read some of his works, I was especially impressed by his paper in the castle volume. It’s a beautiful precursor of what was to come and I suspect that people had followed his lead more than some of the others in the ‘30’s. The development of macroeconomics would not have taken as long as it did. And also, of course, Frisch was instrumental in starting the Econometric Society and he was the editor for 25 years, and that became probably the most prominent society in Economics. Haavelmo, I didn’t know quite as well and I have to admit that what he got the prize for is sort of further removed from what I do. Although I have written other things by him, his work on development theory and investment theory is quite impressive. But I don’t believe I ever met him. But I would like to point out that I think Norwegians are proud of the per capita number of Nobel Prizes in Economics. We now have one per 1,5 million people and that would be hard to beat.  Edward C Prescott: I thought Frisch was the great one. He had the vision, making economics quantitative. I guess it was neoclassic economics quantitative. Back then there were different schools of thought in economics. Now there’s only one. And when they started the Econometric Journal for 25 years that was really the only scientific journal in economics and the leadership he provided. But I don’t think his vision was really realised until after that time the bill paper, then we can start doing the things he wanted to be able to do, really disciplined, providing these quantitative answers to public policy questions. He was very frustrated in the ‘60‘s and he would talk that as much progress was not being made in providing enough discipline and he referred to player metrics, or that term. But that’s just out of frustration that he wanted economists to be able to do so much more than they were able to do then. But now we can. |
| Q2 | So when do you think the first woman will receive the prize in memory of Alfred Nobel? |
|  | Edward C Prescott: Could be any year. I really can’t say who my candidate is. I can think of a couple of others in the pipeline that are 10 or 15 years away. But it could be anytime. There’s one that could’ve gotten it, but then she became a little bit extreme Maoist, but she had done some important economic research on the monopolists in competition many years before, Joan Robinson. I suspect if she didn’t have the, I think the Nobel Prize committee may have been a little bit nervous about the political thing, not the gender thing. They would love to.  Finn E Kydland: It is gratifying to see so many women coming into the field and in some nations more than others. When I’ve been to Spain, Italy and so on, it seems that the proportion has gone up to about 50%. And one has to remember that what you tend to get the prize for still is work you did 20 or 25 years ago, as was the case for us. And so if this is a recent trend then that by itself will delay the process. But it’s going to happen pretty soon. |
| Q25 | So what kind of research do you do today? Has your interest shifted over the years? |
|  | Edward C Prescott: Methodology, no. Topic, yes. One thing I was quite excited about is this research with Ellen McGrattan on the stock market valuation. The business cycle model says what the value of the stock market should be and, you know, I had to a few things, put in a corporates sector, you have to build in the tax system, because it turns out to be important and also a regulatory constraint. And the theory just does spectacularly well. But that sort of shifted the more interest in the bigger movements. Prosperities and depressions, these large movements in relative levels. Then I’ll be coming back to this problem about why isn’t the whole world rich type things?  And it seems to be barriers to riches. All the groups of people, no matter what nation they live in, if they can set up a good system, they’re rich. They become rich very quickly. But it’s related to the time inconsistency, it’s easier said than done, in what sort of institutions that might be set up that mitigates the creations of these barriers to efficient production. If you look at across countries, you can determine living standards just by knowing how much output is produced per hour worked. The amount of hours worked per person doesn’t vary that much, but the amount produced per hour can vary by a factor of 25 between the rich and the poor. And the relative productivity can change pretty fast, as we know from the growth miracles in Asia. And there were some growth miracles in Europe too after the World War II. The Italians, the German recovery.  Finn E Kydland: I might mention two things. One is there has been this belief that monetary policy affects the real economy and I was pondering how can it be there are so many people believe that out there and that it’s so hard to find a good propagation mechanism as Frisch would have called it. And so I’ve been searching to see if a few factors could be quantitatively important. One is working through the interaction with the fiscal tax system, for example if they’re non-indexed, which was the case in the US in the ‘60’s and the ‘70’s especially, but then the tax system changed somewhat in the last 20 years. And low and behold, it turned out that you can get some quantitative effects before the early ‘80’s.  It’s interesting that the investment in durables usually bought with loans, the cyclical behaviour is different depending on whether the nation has for some reason or other fixed normal interest rate loans as opposed to flexible interest rate loans. And that may lead one to wonder whether monetary policy or monetary shocks may have had an effect. But in the vein of what Ed talked about, what I found, Ed has studied the world as a whole and compared wealth and incomes all over the world. For various reasons I got interested in particular countries, and so I already mentioned Argentina which I have studied intensively and it’s fascinating but also depressing to study a country that used to be one of the richest nations in the world, and now has a population that is by usual classifications, it contains 40 or 50% who are poor. And why is that and what can be learnt for other nations? Then we have the contrast with Ireland, which used to be poor, and now has become one of the richest nations in the world. So I think one can learn by choosing particular countries to study. |
| Q13 | You have worked a little against the mainstream. You talked about this conference in Cambridge. And they thought it was wrong and they got irritated by your business cycle results. But would you give a person advice to really lean against the mainstream or would you give them any other advice? |
|  | Finn E Kydland: Yes certainly. You’ve got to do what you believe is going to lead to scientific progress. And we believed strongly that this was the way to go. But if the framework already is a reasonable one for the questions you want to address, there’s not much point in trying to lean against it just for the purpose of leaning.  Edward C Prescott: We listen to criticism and if some unfortunate criticism comes up, we fess up to it. |
| Q4 | I’ve heard that you are a big football enthusiast, so if you had to choose Norway winning the world championships or you winning the Nobel Prize, what will you choose? |
|  | Finn E Kydland: That question is too easy. I wish I would choose the Nobel Prize, but a Norwegian journalist just after the prize was announced asked a harder question, if you could choose to be who you are and win the Nobel Prize, or be a world famous soccer player? Then, and that’s a much harder question (laugh). So if I could be Martin Palermo or Tore Andre Flo or someone like that, that will make it hard (laugh). |
| Q28 | I guess that you, Professor Prescott, have been interested in soccer? |
|  | Edward C Prescott: Yes I got involved in soccer. Soccer was not played much when I was younger, but it’s become a big time sport in the US. I guess I got drafted into that organisation, to coach a team at something and stuck with them. They’re just a great group of kids. I used to make some of my graduate students help me referee or coach, we paid them. I made my sons help out the coach. It’s hard when you want to get the people to do this and the team that I manage was just the greatest group of kids and really talented athletic-wise. And so they can play with the best clubs in the world, including ones from Sweden, and Norway. We hosted a team from Norway, they have the biggest youth tournament over there in Minneapolis in the world. Sweden has big ones too I understand. |
| ID | 0842 |
| Biographical | I was born on December 26, 1940, in Glens Falls, New York, to Mathilde Helwig Prescott and William Clyde Prescott. My mother’s parents were German immigrants. My mother was forced to drop out of high school at age 16 to care for her younger brother and sister after the death of their mother. Later, she managed to complete high school, and with the help of a family friend (whose name I wish I could remember) she continued with school and became a librarian. My mother’s ambition and fortitude, even in hard times, were exceeded only by her generosity and kindness. My father told me it was she who insisted that he return to Penn State University to complete his education. He had dropped out of college in 1932 when his father lost his job in the Great Depression.  My father had dreams of being a writer, but ended up becoming an industrial engineer. Upon graduation from Penn State in 1935, he found a good job at the Imperial Wallpaper and Pigment Company in Glens Falls, New York. In 1935 he and my mother were married; a year later, my brother William III was born. My sister Prudence came next, in 1939. I was the third and last child. I was a shy and not very social child. I had dreams of doing something special. When a challenging problem came up, I attacked this problem with intensity until I solved it. We lived in Glens Falls throughout my childhood, except for two and a half years during World War II when my father worked for a defense firm in Bristol, Pennsylvania.  My father taught me how to play chess and we played until I gained the upper hand. History has a way of repeating itself. I taught my oldest son how to play chess when he was young and we played often until he became a much better chess player than I. My father and I also played golf. We had similar temperaments and this temperament was not that of a good golfer. We each got upset when we made a bad shot and you cannot play golf well when you are upset. I must admit that I derived great pleasure from the rare occasion when I beat him.  My father’s company was acquired by Hercules Chemical, a multinational corporation, in the early 1960s. At the end of that decade my parents moved to Wilmington, Delaware, where my father became the comptroller of its international division. He retired and moved back to Glens Falls in 1974. Through discussions with him, I learned a lot about the way businesses operated. This was one reason why I liked my microeconomics course so much in my first year at Swarthmore College. The price theory that I learned in that course rationalized what I had learned from him about the way businesses operate. The other reason was the textbook used in that course, Paul A. Samuelson’s *Principles of Economics*. I loved the way Samuelson laid out the theory in his textbook, so simply and clearly.  One thing that I remember from discussions with my father was the importance one person can have for the success of an organization. There was one such person, George Mellon, at my father’s company. Mellon built Imperial into the dominant pigment producer in the world. George Mellon was just one of those special people that could make good things happen.  Glens Falls is an interesting small town located in the foothills of the Adirondack Mountains on the Hudson River. The town was once wealthy with lumber money. Residents chose not to let the railroad connecting Montreal and New York pass through the town, as that would detract from the community. Glens Falls had a number of high-tech businesses, including Imperial, which through research and development became the leading pigment producer in the world. There were also a number of small electronic and medical equipment firms, as well as an insurance company, a paper mill, and the capacitor division of General Electric Co.  In high school I spent a lot of time playing bridge at the home of a friend. I remember two large sculptures by David Smith that decorated the living room. I know these artworks were special because a dozen years later I saw them at a special exhibition of Smith’s work in Washington, D.C. Douglas Crockwell, the father of this friend, was a famous illustrator who was a distant second to Norman Rockwell in the number of covers done for the *Saturday Evening Post*. He showed me drawings that he prepared for the famous physicist Edward Teller. Not being an atomic physicist, they didn’t mean much to me.  Another friend’s father was a leading orthopedic surgeon who worked out of New York City, although he also had a local practice. He experimented with hypnosis in his local practice, and he let us watch when he hypnotized a patient. I tried to hypnotize someone once and was successful, but one time was enough. With its high-tech industries and interesting set of talented residents, Glens Falls in many ways was not a typical American small town.  But in many other ways, it was. We all played Little League baseball, played pickup basketball after school, and went skiing. There were Friday and Saturday community activities in town for the young people. The high school had an exceptional science program headed by Mr. Bosworth. A math course I vividly recall as special was Miss Mabel’s plane geometry course. There I discovered a new “language” and learned the concept of a proof. The other math courses were good, but they were not the same. When I received a 99 on one of the New York State Regent Exams in mathematics (not plane geometry), Miss Mabel had tears in her eyes. She so much wanted me to have perfect scores in all the math exams! I felt bad about letting her down.  My goal was to make the high school basketball team, but being the second smallest and second youngest in my class of 160 put me at a disadvantage. I have always loved a challenge, so this didn’t discourage me from sports. By the time I reached my senior year I had grown some (weight: 133 pounds) and made the varsity football team. I was a pretty good defensive back and the second-string quarterback. Our team was loaded with talent and easily won all our games. This experience was not exciting. There was no challenge. I also was a good pole vaulter, setting the school record in my senior year. This was a challenge and even though no one was concerned with pole vaulting except me and my coach, I was happy with myself that I met this challenge. I had other interests as well: I was an avid reader of science fiction and was enamored with the writings of the Fabian Socialists. I also played tournament bridge.  During the summers my friends and I all held jobs. I became a golf caddy when I was 12 and a camp counselor for troubled kids when I was 17. When I returned from college in the summers I worked in the paper mills, which paid well. This helped pay for college, but more importantly, I got to know, like, and respect my fellow workers who didn’t have the opportunities I had.  Back in 1958, it was not hard to get into even the most prestigious universities. But I decided to go to Swarthmore College because it was less intimidating and somewhat less expensive than the Ivy League universities. Many years later my father told me that my mother had been disappointed in my decision. She had hoped that I would choose a more prestigious institution, but I have never been concerned with social status and prestige.  In high school I dreamed of being a rocket scientist, and therefore I majored in physics in college – probably the most demanding major at that time, given this was the *Sputnik* era. What disappointed me about Swarthmore was the dearth of intellectual discourse. Fellow students spent much of their time memorizing, rather than thinking and figuring things out. Nearly all were too idealogical to carry on an intellectual discussion. I could always predict what they would say. How anyone could defend the killing of tens of millions by Stalin and Mao was (and is) beyond me. This experience cured me of my socialistic leanings.  I have always had a need for physical activities and challenges. In college I played football for four years, and was the captain in my senior year. I enjoyed this diversion. Most of my teammates were the “non-intellects” – the engineers and premeds at Swarthmore. They had positive, cooperative attitudes and we worked well as a team. Winning or losing did not matter as much as playing as well as we could.  I dropped out of the physics honors program after my third year because I did not like the day-long laboratories. I liked to create things, and I found it difficult if not impossible to be careful and meticulous. Perhaps if the physics program had been more theoretical, I would have ended up a physicist rather than an economist. In my junior year I had taken an advanced seminar with the math honors students and liked it, so I switched to a math major in my senior year. That year I took a fascinating course in engineering economics taught by the charismatic professor Sam Carpenter. This led me to enroll in graduate school in operations research at Case Tech, which subsequently merged with Western University to become Case Western. I had no financial aid except for free room and board provided by a benefactor of Case. Given my financial situation, I worked intensely and completed the two-year master’s degree program in 15 months. It was all work and no play, but what I learned in the operations research program at Case proved to be of great value in subsequent years. In particular, I learned some recursive methods in the queuing theory course and thoroughly enjoyed learning some probability theory in a course that used the Feller’s classic text.  After earning my master’s degree, I had to decide what to do next. If MIT had offered aid, I would have gone there. But they didn’t, and the choice was between staying at Case or going to the Graduate School of Industrial Administration (GSIA) at Carnegie Tech (now Carnegie Mellon University) in Pittsburgh, Pennsylvania. I am not sure what led me to the decision that I made, but I chose GSIA with its multi-disciplinary program. In hindsight it was the right decision.  I truly enjoyed my graduate school days at GSIA. I arrived there as a student the same year [Robert E. Lucas, Jr.](https://www.nobelprize.org/nobel_prizes/economics/laureates/1995/index.html) (the 1995 Nobel Laureate in Economics) arrived as a freshly minted assistant professor. Under GSIA’s system, there was an informal assignment of students to faculty. I was assigned to Michael C. Lovell, who became my dissertation advisor – although Morris M. DeGroot, a great statistician, played an important role in supervising my research. Mike and I wrote two joint papers, one on an interesting mathematic statistics problem and the other on developing a business cycle model.  I learned something else from Mike that turned out to be important for the success of my career. I learned from him how to help students in that very difficult transition from student to researcher. What is the key in this transition is that students gain confidence in their own judgment and that they learn to listen to, and even seek, criticism of their research papers. I think a number of my students have benefited from interacting with me. I know I have benefited greatly from interacting with them. When my former students contribute to the advancement of economics, I take great pleasure in their success.  I did not take many economics courses at GSIA, but the capital theory of Bob Lucas was important, as was the growth theory course that Mike Lovell and Mort Kamien taught. I took a number of courses outside of GSIA, and the one taught by Allen Newell, one of the fathers of artificial intelligence, was exciting. The reason I took Newell’s course was that I found what [Herbert A. Simon](https://www.nobelprize.org/nobel_prizes/economics/laureates/1978/index.html) (1978 Nobel Laureate in Economics) said about artificial intelligence fascinating. Herb was a person who forced you to think and to take clean positions. With exceptional minds like these around, GSIA was an intellectually exciting place to be.  There was considerable interaction between the younger faculty and the graduate students. Bob Lucas, a junior faculty member, and I became lifelong friends. Much of the interaction occurred during the coffee times at 10:30 and 3:30. You could be certain Bob would be there, and the discussion would concern economics, broadly defined.  While at GSIA I received a generous fellowship and I worked hard. Having found a good group of friends, I partied hard as well. In September of 1964 I met Janet Dale Simpson. We were married June 5, 1965, the day before she graduated from Chatham College. Meeting Jan alone made my decision to go to GSIA the right one. There also were sporting activities, which I love. Carnegie Tech had an exceptional intramural program, and we doctoral students along with some master’s students had highly competitive teams in most sports. I even realized my high school dream of playing on a good basketball team! One year we were the intramural champions; another year we lost in the finals by two points.  After three years at GSIA, I left to join the faculty at the University of Pennsylvania in Philadelphia. There, my oldest son, Edward Simpson Prescott, and my daughter, Wynne Fraser Prescott, were born. The Penn years were good ones, though I must admit to a fear that I would not make it as a professor. The junior faculty members were bright and talented; I learned a lot from them. I have a great debt to [Lawrence R. Klein](https://www.nobelprize.org/nobel_prizes/economics/laureates/1980/index.html) (1980 Nobel Laureate in Economics), who provided summer support. I like models of the macroeconomy, and Larry Klein more than anyone is responsible for the development of this interest.  I benefited greatly from interaction with Larry Klein, and even more from Ned Phelps. He had the vision of the neoclassical synthesis; namely, providing the economic foundations for macroeconometric models. Ned posed the questions that had to be answered to unify macroeconometric models with the rest of economics. He ran the famous Conference on the Micro Foundations of Wage and Price Determination at Penn in 1969. Bob Lucas and a number of other people whose research was germane to this topic were there.  At the conference, Bob and I talked about the dominant firm problem in industrial organization and thought we had it solved. We ran into problems, however, and given that we were committed to presenting a paper at the summer Econometric Society meetings, we had to write something else. The paper we wrote in 1969 was “Investment under Uncertainty.” To deal with this partial equilibrium problem, we embedded it in a general equilibrium framework.  In this framework the number of commodities was large, too many to even be counted. This is when I was introduced to [Gerard Debreu](https://www.nobelprize.org/nobel_prizes/economics/laureates/1983/index.html)‘s classic paper[1](https://www.nobelprize.org/prizes/economic-sciences/2004/prescott/biographical/#not1) on valuation equilibrium and Pareto optimum. Debreu’s paper is less than five pages. The mathematics is beautiful. Each of the minimal number of assumptions is clearly specified and the welfare theorems established. I realized an economy that did not have the required mathematical structure with a given set of traded commodities often has the required structure with a richer set of traded commodities. This theory makes transparent [Arrow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1972/index.html)-Debreu general equilibrium theory with event-contingent commodities. I learned that apparent market failures often disappear when mutually beneficial trades are not prohibited. This simple insight has been crucial in some of my most important papers. The Debreu paper makes clear the importance and power of good language.  Writing the “Investment under Uncertainty” paper transformed me into an economist. Before that I was more a statistician than an economist. I am an econometrician in Ragnar Frisch’s original sense of the word; namely, someone dedicated to making neoclassical economics quantitative.  Among the highlights of my Penn experience was my interaction with one of the students there – Tom Cooley, who was (and remains) a family friend, and who subsequently became a valued collaborator. His dissertation supervisor was Larry Klein, but I also played a role in the supervision, and I like to take some of the credit for Tom developing into an exceptional economist.  In 1970 I was on leave as a Brookings Economic Policy Fellow assigned to the U.S. Department of Labor. When I returned, Penn had two new young faculty members that I came to admire greatly. They were Steve Ross and Tom Sargent. With colleagues like this, Penn was a very good place for me to be, but I was worried that I might not be granted tenure. When Richard Cyert, Dean of GSIA at Carnegie Mellon, called and made an attractive offer, I accepted.  Upon arriving at GSIA I met two advanced graduate students, Costas Azariadis and Finn Kydland. Even though they were more my colleagues than my students, I became their dissertation advisor and, like Tom Cooley, they became lifelong friends. The Ph.D. program became an exciting one. Credit for this goes to a number of people. David Cass and Bob Lucas played crucial roles and I think I did too.  After only one year, Dick Cyert moved to the presidency of Carnegie Mellon University, where he was critical in transforming it into a major research university. He is another example of one person being responsible for something happening. But even though this move was clearly good for the university, it was not good for GSIA. A dean who did not appreciate what he had (and did not understand the great tradition of the school) took actions that resulted in the departure of Dave Cass and Bob Lucas. I was saddened to see something good destroyed.  In 1973 our youngest son, Andy, was born. We joined the Chapel Gate Swimming and Tennis Club. I played tennis frequently. During this time tennis was how I got my exercise.  Jan, having an adventuresome soul, decided we would spend 1974-75 in Bergen, Norway, at the Norwegian School of Business and Economics, where Finn Kydland had become a faculty member. One thing that made it an adventure was that none of us spoke Norwegian and there were no English-speaking schools in Bergen. The visit turned out to be both personally and professionally rewarding. In the spring of 1975, Finn and I returned to the problem of policy selection in dynamic, uncertain environments. This time we got it right and found that the principle of optimality fails. Macro policy is a game, not a control problem as we had thought.  After returning from Norway, I decided to leave Carnegie Mellon as soon as a good opportunity arose. Finding a good match was not so easy, though. I was then established as an econometrician and was informally offered a good position in that area. But, I was no longer an econometrician. I found economics much more exciting, and upon my return to GSIA I chose not to teach the graduate econometrics course and instead taught an industrial organization course. A number of my students wrote innovative dissertations in this area. This was the rebirth of the field of industrial organization. The dissertations of Therese Flaherty, Chandra Kanodia, Charles Holt, Jean-Pierre Danthine, Edward Green, Barbara Spencer, Thore Johnsen, Léon Courville, and of course Finn Kydland, were all in this area. I wrote a couple of industrial organization papers with Michael Visscher, an assistant professor at Carnegie Mellon.  During this time I collaborated with Rajnish Mehra, the only Carnegie student other than Finn with whom I collaborated. In late 1979 we wrote “Equity Premium Puzzle” (which appeared in 1985) and “Recursive Competitive Equilibrium.” The shift from trying to come up with models that fit the data to using theory and measurement to provide quantitative answers to questions was difficult for me, given the way I was programmed to think.  A bumper crop of exceptional economists came on the market in the mid- 1970s, and Carnegie Mellon was fortunate to hire some of them. Upon my return I introduced a regular faculty seminar for these young faculty members. One of them, Rob Townsend, significantly altered my thinking about economics. He was a Neil Wallace student, and therefore one who uses the general equilibrium paradigm (as I do). But, he had been influenced by Leo Hurwicz’s mechanism design thinking. Subsequently, in the late 1970s Rob and I introduced private information into classical valuation equilibrium theory in the sense of the Debreu paper I mentioned above. The commodity point we introduced turned out to be key to endogenizing the length of the work week, which in turn is key in business cycle theory.  In 1978-79 I visited the University of Chicago as its Ford Foundation Research Professor. The following year I visited the Northwestern University economics department, where I took up running. This became my form of exercise for seven years until my knee gave way. That year at Northwestern I received an offer from the University of Minnesota. I accepted it, given that Minnesota had the best graduate program and was the leading producer of new assistant professors, with Thomas Sargent and Neil Wallace in macro. I think the success of the Minnesota program was due to its tradition of insisting that students first define terms, state their proposition, and then rigorously establish their proposition. With this approach, if you disagree with a conclusion, you have to disagree with one of the assumptions. The ample supply of ambitious, imaginative students was also crucial to the success of the program.  I began my association with the Federal Reserve Bank of Minneapolis in the fall of 1981, following in the footsteps of Tom Sargent and Neil Wallace. I had joined the Minnesota faculty in the fall of 1980, but was on leave at Northwestern’s finance department most of that year so that my wife could complete the core part of her doctoral program in industrial psychology at Illinois Institute of Technology. A few years after beginning my association with the Federal Reserve Bank of Minneapolis, Gary Stern became president of the Bank, and Art Rolnick, director of research. Gary, a good economist, wanted a small set of top economists around him with whom to consult on a variety of economic matters. Under Art’s leadership, this objective was achieved and the bank’s research department became, and still is, a major center in macroeconomics research.  Minnesota did not hire me in macroeconomics, but rather in the field of industrial organization. I think the industrial organization group was excellent, with Herb Mohring, Rob Porter, and me. Rob’s leaving was a great loss. Our efforts at replacing him were unsuccessful, but in going after Boyan Jovanovic and Ariel Pakes, we did go after the right people.  In Minnesota I became an applied game theorist – heavily involved in youth soccer. Here is how it happened. When I took my son Andy, then 8 years old, to the neighborhood park for the fall park board soccer program, I learned that Jan had signed me up as a coach. That evening I began my coaching career in a sport that I had never played. This was a challenge. Players from the local park board teams formed a traveling team that played in the late spring and summer. William A. George and I helped coach this team, and after a few years I took over the coaching. Bill was an exceptional manager, and I was not surprised when he later made Medtronic the leading medical equipment firm in the world. Here is another individual that made good things happen. I have limited coaching skills, but with the strong support of the parents over the years, the team attracted a talented set of players and we moved to the highest level of competition.  Soon these players became too good for me to coach, so I became the manager and hired a skilled coach to work with that team during the season. Given there was no one else available, I ran the entire Westside Football Club. While I was happy when a new generation came along to run the club, I was proud of the fact that the program did well under my stewardship. This was a challenge that I met.  The Minnesota economics department did as well in the 1980s as it had in the 1970s – in part due to the skills of the chair, Jim Simler. Here is another person that created an environment in which good things happened: consensus was reached quickly and there was hardly any politicking. This made the Minnesota department unique. The department had no required courses, a principle that served it well since its founding. The students who were betting their career made the decisions. If no one signed up for your graduate course, the course was dropped. Having this rule mitigates a time consistency problem.  I was fortunate to have worked with a large number of outstanding students at Minnesota. Minnesota grads have done more than any group to advance macroeconomics. They are special. I will mention Stephen Parente, who helped me coach soccer, because subsequently we had a long and fruitful collaboration culminating in our book *Barriers to Riches* (2000). The collaboration was inspired by Stephen’s highly imaginative and original dissertation on growth. At the time, everyone in the profession was concerned with what a country should do to foster growth. We took the opposite tack. Our position was that all market economies would be rich absent barriers to efficient production. To put it another way, to go fast, ease up on the brakes. In our studies we adopted the methodology that Finn and I developed for addressing business cycle questions.  Stephen is not only a great economist, but also a great cook. For a number of years Stephen’s famous clam bakes were held at our house. He would have the lobsters flown in from Boston, where his family was still in the seafood business.  In the midst of these good times, an ominous development loomed in the department. One group could not attract students and insisted on the imposition of a required course sequence. At that point, I considered leaving and was thinking about Chicago. The commute would have been only a total of five hours per week. It is a great department and in many ways I was more a Chicagoan than a Minnesotan, as I had more interaction with the Chicago faculty than I did with the Minnesota faculty. Bob Lucas, Nancy Stokey, and Rob Townsend – all on the faculty at Chicago – had been collaborators in the past. In addition, I valued the interaction with [Gary Becker](https://www.nobelprize.org/prizes/economic-sciences/1992/becker/facts/), [Lars Hansen](https://www.nobelprize.org/prizes/economic-sciences/2013/hansen/facts/), [Jim Heckman](https://www.nobelprize.org/prizes/economic-sciences/2000/heckman/facts/), and Sherwin Rosen. Chicago had a large and respected graduate program, and I thought I could help make it better. But, there was a problem: One of the students in the program was Edward Simpson Prescott, my oldest son. It would have been unfair to him if I were to join the Chicago faculty while he was there.  In the mid-1990s, a new dean of Minnesota’s College of Liberal Arts dramatically cut the size of the Minnesota economics department. The problems can be traced back to the politicization of the selection of the regents of the university. The regents had become representatives of special-interest groups instead of supporters of the mission of the university. In 1995 their plan to re-engineer the university included a proposal to eliminate tenure.  In 1998 I received offers from a number of departments, including Chicago, and the University of Minnesota chose not to match them. My oldest son was no longer a student at Chicago, and I accepted their offer. Except for the commute, I loved Chicago, which is in a class by itself when it comes to economics. I was depressed when I had to leave because of a family health problem. What particularly attracted me to Chicago was that it presented a challenge, and to repeat, I love challenges. In the single year I was there as a member of its economics faculty, I detected a rise in student morale. The students had gained a little confidence and become less intimidated by the profession. (Chicago can be an intimidating place, with all those gargoyles staring down at you.) After one year as a faculty member at Chicago, I was rehired and returned to troubled Minnesota.  Three good things happened after my return. At the Bank, I began a fruitful collaboration with Ellen McGrattan, in which we derived the implications of the neoclassical growth model that Finn and I used in the study of business cycles for the value of the stock market. The theory worked for predicting the value of the stock market as it had worked for predicting the magnitude and nature of business cycle fluctuations. When Ellen’s (senior author) and my paper was accepted by the *Review of Economic Studies* in late November of 2004, I was a happy man. Needless to say, I was not the only happy person. Now, theory can be used to say whether the market is over- or under-valued, and by how much. The integration of the stock market into macro models has important ramifications for understanding the boom in employment and output in the United States during the late 1990s. These certainly are exciting times in macroeconomics, with so much progress being made using the methodology that Finn and I had developed.  The second good thing was the teaching of an undergraduate macro course in modern macro. It turned out to influence my subsequent research. In that course I used the neoclassical growth model, which Finn and I used to study business cycles. I also used the same model, but with land as a crucial input to production, to account for the 5,000 years prior to 1800 when there was little or no growth in living standards. One of the students in the class repeatedly asked the question, why use one production function for one period and another one for the other? He was a wise guy, but I liked him. His question was a good one, and was one that the profession should have been asking itself. Gary Hansen and I came up with the answer in our “Malthus to Solow” paper. We permitted both production functions to be used at all times. This unified the models of the two periods and accounted for the long transition commonly referred to as the Industrial Revolution. Economics owes a debt to that student.  The primary reason I taught this course was that I saw the need to develop material that could be used to teach modern macro. Typically, the material taught in undergraduate macro courses teaches the Keynesian macroeconomics models, which failed, and failed spectacularly, in the 1970s. I wish that I had just a little of the Samuelson genius and could write a great textbook for the teaching of modern macroeconomics. It is needed.  In that course, I wanted the students to use the theory to evaluate policy. Therefore, I made up an exercise in which they had to evaluate whether a consumption tax or an income tax is a better way to finance a transfer payment. The model economy that they used is basically the one that Finn and I use in our study of business cycles, but without technology shocks. The students came to me and said there had to be something wrong – they could not find any income tax rate that finances the transfer. I was sure they were wrong and did the exercise myself. They were right, there was none. This is when I realized the quantitative importance of the tax system for output and employment. This was the topic of my American Economic Association’s 2002 Richard Ely Lecture and subsequent work establishing that tax rates account for most of the huge differences in labor supply across the major advanced industrial countries and for the large change in European labor supply over time.  The third good thing that happened upon my return was that my Bank colleagues, Hal Cole and Lee Ohanian, broke a taboo and used the neoclassical growth model to examine the Great Depression. What they found was eye opening. This motivated Tim Kehoe and me to organize a conference where a number of scholars used the neoclassical growth model to analyze a number of depressions. In this project, I was fortunate to make a match with Fumio Hayashi. We studied the lost decade of Japanese growth that began in 1992. Treating total factor productivity (TFP) as exogenous, the theory accounts well for this episode. This TFP factor, which is so important in accounting for business cycle fluctuations, is also important for understanding periods of depression and prosperity. This is further support for Parente’s and my position that the central issue in macroeconomics is to understand the mapping from policy arrangements to TFP levels.  In early 2003, my wife notified her company that she was retiring in January 2004. This made it possible for me to resign from Minnesota when the Minnesota administration refused to honor a written provision in my rehire agreement. This provision promised much-needed graduate student financial aid.  Once I was committed to leaving the University of Minnesota, the only question was where to go. We considered three places. My wife picked Arizona State University in Tempe, Arizona. One thing I particularly like about ASU is that its economics department is on track to becoming a great one. The people have good judgment. The department enjoys the strong support of administration, which is getting the needed resources. I welcome the challenge of helping to make the ASU economics department a great one.  1. “Valuation Equilibrium and Pareto Optimum,” *Proceedings of the National Academy of Sciences of the U.S.A.*, 40, 588-92, 1954.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 2004*, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 2005  This autobiography/biography was written at the time of the award and later published in the book series [*Les Prix Nobel/*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)[*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*/*[*The Nobel Prizes*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/nobel-prizes.html). The information is sometimes updated with an addendum submitted by the Laureate.  *Edward C. Prescott died on 6 November 2022.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0842  – Hello Mr. Prescott. My name is Marika Griehsel. Congratulations. I’m calling from Stockholm. How are you?  – Excited. I’ve just been calling people.  – We have woken you up very early. Have you been able to receive the news that you have received the Economic Prize in Memory of Alfred Nobel?  – Yes. The Committee called.  – The Committee called.  – This morning.  – What was your thoughts?  – It’s a one-in-a-lifetime event. It’s exciting. It was such an honor that Finn Kydland and I were awarded this great prize. We – I’m just so excited.  – We have understood that you have just got the news. Have you and Kydland worked for a long time together? Do you have any associations?  – Oh yes! We worked in the early 1970’s. I went to Carnegie-Mellon where – as an untenured faculty member – and he was a graduate – advanced graduate student there. I guess I ended up being his advisor. In 1974–75, he had arranged for me to visit Bergen, at The Norwegian School of Business and Economics, and that’s where we wrote our “Time and Consistency” paper, which is one of the – well, the award is for that paper, I was told. Along with the “Foundations of Business” articles. He was hired by Carnegie-Mellon in the late 70s, and we worked together at that time. And at the end of that time is when we wrote our second paper – we actually wrote it about 1980 on the – I think it was – that laid the foundations of “Business Cycle” theory, which is the other part of the award.  – To you, what is the biggest challenges – the economical challenges – that we are facing in the world today?  – I look at the world as a whole. Things are going quite well. The rich countries are growing nicely – not spectacularly – but the great thing is that the less developed countries are catching up, and are becoming rich. The world is becoming economically integrated. And this has … You said, “what’s the biggest challenge,” I guess is – keep the momentum going. Sustain these – I tend to think of them as cooperative international relationships, where countries enter into – or people in various countries enter into voluntary trades with people in other countries, where the technology is transferred between countries, where there’s a lot of insourcing as well as outsourcing. A lot of trade – I think that fosters economic efficiency.  – How do you tend to spend the rest of the day? What will you be – what will you do?  – Well, I think I’ll – pretty soon I’m going to drive down to the office and get – try to get a bit better prepared to answer questions – such as the ones you’re asking. As I’m sure the – there will be contacts from the press. The – I’ve – so far I’ve just been making calls to my wife who happens to be in San Francisco, and one son who’s in Miami Beach, another son that’s in Richmond, to let them know.  – We congratulate you here from Stockholm, and I hope we will see you here together with your colleague. Will you be here on the tenth of December?  – You couldn’t keep me away – couldn’t keep my wife away either.  – Congratulations and all the best.  – Thank you very much for your call.  – Thank you.  – Bye bye.  – Bye. |
| Interview |  |
| Q1 | So how did all start? Why did you decide to become economists? Was it just a coincidence or was it something that you had planned for a long time? |
|  | I certainly didn’t plan for it when I was almost through with high school, I had done pretty well in mathematics and I thought I would become an engineer. But then through various coincidences I ended up studying at the business school, *Norges Handelshøyskole,* and still then I didn’t think I would become an economist. I assumed this was an education that would lead up to a management position somewhere, maybe become a director of some company. And I just happened to take an advanced seminar from a very exciting professor, Sten Thore, exciting and excitable. He had us read very interesting and advanced papers in management science, operations research and I wrote my first FORTRAN programme using dynamic programming.  Then just before I was done, he came and asked me to become his research assistant and I followed him to Carnegie Mellon where he went on leave for a year and decided I had to get a piece too to make it in this field. So I never applied anywhere other than Carnegie Mellon. And then that was another great fortune because Carnegie Mellon has such a great research training programme, rather than doing all kinds of courses they get you started on doing papers right away. And so I did a first year and second year research paper and got them both published, and that’s a great start.  Certainly. And you Professor Prescott, how did you start?  Edward C Prescott: Tend to be drifting in, into that field. When I went to college it was the Sputnik era, so everybody wanted to go into physics. I was in that programme at Swarthmore College and was in the honours programme in my junior year and I decided I didn’t like laboratories. I didn’t like sitting in there all day being very careful and meticulous. And so I said, Well, I’ll major in math. I did have a very good special teacher, one course and sort of engineering economics or engineering science, tied to management, Sam Carpenter.  And so I went to get a degree in operations research, which was a new field of applying mathematical tools and modelling tools to management problems. And then I went to Carnegie Mellon, it was Carnegie Tech then. It’s a multi-disciplinary programme, a minimal number of course requirements. I don’t know if they had any course requirements, but you had to take a certain number of exams in different areas. But then I got attracted to the people in Economics because it seemed to be where the action was. [Bob Lucas](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1995/lucas-facts.html) came as an assistant professor the same year I came as a graduate student.  It must’ve been a very interesting period there at Carnegie Mellon at the end of the 60’s.  Edward C Prescott: Yes. Actually I was thinking about going into artificial intelligence because that was an exciting field there, too. And took some of Alan Newell and Herb Simon’s courses in that field. So then I ended up in economics. |
| Q8 | I know of course that you were Finn’s advisor, but I’m really very curious about this time inconsistency, or consistency result, however you like to put it. How did you start with this in the first place? I know you produced the thing in Bergen in 1975, but I guess you had some idea, why you just picked this problem and you got together in Bergen in ‘75? |
|  | There was a change. We recognised that thinking in terms of using these old system of equation models, waiting to say What should you do now? was not a question in the language of economics. But you could evaluate rules. So the obvious question is what’s the best rule? And so we set out to find that. And the finding was that there was no best rule because you’re dealing with people who anticipate and think. Initially we were a little disappointed because we didn’t find what we were searching for, but then we thought, this is a neat result.  Do you have any comment on that?  Finn E Kydland: And then I had already encountered the time inconsistency issue in my thesis. I had looked around for a thesis topic, at that time the people were worried about something called the assignment problem and it had to do with does monetary policy and fiscal policy, what do they target? But there seemed to be no coherence to that literature in my opinion and so I decided to formulate the problem as a dynamic game between monetary and fiscal policy makers, because of the way decisions are made it seemed that the fiscal policy maker would be, what I would call, the dumber player. And right away it was a model in which the optimal policy for the fiscal policy maker was time inconsistent. In the long run I didn’t regard that particular application as that interesting. I dabbled around also with oligopoly theory with a dominant player, there’s literature on dominant player industries. And again the issue was very much prominent. The dominant player’s optimal plan was not time consistent.  But it became a much more interesting issue once we thought about having the government as a whole as the dominant player, sort of playing against the rest of the economy. And there were followers. It was not without challenges because once you do so and you want to do it right, you want to make sure that you don’t treat the rest of the economy as one player, they’ve got to be viewed as atomistic, so we struggled for a little bit with that issue, but it became a methodology that was very useful both for that particular paper and what was to come later. |
| Q6 | When you submitted the paper did you then, at that time, know that this is really a discovery? |
|  | Finn E Kydland: I was pretty sure it was an interesting discovery. We had written the first draft of the paper. I’d gone back to Norway and I arranged for Ed to come and visit for a year and we wrote that paper. And then I decided to talk about it at a conference in Cambridge, Massachusetts, in May of 1975. And this was a conference with quite prominent people in the field of economics and control theory, which was quite popular in those days. It’s true the title was kind of inflammatory, initially it was something about the inapplicability of control theory to policy making. So maybe that set them off. But anyway, everyone in the audience thought the result was wrong and a huge discussion ensued and I of course knew I was correct. But the fact that problem people such as Dan Fisher, Gregory Chow and others, David Kendrick, thought it was wrong, that just made it clear this had to be pretty big. |
| Q18 | How important do you think that your time consistency result has been for the subsequent central bank reforms? |
|  | Edward C Prescott: Not sure, but they always use it as justification. And if we help in a little way bringing about these reforms, we’re proud of what we’ve contributed. They have moved in that direction and really what the people say, including the head of the Bank of England for example and /- – -/, at the Central Bank here in Sweden, I never heard Greenspan say anything specific about it but many people on the board of governors articulate these views strongly and push in this direction of a good rule and along with the independents, then you can follow it.  Do you want to add anything?  Finn E Kydland: I have done a little travelling in South America and to me, I’ve seen the contrast between countries in which they haven’t worried too much yet about following your rule or trying to make policy credible, and one sees how much a nation can be hurt by not doing so. And so that’s very depressing. Now exactly what to do about the ones you have lost your credibility, it’s not so easy. But at the same time it provides a contrast where I guess we are quite convinced that in nations where they use transparent polices where one has a good idea about what’s going to happen in the next 5 or 10 years, those nations will be much better off. And if this theory has served to lay the foundation for making them convinced of that, that’s rather better. |
| Q73 | The second part of your prize concerns business cycle theory. How do you think that this branch of economics has developed since your pioneering article? |
|  | Edward C Prescott: I think in the pioneering article we developed a methodology. I’d look at what was done just a couple of years before, a statement from, for example, Lucas and Sergeant, who were attuned to the modern dynamic economic theory with the rational expectations. Just bringing that macro with the growth things just clicked and in the way we sort of matched things up with the national accounts and the growth facts and looking at the same set of statistics. And in the process a huge amount has been learned and there’s a multitude of studies within the framework.  We happened to focus jointly on the consequences of total factor productivity shocks, but Finn and others have focused on monetary factors, bringing them into that same analytic framework using it in the same tools and techniques. And in the process there’s discipline, theory tells you the answer. You have guesses, you have conjectures, and sometimes they’re right, sometimes they’re wrong. You don’t know until you quantitatively, it’s a big word, quantitatively, work things out. And we developed a way to do that. I think that was a key contribution.  Finn E Kydland: Yes, initially of course the theory was relatively simple, although we put in a few bells and whistles that we thought could be important for other business cycle questions we were studying. Since then economists have made great progress in expanding the set of questions you can ask. For example we made the assumption for attract ability and it turned out there was an assumption that didn’t matter for the answer that everyone is alike and is immortal.  Since then computing capability has expanded, knowledge of theories, the theoretical framework has been expanded so that it’s easy to bring to bear what we know about life cycle behaviour, for questions where we know that’s important. And they seem to go about these studies and using the same methodology. It’s just that the framework has been expanded. |
| Q3 | Swedes have a problem now, we have only two Laureates in Economics. We have three in Norway in now and well, today we can’t do very much about that. But I would like to know, did you know [Frisch](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1969/frisch-facts.html) or [Haavelmo](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1989/haavelmo-facts.html), have they inspired you or have you heard them lecture? |
|  | Finn E Kydland: I once heard Frisch lecture. When I was an undergraduate at the Norwegian School of Economics and Business Administration, Frisch came to give a guest lecture and I and a lot of the undergraduates went to hear him. The auditorium A, as it was called, was filled to capacity and I still remember Frisch coming in, in a dark blue suit and jogging shoes. And he sounded very excited about what he did. But I have to admit that at that time I didn’t know much about what he was famous for.  Since then I have read some of his works, I was especially impressed by his paper in the castle volume. It’s a beautiful precursor of what was to come and I suspect that people had followed his lead more than some of the others in the ‘30’s. The development of macroeconomics would not have taken as long as it did. And also, of course, Frisch was instrumental in starting the Econometric Society and he was the editor for 25 years, and that became probably the most prominent society in Economics. Haavelmo, I didn’t know quite as well and I have to admit that what he got the prize for is sort of further removed from what I do. Although I have written other things by him, his work on development theory and investment theory is quite impressive. But I don’t believe I ever met him. But I would like to point out that I think Norwegians are proud of the per capita number of Nobel Prizes in Economics. We now have one per 1,5 million people and that would be hard to beat.  Edward C Prescott: I thought Frisch was the great one. He had the vision, making economics quantitative. I guess it was neoclassic economics quantitative. Back then there were different schools of thought in economics. Now there’s only one. And when they started the Econometric Journal for 25 years that was really the only scientific journal in economics and the leadership he provided. But I don’t think his vision was really realised until after that time the bill paper, then we can start doing the things he wanted to be able to do, really disciplined, providing these quantitative answers to public policy questions. He was very frustrated in the ‘60‘s and he would talk that as much progress was not being made in providing enough discipline and he referred to player metrics, or that term. But that’s just out of frustration that he wanted economists to be able to do so much more than they were able to do then. But now we can. |
| Q2 | So when do you think the first woman will receive the prize in memory of Alfred Nobel? |
|  | Edward C Prescott: Could be any year. I really can’t say who my candidate is. I can think of a couple of others in the pipeline that are 10 or 15 years away. But it could be anytime. There’s one that could’ve gotten it, but then she became a little bit extreme Maoist, but she had done some important economic research on the monopolists in competition many years before, Joan Robinson. I suspect if she didn’t have the, I think the Nobel Prize committee may have been a little bit nervous about the political thing, not the gender thing. They would love to.  Finn E Kydland: It is gratifying to see so many women coming into the field and in some nations more than others. When I’ve been to Spain, Italy and so on, it seems that the proportion has gone up to about 50%. And one has to remember that what you tend to get the prize for still is work you did 20 or 25 years ago, as was the case for us. And so if this is a recent trend then that by itself will delay the process. But it’s going to happen pretty soon. |
| Q25 | So what kind of research do you do today? Has your interest shifted over the years? |
|  | Edward C Prescott: Methodology, no. Topic, yes. One thing I was quite excited about is this research with Ellen McGrattan on the stock market valuation. The business cycle model says what the value of the stock market should be and, you know, I had to a few things, put in a corporates sector, you have to build in the tax system, because it turns out to be important and also a regulatory constraint. And the theory just does spectacularly well. But that sort of shifted the more interest in the bigger movements. Prosperities and depressions, these large movements in relative levels. Then I’ll be coming back to this problem about why isn’t the whole world rich type things?  And it seems to be barriers to riches. All the groups of people, no matter what nation they live in, if they can set up a good system, they’re rich. They become rich very quickly. But it’s related to the time inconsistency, it’s easier said than done, in what sort of institutions that might be set up that mitigates the creations of these barriers to efficient production. If you look at across countries, you can determine living standards just by knowing how much output is produced per hour worked. The amount of hours worked per person doesn’t vary that much, but the amount produced per hour can vary by a factor of 25 between the rich and the poor. And the relative productivity can change pretty fast, as we know from the growth miracles in Asia. And there were some growth miracles in Europe too after the World War II. The Italians, the German recovery.  Finn E Kydland: I might mention two things. One is there has been this belief that monetary policy affects the real economy and I was pondering how can it be there are so many people believe that out there and that it’s so hard to find a good propagation mechanism as Frisch would have called it. And so I’ve been searching to see if a few factors could be quantitatively important. One is working through the interaction with the fiscal tax system, for example if they’re non-indexed, which was the case in the US in the ‘60’s and the ‘70’s especially, but then the tax system changed somewhat in the last 20 years. And low and behold, it turned out that you can get some quantitative effects before the early ‘80’s.  It’s interesting that the investment in durables usually bought with loans, the cyclical behaviour is different depending on whether the nation has for some reason or other fixed normal interest rate loans as opposed to flexible interest rate loans. And that may lead one to wonder whether monetary policy or monetary shocks may have had an effect. But in the vein of what Ed talked about, what I found, Ed has studied the world as a whole and compared wealth and incomes all over the world. For various reasons I got interested in particular countries, and so I already mentioned Argentina which I have studied intensively and it’s fascinating but also depressing to study a country that used to be one of the richest nations in the world, and now has a population that is by usual classifications, it contains 40 or 50% who are poor. And why is that and what can be learnt for other nations? Then we have the contrast with Ireland, which used to be poor, and now has become one of the richest nations in the world. So I think one can learn by choosing particular countries to study. |
