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| Biographical | Since 1974, Robert Mundell (born 1932) has been Professor of Economics at Columbia University in New York. After studying at M.I.T. and the London School of Economics, he received his Ph.D. from M.I.T. in 1956, and was the Post-Doctoral Fellow in Political Economy at the University of Chicago in 1956-57. He taught at Stanford University and The Johns Hopkins Bologna Center of Advanced International Studies before joining the staff of the International Monetary Fund in 1961. From 1966 to 1971 he was a Professor of Economics at the University of Chicago and Editor of the journal of Political Economy; and from 1965 to 1975, he was (summer) Professor of International Economics at the Graduate Institute of International Studies in Geneva, Switzerland. For 1997-98 he was the AGIP Professor of Economics at the Johns Hopkins Bologna Center of the Paul H. Nitze School of Advanced International Studies.  Professor Mundell has been an adviser to a number of international agencies and organizations including the United Nations, the IMF, the World Bank, the European Commission, and several governments in Latin America and Europe, the Federal Reserve Board, the US Treasury and the Government of Canada. In 1970, he was a consultant to the Monetary Committee of the European Economic Commission, and in 1972-73 a member of its Study Group on Economic and Monetary Union in Europe¨. He was a member of the Bellagio-Princeton Study Group on International Monetary Reform from 1964 to 1978, and Chairman of the Santa Colomba Conferences on International Monetary Reform between 1971 and 1987.  The author of numerous works and articles on economic theory of international economics, he prepared one of the first plans for a common currency in Europe and is known as the father of the theory of optimum currency areas. He was a pioneer of the theory of the monetary and fiscal policy mix, the theory of inflation and interest and growth, the monetary approach to the balance of payments, and the co-founder of supply-side economics. He has also written extensively on the history of the international monetary system.  Mundell’s writings include over a hundred articles in the scientific journals and the following books: The International Monetary System: Conflict and Reform (1965); Man and Economics and International Economics (1968); Monetary Theory: Interest, Inflation and Growth in the World Economy 1971; and co-edited A Monetary Agenda for the World Economy (1983); Global Disequilibrium (1990); Debts, Deficits and Economic Performance (1991); Building the New Europe (1992); Inflation and Growth in China (1996).  Professor Mundell presented the Frank Graham Memorial Lecture at Princeton University in 1965, the Marshall Lectures at Cambridge University in 1974, and the Ohlin Lectures in 1998. He was the first Rockefeller Research Professor of International Economics at the Brookings Institution in 1964-65, the Ford Foundation Research Professor of Economics at the University of Chicago in 1965-66, the Annenberg Professor of Communications at the University of Southern California in 1980, the Repap Professor of Economics at McGill University in 1989-90, the Richard Fox Professor of Economics at the University of Pennsylvania in 1990-91, and the Agip Professor of Economics at the Bologna Center in 1997-98. He received a Guggenheim Prize in 1971, the Jacques Rueff Medal and Prize in 1983, the Docteur Honoris Causa from the University of Paris in 1992, an Honorary Professorship at Renmin University in China in 1995, the Distinguished Fellow Award from the American Economic Association in 1997, and was made a fellow of the American Academy of Arts and Sciences in October 1998. |
| Autobiographical |  |
| Podcast |  |
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| Interview |  |
| Q4 | And second, you’re very welcome now to the Nobel Foundation for this discussion or conversation. You were rewarded for, in particular, having provided the basic tool of analysis or model to help us understand how economic policy functions in a world with high international mobility of capital. And how monetary and fiscal policy have different effects on national economies if we have floating exchange rates or fixed exchange rates. What was the inspiration of your work? To what extend was it that you looked up the world and saw some problems? And to what extent were you inspired by peer groups, by other scholars, by your teachers, etcetera? |
|  | Well, my first inclination is to say all of the above. But all three of those were important. But in my early days, I wrote my dissertation for MIT at the London School of Economics, really under [James Meade](http://www.nobelprize.org/nobel_prizes/economics/laureates/1977/meade-facts.html), but my dissertation was five chapters on the theory of capital movement, but it didn’t mention money. Money was not … it was a pure development of the classical model to which its higher form is, I think it had been developed, and so there was no monetary or macro, what you would call, what we would call macro considerations, in it. But in the … after I had completed that framework, I felt that now that was a very safe and good box to have for studying economic, long running economic problems, but it wasn’t a good way of analysing, of course, exchange rates and financial problems that hadn’t been modelled in the international economy. And it was then the attempt to find a way of modelling it that became important. And where, I think, the breakthrough in my own thinking of this occurred was with the development with what became my paper. My first paper and most important paper on this model was published in the Quarterly Journal of Economics in May 1960, with the monetary dynamics of fixed and flexible exchange rates.  And in that paper, I think the key element, breakthrough, that I thought for myself that I was making was in the seeing the economy as determined by a combination of two basic macro economic conditions. One is equilibrium in the goods and services market, and equilibrium in the foreign exchange market. And so that led to a kind of international counterpart, if you like, of an IS/LM framework, and so had one curve which gave us macroeconomic equilibrium which was very similar to a kind of generalised IS/LM curve, but with exports and imports in it, and then the other curve was a balance of payments equilibrium equation. And then interest rates and exchange rates would then be the principle variable thing, determining that kind of equilibrium. Now for me, that was an illumination of thinking because it was a way in which I could quickly move from that framework into a general equilibrium framework and put as many markets in as I wanted. |
| Q18 | Your main contribution I think was that you made some general analysis which meant that previous analysis fell out as special cases of your general approach. |