| Q13 | You have worked a little against the mainstream. You talked about this conference in Cambridge. And they thought it was wrong and they got irritated by your business cycle results. But would you give a person advice to really lean against the mainstream or would you give them any other advice? |
|  | Finn E Kydland: Yes certainly. You’ve got to do what you believe is going to lead to scientific progress. And we believed strongly that this was the way to go. But if the framework already is a reasonable one for the questions you want to address, there’s not much point in trying to lean against it just for the purpose of leaning.  Edward C Prescott: We listen to criticism and if some unfortunate criticism comes up, we fess up to it. |
| Q4 | I’ve heard that you are a big football enthusiast, so if you had to choose Norway winning the world championships or you winning the Nobel Prize, what will you choose? |
|  | Finn E Kydland: That question is too easy. I wish I would choose the Nobel Prize, but a Norwegian journalist just after the prize was announced asked a harder question, if you could choose to be who you are and win the Nobel Prize, or be a world famous soccer player? Then, and that’s a much harder question (laugh). So if I could be Martin Palermo or Tore Andre Flo or someone like that, that will make it hard (laugh). |
| Q28 | I guess that you, Professor Prescott, have been interested in soccer? |
|  | Edward C Prescott: Yes I got involved in soccer. Soccer was not played much when I was younger, but it’s become a big time sport in the US. I guess I got drafted into that organisation, to coach a team at something and stuck with them. They’re just a great group of kids. I used to make some of my graduate students help me referee or coach, we paid them. I made my sons help out the coach. It’s hard when you want to get the people to do this and the team that I manage was just the greatest group of kids and really talented athletic-wise. And so they can play with the best clubs in the world, including ones from Sweden, and Norway. We hosted a team from Norway, they have the biggest youth tournament over there in Minneapolis in the world. Sweden has big ones too I understand. |
| ID | 0843 |
| Biographical | Wake up, Robin”, a gentle hand shook my shoulder. “Let’s go.” It was so very early but I loved those mornings when my father would wake me and take me on an adventure. When I was a boy and we were camping, we would leave in the quiet morning hours to enjoy the lake together, and attempt to catch some fish. He was an experienced fisherman and a kindly teacher. While he was in graduate school at Cornell and the nation was still in the aftermath of the Great Depression, he traded fish and hunted meat for room and board. I did love those mornings and their crisp dawns.  My mother says that my father truly enjoyed having a son. My two years younger twin sisters felt that he didn’t quite know how to enjoy them. But, I wasn’t aware of those things then. So many of my childhood memories involve him. All the excursions into science were shaped by his knowledge and enthusiasm. As a Ph.D. chemist at Dupont, he also had access to many materials. So when my friend, Peter Hotz, and I decided to build and shoot off rockets, my Dad supplied different chemicals and a notebook. With each rocket we built, he would help us construct an explosive recipe. We had to write down the ingredients measuring carefully. He taught us to vary one ingredient at a time and then measure the effects. We recorded everything neatly. By the time he was finished, we had learned the scientific method and we had learned the math of measuring the distance that each rocket traveled. Of course, we also varied the rocket design. It was the way science should be done, passionately and carefully.  There were some experiments that Peter and I didn’t tell him about. Like the time that we built mazes and caught flies to go through them. We found it difficult to measure their progress while they were flying, so we varied the experiment by removing their wings. It’s a little painful to think about those boyish decisions now.  He also had a wonderfully equipped workshop where he built furniture and fixed all manner of things. One of my favorite tools was the lathe. My mother said that she always appreciated my constructions! As time progressed, the combination of the science explorations and the workshop led to many science fair entries culminating in the project that won the Philadelphia Science Fair in my Senior year of high school. For that experiment, I built a Van de Graff generator and ran experiments with it on X-ray transmission rates for thin metal sheets. The generator had a big copper toilet float to collect the static electricity and the X-ray tube was an old medical discard. A Geiger counter with homemade electronics recorded each X-ray photon as it passed through the test sample. It was pretty exciting stuff.  My mother was the unsung hero. She loved parenting the three of us and was always thinking up creative projects. We still watch the home slide shows she made of us and the neighborhood kids acting out fairy tales. She made the costumes, wrote the plays, and drove us to scenic locations around Philadelphia (the haunted house, the secret garden, etc). We all loved them. Except when I was supposed to kiss my sister, Sleeping Beauty, to awake her from the long sleep. I faked that part.  She also supervised our homework and made us feel that doing well in school was a wonderful thing. When I was in junior high school, she arranged for me to go to the Science Library at her alma mater, Swarthmore College. It was a small, beautiful place with wood shelves and books to the ceiling. I relished the time there looking through many books dreaming of when I would be able to really master science and be in the “big time”.  Both of my parents nurtured my dreams and me. It was an idyllic childhood in many ways. We lived in a large somewhat ancient three-story house on 15 acres in Media, Pennsylvania just outside of Philadelphia. Both of my parents grew up in Philadelphia. My father, Robert Fry Engle, Jr., was named after his father who owned a lovely large hotel on the Jersey shore called the Engleside. Summers were spent in Beach Haven and school years in Philadelphia. My Dad didn’t really like the hotel business and told stories about eating in the kitchen with the staff so that he didn’t have to fuss every night with fancy clothes and fancy food. He loved to sail and won many races. We still have silver bowls and dishes from these events. Instead of the hotel business he headed off to Cornell where he earned a Ph.D. in chemistry.  The Engles were a Quaker (Society of Friends) family who emigrated from Cambridge, England in the 1600’s. We assume that they came to escape religious persecution and set up their lives in Pennsylvania with the other Quakers. My great-grandfather, Robert Barkley Engle built the Engleside in 1876. A wonderful history of this has been recounted by John Bailey Lloyd in his book *Eighteen Miles of History* on Long Beach Island. The hotel was very successful for a time but two things eventually led to its demise. One was the spread of the use of the automobile. Families use to pack up and go to the beach for a month at a time bringing their servants with them. Once the bridges were built and cars became more common (and servants less common), families took shorter vacations bringing fewer people along. The second factor had to do with my grandmother, Sarah Atkinson. As a strict Quaker, she didn’t believe in drinking and insisted that the hotel be dry. This opened up an opportunity for nearby hotels who then benefited from the additional revenue.  My father proposed to my mother on one of the round towers of the hotel. My mother said that she thought she was marrying into a rich family with a wonderful hotel. The hotel went bankrupt and was torn down a mere 4 years later. My father never seemed to regret the demise of the hotel. Probably he was relieved to know that he could proceed with his science and not be called in to take over a business that did not interest him.  Recently, my wife, Marianne and I went to find the location of the Engleside. There is a park and another small hotel (also called the Engleside) on the land. Many of the old-timers recounted stories of the wonderful days of the Engleside. It gave us great pleasure. My mother also told me that she and my father took me to the Engleside when I was a baby just before it closed.  My mother’s family had American, French, and Welsh origins. Her father, William Vernon Phillips, immigrated to Philadelphia as a young man with his three brothers and two sisters. They came from Cardiff and worked in the scrap metal business. Vernon had great success. His import and export iron and steel business, F.R. Phillips and Sons was based in Philadelphia and Milan, Italy. He was also President of the Phillips-Laffitte Company and Chairman of the Board of the Perry Buston Doane Company. He served as mayor of his town, president of his country club, and deacon in his church. During World War I he was Chief of the War Industry Board in Washington, D.C. and on the Council of National Defense. After the Great War, he received the Knight of the Crown of Italy from the King. His obituary called him “an eminent statesman and capitalist.” He was clearly a busy man!  He married Florence Starr, a native of Philadelphia in 1912. They had three children, Billy, Isabel, and Mary whom they nicknamed Murry. In tragic succession, Isabel died of then unknown childhood ailments when she was 7, Billy was killed in an automobile accident when he was 16, and Vernon died of a heart attack in 1931 after the Crash of 1929. Murry and Florence were left on their own. They had to move from their beautiful home to smaller quarters. Murry remembers the sadness and loneliness of this time. One of her favorite memories is of the European tour that she took with her mother when she was 18. The pictures of that trip show a tall, slim beautifully dressed young woman full of charm and grace. She attended Swarthmore College majoring in French and then took numerous postgraduate courses in education at the University of Pennsylvania. Although an Episcopalian, she attended Friends schools throughout her life. When she married Bob Engle, she decided to join him in the Society of Friends and has been active her whole life in the Quaker community.  Once her children were older, she started to teach French at Media Friends School and eventually became its head. She sheparded the school through the addition of a junior and senior high school and a building campaign. She remained on its board for many years contributing both expertise and funds for new buildings and programs.  After she and my father married in 1939, they moved to Syracuse. I was born there on November 10, 1942 and soon after we moved back to the Philadelphia area in Swarthmore. These were happy times. I was named Robert Fry Engle III after my father and grandfather. Small Fry for short! They also had a favorite Springer spaniel named Dukie. My father had taken up ice dancing and there are pictures of my parents skating on ponds and at the Philadelphia Skating Club and Humane Society. They would hook up a record player to a car battery and play music while they skated outdoors.  Two years later, in December 1944, my twin sisters, Patricia Lee (Patty) and Sally Starr were born. It was a shock to have three children. Grandmother Florence Starr (nicknamed Twinkle) helped by taking me over to her apartment where I had a wonderful time visiting the giant steam engine trains and learning card games. In 1947, our family moved a few miles away to a big, old house in Media that the Engles called home until my mother sold the house following my father’s death in 1983.  I was a very active child and adolescent. I loved science, sports, music, and friends. Our yard was so big that we crafted a sloping baseball diamond. After school, my friends would bicycle over and we would play until it got dark or our mothers’ called. Eventually in high school I learned to play lacrosse and played goalie on the high school team. I began to play the tuba in junior high school. My teacher said that any tuba player should also know the string bass! I loved the instrument, took private lessons from Fred Maresch, a bass player of the Philadelphia Orchestra and was named to the All State Orchestra. Later, I played in the symphonies at Williams College, Cornell University, and MIT. Eventually my bass was stolen and I took up the cello. I still have my cello but it doesn’t get much exercise.  I graduated as Valedictorian of Penncrest High School in 1960. Ours was the second graduating class of the newly built school. There are some things that I said in my valedictory address that I still believe. I talked about the need for science to be both relevant and sensitive to the needs of humanity and I emphasized the importance of a balanced life. I am still happiest when I can do research that others can apply widely and when my days have some time for ice dancing or skiing, cultural activities, traveling, and being with my wife and children.  My four years at Williams College were filled with fun, growing up, and more science. I decided to call myself “Rob” as it was more masculine and mature. I still use this today. I majored in Physics, joined the Beta Theta Pi fraternity, and played more lacrosse. In my sophomore year, I was named to the All American Team as a lacrosse goalie, which was quite a thrill. By senior year I was nominated to Phi Beta Kappa. Another delight was roaming all over the countryside going to the women’s colleges looking for dates and parties on the weekends! I seemed to have one close girlfriend each year. Some of my best friends were made in the fraternity house although tragically, two of them, Stan Allen and Dave Kershaw, died young. They would have hooted with pleasure over the Nobel honor.  Academically, I started Physics and Math at the sophomore level since I had done advanced work and lots of outside reading in high school. A small event happened in my senior year that would have great consequences. I could take one more elective to finish out my courses. I almost took the religion course that everyone said was great, but I decided instead to try Introductory Economics. My roommate, Walt Nicholson, and many of my fraternity brothers were economics majors and spent hours discussing economic issues. I loved the course. It was interesting and came easily to me. Who could have predicted that this one elective would later serve as the catalyst that changed the course of my life?  During my senior year, my advisors in Physics encouraged me to apply to graduate school. It seemed natural since I had always dreamed of being a scientist. But something was lacking. I was losing my passion for physics. I did finally apply at the last minute and was accepted at Cornell and UC Berkeley. I called to accept Berkeley, but it was lunchtime. By the time they called back, my advisors had suggested I go to the famous labs at Cornell and so I did, following in my father’s footsteps. I graduated from Williams Cum Laude with Highest Honors in Physics and went off to Ithaca, New York.  Graduate school had its own pleasures. I lived initially in Sage Hall, which was the graduate student dorm. It was wonderful to be on campus with both men and women. I began to date a girl who was an ice skater who took me to the Cornell Figure Skating Club to learn ice dancing. I was very pleased to be in an environment with families and skaters of all ages. As a child my father tried to get me to skate with him, but I had no interest. Now, I needed a new sport since my lacrosse days were over. Ice-skating began to look like a lot of fun. Men are always in short supply in ice dancing so there were plenty of talented pretty girls to help teach me. There was also Eddie Collins, a former Canadian competitor, who took me on as student. The girl friend quit the club and me, but I have enjoyed ice dancing since then.  I worked in Professor Watt Webb’s lab doing low temperature physics and studied quantum mechanics from Nobel Laureate [Hans Bethe](https://www.nobelprize.org/nobel_prizes/physics/laureates/1967/index.html). Watt Webb was a great advisor with lots of insight and creativity and the study of super-conductivity was certainly exciting. However by mid-year, I realized that I did not want to spend the rest of my life as a physicist. Physics wasn’t what I expected, or maybe, what I expected was what I realized I did not want anymore. Working in the bowels of a building on projects that interested very few people in the world held less interest for me. After agonizing over these feelings and talking to my friends, I approached Alfred Kahn, chairman of the Economics department at Cornell. I asked if his department would be interested in considering me for their Ph.D. program. Serendipitously, he said that an NDEA fellowship had just become available because the intended recipient had turned it down. He offered it to me but said that I needed to make an immediate decision or they would offer it to someone else. My head spinning, I accepted the offer. Now, I had to tell my father and Professor Webb. Both were supportive but with heavy hearts. My father had trained me to be a scientist and now I was leaving for one of those “soft” science fields. At that point, neither he nor I realized how important all that scientific training would be in my contributions to economics.  I loved economics. I attacked it with an energy that I had lost. I took undergraduate classes to make up my deficits in knowledge while I finished my Masters in Physics on using nuclear magnetic resonance to study the performance of a high temperature-superconducting magnet. Intriguingly, both superconductivity and NMR, now named MRI, were awarded Nobel prizes this year! In fact the superconductivity lab next to Webb’s had been honored by the prize in 1996 and they were in the audience this year.  As I began the graduate program, my new advisor, Ta Chung Liu, was in Taiwan working with Chiang Kai Shek on their economic development plan. I took courses in Econometrics from Berndt Stigum where we read Malinvaud in its new English edition, and with John Fei who introduced me to modern microeconomics. My knowledge of math and statistics was put to use instantly. I took Kiefer’s probability and Wolfowitz’s statistics. I was extremely happy.  The next year, Ta Chung was back and I took his Econometrics class which gave me a solid basis for understanding the field. He was working on his third model of the U.S. economy. He had built an annual model and a quarterly model, having beaten the enormous Brookings Project to this goal. Now he was building a monthly model with all the new data and econometric problems that engendered. I learned trade from Ron Jones and Jaroslav Vanek, growth and development from John Fei and economic history from Woody Fleisig. It was so much fun. I found that I could solve the problems easily but quickly got stuck if I needed to formulate a problem. The casual discussions of short run and long run elasticities always required tremendous thought. It was probably ten years before I really absorbed the economic way of thinking.  The years spent in graduate school passed quickly. A few summers were spent in Washington, D.C. at the Bureau of the Budget doing program budgeting. I worked for John Deutsch who later became CIA director. It was there that I first tried to solve a cost benefit problem for public transportation. I recognized that land use was endogenous in the long run and this was critical for the analysis. This was my introduction to urban economics. I met John Kain and Charles Schultz.  As the years passed, my work became more focused on time series econometrics. Ta Chung suggested that I try to theoretically analyze the relationship between the different time scales for economic modeling. I was able to use some of my physics skills by formulating the problem in the frequency domain and applying Clive Granger’s “typical spectral shape” for an economic time series. This was my first introduction to his work.  In January of 1968 another important event occurred. After a ski trip to Aspen, Colorado with friends, I returned to start the second semester. There was a party down the street and a beautiful new young woman caught my eye. She had graduated early from a California college and was just beginning graduate study in Child Development at Cornell. Her name was Marianne Eger and she became my wife in August 1969.  We were first good friends and then I asked her to come with me and my friend Linus Schrage on a week-long canoe trip to the Algonquin Wilderness in Canada. She had never slept in a tent or been in a canoe but she thought it might be fun. Her sense of adventure has been a hallmark of our married life. We love traveling the world, hiking, skiing, and snorkeling while meeting new people and appreciating natural environments. We have come to understand many of the world views that exist on this planet. Our children have embraced the sense of adventure and we continue to have great fun together.  She is also a wonderful intellectual companion and mother. Although I do not understand the intricacies of psychological theory and she, likewise, is not a trained economist, we discuss our ideas and careers with each other at length. And our children, Lindsey and Jordan, are the joys of our lives. I consider myself a very fortunate man.  Marianne was born in Presov, Slovakia and immigrated to the USA in 1949 when she was 2. Her mother, Edith Elefant, grew up in Kosice and was a teenage survivor of Auschwitz. Her parents perished there. She met her husband, Albert Bela Eger, at a recuperation spa after the war. He was a resistance fighter from Presov. After WW II, the Communists made life uncomfortable and they left after an assassination attempt. They were fortunate to receive a visa and joined his brother and her sister in Baltimore, Maryland. They had to leave the Eger fortune behind and like many immigrants, found a way to reinvent their lives. Bela became an accountant, Edith, a bookkeeper. Marianne, meanwhile, went to nursery school. The family moved to El Paso, Texas to pursue more lucrative opportunities. El Paso was good to the family but Marianne was ready to leave. She went to Whittier College on scholarship, graduated early and then went on to a Cornell.  Although our marriage made finishing her Ph.D. more difficult, Marianne did receive it 1978, two years after our daughter Lindsey, was born. She has gone on to have a very successful career as a clinical child psychologist and as a sports psychologist. She has an active speaking career. She even wrote a weekly food column for a number of years.  But I’ve gotten ahead of my story. We married on August 10, 1969. On that day, I turned in my dissertation, received my Ph.D. and we left Cornell for good to take my first academic job at MIT. It was strange to arrive at a place with so many famous economists, none of whom were particularly interested in time series. For one overlapping year, Christopher Sims was at Harvard and a few years later, Jerry Hausman came to MIT. They were both helpful and enjoyable to talk with.  The first summer, 1970, I developed the theory for Band Spectrum Regression and attended the World Congress of the Econometric Society in Cambridge, England. I met economists there who have become lifelong friends and collaborators – Clive Granger, Ken Wallis, David Hendry. I began to go to London and the LSE every chance I could to pursue my fascination with time series. But finding my place was still complicated at MIT.  I enjoyed the teaching and the students there. Many of my students from that time have gone on to do quite well themselves: Larry Summers, Larry Backow, Ric Mishkin, Hal White, and many others. I even received an Outstanding Teacher of the Year Award from the graduate students. But I thought that my research needed a new focus.  Frank Fisher, [Bob Solow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1987/index.html), and Jerry Rothenberg encouraged me to join them on a new project to build a model of the city of Boston. A fellow economist and friend from Cornell, Tom O’Brien, was research head of the Boston Redevelopment Authority. Over the next 5 years and with many graduate students, the model took shape. I became an urban economist publishing very elaborate statistical models in a field not known for its mathematical sophistication.  Although MIT promoted me to Associate Professor, it was clear that I would not get tenure there. I wanted to work more in time series and I was attracted to Clive Granger’s interest in spectral analysis. At a meeting in Washington, D.C., I asked him if his new university, UCSD, had any openings. He said he’d check and I was invited to La Jolla, California to give a talk there. It was mid-winter, they housed me in a lovely hotel on the beach, a Williams Beta, Dick Attiyeh, was Economics Department Chair, Clive Granger had recently arrived – it was irresistible. I was hired as an Urban Economist and for years I did teach their urban economics course.  It was the beginning of a golden time for time series econometrics. Clive and I began an econometric seminar series, hired Hal White and later Jim Hamilton. We had funding for visitors and the best and brightest came from all over the world. We had numerous visits from David Hendry, Svend Hylleberg, Søren Johannson, Katerina Juselius, Timo Teräsvirta, Ken Wallis, Grayham Mizon, Tony Hall, Adrian Pagan, Max King, Giampierro Gallo, Tony Espasa, Keith MacLaren, Bernt Stigum, Eilev Jansen, Øivind Eitrheim, Helmut Lütkepohl, Hermann van Dijk and many others.  I took a sabbatical at LSE in 1979. Lindsey was then 2 and we rented a lovely row house in Hampstead with a back garden and a study in the back of the first floor. It was there that a new idea came.  I was interested in [Milton Friedman](https://www.nobelprize.org/nobel_prizes/economics/laureates/1976/index.html)‘s conjecture that inflation uncertainty was a central cause of business cycles. Investors who did not know what prices and wages would be in the future might invest less. To test this, a time series model was needed with variances that could change over time. There were two tools that came together to solve this problem. I had done a lot of work with the Kalman Filter and recognized that a one step predictive density would be sufficient to define a likelihood function. The second tool was a test. Clive had recently proposed a test for bilinear time series models. He came by my computer one day before I left and suggested I square the residuals and then fit an autoregression. To my amazement, it was quite significant. I suspected that this test was the optimal Lagrange Multiplier test for some new type of model, but not the bilinear model. I was later to discover that it is indeed the optimal test for ARCH and it is so called today.  Lunch and tea at the LSE were very stimulating times for me. Each day I would get a little further on this new model and would talk with Sargan or Durbin or Hendry or Harvey about its properties and my proofs. David Hendry eventually named it AutoRegressive Conditional Heteroskedasticity and offered to have Frank Srba program it. We applied it to UK inflation data and the ARCH model was launched.  Looking back now, one might think that new ideas are easy to publish. At least for me, they are not. It took quite a bit of rewriting and persuading to finally get it accepted in *Econometrica*. In fact, I don’t think that any of my papers have had an easy time of it!  Coming back to La Jolla, I was pleased that two events had occurred. The ARCH model was on its way and so was our son, Jordan, who was born in May 1980. I became very interested in Clive’s new concept of low frequency correlation that he called cointegration. It seemed to me that the new tests for unit roots of Dickey and Fuller, which had been so successfully applied by Nelson and Plosser, could be extended to this case. So I constructed an econometric approach to estimation and testing of cointegrated systems. Initially we wrote two joint papers and presented these at a time series meeting at UC Davis. There was a lot of discussion with many opinions as to whether this was a big or a small innovation. We decided to put these together but again, it took rewriting and persuasion, before *Econometrica* accepted this one too. Clive lost patience and published another version of the paper in the Oxford Bulletin.  Also, my excellent students, Mark Watson and Tim Bollerslev were busy. Mark had carried the Kalman Filter and state space models to a wide range of problems in macroeconometrics. We wrote a paper combining factor analysis and time series modeling and called it DYMIMIC. His first job was at Harvard where he worked with Jim Stock on models of leading indicators. This eventually became an official NBER business cycle forecasting model. Tim took the ARCH model, added a moving average and created GARCH. The GARCH model is an infinite order ARCH model with a geometrically declining set of weights. This extension made the model even more useful. The simplest GARCH model often performs successfully in a wide range of data.  Although these lines of research are now the most visible, there were a lot of other interesting things going on. I had visited LSE in 1975 on a Ken Wallis grant. There I learned about Lagrange Multiplier Tests and wrote a series of papers of which the Handbook of Econometrics survey is the one that gets the most attention. I gave a talk at C.O.R.E. and got into a big argument with Jean Francois Richard on the meaning of exogeneity. Afterward we looked up Koopmans’ definition and realized there were two different notions which we dubbed weak and strong exogeneity. The difference between these two was Clive’s concept of “Granger Causality” which was being used as a test for exogeneity by many authors. We invited David Hendry to join us, added a characterization of “super exogeneity” to deal with the “Lucas Critique” and wrote a quite controversial paper. After the dust settled, the paper was published by *Econometrica* and the profession has more or less accepted these exogeneity concepts.  Ramu Ramanathan, Clive and I founded a small consulting company, QUERI, that was dedicated to doing econometric research. We spent many happy hours on EPRI (Electric Power Research, Inc.) and other projects working with excellent graduate students to estimate better electricity demand systems, or load factor systems, or weather sensitivity models. These models have found an important place in electric utilities and in EPRI research, but only the spline model done with John Rice has received much academic attention. I wonder whether we would have been involved in the California energy crisis if QUERI had still been in existence?  I loved going to the European meetings of the Econometric Society. Time series was a big part of the program each summer. The meetings were invariably in nice places and we developed long term friendships that were renewed annually. By the mid 80’s these meetings featured multiple sessions on cointegration and ARCH. It was so exciting to see the new directions people were taking this research. We kept working to keep up with these fast flowing fields of research. Each year we would have several students working on these topics. Many of these students have had highly successful careers in and out of universities.  The profession rapidly developed a wide range of extensions of ARCH and GARCH models. Dan Nelson who sadly passed away at a very early age, introduced the Exponential GARCH model called the EGARCH. This model allowed an asymmetric response to returns. This was followed by many versions with names – TARCH, GJR-GARCH, NARCH, VGARCH, APARCH, FIGARCH, FIE-GARCH, STARCH, SWARCH, CES-GARCH, SQGARCH, component GARCH and many more. It was an exciting time. Students such as Gary Lee, Ding, Raul Susmel, Victor Ng, Ted Hong, and Ray Chou contributed to this growing literature.  As my research career developed, so did my ice skating hobby. I began skating with Brigit Luciani and we competed in many adult competitions. We often were successful in regional competitions. I passed my gold dance test around 1990 and shortly after that we did a week long workshop with Jane Torvill and Christopher Dean, legendary innovative ice dance champions. Finally, the United States Figure Skating Association agreed to have a national adult skating competition. The first year was 1995 in Wilmington Delaware. There were 21 teams and we eventually ended up 4th. We were very pleased. In 1996 we managed to move up to second place. Later I began skating with Dr. Wendy Buchi and we were again quite successful, placing second in 1999. I’ve had lots of incredible coaches over the years including Barret Brown, Michelle Ford, Jeannie Miley, Kent Weigle, Tatiana Navka, Judy Blumberg, and now Natalia Dubova and Eve Chalom. When I am skating, economics is far away. I always return refreshed and ready to carry on.  Ultimately, my interests gravitated more and more to finance. My colleagues David Lilien and Mike Rothschild and wonderful graduate students, Russ Robins, Victor Ng, Ken Kroner, Mustafa Choudhury and Aaron Smith helped me see this new way of thinking. The trade-off between risk and return was a central feature of financial analysis and the ARCH model had a mechanism for measuring this. We called it the ARCH-M model. A multivariate version with Tim Bollerslev and Jeff Wooldridge generalized the Capital Asset Pricing Model, CAPM. Options markets traded volatility and gave a quantitative validation of the ARCH models. Alex Kane, Jaesun Noh and I explored the possibility of using GARCH to trade options; it was temporarily successful. We flirted with the idea of managing money or investing our own but ultimately didn’t take that route. I ran several conferences in San Diego on the ARCH model designed for both academics and practitioners. I remember being so delighted that finance faculty such as Michael Brennan, Eduardo Schwartz and Bill Schwert came. It was a peek into a new profession. I knew Michael from MIT where I had stepped in to be part of his thesis committee when I first arrived. He was about my age and we still joke about how he is my first student. The rest of the committee was [Franco Modigliani](https://www.nobelprize.org/nobel_prizes/economics/laureates/1985/index.html) and [Myron Scholes](https://www.nobelprize.org/nobel_prizes/economics/laureates/1997/index.html) – a hat trick!  Gradually this new area of research became known as financial econometrics. It is the development of statistical tools specifically designed for financial applications. At conferences and meetings there were now quite a few sessions on financial econometrics. At UCSD we developed a specialization in financial econometrics. The arrival of Bruce Lehman and Allan Timmerman were very important in buttressing the finance side of this research area.  As these ideas spread around the globe, I was increasingly asked to give long workshops or mini-courses. This was fun as a travel experience and introduced me to students I still meet. The first was in Nairobi where Marianne and I were able to spend time with [Jim Tobin](https://www.nobelprize.org/nobel_prizes/economics/laureates/1981/index.html) and his wife. We did a short course at Peoples University in Beijing in 1995 flanked by Gregory Chow and [Angus Deaton](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2015/deaton-facts.html). Mini-courses in Maastrict, Bagni di Lucca, Helsinki, Uppsala, Madrid, London, Sydney, Stockholm, Vienna, Vaasa, and Rotterdam followed and gave opportunities for me to survey research in particular areas.  Joint work with Sharon Kozicki, Farshid Vahid, Joao Issler and Raul Susmel introduced another idea into econometrics – common features. This is a generalization of factor analysis that has applications to cointegrated systems, multivariate ARCH models and many areas of economics. The goal is to simplify multivariate systems into a small number of variables that endow the system with its typical features. In this case the features are common. This simplifies analysis in a way that is widely used in financial research. I will be surprised if there aren’t important research developments using common features in the next few years.  Somewhat later Simone Manganelli and I developed a new approach to measuring the extremes of a distribution. This method quantifies the probability of large losses on a portfolio by predicting the future quantiles. It has the tastiest name of any of my models, the CAViaR model. It stands for conditional autoregressive value at risk. Peter Hansen came up with this clever mnemonic. The CAViaR model uses the theory of regression quantiles in a time series context to give an updating formula for the Value at Risk (extreme quantile) of a portfolio.  In the early 1990’s I gave my first talks to financial practitioners. In a series of RISK meetings and Q-Group meetings, I introduced the audience to ARCH models and learned about the fascinating questions facing practitioners doing risk management and derivatives trading. I began a consulting project at Salomon Brothers in building seven of the World Trade Center. This project with Joe Mezrich and Eric Sorensen was to incorporate the GARCH model into a series of client oriented trading systems. Volatility forecasts and trading strategies were proposed based on innovative models. Advanced software developed by Pat Burns and Aslihan Salih, was used to structure client and proprietary portfolio strategies. I learned a great deal from this experience and made many contacts throughout the financial world.  I was asked to serve on the steering committee of a Zurich Financial Services company, Olsen and Associates, who were doing path breaking work on the analysis of very high frequency financial data. They were partly data vendors who delivered tick by tick currency and other data to a collection of European clients, and partly model builders. Richard Olsen and Michel Dacarogna proposed joint academic/practitioner conferences on the use of these data. In the first high frequency data conference, the same data set was analyzed by statisticians, economists, physicists, financial practitioners, and traders. The comparison was fascinating and highly informative.  Jeff Russell and I wrestled with the question of what we could learn from such high frequency data. Every way of handling the data in calendar time involved a loss of information. One day we realized that we could treat the time between trades as a random variable itself and model the speed of trading. We developed the Autoregressive Conditional Duration or ACD model for this purpose. It is a Poisson process with an intensity that is conditional on the past information. Financial data show variations in trading rates that can be called duration clustering. The model we proposed had the same structure as the GARCH model and recognizes the close relation between trading intensity and volatility. The simple ACD(1,1) very rapidly led to generalizations with different functional forms, error densities and types of data. Initially we only observed quotes so we built a model for the time it took for prices to move a fixed amount. The ACD model of such price durations is really an alternative volatility model. Then we obtained transactions data and modeled the time between trades. In the Fisher Schultz Lecture that I gave in Istanbul, I combined the time between trades and the prices at which trades occurred into an “ultra high frequency” or tick by tick GARCH model.  Although there was a lot of interest in the statistical model of trade arrivals, some economists at the time thought this was a model of the least interesting variable in finance. I was convinced that the trading frequency measured a fundamental heartbeat of financial markets. Clearly it reflected the flow of information. It turns out also to be closely related to measures of liquidity. I began reading the literature on market microstructure and soon came to Maureen O’Hara and David Easley (1992). Maureen was also on the Olsen steering committee and I recognized that the speed of trading was related to information flow in their model and had a direct link with measures of liquidity such as bid-ask spread and price impact. In papers with Alfonso Dufour, Joe Lange and Andrew Patton, we showed that when markets are more active, in the sense that the time between trades is short, they are less liquid. Information flows correspond to illiquid markets over time.  This research leads naturally to an interest in timing of trades to achieve good execution. Several years later, a group at Morgan Stanley headed by Robert Ferstenberg and Rohit d’Soza approached me and I have spent many interesting days with them developing a microstructure approach to optimizing and evaluating trades. We have become good friends as we push this frontier between theory and practice.  New York was a very exciting place to visit when I was consulting for Salomon. Now that Lindsey and Jordan were both on their own, I convinced Marianne to spend the fall semester of 1999 visiting NYU in the finance department. I knew many people there. My sister, Patty, had also moved there to work as the child development officer for UNICEF. Joel Hasbrouck was the leading microstructure econometrician; Steve Figlewski, who was a student of mine many years ago at MIT, was a leading empirical options researcher, and my recent student Josh Rosenberg was there. The semester was too short. I saw the financial markets at work and got to know more finance faculty. We ate well and played hard. When NYU offered me a permanent position, I took it and we moved in September 2000. I thought we would stay for only a year, but even that was not enough. Eventually I retired from UCSD, becoming an emeritus professor. We maintain close ties with San Diego as we have a house there where we spend summers and have lots of friends. UCSD has kept my office and I still see a few students.  Since I have been in New York, my work has actually moved back to volatility models, but in large multivariate systems. The extension of ARCH models from univariate processes to multivariate processes began in the early 1980’s. Ken Kroner and I introduced one new family, he and Victor Ng introduced another and Tim Bollerslev introduced a third. However, there have not yet been many empirical applications of these models. This is because they are difficult to specify, estimate and interpret. In a new general class of models called Dynamic Conditional Correlation or DCC models, I have developed a potential solution. This class is parsimonious and appears to give satisfactory performance for both small and large systems. In a series of lectures at Erasmus University in May 2003, this model was fully described and these lectures will soon appear as a Princeton University Press monograph.  To evaluate alternative covariance matrices, I have developed a loss function with Riccardo Colacito, based on the effectiveness for asset allocation. The same model can be applied to Credit Risk as the correlation between defaults is determined by the dynamic structure of the covariance matrix and the tail properties of the model. New high frequency volatility models have come out of research with Giampiero Gallo and models for the volatility of volatility have been developed with Isao Ishida. The new and interesting research topics that have opened up since I have been in New York are a constant source of stimulation.  While professional interests have always been important in my life, so have my family and my hobbies. Marianne and I have had a wonderful busy time raising Lindsey and Jordan. We stay as close as we can to them and over the years they have traveled with us all over the world. Lindsey was always a fine student and devoted ballerina. Her warm, friendly personality and her strong determination have led to much success for her and a good time for us. She has been a wonderful sister to her younger brother. When she was a baby, she would come to my office one day a week and sleep on a mat behind a chair to take her nap. She would come to lunch with Clive and the other economists. Unfortunately, all that good input didn’t lead to a career in economics! She graduated from Princeton Cum Laude in anthropology and has now finished a Ph.D. in developmental psychology at UCLA. In May, 2003, she married Justin Richland, JD, a legal anthropologist from Los Angeles, also finishing his Ph.D. They both hope to have careers in academia.  Jordan is the outgoing charmer. He is a wonderful athlete, excellent soccer player, and has a probing mind. He graduated from Williams College in English literature with a certificate in theater and is now in Los Angeles working as an actor and cinematographer. During his college years, he spent 2 semesters at the University of Cape Town, South Africa. He was very affected by the country and its people. Our trip with him to Botswana was a highlight for us. Jordan can talk to anyone comfortably and learn about them. He is also an excellent writer. His career path will be an interesting one. As a brother and a son, he is a pleasure for all of us. He has had lots of girlfriends and was especially sorry that the Swedish princesses did not seem interested in him!!  My sisters Sally and Patty have also had very successful careers and I am so proud of them. It is remarkable how many parallels there are among us. I guess it must be something about either genetics or our upbringing. Sally received her Ph.D. in anthropology and has been teaching at Wellesley for 30 years. She developed an interest in the relation between legal systems and cultural systems. This lead to books on Urban Danger in Boston, the ways of Getting Justice and Getting Even in and outside the Massachusetts court system, law, culture and the U.S. colonization in Hawaii, human rights and the U.N. and lots of papers with sharp insights into modern society. She was president of the Law and Society Association and a regular visitor at the American Bar Foundation. She is now being courted by NYU and the University of Pennsylvania. Maybe there will be another Engle in New York.  Patty received her Ph.D. in psychology at Stanford and has spent a career studying cross cultural child development focusing often on the role of nutrition. She has been involved in projects in Guatemala, Nicaragua, Peru, Uganda, India, and many other places. Her academic publications describe many of these research settings; her students and colleagues at her long time university, California State University at San Louis Obisbo, were able to hear about these first hand. Her current position as Senior Advisor for Child Development at UNICEF in New York gives her scope to pursue the goal of child health from a broad perspective. She has become a vocal advocate for global children as she works to improve the quality of life in so many different cultures.  A stable, happy home life is good for all of us and allows us each to focus on our careers knowing that when we are back together, there is more to share. As a family we love food, wine, music, opera, art, theater, traveling, hiking, fishing, and watching sports. Separately we have our individual career paths and our hobbies. It makes for a rich interaction. I still ice skate 2-3 times a week. I enjoy skating with Wendy Buchi when I am in La Jolla. Marianne is learning to play golf. Lindsey runs with her dogs, Jordan finds soccer games and surfs when he can.  The Nobel Prize in Economics is an incredible recognition for the work that my students, colleagues and I have done over the years. We all worked hard but we were also lucky that the financial applications were so important. It continues to amaze me how far this simple idea has traveled. I am starting a Financial Econometrics Research Center at NYU to foster the continuing development of this field.  As I look back over my career, this Prize is the high point. I find myself reflecting with great affection on smaller, perhaps less dramatic, moments. These are moments of insight; moments that started a new research topic or recognized a connection between things previously thought to be distinct. These are also moments of family time – wonderful moments with Lindsey and Jordan, sharing ideas, eating, hiking, skiing and yes, even fishing – and moments with my lifelong companion and soul-mate, Marianne. It has been already a lengthy and varied career but I think I am only part way through it. There are many exciting adventures in our future and I am looking forward to ever so many special moments more. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0843 |
| Interview |  |
| Q39 | To my knowledge none of you has studied economics as undergraduates and this means that you have a background in another discipline. How did that affect your research when you were doing? |
|  | Robert Engle: In fact, when I was an undergraduate, I was a physics major and I had lots of room mates and friends who studied economics and they kept telling me I should take an economics class so I could find out what this was really like. So, my senior year I had a choice between taking a religion class and economics class because I had an extra spot, and I took economics and it was fascinating. That was my first actual experience and that’s probably why I ended up doing the switch. But then when I did my switch from physics to economics I took pretty much a full set of undergraduate classes on a listening basis while I was taking the graduate courses.  Clive Granger: I had just one third of my first undergraduate year in economics which I enjoyed and it was a very non-mathematical approach. I kept trying to translate into my mathematics that I was more used to and I had trouble doing that, but I enjoyed what I did. I always felt that it was both a disadvantage not to know economics and an advantage because economists think about things differently than everybody else, but also when I came to economics proper I came to it in a different way than all my colleagues did and I think that added an extra dimension to the approach. Then in pair with economists together we would then have a wider way of looking at things and so it was both a disadvantage and an advantage. |
| Q3 | Has any particular teacher really been important to you? As an undergraduate or graduate? |
|  | Clive Granger: Yes, when I was undergraduate my professor was Brian Tew who was a very good macroeconomist in England and he was one of these people who was totally non-analytical but he was appreciative of mathematical methods. He was very /- – -/ believed the future was in a more mathematical, technical approach to economics. At Princeton I worked with Oscar Morgenstern and he was a great man and had very wide interests and again he didn’t teach me any game theory, I learnt some again just being around him and others. But I found that his leadership was just magnificent.  Robert Engle: I guess the teachers that I would like to mention were my couple of teachers at Cornell in graduate school. The very first day I arrived there I met my thesis supervisor whose name is Ta Chung Liu or as you would say in Chinese Liu Ta Chung and he was a wonderful mentor to me. He was interested in econometrics as a tool to do real problems. He was building models of the US economy and the Chinese economy and you could really see that this was a tool that could solve problems. Then two other men that were really very important were Berndt Stigum who is now in Oslo and was really a great inspiration in teaching a lot of the advanced econometrics that I later used, and John Fei who really gave me my introduction to some of the more traditional economic areas. |
| Q8 | Some researchers work in teams and some others do more alone work, how does a usual workday look for you? |
|  | Robert Engle: I write a surprisingly large number of papers with my students. I like taking a graduate student and just starting on a project at early stages of their career and then slowly it develops and we get a little further and little further and little further and by the time maybe two years has gone by we have a paper, or maybe three years. Then pretty soon maybe there’s a new important idea that they do. That’s one way I do it. But the other way, which is a wonderful way to do it, is to collaborate with colleagues like Clive and we’ve had an awful lot of wonderful times just talking about big problems and how could you solve this and how could we solve that? I’ve seen this thing and what do you make of this? I don’t know what I make of that, what do you make of it? We just kind of go round and round in circles and all of a sudden there’s something new there.  Clive Granger: I second that totally. Occasionally I spend time just by myself playing with a model and just trying different alternative approaches. But I’ve had over 80 collaborators with my publications over the years, so you see I get along well with people and together we produce. When I was at Nottingham recently, I was there just by myself for quite some years and then Paul Newbold arrived and the difference in my ability to produce and to get my ideas was greatly enhanced by two of us, two of us was much better than two single individuals. Then I moved to San Diego and then I had a year there where I was by myself and then Rob came along and suddenly everything blossomed and we could interact and be very productive. Then we have some very great students come along and the whole thing was just built up. So the answer is there’s no single answer, we do both individual work to initially begin a project but then the discussion and interaction and throwing ideas out to people is very nice. One thing we did at San Diego was to have a regular lunchtime meeting every Tuesday of all the econometricians and we always tried to make that meeting and it was a totally un-programmed meeting, we just would go along and see what one talk about. It was a great help to people, sometimes we’d talk about nothing. just sport or something, but other days it was a really interesting meeting and visitors would come along to it and that was I think a helpful thing to do.  Robert Engle: We also did some gossip at these meetings.  Clive Granger: Don’t tell that!  Robert Engle: Who was running around with whose wife or switching universities or what was happening so … a lot of things happened! |
| Q67 | According to Nobel you are supposed to have done a discovery to be awarded the prize and I think this time it is obvious that both cointegration and ARCH is really a discovery, but when did you find out or how did you find out that this was really something that could change the way statistics and also financial econometrics could be? |
|  | Clive Granger: Certainly the cointegration idea was one of these things that once it became clear that we understood what we’d found and what the implication of it were to other things that it solved many problems. Things that we were being puzzled about beforehand, we were seeing papers which were inconclusive and people didn’t know quite what to do with their data, suddenly we found we found we could solve all those problems. It became clear how to explain all many things. I think almost within days we both realised that this was going to be an important discovery. Exactly how you sell that is another question but I think we had no doubt how important are the question but we were excited by it and we quickly told people about it and other people were excited by it. Word spread quite quickly on that, on this cointegration.  What about ARCH?  Robert Engle: Arch really came about when I was on leave on sabbatical from San Diego at the London School of Economics but it was stimulated by things we had been doing in San Diego beforehand and it was a project I was working on and kind of in the back of my mind. I had a project in the front of my mind that I felt like I had to do first before I could really think about this time-varying volatility question. Then my wonderful student Mark Watson wrote me a letter I guess, couldn’t have been email in those days, saying that he really wanted to figure out the first problem that I felt I had to solve first for his thesis. I thought to myself, hm, if I let him work on that then I can try to work on this other thing that I’m interested in and all of a sudden I had days of empty time ahead of me and I had these ideas that I wanted to build models of time-varying variances and I had a lot of external stimulation from people at the London School of Economics and all of a sudden the idea came, got put together and it was just very exciting. I did feel like that was important too although that is an idea that it seems to me it took a lot more time for the profession to realise it was interesting than the cointegration idea where everybody just jumped on it. |
| Q10 | You seem to talk a lot about students and I wonder what would the dream student be for you? |
|  | Robert Engle: You know students are very deceptive, it’s not necessarily the ones that do the best in class that are best to work with. What I find is that there is this transition that students go through, that at the beginning you’re explaining things to them. I always like them to have taken my class so that we’re talking a common language. Then at the beginning I keep explaining to them and I always like talking about things I don’t understand, how could we think about it, this way or that way? They have something to say or they don’t have something to say. Then in a few short months all of a sudden they’re explaining things to me or maybe a year or two later: Could you explain that to me again? They’ll tell me again sort of why this is the right answer. That’s the dream student for me.  Clive Granger: I find some students are very compatible, that you talk to them and you’re very comfortable talking to them. They’re just naturally intelligent and they absorb what’s being said and initially they’re taking everything from you. Then slowly your relationship changes and you find that they’re doing more than you asked, they’re bringing back to you more than you expected and they have their own results which you say Wow, I didn’t realise that was going to occur from this problem. That’s great and you’ve established somebody on the road to doing good research and you know they’re going to have good ideas from thereon.  Robert Engle: Another thing to say along the same lines is that when you’re working on something that’s new and actually that’s mostly what we’re doing, you’re headed into the uncharted area so you don’t really know what’s going to happen and some students get stuck a lot and you spend all your time with them trying to get them unstuck. Other people seem to avoid getting stuck, they figure out a way around it or get a good idea sometimes that you sort of think back on, you say Wow, you know we really made a lot of progress, versus in the first case Oh, there are all these roads blocks, road blocks, road blocks, we couldn’t get anywhere. I think that’s actually an important quality that students can have and we have to have ourselves as we develop in these areas. |
| Q49 | As I see it, it has been a fantastic develop in econometrics during say the last 20, 30 years, this is not the first prize in econometrics. What would you say, computers, better computers, has that contributed to this or is there something else? |
|  | Clive Granger: We discussed this in the past when computers first became rather fast, the data became more plentiful we wondered whether there were going to be lots of fantastic results appearing. New things about the economics were appearing and it hasn’t really quite happened as to the extent we expect. We can now think of quite different ways to approach econometrics, we’re not having the constraints we used to have on computing and often data availability now. Whereas at one time it was an effort to do analysis with 100 pieces of data for a single series, now if you go out with 400 pieces of data and 500 series at the same time and have this enormous paper and how much better that is than the one series done properly is not always so clear. We learn a bit more but not a huge amount more, so I think we haven’t yet learnt how to best summarise and how to best search this extra tool. It’s going to get worse because we start dealing with multi-freight distributions next and even to describe some of these things and to know, we’ll have a lot of results we can’t even look at. It’ll be too complicated for anyone to look at the results, they’ll be on the computer, we can ask the computer question about what the results say to us but they’ll be too complicated, they’re just too numerous. We have to learn how to deal with that situation and that’s exciting, in my opinion that’s something which the physicists have faced for many years, they have multi-dimensions and too many data points. We haven’t quite got there yet.  Robert Engle: In time series one of the things that happened is that the big macro-models were pretty much static models or almost static models. They had very simple kinds of dynamics in them and when people started to look at how well they forecast especially short run forecasting they found out that very simple time series models could out-perform the static which I would call traditional macro econometric models. By thinking about this short-run forecasting problem it gives you a different way of formulating econometric models. Econometric models are formulated very largely in terms of given what we know today what’s tomorrow likely to look like, so you build up these models in what we call recursive fashion. That point of view gives rise to an awful lot of these developments and time series, so it maybe just that change in formulation that’s given rise to a variety of different things. |
| Q28 | Doing research is certainly good exercise for the brain, what other kind of exercise do you do? |
|  | Robert Engle: You’ve been peeking! We both do a lot of other kinds of exercise but one of the things that I love to do is ice dancing and you’ve probably seen some of it here in this Nobel Week, film footage and I’ve been adult competitive ice dancer for many, many years and it’s a wonderful escape from my economics. My skating friends don’t know that I do economics and my economics friends don’t know that I skate, it’s like you just change your personality and that lets your brain relax and other things happen and then when you come back to work its fresh.  Clive Granger: I’ve nothing as spectacular as this to discuss but every day I try and walk on the beach for half an hour, one of the advantages of San Diego it’s very relaxing on the beach. The sound’s nice and the whole atmosphere’s nice and in the summer the water’s quite warm in San Diego so all 25 years, every day in the summer I’ve surfed, I’ve body surfed, I call it body surfing it’s really not that spectacular but it’s fun and it’s very good exercise.  Robert Engle: You take a boogie board don’t you?  Clive Granger: No, I just use my body, just lie on the surf. It’s fun and I really enjoy it and exercise that you enjoy doing you do, and you have a good time and you come back and you’re fitter from doing it.  Don’t you enjoy art too?  Clive Granger: Yes, I go to art galleries quite a lot, whenever I’m in a city on a conference I always put half a day aside if I can and go to that local art gallery and look, I’m not expert in art I just like looking at art. It’s relaxing.  Do you talk a lot about economics at home or what kind of discussions?  Robert Engle: My wife’s not much of an economist; I don’t think Pat is either.  Clive Granger: Not at all, no. How to spend the household money is one of the problems!  Robert Engle: The closest we come to an economic discussion is talk about how was your day and all this sort of things. Then I’ll say something like You know, I had a good idea today and she’ll say Oh really, oh that’s great. Because a good idea to me is so exciting and she knows it’s exciting and she appreciates that, and it feels like a real accomplishment to her as well as to me.  Clive Granger: I’m told off for working too long and too hard and I go away in my study and do my own thing but that’s all, we don’t discuss economics at all. |
| Q74 | But you have children both of you and are they economists or academics? |
|  | Clive Granger: My daughter is on the edge of an academic career. She’s a science writer and she’s thinking about working in the university doing administrative type academic jobs. My son’s a programmer or developer of animation programmes, so he’s not academic at all but quite successful.  Robert Engle: My daughter took a class at Princeton from Helen Blinder and was very interested in it and I got my anticipation up a little bit but she decided that wasn’t what she wanted to do. She’s just finished her PhD in cognitive psychology at UCLA and I think wants to be a professor so I feel like there’s some carry over there even though it’s not economics. My son took an economics class at Williams and after he was done he said, You know Dad I really learned something important in this class – that I’m never going to be an economist! So, he’s more on the creative side and he wants to be an actor and has been doing a lot of high-level photography and pursuing a more creative career. |
| ID | 0844 |
| Biographical | I was born in Swansea in the Principality of Wales in September 1934 and named Clive William John Granger. The “William John” names were traditional Granger boy’s names and my mother liked the name Clive because some popular musician at the time had it. My father, Edward John Granger, and with his wife Evelyn (both English) left Wales with me when I was about one year old to go to Lincoln in Eastern England. He was a commercial traveler for a well respected firm called Chivers that made jams, marmalades, jellies, and so forth. He traveled around whatever area he was directed to by the company and took orders for the products from wholesalers and individual grocers. My memory of Wales in this early period is non-existent but it did qualify me to play rugby for Wales, if I had been good enough. I did not get the Welsh ability to sing, unfortunately.  I can remember little about Lincoln except for the night of September 3, 1939, when war was declared and everyone cried. At about ten p.m. the air raid warning sirens went off and we all huddled nervously under the kitchen table. Of course, I now realize that the probability of a Nazi bomber flying over Lincoln on the first day of the war was extremely slight; it was just a wakeup call for the English people that a real war was upon them.  A year or so later my father went off to war to serve as a driver of large support vehicles for the Royal Air Force, first in the south of England and later in North Africa. My mother had three brothers serving in the forces, but we were a fortunate family as they and my father all returned unhurt. Whilst my father was away my mother and I moved to Cambridge to stay with her mother, who was a very short, fierce woman who had brought up many children and had a very strong personality. She was a professional cook and could make a great meal out of almost nothing, a worthwhile ability during the shortages of the war. Earlier in her career she had cooked for gentry and it was said that she once cooked for Queen Victoria. When she was just starting out she had worked as a very junior cook at Windsor Castle and her two sons had played with the princes Edward and George, both later Kings of England. Later we moved across Cambridge to stay with my father’s parents. My grandfather repaired shoes in his own shop and sold shoes in a successful small business. I went to a local primary school, did well in mathematics but not much else. A teacher told my mother that “I would never become successful,” which illustrates the difficulties of long-run forecasting on inadequate data.  At the age of 11 I passed the necessary exams and was accepted by the Cambridgeshire High School for Boys, where I performed fairly well but showed no particular ability. The headmaster was Mr. Newton-John, who later moved to Australia and had a famous daughter, Olivia.  At this stage of my life I had largely drifted through, just taking things as they came. I had no clear-cut ambition or long-term plan, and that policy has generally continued. My career has largely been determined by a sequence of “lucky breaks,” which will be discussed below.  Lucky break #1 My first lucky break was in 1946 when my father returned from the war and his job took him to Nottingham. He bought a nice house in a middle-class suburb called West Bridgeford and I would cycle many miles each day to and from the Grammar School, which was middled sized. At the end of the fifth form, the year when most of the students left and entered the job market, I asked my friends what they planned to do, and they all decided to stay on into the sixth-form, which prepares you for the A-level exams and possible entry to college. Until that time I had never considered going to university, I had expected to leave school at sixteen and work in a bank or an insurance company. My father was very supportive, even though my staying at school would be a drain on his finances. Sixth-form takes two years and I concentrated on Pure Mathematics, Applied Mathematics, and Physics. A decisive event took place, I believe, towards the end of the first year. In one of the classes we all had to stand up and state our planned career. By then I realized that I was quite good at mathematics, but I did not want a career in it, which I thought of as being a school teacher. I preferred to use mathematics in some practical fashion and thought that meteorology sounded promising. In those days I stuttered somewhat and when my turn came to stand up, I tried to say “meteorology” but found I could not get the word out, so I just said “statistics,” thereby determining my future path.  It so happened that a particular strength at the West Bridgeford Grammar School was the quality of the mathematics teachers. Mr. Bradshaw and Mr. Midgley were both very well trained and I was told they had firsts at Oxford and/or Cambridge. They were still very enthusiastic about the subject and were very able to pass on this enthusiasm and their knowledge to the students. The fact that our sixth-form produce several First Class Degrees, several Ph.D’s, and at least four Professors is largely due to this strong background in mathematics.  Lucky break #2 Applying to universities was a difficult process in those days as quite a small percentage of British youth went to university. Finding suitable courses that also had available places was not easy. However, I was told that the University of Nottingham was just starting the first joint degree in Mathematics and Economics which sounded ideal for my interests, so I applied and was quickly accepted. As far as I could tell, I was the first person anywhere in my family tree to go to university.  My courses in the first year at Nottingham were one third economics consisting of micro and national accounts taught by André Gabor and Brian Tew, with a lot of ability and enthusiasm. I also took large doses of pure and applied mathematics. To my delight I found pure mathematics not so difficult because of my training at Grammar School. At the end of the first year I switched to a Mathematics Degree and I eventually obtained my First. My only formal training in economics was in the first year where I attempted to reformulate what I was being taught in pure mathematical terms, but was usually unsuccessful.  Lucky break #3 On completing my degree I started a Ph.D. in statistics, although I knew very little about the topic. My supervisor was Professor Harry Pitt, who was an excellent pure mathematician and probabilist. He taught me by going through Harold Cramer’s book on Mathematical Statistics which is very good text but makes no mention of data. I needed to find a thesis topic and I wanted something of relevance to economics. I went to our university library and found that they had only one book on Economic Time Series by H.T. Davis. I thought that this was both an appropriate topic and also that there must be plenty of opportunities, as there were so few books in the area. I still think that my judgement, made at the age of 22, was correct.  Lucky break #4 The next structural break in my life occurred after I had been doing research for just six months. As background, it is important to know that this was a time when British universities were expanding rapidly, particularly in certain fields which included statistics. Nottingham had received funding for a junior lecturer in statistics and advertised the position, but were embarrassed by having only a single applicant, even though he was from Cambridge and was very well qualified. The professors of the Mathematics Department asked me to apply and my initial reaction was to say no to the idea as I was not qualified and knew that I had little understanding of the area. They responded that I would not get the position anyway but it would be good experience for me to go through with the interview and be helpful to them, so how could I refuse? The selection committee was chaired by the Vice-Chancellor along with about fifteen full professors. The Vice-Chancellor and I had fought over certain undergraduate topics the previous year and my position had prevailed, so I knew for certain that did not help my chances of getting the job. As I knew that I would not get the position I was not at all nervous, and the Vice-Chancellor started the proceedings by making a kind remark about our previous battles and I actually enjoyed the interview. The other candidate came before the committee, got into a strong fight with the Vice-Chancellor, and did not make a good impression. The result was that I got the job. I was just 23 when I started the position in 1956 with what had to be a record low knowledge of statistics and no established teaching ability. What made it more difficult was that some of the students, returning from Army service (sometimes with battle experience) were older than me and had strong ambitions to learn and to get on with their life. I always found it difficult to accept that they should call me “sir.”  One advantage was that I was the only person in the university calling himself a “statistician,” so that I was visited by workers from many fields (geography, history, psychology, chemistry, and economic history) asking me questions. Each data set presented unique problems and being confronted with them provided excellent training for a young statistician that many beginners would not get these days with such emphasis on specialization. I even got a few publications from these studies. An even better outcome came from my work with Professor David Chambers, a good economic historian who would often send his research assistant, Patricia, over to ask me questions. She said “yes” to the right one from me and we were married in 1960, which is a significant part of “Lucky Break #4.”  In 1959 I obtained a Ph.D. on “Testing for Non-stationary” and applied for a Harkness Fellowship of the Commonwealth Fund. Apparently the Harkness family made a fortune selling saddles and leather wear for horses at the turn of the twentieth century but realizing that that industry may have a limited future, they lent a large sum of money to a friend called Rockefeller, who was having a problem with his oil company. From that investment the family became extremely wealthy, but died out in the mid-century, leaving large funds to various good projects, mostly medical research. Part of the funds went to the Harkness Fellowships, which were for about twenty young people from the United Kingdom, Australia, and New Zealand, mostly from academic subjects but also included artists, writers, and film makers. Each could spend either one or two years in the United States at any location they chose, being given sufficient funds to support themselves. In addition they had to agree to spend one month traveling around the country in December and three further months in the summer, again with all expenses paid.  Lucky break #5 I wrote to many major universities and had two positive responses, one from the Cowles’ Commission at Yale, and the other from Morgenstern’s Econometric Research Project at Princeton. As Oscar Morgenstern asked me to join his new “Time Series Project” I accepted with enthusiasm for a one year period. Getting the Harkness was certainly a lucky break as the fellowships were highly competitive and rarely went to candidates outside Oxford or Cambridge, and I was especially fortunate to go to Princeton.  When I arrived I found that the “Time Series Project” consisted of myself and Michio Hatanaka, an excellent econometrician who had recently finished a book on “Input-Output Analysis.” He had done the calculations on the original Von Neumann computer at the Institute for Advanced Studies, which is a story in itself. However, Morgenstern had a specific project for us. His close friend, J. Von Neumann, a truly great mathematician, had told him that economists should be using the Fourier methods with their data, so Morgenstern had organized with John Tukey, a very famous Princeton statistician, to teach us how to do that. Tukey has a distinctly original way of teaching. We provided the data, he would describe what calculations he wanted us to do on the computer, Michio would do the programming and produce the results, and I would write mathematically what we were doing. John Tukey would then interpret the results and suggest a new set of calculations. This continued weekly for most of the academic year, by which time we had learnt a lot about filtering, the spectrum for a single series, and the cross-spectrum for a pair of series. This last topic was one that Tukey has been working on and was totally new. Michio and I realized that we had enough material for a book but we told John Tukey that we could not try to publish until he published his results. He said he was far too busy doing new research to publish, and that we should just go ahead, which we did. The book “Spectral Analysis in Economic Time Series” by Michio and myself appeared from Princeton University Press in 1969 and sold over three thousand copies, much to everyone’s surprise. We tried to dedicate the book to Morgenstern and Tukey, but they would not allow it.  At the end of my academic year at Princeton, Pat and I were married in the Princeton University Chapel and we used the Harkness funds to finance a trip around the United States as an extended camping trip and honeymoon. After twelve thousand spectacular miles we still liked each other and I had acquired a beard.  I returned to Princeton the next two summers to work on stock market data. Two papers appeared in the journal *Kyklos*, on testing the random walk theory using spectral methods. In one of these, with Mike Godfrey and Oscar Morgenstern, we claimed the first published diagram in economics that was generated by a computer. It appeared in a cathode ray tube and was then photographed. During this period, in 1963, I wrote a very short paper on “The Typical Spectral Shape of An Economic Model.” It was quickly submitted to *Econometrica* where it was accepted two years later with no referee reports, and essentially published three and a half years after submission. It has been cited often and I learned that a simple observation is enough to produce a successful paper, but you need patience with its appearance. I also published my second book, on commodity markets, written with Walt Labys, who left Harvard to study with me in Nottingham. A book with Morgenstern on the stock market and the one with Labys both appeared in 1970.  During this period I was also involved with André Gabor on some practical price research. To get data to test our theories and estimate models, we arranged with local supermarkets to conduct experiments in which we altered prices of popular products and recorded the change in sales. I believe that more economic micro-theory could be better tested by doing real world experiments rather than believing such an approach is impossible.  Lucky break #6 Around this time I thought that I should try to find a new line of research. In 1968 George Box and Gwilym Jenkins sent an advanced copy of their book “Time Series Analysis, Forecasting and Control” (published in 1970) and asked me for comments. I realized that I knew very little about forecasting and nothing about control. I decided that forecasting had great potential and applied for a grant to get a post-doctoral student to join me. The application was successful and I advertised around the world. I was rather surprised to get only one application, from Paul Newbold – a student of George Box, but one is all you need if the applicant is ideal. We were compatible, with a lot of common knowledge, but we had each been trained quite differently. He also proved to be a good writer, having received a good British education. Together we started a five year period of intense research, mostly on forecasting, leading to the publication of our book “Forecasting Economic Time Series” (Academic Press, 1976, second edition, 1986) which was widely used internationally in graduate courses.  During this period Paul and I conducted a small simulation study showing that if you take a pair of smooth, “highly autocorrelated” time series that are independent and put them into an ordinary least squares (OLS) regression, a surprisingly large number of times an incorrect, spurious, relationship is suggested by the standard statistical procedure. As many major economic publications used this type of analysis a lot of reconsideration of previous results had to be reconsidered. The size of the simulation was quite substantial at the time (1974) but would now be laughable. It shows that the usefulness of the result of a computer exercise depends on the quality of the question asked rather than the amount of computing undertaken.  Lucky break #7 In 1973 I was offered a professorship at the University of California, San Diego. Although I was certainly not unhappy at Nottingham, I had been there over twenty years from starting undergraduate studies to Professor of Applied Statistics and Econometrics and I thought that a change of scene was worth considering. We first decided to go for five years to see if we enjoyed California. We arrived in San Diego in August 1974 and several friends wrote advising me “not to retire,” meaning do not spend all my time enjoying the sun and sand. By now the family consisted of Pat and myself, plus our children Mark William John, then aged ten and our daughter Claire Amanda Jane, aged six. They both eventually got their undergraduate degrees from California universities. Mark became a computer software developer and some of his earlier work was included in a movie that starred a future governor of California. Claire obtained her MA from Stanford in Molecular Biology and is now a science writer.  When I joined the Department of Economics at UCSD the theory group was already very strong but I was the only active research econometrician, although Paul Newbold came for the first year as a visitor to finish our book. That was soon to change.  Lucky break #8 In 1975 I was on a committee in Washington, DC chaired by Arnold Zellner. It discussed the organization of a conference on seasonal adjustment. [Robert Engle](https://www.nobelprize.org/nobel_prizes/economics/laureates/2003/index.html), then at MIT, was on the same committee and during a break asked if I knew anywhere looking for a time series econometrician. I said that we were, so he visited, liked what he saw, and joined us. A year or so later Hal White came for a six month visit from Rochester, decided that we had the better climate, and accepted an offer. That is how a world ranked (top three) econometric group is formed. Take three active workers who interact, mix in a group of good students, and later add Jim Hamilton, Graham Elliott, and Allan Timmermann, and in under thirty years you produce a couple of Nobel Prizes, at least.  The students come from all over the world, and some started getting positions at good universities, which attracted further excellent students to us. If I start naming the better students I will only disappoint those that I leave out as there are so many that deserve mention. We claim to not only get good students, but also to have a particularly high value added.  Over the years we worked on a variety of questions including seasonal adjustment, aggregation, and several new types of time series models. These included the fractionally integrated model, done with Roselyn Joyeux, which has a so-called “long-memory property” and provided a discrete-time form of a continuous time model discussed by Benoit Mandelbroit. This whole area has resulted in a lot of interest by statisticians. Also considered was a variety of nonlinear models, including the bilinear model which produced a small book with an Australian visitor, Allan Anderson. Here the mathematics proved of interest but the models do not seem to be of great practical importance in economics. I also observed that a series could be serially uncorrelated but could still be forecast, nonlinearly, from its past. This produced a paper on “forecasting white noise” which produce reactions of surprise from those who thought the economic series were necessarily multivariate Gaussian processes. One observation that comes from this work was that squares of white noise series were not always white noise. I discussed this with Rob Engle and it might just have been the rolling pebble that much later produced the avalanche known as ARCH. Rob returned from a year at LSE with the ARCH model which we discussed frequently. As I was on the committees of his many students in the area I learnt a lot about its development.  One of the benefits of joining an American university was the availability of sabbatical leaves plus long summer breaks when one could travel. The family made good use of these opportunities, plus some nice requests, and we spent lengthy periods at Oxford (Nuffied and All Souls), Cambridge (Trinity), Australian National University (Canberra), Aarhus (Denmark), and Victoria University (Wellington, New Zealand). At the last of these we had a splendid view of Haley’s Comet, especially as the city shut off all the street lights for a couple of hours each night.  I find that traveling is usually enjoyable and certainly broadening both in general knowledge and also through cross-fertilization of ideas. Over my career I have visited over thirty countries.  Lucky break #9 The discovery, or invention, of cointegration was discussed in my Nobel Lecture and was a direct result of talking with a scholar from another university. It has an even bigger impact that causality, but cointegration was much less controversial and had more important applications.  The original theory of cointegration was from linear processes whereas causality theory had no such constraints. In 1993 Timo Teräsvirta and I wrote an overview of the field of nonlinear time series in economics which appears to have been helpful to people entering the field. This book appeared in the “Advanced Texts in Econometrics” series published by Oxford University Press. This series was edited by Grayham Mizon and myself, and was suggested by Andrew Schuler. Grayham and I agreed to edit the series provided that the books would be issued in both hard cover and paperback versions, and this seems to have been a successful strategy.  One of my latest projects involved analysis of a panel of data on the economy of regions in the Brazilian Amazon based on data collected every five years. The project was concerned with the dynamics and economics of the deforestation process going on in the Amazon forest which covers a huge area. With the help of a one year NSF grant a group was assembled which included Diana Weinhold (a former student) and Lykke Andersen visiting from the University of Aarhus in Denmark. We had available a very useful data set organized by Eustaquio Reis from Rio de Janeiro and built what seemed to be a useful model. The results were published in the book “The Dynamics of Deforestation and Economics Growth in the Brazilian Amazon,” by L. Anderson, *et al.*, Cambridge University Press, 2003. This was an enjoyable and potentially important project from which I learnt a great deal.  As a schoolboy, aged around ten and living in Cambridge, England, I decided I did have a mild ambition, to see the year 2000. To achieve that I had to live to the age of sixty-six. In 1944, when I thought about this, such an age was by no means assured, many people did not make their mid-sixties. In 2000 Pat and I were living in La Jolla, next to the UCSD campus and quite near the ocean, Mark and his wife Kate were living in Sacramento in central California, and Claire was about to move back to England.  In 2003 I decided to retire at the age of almost 69 after about 48 years of teaching. I was willing to give up my teaching and administrative duties in exchange for a reduced income and greater freedom of movement. My retirement date was July 31, 2003 and my long-term colleague, Rob Engle, retired from the department on the same day and moved permanently to New York. Pat and I started a visit to Christchurch on the South Island of New Zealand in that October. We greatly enjoy spending time in the Department of Economics at the University of Canterbury where we receive a warm welcome. We particularly like the spectacular Spring in the area.  Lucky break #10 At 3:00 am on October 8, 2003 a telephone call from Sweden turned our world upside down, and it will never return to normality. My colleagues at Nottingham, San Diego, and Canterbury were greatly excited, as were we. It seems all my lifetime friends, students, and acquaintances enjoyed the Prize as much as I did, which was terrific.  My story ends with a recipe for success. Do not start too high on the ladder, move to a good but not top university, work hard, have a few good ideas, chose good collaborators (I had over eighty in my career), attract some excellent students, wait twenty years or so, and then retire. It worked for Rob and I.  Addendum I have been asked if I consider myself to be naturally lucky. Certainly in my career the breaks have fallen my way. Sometimes I got a scholarship or an appointment at the right time, or I fell into good situations and others, particularly Rob, Hal, and Paul, accepted offers to join me. Even in general life I feel that I am often fortunate, coming across useful information or scarce parking spaces. I have never tried to use the ability by gambling but I do have friends who call on my name when trying to find somewhere to park their car, and they say that it works, even in Florence! I would like to thank my assistant, Mike Bacci, for handling all of the work involved with the Nobel presentations and essays, and much else. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0844 |
| Interview |  |
| Q39 | To my knowledge none of you has studied economics as undergraduates and this means that you have a background in another discipline. How did that affect your research when you were doing? |
|  | Robert Engle: In fact, when I was an undergraduate, I was a physics major and I had lots of room mates and friends who studied economics and they kept telling me I should take an economics class so I could find out what this was really like. So, my senior year I had a choice between taking a religion class and economics class because I had an extra spot, and I took economics and it was fascinating. That was my first actual experience and that’s probably why I ended up doing the switch. But then when I did my switch from physics to economics I took pretty much a full set of undergraduate classes on a listening basis while I was taking the graduate courses.  Clive Granger: I had just one third of my first undergraduate year in economics which I enjoyed and it was a very non-mathematical approach. I kept trying to translate into my mathematics that I was more used to and I had trouble doing that, but I enjoyed what I did. I always felt that it was both a disadvantage not to know economics and an advantage because economists think about things differently than everybody else, but also when I came to economics proper I came to it in a different way than all my colleagues did and I think that added an extra dimension to the approach. Then in pair with economists together we would then have a wider way of looking at things and so it was both a disadvantage and an advantage. |
| Q3 | Has any particular teacher really been important to you? As an undergraduate or graduate? |
|  | Clive Granger: Yes, when I was undergraduate my professor was Brian Tew who was a very good macroeconomist in England and he was one of these people who was totally non-analytical but he was appreciative of mathematical methods. He was very /- – -/ believed the future was in a more mathematical, technical approach to economics. At Princeton I worked with Oscar Morgenstern and he was a great man and had very wide interests and again he didn’t teach me any game theory, I learnt some again just being around him and others. But I found that his leadership was just magnificent.  Robert Engle: I guess the teachers that I would like to mention were my couple of teachers at Cornell in graduate school. The very first day I arrived there I met my thesis supervisor whose name is Ta Chung Liu or as you would say in Chinese Liu Ta Chung and he was a wonderful mentor to me. He was interested in econometrics as a tool to do real problems. He was building models of the US economy and the Chinese economy and you could really see that this was a tool that could solve problems. Then two other men that were really very important were Berndt Stigum who is now in Oslo and was really a great inspiration in teaching a lot of the advanced econometrics that I later used, and John Fei who really gave me my introduction to some of the more traditional economic areas. |
| Q8 | Some researchers work in teams and some others do more alone work, how does a usual workday look for you? |
|  | Robert Engle: I write a surprisingly large number of papers with my students. I like taking a graduate student and just starting on a project at early stages of their career and then slowly it develops and we get a little further and little further and little further and by the time maybe two years has gone by we have a paper, or maybe three years. Then pretty soon maybe there’s a new important idea that they do. That’s one way I do it. But the other way, which is a wonderful way to do it, is to collaborate with colleagues like Clive and we’ve had an awful lot of wonderful times just talking about big problems and how could you solve this and how could we solve that? I’ve seen this thing and what do you make of this? I don’t know what I make of that, what do you make of it? We just kind of go round and round in circles and all of a sudden there’s something new there.  Clive Granger: I second that totally. Occasionally I spend time just by myself playing with a model and just trying different alternative approaches. But I’ve had over 80 collaborators with my publications over the years, so you see I get along well with people and together we produce. When I was at Nottingham recently, I was there just by myself for quite some years and then Paul Newbold arrived and the difference in my ability to produce and to get my ideas was greatly enhanced by two of us, two of us was much better than two single individuals. Then I moved to San Diego and then I had a year there where I was by myself and then Rob came along and suddenly everything blossomed and we could interact and be very productive. Then we have some very great students come along and the whole thing was just built up. So the answer is there’s no single answer, we do both individual work to initially begin a project but then the discussion and interaction and throwing ideas out to people is very nice. One thing we did at San Diego was to have a regular lunchtime meeting every Tuesday of all the econometricians and we always tried to make that meeting and it was a totally un-programmed meeting, we just would go along and see what one talk about. It was a great help to people, sometimes we’d talk about nothing. just sport or something, but other days it was a really interesting meeting and visitors would come along to it and that was I think a helpful thing to do.  Robert Engle: We also did some gossip at these meetings.  Clive Granger: Don’t tell that!  Robert Engle: Who was running around with whose wife or switching universities or what was happening so … a lot of things happened! |
| Q67 | According to Nobel you are supposed to have done a discovery to be awarded the prize and I think this time it is obvious that both cointegration and ARCH is really a discovery, but when did you find out or how did you find out that this was really something that could change the way statistics and also financial econometrics could be? |
|  | Clive Granger: Certainly the cointegration idea was one of these things that once it became clear that we understood what we’d found and what the implication of it were to other things that it solved many problems. Things that we were being puzzled about beforehand, we were seeing papers which were inconclusive and people didn’t know quite what to do with their data, suddenly we found we found we could solve all those problems. It became clear how to explain all many things. I think almost within days we both realised that this was going to be an important discovery. Exactly how you sell that is another question but I think we had no doubt how important are the question but we were excited by it and we quickly told people about it and other people were excited by it. Word spread quite quickly on that, on this cointegration.  What about ARCH?  Robert Engle: Arch really came about when I was on leave on sabbatical from San Diego at the London School of Economics but it was stimulated by things we had been doing in San Diego beforehand and it was a project I was working on and kind of in the back of my mind. I had a project in the front of my mind that I felt like I had to do first before I could really think about this time-varying volatility question. Then my wonderful student Mark Watson wrote me a letter I guess, couldn’t have been email in those days, saying that he really wanted to figure out the first problem that I felt I had to solve first for his thesis. I thought to myself, hm, if I let him work on that then I can try to work on this other thing that I’m interested in and all of a sudden I had days of empty time ahead of me and I had these ideas that I wanted to build models of time-varying variances and I had a lot of external stimulation from people at the London School of Economics and all of a sudden the idea came, got put together and it was just very exciting. I did feel like that was important too although that is an idea that it seems to me it took a lot more time for the profession to realise it was interesting than the cointegration idea where everybody just jumped on it. |
| Q10 | You seem to talk a lot about students and I wonder what would the dream student be for you? |
|  | Robert Engle: You know students are very deceptive, it’s not necessarily the ones that do the best in class that are best to work with. What I find is that there is this transition that students go through, that at the beginning you’re explaining things to them. I always like them to have taken my class so that we’re talking a common language. Then at the beginning I keep explaining to them and I always like talking about things I don’t understand, how could we think about it, this way or that way? They have something to say or they don’t have something to say. Then in a few short months all of a sudden they’re explaining things to me or maybe a year or two later: Could you explain that to me again? They’ll tell me again sort of why this is the right answer. That’s the dream student for me.  Clive Granger: I find some students are very compatible, that you talk to them and you’re very comfortable talking to them. They’re just naturally intelligent and they absorb what’s being said and initially they’re taking everything from you. Then slowly your relationship changes and you find that they’re doing more than you asked, they’re bringing back to you more than you expected and they have their own results which you say Wow, I didn’t realise that was going to occur from this problem. That’s great and you’ve established somebody on the road to doing good research and you know they’re going to have good ideas from thereon.  Robert Engle: Another thing to say along the same lines is that when you’re working on something that’s new and actually that’s mostly what we’re doing, you’re headed into the uncharted area so you don’t really know what’s going to happen and some students get stuck a lot and you spend all your time with them trying to get them unstuck. Other people seem to avoid getting stuck, they figure out a way around it or get a good idea sometimes that you sort of think back on, you say Wow, you know we really made a lot of progress, versus in the first case Oh, there are all these roads blocks, road blocks, road blocks, we couldn’t get anywhere. I think that’s actually an important quality that students can have and we have to have ourselves as we develop in these areas. |
| Q49 | As I see it, it has been a fantastic develop in econometrics during say the last 20, 30 years, this is not the first prize in econometrics. What would you say, computers, better computers, has that contributed to this or is there something else? |
|  | Clive Granger: We discussed this in the past when computers first became rather fast, the data became more plentiful we wondered whether there were going to be lots of fantastic results appearing. New things about the economics were appearing and it hasn’t really quite happened as to the extent we expect. We can now think of quite different ways to approach econometrics, we’re not having the constraints we used to have on computing and often data availability now. Whereas at one time it was an effort to do analysis with 100 pieces of data for a single series, now if you go out with 400 pieces of data and 500 series at the same time and have this enormous paper and how much better that is than the one series done properly is not always so clear. We learn a bit more but not a huge amount more, so I think we haven’t yet learnt how to best summarise and how to best search this extra tool. It’s going to get worse because we start dealing with multi-freight distributions next and even to describe some of these things and to know, we’ll have a lot of results we can’t even look at. It’ll be too complicated for anyone to look at the results, they’ll be on the computer, we can ask the computer question about what the results say to us but they’ll be too complicated, they’re just too numerous. We have to learn how to deal with that situation and that’s exciting, in my opinion that’s something which the physicists have faced for many years, they have multi-dimensions and too many data points. We haven’t quite got there yet.  Robert Engle: In time series one of the things that happened is that the big macro-models were pretty much static models or almost static models. They had very simple kinds of dynamics in them and when people started to look at how well they forecast especially short run forecasting they found out that very simple time series models could out-perform the static which I would call traditional macro econometric models. By thinking about this short-run forecasting problem it gives you a different way of formulating econometric models. Econometric models are formulated very largely in terms of given what we know today what’s tomorrow likely to look like, so you build up these models in what we call recursive fashion. That point of view gives rise to an awful lot of these developments and time series, so it maybe just that change in formulation that’s given rise to a variety of different things. |
| Q28 | Doing research is certainly good exercise for the brain, what other kind of exercise do you do? |
|  | Robert Engle: You’ve been peeking! We both do a lot of other kinds of exercise but one of the things that I love to do is ice dancing and you’ve probably seen some of it here in this Nobel Week, film footage and I’ve been adult competitive ice dancer for many, many years and it’s a wonderful escape from my economics. My skating friends don’t know that I do economics and my economics friends don’t know that I skate, it’s like you just change your personality and that lets your brain relax and other things happen and then when you come back to work its fresh.  Clive Granger: I’ve nothing as spectacular as this to discuss but every day I try and walk on the beach for half an hour, one of the advantages of San Diego it’s very relaxing on the beach. The sound’s nice and the whole atmosphere’s nice and in the summer the water’s quite warm in San Diego so all 25 years, every day in the summer I’ve surfed, I’ve body surfed, I call it body surfing it’s really not that spectacular but it’s fun and it’s very good exercise.  Robert Engle: You take a boogie board don’t you?  Clive Granger: No, I just use my body, just lie on the surf. It’s fun and I really enjoy it and exercise that you enjoy doing you do, and you have a good time and you come back and you’re fitter from doing it.  Don’t you enjoy art too?  Clive Granger: Yes, I go to art galleries quite a lot, whenever I’m in a city on a conference I always put half a day aside if I can and go to that local art gallery and look, I’m not expert in art I just like looking at art. It’s relaxing.  Do you talk a lot about economics at home or what kind of discussions?  Robert Engle: My wife’s not much of an economist; I don’t think Pat is either.  Clive Granger: Not at all, no. How to spend the household money is one of the problems!  Robert Engle: The closest we come to an economic discussion is talk about how was your day and all this sort of things. Then I’ll say something like You know, I had a good idea today and she’ll say Oh really, oh that’s great. Because a good idea to me is so exciting and she knows it’s exciting and she appreciates that, and it feels like a real accomplishment to her as well as to me.  Clive Granger: I’m told off for working too long and too hard and I go away in my study and do my own thing but that’s all, we don’t discuss economics at all. |
| Q74 | But you have children both of you and are they economists or academics? |
|  | Clive Granger: My daughter is on the edge of an academic career. She’s a science writer and she’s thinking about working in the university doing administrative type academic jobs. My son’s a programmer or developer of animation programmes, so he’s not academic at all but quite successful.  Robert Engle: My daughter took a class at Princeton from Helen Blinder and was very interested in it and I got my anticipation up a little bit but she decided that wasn’t what she wanted to do. She’s just finished her PhD in cognitive psychology at UCLA and I think wants to be a professor so I feel like there’s some carry over there even though it’s not economics. My son took an economics class at Williams and after he was done he said, You know Dad I really learned something important in this class – that I’m never going to be an economist! So, he’s more on the creative side and he wants to be an actor and has been doing a lot of high-level photography and pursuing a more creative career. |
| ID | 0845 |
| Biographical | Early years I was born in Tel Aviv, in what is now Israel, in 1934, while my mother was visiting her extended family there; our regular domicile was in Paris. My parents were Lithuanian Jews, who had immigrated to France in the early 1920s and had done quite well. My father was the chief of research in a large chemical factory. But although my parents loved most things French and had some French friends, their roots in France were shallow, and they never felt completely secure. Of course, whatever vestiges of security they’d had were lost when the Germans swept into France in 1940. What was probably the first graph I ever drew, in 1941, showed my family’s fortunes as a function of time – and around 1940 the curve crossed into the negative domain.  I will never know if my vocation as a psychologist was a result of my early exposure to interesting gossip, or whether my interest in gossip was an indication of a budding vocation. Like many other Jews, I suppose, I grew up in a world that consisted exclusively of people and words, and most of the words were about people. Nature barely existed, and I never learned to identify flowers or to appreciate animals. But the people my mother liked to talk about with her friends and with my father were fascinating in their complexity. Some people were better than others, but the best were far from perfect and no one was simply bad. Most of her stories were touched by irony, and they all had two sides or more.  In one experience I remember vividly, there was a rich range of shades. It must have been late 1941 or early 1942. Jews were required to wear the Star of David and to obey a 6 p.m. curfew. I had gone to play with a Christian friend and had stayed too late. I turned my brown sweater inside out to walk the few blocks home. As I was walking down an empty street, I saw a German soldier approaching. He was wearing the black uniform that I had been told to fear more than others – the one worn by specially recruited SS soldiers. As I came closer to him, trying to walk fast, I noticed that he was looking at me intently. Then he beckoned me over, picked me up, and hugged me. I was terrified that he would notice the star inside my sweater. He was speaking to me with great emotion, in German. When he put me down, he opened his wallet, showed me a picture of a boy, and gave me some money. I went home more certain than ever that my mother was right: people were endlessly complicated and interesting.  My father was picked up in the first large-scale sweep for Jews, and was interned for six weeks in Drancy, which had been set up as a way station to the extermination camps. He was released through the intervention of his firm, which was directed (a fact I learned only from an article I read a few years ago) by the financial mainstay of the Fascist anti-Semitic movement in France in the 1930s. The story of my father’s release, which I never fully understood, also involved a beautiful woman and a German general who loved her. Soon afterward, we escaped to Vichy France, and stayed on the Riviera in relative safety, until the Germans arrived and we escaped again, to the center of France. My father died of inadequately treated diabetes, in 1944, just six weeks before the D-day he had been waiting for so desperately. Soon my mother, my sister, and I were free, and beginning to hope for the permits that would allow us to join the rest of our family in Palestine.  I had grown up intellectually precocious and physically inept. The ineptitude must have been quite remarkable, because during my last term in a French lycée, in 1946, my eighth-grade physical-education teacher blocked my inclusion in the Tableau d’Honneur – the Honor Roll – on the grounds that even his extreme tolerance had limits. I must also have been quite a pompous child. I had a notebook of essays, with a title that still makes me blush: “What I write of what I think.” The first essay, written before I turned eleven, was a discussion of faith. It approvingly quoted Pascal’s saying “Faith is God made perceptible to the heart” (“How right this is!”), then went on to point out that this genuine spiritual experience was probably rare and unreliable, and that cathedrals and organ music had been created to generate a more reliable, ersatz version of the thrills of faith. The child who wrote this had some aptitude for psychology, and a great need for a normal life.  Adolescence The move to Palestine completely altered my experience of life, partly because I was held back a year and enrolled in the eighth grade for a second time – which meant that I was no longer the youngest or the weakest boy in the class. And I had friends. Within a few months of my arrival, I had found happier ways of passing time than by writing essays to myself. I had much intellectual excitement in high school, but it was induced by great teachers and shared with like-minded peers. It was good for me not to be exceptional anymore.  At age seventeen, I had some decisions to make about my military service. I applied to a unit that would allow me to defer my service until I had completed my first degree; this entailed spending the summers in officer-training school, and part of my military service using my professional skills. By that time I had decided, with some difficulty, that I would be a psychologist. The questions that interested me in my teens were philosophical – the meaning of life, the existence of God, and the reasons not to misbehave. But I was discovering that I was more interested in what made people believe in God than I was in whether God existed, and I was more curious about the origins of people’s peculiar convictions about right and wrong than I was about ethics. When I went for vocational guidance, psychology emerged as the top recommendation, with economics not too far behind.  I got my first degree from the Hebrew University in Jerusalem, in two years, with a major in psychology and a minor in mathematics. I was mediocre in math, especially in comparison with some of the people I was studying with – several of whom went on to become world-class mathematicians. But psychology was wonderful. As a first-year student, I encountered the writings of the social psychologist Kurt Lewin and was deeply influenced by his maps of the life space, in which motivation was represented as a force field acting on the individual from the outside, pushing and pulling in various directions. Fifty years later, I still draw on Lewin’s analysis of how to induce changes in behavior for my introductory lecture to graduate students at the Woodrow Wilson School of Public Affairs at Princeton. I was also fascinated by my early exposures to neuropsychology. There were the weekly lectures of our revered teacher Yeshayahu Leibowitz – I once went to one of his lectures with a fever of 41 degrees Celsius; they were simply not to be missed. And there was a visit by the German neurosurgeon Kurt Goldstein, who claimed that large wounds to the brain eliminated the capacity for abstraction and turned people into concrete thinkers. Furthermore, and most exciting, as Goldstein described them, the boundaries that separated abstract from concrete were not the ones that philosophers would have set. We now know that there was little substance to Goldstein’s assertions, but at the time the idea of basing conceptual distinctions on neurological observations was so thrilling that I seriously considered switching to medicine in order to study neurology. The Chief of Neurosurgery at the Hadassah Hospital, who was a neighbor, wisely talked me out of that plan by pointing out that the study of medicine was too demanding to be undertaken as a means to any goal other than practice.  The military experience In 1954, I was drafted as a second lieutenant, and after an eventful year as a platoon leader I was transferred to the Psychology branch of the Israel Defense Forces. There, one of my occasional duties was to participate in the assessment of candidates for officer training. We used methods that had been developed by the British Army in the Second World War. One test involved a leaderless group challenge, in which eight candidates, with all insignia of rank removed and only numbers to identify them, were asked to lift a telephone pole from the ground and were then led to an obstacle, such as a 2.5-meter wall, where they were told to get to the other side of the wall without the pole touching either the ground or the wall, and without any of them touching the wall. If one of these things happened, they had to declare it and start again. Two of us would watch the exercise, which often took half an hour or more. We were looking for manifestations of the candidates’ characters, and we saw plenty: true leaders, loyal followers, empty boasters, wimps – there were all kinds. Under the stress of the event, we felt, the soldiers’ true nature would reveal itself, and we would be able to tell who would be a good leader and who would not. But the trouble was that, in fact, we could not tell. Every month or so we had a “statistics day,” during which we would get feedback from the officer-training school, indicating the accuracy of our ratings of candidates’ potential. The story was always the same: our ability to predict performance at the school was negligible. But the next day, there would be another batch of candidates to be taken to the obstacle field, where we would face them with the wall and see their true natures revealed. I was so impressed by the complete lack of connection between the statistical information and the compelling experience of insight that I coined a term for it: “the illusion of validity.” Almost twenty years later, this term made it into the technical literature (Kahneman and Tversky, 1973). It was the first cognitive illusion I discovered.  Closely related to the illusion of validity was another feature of our discussions about the candidates we observed: our willingness to make extreme predictions about their future performance on the basis of a small sample of behavior. In fact, the issue of willingness did not arise, because we did not really distinguish predictions from observations. The soldier who took over when the group was in trouble and led the team over the wall was a leader at that moment, and if we asked ourselves how he would perform in officer-training, or on the battlefield, the best bet was simply that he would be as good a leader then as he was now. Any other prediction seemed inconsistent with the evidence. As I understood clearly only when I taught statistics some years later, the idea that predictions should be less extreme than the information on which they are based is deeply counterintuitive.  The theme of intuitive prediction came up again, when I was given the major assignment for my service in the Unit: to develop a method for interviewing all combat-unit recruits, in order to screen the unfit and help allocate soldiers to specific duties. An interviewing system was already in place, administered by a small cadre of interviewers, mostly young women, themselves recent graduates from good high schools, who had been selected for their outstanding performance in psychometric tests and for their interest in psychology. The interviewers were instructed to form a general impression of a recruit and then to provide some global ratings of how well the recruit was expected to perform in a combat unit. Here again, the statistics of validity were dismal. The interviewers’ ratings did not predict with substantial accuracy any of the criteria in which we were interested.  My assignment involved two tasks: first, to figure out whether there were personality dimensions that mattered more in some combat jobs than in others, and then to develop interviewing guidelines that would identify those dimensions. To perform the first task, I visited units of infantry, artillery, armor, and others, and collected global evaluations of the performance of the soldiers in each unit, as well as ratings on several personality dimensions. It was a hopeless task, but I didn’t realize that then. Instead, spending weeks and months on complex analyses using a manual Monroe calculator with a rather iffy handle, I invented a statistical technique for the analysis of multi-attribute heteroscedastic data, which I used to produce a complex description of the psychological requirements of the various units. I was capitalizing on chance, but the technique had enough charm for one of my graduate-school teachers, the eminent personnel psychologist Edwin Ghiselli, to write it up in what became my first published article. This was the beginning of a lifelong interest in the statistics of prediction and description.  I had devised personality profiles for a criterion measure, and now I needed to propose a predictive interview. The year was 1955, just after the publication of “Clinical versus statistical prediction” (Meehl, 1954), Paul Meehl’s classic book in which he showed that clinical prediction was consistently inferior to actuarial prediction. Someone must have given me the book to read, and it certainly had a big effect on me. I developed a structured interview schedule with a set of questions about various aspects of civilian life, which the interviewers were to use to generate ratings about six different aspects of personality (including, I remember, such things as “masculine pride” and “sense of obligation”). Soon I had a near-mutiny on my hands. The cadre of interviewers, who had taken pride in the exercise of their clinical skills, felt that they were being reduced to unthinking robots, and my confident declarations -“Just make sure that you are reliable, and leave validity to me”-did not satisfy them. So I gave in. I told them that after completing “my” six ratings as instructed, they were free to exercise their clinical judgment by generating a global evaluation of the recruit’s potential in any way they pleased. A few months later, we obtained our first validity data, using ratings of the recruits’ performance as a criterion. Validity was much higher than it had been. My recollection is that we achieved correlations of close to .30, in contrast to about .10 with the previous methods. The most instructive finding was that the interviewers’ global evaluation, produced at the end of a structured interview, was by far the most predictive of all the ratings they made. Trying to be reliable had made them valid. The puzzles with which I struggled at that time were the seed of the paper on the psychology of intuitive prediction that Amos Tversky and I published much later.  The interview system has remained in use, with little modification, for many decades. And if it appears odd that a twenty-one-year-old lieutenant would be asked to set up an interviewing system for an army, one should remember that the state of Israel and its institutions were only seven years old at the time, that improvisation was the norm, and that professionalism did not exist. My immediate supervisor was a man with brilliant analytical skills, who had trained in chemistry but was entirely self-taught in statistics and psychology. And with a B.A. in the appropriate field, I was the best-trained professional psychologist in the military.  Graduate school years I came out of the Army in 1956. The academic planners at the Hebrew University had decided to grant me a fellowship to obtain a PhD abroad, so that I would be able to return and teach in the psychology department. But they wanted me to acquire some additional polish before facing the bigger world. Because the psychology department had temporarily closed, I took some courses in philosophy, did some research, and read psychology on my own for a year. In January of 1958, my wife, Irah, and I landed at the San Francisco airport, where the now famous sociologist Amitai Etzioni was waiting to take us to Berkeley, to the Flamingo Motel on University Avenue, and to the beginning of our graduate careers.  My experience of graduate school was quite different from that of students today. The main landmarks were examinations, including an enormous multiple-choice test that covered all of psychology. (A long list of classic studies preceded by the question “Which of the following is not a study of latent learning?” comes to mind.) There was less emphasis on formal apprenticeship, and virtually no pressure to publish while in school. We took quite a few courses and read broadly. I remember a comment of Professor Rosenweig’s on the occasion of my oral exam. I should enjoy my current state, he advised, because I would never again know as much psychology. He was right.  I was an eclectic student. I took a course on subliminal perception from Richard Lazarus, and wrote with him a speculative article on the temporal development of percepts, which was soundly and correctly rejected. From that subject I came to an interest in the more technical aspects of vision and I spent some time learning about optical benches from Tom Cornsweet. I audited the clinical sequence, and learned about personality tests from Jack Block and from Harrison Gough. I took classes on Wittgenstein in the philosophy department. I dabbled in the philosophy of science. There was no particular rhyme or reason to what I was doing, but I was having fun.  My most significant intellectual experience during those years did not occur in graduate school. In the summer of 1958, my wife and I drove across the United States to spend a few months at the Austen Riggs Clinic in Stockbridge, Massachusetts, where I studied with the well-known psychoanalytic theorist David Rapaport, who had befriended me on a visit to Jerusalem a few years earlier. Rapaport believed that psychoanalysis contained the elements of a valid theory of memory and thought. The core ideas of that theory, he argued, were laid out in the seventh chapter of Freud’s “Interpretation of Dreams,” which sketches a model of mental energy (cathexis). With the other young people in Rapaport’s circle, I studied that chapter like a Talmudic text, and tried to derive from it experimental predictions about short-term memory. This was a wonderful experience, and I would have gone back if Rapaport had not died suddenly later that year. I had enormous respect for his fierce mind. Fifteen years after that summer, I published a book entitled “Attention and Effort,” which contained a theory of attention as a limited resource. I realized only while writing the acknowledgments for the book that I had revisited the terrain to which Rapaport had first led me.  Austen Riggs was a major intellectual center for psychoanalysis, dedicated primarily to the treatment of dysfunctional descendants of wealthy families. I was allowed into the case conferences, which were normally scheduled on Fridays, usually to evaluate a patient who had spent a month of live-in observation at the clinic. Those attending would have received and read, the night before, a folder with detailed notes from every department about the person in question. There would be a lively exchange of impressions among the staff, which included the fabled Erik Erikson. Then the patient would come in for a group interview, which was followed by a brilliant discussion. On one of those Fridays, the meeting took place and was conducted as usual, despite the fact that the patient had committed suicide during the night. It was a remarkably honest and open discussion, marked by the contradiction between the powerful retrospective sense of the inevitability of the event and the obvious fact that the event had not been foreseen. This was another cognitive illusion to be understood. Many years later, Baruch Fischhoff wrote, under my and Amos Tversky’s supervision, a beautiful PhD thesis that illuminated the hindsight effect.  In the spring of 1961, I wrote my dissertation on a statistical and experimental analysis of the relations between adjectives in the semantic differential. This allowed me to engage in two of my favorite pursuits: the analysis of complex correlational structures and FORTRAN programming. One of the programs I wrote would take twenty minutes to run on the university mainframe, and I could tell whether it was working properly by the sequence of movement on the seven tape units that it used. I wrote the thesis in eight days, typing directly on the purple “ditto” sheets that we used for duplication at the time. That was probably the last time I wrote anything without pain. The paper itself, by sharp contrast, was so convoluted and dreary that my teacher, Susan Ervin, memorably described the experience of reading it as “wading through wet mush.” I spent the summer of 1961 in the ophthalmology department, doing research on contour interference. And then it was time to go home to Jerusalem, and start teaching in the psychology department at the Hebrew University.  Training to become a professional I loved teaching undergraduates and I was good at it. The experience was consistently gratifying because the students were so good: they were selected on the basis of a highly competitive entrance exam, and most were easily PhD material. I took charge of the basic first-year statistics class and, for some years, taught both that course and the second-year course in research methods, which also included a large dose of statistics. To teach effectively I did a lot of serious thinking about valid intuitions on which I could draw and erroneous intuitions that I should teach students to overcome. I had no idea, of course, but I was laying the foundation for a program of research on judgment under uncertainty. Another course I was teaching concerned the psychology of perception, which also contributed quite directly to the same program.  I had learned a lot in Berkeley, but I felt that I had not been adequately trained to do research. I therefore decided that in order to acquire the basic skills I would need to have a proper laboratory and do regular science – I needed to be a solid short-order cook before I could aspire to become a chef. So I set up a vision lab, and over the next few years I turned out competent work on energy integration in visual acuity. At the same time, I was trying to develop a research program to study affiliative motivation in children, using an approach that I called a “psychology of single questions.” My model for this kind of psychology was research reported by Walter Mischel (1961a, 1961b) in which he devised two questions that he posed to samples of children in Caribbean islands: “You can have this (small) lollipop today, or this (large) lollipop tomorrow,” and “Now let’s pretend that there is a magic man … who could change you into anything that you would want to be, what you would want to be?” The answer to the latter question was scored 1, if it referred to a profession or to an achievement-related trait, otherwise 0. The responses to these lovely questions turned out to be plausibly correlated with numerous characteristics of the child and the child’s background. I found this inspiring: Mischel had succeeded in creating a link between an important psychological concept and a simple operation to measure it. There was (and still is) almost nothing like it in psychology, where concepts are commonly associated with procedures that can be described only by long lists or by convoluted paragraphs of prose.  I got quite nice results in my one-question studies, but never wrote up any of the work, because I had set myself impossible standards: in order not to pollute the literature, I wanted to report only findings that I had replicated in detail at least once, and the replications were never quite perfect. I realized only gradually that my aspirations demanded more statistical power and therefore much larger samples than I was intuitively inclined to run. This observation also came in handy some time later.  My achievements in research in these early years were quite humdrum, but I was excited by several opportunities to bring psychology to bear on the real world. For these tasks, I teamed up with a colleague and friend, Ozer Schild. Together, we designed a training program for functionaries who were to introduce new immigrants from underdeveloped countries, such as Yemen, to modern farming practices (Kahneman and Schild, 1966). We also developed a training course for instructors in the flight school of the Air Force. Our faith in the usefulness of psychology was great, but we were also well aware of the difficulties of changing behavior without changing institutions and incentives. We may have done some good, and we certainly learned a lot.  I had the most satisfying Eureka experience of my career while attempting to teach flight instructors that praise is more effective than punishment for promoting skill-learning. When I had finished my enthusiastic speech, one of the most seasoned instructors in the audience raised his hand and made his own short speech, which began by conceding that positive reinforcement might be good for the birds, but went on to deny that it was optimal for flight cadets. He said, “On many occasions I have praised flight cadets for clean execution of some aerobatic maneuver, and in general when they try it again, they do worse. On the other hand, I have often screamed at cadets for bad execution, and in general they do better the next time. So please don’t tell us that reinforcement works and punishment does not, because the opposite is the case.” This was a joyous moment, in which I understood an important truth about the world: because we tend to reward others when they do well and punish them when they do badly, and because there is regression to the mean, it is part of the human condition that we are statistically punished for rewarding others and rewarded for punishing them. I immediately arranged a demonstration in which each participant tossed two coins at a target behind his back, without any feedback. We measured the distances from the target and could see that those who had done best the first time had mostly deteriorated on their second try, and vice versa. But I knew that this demonstration would not undo the effects of lifelong exposure to a perverse contingency.  My first experience of truly successful research came in 1965, when I was on sabbatical leave at the University of Michigan, where I had been invited by Jerry Blum, who had a lab in which volunteer participants performed various cognitive tasks while in the grip of powerful emotional states induced by hypnosis. Dilation of the pupil is one of the manifestations of emotional arousal, and I therefore became interested in the causes and consequences of changes of pupil size. Blum had a graduate student called Jackson Beatty. Using primitive equipment, Beatty and I made a real discovery: when people were exposed to a series of digits they had to remember, their pupils dilated steadily as they listened to the digits, and contracted steadily when they recited the series. A more difficult transformation task (adding 1 to each of a series of four digits) caused a much larger dilation of the pupil. We quickly published these results, and within a year had completed four articles, two of which appeared in *Science*. Mental effort remained the focus of my research during the subsequent year, which I spent at Harvard. During that year, I also heard a brilliant talk on experimental studies of attention by a star English psychologist named Anne Treisman, who would become my wife twelve years later. I was so impressed that I committed myself to write a chapter on attention for a Handbook in Cognitive Psychology. The Handbook was never published, and my chapter eventually became a rather ambitious book. The work on vision that I did that year was also more interesting than the work I had been doing in Jerusalem. When I returned home in 1967, I was, finally, a well-trained research psychologist.  The collaboration with Amos Tversky From 1968 to 1969, I taught a graduate seminar on the applications of psychology to real-world problems. In what turned out to be a life-changing event, I asked my younger colleague Amos Tversky to tell the class about what was going on in his field of judgment and decision-making. Amos told us about the work of his former mentor, Ward Edwards, whose lab was using a research paradigm in which the subject is shown two bookbags filled with poker chips. The bags are said to differ in their composition (e.g., 70:30 or 30:70 white/red). One of them is randomly chosen, and the participant is given an opportunity to sample successively from it, and required to indicate after each trial the probability that it came from the predominantly red bag. Edwards had concluded from the results that people are “conservative Bayesians”: they almost always adjust their confidence interval in the proper direction, but rarely far enough. A lively discussion developed around Amos’s talk. The idea that people were conservative Bayesian did not seem to fit with the everyday observation of people commonly jumping to conclusions. It also appeared unlikely that the results obtained in the sequential sampling paradigm would extend to the situation, arguably more typical, in which sample evidence is delivered all at once. Finally, the label of ‘conservative Bayesian’ suggested the implausible image of a process that gets the correct answer, then adulterates it with a bias. I learned recently that one of Amos’s friends met him that day and heard about our conversation, which Amos described as having severely shaken his faith in the neo-Bayesian idea. I do remember that Amos and I decided to meet for lunch to discuss our hunches about the manner in which probabilities are “really” judged. There we exchanged personal accounts of our own recurrent errors of judgment in this domain, and decided to study the statistical intuitions of experts.  I spent the summer of 1969 doing research at the Applied Psychological Research Unit in Cambridge, England. Amos stopped there for a few days on his way to the United States. I had drafted a questionnaire on intuitions about sampling variability and statistical power, which was based largely on my personal experiences of incorrect research planning and unsuccessful replications. The questionnaire consisted of a set of questions, each of which could stand on its own – this was to be another attempt to do psychology with single questions. Amos went off and administered the questionnaire to participants at a meeting of the Mathematical Psychology Association, and a few weeks later we met in Jerusalem to look at the results and write a paper.  The experience was magical. I had enjoyed collaborative work before, but this was something different. Amos was often described by people who knew him as the smartest person they knew. He was also very funny, with an endless supply of jokes appropriate to every nuance of a situation. In his presence, I became funny as well, and the result was that we could spend hours of solid work in continuous mirth. The paper we wrote was deliberately humorous – we described a prevalent belief in the “law of small numbers,” according to which the law of large numbers extends to small numbers as well. Although we never wrote another humorous paper, we continued to find amusement in our work – I have probably shared more than half of the laughs of my life with Amos.  And we were not just having fun. I quickly discovered that Amos had a remedy for everything I found difficult about writing. No wet-mush problem for him: he had an uncanny sense of direction. With him, movement was always forward. Progress might be slow, but each of the myriad of successive drafts that we produced was an improvement – this was not something I could take for granted when working on my own. Amos’s work was always characterized by confidence and by a crisp elegance, and it was a joy to find those characteristics now attached to my ideas as well. As we were writing our first paper, I was conscious of how much better it was than the more hesitant piece I would have written by myself. I don’t know exactly what it was that Amos found to like in our collaboration – we were not in the habit of trading compliments -but clearly he was also having a good time. We were a team, and we remained in that mode for well over a decade. The Nobel Prize was awarded for work that we produced during that period of intense collaboration.  At the beginning of our collaboration, we quickly established a rhythm that we maintained during all our years together. Amos was a night person, and I was a morning person. This made it natural for us to meet for lunch and a long afternoon together, and still have time to do our separate things. We spent hours each day, just talking. When Amos’s first son Oren, then fifteen months old, was told that his father was at work, he volunteered the comment “Aba talk Danny.” We were not only working, of course – we talked of everything under the sun, and got to know each other’s mind almost as well as our own. We could (and often did) finish each other’s sentences and complete the joke that the other had wanted to tell, but somehow we also kept surprising each other.  We did almost all the work on our joint projects while physically together, including the drafting of questionnaires and papers. And we avoided any explicit division of labor. Our principle was to discuss every disagreement until it had been resolved to mutual satisfaction, and we had tie-breaking rules for only two topics: whether or not an item should be included in the list of references (Amos had the casting vote), and who should resolve any issue of English grammar (my dominion). We did not initially have a concept of a senior author. We tossed a coin to determine the order of authorship of our first paper, and alternated from then on until the pattern of our collaboration changed in the 1980s.  One consequence of this mode of work was that all our ideas were jointly owned. Our interactions were so frequent and so intense that there was never much point in distinguishing between the discussions that primed an idea, the act of uttering it, and the subsequent elaboration of it. I believe that many scholars have had the experience of discovering that they had expressed (sometimes even published) an idea long before they really understood its significance. It takes time to appreciate and develop a new thought. Some of the greatest joys of our collaboration-and probably much of its success – came from our ability to elaborate each other’s nascent thoughts: if I expressed a half-formed idea, I knew that Amos would be there to understand it, probably more clearly than I did, and that if it had merit he would see it. Like most people, I am somewhat cautious about exposing tentative thoughts to others – I must first make sure that they are not idiotic. In the best years of the collaboration, this caution was completely absent. The mutual trust and the complete lack of defensiveness that we achieved were particularly remarkable because both of us – Amos even more than I – were known to be severe critics. Our magic worked only when we were by ourselves. We soon learned that joint collaboration with any third party should be avoided, because we became competitive in a threesome.  Amos and I shared the wonder of together owning a goose that could lay golden eggs – a joint mind that was better than our separate minds. The statistical record confirms that our joint work was superior, or at least more influential, than the work we did individually (Laibson and Zeckhauser, 1998). Amos and I published eight journal articles during our peak years (1971-1981), of which five had been cited more than a thousand times by the end of 2002. Of our separate works, which in total number about 200, only Amos’ theory of similarity (Tversky, 1977) and my book on attention (Kahneman, 1973) exceeded that threshold. The special style of our collaborative work was recognized early by a referee of our first theoretical paper (on representativeness), who caused it to be rejected by *Psychological Review*. The eminent psychologist who wrote that review – his anonymity was betrayed years later – pointed out that he was familiar with the separate lines of work that Amos and I had been pursuing, and considered both quite respectable. However, he added the unusual remark that we seemed to bring out the worst in each other, and certainly should not collaborate. He found most objectionable our method of using multiple single questions as evidence – and he was quite wrong there as well.  The Science ’74 article and the rationality debate From 1971 to 1972, Amos and I were at the Oregon Research Institute (ORI) in Eugene, a year that was by far the most productive of my life. We did a considerable amount of research and writing on the availability heuristic, on the psychology of prediction, and on the phenomena of anchoring and overconfidence – thereby fully earning the label “dynamic duo” that our colleagues attached to us. Working evenings and nights, I also completely rewrote my book on *Attention and Effort*, which went to the publisher that year, and remains my most significant independent contribution to psychology.  At ORI, I came into contact for the first time with an exciting community of researchers that Amos had known since his student days at Michigan: Paul Slovic, Sarah Lichtenstein, and Robyn Dawes. Lewis Goldberg was also there, and I learned much from his work on clinical and actuarial judgment, and from Paul Hoffman’s ideas about paramorphic modeling. ORI was one of the major centers of judgment research, and I had the occasion to meet quite a few of the significant figures of the field when they came visiting, Ken Hammond among them.  Some time after our return from Eugene, Amos and I settled down to review what we had learned about three heuristics of judgment (representativeness, availability, and anchoring) and about a list of a dozen biases associated with these heuristics. We spent a delightful year in which we did little but work on a single article. On our usual schedule of spending afternoons together, a day in which we advanced by a sentence or two was considered quite productive. Our enjoyment of the process gave us unlimited patience, and we wrote as if the precise choice of every word were a matter of great moment.  We published the article in *Science* because we thought that the prevalence of systematic biases in intuitive assessments and predictions could possibly be of interest to scholars outside psychology. This interest, however, could not be taken for granted, as I learned in an encounter with a well-known American philosopher at a party in Jerusalem. Mutual friends had encouraged us to talk about the research that Amos and I were doing, but almost as soon as I began my story he turned away, saying, “I am not really interested in the psychology of stupidity.”  The *Science* article turned out to be a rarity: an empirical psychological article that (some) philosophers and (a few) economists could and did take seriously. What was it that made readers of the article more willing to listen than the philosopher at the party? I attribute the unusual attention at least as much to the medium as to the message. Amos and I had continued to practice the psychology of single questions, and the *Science* article – like others we wrote – incorporated questions that were cited verbatim in the text. These questions, I believe, personally engaged the readers and convinced them that we were concerned not with the stupidity of Joe Public but with a much more interesting issue: the susceptibility to erroneous intuitions of intelligent, sophisticated, and perceptive individuals such as themselves. Whatever the reason, the article soon became a standard reference as an attack on the rational-agent model, and it spawned a large literature in cognitive science, philosophy, and psychology. We had not anticipated that outcome.  I realized only recently how fortunate we were not to have aimed deliberately at the large target we happened to hit. If we had intended the article as a challenge to the rational model, we would have written it differently, and the challenge would have been less effective. An essay on rationality would have required a definition of that concept, a treatment of boundary conditions for the occurrence of biases, and a discussion of many other topics about which we had nothing of interest to say. The result would have been less crisp, less provocative, and ultimately less defensible. As it was, we offered a progress report on our study of judgment under uncertainty, which included much solid evidence. All inferences about human rationality were drawn by the readers themselves.  The conclusions that readers drew were often too strong, mostly because existential quantifiers, as they are prone to do, disappeared in the transmission. Whereas we had shown that (some, not all) judgments about uncertain events are mediated by heuristics, which (sometimes, not always) produce predictable biases, we were often read as having claimed that people cannot think straight. The fact that men had walked on the moon was used more than once as an argument against our position. Because our treatment was mistakenly taken to be inclusive, our silences became significant. For example, the fact that we had written nothing about the role of social factors in judgment was taken as an indication that we thought these factors were unimportant. I suppose that we could have prevented at least some of these misunderstandings, but the cost of doing so would have been too high.  The interpretation of our work as a broad attack on human rationality – rather than as a critique of the rational-agent model – attracted much opposition, some quite harsh and dismissive. Some of the critiques were normative, arguing that we compared judgments to inappropriate normative standards (Cohen, 1981; Gigerenzer, 1991, 1996). We were also accused of spreading a tendentious and misleading message that exaggerated the flaws of human cognition (Lopes, 1991, and many others). The idea of systematic bias was rejected as unsound on evolutionary grounds (Cosmides & Tooby, 1996). Some authors dismissed the research as a collection of artificial puzzles designed to fool undergraduates. Numerous experiments were conducted over the years, to show that cognitive illusions could “be made to disappear” and that heuristics had been invented to explain “biases that do not exist” (Gigerenzer, 1991). After participating in a few published skirmishes in the early 80’s, Amos and I adopted a policy of not criticizing the critiques of our work, although we eventually felt compelled to make an exception (Kahneman and Tversky, 1996).  A young colleague and I recently reviewed the experimental literature, and concluded that the empirical controversy about the reality of cognitive illusions dissolves when viewed in the perspective of a dual-process model (Kahneman and Frederick, 2002). The essence of such a model is that judgments can be produced in two ways (and in various mixtures of the two): a rapid, associative, automatic, and effortless intuitive process (sometimes called System 1), and a slower, rule-governed, deliberate and effortful process (System 2) (Sloman, 1996; Stanovich and West, 1999). System 2 ‘knows” some of the rules that intuitive reasoning is prone to violate, and sometimes intervenes to correct or replace erroneous intuitive judgments. Thus, errors of intuition occur when two conditions are satisfied: System 1 generates the error and System 2 fails to correct. In this view, the experiments in which cognitive illusions were “made to disappear” did so by facilitating the corrective operations of System 2. They tell us little about the intuitive judgments that are suppressed.  If the controversy is so simply resolved, why was it not resolved in 1971, or in 1974? The answer that Frederick and I proposed refers to the conversational context in which the early work was done:  A comprehensive psychology of intuitive judgment cannot ignore such controlled thinking, because intuition can be overridden or corrected by self-critical operations, and because intuitive answers are not always available. But this sensible position seemed irrelevant in the early days of research on judgment heuristics. The authors of the “law of small numbers” saw no need to examine correct statistical reasoning. They believed that including easy questions in the design would insult the participants and bore the readers. More generally, the early studies of heuristics and biases displayed little interest in the conditions under which intuitive reasoning is preempted or overridden – controlled reasoning leading to correct answers was seen as a default case that needed no explaining. A lack of concern for boundary conditions is typical of “young” research programs, which naturally focus on demonstrating new and unexpected effects, not on making them disappear. (Kahneman and Frederick, 2002, p. 50).  What happened, I suppose, is that because the 1974 paper was influential it altered the context in which it was read in subsequent years. Its being misunderstood was a direct consequence of its being taken seriously. I wonder how often this occurs.  Amos and I always dismissed the criticism that our focus on biases reflected a generally pessimistic view of the human mind. We argued that this criticism confuses the medium of bias research with a message about rationality. This confusion was indeed common. In one of our demonstrations of the availability heuristic, for example, we asked respondents to compare the frequency with which some letters appeared in the first and in the third position in words. We selected letters that in fact appeared more frequently in the third position, and showed that even for these letters the first position was judged more frequent, as would be predicted on the idea that it is easier to search through a mental dictionary by the first letter. The experiment was used by some critics as an example of our own confirmation bias, because we had demonstrated availability only in cases in which this heuristic led to bias. But this criticism assumes that our aim was to demonstrate biases, and misses the point of what we were trying to do. Our aim was to show that the availability heuristic controls frequency estimates *even when that heuristic leads to error* – an argument that cannot be made when the heuristic leads to correct responses, as it often does.  There is no denying, however, that the name of our method and approach created a strong association between heuristics and biases, and thereby contributed to giving heuristics a bad name, which we did not intend. I recently came to realize that the association of heuristics and biases has affected me as well. In the course of an exchange of messages with Ralph Hertwig (no fan of heuristics and biases), I noticed that the phrase “judging by representativeness” was in my mind a label for a cluster of errors in intuitive statistical judgment. Judging probability by representativeness is indeed associated with systematic errors. But a large component of the process is the judgment of representativeness, and that judgment is often subtle and highly skilled. The feat of the master chess player who instantly recognizes a position as “white mates in three” is an instance of judgment of representativeness. The undergraduate who instantly recognizes that enjoyment of puns is more representative of a computer scientist than of an accountant is also exhibiting high skill in a social and cultural judgment. My long-standing failure to associate specific benefits to the concept of representativeness was a revealing mistake.  What did I learn from the controversy about heuristics and biases? Like most protagonists in debates, I have few memories of having changed my mind under adversarial pressure, but I have certainly learned more than I know. For example, I am now quick to reject any description of our work as demonstrating human irrationality. When the occasion arises, I carefully explain that research on heuristics and biases only refutes an unrealistic conception of rationality, which identifies it as comprehensive coherence. Was I always so careful? Probably not. In my current view, the study of judgment biases requires attention to the interplay between intuitive and reflective thinking, which sometimes allows biased judgments and sometimes overrides or corrects them. Was this always as clear to me as it is now? Probably not. Finally, I am now very impressed by the observation I mentioned earlier, that the most highly skilled cognitive performances are intuitive, and that many complex judgments share the speed, confidence and accuracy of routine perception. This observation is not new to me, but did it always loom as large in my views as it now does? Almost certainly not.  As my obvious struggle with this topic reveals, I thoroughly dislike controversies where it is clear that no minds will be changed. I feel diminished by losing my objectivity when in point-scoring mode, and downright humiliated when I get angry. Indeed, my phobia for professional anger is such that I have allowed myself for many years the luxury of refusing to referee papers that might arouse that emotion: If the tone is snide, or the review of the facts more tendentious than normal, I return the paper back to the editor without commenting on it. I consider myself fortunate not to have had too many of the nasty experiences of professional quarrels, and am grateful for the occasional encounters with open minds across lines of sharp debate (Ayton, 1998; Klein, 2000).  Prospect theory After the publication of our paper on judgment in *Science* in 1974, Amos suggested that we study decision-making together. This was a field in which he was already an established star, and about which I knew very little. For an introduction, he suggested that I read the relevant chapters of the text “Mathematical Psychology,” of which he was a co-author (Coombs, Dawes and Tversky, 1970). Utility theory and the paradoxes of Allais and Ellsberg were discussed in the book, along with some of the classic experiments in which major figures in the field had joined in an effort to measure the utility function for money by eliciting choices between simple gambles.  I learned from the book that the name of the game was the construction of a theory that would explain Allais’s paradox parsimoniously. As psychological questions go, this was not a difficult one, because Allais’s famous problems are, in effect, an elegant way to demonstrate that the subjective response to probability is not linear. The subjective non-linearity is obvious: the difference between probabilities of .10 and .11 is clearly less impressive than the difference between 0 and .01, or between .99 and 1.00. The difficulty and the paradox exist only for decision theorists, because the non-linear response to probability produces preferences that violate compelling axioms of rational choice and are therefore incompatible with standard expected utility theory. The natural response of a decision theorist to the Allais paradox, certainly in 1975 and probably even today, would be to search for a new set of axioms that have normative appeal and yet permit the non-linearity. The natural response of psychologists was to set aside the issue of rationality and to develop a descriptive theory of the preferences that people actually have, regardless of whether or not these preferences can be justified.  The task we set for ourselves was to account for observed preferences in the quaintly restricted universe within which the debate about the theory of choice has traditionally been conducted: monetary gambles with few outcomes (all positive), and definite probabilities. This was an empirical question, and data were needed. Amos and I solved the data collection problem with a method that was both efficient and pleasant. We spent our hours together inventing interesting choices and examining our preferences. If we agreed on the same choice we provisionally assumed that other people would also accept it, and we went on to explore its theoretical implications. This unusual method enabled us to move quickly, and we constructed and discarded models at a dizzying rate. I have a distinct memory of a model that was numbered 37, but cannot vouch for the accuracy of our count.  As was the case in our work on judgment, our central insights were acquired early and, as was the case in our work on judgment, we spent a vast amount of time and effort before publishing a paper that summarized those insights (Kahneman and Tversky, 1979). The first insight came as a result of my naïveté. When reading the mathematical psychology textbook, I was puzzled by the fact that all the choice problems were described in terms of gains and losses (actually, almost always gains), whereas the utility functions that were supposed to explain the choices were drawn with wealth as the abscissa. This seemed unnatural, and psychologically unlikely. We immediately decided to adopt changes and/or differences as carriers of utility. We had no inkling that this obvious move was truly fundamental, or that it would open the path to behavioral economics. Harry Markowitz, who won the Nobel Prize in economics in 1990, had proposed changes of wealth as carriers of utility in 1952, but he did not take this idea very far.  The shifts from wealth to changes of wealth as carriers of utility is significant because of a property of preferences that we later labeled *loss-aversion*: the response to losses is consistently much more intense than the response to corresponding gains, with a sharp kink in the value function at the reference point. Loss aversion is manifest in the extraordinary reluctance to accept risk that is observed when people are offered a gamble on the toss of a coin: most will reject a gamble in which they might lose $20, unless they are offered more than $40 if they win. The concept of loss aversion was, I believe, our most useful contribution to the study of decision making. The asymmetry between gains and losses solves quite a few puzzles, including the widely noted and economically irrational distinction that people draw between opportunity costs and ‘real’ losses. Loss aversion also helps explain why real-estate markets dry up for long periods when prices are down, and it contributes to the explanation of a widespread bias favoring the status quo in decision making. Finally, the asymmetric consideration of gains and losses extends to the domain of moral intuitions, in which imposing losses and failing to share gains are evaluated quite differently. But of course, none of that was visible to Amos and me when we first decided to assume a kinked value function – we needed that kink to account for choices between gambles.  Another set of early insights came when Amos suggested that we flip the signs of outcomes in the problems we had been considering. The result was exciting. We immediately detected a remarkable pattern, which we called “reflection”: changing the signs of all outcomes in a pair of gambles almost always caused the preference to change from risk averse to risk seeking, or viceversa. For example, we both preferred a sure gain of $900 over a .9 probability of gaining $1,000 (or nothing), but we preferred a gamble with a .9 probability of losing $1,000 over a sure loss of $900. We were not the first to observe this pattern. Raiffa (1968) and Williams (1966) knew about the prevalence of risk-seeking in the negative domain. But ours was apparently the first serious attempt to make something of it.  We soon had a draft of a theory of risky choice, which we called “value theory” and presented at a conference in the spring of 1975. We then spent about three years polishing it, until we were ready to submit the article for publication. Our effort during those years was divided between the tasks of exploring interesting implications of our theoretical formulation and developing answers to all plausible objections. To amuse ourselves, we invented the specter of an ambitious graduate student looking for flaws, and we labored to make that student’s task as thankless as possible. The most novel idea of prospect theory occurred to us in that defensive context. It came quite late, as we were preparing the final version of the paper. We were concerned with the fact that a straightforward application of our model implied that the value of the prospect ($100, .01; $100, .01) is larger than the value of ($100, .02). The prediction is wrong, of course, because most decision makers will spontaneously transform the former prospect into the latter and treat them as equivalent in subsequent operations of evaluation and choice. To eliminate the problem we proposed that decision-makers, prior to evaluating the prospects, perform an editing operation that collects similar outcomes and adds their probabilities. We went on to propose several other editing operations that provided an explicit and psychologically plausible defense against a variety of superficial counter-examples to the core of the theory. We had succeeded in making life quite difficult for that pedantic graduate student. But we had also made a truly significant advance, by making it explicit that the objects of choice are mental representations, not objective states of the world. This was a large step toward the development of a concept of framing, and eventually toward a new critique of the model of the rational agent.  When we were ready to submit the work for publication, we deliberately chose a meaningless name for our theory: “prospect theory.” We reasoned that if the theory ever became well known, having a distinctive label would be an advantage. This was probably wise.  I looked at the 1975 draft recently, and was struck by how similar it is to the paper that was eventually published, and also by how different the two papers are. Most of the key ideas, most of the key examples, and much of the wording were there in the early draft. But that draft lacks the authority that was gained during the years that we spent anticipating objections. “Value theory” would not have survived the close scrutiny that a significant article ultimately gets from generations of scholars and students, who only are obnoxious if you give them a chance.  We published the paper in *Econometrica*. The choice of venue turned out to be important; the identical paper, published in *Psychological Review*, would likely have had little impact on economics. But our decision was not guided by a wish to influence economics. *Econometrica* just happened to be the journal where the best papers on decision-making to date had been published, and we were aspiring to be in that company.  And there was another way in which the impact of prospect theory depended crucially on the medium, as well as the message. Prospect theory was a formal theory, and its formal nature was the key to the impact it had in economics. Every discipline of social science, I believe, has some ritual tests of competence, which must be passed before a piece of work is considered worthy of attention. Such tests are necessary to prevent information overload, and they are also important aspects of the tribal life of the disciplines. In particular, they allow insiders to ignore just about anything that is done by members of other tribes, and to feel no scholarly guilt about doing so. To serve this screening function efficiently, the competence tests usually focus on some aspect of form or method, and have little or nothing to do with substance. Prospect theory passed such a test in economics, and its observations became a legitimate (though optional) part of the scholarly discourse in that discipline. It is a strange and rather arbitrary process that selects some pieces of scientific writing for relatively enduring fame while committing most of what is published to almost immediate oblivion.  Framing and mental accounting Amos and I completed prospect theory during the academic year of 1977 to 1978, which I spent at the Center for Advanced Studies at Stanford, while he was visiting the psychology department there. Around that time, we began work on our next project, which became the study of framing. This was also the year in which the second most important professional friendship in my life – with Richard Thaler – had its start.  A framing effect is demonstrated by constructing two transparently equivalent versions of a given problem, which nevertheless yield predictably different choices. The standard example of a framing problem, which was developed quite early, is the ‘lives saved, lives lost’ question, which offers a choice between two public-health programs proposed to deal with an epidemic that is threatening 600 lives: one program will save 200 lives, the other has a 1/3 chance of saving all 600 lives and a 2/3 chance of saving none. In this version, people prefer the program that will save 200 lives for sure. In the second version, one program will result in 400 deaths, the other has a 2/3 chance of 600 deaths and a 1/3 chance of no deaths. In this formulation most people prefer the gamble. If the same respondents are given the two problems on separate occasions, many give incompatible responses. When confronted with their inconsistency, people are quite embarrassed. They are also quite helpless to resolve the inconsistency, because there are no moral intuitions to guide a choice between different sizes of a surviving population.  Amos and I began creating pairs of problems that revealed framing effects while working on prospect theory. We used them to show sensitivity to gains and losses (as in the lives example), and to illustrate the inadequacy of a formulation in which the only relevant outcomes are final states. In that article, we also showed that a single-stage gamble could be rearranged as a two-stage gamble in a manner that left the bottom-line probabilities and outcomes unchanged but reversed preferences. Later, we developed examples in which respondents are asked to make simultaneous choices in two problems, A and B. One of the problems involves gains and elicits a risk-averse choice; the other problem involves losses and elicits risk-seeking. A majority of respondents made both these choices. However, the problems were constructed so that the combination of choices that people made was actually dominated by the combination of the options they had rejected.  These are not parlor-game demonstrations of human stupidity. The ease with which framing effects can be demonstrated reveals a fundamental limitation of the human mind. In a rational-agent model, the agent’s mind functions just as she would like it to function. Framing effects violate that basic requirement: the respondents who exhibit susceptibility to framing effects wish their minds were able to avoid them. We were able to conceive of only two kinds of mind that would avoid framing effects: (1) If responses to all outcomes and probabilities were strictly linear, the procedures that we used to produce framing effects would fail. (2) If individuals maintained a single canonical and all-inclusive view of their outcomes, truly equivalent problems would be treated equivalently. Both conditions are obviously impossible. Framing effects violate a basic requirement of rationality which we called invariance (Kahneman and Tversky, 1984) and Arrow (1982) called extensionality. It took us a long time and several iterations to develop a forceful statement of this contribution to the rationality debate, which we presented several years after our framing paper (Tversky and Kahneman, 1986).  Another advance that we made in our first framing article was the inclusion of riskless choice problems among our demonstrations of framing. In making that move, we had help from a new friend. Richard Thaler was a young economist, blessed with a sharp and irreverent mind. While still in graduate school, he had trained his ironic eye on his own discipline and had collected a set of pithy anecdotes demonstrating obvious failures of basic tenets of economic theory in the behavior of people in general – and of his very conservative professors in Rochester in particular. One key observation was the endowment effect, which Dick illustrated with the example of the owner of a bottle of old wine, who would refuse to sell it for $200 but would not pay as much as $100 to replace it if it broke. Sometime in 1976, a copy of the 1975 draft of prospect theory got into Dick’s hands, and that event made a significant difference to our lives. Dick realized that the endowment effect, which is a genuine puzzle in the context of standard economic theory, is readily explained by two assumptions derived from prospect theory. First, the carriers of utility are not states (owning or not owning the wine), but changes – getting the wine or giving it up. And giving up is weighted more than getting, by loss aversion. When Dick learned that Amos and I would be in Stanford in 1977/8, he secured a visiting appointment at the Stanford branch of the National Bureau of Economic Research, which is located on the same hill as the Center for Advanced Studies. We soon became friends, and have ever since had a considerable influence on each other’s thinking.  The endowment effect was not the only thing we learned from Dick. He had also developed a list of phenomena of what we now call “mental accounting.” Mental accounting describes how people violate rationality by failing to maintain a comprehensive view of outcomes, and by failing to treat money as fungible. Dick showed how people segregate their decisions into separate accounts, then struggle to keep each of these accounts in the black. One of his compelling examples was the couple who drove through a blizzard to a basketball game because they had already paid for the tickets, though they would have stayed at home if the tickets had been free. As this example illustrates, Dick had independently developed the skill of doing “one-question economics.” He inspired me to invent another story, in which a person who comes to the theater realizes that he has lost his ticket (in one version), or an amount of cash equal to the ticket value (in another version). People report that they would be very likely still to buy a ticket if they had lost the cash, presumably because the loss has been charged to general revenue. On the other hand, they describe themselves as quite likely to go home if they have lost an already purchased ticket, presumably because they do not want to pay twice to see the same show.  Behavioral economics Our interaction with Thaler eventually proved to be more fruitful than we could have imagined at the time, and it was a major factor in my receiving the Nobel Prize. The committee cited me “for having integrated insights from psychological research into economic science ….”. Although I do not wish to renounce any credit for my contribution, I should say that in my view the work of integration was actually done mostly by Thaler and the group of young economists that quickly began to form around him, starting with Colin Camerer and George Loewenstein, and followed by the likes of Matthew Rabin, David Laibson, Terry Odean, and Sendhil Mullainathan. Amos and I provided quite a few of the initial ideas that were eventually integrated into the thinking of some economists, and prospect theory undoubtedly afforded some legitimacy to the enterprise of drawing on psychology as a source of realistic assumptions about economic agents. But the founding text of behavioral economics was the first article in which Thaler (1980) presented a series of vignettes that challenged fundamental tenets of consumer theory. And the respectability that behavioral economics now enjoys within the discipline was secured, I believe, by some important discoveries Dick made in what is now called behavioral finance, and by the series of “Anomalies” columns that he published in every issue of the *Journal of Economic Perspectives* from 1987 to 1990, and has continued to write occasionally since that time.  In 1982, Amos and I attended a meeting of the Cognitive Science Society in Rochester, where we had a drink with Eric Wanner, a psychologist who was then vice-president of the Sloan Foundation. Eric told us that he was interested in promoting the integration of psychology and economics, and asked for our advice on ways to go about it. I have a clear memory of the answer we gave him. We thought that there was no way to “spend a lot of money honestly” on such a project, because interest in interdisciplinary work could not be coerced. We also thought that it was pointless to encourage psychologists to make themselves heard by economists, but that it could be useful to encourage and support the few economists who were interested in listening. Thaler’s name surely came up. Soon after that conversation, Wanner became the president of the Russell Sage Foundation, and he brought the psychology/economics project with him. The first grant that he made in that program was for Dick Thaler to spend an academic year (1984-85) visiting me at the University of British Columbia, in Vancouver.  That year was one of the best in my career. We worked as a trio that also included the economist Jack Knetsch, with whom I had already started constructing surveys on a variety of issues, including valuation of the environment and public views about fairness in the marketplace. Jack had done experimental studies of the endowment effect and had seen the implications of that effect for the Coase theorem and for issues of environmental policy. We made a very good team: Jack’s wisdom and imperturbable calm withstood the stress of Dick’s boisterous temperament and of my perfectionist anxieties and intellectual restlessness.  We did a lot together that year. We conducted a series of market experiments involving real goods (the “mugs” studies), which eventually became a standard in that literature (Kahneman, Knetsch and Thaler, 1990). We also conducted multiple surveys in which we used experimentally varied vignettes to identify the rules of fairness that the public would apply to merchants, landlords, and employers (Kahneman, Knetsch and Thaler, 1986a). Our central observation was that in many contexts the existing situation (e.g., price, rent, or wage) defines a “reference transaction,” to which the transactor (consumer, tenant, and employee) has an entitlement – the violation of such entitlements is considered unfair and may evoke retaliation. For example, cutting the wages of an employee merely because he could be replaced by someone who would accept a lower wage is unfair, although paying a lower wage to the replacement of an employee who quit is entirely acceptable. We submitted the paper to the *American Economic Review* and were utterly surprised by the outcome: the paper was accepted without revision. Luckily for us, the editor had asked two economists quite open to our approach to review the paper. We later learned that one of the referees was [George Akerlof](https://www.nobelprize.org/nobel_prizes/economics/laureates/2001/index.html) and the other was Alan Olmstead, who had studied the failures of markets to clear during an acute gas shortage.  One question that arose during this research was whether people would be wiling to pay something to punish another agent who treated them “unfairly”, and in some circumstances would share a windfall with a stranger in an effort to be “fair”. We decided to investigate these ideas using experiments for real stakes. The games that we invented for this purpose have become known as the ultimatum game and the dictator game. Alas, while writing up our second paper on fairness (Kahneman, Knetsch and Thaler, 1986b) we learned that we had been scooped on the ultimatum game by Werner Guth and his colleagues, who had published experiments using the same design a few years earlier. I remember being quite crestfallen when I learned this. I would have been even more depressed if I had known how important the ultimatum game would eventually become.  Most of the economics I know I learned that year, from Jack and Dick, my two willing teachers, and from what was in fact my first experience of communicating across tribal boundaries. I was also much impressed by an experimental game that Dick Thaler, James Brander, and I invented and called the N\* game. The game is played by a group of, say, fifteen people. On each trial, a number 0< N\* <15 is announced. The participants then make simultaneous choices of whether or not to “enter.” Those who decide to enter announce their choice simultaneously. The payoff to the N entrants depends on their number, according to the following formula: $.25(N\* – N). We played the game a few times, once with the faculty of the psychology department at U.B.C. The results, although not surprising to an economist, struck me as magical. Within very few trials, a pattern emerged in which the number of entrants, N, was within 1 or 2 of N\*, with no obvious systematic tendency to be higher or lower than N\*. The group was doing the right thing collectively, although conversations with the participants and the obvious statistical analyses did not reveal any consistent strategies that made sense. It took me some time to realize that the magic we were observing was an equilibrium: the pattern we saw existed because no other pattern could be sustained. This idea had not been in my intellectual bag of tools. We never formally published the N\* game – I described it informally in Kahneman (1987) – but it has been taken up by others (Erev & Rapoport, 1998).  That was the closest my research ever came to core economics, and since that time I have been mostly cheering Thaler and behavioral economics from the sidelines. There has been much to cheer about. As a mark of the progress that has been made, I recall a seminar in psychology and economics that I co-taught with George Akerlof, after Anne Treisman and I had moved from the University of British Columbia to Berkeley in 1986. I remember being struck by the reverence with which the rationality assumption was treated even by a free thinker such as George, and also by his frequent warnings to the students that they should not let themselves be seduced by the material we were presenting, lest their careers be permanently damaged. His advice to them was to stick to what he called “meat-and-potatoes economics,” at least until their careers were secure. This opinion was quite common at the time. When Matthew Rabin joined the Berkeley economics department as a young assistant professor and chose to immerse himself in psychology, many considered the move professional suicide. Some fifteen years later, Rabin had earned the Clark medal, and George Akerlof had delivered a Nobel lecture entitled “Behavioral Macroeconomics.”  Eric Wanner and the Russell Sage Foundation continued to support behavioral economics over the years. I was instrumental in the idea of using some of that support to set up a summer school for graduate students and young faculty in that field, and I helped Dick Thaler and Colin Camerer organize the first one, in 1994. When the fifth summer school convened in 2002, David Laibson, who had been a participant in 1994, was tenured at Harvard and was one of the three organizers. Terrance Odean and Sendhil Mullainathan, who had also participated as students, came back to lecture as successful researchers with positions in two of the best universities in the world. It was a remarkable experience to hear Matthew Rabin teach a set of guidelines for developing theories in behavioral economics – including the suggestion that the standard economic model should be a special case of the more complex and general models that were to be constructed. We had come a long way.  Although behavioral economics has enjoyed much more rapid progress and gained more respectability in economics than appeared possible fifteen years ago, it is still a minority approach and its influence on most fields of economics is negligible. Many economists believe that it is a passing fad, and some hope that it will be. The future may prove them right. But many bright young economists are now betting their careers on the expectation that the current trend will last. And such expectations have a way of being self-fulfilling.  Later years Anne Treisman and I married and moved together to U.B.C. in 1978, and Amos and Barbara Tversky settled in Stanford that year. Amos and I were then at the peak of our joint game, and completely committed to our collaboration. For a few years, we managed to maintain it, by spending every second weekend together and by placing multiple phone calls each day, some lasting several hours. We completed the study of framing in that mode, as well as a study of the ‘conjunction fallacy’ in judgment (Tversky and Kahneman, 1983). But eventually the goose that had laid the golden eggs languished, and our collaboration tapered off. Although this outcome now appears inevitable, it came as a painful surprise to us. We had completely failed to appreciate how critically our successful interaction had depended on our being together at the birth of every significant idea, on our rejection of any formal division of labor, and on the infinite patience that became a luxury when we could meet only periodically. We struggled for years to revive the magic we had lost, but in vain.  We were again trying when Amos died. When he learned in the early months of 1996 that he had only a few months to live, we decided to edit a joint book on decision-making that would cover some of the progress that had been made since we had started working together on the topic more than twenty years before (Kahneman and Tversky, 2000). We planned an ambitious preface as a joint project, but I think we both knew from the beginning that we would not be granted enough time to complete it. The preface I wrote alone was probably my most painful writing experience.  During the intervening years, of course, we had continued to work, sometimes together sometimes with other collaborators. Amos took the lead in our most important joint piece, an extension of prospect theory to the multipleoutcome case in the spirit of rank-dependent models. He also carried out spectacular studies of the role of argument and conflict in decision-making, in collaborations with Eldar Shafir and with Itamar Simonson, as well as influential work on violations of procedural invariance in collaborations with Shmuel Sattath and with Paul Slovic. He engaged in a deep exploration of the mathematical structure of decision theories with Peter Wakker. And, in his last years, Amos was absorbed in the development of support theory, a general approach to thinking under uncertainty that his students have continued to explore. These are only his major programmatic research efforts in the field of decision-making – he did much more.  I, too, kept busy, and also kept moving. Anne Treisman and I moved to UC Berkeley in 1986, and from there to Princeton in 1993, where I happily took a split appointment that located me part-time in the Woodrow Wilson School of Public Affairs. Moving East also made it easier to maintain frequent contacts with friends, children and adored grandchildren in Israel.  Over the years I enjoyed productive collaborations with Dale Miller in the development of a theory of counterfactual thinking (Kahneman and Miller, 1986), and with Anne Treisman, in studies of visual attention and object perception. In addition to the work on fairness and on the endowment effect that we did with Dick Thaler, Jack Knetsch and I carried out studies of the valuation of public goods that became quite controversial and had a great influence on my own thinking. Further studies of that problem with Ilana Ritov eventually led to the idea that the translation of attitudes into dollars involves the almost arbitrary choice of a scale factor, leading some people who have quite similar values to state very different values of their willingness to pay, for no good reason (Kahneman, Ritov and Schkade, 1999). With David Schkade and the famous jurist Cass Sunstein I extended this idea into a program of research on arbitrariness in punitive damage decisions, which may yet have some influence on policy (Sunstein, Kahneman, Schkade and Ritov, 2002).  The focus of my research for the past fifteen years has been the study of various aspects of experienced utility – the measure of the utility of outcomes as people actually live them. The concept of utility in which I am interested was the one that Bentham and Edgeworth had in mind. However, experienced utility largely disappeared from economic discourse in the twentieth century, in favor of a notion that I call decision utility, which is inferred from choices and used to explain choices. The distinction could be of little relevance for fully rational agents, who presumably maximize experienced utility as well as decision utility. But if rationality cannot be assumed, the quality of consequences becomes worth measuring and the maximization of experienced utility becomes a testable proposition. Indeed, my colleagues and I have carried out experiments in which this proposition was falsified. These experiments exploit a simple rule that governs the assignment of remembered utility to past episodes in which an agent is passively exposed to a pleasant or unpleasant experience, such as watching a horrible film or an amusing one (Frederickson and Kahneman, 1993), or undergoing a colonoscopy (Redelmeier and Kahneman, 1993). Remembered utility turns out to be determined largely by the peak intensity of the pleasure or discomfort experienced during the episode, and by the intensity of pleasure or discomfort when the episode ended. The duration of the episode has almost no effect on its remembered utility. In accord with this rule, an episode of 60 seconds during which one hand is immersed in painfully cold water will leave a more aversive memory than a longer episode, in which the same 60 seconds are followed by another 30 seconds during which the temperature rises slightly. Although the extra 30 seconds are painful, they provide an improved end. When experimental participants are exposed to the two episodes, then given a choice of which to repeat, most choose the longer one (Kahneman, Fredrickson, Schreiber and Redelmeier, 1993). In these and in other experiments of the same kind (Schreiber and Kahneman, 2000), people make wrong choices between experiences to which they may be exposed, because they are systematically wrong about their affective memories Our evidence contradicts the standard rational model, which does not distinguish between experienced utility and decision utility. I have presented it as a new type of challenge to the assumption of rationality (Kahneman, 1994).  Most of my empirical work in recent years has been done in collaboration with my friend David Schkade. The current topic of our research is a study of well-being that builds on my previous research on experienced utility. We have assembled a multi-disciplinary team for an attempt to develop tools for measuring welfare, with the design specification that economists should be willing to take the measurements seriously.  Another major effort went into an essay that attempted to update the notion of judgment heuristics. That work was done in close collaboration with a young colleague, Shane Frederick. In the pains we took in the choice of every word it came close to matching my experiences with Amos (Kahneman and Frederick, 2002). My Nobel lecture is an extension of that essay.  One line of work that I hope may become influential is the development of a procedure of *adversarial collaboration*, which I have championed as a substitute for the format of critique-reply-rejoinder in which debates are currently conducted in the social sciences.[1](https://www.nobelprize.org/prizes/economic-sciences/2002/kahneman/biographical/#not1) Both as a participant and as a reader I have been appalled by the absurdly adversarial nature of these exchanges, in which hardly anyone ever admits an error or acknowledges learning anything from the other. Adversarial collaboration involves a good-faith effort to conduct debates by carrying out joint research – in some cases there may be a need for an agreed arbiter to lead the project and collect the data. Because there is no expectation of the contestants reaching complete agreement at the end of the exercise, adversarial collaborations will usually lead to an unusual type of joint publication, in which disagreements are laid out as part of a jointly authored paper. I have had three adversarial collaborations, with Tom Gilovich and Victoria Medvec (Gilovich, Medvec and Kahneman, 1998), with Ralph Hertwig (where Barbara Mellers was the agreed arbiter, see Mellers, Hertwig and Kahneman, 2001), and with a group of experimental economists in the UK (Bateman *et al*., 2003). An appendix in the Mellers *et al*. article proposes a detailed protocol for the conduct of adversarial collaboration. In another case I did not succeed in convincing two colleagues that we should engage in an adversarial collaboration, but we jointly developed another procedure that is also more constructive than the reply-rejoinder format. They wrote a critique of one of my lines of work, but instead of following up with the usual exchange of unpleasant comments we decided to write a joint piece, which started by a statement of what we did agree on, then went on to a series of short debates about issues on which we disagreed (Ariely, Kahneman, & Loewenstein, 2000). I hope that more efficient procedures for the conduct of controversies will be part of my legacy.  Part 2 – Eulogy for Amos Tversky (June 5, 1996) People who make a difference do not die alone. Something dies in everyone who was affected by them. Amos made a great deal of difference, and when he died, life was dimmed and diminished for many of us.  There is less intelligence in the world. There is less wit. There are many questions that will never be answered with the same inimitable combination of depth and clarity. There are standards that will not be defended with the same mix of principle and good sense. Life has become poorer.  There is a large Amos-shaped gap in the mosaic, and it will not be filled. It cannot be filled because Amos shaped his own place in the world, he shaped his life, and even his dying. And in shaping his life and his world, he changed the world and the life of many around him.  Amos was the freest person I have known, and he was able to be free because he was also one of the most disciplined.  Some of you may have tried to make Amos do something he did not want to do. I don’t think that there are many with successes to recount. Unlike many of us, Amos could not be coerced or embarrassed into chores or empty rituals. In that sense he was free, and the object of envy for many of us. But the other side of freedom is the ability to find joy in what one does, and the ability to adapt creatively to the inevitable. I will say more about the joy later. The supreme test of Amos’s ability to accept what cannot be changed came in the last few months. Amos loved living. Death, at a cruelly young age was imposed on him, before his children’s lives had fully taken shape, before his work was done. But he managed to die as he had lived – free. He died as he intended. He wanted to work to the last, and he did. He wanted to keep his privacy, and he did. He wanted to help his family through their ordeal, and he did. He wanted to hear the voices of his friends one last time, and he found a way to do that through the letters that he read with pleasure, sadness and pride, to the end.  There are many forms of courage, and Amos had them all. The indomitable serenity of his last few months is one. The civic courage of adopting principled and unpopular positions is another, and he had that too. And then there is the heroic, almost reckless courage, and he had that too.  My first memory of Amos goes back to 1957, when someone pointed out to me a thin and handsome lieutenant, wearing the red beret of the paratroopers, who had just taken the competitive entrance exam to the undergraduate program in Psychology at Hebrew University. The handsome lieutenant looked very pale, I remember. He had been wounded.  The paratrooper unit to which he belonged had been performing an exercise with live fire in front of the general staff of the Israel Defense Forces and all the military attaches. Amos was a platoon commander. He sent one of his soldiers carrying a long metal tube loaded with an explosive charge, which was to be slid under the barbed wire of the position they were attacking, and was to be detonated to create an opening for the attacking troops. The soldier moved forward, placed the explosive charge, and lit the fuse. And then he froze, standing upright in the grip of some unaccountable attack of panic. The fuse was short and the soldier was certainly about to be killed. Amos leapt from behind the rock he was using for cover, ran to the soldier, and managed to jump at him and bring him down just before the charge exploded. This was how he was wounded. Those who have been soldiers will recognize this act as one of almost unbelievable presence of mind and bravery. It was awarded the highest citation available in the Israeli army.  Amos almost never mentioned this incident, but some years ago, in the context of one of our frequent conversations about the importance of memory in our lives, he mentioned it and said that it had greatly affected him. We can probably appreciate what it means for a 20-year old to have passed a supreme test, to have done the impossible. We can understand how one could draw strength from such an event, especially if – as was the case for Amos – achieving the almost impossible was not a once-off thing. Amos achieved the almost impossible many times, in different contexts.  Amos’ almost impossible achievements, as you all know, extended to the academic life. Amos derived some quiet pleasure from one aspect of his record: by a large margin, he published more articles in *Psychological Review*, the prestigious theory journal of the discipline, than anyone else in the history of that journal, which goes back more than 100 years. He had two pieces in press in *Psychological Review* when he died.  But other aspects of the record are even more telling than this statistic. The number of gems and enduring classics sets Amos apart even more. His early work on transitivity violations, elimination by aspects, similarity, the work we did together on judgment, prospect theory and framing, the Hot Hand, the beautiful work on the disjunction effect and Argument-Based Choice, and most recently an achievement of which Amos was particularly proud: Support Theory.  How did he do it? There are many stories one could tell. Amos’ lifelong habit of working alone at night while others slept surely helped, but that wouldn’t quite do it. Then there was that mind – the bright beam of light that would clear out an idea from the fog of other people’s words, the inventiveness that could come up with six different ways of doing anything that needed to be done. You might think that having the best mind in the field and the most efficient work style would suffice. But there was more.  Amos had simply perfect taste in choosing problems, and he never wasted much time on anything that was not destined to matter. He also had an unfailing compass that always kept him going forward. I can attest to that from long experience.  It is not uncommon for me to write dozens of drafts of a paper, but I am never quite sure that they are actually improving, and often I wander in circles. Almost everything I wrote with Amos also went through dozens of drafts, but when you worked with Amos you just knew. There would be many drafts, and they would get steadily better.  Amos and I wrote an article in *Science* in 1974. It took us a year. We would meet at the van Leer Institute in Jerusalem for 4-6 hours a day. On a good day we would mark a net advance of a sentence or two. It was worth every minute. And I have never had so much fun. When we started work on Prospect Theory it was 1974, and in about 6 months we had been through 30- odd versions of the theory and had a paper ready for a conference. The paper had about 90% of the ideas of Prospect Theory, and quite properly did not impress anyone. We spent the better part of the following four years debugging it, trying to anticipate every objection.  What kept us at it was a phrase that Amos often used: “Let’s do it right”. There was never any hurry, any thought of compromising quality for speed. We could do it because Amos said the work was important, and you could trust him when he said that. We could also do it because the process was so intensely enjoyable.  But even that is not all. To understand Amos’ genius – not a word I use lightly – you have to consider a phrase that he was using increasingly often in the last few years: “Let us take what the terrain gives”. In his growing wisdom Amos believed that Psychology is almost impossible, because there is just not all that much we can say that is both important and demonstrably true. “Let us take what the terrain gives” meant not over-reaching, not believing that setting a problem implies it can be solved.  The unique ability Amos had – no one else I know comes close – was to find the one place where the terrain will yield (for Amos, usually gold) – and then to take it all. This skill in taking it all is what made so many of Amos’ papers not only classics, but definitive. What Amos had done did not need redoing.  Whether or not to over-reach was a source of frequent, and frequently productive tension between Amos and me over nearly 30 years. I have always wanted to do more than could be done without risk of error, and have always taken pride in preferring to be approximately right rather than precisely wrong. Amos thought that if you pick the terrain properly you won’t have to choose, because you can be precisely right. And time and time again he managed to be precisely right on things that mattered. Wisdom was part of his genius.  Fun was also part of Amos’ genius. Solving problems was a lifelong source of intense joy for him, and the fact that he was richly rewarded for his problem solving never undermined that joy.  Much of the joy was social. Almost all of Amos’ work was collaborative. He enjoyed working with colleagues and students, and he was supremely good at it. And his joy was infectious. The 12 or 13 years in which most of our work was joint were years of interpersonal and intellectual bliss. Everything was interesting, almost everything was funny, and there was the recurrent joy of seeing an idea take shape. So many times in those years we shared the magical experience of one of us saying something which the other would understand more deeply than the speaker had done. Contrary to the old laws of information theory, it was common for us to find that more information was received than had been sent. I have almost never had that experience with anyone else. If you have not had it, you don’t know how marvelous collaboration can be …   |  | | --- | | References | | Ariely, D., Kahneman, D. & Loewenstein, G. (2000). Joint comment on “When does duration matter in judgment and decision making”. *Journal of Experimental Psychology*: General, 129, 524-529. | | Arrow, K. J. (1982). Risk perception in psychology and economics. *Economic Inquiry*, 20, 1-9. | | Ayton, P. (1998). 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| Autobiographical |  |
| Podcast | How does it feel to be one of the most famous behavioural psychologists of our time? Daniel Kahneman says that it is perfectly fine to be famous as long as you don’t let it go to your head. From an early age, Kahneman was interested in people – he took after his mother who, both with irony and objectivity, was fascinated by the people in her surroundings. When he was just ten years old he wrote his first essay on the psychology of religion.  In this podcast episode host Adam Smith speaks to Daniel Kahneman in New York on eureka moments, scientific collaborations, stereotypes and racial discrimination, and also advice: “In general I try to give as little advice as possible.” |
| Telephone  interview | 0845 |
| Interview |  |
| Q1 | Well Professor Kahneman, what made you decide to become a scientist? |
|  | Daniel Kahneman: I wanted to be a philosopher actually and I decided, I became interested in psychology as a substitute for philosophy, as a way of answering questions about the human condition but answering them by looking at facts rather than by discussing words. So that’s how I became a psychologist.  And Professor Smith, your answer too?  Vernon Smith: Actually I was interested in philosophy at one point. This is before I really was seriously into my undergraduate training, so it was just a reading interest of my own and I read Sir James Jeans’ Physics and Philosophy and Sir Arthur Eddington’s books and Burton Russell; I was interested in science and philosophy and originally I expected to study science and I went to Caltech for that reason and I did study physics and would have taken my degree in physics, my undergraduate degree, except for one hurdle.  So I took electrical engineering because I chickened out …  To take the degree in physics you had to take Smyth’s course, a famous course that was very, very hard to get through. By taking my degree in electrical engineering I didn’t have to take Smyth’s course but I took everything else in physics. So I took electrical engineering because I chickened out and I wanted to finish on time. But I got interested in economics as a senior; I took a course in economics and at the time I thought, well you know, this just looks like physics and little did I know how deceptive that was. But anyway that was my naïve view at the time. |
| Q8 | Well if we turn to your research. I mean one of you has shown that people are frequently irrational in their economic decision making and the other has shown that the market mechanism works efficiently, at least in the lab. How would you reconcile these seemingly different views, I mean both of you? |
|  | Vernon Smith: Question to me?  To both of you.  Vernon Smith: Well, let me begin. I think it’s fairly simple. In experimental economics we have three classes of results. We have situations in which people do better for themselves as individuals and as a group and is predicted by economic theory. Ok. I call that super rationality. We have a second class of results where the predictions of economic theory do very well and they conform and I’ll come back to what those are shortly and thirdly we have results which people do not do as well as predicted by theory. The first is in two person extensive form games. We use the term personal exchange there. Too many people cooperate relative to the predictions of the economic, not everyone in single play games but up to half and so we have to come to terms with that. The experiments where the theory of markets does extremely well is where we’re talking about production and consumption markets; flows. We think of consumers as daily or regularly enjoying the value from what they buy in markets and producers regularly incurring a cost to produce that.  Those markets, if you study them in the lab are remarkable efficient and although their ability to converge to the equilibrium predicted by the theory varies with the institution, they basically all function quite well but the third class of phenomena where the predictions are not good is in what we call asset markets or capital markets and there’s an inherent uncertainty in stock markets and we capture that in these laboratory games and those do not converge quickly and easily to what you might call rational expectations equilibrium. If you hold the environment constant for kind of 3 times back, and now we’re talking about 6 hours of experiment, it gets home, ok, but that’s not very inspiring or encouraging because the world out there it doesn’t stand still while people look for the rational expectations equilibrium. So does that help to clarify?  Yes, I think so. Professor Kahneman?  Daniel Kahneman: Well the first comment I would make is about the word irrationality as characterising the research that we have done. I never think of myself as having demonstrated irrationality. There is a definition of rationality within the contest of economic theory or decision theory more broadly, which is a completely unrealistic conception of a human agent with a complete preference order about all states of the world, with a Bayesian set of beliefs about all possible states and this defines rationality in the context of economic theory.  Now as a descriptive hypothesis this is a totally implausible hypothesis and, you know, it is fairly easy to show that that hypothesis isn’t true and we’ve been doing that, my late colleague Amos Tversky and I, and many others. It’s also not particularly interesting to show that it isn’t true because it’s so easy to do. We have been able to show some of the ways in which people depart from this ideal of rationality but this is not irrationality. People are reasonable, they’re prudent agents. It’s just that the definition of rationality that is used in economic theory is, I think, a very implausible definition and it fails descriptively and we have been able to document some of these failures and explain them. |
| Q11 | Both of you have worked in the frontier between different fields; economics and psychology and natural sciences with experiments. What kind of difficulties or what kind of challenges did that implement for you? |
|  | Well, I think if you are curious about some of the things we observe in economics and you want to better understand this phenomena, whether it’s in a laboratory or the field, I really think you have to reach outside of economics because economics, although it has an incredible body of technique it’s developed and the methodology that has value it’s much too, I think, restrictive to embrace the range of observations. So you have to reach outside and actually if you go back to the Scottish philosophers, David Hume and Adam Smith, they were not narrowly oriented in the way that we often think of as modern economics. Adam Smith had huge breadth. He wrote the definitive History of Astronomy, 18th century. He was probably the first great post Newtonian scientist and he wrote on other aspects of human sociality besides just what has become known as economics narrowly construed. |
| Q8 | I guess he was a philosopher of moral philosophy wasn’t he? |
|  | Well, yes and of course we didn’t have a clear delineation of the fields like we have now. Well I think a lot of the work on experimental economics points us back to that period and a need to pick up on some of the inspiration that was behind people like Smith and Ferguson and Hume and others and I hope in fact my work will help to encourage that, not because they had it right, it’s because we know a lot more, obviously, after 200 years but there’s certain themes there that we’ve lost, that we’ve sort of abandoned and that’s unfortunate.  Professor Kahneman?  Daniel Kahneman: Amos Tversky and I started working together on the field of judgment and decision making and we were just doing psychology. So we were not intending to talk across the disciplines but eventually, and that came as a bit of a surprise to us, the work that we did was, to some extent, influential in economics. I mean this is of course why the prize is given because of the influence. Now what is remarkable to me about this is actually both the ease and the difficulty of communicating across disciplines. In our case I think we’ve had a very easy time. You know, it’s not that economists have flocked to the ideas that we’re bringing, you know, behavioural economics is a minority movement and not everybody is convinced of its value but by and large I would say that, you know, I have been quite surprised by, you know 20 years is a short time for ideas to have an effect and, you know, our ideas have had whatever influence they’ve had relatively quickly.  What impresses me is how chancy this is, that is this is entirely accidental, that is the communication if we had published exactly the same paper, which is cited in the award prospect theory, if we’d published that word for word in Psychological Review, in the Journal of Psychological Theory there would have been no Nobel Prize for this work today. So it was because it happened to be published in Econometrica that this happened.  But also the most cited paper in Econometric ever.  Daniel Kahneman: Yes, but in part it is a very highly cited paper. I should add, you know, this doesn’t make it all that influential in economics because most of the people who cite the paper are not economists. So I don’t know how many economists cited it; it’s a well known paper in economics as well but if it had been published in another discipline it would simply not have had an impact and I think this is in part what Vernon is saying, economists do not spontaneously look outside the discipline. So if you look at the journals that are cited in the economics literature they tend to be economics journals. So we were quite lucky, you know, in the sense that publishing in *Econometrica* and we did that because it was the prestige journal for decision theory at that time. We thought we had a good paper and so we sent it to the best journal that would publish it and we were then lucky in a completely different way in that a young and very brilliant economist, Richard Thaler, read about our work actually before it was published and was influenced by it and he really, not we, developed behavioural economics. So it was his doing and it was through his work that our work became known and he deserves a great deal of the credit for, you know, what’s happened since. |
| Q67 | Ok. Before we leave the research side of things, I mean you published your award winning papers before 50. Is there a life in research after 50 or is economics a young man’s game like mathematics? |
|  | Well I think that’s probably not unusual across most of the prizes although I’m just conjecturing that that’s probably true. Certainly [Albert Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) had some of his basic instincts before he was of age and particularly I think that’s true in physics but it’s young minds that tend to get inspired. I think it also has to do with the sociology of professional work. It’s young people who chart new courses that change things. It’s not the existing scientific community that suddenly has a transformation and, you know, my friend [Paul Samuelson](https://www.nobelprize.org/prizes/economic-sciences/1970/samuelson/facts/) points out that science progresses funeral by funeral.  Daniel Kahneman: Well, I mean to your question of whether there is life after 50, I certainly hope so. I think Vernon has done some of the work that, you know, has done wonderful work in very recent years so there may be life after 70 and, you know, to some extent this again is accidental and is self selection, that is if you are going to have people who are going to have important ideas, they may have them fairly early in their career and then they spend the rest of their careers elaborating on these ideas and so it looks as if, you know, people have their best ideas early on but that’s because they spend the rest of their lives working on them and I think, you know, this happens to most people who have one important theme to develop.  Vernon Smith: Well I think young people are sort of maybe more likely to make technical or mythological breakthroughs but just looking at my own history I didn’t really appreciate the full ramifications at the time I was doing that work. I didn’t come close to it and I think that can come with maturity so that the contribution you can make after aged 50, and I hope after age 75, is perhaps a different type of contribution than one makes when you’re younger.  Daniel Kahneman: Yes, I would echo that. It takes a long time to understand what you’ve been saying. So, you know, you say it first and then, over the years, you understand what it was that you really meant because you don’t know that immediately. In my particular case I was fortunate because there were two of us and that process of understanding what we said worked faster because we understood each other but when you work alone, understanding what you’re saying is a long drawn process.  Vernon Smith: Well and I recognise, understood early that a component of what I was doing had to do with institutions and rule systems but I didn’t begin to understand it the way I now do and a lot of that understanding came from interacting with other people over the years, Charlie Plott is a prime example, and also Martin Shubik, he and I are exactly the same age. I think I’ve known Martin for over 40 years, I think about 45 years and we often exchanged ideas, having to do with institutions and Martin was very interested in and that interchange was very valuable to me even though we never worked together and of course I worked with Charlie Plott and a lot of our insights came working together. |
|  | Oh, you know, there are lots of pieces of advice. I tell my graduate students that if they don’t fall asleep thinking about work, you know, then they’re not working hard enough actually, you know, so that’s the first piece of advice that you would give them and the second one is to try not to get trapped in uninteresting problems just because you began them. So one of the important things to have in science is to avoid the sunk cost fallacy, just to keep going with something just because you began it and have made an investment. So the ability to just make a quick turn when you’ve had an idea that looks better and drop everything else and follow the best idea that you have at the moment, that is certainly one of the thing that I think worked for me and I think it may work for other people as well.  Professor Smith?  Vernon Smith: Yes, I would echo that. Don’t follow the path of least resistance. Be prepared to break the informal rules. You have to of course live in your environment and so there’s a limit to how far you can go in breaking the rules. I was interested in experimental economics long before I could really make a living at it so I did other things and I didn’t get tenure doing experimental economics, I got it doing other things but I returned to experimental economics and one of the problems can be that in getting tenure you develop all these bad habits and then you can’t get out of doing the bad habits, which is doing what’s easy, you know, following your sunk cost and doing trivial kind of extensions of that.  Daniel Kahneman: And trying to salvage failures, that is when something is not working there are people who spend a lot of time trying to get something out of an experiment just because they did it. I’m sure that’s good advice to avoid that.  Vernon Smith: I think young people in economics are well advised to read widely outside of economics too and I think fairly narrowly within economics is good enough because the theories tend to be very similar anyway. Once you sort of get that basic model it’s better to … I read Science and I read Nature, those 2 magazines, they’re weeklies. I can’t follow everything in them but I do find things in there that intrigue me.  Daniel Kahneman: This is unusual, you know, in economics. 18 years ago when we first came to Berkeley, [George Akerlof](https://www.nobelprize.org/prizes/economic-sciences/2001/akerlof/facts/) invited me to co-teach a course with him and we taught a course on economics and psychology and there are two things to be said about that course. One is, he didn’t get credit for teaching it because it was considered a frivolous thing to do and the other one was that he kept advising and warning the students not to be seduced by it because he thought it could ruin their careers if they followed that path. So he would tell them, you had better stick to what he called meat and potatoes economics and, you know, you can afford to do those strange things after you get tenure.  Vernon Smith: Too late, he’d formed all his bad habits. |
| Q1 | Well how important has it been to you? I mean your scientific result can be applied to, what did Nobel say, to the benefit of mankind? I mean you followed your track, you found something that was interesting and pursued that? |
|  | Daniel Kahneman: I would say that the conscious sense of doing something that could be truly beneficial, well I had that early on when I thought that people could be educated to think, you know, more closely and these efforts of mine have not been rewarded. In recent years I’m consciously trying to do something for the benefit of mankind and this is to develop new and better measures of human welfare and human wellbeing that could be applied as another measure of how society is doing and I’m doing that with collaborators, including an economist at Princeton, Alan Krueger, and that is truly with the idea of trying to do something that could be useful to policy making.  You have something to say Vernon?  … the laboratory is a very useful tool for allowing people to get experience …  Vernon Smith: Well, I’ve become more interested, particularly in the last 20 years roughly as against the first 20 years, in utilising what we’ve learned about markets to do a better job of helping to design markets in new areas where people don’t have any experience, any field experience and I think the laboratory is a very useful tool for allowing people to get experience. It doesn’t provide the final experience and the final answers but the point is it is experiential and it gives people an opportunity to try out and test bed new rule systems, practitioners for example, and we found business and government, in some situations, very receptive to that. In governments, particularly in New Zealand and Australia, with respect to the liberalisation of electric power and I think we’ve seen in the California fiasco how bad things can be if you don’t think about some fundamental issues and furthermore, in that case, a lot those issues had long been studied in the laboratory and in fact influenced New Zealand and Australia but we didn’t have an opportunity to have that much influence in California. It isn’t that we didn’t communicate with any of the people that might have made a difference, it’s just that we couldn’t convince them or influence them enough. And also it turned out to be far worse than even we would have imagined as to the consequences of really not getting some elementary features of these markets right.  Daniel Kahneman: They need some help. Many years ago when we were studying failures of rationality in judgement and decision making I thought, you know, that there was a contribution to be made, for example to government decision making or to making political decisions but 25-30 years ago that was a period when the discipline of decision analysis looked extremely helpful and hopeful and it seemed as if the combination of psychology and decision analysis could be very, very useful. It has not been. I mean by and large I think this has been a failure and the reasons are quite interesting. The reasons are that the leaders do not want the help, that is the people who make decisions, important decisions, by and large do not want the kind of help that decision analysis or decision aids have to offer that we would think, you know, would certainly improve the quality of decision making. So there is a great deal of resistance and that, by itself, is quite interesting, that it’s been 30 or 40 years, you know, since decision analysis was first proposed and, by and large, very little has happened. |
| ID | 0846 |
| Biographical | The early years to 1975 I was born Vernon Lomax Smith in Wichita, on the flat plains of Kansas, January 1, 1927 in the years leading to the Great Depression. Like many of my generation I am a product of the strange circumstances of survival, and of successes built on tragedy. This narrative is written from memory and impressions, which are notoriously subject to error. Therefore I have carefully verified certain dates and events from family records, newspaper clippings and published Wichita history that provide some mileposts for accuracy checks.  Grover Bougher, my mother’s first husband and father of her two oldest children, was a fireman – they shoveled coal and maintained their locomotive’s steam pressure – on the Santa Fe railroad. I have a letter Grover wrote to his brother, George, a Private in the American Expeditionary Force, dated October 3, 1918, postmarked in Newton, Kansas on the 4th. On October 5, Grover was killed instantly in a train wreck – not uncommon at the time – when his passenger train engine and its cars were diverted, by a manual cutover switch left open, onto the sidetrack, colliding with a waiting freight engine. The letter was returned to Newton, postmarked the following April with a notation by the Command P.O. that George had been killed on September 17, 1918 fighting the war in France. Neither brother knew of the other’s death. When Grover was killed, my grandfather to be had been laid up for some weeks with a badly injured leg caused by another railroad accident. He was an engineer on the Missouri Pacific Railroad, and was injured as he and his fireman leaped from the engine cab when one of the drive shafts bolted loose from the drive wheel and flailed up through the cab’s wood flooring.  Grover’s tragedy proved pivotal. The life insurance money provided to my mother by the Santa Fe Railroad, and augmented by a retail job selling shoes, guaranteed a less arduous survival to a twenty-two year old widow with two girls, age 3 and 4.5. In those days aid to dependent children came from family and friends, in this case her parents, whose home she moved into. My maternal grandfather and his twin brother had been orphaned at age 3-4, and were sent to a relative’s nearby farm, boys being in demand for farm labor.  Fortune smiled for us all when my father Vernon Chessman Smith (1890-1954), a machinist who had apprenticed in Cleveland Ohio, met my mother, Lulu Belle Lomax (Bougher) (1896-1957) in Wichita, and was delighted to find a warm and caring woman already with a family. My mother often mentioned that she had not intended to have any more children, but my father so loved children, particularly her daughters, that it was unthinkable for her not to have one child by him. If I had been a girl, my name would have been Verna. My father had a brother, Norman (1888-1946), a wildcat driller who had followed his father, a tool dresser, into the oil fields, and a sister, Izella (1892-1918), who died in the great flu epidemic, two months after Grover Bougher had widowed my mother.  The life insurance money was invested in a farm located about 45 miles from Wichita, which was to become our sole means of survival during the difficult years, 1932-1934. A machinist with the Bridgeport Machine Co., maker of oil field equipment, my father was laid off in 1932 for lack of enough work, and the three of us moved to the farm located near Milan, Kansas. My older sisters remained in Wichita, one finishing High School, the other having left high school, and married in 1931.  The farm brought hard work and hard times for my parents – our house had no indoor water, electricity or toilet facilities – but for me it yielded memories of adventure, and learning about chickens (one of which I befriended as a pet until its penchant for following us into the house resulted in a broken neck as the screen door slammed shut behind my mother), milk cows, hogs, gardens, grain crops, priming well pumps, Coleman Lanterns and Kansas windstorms. The farm proved an invigorating childhood with ample opportunity for daily, fatherly lessons in ‘how things work’ – an interest I have carried the breadth of my life. I learned when and how to milk cows and put them to pasture, feed the hogs, chickens and horse (we could afford only one horse, and perforce had to borrow another for plowing), and to tag along and watch my father repair fences, gates, hog sheds, and barn doors, store hay in the barn loft and shoot rabbits for the table with his father’s 1890 vintage lever action Winchester 12 Gauge shotgun.  From my mother I learned about cooking on a wood stove in the Kansas prairie where there is precious little wood, so we supplemented by burning dried corncobs, and – euphemistically – dried sun-baked ‘cow chips.’ The early settlers had burned Buffalo (sic, Bison) chips, as had the Indians before them. Decades latter I learned that the first Americans who crossed to Alaska on the land bridge from Siberia burned Mammoth chips, and I felt connected to those ancient peoples of 14,000 years ago. I can still memory – taste the fresh buttermilk pancakes and hot buttermilk biscuits – both made with lard! – that were cooked on the top, or in the oven, of that ancient iron stove. If the egg basket was empty, I can remember being sent to the chicken coop to gather fresh warm eggs, occasionally containing two yolks, for the pancakes. I also remember my mother axing the head of a chicken for the table, noting that she did not relish that necessity. Yes, long hours and a hard life for my parents, but for a six to seven year old every new day dawned with fresh excitement when you have not a care in the world, and so much to learn and witness.  At age six I took my place alongside more seasoned farm children for primary education in the classic rural one-room schoolhouse. A neighbor, Mr. Hemburger, had the distinction of being able to read and write, and therefore was deemed fully equipped to be our grade school teacher. A wise decision, I think, although my mother was always a bit irritated when he used the word “ain’t,” a completely grammatical contraction of which English has many; Mom was something of a language maven. Each morning my teacher/neighbor faced six rows of desks. The first row on his right, where I sat, was grade 1, the second row to his left, grade 2 and so on for all six grades. After some first row recitation, and left with an assignment, I had the opportunity to listen in on the second and third grade recitation lessons – the grades seated closest to me. As I later became aware, this classroom implemented the original progressive system, in which you were part of a single seamless community consisting of six grades. At the end of grade 1, Mr. Hemburger gave me a note to take home to my mother, stating unceremoniously, that ‘Vernon can read the second grade reader and therefore next year he will be in the third grade.’ There were of course only three subjects – reading, writing and arithmetic. Reading was the litmus test; if you were less strong in arithmetic, or writing, the next year you could participate along with those in the row on your left, before Mr. Hemberger got to your row. The whole purpose of this management style was to move each person along at her own pace of accomplishment, get her through school and into farm work where she could be useful. I understand that the earliest achievement tests showed high performance in Kansas and Nebraska because of these rural schools.  In 1934 my father returned to Bridgeport Machine for part time work, and subsequently full time work. This was fortuitous, as we lost ownership of the farm to the mortgage bank, unable to meet payments on the loan, and we had to move back to Wichita. This confirmed my mother’s political commitment to socialism, but my father seemed to take it in stride. At some point in the 1930’s this man refused to apply for help from WPA (the Works Project Administration – my grandfather always said that WPA meant ‘we piddle around’) because he considered it demeaning, a point of contention with my mother who thought he was being completely unreasonable. In 1940 he lost his job permanently when the independent entrepreneur owner of Bridgeport, A. A. Bushaw, closed his factory rather than cede control of his oil field equipment factory to President Franklin Roosevelt’s defense industry production. (Of course, the plant was taken over – by Culiver Aircraft – and used for defense, then war production). My father changed employment to the Coleman Lantern Company for about a year, and then he went to work for Stearman Aircraft, which had been purchased by Boeing in 1938. Lloyd Stearman manufactured the famous PT-13 and PT-17 Kaydet biplane that was a U.S. Primary Trainer then and through much of World War II. In 1941 Boeing started construction of Plant II, where the B-29 was to be built, and the Stearman plant was renamed the Boeing-Wichita Plant I. By 1945, 1000 B-29s and over 10,000 Kaydets had been manufactured by Boeing-Wichita.  Wichita and farm life were separated by location, intellectual and economic activity. The city had homegrown a surprising breadth of business life. Beech, Stearman and Cessna Aircraft, Coleman Lantern Company, Fred Dold meatpacking, and the Fred Koch, Jack Vickers and other petroleum companies provided tangible initial evidence of the machinery of markets, specialization and globalization. Bold independent actions by Coleman, Cessna, Beech, Koch, and many others, instilled a mid-western sense of freedom and entrepreneurship (if not always unqualified patriotism, as with Bushaw). When Walter Beech died, his wife, Olive Ann Beech, cofounder of their company, took over the management of Beech Aircraft to become one of the great early women executives. She built it into an internationally successful manufacturer of light planes, and continued to be active on the Board well into her eighties.  After my stellar first grade academic achievements, I continued to perform well in the city primary schools – except for penmanship, which was not my forte. My school performance, however, deteriorated beginning in the eighth grade and all through high school. I found High school very uninspiring – young coeds were far and away more interesting – but I always expected to go to college. I vividly recall that my mother helped me with my English homework when we were learning to diagram sentences in the 9-10th grades, and complaining about the deterioration in the quality of the public schools, circa 1941-2. She had learned to diagram sentences in the 7-8th grades. It was not evident to me why one should learn it in any grade, but what did I know? I have often wondered whether my mother’s socialist constructions would have survived her pragmatism, and the reality of poor performance in socialist controlled production as well as education.  In the 9th grade I began my first wage work for the West Side Drug store delivering prescriptions and sundries on my bicycle to customers who called in orders. I still have my original social security card signed when I was 13. (To my mother, social security was a great invention. Not realizing that it was just a tax, she wanted me to enroll early. Here I sit 63 years later receiving SS payments of $1900 per month. I wonder what the payments would be if SS had been vested). Between deliveries I waited on customers, learned to ‘soda jerk’ on an old fashion drug store fountain (cokes, milk and malt shakes, ice cream Sundays and sodas, etc.). I was paid fifty cents for a six-hour shift plus occasional tips. In 1941 I finished at Allison Intermediate School (grades 7-9), and started at North High School, commuting by bicycle about 5 miles from home to school. On the way was a restaurant and fountain called the OK Drive Inn, owned and operated by Don Eaton, who was a restaurateur known to my family for years. Throughout the 1930’s my mother home-baked desserts that Don bought for his restaurant – angel food cake, chocolate devil food cake and Boston cream pie were most common – for 25 cents each. At age 14, with my soda fountain experience, Don offered me a job at $1 per day, summers and weekends. That seemed like good wages to me. I remembered in the early 1930s my father made $1 a day working as a temporary construction carpenter making and installing windows. I operated the fountain, learned to cook and Don later gave me a raise to $8 per six-day week.  In the summer of 1943, at age 16, I applied for an entry level job at Boeing Aircraft, and went to work at an incredible – to me – starting wage of 60 cents per hour, with a 10 cent premium for working the grave yard shift from midnight to 8 am. I was earning $5.60 per day. I also attended summer school at North High so that when the fall term started I could graduate in January 1944 by taking only two courses. In that way I could finish High School and continue at Boeing. It was very demanding, but did not become burdensome until the following December when Boeing converted from three 8-hour shifts to two 10 hour shifts per day. I made it through, and two months later, on January 20, I graduated.  At Boeing, because of my high school training in electricity principles and practice, I was in the Functional Testing Department where I studied the training manuals for the Central Fire Control (gun operation) System on the B-29. It was the first high altitude bomber with pressurized cabin for the crew-pilot, co-pilot, engineer, bombardier, nose gunner, two side gunners, an upper gunner and a tail gunner. The gun turrets were each remotely controlled by gun sights with electro mechanical selsyn (self synchronous) motors located in the heated and pressurized cabin areas. My division had responsibility for trouble shooting the system and aligning the gun sights with the firing trajectories. We learned about compensating for windage jump as the spinning bullets emerged from the barrels into the high velocity air, and ‘leading the target’ to account for differential air speed between source and target. This was the first Buck Rogers armament system, and it was fascinating. Some time after I got up to speed I requested and was granted transfer to the second shift flight crew. On this assignment, I would be part of the crew that did the final alignment checks on the system, and fire tested all the guns on the ground. There was a large abutment of stacked wood beams backed by an earth fill into which all the guns were fired. Each plane was set parallel to the firing range. The lower aft, lower forward and upper aft turrets (two fifty caliber machine guns in each) and the upper forward (four fifties) were aligned on the target range, and we fired 25 rounds through each gun. The plane was then turned 90 degrees and the tail turret, consisting of two fifties and a 20 mm cannon, were also fire tested, except that the air force required 50 rounds to be fired through the cannon; I never quite understood why it was called a cannon, as it spit out rounds like a machine gun. We pulled all the barrels and loading bolts, cleaned and reinstalled them in the guns, the air force bought the plane, and it was flown to its base. I was turning 17 years old and Boeing was actually paying me to do this thing called ‘work!’ It ended in August 1944, after 15 months, when I resigned to begin college.  Our sole family ‘intellectual’ was my mother’s uncle, Sullivan Lomax, who injured his leg as a child in a farm accident. It was not set properly, and left him handicapped. Being ill suited for arduous farm work, he studied law at the University of Kansas, passed the Kansas Bar exam, and practiced law in Cherryvale, Kansas his entire life. My mother and indeed all of us were proud of him! Consequently, neither I, nor my parents, or anyone in my family, or any neighbor or friend, had any idea how to go about choosing a college. So, I went to the city library, found a book on choosing a college, and learned among other things that the ‘best’ college in the United States was Caltech. Being naïve and impetuous I decided that I should prepare myself to enter Caltech, as, without preparation, my ‘C’ average in High School would not even qualify me to take the entrance exam. A serious Quaker College, Friend’s University, was located near my home in West Wichita. I enrolled in physics, chemistry, calculus, astronomy and literature courses for one year, earned top grades, and sat for the entrance exams for Caltech.  The exam consisted of problems: how fast must a snowball be thrown against a wall to melt on impact? Lets see now, if the mass of the snowball is ‘m,’ and its velocity, ‘v,’ then its kinetic energy is (1/2) mv2. Since it takes C (was it 528?) calories of heat energy to convert each gram of snowball (ice) into water, you had only to equate Cm with the kinetic energy, and solve for ‘v’ with suitable account taken of units.  I passed, took the Santa Fe’s California Limited passenger train west out of Newton through La Junta, Colorado, Tucumcari, Clovis, Gallop and Albuquerque, New Mexico; Winslow, Flagstaff and Kingman, Arizona; and Barstow, California, arriving in Los Angeles in September, 1945. It was ‘all the way on the Santa Fe,’ as the concessionaire would announce in each car as he went through with peanuts, gum, candy, pop and cigarettes. This route would become familiar in the next four years, whether I was traveling by train or driving the parallel Route 66.  Caltech was a meat grinder like I could never have imagined. I studied night, day, weekends and survived hundreds of problems, but what a joy to take freshman chemistry from [Linus Pauling](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1954/index.html), hear physics lectures by J. Robert Oppenheimer on his frequent visits to Caltech, attend a visiting lecture by [Bertrand Russell](https://www.nobelprize.org/nobel_prizes/literature/laureates/1950/index.html), and regularly see von Karman, Anderson, Zwicky, Tolman, [Millikan](https://www.nobelprize.org/nobel_prizes/physics/laureates/1923/index.html) and other legendary figures of that time, on campus.  I was majoring in physics, but switched to electrical engineering, which was in the same division (Mathematics, Physics and EE) as a senior. In this way I did not have to take the dreaded “Smyth’s course,” required for physics majors, but not EE, and received my BS on schedule in 1949. At the time I relished the unbending facts and mathematics of physics/engineering. Then, as a senior, I took an economics course and found it very intriguing – you could actually learn something about the economic principles underlying the claims of socialism, capitalism and other such ‘isms.’ Curious about advanced economics, I went to the Caltech library, stumbled upon two books, [Samuelson](https://www.nobelprize.org/nobel_prizes/economics/laureates/1970/index.html)‘s *Foundations*, and von Mises’ *Human Action*. From the former, it was clear that economics could be done like physics, but from the latter there seemed to be much in the way of reasoning that was not like physics. I also subscribed to the *Quarterly Journal of Economics*, and one of the first issues had a paper by Hollis Chenery on Engineering Production Functions. So, economics was also like engineering! I had not a hint then as to how much those first impressions would be changed in my thinking over the decades to follow. But in 1962, my book, *Investment and Production*, would have a chapter on engineering production functions.  After graduating in engineering I went to the University of Kansas to get an MA in economics as a vehicle for allowing me to decide if I wanted to continue in economics. At KU I took classes from Dick Howey: price theory, math economics, imperfect competition, but significantly, a full year course in the Development of Economic Thought. Howey was a surviving member of an endangered species, a History of Economic Thought scholar, but it was from him that I learned what scholarship really meant. To be good at whatever you did, you needed to acquire knowledge of all the supporting structure, tools and primary sources of inspiration. If you were Howey, that meant knowing mathematics and being fluent in French, German and Italian. As one who just barely knew English, he much impressed me. His model seemed just right and it generalized to whatever might interest you. With Dick as a mentor, I decided economics was for me, and I continued by pursuing an economics Ph.D. at Harvard beginning in 1952. I met Joyce Harkleroad at KU. We were married in 1950. A year later she gave birth to twins, Deborah and Eric in that order, and I was part of a rapidly growing family.  At Harvard, I had macroeconomics from Alvin Hansen – the foremost American Keynesian, but he was also very eclectic. You read everything from Foster and Catchings to [Hayek](https://www.nobelprize.org/nobel_prizes/economics/laureates/1974/index.html), and not only Keynes, his interpreters and critics – Hicks, Samuelson, Metzler, [Friedman](https://www.nobelprize.org/nobel_prizes/economics/laureates/1976/index.html), etc. The Keynesian economics was tempered by the dry wit of Gottfried Haberler, the sarcasm of [Wassily Leontief](https://www.nobelprize.org/nobel_prizes/economics/laureates/1973/index.html), Guy Orcutt’s deeply serious search for the messages hidden in all data, Alexander Gershenkron who lectured on ‘ven Breetan vas ze voikshop of ze woild,’ and a coterie of graduate students trying to make sense of it all for their own careers. When Fritz Machlup visited, I wondered how the two polite Austrians – he and Haberler – would determine which one would get through a door first. Schumpeter was no longer alive, but his ghost was lurking in the halls with Haberler countering any claims that inflation (‘ze monster’ to Schumpeter), if not too large, was good for the soul and spirit of the economy.  For micro I supplemented with courses Samuelson taught down the Charles River at MIT. After Caltech, Harvard seemed easy, and I got virtually straight A’s. I remember that my classmate Dick Quant and I always scored high, and close together, on exams. But at best we were vying for second – the top score always went to Barbara Jay, who married an artist and dropped out before dissertation time. Graduate school is an endurance test, but was not that demanding for me after having survived the undergraduate meat grinder.  In the Spring of 1955, as I was finishing my PhD dissertation, and Joyce was typing it, my second daughter, Torrie, was born, and in August we moved to my first teaching post, at Purdue University, nestled in comfortably familiar Midwestern plains. In the Autumn semester, 1955, I taught Principles of Economics, and found it a challenge to convey basic microeconomic theory to students. Why/how could any market approximate a competitive equilibrium? I resolved that on the first day of class the following semester, I would try running a market experiment that would give the students an opportunity to experience an actual market, and me the opportunity to observe one in which I knew, but they did not know what were the alleged driving conditions of supply and demand in that market.  But let me backtrack to 1952. Many generations of Harvard graduate students had been exposed to E. H. Chamberlin’s beginning graduate course in Monopolistic Competition. On the first day he would set the stage for the semester using a classroom demonstration experiment showing that competitive price theory was an unrealistic idealization of the real world. He gave half the class buyer reservation values, and the other half seller reservation costs. The value/cost environment was like Bohm-Bawerk’s (1884/1959, book III, pp 207-235; 432-436) representation of supply and demand in a horse market with multiple buyers and sellers in two-sided competition – perhaps Chamberlin’s source of inspiration. I knew Bohm-Bawerk because of Dick Howey’s course, but I did not pick up on this similarity until much later. Chamberlin, unlike Bohm-Bawerk’s description, had the buyers and sellers circulate, form pairs, and bargain over a bilateral trade; if successful the price was posted on the blackboard; if not successful, each would seek a new trading partner. This continued until the market was closed. The prices in sequence were volatile and failed to support the equilibrium prediction. Chamberlin used this first-day exercise to set the stage for his theory of monopolistic competition. I decided to use the same value/cost setup but changed the institution. Secondly, I decided to repeat the experiment for several trading periods to allow the traders to obtain experience and to adapt over time, as in Marshall’s conception of the dynamics of competition. For the institution, I reasoned that if you were going to show that the competitive model did not work, then you should choose a more competitive trading procedure, so that when the competitive model failed to predict the outcomes, you would have a stronger case than had been made by Chamberlin. I went to the Purdue Library and found a book by Lefler, *The Stock Market* (1951), giving details on the bid/ask double auction used in the stock and commodity Exchanges. In January 1956 I carried out my plan. To my amazement the experimental market converged ‘quickly’ to near the predicted equilibrium price and exchange volume, although there were ‘only’ 22 buyers and sellers, none of whom had any information on supply and demand except their own private cost or value. I thought perhaps that it was an accident of symmetry in the buyer and seller surpluses. I shot that idea down with an experiment later using a design in which the seller surplus was much greater than that of the buyers. Thus did I seem to have stumbled upon an engine for testing ideas inside and outside traditional economic theory.  Over the years 1956-1960 I continued to do many variations on this original experiment, altering the supply and demand environment, examining shifts in the demand or supply, varying the trading rules and introducing cash rewards showing that they made a difference. I gradually became persuaded that the subjects, without intending to, had revealed to me a basic truth about markets that was foreign to the literature of economics. I reported my early experiments – crude as they were compared with what I would later learn to do – in a paper, accepted for publication in *The Journal of Political Economy*, 1962, after two revisions, four negative referee reports and an initial rejection. (See, Smith, *Papers in Experimental Economics*, Cambridge University Press, 1991, pp 157-158).  During these years most of my research and teaching dealt with capital and investment theory, and corresponding pricing problems (Smith, 1961, *Investment and Production*, Harvard University Press). In 1961-1962 I was a visiting associate professor at Stanford, and at the beginning of the Autumn quarter had the significant experience of meeting Sydney Siegel and discovering that we had both been doing experimental economics. Unknown to both of us at the time, [Reinhard Selten](https://www.nobelprize.org/prizes/economic-sciences/1994/selten/facts/) had also been pioneering economics experiments in Germany. Syd – a truly powerful experimental intellect – strongly influenced me in becoming committed to experimental economics, but he died unexpectedly at age 45 within a few weeks of our meeting. Eventually, I read all of his publications, including his classic, *Nonparametric Statistics*, and his two books coauthored with L. Fouraker (1960, 1963). Syd was not only a master experimentalist; he also used theory and statistics with skill in the design and analysis of experiments.  The growth of my research interests in experiment, together with a modest literature by myself, Siegel, Fouraker and Siegel, Edwards in choice under uncertainty, Anatol Rapoport in Prisoners Dilemma, and others in experimental matrix games, led me to initiate a graduate seminar in experimental economics at Purdue in 1963, which continued until my departure in 1967. In that first seminar I had thirteen students, including Don Rice, Hugo Sonnenschein, Norm Weldon, and Tom Muench, whose careers I have followed. I published new papers in experimental economics in 1964 (with Don Rice), another in 1964, then in 1965 and 1967. Several working papers by students and faculty were also spawned by this effort. In 1963 and again in 1964, under the enabling and supporting influence of Dick Cyert, Jim March (and probably [Herb Simon](https://www.nobelprize.org/nobel_prizes/economics/laureates/1978/index.html) in the background), Lester Lave and I conducted Ford Foundation faculty summer research workshops at Carnegie Mellon. With several experimental papers in the pipeline, and a seminar going, experimental economics was becoming much more than a hobby for me.  In 1967 Joyce, Eric, Deborah, Torrie and I moved to Sherborn, Massachusetts, where we would live until 1972, with Joyce serving in her first position as a Unitarian minister. I was able to get a tenured professorship, first at Brown University, then in 1968 at the University of Massachusetts. Our children had finished high school by 1972, and I accepted a fellowship at the Center for Advanced Study in the Behavioral Sciences, 1972-1973. During all of this period my research turned to the economics of uncertainty, financial theory of the firm, and natural resource economics, but I continued to think about experimental economics and to use it in teaching. This was good in that my brain was continuing to work on experiments, and I was developing a fresh perspective. Also, Charles Plott and I talked experiments (for example the idea of induced valuation) on many bass fishing trips to Lake Powell. At the Center, thanks to the encouragement of the anthropologist, Bob Heiser, I wrote my ‘Pleistocene Extinctions’ paper, and submitted it to the AER. After a year’s delay I received a letter of rejection along with three favorable referee reports. The editor explained that he had been unconvinced by the first two, sought a third report, and was still not convinced. So I sent it to the *Journal of Political Economy* (1975) along with the letter and the three reports; the editor sent it to one referee, it was accepted and published in about six weeks.  While at the Center I continued to be in touch with Charles Plott, who wanted to join me in ‘getting into experimental economics.’ Caltech offered me a Sherman Fairchild Distinguished Scholar position for one year, and this provided the vehicle for us to offer a seminar for student credit in the Spring quarter of 1974. Charlie and I updated my old outline and notes from Purdue, and we had regular meetings attended by three paying customers (including an undergraduate, Ross Miller), and several faculty, including Mo Fiorina, John Ferjohn, Roger Noll, Jim Quirk, Lance Davis and Bill Riker, also a visiting Fairchild scholar that academic year from Rochester. Bill had done some political science game theory experiments and we were off and running. Afterwards at Caltech, experiments, including political economy, would be central to their teaching and research program. Later Bill reported that he had agreed to write a paper for an editor on experimental methods in political science, but after the course reversed his decision, because the seminar had changed his thinking about experiments, and he wanted to reflect more on the experience. I think that story summarizes well the intellectual ferment produced that semester.  As an interesting side-note, I had developed what I called the “Theory of Induced Valuation,” in lecture notes in my Purdue Seminar sometime in the period 1963-65. I talked from those notes in our Caltech seminar in 1974. Charlie pointed out to me that these ideas were catching hold and he and others needed something to cite so at some point I included them in a methods write-up in the Caltech working paper series. Later, I did a write-up of induced valuation for the *American Economic Review* (1976) and it was finally published long after its original genesis. The Nobel citation included that paper.  Except for some loose publication ends for me to complete in resource economics, I was firmly back into experiments, and Charlie, Mo, John and others were creating experimental public choice. I stayed on in 1974-75 with a joint appointment at Caltech and USC, we wrote up our experiments for Miller, Plott and Smith (*Quarterly Journal of Economics*, 1977) (this must have been the first scientific paper in economics with an undergraduate coauthor), Charlie and I wrote our paper on comparing institutions for the *Review of Economic Studies* (1978), and I started the experiments that would lead to a series of papers testing the incentive properties of various public good mechanisms (1977-1984).  I considered staying on at Caltech; also, going to Northwestern, where I had close friends like John Hughes and Stan Reiter, but I feared the tug of a silver cord, and that ‘you can’t go home again.’ I was looking for new opportunity; I could not describe it, but I thought I would know it when I saw it. In 1974 I gave a seminar at the University of Arizona. Rene Manes, a student from Purdue days, was Dean of the College of Business, and they were interested in bringing me to Arizona. I returned in 1975 to give another seminar, visited with their administration, and sensed that this was what I was looking for. They had some recruiting successes in the College, but had much work ahead in building the faculty. Most impressive, however, was a committed top administration: John Schafer, President; Gary Munsinger as Vice President, and Al Weaver, a tough minded Provost. I decided to move to Tucson. Carol Breckner moved with me. Subsequently we married and our son, Joshua, was born in 1981.  In retrospect that was a good decision. I was there for 26 years, before leaving for George Mason University. That is a long and exciting story that continued the basic work begun at Purdue, which formed the primary citation by the Nobel Foundation. That story begs to be told, but I cannot possibly do it justice in the limited time and space I have for this narrative.  Stay tuned. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview |  |
| Interview |  |
| Q1 | Well Professor Kahneman, what made you decide to become a scientist? |
|  | Daniel Kahneman: I wanted to be a philosopher actually and I decided, I became interested in psychology as a substitute for philosophy, as a way of answering questions about the human condition but answering them by looking at facts rather than by discussing words. So that’s how I became a psychologist.  And Professor Smith, your answer too?  Vernon Smith: Actually I was interested in philosophy at one point. This is before I really was seriously into my undergraduate training, so it was just a reading interest of my own and I read Sir James Jeans’ Physics and Philosophy and Sir Arthur Eddington’s books and Burton Russell; I was interested in science and philosophy and originally I expected to study science and I went to Caltech for that reason and I did study physics and would have taken my degree in physics, my undergraduate degree, except for one hurdle.  So I took electrical engineering because I chickened out …  To take the degree in physics you had to take Smyth’s course, a famous course that was very, very hard to get through. By taking my degree in electrical engineering I didn’t have to take Smyth’s course but I took everything else in physics. So I took electrical engineering because I chickened out and I wanted to finish on time. But I got interested in economics as a senior; I took a course in economics and at the time I thought, well you know, this just looks like physics and little did I know how deceptive that was. But anyway that was my naïve view at the time. |
| Q8 | Well if we turn to your research. I mean one of you has shown that people are frequently irrational in their economic decision making and the other has shown that the market mechanism works efficiently, at least in the lab. How would you reconcile these seemingly different views, I mean both of you? |
|  | Vernon Smith: Question to me?  To both of you.  Vernon Smith: Well, let me begin. I think it’s fairly simple. In experimental economics we have three classes of results. We have situations in which people do better for themselves as individuals and as a group and is predicted by economic theory. Ok. I call that super rationality. We have a second class of results where the predictions of economic theory do very well and they conform and I’ll come back to what those are shortly and thirdly we have results which people do not do as well as predicted by theory. The first is in two person extensive form games. We use the term personal exchange there. Too many people cooperate relative to the predictions of the economic, not everyone in single play games but up to half and so we have to come to terms with that. The experiments where the theory of markets does extremely well is where we’re talking about production and consumption markets; flows. We think of consumers as daily or regularly enjoying the value from what they buy in markets and producers regularly incurring a cost to produce that.  Those markets, if you study them in the lab are remarkable efficient and although their ability to converge to the equilibrium predicted by the theory varies with the institution, they basically all function quite well but the third class of phenomena where the predictions are not good is in what we call asset markets or capital markets and there’s an inherent uncertainty in stock markets and we capture that in these laboratory games and those do not converge quickly and easily to what you might call rational expectations equilibrium. If you hold the environment constant for kind of 3 times back, and now we’re talking about 6 hours of experiment, it gets home, ok, but that’s not very inspiring or encouraging because the world out there it doesn’t stand still while people look for the rational expectations equilibrium. So does that help to clarify?  Yes, I think so. Professor Kahneman?  Daniel Kahneman: Well the first comment I would make is about the word irrationality as characterising the research that we have done. I never think of myself as having demonstrated irrationality. There is a definition of rationality within the contest of economic theory or decision theory more broadly, which is a completely unrealistic conception of a human agent with a complete preference order about all states of the world, with a Bayesian set of beliefs about all possible states and this defines rationality in the context of economic theory.  Now as a descriptive hypothesis this is a totally implausible hypothesis and, you know, it is fairly easy to show that that hypothesis isn’t true and we’ve been doing that, my late colleague Amos Tversky and I, and many others. It’s also not particularly interesting to show that it isn’t true because it’s so easy to do. We have been able to show some of the ways in which people depart from this ideal of rationality but this is not irrationality. People are reasonable, they’re prudent agents. It’s just that the definition of rationality that is used in economic theory is, I think, a very implausible definition and it fails descriptively and we have been able to document some of these failures and explain them. |
| Q11 | Both of you have worked in the frontier between different fields; economics and psychology and natural sciences with experiments. What kind of difficulties or what kind of challenges did that implement for you? |
|  | Well, I think if you are curious about some of the things we observe in economics and you want to better understand this phenomena, whether it’s in a laboratory or the field, I really think you have to reach outside of economics because economics, although it has an incredible body of technique it’s developed and the methodology that has value it’s much too, I think, restrictive to embrace the range of observations. So you have to reach outside and actually if you go back to the Scottish philosophers, David Hume and Adam Smith, they were not narrowly oriented in the way that we often think of as modern economics. Adam Smith had huge breadth. He wrote the definitive History of Astronomy, 18th century. He was probably the first great post Newtonian scientist and he wrote on other aspects of human sociality besides just what has become known as economics narrowly construed. |
| Q8 | I guess he was a philosopher of moral philosophy wasn’t he? |
|  | Well, yes and of course we didn’t have a clear delineation of the fields like we have now. Well I think a lot of the work on experimental economics points us back to that period and a need to pick up on some of the inspiration that was behind people like Smith and Ferguson and Hume and others and I hope in fact my work will help to encourage that, not because they had it right, it’s because we know a lot more, obviously, after 200 years but there’s certain themes there that we’ve lost, that we’ve sort of abandoned and that’s unfortunate.  Professor Kahneman?  Daniel Kahneman: Amos Tversky and I started working together on the field of judgment and decision making and we were just doing psychology. So we were not intending to talk across the disciplines but eventually, and that came as a bit of a surprise to us, the work that we did was, to some extent, influential in economics. I mean this is of course why the prize is given because of the influence. Now what is remarkable to me about this is actually both the ease and the difficulty of communicating across disciplines. In our case I think we’ve had a very easy time. You know, it’s not that economists have flocked to the ideas that we’re bringing, you know, behavioural economics is a minority movement and not everybody is convinced of its value but by and large I would say that, you know, I have been quite surprised by, you know 20 years is a short time for ideas to have an effect and, you know, our ideas have had whatever influence they’ve had relatively quickly.  What impresses me is how chancy this is, that is this is entirely accidental, that is the communication if we had published exactly the same paper, which is cited in the award prospect theory, if we’d published that word for word in Psychological Review, in the Journal of Psychological Theory there would have been no Nobel Prize for this work today. So it was because it happened to be published in Econometrica that this happened.  But also the most cited paper in Econometric ever.  Daniel Kahneman: Yes, but in part it is a very highly cited paper. I should add, you know, this doesn’t make it all that influential in economics because most of the people who cite the paper are not economists. So I don’t know how many economists cited it; it’s a well known paper in economics as well but if it had been published in another discipline it would simply not have had an impact and I think this is in part what Vernon is saying, economists do not spontaneously look outside the discipline. So if you look at the journals that are cited in the economics literature they tend to be economics journals. So we were quite lucky, you know, in the sense that publishing in *Econometrica* and we did that because it was the prestige journal for decision theory at that time. We thought we had a good paper and so we sent it to the best journal that would publish it and we were then lucky in a completely different way in that a young and very brilliant economist, Richard Thaler, read about our work actually before it was published and was influenced by it and he really, not we, developed behavioural economics. So it was his doing and it was through his work that our work became known and he deserves a great deal of the credit for, you know, what’s happened since. |
| Q67 | Ok. Before we leave the research side of things, I mean you published your award winning papers before 50. Is there a life in research after 50 or is economics a young man’s game like mathematics? |
|  | Well I think that’s probably not unusual across most of the prizes although I’m just conjecturing that that’s probably true. Certainly [Albert Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) had some of his basic instincts before he was of age and particularly I think that’s true in physics but it’s young minds that tend to get inspired. I think it also has to do with the sociology of professional work. It’s young people who chart new courses that change things. It’s not the existing scientific community that suddenly has a transformation and, you know, my friend [Paul Samuelson](https://www.nobelprize.org/prizes/economic-sciences/1970/samuelson/facts/) points out that science progresses funeral by funeral.  Daniel Kahneman: Well, I mean to your question of whether there is life after 50, I certainly hope so. I think Vernon has done some of the work that, you know, has done wonderful work in very recent years so there may be life after 70 and, you know, to some extent this again is accidental and is self selection, that is if you are going to have people who are going to have important ideas, they may have them fairly early in their career and then they spend the rest of their careers elaborating on these ideas and so it looks as if, you know, people have their best ideas early on but that’s because they spend the rest of their lives working on them and I think, you know, this happens to most people who have one important theme to develop.  Vernon Smith: Well I think young people are sort of maybe more likely to make technical or mythological breakthroughs but just looking at my own history I didn’t really appreciate the full ramifications at the time I was doing that work. I didn’t come close to it and I think that can come with maturity so that the contribution you can make after aged 50, and I hope after age 75, is perhaps a different type of contribution than one makes when you’re younger.  Daniel Kahneman: Yes, I would echo that. It takes a long time to understand what you’ve been saying. So, you know, you say it first and then, over the years, you understand what it was that you really meant because you don’t know that immediately. In my particular case I was fortunate because there were two of us and that process of understanding what we said worked faster because we understood each other but when you work alone, understanding what you’re saying is a long drawn process.  Vernon Smith: Well and I recognise, understood early that a component of what I was doing had to do with institutions and rule systems but I didn’t begin to understand it the way I now do and a lot of that understanding came from interacting with other people over the years, Charlie Plott is a prime example, and also Martin Shubik, he and I are exactly the same age. I think I’ve known Martin for over 40 years, I think about 45 years and we often exchanged ideas, having to do with institutions and Martin was very interested in and that interchange was very valuable to me even though we never worked together and of course I worked with Charlie Plott and a lot of our insights came working together. |
|  | Oh, you know, there are lots of pieces of advice. I tell my graduate students that if they don’t fall asleep thinking about work, you know, then they’re not working hard enough actually, you know, so that’s the first piece of advice that you would give them and the second one is to try not to get trapped in uninteresting problems just because you began them. So one of the important things to have in science is to avoid the sunk cost fallacy, just to keep going with something just because you began it and have made an investment. So the ability to just make a quick turn when you’ve had an idea that looks better and drop everything else and follow the best idea that you have at the moment, that is certainly one of the thing that I think worked for me and I think it may work for other people as well.  Professor Smith?  Vernon Smith: Yes, I would echo that. Don’t follow the path of least resistance. Be prepared to break the informal rules. You have to of course live in your environment and so there’s a limit to how far you can go in breaking the rules. I was interested in experimental economics long before I could really make a living at it so I did other things and I didn’t get tenure doing experimental economics, I got it doing other things but I returned to experimental economics and one of the problems can be that in getting tenure you develop all these bad habits and then you can’t get out of doing the bad habits, which is doing what’s easy, you know, following your sunk cost and doing trivial kind of extensions of that.  Daniel Kahneman: And trying to salvage failures, that is when something is not working there are people who spend a lot of time trying to get something out of an experiment just because they did it. I’m sure that’s good advice to avoid that.  Vernon Smith: I think young people in economics are well advised to read widely outside of economics too and I think fairly narrowly within economics is good enough because the theories tend to be very similar anyway. Once you sort of get that basic model it’s better to … I read Science and I read Nature, those 2 magazines, they’re weeklies. I can’t follow everything in them but I do find things in there that intrigue me.  Daniel Kahneman: This is unusual, you know, in economics. 18 years ago when we first came to Berkeley, [George Akerlof](https://www.nobelprize.org/prizes/economic-sciences/2001/akerlof/facts/) invited me to co-teach a course with him and we taught a course on economics and psychology and there are two things to be said about that course. One is, he didn’t get credit for teaching it because it was considered a frivolous thing to do and the other one was that he kept advising and warning the students not to be seduced by it because he thought it could ruin their careers if they followed that path. So he would tell them, you had better stick to what he called meat and potatoes economics and, you know, you can afford to do those strange things after you get tenure.  Vernon Smith: Too late, he’d formed all his bad habits. |
| Q1 | Well how important has it been to you? I mean your scientific result can be applied to, what did Nobel say, to the benefit of mankind? I mean you followed your track, you found something that was interesting and pursued that? |
|  | Daniel Kahneman: I would say that the conscious sense of doing something that could be truly beneficial, well I had that early on when I thought that people could be educated to think, you know, more closely and these efforts of mine have not been rewarded. In recent years I’m consciously trying to do something for the benefit of mankind and this is to develop new and better measures of human welfare and human wellbeing that could be applied as another measure of how society is doing and I’m doing that with collaborators, including an economist at Princeton, Alan Krueger, and that is truly with the idea of trying to do something that could be useful to policy making.  You have something to say Vernon?  … the laboratory is a very useful tool for allowing people to get experience …  Vernon Smith: Well, I’ve become more interested, particularly in the last 20 years roughly as against the first 20 years, in utilising what we’ve learned about markets to do a better job of helping to design markets in new areas where people don’t have any experience, any field experience and I think the laboratory is a very useful tool for allowing people to get experience. It doesn’t provide the final experience and the final answers but the point is it is experiential and it gives people an opportunity to try out and test bed new rule systems, practitioners for example, and we found business and government, in some situations, very receptive to that. In governments, particularly in New Zealand and Australia, with respect to the liberalisation of electric power and I think we’ve seen in the California fiasco how bad things can be if you don’t think about some fundamental issues and furthermore, in that case, a lot those issues had long been studied in the laboratory and in fact influenced New Zealand and Australia but we didn’t have an opportunity to have that much influence in California. It isn’t that we didn’t communicate with any of the people that might have made a difference, it’s just that we couldn’t convince them or influence them enough. And also it turned out to be far worse than even we would have imagined as to the consequences of really not getting some elementary features of these markets right.  Daniel Kahneman: They need some help. Many years ago when we were studying failures of rationality in judgement and decision making I thought, you know, that there was a contribution to be made, for example to government decision making or to making political decisions but 25-30 years ago that was a period when the discipline of decision analysis looked extremely helpful and hopeful and it seemed as if the combination of psychology and decision analysis could be very, very useful. It has not been. I mean by and large I think this has been a failure and the reasons are quite interesting. The reasons are that the leaders do not want the help, that is the people who make decisions, important decisions, by and large do not want the kind of help that decision analysis or decision aids have to offer that we would think, you know, would certainly improve the quality of decision making. So there is a great deal of resistance and that, by itself, is quite interesting, that it’s been 30 or 40 years, you know, since decision analysis was first proposed and, by and large, very little has happened. |
| ID | 0847 |
| Biographical | 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 welcome.  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](https://www.nobelprize.org/nobel_prizes/economics/laureates/1987/index.html) 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](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2001/) 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](https://www.nobelprize.org/nobel_prizes/economics/laureates/1975/index.html) 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](https://www.nobelprize.org/nobel_prizes/economics/laureates/1981/index.html), 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. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0847 |
| Interview |  |
| Q1 | Yes, and the first question would be how did you decide to study economics? And maybe we could start with George? |
|  | George A. Akerlof: How decide to study economics? Well I think I always wanted to be an economist, if there was such a thing. I think I didn’t know that there were really economists until I went to college. And I knew that there was such a thing. I was always interested in economics, for a very long time. And I remember at the age of 10, I asked the following question: If one person loses their job and then they stop spending, and I was a little boy, so I decided, one father, remember this was the old days, so if one father lost his job, wouldn’t that cause that family to stop spending their money and that would cause another family, another father to lose his job and that would cause that family to stop spending.  And so I was worried that the economy would have a very bleak reaction to this. And it wasn’t until I went and took freshman economics that I learned the answer to that, which is that the family would only stop spending, let’s say, three quarter of it’s money, it would save a quarter, and so the multiplier wouldn’t be that great. So I think I had a reasonable number of such questions long before I knew that economics existed.  What about you Dr Spence?  A. Michael Spence: I was not thinking about infinite multipliers when I was 10. But I did have a father who was a PhD in commerce and finance and an intellectual man. And so I had a feeling, probably about the time I went to college, that I would try to be a scholar and teacher, but I didn’t know which field. And I picked economics at the end of my undergraduate time because it seemed to be a really nice combination of theory, including mathematical theory on one hand, and things that are quite practical that you can touch and see and feel. So I picked it and I consciously thought of it as an experiment to see if I liked it. And it worked.  Finally, Dr Stiglitz?  I love mathematics, but I decided I really wanted to work on problems of society …  Joseph E. Stiglitz: I had always been interested in economics and social problems when I had been young, maybe a little older than George, in high school. But when I went to Amherst College I studied physics and math. And then towards the end of my third year, my junior year, I decided that I was more interested in working on social problems, problems with society and using the mathematics I’d learned and combining that with my interest in history and society, to work on economic problems. And so that was really the decision. I love mathematics, but I decided I really wanted to work on problems of society.  George A. Akerlof: So I think in addition, I felt that the one thing that you could do to make people better off and able to lead self-fulfilling lives was if people have more money then they’ll have fewer constraints on their lives, and so they can make more of themselves and lead happier lives. So that was actually another reason for studying economics. |
| Q39 | What about the asymmetric information? I always wondered why things happened in the ‘60’s and early ‘70’s, why didn’t it happen earlier or why did it happen at this time? |
|  | George A. Akerlof: I can speak for myself, I think as far as I’m concerned it was an outgrowth of the work on quality in growth theory. That in growth theory, especially Joe’s and my thesis advisor, [Robert Solow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1987/solow-facts.html), he worried and made models of different qualities of capital. So vintage capital and capital in which one could choose the capital labour before the capital was built, but not afterwards. And so that made it possible, I think, because he had figured out how to model different qualities that made it possible to make models with asymmetric information, in which the key variable was the quality of the goods. Prior to that time I think we didn’t have enough mathematical ability to deal with what happened, not only when price varied, but when quality varied, especially in some continuous way.  Joseph E. Stiglitz: I think it was in a slightly different origin in my case. I was persuaded that models that we were being taught didn’t make a lot of sense for describing lots of the problems, and that there were a number of key problems, lack of perfect competition, lack of perfect information. Work had been done on the consequences of lack of perfect competition, theories of imperfect competition like Joan Robinson and Chamberlain. And the next natural question was what to do about imperfections of information? I think the key thing was that there were some very specific questions that at least I began to address, posed when I went to Kenya about how much should they be investing in education, that lead to what is the role of education? Was education just human capital, which had been the older view? Or whether other issues of education, like credentials or providing information. I think that was critical in the development.  There had been people working on generic equilibrium models with imperfect information. In other words, bringing in information into very abstract models. But they did it in such an abstract way that the questions weren’t posed in ways that lead to interesting answers. Beginning on the other side, what was a very specific issue and you started thinking about just a simple thing, assuming that two abilities, one low ability and a high ability, how do you sort them out? How do these people who are more able convince others that they’re more able? And by taking the simplest possible problem of information and thinking about how you solve that and then building up from that, rather than the abstract and try and deduce it. I think that was the critical breakthrough and I think it’s what all three of us had in common in our work, beginning with a very concrete problem and then generalising it.  A. Michael Spence: That’s very accurate. I think in addition there may have been some very interesting work in game theory, or at the application of game theory. Once again, as Joe said, not highly mathematical, most general game theory. But a game theory that was used to deal with deterrents problems and what not. And so every time you turn around there was a question about information, where it resided, how it was communicated, who knew what and when? And very bright people like [Tom Schelling](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2005/schelling-facts.html), for example, had started to write about it. When I started doing this, I’ll just add this one thing, there wasn’t anything there.  There was some writing by [Bill Vickrey](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1996/vickrey-facts.html), and he was the one who I think firstly pointed out that information is a very unusual commodity and that when Joe gives it to me, when he possesses it we both have it. And I got quite intrigued. But I was sent up a whole lot of blind alleys. There are people who sent me off to study signal processing theory, because that was what they called information theory. I learned absolutely nothing useful except what the capacity of a fibre optic cable is. So there was all that going on at the same time.  Joseph E. Stiglitz: One of the key things that came out of that earlier work is the importance of how to make inferences. It wasn’t the mathematics of how you make inferences, but the conceptual process of what are the signals, what are the things from which we make inferences about what somebody else is or what the world is like? And I think that, not at the mathematical level, but at the conceptual level of this process of making inferences is really very important I think and is a common element in all three of our work.  … the reaction from the editors, and possibly also from the referees, was this was not economics and therefore should not be accepted …  George A. Akerlof: I think one interesting aspect of this is when this work was initially done it wasn’t considered to be economics. So I submitted the market for lemons to three separate journals before it was finally accepted at a fourth. And the reaction from the editors, and possibly also from the referees, was this was not economics and therefore should not be accepted. And I think that’s because in fact its methodology was different, that this was a different way of looking at price theory.  It took some time before people saw that you could do price theory this way. And I think what was so different was that instead of arguing from the top down, from taking some general principles about how markets work and pricing systems work, instead we argued from the bottom up. So we took a look at examples of such things as insurance markets and education markets, and credit markets, and market for used cars. And then argued from the way we thought those specific examples work to how markets should look.  Joseph E. Stiglitz: We looked for general principles.  George A. Akerlof: From the particular.  Joseph E. Stiglitz: And it turned out that these general principles applied very broadly, but looking at ways in which they applied differently in different markets also gave you a lot of insight into the general principles.  A. Michael Spence: And the editors that rejected George’s paper have since been fired. |
| Q25 | What are you doing now? What kind of research are you doing now? |
|  | Joseph E. Stiglitz: One strand of research is a continuation of the problems of economics of information. It has gone into areas such as macro-economics, organisation theory, the insights of information economics has lead to theories of corporate finance, how the firms finance themselves, that has lead it to theories of firm behaviour, the theory of the risk averse firm. That has lead in turn to macro-economic theories of how the aggregate behaviour of the economy behaves. And that in turn has lead to, as one example, monetary theories, money rather than just being, monetary theory used to focus on transactions, the role of money. But in fact most transactions today use credit. And what is credit? It’s ascertaining who is credit worthy, which is an information issue. So it’s really reformulating monetary economics on the basis of theories of asymmetric information.  There’s another line of research that I’ve been very heavily engaged in which grows out of my work at the World Bank, and that is issues of development, issues of strategies for economies and transition from communism to a market economy. Both of those have a usage of the ideas that have come out, but broaden other ideas as well. Just to give you one example, a key aspect of the doctrines in development economics was a set of ideas called the Washington consensus, which was based on the belief in market fundamentalism. That markets by themselves lead to efficient outcomes. And that’s based on a belief of markets with perfect information, that set of ideas doesn’t work very well in developed economies, but in less developed economies it’s absolutely abysmal theory. And trying to think through how markets in developing countries are affected by the lack of information, as an example, and how that affects development strategies, is one of the key issues with which I’ve been concerned.  … I think the internet actually has moved the parameters, informational structure parameters and the number of markets …  A. Michael Spence: I had this somewhat unusual career and I stopped in mid 1987, became an academic administrator so that I, in the language of venture capital, this is kind of a restart. But I think the thing that I’m going to focus on when I know that I have the time again to do research is I think the internet actually has moved the parameters, informational structure parameters and the number of markets, in ways that, and I’m not sure of this, may require us to really look at the models. So I don’t think that means throwing the whole lot that we have out, but I think it probably does mean looking again. For example, and just to take one that was mentioned briefly this afternoon, if most people post prices and they’re accessible on the internet, the search cost that [George Stigler](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1982/stigler-facts.html) did in some interesting early work, have simply disappeared.  So the naive conclusion I guess is that, you know, there isn’t any other place to hide based on that kind of search cost. But probably the correct answer is that some smart young economists, or maybe even some older ones like us, ought to take a look at the question of whether as a matter of strategy people are going to post prices anymore. And if not, what is actually going to happen in the market? That kind of thing.  Joseph E. Stiglitz: Or we’ll come back to the kinds of issues the quality, you can’t post quality.  A. Michael Spence: No they can’t post quality.  Joseph E. Stiglitz: And that is at the heart of a lot of what our work was concerned with. And that will never be well described on the internet, or perfectly described on the internet.  George A. Akerlof: I think beyond working on asymmetric information, what I’ve sort of been doing and making a career of is thinking about what assumptions or not in economics, which would make a difference. So I think the next thing I did is I worked on what happens when prices are not made at the same time, not set at the same time, and wages are not set at the same time. That’s called staggered wages and staggered prices. Then I worked on what happened when there was some band in which people were indifferent in holding their money, so you didn’t decide to do anything about your money until you had more than some threshold level or less than some threshold, so that’s called target threshold. Then I’ve worked on bringing in anthropology and sociology into economics. Which is again the same thing, it’s sort of seeing what assumptions could be in economics, that should be there but aren’t.  So I’ve worried about what happens when there’s reciprocity, especially in the employment relationship. What’s happening when fairness is an issue? And I’m currently working on the importance, especially to the labour markets and to education, of peoples’ self-concept. I think that probably the most important decision that anyone makes in their life is who they think they are and who they want to be. And economists tend to think of price as being the most important decision that they make. I think working on the asymmetric information said that another aspect of markets was quality.  But then I think there’s this third aspect that hasn’t yet been incorporated, which is who we think we are and who we want to be. And I think that this is the root cause of poverty in developed countries. That if people think that they can do something, they think they can be anything, then in fact they will and so there will be much more social mobility than they have. And I think this is the leading cause of poverty in the United States, that there are people who just don’t see the potential and just don’t have the right self-concept.  So that’s your recent work on identity?  George A. Akerlof: That’s my recent work on identity, yes. |
| Q67 | To what extent have you been involved in implementing your research results? |
|  | Joseph E. Stiglitz: I spent basically from 1993 through 2000, first as President Clinton’s chief economic adviser, then as chief economist of the World Bank. And in those jobs I had to deal with a wide range of issues, not just the ones related to my own work. But an anecdote may illustrate, when I first came into the White House, I went to a meeting in which a big issue on the agenda was health insurance, reforming the health care system in the United States. And I remember going to a meeting and just sitting in the back of the room while they were discussing, and they were talking about moral hazard and average selection as the key ideas, the key problems that had to be resolved in reforming the health insurance market. And it gave me a lot of pleasure to see how quickly some of the ideas that we had developed had gone from theoretical research into being taught at graduate schools and by this point, were just common tools that everybody, you wouldn’t begin that discussion of insurance reform without the concept of moral hazard and average selection.  So in a way they’ve become a tool kit, part of the vocabulary that everybody uses. In the East Asia crisis in the World Bank and IMF, the issue of whether the bail outs were going to cause moral hazard, ie leave the lenders to take less precaution in making good loans was a standard part of the debate. And I thought that they mismanaged that in a number of ways, but partly because the IMF had not really fully integrated some of these ideas, that for instance, with imperfect information you begin to think about bankruptcy. And you start thinking about the impact of monetary policy on the likelihood the firms are going to go into bankruptcy. You worry … the financial system is destroying information and the flow of credit. And so the intellectual frame that I brought to the issue, some of it had been incorporated, some of it had not fully been incorporated, and some of that represented some of the reasons that there were a lot of disputes about the appropriate ways to respond to the crises.  … you have to begin with the legal structure, don’t privatise too rapidly before you do that …  One more example, the work on asymmetry of information brought home the importance of corporate governance. That managers have much more knowledge and discretion about what to do with the resources under their control in the firm. And that they can use that discretion for their own benefit and not for the benefit of the shareholders. In the United States and in Sweden and in most advance industrial countries, we’re aware of that problem and have passed legal structures to prevent the abuse. In Russia they didn’t have that legal structure. Without that legal structure, privatisation lead more to asset stripping than wealth creation. The strategy for moving from communism to a market economy that some of us were very sensitive about these issues, said you have to begin with the legal structure, don’t privatise too rapidly before you do that. And that was again something that was ignored by the people who believed in shock therapy who didn’t understand the subtleties of a market economy. And I think that has contributed to a large extent to the failures that have occurred there over the last decade.  A. Michael Spence: The only piece that I would add is there were two ways that you could go other than jumping into policy, you know, once these theoretical ideas emerged. One was to go and see how people try to use them or might try to use them when they are actually doing things in the world, like business people making strategy decisions.  And you are a coach aren’t you?  A. Michael Spence: I guess a little bit, but mostly I was the dean of a business school. So you’ve got to watch how this got translated. And the other point I guess I would make is that there’s an empirical component to this, you know, there are theoretical structures and there are the observations that all three of us made just looking at how markets behaved that caused us to try to create the models and theories. But there’s a serious empirical side to this as well where you actually go take the theories and then go try to verify. I started out down that route and ended up taking the practical side. The thing that I was never interested in doing was, and I think Joe and I are different in this respect, I was always interested in the sort of economic science of it and was happy to have others really do the policy part. This is entirely personal, it wasn’t my driving motivation.  George A. Akerlof: I think I’ve done some policy work. I’ve worked at Brookings Institution for some time. And I think that probably in the last decade or so, especially jointly with Janet Yellen, I’ve developed a style in which institutions are very important. And one should pay a great deal of attention to the detail of how institutions look and then specifically analyse special historical cases describing the institutions in detail. So Janet Yellen and Andy Rose and Helga Hessenius and I wrote a paper on East Germany during the transition. We looked at details of the institution. We did a similar paper explaining the reason for out of wedlock births, in which we looked in great detail at the history of legislation regarding abortion and also the availability of contraception.  … when you understand the details the markets are actually much more interesting than you would otherwise think …  And so I’ve done a reasonable number of policy pieces in which we are very careful about getting the institutions right. And I think that’s in the same spirit as to the original asymmetric information. We thought that the details mattered as to how the market was going to work itself out. So issues that in used cars, in insurance and so forth. So it’s a matter of being a little bit more careful than the economics was prior to this work. You have to really understand the details and when you understand the details the markets are actually much more interesting than you would otherwise think.  Joseph E. Stiglitz: I just want to add, one of the aspects of traditional economics, this is called neoclassical economics, was that you didn’t need to look at the details. The theory was that demand and supply determined everything. It was really conceived as institution free economics. So it was not only that they didn’t look at the details the theory said, they didn’t have to. And in a sense we began for the premise that that was wrong. And as you began you saw that they did matter and in a very concrete way.  George A. Akerlof: I think that’s actually why originally when I submitted market for lemons for publication, it was said this wasn’t economics. They said if this is economics, we don’t do it. That is what one of the referee’s reports said. If we accept this for publication, what are economists going to do?  George A. Akerlof: I just want to make a plug, because there’s a wonderful book out which is codified all of this, and that of course is Joe’s wonderful text book, his elementary book on economics. And I was in China last summer and it had sold over 1 million copies in China. And it’s a great book; I actually refer to it whenever I want to look at some detailed question. |
| ID | 0848 |
| Biographical | Getting started I was born during the second World War in Montclair New Jersey. This was more or less an accident (the location that is). My father was based in Ottawa as a member of the War Time Prices and Trades Board, the Canadian version of wartime price controls. That work entailed frequent trips to Washington to coordinate with their American counterparts. New Jersey is more or less half way between the two capitals and my mother was visiting friends. So although I grew up in Canada during and after the war until leaving for college in the United States, I managed to also be an American by birth.  My father once said about being a parent that it is the only thing you do that requires a very long period of learning and at about the time that you are becoming competent, you don’t need the skills anymore. Notwithstanding this modest assessment of their parenting skills, they were wonderful parents. My father was the son of the registrar of the University of Manitoba. He was an intellectual by instinct (he had a PhD from Northwestern University in Commerce and Finance) and in another time, might very well have chosen a career in academic life. I learned from him to love precision in thought, the power of abstraction and the use of symbols to capture structures and relationships. He was also a very good athlete and we spent a lot of time playing basketball, football, hockey, just about anything.  My mother was the only child of my grandparents, who lived in Minnesota at the time of her birth and later moved to Winnipeg. My grandfather was an engineer with the Canadian Pacific Railway. [For the younger generation, an engineer in those days was the person who drove the train.] My mother was strong-willed, demanding and very supportive all at the same time. I think I inherited from her a kind of tenacity (sometimes referred to as stubbornness) that served me well. It partially filled gaps when sheer intellectual horsepower (of which others had much more than I) proved insufficient.  The overall effect of my parents upbringing was to provide a great sense of security, being surrounded by love and affection, a great (perhaps too great) sense of self-confidence (there really weren’t any challenges that were deemed beyond reach for any reason), and an equally great sense of intellectual adventure, a world populated by opportunities and challenges rather than obstacles and roadblocks. I don’t have the professional expertise to know whether and how important these general frames of reference that one’s parents impart, are. An amateur’s guess based on my own experience is that they are important and I believe the sense of freedom and the confidence to try to use it were among our parents’ greatest gifts to us.  I have included a time line that places events in my life and in my family’s life generally in the right place and the right order. I hope that this has the advantage from the reader’s point of view, that it is fairly easy to skim without using up much time, and also easy just to skip.  Education Next to my family, it seems clear to me that the educational institutions and the teachers from whom I had the privilege of learning, were especially important. They were (i) excellent and (ii) a liberating force. In looking back, what is surprising is how uniformly true this was. My middle and high school in Canada, UTS, attached to the College of Education at the University of Toronto, was for my American friends, not dissimilar to the Lab School at the University of Chicago. It was then and it still is excellent in two respects. The teaching achieved a very high average quality and topped out in the superlative range, and the students were without question in the same league. This combination that I have now seen (and even presided over in academic administration) several times strikes me as particularly potent.  I would also add that all of the schools and universities I attended seemed to me to be excellent at the time and in measurable ways they are even better now. This is really very encouraging. The combination of a workable basic formula and the capacity to improve over time is what one hopes for in any aspect of society: business, government, the non-profit sector. Thus the pattern of excellence was repeated at Princeton, Oxford and Harvard. There are probably many reasons for the high standards and the continuous improvement. One is the healthy synergy between teaching and research – the excitement of the research is transmitted to the learning process, and the energy and curiosity of the students produces new ideas in research. A second is competition. All of these institutions have very successful competitors who, with the help of vigilant alumnae and alumni, keep them constantly on their toes.  Teachers, colleagues and students Education is in the end about individual interactions and about learning. As someone who aspired to become a teacher and a scholar, it is hard for me now to imagine a better group of teachers and mentors and colleagues than those with whom I worked and learned.  The research side of academic life is often viewed from the outside as a solo and at times lonely activity. In fact it is quite the opposite, a communal activity in significant part where interaction and interchange generate ideas and critiques of them. The research for which we were recognized this year was part of an exciting time in which many helped build the applied microeconomic foundations of several applied fields. I personally owe a great debt to my colleagues and former students who were part of that effort. While it is not possible to acknowledge all those debts, there are many whose help was both generous and invaluable. Jim Rosse and Bruce Owen at Stanford in the 1970’s helped me make the link between theory and industrial organization. Many of the problems that I had the opportunity to work on were the result of teaching and working side by side with them.  My thesis advisors are very different and very gifted. [Kenneth Arrow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1972/index.html) taught me (and many others) mathematical economics and general equilibrium theory. When I took his course in general equilibrium theory, the take-home exam that I produced was lost. After a frightening phone call and a successful search for a copy, I rushed it in and waited while Ken read it, which he did (all twelve pages) at a rate of about 2 seconds a page. Now I have to say that there was a lot of mathematics on those pages. I just assumed that he wasn’t really reading it, until he finished and then started asking me some detailed questions about the assumptions and analysis on page five. Describing Ken Arrow’s contributions to economics in the second half of the 20th century would come quite close to just describing the evolution of economics during that period.  Tom Schelling, as all who studied with him knew, had an extraordinarily original mind. Unique in our experience was his capacity to analyze using carefully constructed analogies, with just the right number of similarities and differences. Many of my younger colleagues were motivated in the best possible way by normative and policy questions. I think I tended more to being just fascinated by how markets and mechanisms like them worked. A great deal of that interest and motivation came from hours spent with Tom Schelling. It could be tipping points, focal points, sorting out congestion on a beach with surfers and swimmers, deterrence. Schelling’s curiosity seemed endless and his capacity to shed light remarkable.  Richard Zeckhauser saved me from exiting from academia prematurely, after perhaps too many years in classes and libraries. He started me teaching and tolerated with grace the first few embarrassing outings. I wrote my first paper with him on insurance markets, moral hazard and adverse selection. Richard’s lightning quick mind was and is matched by the tremendous breadth of his interests and his intuitive grasp of complex phenomena. More than once I had the experience of having him tell me the answer to some complex market problem, my not completely understanding the answer, going away for a couple of weeks to try to capture it in a model, and deciding after that effort that he was right in the first place.  It is not uncommon for graduate students to be encouraged to pick something relatively safe or at least manageable as a thesis topic. Honestly, it is not bad advice. But I have to say that I never received advice like that, or if I did, I didn’t hear it. In addition to their time and insight, I owe to my advisors their encouragement and support for grappling with the informational structure of markets, however risky that might have been.  I should like to acknowledge and thank Gilbert Harmon, Richard Ludwig and Robert Kuenne at Princeton University. They are largely responsible for my interest in philosophy and economics and for the interest I developed in trying scholarship and teaching as a career.  At UTS, Don Fawcett taught several generations of us about hard work, competing, winning and losing gracefully, and about the importance of trying to do something that makes a difference.  As one can tell from a glance at the autobiographical timeline, I have had two careers (first as teacher and scholar and then in academic administration) and with luck I am embarking on a third, though the last will in part be a return to the first. My colleagues and friends, Edward Lazear and Mark Wolfson have shared the excitement and the challenges in all three, and enriched the whole experience in ways that are hard to describe accurately.  As an administrator, I had the privilege of working with two associate deans, Phyllis Keller at Harvard and Paul Johnson at Stanford. These two served with and trained several deans, and had a lot to do with the sustained excellence in their respective faculties and schools.  Derek Bok, the President of Harvard for over 20 years, gave me the chance to be a dean and then used his legendary skills as a leader and educator to help me become competent at my job. Whether he succeeded in this is for others to judge, but with his patience and wisdom, he taught me much that I value greatly.  Our family It has always struck me as ironic and wonderful that the most important parts of life, one’s family, the unconditional love for one’s spouse and children and the joy of seeing children learn and grow to wonderful human beings, are exactly those things that one shares with all the rest of humanity. Monica and I have had a wonderful ten years together. Our children (Monica’s step children) Graham, Catherine and Marya have thrived with the continued support of their mother, Ann Bennett Spence. We all hope that as they enter adulthood, that the world that they will live and work in has as much freedom and opportunity as the one we enjoyed.  The Nobel Prize It is a wonderful and unexpected honor to receive the Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel. Receiving this prize with Joseph Stiglitz and George Akerlof, whose work I have learned from and admired makes it even more gratifying. It was also very nice to see that Michael Rothschild’s important contributions were recognized during Nobel week in December. And perhaps most importantly, it is a source of great satisfaction that the work on the informational structure of markets seems to have taken hold in microeconomics and that it has had some influence in other disciplines. The three of us, and many others, contributed to the development of this sub-field and all share in the recognition that goes with this extraordinary prize.   |  |  |  | | --- | --- | --- | | Michael Spence brief autobiography | | | | Year |  |  | | 1943 | Born Montclair New Jersey |  | | 1944 | Brother Randy born in Ottawa |  | | 1945 |  |  | | 1946 | Family moved to Winnipeg |  | | 1947 |  |  | | 1948 |  |  | | 1949 |  |  | | 1950 | Moved to farm outside of Toronto – attended school in two room school house |  | | 1951 |  |  | | 1952 | Moved to Toronto – attended John in Ross Robertson elementary school | Flunked first test in grammar grade 4 | | 1953 | Brother Allan born |  | | 1954 |  | Built houses on our island in Georgian Bay (north of Toronto) in summers with Father and brothers | | 1955 | Attended University of Toronto Schools (part of U of Toronto) in grade 7 |  | | 1956 |  | Played Pee Wee hockey for Ted’s Pal’s – 96 games – school suffered – chose school over hockey – never looked back | | 1957 |  |  | | 1958 |  |  | | 1960 |  |  | | 1961 |  | Went to Europe for the first time – later took young people to Europe in the summers | | 1962 | School Captain in last year in High School – Graduated from High School (UTS): went to Princeton University as a freshman – liberal arts not prevalent in Canada at the time | Met Cook family – roommate Steve, father Peter (portrait painter and freshman hockey coach) and mother Joan – became family friends | | 1963 |  | Randy comes to Princeton – family dog David (a female springer spaniel – don’t ask) dies | | 1964 Played ice hockey for Princeton for 4 years | | | | 1965 | Brother Allan goes to UTS |  | | 1966 | Graduated from Princeton | Majored in philosophy – met Ann Bennett in last year – received Canadian Rhodes Scholarship – went to Magdalen College Oxford – decided to try an academic career with encouragement from parents | | 1967 | Majored in Mathematics at Oxford | Randy graduated from Princeton – went on to a PhD at University of Toronto focussing on Economic Development | | 1968 | Graduated from Oxford – entered PhD program in Economica at Harvard in the fall – supported by Danforth Fellowship | Married Ann Bennett – recovered from mononucleosis – spend summer in Georgian Bay | | 1969 |  | The Vietnam War dominated the environment for all of the time I spent in the PhD | | 1970 | Rapporteur in Faculty Seminar in Kennedy School – included my thesis advisors, Thomas Schelling, Richard Zeckhauser and Kenneth Arrow | Wonderful teachers and mentors – owe a particular debt also to Martin Feldstein for his mentorship at that time | | 1971 | Began teaching analytic methods at the Kennedy School of Government at Havard – it was one year old at the time | Summer spent at Berkeley in mathematical economics seminar – realized Andreu MasCollel knew more math than I | | 1972 | Received PhD – received David A Wells prize for doctoral thesis | Wrote thesis call “Market Signaling” – seemed quite well received | | 1973 | Moved to Stanford in the Economics Dept as Associate Professor – taught and did research in applied microeconomic theory and Industrial Organization | Ann enrolled in MBA at Stanford Business School – exciting group of young people in Economics – wonderful support from Bruce Owen and Jim Rosse getting into Industrial Organization – visited Brother Randy in East Africa in the summer | | 1974 |  |  | | 1975 | Returned to Harvard Economics as Professor – began to teach graduate economic theory and undergraduate industrial organization using Michael Porter strategy cases – | Met Mike Porter | | 1976 |  | Had two undergraduates in graduate theory course – Bill Gates and Steve Ballmer – both got A’s | | 1977 |  |  | | 1978 |  |  | | 1979 | Became a joint appointment in Economics and the Harvard Business School at Harvard – received Galbraith prize for teaching at Harvard | Son Graham born in the spring | | 1980 |  |  | | 1981 |  |  | | 1982 | Received the John Bate’s Clark medal from American Economic Association (given every two years to an economist under age 40) | Daughter Catherine born in January | | 1983 | Became chairman of the Department of Economics at Harvard |  | | 1984 | President Derek Bok asked me to succeed Henry Rosovsky as Dean of the Faculty of Arts and Sciences – accepted – learned a lot from President Bok | Phyllis Keller as associate dean taught me how to be a dean – wonderful colleague and friend | | 1985 |  | Daughter Marya born in March – I am learning how to be a dean in my first year | | 1986 | Joined board of directors of Polaroid Corporation – Harvard celebrated its 350th anniversary – | My father died in the summer after a difficult struggle with dementia – I missed him a lot | | 1987 |  |  | | 1988 |  |  | | 1989 |  |  | | 1990 | Stepped down as dean of FAS – Became Dean of the Graduate School of Business at Stanford University – succeeded Bob Jaedicke – | Family moved to California – joined by Mark Wolfson in dean’s office – became very good friends | | 1991 |  | Separated from Ann and later divorced in 1995 | | 1992 | Stanford gets into altercation with Government over Overhead cost recovery | Paul Johnson as associate dean for many years in the business school – wonderful colleague and later good friend – Took up windsurfing with good friend Ed Lazear who was nice enough to watch out for me | | 1993 |  | Met Monica Cappuccini – later married in 1997 | | 1994 |  | Graham takes up golf – achieves handicap of 4 after 18 months | | 1995 |  | Met Tom Siebel and joined board of Siebel Systems – his new company | | 1996 |  |  | | 1997 |  | Monica and I are married in the summer – Graham graduates from Menlo School and heads for Princeton as a freshman – Monica and I take trip to Himalaya’s in the fall | | 1998 |  |  | | 1999 | Stepped down as dean of the business school at Stanford after 9 years – rejoined Mark Wolfson and his colleagues at Oak Hill Capital Partners – | Catherine graduated from Menlo School and headed for Columbia University as a freshman – visited Singapore as Lee Kwan Yew distinguished visitor | | 2000 |  | Our mother made it to the new millennium and passed away peacefully on January 5. – developed and taught a new course in electronic commerce with colleague Garth Saloner – acquired a small condo on Maui in Hawaii | | 2001 | Received word of receipt of Nobel Prize in October – family together for the award in December – a wonderful shared moment | Graham graduated from Princeton and I had my 35th reunion in June – almost a definition of aging. – brief bout with melanoma – successfully caught early thanks to a very alert dermatologist – Recia Blumenkranz | |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0848 |
| Interview |  |
| Q1 | Yes, and the first question would be how did you decide to study economics? And maybe we could start with George? |
|  | George A. Akerlof: How decide to study economics? Well I think I always wanted to be an economist, if there was such a thing. I think I didn’t know that there were really economists until I went to college. And I knew that there was such a thing. I was always interested in economics, for a very long time. And I remember at the age of 10, I asked the following question: If one person loses their job and then they stop spending, and I was a little boy, so I decided, one father, remember this was the old days, so if one father lost his job, wouldn’t that cause that family to stop spending their money and that would cause another family, another father to lose his job and that would cause that family to stop spending.  And so I was worried that the economy would have a very bleak reaction to this. And it wasn’t until I went and took freshman economics that I learned the answer to that, which is that the family would only stop spending, let’s say, three quarter of it’s money, it would save a quarter, and so the multiplier wouldn’t be that great. So I think I had a reasonable number of such questions long before I knew that economics existed.  What about you Dr Spence?  A. Michael Spence: I was not thinking about infinite multipliers when I was 10. But I did have a father who was a PhD in commerce and finance and an intellectual man. And so I had a feeling, probably about the time I went to college, that I would try to be a scholar and teacher, but I didn’t know which field. And I picked economics at the end of my undergraduate time because it seemed to be a really nice combination of theory, including mathematical theory on one hand, and things that are quite practical that you can touch and see and feel. So I picked it and I consciously thought of it as an experiment to see if I liked it. And it worked.  Finally, Dr Stiglitz?  I love mathematics, but I decided I really wanted to work on problems of society …  Joseph E. Stiglitz: I had always been interested in economics and social problems when I had been young, maybe a little older than George, in high school. But when I went to Amherst College I studied physics and math. And then towards the end of my third year, my junior year, I decided that I was more interested in working on social problems, problems with society and using the mathematics I’d learned and combining that with my interest in history and society, to work on economic problems. And so that was really the decision. I love mathematics, but I decided I really wanted to work on problems of society.  George A. Akerlof: So I think in addition, I felt that the one thing that you could do to make people better off and able to lead self-fulfilling lives was if people have more money then they’ll have fewer constraints on their lives, and so they can make more of themselves and lead happier lives. So that was actually another reason for studying economics. |
| Q39 | What about the asymmetric information? I always wondered why things happened in the ‘60’s and early ‘70’s, why didn’t it happen earlier or why did it happen at this time? |
|  | George A. Akerlof: I can speak for myself, I think as far as I’m concerned it was an outgrowth of the work on quality in growth theory. That in growth theory, especially Joe’s and my thesis advisor, [Robert Solow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1987/solow-facts.html), he worried and made models of different qualities of capital. So vintage capital and capital in which one could choose the capital labour before the capital was built, but not afterwards. And so that made it possible, I think, because he had figured out how to model different qualities that made it possible to make models with asymmetric information, in which the key variable was the quality of the goods. Prior to that time I think we didn’t have enough mathematical ability to deal with what happened, not only when price varied, but when quality varied, especially in some continuous way.  Joseph E. Stiglitz: I think it was in a slightly different origin in my case. I was persuaded that models that we were being taught didn’t make a lot of sense for describing lots of the problems, and that there were a number of key problems, lack of perfect competition, lack of perfect information. Work had been done on the consequences of lack of perfect competition, theories of imperfect competition like Joan Robinson and Chamberlain. And the next natural question was what to do about imperfections of information? I think the key thing was that there were some very specific questions that at least I began to address, posed when I went to Kenya about how much should they be investing in education, that lead to what is the role of education? Was education just human capital, which had been the older view? Or whether other issues of education, like credentials or providing information. I think that was critical in the development.  There had been people working on generic equilibrium models with imperfect information. In other words, bringing in information into very abstract models. But they did it in such an abstract way that the questions weren’t posed in ways that lead to interesting answers. Beginning on the other side, what was a very specific issue and you started thinking about just a simple thing, assuming that two abilities, one low ability and a high ability, how do you sort them out? How do these people who are more able convince others that they’re more able? And by taking the simplest possible problem of information and thinking about how you solve that and then building up from that, rather than the abstract and try and deduce it. I think that was the critical breakthrough and I think it’s what all three of us had in common in our work, beginning with a very concrete problem and then generalising it.  A. Michael Spence: That’s very accurate. I think in addition there may have been some very interesting work in game theory, or at the application of game theory. Once again, as Joe said, not highly mathematical, most general game theory. But a game theory that was used to deal with deterrents problems and what not. And so every time you turn around there was a question about information, where it resided, how it was communicated, who knew what and when? And very bright people like [Tom Schelling](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2005/schelling-facts.html), for example, had started to write about it. When I started doing this, I’ll just add this one thing, there wasn’t anything there.  There was some writing by [Bill Vickrey](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1996/vickrey-facts.html), and he was the one who I think firstly pointed out that information is a very unusual commodity and that when Joe gives it to me, when he possesses it we both have it. And I got quite intrigued. But I was sent up a whole lot of blind alleys. There are people who sent me off to study signal processing theory, because that was what they called information theory. I learned absolutely nothing useful except what the capacity of a fibre optic cable is. So there was all that going on at the same time.  Joseph E. Stiglitz: One of the key things that came out of that earlier work is the importance of how to make inferences. It wasn’t the mathematics of how you make inferences, but the conceptual process of what are the signals, what are the things from which we make inferences about what somebody else is or what the world is like? And I think that, not at the mathematical level, but at the conceptual level of this process of making inferences is really very important I think and is a common element in all three of our work.  … the reaction from the editors, and possibly also from the referees, was this was not economics and therefore should not be accepted …  George A. Akerlof: I think one interesting aspect of this is when this work was initially done it wasn’t considered to be economics. So I submitted the market for lemons to three separate journals before it was finally accepted at a fourth. And the reaction from the editors, and possibly also from the referees, was this was not economics and therefore should not be accepted. And I think that’s because in fact its methodology was different, that this was a different way of looking at price theory.  It took some time before people saw that you could do price theory this way. And I think what was so different was that instead of arguing from the top down, from taking some general principles about how markets work and pricing systems work, instead we argued from the bottom up. So we took a look at examples of such things as insurance markets and education markets, and credit markets, and market for used cars. And then argued from the way we thought those specific examples work to how markets should look.  Joseph E. Stiglitz: We looked for general principles.  George A. Akerlof: From the particular.  Joseph E. Stiglitz: And it turned out that these general principles applied very broadly, but looking at ways in which they applied differently in different markets also gave you a lot of insight into the general principles.  A. Michael Spence: And the editors that rejected George’s paper have since been fired. |
| Q25 | What are you doing now? What kind of research are you doing now? |
|  | Joseph E. Stiglitz: One strand of research is a continuation of the problems of economics of information. It has gone into areas such as macro-economics, organisation theory, the insights of information economics has lead to theories of corporate finance, how the firms finance themselves, that has lead it to theories of firm behaviour, the theory of the risk averse firm. That has lead in turn to macro-economic theories of how the aggregate behaviour of the economy behaves. And that in turn has lead to, as one example, monetary theories, money rather than just being, monetary theory used to focus on transactions, the role of money. But in fact most transactions today use credit. And what is credit? It’s ascertaining who is credit worthy, which is an information issue. So it’s really reformulating monetary economics on the basis of theories of asymmetric information.  There’s another line of research that I’ve been very heavily engaged in which grows out of my work at the World Bank, and that is issues of development, issues of strategies for economies and transition from communism to a market economy. Both of those have a usage of the ideas that have come out, but broaden other ideas as well. Just to give you one example, a key aspect of the doctrines in development economics was a set of ideas called the Washington consensus, which was based on the belief in market fundamentalism. That markets by themselves lead to efficient outcomes. And that’s based on a belief of markets with perfect information, that set of ideas doesn’t work very well in developed economies, but in less developed economies it’s absolutely abysmal theory. And trying to think through how markets in developing countries are affected by the lack of information, as an example, and how that affects development strategies, is one of the key issues with which I’ve been concerned.  … I think the internet actually has moved the parameters, informational structure parameters and the number of markets …  A. Michael Spence: I had this somewhat unusual career and I stopped in mid 1987, became an academic administrator so that I, in the language of venture capital, this is kind of a restart. But I think the thing that I’m going to focus on when I know that I have the time again to do research is I think the internet actually has moved the parameters, informational structure parameters and the number of markets, in ways that, and I’m not sure of this, may require us to really look at the models. So I don’t think that means throwing the whole lot that we have out, but I think it probably does mean looking again. For example, and just to take one that was mentioned briefly this afternoon, if most people post prices and they’re accessible on the internet, the search cost that [George Stigler](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1982/stigler-facts.html) did in some interesting early work, have simply disappeared.  So the naive conclusion I guess is that, you know, there isn’t any other place to hide based on that kind of search cost. But probably the correct answer is that some smart young economists, or maybe even some older ones like us, ought to take a look at the question of whether as a matter of strategy people are going to post prices anymore. And if not, what is actually going to happen in the market? That kind of thing.  Joseph E. Stiglitz: Or we’ll come back to the kinds of issues the quality, you can’t post quality.  A. Michael Spence: No they can’t post quality.  Joseph E. Stiglitz: And that is at the heart of a lot of what our work was concerned with. And that will never be well described on the internet, or perfectly described on the internet.  George A. Akerlof: I think beyond working on asymmetric information, what I’ve sort of been doing and making a career of is thinking about what assumptions or not in economics, which would make a difference. So I think the next thing I did is I worked on what happens when prices are not made at the same time, not set at the same time, and wages are not set at the same time. That’s called staggered wages and staggered prices. Then I worked on what happened when there was some band in which people were indifferent in holding their money, so you didn’t decide to do anything about your money until you had more than some threshold level or less than some threshold, so that’s called target threshold. Then I’ve worked on bringing in anthropology and sociology into economics. Which is again the same thing, it’s sort of seeing what assumptions could be in economics, that should be there but aren’t.  So I’ve worried about what happens when there’s reciprocity, especially in the employment relationship. What’s happening when fairness is an issue? And I’m currently working on the importance, especially to the labour markets and to education, of peoples’ self-concept. I think that probably the most important decision that anyone makes in their life is who they think they are and who they want to be. And economists tend to think of price as being the most important decision that they make. I think working on the asymmetric information said that another aspect of markets was quality.  But then I think there’s this third aspect that hasn’t yet been incorporated, which is who we think we are and who we want to be. And I think that this is the root cause of poverty in developed countries. That if people think that they can do something, they think they can be anything, then in fact they will and so there will be much more social mobility than they have. And I think this is the leading cause of poverty in the United States, that there are people who just don’t see the potential and just don’t have the right self-concept.  So that’s your recent work on identity?  George A. Akerlof: That’s my recent work on identity, yes. |
| Q67 | To what extent have you been involved in implementing your research results? |
|  | Joseph E. Stiglitz: I spent basically from 1993 through 2000, first as President Clinton’s chief economic adviser, then as chief economist of the World Bank. And in those jobs I had to deal with a wide range of issues, not just the ones related to my own work. But an anecdote may illustrate, when I first came into the White House, I went to a meeting in which a big issue on the agenda was health insurance, reforming the health care system in the United States. And I remember going to a meeting and just sitting in the back of the room while they were discussing, and they were talking about moral hazard and average selection as the key ideas, the key problems that had to be resolved in reforming the health insurance market. And it gave me a lot of pleasure to see how quickly some of the ideas that we had developed had gone from theoretical research into being taught at graduate schools and by this point, were just common tools that everybody, you wouldn’t begin that discussion of insurance reform without the concept of moral hazard and average selection.  So in a way they’ve become a tool kit, part of the vocabulary that everybody uses. In the East Asia crisis in the World Bank and IMF, the issue of whether the bail outs were going to cause moral hazard, ie leave the lenders to take less precaution in making good loans was a standard part of the debate. And I thought that they mismanaged that in a number of ways, but partly because the IMF had not really fully integrated some of these ideas, that for instance, with imperfect information you begin to think about bankruptcy. And you start thinking about the impact of monetary policy on the likelihood the firms are going to go into bankruptcy. You worry … the financial system is destroying information and the flow of credit. And so the intellectual frame that I brought to the issue, some of it had been incorporated, some of it had not fully been incorporated, and some of that represented some of the reasons that there were a lot of disputes about the appropriate ways to respond to the crises.  … you have to begin with the legal structure, don’t privatise too rapidly before you do that …  One more example, the work on asymmetry of information brought home the importance of corporate governance. That managers have much more knowledge and discretion about what to do with the resources under their control in the firm. And that they can use that discretion for their own benefit and not for the benefit of the shareholders. In the United States and in Sweden and in most advance industrial countries, we’re aware of that problem and have passed legal structures to prevent the abuse. In Russia they didn’t have that legal structure. Without that legal structure, privatisation lead more to asset stripping than wealth creation. The strategy for moving from communism to a market economy that some of us were very sensitive about these issues, said you have to begin with the legal structure, don’t privatise too rapidly before you do that. And that was again something that was ignored by the people who believed in shock therapy who didn’t understand the subtleties of a market economy. And I think that has contributed to a large extent to the failures that have occurred there over the last decade.  A. Michael Spence: The only piece that I would add is there were two ways that you could go other than jumping into policy, you know, once these theoretical ideas emerged. One was to go and see how people try to use them or might try to use them when they are actually doing things in the world, like business people making strategy decisions.  And you are a coach aren’t you?  A. Michael Spence: I guess a little bit, but mostly I was the dean of a business school. So you’ve got to watch how this got translated. And the other point I guess I would make is that there’s an empirical component to this, you know, there are theoretical structures and there are the observations that all three of us made just looking at how markets behaved that caused us to try to create the models and theories. But there’s a serious empirical side to this as well where you actually go take the theories and then go try to verify. I started out down that route and ended up taking the practical side. The thing that I was never interested in doing was, and I think Joe and I are different in this respect, I was always interested in the sort of economic science of it and was happy to have others really do the policy part. This is entirely personal, it wasn’t my driving motivation.  George A. Akerlof: I think I’ve done some policy work. I’ve worked at Brookings Institution for some time. And I think that probably in the last decade or so, especially jointly with Janet Yellen, I’ve developed a style in which institutions are very important. And one should pay a great deal of attention to the detail of how institutions look and then specifically analyse special historical cases describing the institutions in detail. So Janet Yellen and Andy Rose and Helga Hessenius and I wrote a paper on East Germany during the transition. We looked at details of the institution. We did a similar paper explaining the reason for out of wedlock births, in which we looked in great detail at the history of legislation regarding abortion and also the availability of contraception.  … when you understand the details the markets are actually much more interesting than you would otherwise think …  And so I’ve done a reasonable number of policy pieces in which we are very careful about getting the institutions right. And I think that’s in the same spirit as to the original asymmetric information. We thought that the details mattered as to how the market was going to work itself out. So issues that in used cars, in insurance and so forth. So it’s a matter of being a little bit more careful than the economics was prior to this work. You have to really understand the details and when you understand the details the markets are actually much more interesting than you would otherwise think.  Joseph E. Stiglitz: I just want to add, one of the aspects of traditional economics, this is called neoclassical economics, was that you didn’t need to look at the details. The theory was that demand and supply determined everything. It was really conceived as institution free economics. So it was not only that they didn’t look at the details the theory said, they didn’t have to. And in a sense we began for the premise that that was wrong. And as you began you saw that they did matter and in a very concrete way.  George A. Akerlof: I think that’s actually why originally when I submitted market for lemons for publication, it was said this wasn’t economics. They said if this is economics, we don’t do it. That is what one of the referee’s reports said. If we accept this for publication, what are economists going to do?  George A. Akerlof: I just want to make a plug, because there’s a wonderful book out which is codified all of this, and that of course is Joe’s wonderful text book, his elementary book on economics. And I was in China last summer and it had sold over 1 million copies in China. And it’s a great book; I actually refer to it whenever I want to look at some detailed question. |
| ID | 0849 |
| Biographical | 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 peregrinations 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](https://www.nobelprize.org/nobel_prizes/economics/laureates/1970/index.html), 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 midseventies, 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 MIT, 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 MIT 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 MIT, 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 MIT 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](https://www.nobelprize.org/nobel_prizes/peace/laureates/1964/index.html) 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 hey-day of MIT with first-rate professors (I had at least four Nobel Prize winners as professor: [Samuelson](https://www.nobelprize.org/nobel_prizes/economics/laureates/1970/index.html) (Nobel Laureate in 1970), [Solow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1987/index.html) (Nobel Laureate in 1987), [Modigliani](https://www.nobelprize.org/nobel_prizes/economics/laureates/1985/index.html) (Nobel Laureate in 1985), and [Arrow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1972/index.html) (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 Prize 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, which I did not always 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 Sheshinki 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 MIT 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/subdiscipline 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 reining 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 re-education. 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 eminces: [Jim Mirrlees](https://www.nobelprize.org/nobel_prizes/economics/laureates/1996/index.html) (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](https://www.nobelprize.org/nobel_prizes/economics/laureates/1977/index.html) (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](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not1) 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](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not2)  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 some of my research program, 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 workshops – was published as a series of articles over the next decade.[3](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not3) 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 most recent of which was not published until 1992).[4](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not4)  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 MIT, 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 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](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not5)  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](https://www.nobelprize.org/nobel_prizes/economics/laureates/1983/index.html) (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 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](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not6) 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](https://www.nobelprize.org/nobel_prizes/economics/laureates/1990/index.html) (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 Prize 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.  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 approached 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 MIT. 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 like 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 (as 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: in one’s own environment, 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 prejudiced. 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 the population 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 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) mode, or on 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 Prize 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 structures[7](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not7). 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.  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 Prize lecture.)  I referred earlier to my work in the economics of the public sector.[8](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not8) I was convinced that there was an important role for government to play. Given that, 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 is 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 Prize 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 Dixit[9](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not9), 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 is 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 Chamberlain 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 immense simplicity – in which the market made that trade-off perfectly; but more generally, it did not.[10](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not10)  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](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not11)  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](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not12) The efficiency wage theories (described in greater detail in my Prize 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 rationing[13](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not13) 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, 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 that 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 deflation[14](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not14).) 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.  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 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.  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](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not15)  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.  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 putting 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](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not16) 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 countries 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 alternatively 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 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 polices 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 democrat, 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 US 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 macro-economics, and monetary theory in particular[17](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/biographical/#not17) – 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, 2001c.) 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See Stiglitz (1974b, 1982a). See also Stiglitz (1974c, 1992).  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 of 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).  6. Sanford Grossman and I pursued these ideas further in Grossman and Stiglitz (1977, 1980).  7. See e.g. Sah and Stiglitz (1985a, 1986).  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 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 I 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 in- fluence 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).  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)  14. See Fisher (1933).  15. See Stiglitz (1997b, 1998a) for brief descriptions of some of my views concerning these experiences.  16. See Stiglitz (2001a).  17. See Greenwald and Stiglitz (1999).  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 2001*, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 2002  This autobiography/biography was written at the time of the award and later published in the book series [*Les Prix Nobel/*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)[*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*/*[*The Nobel Prizes*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/nobel-prizes.html). The information is sometimes updated with an addendum submitted by the Laureate.  (Revised by J.E. Stiglitz in December, 2002) |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0849 |
| Interview |  |
| Q1 | Yes, and the first question would be how did you decide to study economics? And maybe we could start with George? |
|  | George A. Akerlof: How decide to study economics? Well I think I always wanted to be an economist, if there was such a thing. I think I didn’t know that there were really economists until I went to college. And I knew that there was such a thing. I was always interested in economics, for a very long time. And I remember at the age of 10, I asked the following question: If one person loses their job and then they stop spending, and I was a little boy, so I decided, one father, remember this was the old days, so if one father lost his job, wouldn’t that cause that family to stop spending their money and that would cause another family, another father to lose his job and that would cause that family to stop spending.  And so I was worried that the economy would have a very bleak reaction to this. And it wasn’t until I went and took freshman economics that I learned the answer to that, which is that the family would only stop spending, let’s say, three quarter of it’s money, it would save a quarter, and so the multiplier wouldn’t be that great. So I think I had a reasonable number of such questions long before I knew that economics existed.  What about you Dr Spence?  A. Michael Spence: I was not thinking about infinite multipliers when I was 10. But I did have a father who was a PhD in commerce and finance and an intellectual man. And so I had a feeling, probably about the time I went to college, that I would try to be a scholar and teacher, but I didn’t know which field. And I picked economics at the end of my undergraduate time because it seemed to be a really nice combination of theory, including mathematical theory on one hand, and things that are quite practical that you can touch and see and feel. So I picked it and I consciously thought of it as an experiment to see if I liked it. And it worked.  Finally, Dr Stiglitz?  I love mathematics, but I decided I really wanted to work on problems of society …  Joseph E. Stiglitz: I had always been interested in economics and social problems when I had been young, maybe a little older than George, in high school. But when I went to Amherst College I studied physics and math. And then towards the end of my third year, my junior year, I decided that I was more interested in working on social problems, problems with society and using the mathematics I’d learned and combining that with my interest in history and society, to work on economic problems. And so that was really the decision. I love mathematics, but I decided I really wanted to work on problems of society.  George A. Akerlof: So I think in addition, I felt that the one thing that you could do to make people better off and able to lead self-fulfilling lives was if people have more money then they’ll have fewer constraints on their lives, and so they can make more of themselves and lead happier lives. So that was actually another reason for studying economics. |
| Q39 | What about the asymmetric information? I always wondered why things happened in the ‘60’s and early ‘70’s, why didn’t it happen earlier or why did it happen at this time? |
|  | George A. Akerlof: I can speak for myself, I think as far as I’m concerned it was an outgrowth of the work on quality in growth theory. That in growth theory, especially Joe’s and my thesis advisor, [Robert Solow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1987/solow-facts.html), he worried and made models of different qualities of capital. So vintage capital and capital in which one could choose the capital labour before the capital was built, but not afterwards. And so that made it possible, I think, because he had figured out how to model different qualities that made it possible to make models with asymmetric information, in which the key variable was the quality of the goods. Prior to that time I think we didn’t have enough mathematical ability to deal with what happened, not only when price varied, but when quality varied, especially in some continuous way.  Joseph E. Stiglitz: I think it was in a slightly different origin in my case. I was persuaded that models that we were being taught didn’t make a lot of sense for describing lots of the problems, and that there were a number of key problems, lack of perfect competition, lack of perfect information. Work had been done on the consequences of lack of perfect competition, theories of imperfect competition like Joan Robinson and Chamberlain. And the next natural question was what to do about imperfections of information? I think the key thing was that there were some very specific questions that at least I began to address, posed when I went to Kenya about how much should they be investing in education, that lead to what is the role of education? Was education just human capital, which had been the older view? Or whether other issues of education, like credentials or providing information. I think that was critical in the development.  There had been people working on generic equilibrium models with imperfect information. In other words, bringing in information into very abstract models. But they did it in such an abstract way that the questions weren’t posed in ways that lead to interesting answers. Beginning on the other side, what was a very specific issue and you started thinking about just a simple thing, assuming that two abilities, one low ability and a high ability, how do you sort them out? How do these people who are more able convince others that they’re more able? And by taking the simplest possible problem of information and thinking about how you solve that and then building up from that, rather than the abstract and try and deduce it. I think that was the critical breakthrough and I think it’s what all three of us had in common in our work, beginning with a very concrete problem and then generalising it.  A. Michael Spence: That’s very accurate. I think in addition there may have been some very interesting work in game theory, or at the application of game theory. Once again, as Joe said, not highly mathematical, most general game theory. But a game theory that was used to deal with deterrents problems and what not. And so every time you turn around there was a question about information, where it resided, how it was communicated, who knew what and when? And very bright people like [Tom Schelling](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2005/schelling-facts.html), for example, had started to write about it. When I started doing this, I’ll just add this one thing, there wasn’t anything there.  There was some writing by [Bill Vickrey](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1996/vickrey-facts.html), and he was the one who I think firstly pointed out that information is a very unusual commodity and that when Joe gives it to me, when he possesses it we both have it. And I got quite intrigued. But I was sent up a whole lot of blind alleys. There are people who sent me off to study signal processing theory, because that was what they called information theory. I learned absolutely nothing useful except what the capacity of a fibre optic cable is. So there was all that going on at the same time.  Joseph E. Stiglitz: One of the key things that came out of that earlier work is the importance of how to make inferences. It wasn’t the mathematics of how you make inferences, but the conceptual process of what are the signals, what are the things from which we make inferences about what somebody else is or what the world is like? And I think that, not at the mathematical level, but at the conceptual level of this process of making inferences is really very important I think and is a common element in all three of our work.  … the reaction from the editors, and possibly also from the referees, was this was not economics and therefore should not be accepted …  George A. Akerlof: I think one interesting aspect of this is when this work was initially done it wasn’t considered to be economics. So I submitted the market for lemons to three separate journals before it was finally accepted at a fourth. And the reaction from the editors, and possibly also from the referees, was this was not economics and therefore should not be accepted. And I think that’s because in fact its methodology was different, that this was a different way of looking at price theory.  It took some time before people saw that you could do price theory this way. And I think what was so different was that instead of arguing from the top down, from taking some general principles about how markets work and pricing systems work, instead we argued from the bottom up. So we took a look at examples of such things as insurance markets and education markets, and credit markets, and market for used cars. And then argued from the way we thought those specific examples work to how markets should look.  Joseph E. Stiglitz: We looked for general principles.  George A. Akerlof: From the particular.  Joseph E. Stiglitz: And it turned out that these general principles applied very broadly, but looking at ways in which they applied differently in different markets also gave you a lot of insight into the general principles.  A. Michael Spence: And the editors that rejected George’s paper have since been fired. |
| Q25 | What are you doing now? What kind of research are you doing now? |
|  | Joseph E. Stiglitz: One strand of research is a continuation of the problems of economics of information. It has gone into areas such as macro-economics, organisation theory, the insights of information economics has lead to theories of corporate finance, how the firms finance themselves, that has lead it to theories of firm behaviour, the theory of the risk averse firm. That has lead in turn to macro-economic theories of how the aggregate behaviour of the economy behaves. And that in turn has lead to, as one example, monetary theories, money rather than just being, monetary theory used to focus on transactions, the role of money. But in fact most transactions today use credit. And what is credit? It’s ascertaining who is credit worthy, which is an information issue. So it’s really reformulating monetary economics on the basis of theories of asymmetric information.  There’s another line of research that I’ve been very heavily engaged in which grows out of my work at the World Bank, and that is issues of development, issues of strategies for economies and transition from communism to a market economy. Both of those have a usage of the ideas that have come out, but broaden other ideas as well. Just to give you one example, a key aspect of the doctrines in development economics was a set of ideas called the Washington consensus, which was based on the belief in market fundamentalism. That markets by themselves lead to efficient outcomes. And that’s based on a belief of markets with perfect information, that set of ideas doesn’t work very well in developed economies, but in less developed economies it’s absolutely abysmal theory. And trying to think through how markets in developing countries are affected by the lack of information, as an example, and how that affects development strategies, is one of the key issues with which I’ve been concerned.  … I think the internet actually has moved the parameters, informational structure parameters and the number of markets …  A. Michael Spence: I had this somewhat unusual career and I stopped in mid 1987, became an academic administrator so that I, in the language of venture capital, this is kind of a restart. But I think the thing that I’m going to focus on when I know that I have the time again to do research is I think the internet actually has moved the parameters, informational structure parameters and the number of markets, in ways that, and I’m not sure of this, may require us to really look at the models. So I don’t think that means throwing the whole lot that we have out, but I think it probably does mean looking again. For example, and just to take one that was mentioned briefly this afternoon, if most people post prices and they’re accessible on the internet, the search cost that [George Stigler](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1982/stigler-facts.html) did in some interesting early work, have simply disappeared.  So the naive conclusion I guess is that, you know, there isn’t any other place to hide based on that kind of search cost. But probably the correct answer is that some smart young economists, or maybe even some older ones like us, ought to take a look at the question of whether as a matter of strategy people are going to post prices anymore. And if not, what is actually going to happen in the market? That kind of thing.  Joseph E. Stiglitz: Or we’ll come back to the kinds of issues the quality, you can’t post quality.  A. Michael Spence: No they can’t post quality.  Joseph E. Stiglitz: And that is at the heart of a lot of what our work was concerned with. And that will never be well described on the internet, or perfectly described on the internet.  George A. Akerlof: I think beyond working on asymmetric information, what I’ve sort of been doing and making a career of is thinking about what assumptions or not in economics, which would make a difference. So I think the next thing I did is I worked on what happens when prices are not made at the same time, not set at the same time, and wages are not set at the same time. That’s called staggered wages and staggered prices. Then I worked on what happened when there was some band in which people were indifferent in holding their money, so you didn’t decide to do anything about your money until you had more than some threshold level or less than some threshold, so that’s called target threshold. Then I’ve worked on bringing in anthropology and sociology into economics. Which is again the same thing, it’s sort of seeing what assumptions could be in economics, that should be there but aren’t.  So I’ve worried about what happens when there’s reciprocity, especially in the employment relationship. What’s happening when fairness is an issue? And I’m currently working on the importance, especially to the labour markets and to education, of peoples’ self-concept. I think that probably the most important decision that anyone makes in their life is who they think they are and who they want to be. And economists tend to think of price as being the most important decision that they make. I think working on the asymmetric information said that another aspect of markets was quality.  But then I think there’s this third aspect that hasn’t yet been incorporated, which is who we think we are and who we want to be. And I think that this is the root cause of poverty in developed countries. That if people think that they can do something, they think they can be anything, then in fact they will and so there will be much more social mobility than they have. And I think this is the leading cause of poverty in the United States, that there are people who just don’t see the potential and just don’t have the right self-concept.  So that’s your recent work on identity?  George A. Akerlof: That’s my recent work on identity, yes. |
| Q67 | To what extent have you been involved in implementing your research results? |
|  | Joseph E. Stiglitz: I spent basically from 1993 through 2000, first as President Clinton’s chief economic adviser, then as chief economist of the World Bank. And in those jobs I had to deal with a wide range of issues, not just the ones related to my own work. But an anecdote may illustrate, when I first came into the White House, I went to a meeting in which a big issue on the agenda was health insurance, reforming the health care system in the United States. And I remember going to a meeting and just sitting in the back of the room while they were discussing, and they were talking about moral hazard and average selection as the key ideas, the key problems that had to be resolved in reforming the health insurance market. And it gave me a lot of pleasure to see how quickly some of the ideas that we had developed had gone from theoretical research into being taught at graduate schools and by this point, were just common tools that everybody, you wouldn’t begin that discussion of insurance reform without the concept of moral hazard and average selection.  So in a way they’ve become a tool kit, part of the vocabulary that everybody uses. In the East Asia crisis in the World Bank and IMF, the issue of whether the bail outs were going to cause moral hazard, ie leave the lenders to take less precaution in making good loans was a standard part of the debate. And I thought that they mismanaged that in a number of ways, but partly because the IMF had not really fully integrated some of these ideas, that for instance, with imperfect information you begin to think about bankruptcy. And you start thinking about the impact of monetary policy on the likelihood the firms are going to go into bankruptcy. You worry … the financial system is destroying information and the flow of credit. And so the intellectual frame that I brought to the issue, some of it had been incorporated, some of it had not fully been incorporated, and some of that represented some of the reasons that there were a lot of disputes about the appropriate ways to respond to the crises.  … you have to begin with the legal structure, don’t privatise too rapidly before you do that …  One more example, the work on asymmetry of information brought home the importance of corporate governance. That managers have much more knowledge and discretion about what to do with the resources under their control in the firm. And that they can use that discretion for their own benefit and not for the benefit of the shareholders. In the United States and in Sweden and in most advance industrial countries, we’re aware of that problem and have passed legal structures to prevent the abuse. In Russia they didn’t have that legal structure. Without that legal structure, privatisation lead more to asset stripping than wealth creation. The strategy for moving from communism to a market economy that some of us were very sensitive about these issues, said you have to begin with the legal structure, don’t privatise too rapidly before you do that. And that was again something that was ignored by the people who believed in shock therapy who didn’t understand the subtleties of a market economy. And I think that has contributed to a large extent to the failures that have occurred there over the last decade.  A. Michael Spence: The only piece that I would add is there were two ways that you could go other than jumping into policy, you know, once these theoretical ideas emerged. One was to go and see how people try to use them or might try to use them when they are actually doing things in the world, like business people making strategy decisions.  And you are a coach aren’t you?  A. Michael Spence: I guess a little bit, but mostly I was the dean of a business school. So you’ve got to watch how this got translated. And the other point I guess I would make is that there’s an empirical component to this, you know, there are theoretical structures and there are the observations that all three of us made just looking at how markets behaved that caused us to try to create the models and theories. But there’s a serious empirical side to this as well where you actually go take the theories and then go try to verify. I started out down that route and ended up taking the practical side. The thing that I was never interested in doing was, and I think Joe and I are different in this respect, I was always interested in the sort of economic science of it and was happy to have others really do the policy part. This is entirely personal, it wasn’t my driving motivation.  George A. Akerlof: I think I’ve done some policy work. I’ve worked at Brookings Institution for some time. And I think that probably in the last decade or so, especially jointly with Janet Yellen, I’ve developed a style in which institutions are very important. And one should pay a great deal of attention to the detail of how institutions look and then specifically analyse special historical cases describing the institutions in detail. So Janet Yellen and Andy Rose and Helga Hessenius and I wrote a paper on East Germany during the transition. We looked at details of the institution. We did a similar paper explaining the reason for out of wedlock births, in which we looked in great detail at the history of legislation regarding abortion and also the availability of contraception.  … when you understand the details the markets are actually much more interesting than you would otherwise think …  And so I’ve done a reasonable number of policy pieces in which we are very careful about getting the institutions right. And I think that’s in the same spirit as to the original asymmetric information. We thought that the details mattered as to how the market was going to work itself out. So issues that in used cars, in insurance and so forth. So it’s a matter of being a little bit more careful than the economics was prior to this work. You have to really understand the details and when you understand the details the markets are actually much more interesting than you would otherwise think.  Joseph E. Stiglitz: I just want to add, one of the aspects of traditional economics, this is called neoclassical economics, was that you didn’t need to look at the details. The theory was that demand and supply determined everything. It was really conceived as institution free economics. So it was not only that they didn’t look at the details the theory said, they didn’t have to. And in a sense we began for the premise that that was wrong. And as you began you saw that they did matter and in a very concrete way.  George A. Akerlof: I think that’s actually why originally when I submitted market for lemons for publication, it was said this wasn’t economics. They said if this is economics, we don’t do it. That is what one of the referee’s reports said. If we accept this for publication, what are economists going to do?  George A. Akerlof: I just want to make a plug, because there’s a wonderful book out which is codified all of this, and that of course is Joe’s wonderful text book, his elementary book on economics. And I was in China last summer and it had sold over 1 million copies in China. And it’s a great book; I actually refer to it whenever I want to look at some detailed question. |
| ID | 0850 |
| Biographical | I was born in the Chicago, IL neighbourhood of Hyde Park on April, 1944, to my parent Bernice Irene Medley Heckman and John Jacob Heckman. I have one sister, Jean Ellen Heckman Bates, who is four years older. Although I was born near the University of Chicago, my family was not connected with it. Our family lived in the Chicago area until 1956, when we moved to the border South (Kentucky, 1956-1957 and Oklahoma, 1957-1958). My brief time in the South and a later trip to the Deep South in the early 1960s with my Nigerian college roommate left lasting impressions on me as I encountered the system of racial discrimination known as “Jim Crow” in its final manifestation. The separate water foundations, park benches, bathrooms and restaurants of the Jim Crow South startled me. These experiences motivated my lifelong study of the status of African Americans, and the sources of improvement in that status.  My high school years were spent in Lakewood, Colorado, a suburb of Denver. A decisive influence on my intellectual development was my exposure to Frank Oppenheimer, brother of J. Robert Oppenheimer, Scientific Director of the Manhattan Project that developed the atomic bomb in World War II. Frank Oppenheimer was a distinguished experimental physicist in his own right. Because of his membership in the Communist Party, he lost his position at the University of Minnesota in the early 1950s. He then became a cattle rancher in Colorado. In 1958, the Superintendent of the local school district in Lakewood asked him to teach physics to a class of students chosen by competitive exam. Oppenheimer closely linked theory to evidence when he taught physics. Under his guidance, I learned the beauty of experimental science and the pleasure of matching theory to evidence. Although I later abandoned physics for economics, my enthusiasm for scientific empirical work guided by theory was born in his classroom. Oppenheimer later went on to found the Exploratorium in San Francisco.  Through a series of fortuitous circumstances, I attended Colorado College in Colorado Springs, Colorado on a generous Boettcher Foundation Fellowship given to students from Colorado to attend colleges in Colorado. I received a good education there, and majored in mathematics. I also took a wide array of liberal arts courses, including a course on economic development taught by Ray Werner that read through the classics of economics. Reading Adam Smith, David Ricardo, [Arthur Lewis](https://www.nobelprize.org/prizes/economic-sciences/1979/lewis/facts/) and, in a supplement to that course, [Samuelson](https://www.nobelprize.org/prizes/economic-sciences/1970/samuelson/facts/)‘s Foundations of Economic Analysis, was an exciting experience that shaped my desire to learn more economics.  After college, I briefly attended the University of Chicago in economics. I found [Milton Friedman](https://www.nobelprize.org/prizes/economic-sciences/1976/friedman/facts/) fascinating and also enjoyed the lectures of Harry Johnson.  I transferred to Princeton in large part because of Arthur Lewis and his work on economic development. He did not disappoint. My interests in development waned, however, and I was increasingly drawn to the study of labor economics and econometrics. During my graduate student days, there was an ongoing large scale empirical project on labor supply (conducted by William Bowen and T. Aldrich Finegan and published in 1969), and projects on labor market models (conducted by Orley Ashenfelter, Stanley Black, Ray Fair and Harry Kelejian). These empirical projects were a major source of stimulation to my intellectual development. In addition I was fascinated by the pioneering work of Richard Quandt on estimating travel demand – a field that would later come to called “discrete choice theory” – and in particular the problem of using econometrics to estimate the demand for new goods. The econometrics group was young and interactive. The ethos at Princeton at that time encouraged the application of economic theory and econometric methods to solve policy problems.  There was ferment in the air both at Princeton and elsewhere as many new sources of microdata became available to study the labor market. There was great intellectual challenge in devising methods to use these data creatively. Modern labor economics as developed by [Gary Becker](https://www.nobelprize.org/prizes/economic-sciences/1992/becker/facts/) and Jacob Mincer provided an exciting new intellectual framework for interpreting the new microdata. I was particularly struck by the simplicity and elegance of Jacob Mincer’s seminal paper on estimating the labor supply of women. The novelty of that work and the open questions raised by it offered fascinating research opportunities. One of my close advisors, Al Rees, was actively engaged in setting up the first large scale social experiment. I followed this research with interest, enrolling some of the first participants into the New Jersey experiment. The scientific study of labor economics provided the opportunity for me to unite theory with evidence my lifetime intellectual passion.  After graduate school, I was fortunate to be offered a position at Columbia University, and learned much from my colleagues there. It was from Kelvin Lancaster that I learned about problems with the representative consumer model and methods for dealing with them. Like Quandt, Lancaster was interested in the problem of estimating the demand for a new good. From Ned Phelps, I learned about the importance of securing micro foundations for macroeconomics and how to write for an audience of professional economists.  The atmosphere at Columbia was open and encouraging. I was drawn to the labor workshop headed by Jacob Mincer, and learned from numerous student dissertations written at Columbia at the time. I had the good fortune of being invited to join the National Bureau of Economic Research (NBER) which was then located in New York. NBER had a first rate group of highly interactive empirical scholars. Victor Fuchs played an important role in shaping this group and holding it together. Jacob Mincer and Finis Welch were the guiding lights of a brilliant empirical environment. I learned much from all of my NBER colleagues and developed a close relationship with Bob Willis which greatly influenced my thinking about the importance of heterogeneity in economics. I also learned from frequent visitors to NBER such as Gary Becker, Reuben Gronau and Sherwin Rosen. NBER at that time was an intense intellectual environment in which data, theory and econometrics were all taken seriously. The research agenda for much of my subsequent research was shaped by stimulating interactions at the New York NBER.  In the Summer of 1974, I visited the working group of Daniel McFadden at Berkeley. His ability to unite theory and evidence to solve practical problems set a valuable example and affected my own approach to empirical work. I later generalized his work to dynamic settings.  I was recruited by the University of Chicago in 1973. I have been there ever since except for an occasional leave and a two year appointment at Yale, 1988-1990. Chicago is an exciting place which renews itself. The workshop system encourages close reading and frank discussions of papers and ideas. When I first arrived, Milton Friedman was the most prominent economist there and set the standard for open in-depth discussions on almost any topic. Others filled his shoes after he retired.  Throughout the years, I have benefited greatly from many colleagues and from many first rate students at Chicago. There is a very rigorous intellectual standard in the Chicago environment. Discussions are conducted at a high level on all aspects of economics. Gary Becker, William Brock, [Lars Hansen](https://www.nobelprize.org/prizes/economic-sciences/2013/hansen/facts/) and Jose Scheinkman have been especially stimulating and helpful. I also enjoyed the cross disciplinary stimulation of my 20 year interaction with the sociologist James Coleman. I have also benefited from many interactions and co-authorships with the versatile Burton Singer of Princeton.  At Chicago, I have had close relationships with many students with whom I have co-authored numerous papers and from I learned much. One of the greatest pleasures of academic life is watching young tentative students form into finished mature scholars with well developed ideas. Much of my work in the past 20 years is joint work that emerged from interactions with my students and colleagues in offices and classrooms. The Chicago environment of open rigorous discussion has greatly enriched my research.  In particular, long-term collaborations with Chicago students such as Bo Honoré, Tom MaCurdy and Richard Robb have been a source of personal pleasure and intellectual stimulation. More recent interactions with Lance Lochner, Jeffrey Smith, Christopher Taber, Petra Todd, and Edward Vytlacil have been very fruitful. I look forward to continued intellectual relationships with all of my former students, and future generations of students.  Since 1991, the American Bar Foundation has supported my work on the impact of law on the economy. My colleagues there have given me a new vista on law and social sciences. I am especially grateful to Bryant Garth, the director, for supporting me throughout the years. I am also grateful to the National Science Foundation and in particular, Daniel Newlon, for long term support for my research. I am equally grateful to the National Institutes of Health, and in particular, V. Jeffrey Evans, for continued long term support of my research.  On a personal note, I married Lynne Pettler in 1979. She has shared her knowledge of sociology, and raised our two able children: Jonathan, an aspiring mathematician born in 1982, and Alma, an aspiring actress born in 1986. My family life is a deep source of satisfaction. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0850 |
| Interview |  |
| Q1 | My name is Bertil Holmlund. I am interviewing the two Laureates in Economic Sciences this year: Professor Daniel McFadden from University of California at the Berkley and Professor James Heckman from University of Chicago. They have received the Prize for their contributions to micro econometrics, which is a field on the boundary between economics and statistics. This interview takes place at the Nobel Foundation in Stockholm on December 13, a couple of days after the Laureates have received the Prize. I think I should like to start by asking you how it all began. How come that you started to study economics seriously? Was it more or less by accident or was it something that you had wanted to do for a long time? |
|  | James Heckman: In my case it probably is an accident. I went to a liberal arts college and as part of my background I was majoring in mathematics and physics. But a part of the liberal arts college there’s an option to take readings courses. Working one and one or in small groups. And out of curiosity I chose a readings class in economics where the classics were taught. Allegedly the theme was economic development. but it was reading people like Marshall, Ricardo, Smith and some of the more modern people, [Arthur Lewis](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1979/lewis-facts.html). I had always had a deep interest in social science, history. So even when I was in high school I was debating, and in college debating, and interested in contemporary events. And I had some interest in mathematics. And I was amazed. And then probably the most single event, the important event, was in the same class. The instructor gave me a copy of [Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/samuelson-facts.html)‘s *Foundations* which I found to be an amazingly nice synthesis of things. It was an accident.  Also a social interest?  James Heckman: Yes. I found it interesting because I could then pursue several of my interests simultaneously. I was interested in quantitative analysis, but I also thought economists were asking interesting questions.  What about you?  Daniel McFadden: Well, I came from the natural sciences, from physics. I was a graduate student in physics and found psychology very interesting, and the psychology of behaviour. I had an opportunity to enter this disciplinary programme in the behavioural sciences. And I pursued that for my PhD and got into economics pretty much by accident, because the formal modelling, the axiomatic work that I was interested in was being done primarily by two economists, John Chipman and [Leo Hurwicz](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2007/hurwicz-facts.html). So I made economics my speciality in order to work with them.  Economists typically analyse occupational choice affected by income prospects. Did your choice of occupation to some extent reflect comparisons of incomes across occupations?  Daniel McFadden: For myself personally I would say no. I expected to be a poor academician all my life. And that was my choice. Non pecuniary returns.  What about you Jim?  It was a lifestyle choice more than a choice of money …  James Heckman: I was going to say non pecuniary returns are what drove it. It was a lifestyle choice more than a choice of money. I never expected or particularly sought after financial resources and it was an accident. I always felt I earned a huge rent. I shouldn’t say that on the television, but in the sense that I enjoy very much what I do. And the occupational choice is probably driven more by the psychic than … |
| Q8 | You have both a background in mathematics and physics and perhaps physics is often regarded as an ideal for research. Also economics. Some critics may say that perhaps at the same time economics become more and more like a branch of applied mathematics, how do you react to that criticism of economics? |
|  | Daniel McFadden: I think it’s natural in the development of economics to quantify it. That is to say to move from general theories about how economic systems respond to numerical predictions on what will happen if you change some economic variable. And so mathematics is a natural language for developing a quantitative version of economic theory. Mathematics for its own sake sometimes may have insufficient contact with the facts because in the end a science has to be an interplay between the logical development of the theory and the reality of the facts.  What about you Jim?  James Heckman: I think the example, the contrast between physics and mathematics is a good one, because in some sense physics is driven by mathematics. Mathematics is very useful there. But it always orients itself towards data. Some sort of empirical phenomena or maybe not string theory but in the traditional physics. And in some sense I think that is a major model. It certainly was a major model for me. And I think it’s not a bad model for economics generally to think of using mathematical models but to try to explain some aspect of reality, in this case social reality rather than a physical reality. |
| Q18 | What about econometrics and its role to help us to choose between different models or theories? Ideally statistical testing should be a way of discriminating between alternative theories to weed out the weak theories. Do you think econometrics has been reasonably successful in this regard? |
|  | Daniel McFadden: I certainly think there has been great progress in the last few decades in doing real windowing out of hypothesis about behaviour. I think you see it in the kinds of things that Jim has looked at. Job, the effectiveness of various alternatives for job training. You see it a great deal in labour market and public finance applications. And there I think econometrics has been very successful. If you ask is it successful in modifying the deeper theory of economics I think the answer is less so. That’s partly because that theory is more closely held by economists and they’re less willing to change. Perhaps also because the deepest theory of economics is often viewed as a kind of a parable for how people should behave rather than something that is predictive act by act. So when evidence appears that seems to violate one particular act people will say it’s not a failure of theory. It’s a failure of interpretation or a failure of approximation.  Jim?  James Heckman: I think econometrics has had a very large role, but I think probably one of the biggest developments, it’s been around since the beginning of econometrics. But one of the most important developments has been understanding when we can use data to sort out hypotheses. You know identification questions. And when we cannot. When essentially the matters given available data do not allow a decisive resolution of the issue. I think economists have made a lot of progress on this question in the last 20 or 30 years. So we understand that some matters can’t be settled with the available data and that stimulates the collection of new data where that might be settled.  … testing is like an event, an activity which is very useful …  I think econometrics plays a huge role in thinking through the issues very clearly. So testing is like an event, an activity which is very useful, but I think considering identification and considering what we can in principle separate from what we can’t is extremely useful and is productive in many areas. Observational equivalents is what the macro economists call it. And identification is what the micro economists call it. But it’s the same idea. Same pressures. |
| Q18 | Do you think we should have much more of replication of existing empirical studies to check how robust the results are? Replications of econometrics studies to a much more systematic extent than we have seen so far. |
|  | James Heckman: I’m thinking in particular of a book that you may know of edited by Mary Morgan and Magnus and others, where there was an attempt to replicate [Tobin](https://www.nobelprize.org/nobel_prizes/economics/laureates/1981/tobin-facts.html)‘s study of the demand for food. You know this study? No. Ed Lamar and others participated in this. And it was a little disturbing. Certainly the act of replication was very important but it seemed there were many other judgements that were brought in. And it led people, who were participating in this study of replication, to become aware of something. They started talking about the art of econometrics or the practise. The term, I’m forgetting right now, but there was a term about all the implicit assumptions, tacit econometrics I think was the term. So I think replication is extremely important precisely because in the past to any number there are a lot of other, quote enabling auxiliary assumptions.  Do you want to comment on this?  Daniel McFadden: I certainly think that economics will progress as replication becomes more important. And I would criticise the way our journals currently operate. They tend to always look for things which involve some new technique or some completely new data or some completely new idea. There’s probably insufficient value placed on good work which verifies and checks things that have been done before. |
| Q72 | On the same topic I think of [Wassily Leontief](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1973/leontief-facts.html). He’s actually a former Nobel Laureate in economics and he has drawn the attention to the dominance of theory in economics journals. It seems as if the fraction of pure theory papers without any data is much higher in economics than in say physics or chemistry. Do you think that this is a problem? That it seems as if theory is given much more emphasis in economics than perhaps it is in other sciences. |
|  | James Heckman: I would make the remark that his remark was written in 1972. And it may have been more of a problem in 1972 or 74. I think it was his presidential address for the American Economic Association. It was certainly much earlier. I think there’s been a huge growth in the last 20 years of applied economics. And empirical economics. And if anything one might criticise the other way. That there’s been a huge amount of empiricism without any theory which I think may in fact be equally harmful and possibly more harmful for reasons we talked about earlier. So I’d be less worried now than I think I would have been, or less sympathetic with that comment, than I might have been 20 years ago.  Daniel McFadden: I’ve actually encouraged that traditionally empirical econometric work is hard to present within the bounds of a journal article simply because so much background documentation is required to fully explain what you do empirically. And I think with the development of electronic journals and the possibility of referencing things in a way which is available to readers, to users. Empirical presentation of results should get better and should begin to have more sway I think because you can present the results and provide at the same time the adequate background for those who want to fill in the gaps. |
| Q72 | Empirical research requires data and good quality data and to compile data takes time. Do you think that the collection in improving data is valued as much as it should in the profession? Jim. |
|  | James Heckman: In some quarters yes. If you could point for example study done by Truman Bewley a few years ago. Truman Bewley is a very first rate mathematical economist who suddenly developed a mid career, I wouldn’t want to call it a mid career crisis but it was certainly a mid career development, where he thought it would be very useful to interview firms about their wage setting policies. I think the profession has actually become more and more appreciative of data. I mean we’ve had large efforts across the board. Many fields. I think it’s much more common now for individuals to collect their own data and to encourage the collection of data. I think 20 or 30 years ago it used to be the case that economists and sociologists were completely opposite ends of the spectrum. That only very few economists were collecting data. And now I think there’s a very, very active group of many economists in many fields collecting their own data whether through experiments or through the secondary collection of data. So I think it’s a major activity.  Daniel McFadden: I agree with Jim. My recommendation to young people though is get tenure first and then develop large data sessions … |
| Q72 | There is another way of testing, at least a complimentary way, and that is experiments and economics is becoming more and more an experimental science. Do you think that this development is all for the good or are there some drawbacks here? Dan. |
|  | Daniel McFadden: I’m very enthusiastic about the opportunities that experiments offer for understanding economic behaviour. And, and for that matter understanding more about economics including the organisation of markets and alternative market forms. So I’m very encouraged. The experimentalists and the econometricians could benefit from talking more to each other because right now the experimentalists have even more than the econometricians difficulty presenting their results in a form which is as concise and informative as a theorem that the theorists can do. And so I think a great opportunity is for the possibility of using econometrics to distil and extract the essence of experimental results. |
| Q71 | Jim, you have voiced some scepticism towards the use of social experiments or at least argued that these experiments have their limitations as a source of knowledge for example to evaluate the effects of programmes. |
|  | James Heckman: Yes, and I think the kind of experiment that Dan’s talking about and the social experiment I think maybe different. And I think one has greater control of the laboratory experiment of the [Vernon Smith](https://www.nobelprize.org/nobel_prizes/economics/laureates/2002/smith-facts.html) variety for example. In the social experimental context I’ve been worried, and have written some papers on this, where I’ve seen misuse of experiments. The danger in a lot of empirical work is that sloganeering takes over. Like it’s true everywhere, I suppose. But in the context of social experiment the very name experiment seems to bring up the image of science, Bunsen burners and truth. Whereas in fact there are serious compliance problems, attrition problems, and those can substantially degrade the inference from an experiment. So that the kind of careful analysis from an experiment that has to be done at the end of the day starts to resemble very much the kind of analysis that comes from a non experimental study. It’s an additional source of variation.  I had a paper published in the May issue of the *Quarterly Journal of Economics*, the May 2000 issue. Where we saw that a social experiment which I was engaged had on a naïve basis led to the notion that job training, all types of job training, were extremely poor. However when you looked at crossover and attrition problems in that study there was a major reworking of the evidence. So very simple corrections. Non-experimental corrections to the experiment caused a rethinking and I think a reshaping conclusion. So it’s a delicate tool. It’s a valuable source of information. But unfortunately in policy circles it’s viewed as somehow a panacea. You know again any slogan whether it’s multiple regression or [INAUDIBLE] or a selection bias correction, any un-critical notion econometric tool is taken out of context can be a very dangerous tool and misapply. |
| Q18 | Your works have been highly relevant for policy making. Do you see cases where your studies have had some impact on actual policy decisions? Dan. |
|  | Daniel McFadden: I think certainly in the area of labour force participation both our works have had major impacts. The question of whether people enter the labour force or not. If they become unemployed how long they stay unemployed. I think our ability to understand the incentives that influence that and how people respond to those incentives have greatly improved.  Can you point to some specific instances? Where do you believe that your studies made a difference in terms of policy decisions?  James Heckman: I would give some examples. Just for the work in job training. Both here and in other countries has had a direct impact in the sense of advice for governments. I was actually involved in the job training partnership act experiment and non experimental study. And as a result of that study, this was a large scale manpower training programme in the US, the programme was fundamentally altered. Now unfortunately I wouldn’t say it was my particular study alone that cost the body of work. There was a group of associates. But there are specific examples. There is now an examination under way about what’s called the GED programme. Exam Certification Programmes. Partly based on this work. So I think there has been. Dan’s being modest but I would say the area of rapid transit study has had huge impact on policy and that’s a prototype for analysis of other policies |
| Q27 | This policy of economics also creates a greater demand for policy advice. Have you been personally involved in policy advice informal or formal? |
|  | Daniel McFadden: Personally I’m rather heavily involved in research related to health outcomes and the economics of health. And economic planning particularly for the elderly. And yes, I get requested quite regularly to try to understand what the impacts are going to be of changes say for example in the healthcare delivery system and how that will influence in turn people’s behaviour both in terms of how they stay healthy and what they do and in terms of their economic planning.  What about you Jim?  James Heckman: Yes I’ve been involved even at a very micro scale. Some of my students and the Cook County Chicago area we’ve been involved in the design and analysis of very small scale job training programmes for concentrated poverty. But also national policy advice. Although I wouldn’t consider myself a policy pundit in the sense of travelling and doing this frequently, more actually in Latin America recently. In the last four or five years through a variety of circumstances I have been giving advice. But only on broad scale issues. Trying to look at education training. |
| Q72 | I would like to conclude by economics as a science with imperialistic ambitions towards the other sciences. Economics invade political science, sociology, demography and so on. Presumably economists find that natural or perhaps a good thing. What about influences in the other direction? Do you believe that we can learn something from say psychology or sociology? Dan? |
|  | Daniel McFadden: I believe we could and we must learn a great deal about behaviour from the people who are studying the individual more as an observational unit itself. That is to say economists tend to concentrate on the outer man. The person who goes to the store and buys products. But you can understand a lot more about behaviour by looking a little bit inside. The processes by which decisions are made. And I think we have a great deal to learn. Right now from the psychologists. Perhaps eventually from the people who are closer to physiology and medicine. The operation of the brain. How perceptions are formed physiologically.  What about you Jim?  James Heckman: I think certainly in my own case I would argue in the 70s a major stimulus to my own thinking was the work of the sociologist on panel data. I think people like Paul Lazarsfeld and his students like Jim Coleman actually were way ahead of economists at one time in analysing panel data. So in the 1970s I taught at a course with Jim Coleman for several years on panel data analysis and the flow was one way for a while. I think we caught up and went ahead of the sociologists. The demographers for years have been dealing with things that are called distributions of fecundicity and mortality, frailty it’s now called. But there’s been a huge amount of stimulus the other way. At least in technical statistical tools and also questions of social science. Issues of social interactions and social processes. I think the sociologists have been ahead of economists and have been stimulating to us and political science as well. Voting theory has actually been quite a productive source of ideas for economists. |
| ID | 0851 |
| Biographical | My wife Beverlee Tito Simboli and I married in 1962. We have three grown children, Nina, Robert, and Raymond, and three grandchildren, Emily, Anne, and Daniel William. Beverlee is a photographer who is best known for her large-format Polaroid works with industrial and abstract subjects. She is the daughter of Raymond Simboli, who immigrated from Italy to Pittsburgh, PA, and was a professor of art in the School of Architecture at Carnegie-Mellon University. My daughter Nina has a B.A. in child psychology and is the executive chef for a corporation in Tucson, Arizona. Robert received his Ph.D. in material science from Carnegie-Mellon University, and heads a research group in the Intel research labs. Raymond received an MBA from the University of California, Berkeley, with specialization in technology management and directs software development at the Excite AtHome Company. Emily and Anne are Robert’s daughters, and Daniel William is Ray’s son.  I was born in 1937 in North Carolina, the eldest son of Robert Sain McFadden and Alice Little McFadden. My father was raised in the mountains of North Carolina, where the McFadden family first settled in 1740. He had only four years of formal schooling, but was a lightning calculator who at age 14 was hired to keep the books for the local bank. He was a gregarious man with a photographic memory for names, faces, and words. My mother was raised in a small Minnesota town on the South Dakota border. Her father Jim Little was born in Minnesota in 1856, the son of an immigrant from Ireland. He spent his early years as a horse trader in Dakota Territory, and became a prosperous small-town businessman. My mother received a degree in architecture from the University of Minnesota in 1922, and an MFA from Columbia. She moved from New York to an architectural practice in Ohio, and later joined the faculty at the University of Cincinnati. She was a quiet, generous person with a fine mind for logic.  My parents met in 1929 when my mother was teaching for a semester at the University of North Carolina. In 1936, she left university life in Cincinnati and married my father. They settled on a remote farm in rural North Carolina, and led an unconventional life, with no electricity or running water and little money. My father was a great collector and reader of books, and I grew up surrounded by his library. My mother became a high school mathematics teacher. Most of our food was grown on the farm. Neighbors were remote, and reading was the primary recreation. I grew up planning to become a farm agent, or a novelist in the florid style of Thomas Wolfe. I was active in 4-H, winning a state championship for my soil conservation projects, and blue ribbons for my sheep and geese. I milked three to five cows each day, and we sold butter, cottage cheese, peanuts, corn, and hay. My parents taught me that to lead a virtuous life, I should be modest, take my satisfaction from work done well, and avoid being drawn into competition for status and rewards.  2. Education I attended rural North Carolina public schools. I was a good student, and my teachers allowed me to read during most of my classes, usually racing through four or five books a day. The offerings in my high school were limited, but I was able to complete correspondence courses in algebra and geometry with help from my mother. During my junior year in high school in 1953, a policy was instituted of automatic suspension for students reported off-campus by police. I started a petition drive among my classmates demanding the right for judicial review. In that time and place, this was enough to get me suspended from school and gave me an opportunity to seek new horizons. I worked for a season on an uncle’s dairy farm in Minnesota, and at age 16 entered the University of Minnesota by examination. At this point, my interests had shifted to science. The deficiencies in my college preparatory training were quickly made up, and at age 19, I received a B.S. in Physics with highest honors. While still an undergraduate, I was hired by Prof. John Winckler to work in his Cosmic Ray Laboratory. In this laboratory, I designed and built an X-ray telescope, and a very early transistorized computer for data processing and telemetry. I learned a great deal from this research experience, far more than I understood at the time, and this shaped my understanding of the scientific enterprise and the interaction of theory and measurement.  Another job I had as an undergraduate was to program card sorters that were being used to construct psychological tests. This led to a great interest in psychological measurement. I continued my studies in physics as a graduate student at Minnesota, but was strongly attracted to the study of human behavior. At that time, the Ford Foundation sponsored an ambitious Behavioral Science Training Program at Minnesota designed to produce scholars who spanned the social sciences. I gained admission to this program in 1958, and embarked on a course of study that included the core Ph.D. courses in psychology, sociology, economics, anthropology, political science, mathematics, and statistics, a total of more than 70 graduate-level courses. I worked as a research assistant for Professors Hal Kelley and Stanley Schacter in Social Psychology, conducting experiments on behavior in the repeated prisoner’s dilemma game, and on the effects of mood-shifting drugs on social interaction. I developed an interest in mathematical models of learning and choice, and found that at Minnesota the faculty with the greatest interests in this subject were Professors John Chipman and [Leo Hurwicz](https://www.nobelprize.org/nobel_prizes/economics/laureates/2007/) in the Economics Department. To work with these professors, I made economics the lead field in my behavioral science program, and in 1960-61 did the course and exam requirements for the economics Ph.D. I was strongly influenced by Chipman and Hurwicz, particularly by their emphasis on axiomatic development of economic theory and the power of formal models. The Ford Foundation program had an externship in the summer before the last year of study, to give the trainees exposure to other research groups. I was sent to Stanford to work for Professors Kenneth Arrow and Marc Nerlove. While there, I had a brief interaction with Prof. Hirofumi Uzawa that proved to be a pivotal point in my research training, giving me a dissertation topic, and, most importantly, a flash of understanding of how to use mathematics as a research tool.  3. Academic career Following the completion of my Ph.D. in 1962, I went to the University of Pittsburgh as a Mellon post-doctoral fellow, and the following year I joined the faculty at the University of California, Berkeley. I continued my interests in choice behavior, but was now also interested rather broadly in the problem of linking economic theory and measurement. I benefitted a great deal from interaction with my colleagues [Peter Diamond](https://www.nobelprize.org/nobel_prizes/economics/laureates/2010/), Roy Radner, Dale Jorgenson, and [Gerard Debreu](https://www.nobelprize.org/nobel_prizes/economics/laureates/1983/index.html), with whom I shared many common interests.  In 1977, I moved to the economics faculty at MIT. In those days, [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economics/laureates/1970/), [Robert Solow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1987/index.html), and [Franco Modigliani](https://www.nobelprize.org/nobel_prizes/economics/laureates/1985/) were intensely active, and intellectual life there was lively. I was given a chair in the name of James Killian, the revered former president of MIT and science advisor to President Eisenhower. In a conversation with Dr. Killian, I learned that his grandfather had owned the cotton mill in which my grandfather was the chief mechanic. When I related this to Bob Solow, he said, “So much for social mobility in America; after two generations, you are still a mechanic in Killian’s mill.”  MIT did not have a department of statistics, and in its place had a Statistics Research Center. In 1986, I became the Director, primarily because my own research relied on good resources in statistics. However, I did not prove administratively adept in improving MIT’s statistics program, and in 1991 chose to return to Berkeley to take advantage of its resources in statistics, and to establish the Econometrics Laboratory, a facility devoted to improving statistical computation for economics applications. I am the holder of the E. Morris Cox Chair, and the endowment from this chair has supported much of my research.  In addition to my regular teaching appointments, I visited the University of Chicago in 1966-67, Yale in 1976-77 as the Irving Fisher Research Professor, and California Institute of Technology in 1990 as a Fairchild Fellow.  4. Research In 1964, I was working with a graduate student, Phoebe Cottingham, who had data on freeway routing decisions of the California Department of Transportation, and was looking for a way to analyze these data to study institutional decision-making behavior. I worked out for her an econometric model based on an axiomatic theory of choice behavior developed by the psychologist Duncan Luce. Drawing upon the work of Thurstone and Marshak, I was able to show how this model linked to the economic theory of choice behavior. These developments, now called the multinomial logit model and the random utility model for choice behavior, have turned out to be widely useful in economics and other social sciences. They are used, for example, to study travel modes, choice of occupation, brand of automobile purchase, and decisions on marriage and number of children.  Over the years, I have written papers on a variety of topics in economics and choice theory, almost all having origins in applied problems. A common theme of my research has been an emphasis on tightly binding economic theory and the problem of economic measurement and analysis, and on developing theoretical and statistical tools that expand the options available to applied economists. I have a strong appreciation for elegant and innovative mathematics and statistics, but as a matter of scientific priority try to keep my research focused on concrete applications, and provide templates for applied economists to follow. I have benefitted from interactions with many colleagues and students over the years. Developments in my core research area of choice behavior have grown particularly from interactions with Professors [Peter Diamond](https://www.nobelprize.org/nobel_prizes/economics/laureates/2010/) and Moshe Ben-Akiva of MIT, Professor James Heckman of the University of Chicago, Professor Charles Manski of Northwestern, and Professor Kenneth Train of Berkeley.  In recent years, my research has concentrated on the deviations from the economic theory of choice, found particularly in the experiments in cognitive psychology conducted by Danny Kahneman and Amos Tversky, and their implications for economic analysis and the interpretation of economic data. I have been studying how people answer questions in economic surveys, and have been developing methods for conducting surveys and experiments on the internet to study these issues. With support from the National Institute on Aging of the National Institute of Health, I have been working on the economic status of elderly Americans, and looking at questions such as the adequacy of housing arrangements, financial planning, and the delivery and cost of health services. I find, for example, that the elderly on average hold on to their assets too long, rather than converting them to income, because they are unrealistically optimistic about the length of remaining life.  5. Personal interests My main avocation, almost a second vocation, is farming. Beverlee and I own a small farm and vineyard in the Napa Valley. We grow and sell grapes, and make wine for our own use. We also grow and sell figs and olive oil. We have five cows, three ducks, and eleven chickens. I find that farm work clears the mind, and the vineyard is a great place to prove theorems.  6. The Nobel Prize I am amazed to win this prize, and delighted that it is shared with Jim Heckman, an old friend with whom I have exchanged ideas over three decades, and whose work is a constant source of ideas and inspiration for me. I am very pleased that the Nobel committee has recognized the scientific value of microeconometrics. I regret that two great scientists, Zvi Griliches and Amos Tversky, who made giant contributions to economics and to my own research, did not live long enough to receive this prize before me. A great deal of credit for what I have achieved over my career goes to Beverlee and my family, who accepted gracefully my dedication to research and provided the perspective needed to balance economics and life. I am donating the prize money to the East Bay Community Foundation, and will direct it to be used to promote arts and education. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0851 |
| Interview |  |
| Q1 | My name is Bertil Holmlund. I am interviewing the two Laureates in Economic Sciences this year: Professor Daniel McFadden from University of California at the Berkley and Professor James Heckman from University of Chicago. They have received the Prize for their contributions to micro econometrics, which is a field on the boundary between economics and statistics. This interview takes place at the Nobel Foundation in Stockholm on December 13, a couple of days after the Laureates have received the Prize. I think I should like to start by asking you how it all began. How come that you started to study economics seriously? Was it more or less by accident or was it something that you had wanted to do for a long time? |
|  | James Heckman: In my case it probably is an accident. I went to a liberal arts college and as part of my background I was majoring in mathematics and physics. But a part of the liberal arts college there’s an option to take readings courses. Working one and one or in small groups. And out of curiosity I chose a readings class in economics where the classics were taught. Allegedly the theme was economic development. but it was reading people like Marshall, Ricardo, Smith and some of the more modern people, [Arthur Lewis](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1979/lewis-facts.html). I had always had a deep interest in social science, history. So even when I was in high school I was debating, and in college debating, and interested in contemporary events. And I had some interest in mathematics. And I was amazed. And then probably the most single event, the important event, was in the same class. The instructor gave me a copy of [Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/samuelson-facts.html)‘s *Foundations* which I found to be an amazingly nice synthesis of things. It was an accident.  Also a social interest?  James Heckman: Yes. I found it interesting because I could then pursue several of my interests simultaneously. I was interested in quantitative analysis, but I also thought economists were asking interesting questions.  What about you?  Daniel McFadden: Well, I came from the natural sciences, from physics. I was a graduate student in physics and found psychology very interesting, and the psychology of behaviour. I had an opportunity to enter this disciplinary programme in the behavioural sciences. And I pursued that for my PhD and got into economics pretty much by accident, because the formal modelling, the axiomatic work that I was interested in was being done primarily by two economists, John Chipman and [Leo Hurwicz](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2007/hurwicz-facts.html). So I made economics my speciality in order to work with them.  Economists typically analyse occupational choice affected by income prospects. Did your choice of occupation to some extent reflect comparisons of incomes across occupations?  Daniel McFadden: For myself personally I would say no. I expected to be a poor academician all my life. And that was my choice. Non pecuniary returns.  What about you Jim?  It was a lifestyle choice more than a choice of money …  James Heckman: I was going to say non pecuniary returns are what drove it. It was a lifestyle choice more than a choice of money. I never expected or particularly sought after financial resources and it was an accident. I always felt I earned a huge rent. I shouldn’t say that on the television, but in the sense that I enjoy very much what I do. And the occupational choice is probably driven more by the psychic than … |
| Q8 | You have both a background in mathematics and physics and perhaps physics is often regarded as an ideal for research. Also economics. Some critics may say that perhaps at the same time economics become more and more like a branch of applied mathematics, how do you react to that criticism of economics? |
|  | Daniel McFadden: I think it’s natural in the development of economics to quantify it. That is to say to move from general theories about how economic systems respond to numerical predictions on what will happen if you change some economic variable. And so mathematics is a natural language for developing a quantitative version of economic theory. Mathematics for its own sake sometimes may have insufficient contact with the facts because in the end a science has to be an interplay between the logical development of the theory and the reality of the facts.  What about you Jim?  James Heckman: I think the example, the contrast between physics and mathematics is a good one, because in some sense physics is driven by mathematics. Mathematics is very useful there. But it always orients itself towards data. Some sort of empirical phenomena or maybe not string theory but in the traditional physics. And in some sense I think that is a major model. It certainly was a major model for me. And I think it’s not a bad model for economics generally to think of using mathematical models but to try to explain some aspect of reality, in this case social reality rather than a physical reality. |
| Q18 | What about econometrics and its role to help us to choose between different models or theories? Ideally statistical testing should be a way of discriminating between alternative theories to weed out the weak theories. Do you think econometrics has been reasonably successful in this regard? |
|  | Daniel McFadden: I certainly think there has been great progress in the last few decades in doing real windowing out of hypothesis about behaviour. I think you see it in the kinds of things that Jim has looked at. Job, the effectiveness of various alternatives for job training. You see it a great deal in labour market and public finance applications. And there I think econometrics has been very successful. If you ask is it successful in modifying the deeper theory of economics I think the answer is less so. That’s partly because that theory is more closely held by economists and they’re less willing to change. Perhaps also because the deepest theory of economics is often viewed as a kind of a parable for how people should behave rather than something that is predictive act by act. So when evidence appears that seems to violate one particular act people will say it’s not a failure of theory. It’s a failure of interpretation or a failure of approximation.  Jim?  James Heckman: I think econometrics has had a very large role, but I think probably one of the biggest developments, it’s been around since the beginning of econometrics. But one of the most important developments has been understanding when we can use data to sort out hypotheses. You know identification questions. And when we cannot. When essentially the matters given available data do not allow a decisive resolution of the issue. I think economists have made a lot of progress on this question in the last 20 or 30 years. So we understand that some matters can’t be settled with the available data and that stimulates the collection of new data where that might be settled.  … testing is like an event, an activity which is very useful …  I think econometrics plays a huge role in thinking through the issues very clearly. So testing is like an event, an activity which is very useful, but I think considering identification and considering what we can in principle separate from what we can’t is extremely useful and is productive in many areas. Observational equivalents is what the macro economists call it. And identification is what the micro economists call it. But it’s the same idea. Same pressures. |
| Q18 | Do you think we should have much more of replication of existing empirical studies to check how robust the results are? Replications of econometrics studies to a much more systematic extent than we have seen so far. |
|  | James Heckman: I’m thinking in particular of a book that you may know of edited by Mary Morgan and Magnus and others, where there was an attempt to replicate [Tobin](https://www.nobelprize.org/nobel_prizes/economics/laureates/1981/tobin-facts.html)‘s study of the demand for food. You know this study? No. Ed Lamar and others participated in this. And it was a little disturbing. Certainly the act of replication was very important but it seemed there were many other judgements that were brought in. And it led people, who were participating in this study of replication, to become aware of something. They started talking about the art of econometrics or the practise. The term, I’m forgetting right now, but there was a term about all the implicit assumptions, tacit econometrics I think was the term. So I think replication is extremely important precisely because in the past to any number there are a lot of other, quote enabling auxiliary assumptions.  Do you want to comment on this?  Daniel McFadden: I certainly think that economics will progress as replication becomes more important. And I would criticise the way our journals currently operate. They tend to always look for things which involve some new technique or some completely new data or some completely new idea. There’s probably insufficient value placed on good work which verifies and checks things that have been done before. |
| Q72 | On the same topic I think of [Wassily Leontief](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1973/leontief-facts.html). He’s actually a former Nobel Laureate in economics and he has drawn the attention to the dominance of theory in economics journals. It seems as if the fraction of pure theory papers without any data is much higher in economics than in say physics or chemistry. Do you think that this is a problem? That it seems as if theory is given much more emphasis in economics than perhaps it is in other sciences. |
|  | James Heckman: I would make the remark that his remark was written in 1972. And it may have been more of a problem in 1972 or 74. I think it was his presidential address for the American Economic Association. It was certainly much earlier. I think there’s been a huge growth in the last 20 years of applied economics. And empirical economics. And if anything one might criticise the other way. That there’s been a huge amount of empiricism without any theory which I think may in fact be equally harmful and possibly more harmful for reasons we talked about earlier. So I’d be less worried now than I think I would have been, or less sympathetic with that comment, than I might have been 20 years ago.  Daniel McFadden: I’ve actually encouraged that traditionally empirical econometric work is hard to present within the bounds of a journal article simply because so much background documentation is required to fully explain what you do empirically. And I think with the development of electronic journals and the possibility of referencing things in a way which is available to readers, to users. Empirical presentation of results should get better and should begin to have more sway I think because you can present the results and provide at the same time the adequate background for those who want to fill in the gaps. |
| Q72 | Empirical research requires data and good quality data and to compile data takes time. Do you think that the collection in improving data is valued as much as it should in the profession? Jim. |
|  | James Heckman: In some quarters yes. If you could point for example study done by Truman Bewley a few years ago. Truman Bewley is a very first rate mathematical economist who suddenly developed a mid career, I wouldn’t want to call it a mid career crisis but it was certainly a mid career development, where he thought it would be very useful to interview firms about their wage setting policies. I think the profession has actually become more and more appreciative of data. I mean we’ve had large efforts across the board. Many fields. I think it’s much more common now for individuals to collect their own data and to encourage the collection of data. I think 20 or 30 years ago it used to be the case that economists and sociologists were completely opposite ends of the spectrum. That only very few economists were collecting data. And now I think there’s a very, very active group of many economists in many fields collecting their own data whether through experiments or through the secondary collection of data. So I think it’s a major activity.  Daniel McFadden: I agree with Jim. My recommendation to young people though is get tenure first and then develop large data sessions … |
| Q72 | There is another way of testing, at least a complimentary way, and that is experiments and economics is becoming more and more an experimental science. Do you think that this development is all for the good or are there some drawbacks here? Dan. |
|  | Daniel McFadden: I’m very enthusiastic about the opportunities that experiments offer for understanding economic behaviour. And, and for that matter understanding more about economics including the organisation of markets and alternative market forms. So I’m very encouraged. The experimentalists and the econometricians could benefit from talking more to each other because right now the experimentalists have even more than the econometricians difficulty presenting their results in a form which is as concise and informative as a theorem that the theorists can do. And so I think a great opportunity is for the possibility of using econometrics to distil and extract the essence of experimental results. |
| Q71 | Jim, you have voiced some scepticism towards the use of social experiments or at least argued that these experiments have their limitations as a source of knowledge for example to evaluate the effects of programmes. |
|  | James Heckman: Yes, and I think the kind of experiment that Dan’s talking about and the social experiment I think maybe different. And I think one has greater control of the laboratory experiment of the [Vernon Smith](https://www.nobelprize.org/nobel_prizes/economics/laureates/2002/smith-facts.html) variety for example. In the social experimental context I’ve been worried, and have written some papers on this, where I’ve seen misuse of experiments. The danger in a lot of empirical work is that sloganeering takes over. Like it’s true everywhere, I suppose. But in the context of social experiment the very name experiment seems to bring up the image of science, Bunsen burners and truth. Whereas in fact there are serious compliance problems, attrition problems, and those can substantially degrade the inference from an experiment. So that the kind of careful analysis from an experiment that has to be done at the end of the day starts to resemble very much the kind of analysis that comes from a non experimental study. It’s an additional source of variation.  I had a paper published in the May issue of the *Quarterly Journal of Economics*, the May 2000 issue. Where we saw that a social experiment which I was engaged had on a naïve basis led to the notion that job training, all types of job training, were extremely poor. However when you looked at crossover and attrition problems in that study there was a major reworking of the evidence. So very simple corrections. Non-experimental corrections to the experiment caused a rethinking and I think a reshaping conclusion. So it’s a delicate tool. It’s a valuable source of information. But unfortunately in policy circles it’s viewed as somehow a panacea. You know again any slogan whether it’s multiple regression or [INAUDIBLE] or a selection bias correction, any un-critical notion econometric tool is taken out of context can be a very dangerous tool and misapply. |
| Q18 | Your works have been highly relevant for policy making. Do you see cases where your studies have had some impact on actual policy decisions? Dan. |
|  | Daniel McFadden: I think certainly in the area of labour force participation both our works have had major impacts. The question of whether people enter the labour force or not. If they become unemployed how long they stay unemployed. I think our ability to understand the incentives that influence that and how people respond to those incentives have greatly improved.  Can you point to some specific instances? Where do you believe that your studies made a difference in terms of policy decisions?  James Heckman: I would give some examples. Just for the work in job training. Both here and in other countries has had a direct impact in the sense of advice for governments. I was actually involved in the job training partnership act experiment and non experimental study. And as a result of that study, this was a large scale manpower training programme in the US, the programme was fundamentally altered. Now unfortunately I wouldn’t say it was my particular study alone that cost the body of work. There was a group of associates. But there are specific examples. There is now an examination under way about what’s called the GED programme. Exam Certification Programmes. Partly based on this work. So I think there has been. Dan’s being modest but I would say the area of rapid transit study has had huge impact on policy and that’s a prototype for analysis of other policies |
| Q27 | This policy of economics also creates a greater demand for policy advice. Have you been personally involved in policy advice informal or formal? |
|  | Daniel McFadden: Personally I’m rather heavily involved in research related to health outcomes and the economics of health. And economic planning particularly for the elderly. And yes, I get requested quite regularly to try to understand what the impacts are going to be of changes say for example in the healthcare delivery system and how that will influence in turn people’s behaviour both in terms of how they stay healthy and what they do and in terms of their economic planning.  What about you Jim?  James Heckman: Yes I’ve been involved even at a very micro scale. Some of my students and the Cook County Chicago area we’ve been involved in the design and analysis of very small scale job training programmes for concentrated poverty. But also national policy advice. Although I wouldn’t consider myself a policy pundit in the sense of travelling and doing this frequently, more actually in Latin America recently. In the last four or five years through a variety of circumstances I have been giving advice. But only on broad scale issues. Trying to look at education training. |
| Q72 | I would like to conclude by economics as a science with imperialistic ambitions towards the other sciences. Economics invade political science, sociology, demography and so on. Presumably economists find that natural or perhaps a good thing. What about influences in the other direction? Do you believe that we can learn something from say psychology or sociology? Dan? |
|  | Daniel McFadden: I believe we could and we must learn a great deal about behaviour from the people who are studying the individual more as an observational unit itself. That is to say economists tend to concentrate on the outer man. The person who goes to the store and buys products. But you can understand a lot more about behaviour by looking a little bit inside. The processes by which decisions are made. And I think we have a great deal to learn. Right now from the psychologists. Perhaps eventually from the people who are closer to physiology and medicine. The operation of the brain. How perceptions are formed physiologically.  What about you Jim?  James Heckman: I think certainly in my own case I would argue in the 70s a major stimulus to my own thinking was the work of the sociologist on panel data. I think people like Paul Lazarsfeld and his students like Jim Coleman actually were way ahead of economists at one time in analysing panel data. So in the 1970s I taught at a course with Jim Coleman for several years on panel data analysis and the flow was one way for a while. I think we caught up and went ahead of the sociologists. The demographers for years have been dealing with things that are called distributions of fecundicity and mortality, frailty it’s now called. But there’s been a huge amount of stimulus the other way. At least in technical statistical tools and also questions of social science. Issues of social interactions and social processes. I think the sociologists have been ahead of economists and have been stimulating to us and political science as well. Voting theory has actually been quite a productive source of ideas for economists. |