|  | Well, Meade’s work was … the part of Meade’s work that was … could have been relevant was in his mathematical supplement to the balance of payments, and in that model he had some kind of integration of the Keynes’ and [Hicks](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1972/hicks-facts.html)‘ formulation that was good, and somehow I never picked up on that. It was … of course, I sat there, but he never did anything with it himself, he never seemed to bring that into his, any of his analysis, and … I was working when I was thinking about that in terms of the classical version of the model, not when I was writing my thesis, working, thinking about Meade. But it was in Chicago, I think, where I discovered I say, I mean I first read Metzler’s work, on … saving the rate of interest. And I should say it was rather Metzler’s work that that was the closed economy version of what became my first paper, the first major paper on this, on the macro side in 1960 on monetary dynamics.  So the dynamic part of it was very much from Metzler?  Professor Robert A. Mundell: No, of course Metzler in … this whole framework that was flowering at Chicago at this time, but the dynamics were of course completely different from Metzler’s because the subject was different. But the pattern of development was the same. Simonson and Metzler in all my dynamic works, just as Metzler was in a way a student of [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economics/laureates/1970/samuelson-facts.html), I mean a colleague of his, but it was Samuelson’s dynamic analysis that spurred everybody, despite of me, and I learned that very well. And that shows it, so it was a combination of a little bit of Metzler, definitely Metzler and Samuelson. Samuelson’s technique, Metzler’s analysis of the closed economy dynamics and then my formulation of that in the framework of two grand markets, the dominating policy making in the international economy. And then later became, this is the point I wanted to make, this is 1960 and I think that paper was the most important paper I wrote.  And I came up with a formulation that was really quite different from what the others had thought, …  And then subsequently in my other papers, Kyklos and The International Disequilibrium System, 1961. That was an important paper but that took the framework of the much more the IS/LM framework, the Hicks IS/LM framework, that was not a Metzlerian type paper, that was more a Hicksian type paper in the open economy. Hicksian with Metzlerian and Samsonian dynamics in it. And then came the work on the monetary fiscal policy mixed with the IMF, and that was a question, partly, a question of selling. It was a question of people … I thought, with Jack Polak when I came to the IMF, Jack Polak was there, Fleming was away, and Polak asked me to work … study this question that caused Hicks so much controversy, on the appropriate monetary fiscal policy mix for the United States. And everybody was saying different things about it. And I came up with a formulation that was really quite different from what the others had thought, and evolved the policy mix idea that was important. But then this was put in the Keynesian type framework because the desire … everyone’s speaking Keynesian language in order to communicate with the communists at that time, it was necessary to speak the Keynesian language.  And then later this paper, which is published in March 1962 in Staff Papers, came under attack from the Federal Reserve. Of course, remember I was arguing that the United States should shift its policy mix from the current what was called the Samuelson-[Tobin](https://www.nobelprize.org/nobel_prizes/economics/laureates/1981/tobin-facts.html) neo-classical synthesis of lowering interest rates to expand growth and have a budget surplus to siphon off the inflationary effects of that. I was arguing this policy mix would move away from equilibrium. You had to reverse it completely. So it was subject to attack, couple of attacks from the Federal Reserve papers on that, so I decided then I had to underline the point very clearly and what I assumed in that IMF paper was there was some degree of capital mobility.  But in order to make my points more clearly in this more sharper framework, to respond to the criticisms from the Federal Reserve, I assumed perfect capital mobility. And then this is the model that some people consider the Mundell-Fleming, or my part of the Mundell-Fleming model. But actually the Mundell-Fleming model started in 1960 with those other papers I did, and it doesn’t matter so much whether you use a classical or a Keynesian framework for them because the structure is the same. You see, in a general equilibrium system, you have one set of assumptions of rigid prices and flexible output. In the other you have rigid output and flexible prices. And the isomorphism is there. |
| Q1 | This is very much the international background, but at that time in the world economy, most countries had fixed exchange rates, the Bretton Woods system, and also international mobility of capital was very low. Still, you emphasised the international capital mobility and the floating exchange [- – -] Where you influenced by the fact that you were a Canadian, that Canada had this somewhat special regime? Canada had the floating exchange rate from time to time and maybe capital mobility was higher also between Canada and the United States? Was your Canadian background important for your inspiration? |
|  | I know that my Canadian background is important for both this and for the optimum currency areas framework. Definitely the fact that Canada had a system of the first country to have flexible exchange rates. It’s not really clear to many people, people don’t understand. I don’t think anyone’s ever told people why Canada has had flexible exchange rates. It had a fixed exchange rate and exchange controls in the late 1940’s. And then Britain in September 1949 devalued the pound 30% devaluation, to 2 dollars and 80 cents from 4 dollars and 3 cents. And Canada, the Canadian government for some reason, without any basic theory, devalued the Canadian dollar by 10%. The sort of idea is well we’re sort of in between Britain and the United States and we’re closer to the United States than we are to Britain economically, so we would go 20% or we would go 10%. But it was a mistake. A weighted average, exactly. Maybe a trade. But it was a mistake because there was nothing in the Canadian economy that said they should have devalued. But they did devalue.  But very soon after there was an influx of capital coming into Canada, then the Korean War broke out, raising the demand for Canadian products and raw materials. So Canada was engulfed with capital. What they should have done is just appreciate the Canadian dollar back to a parity where it was, but they couldn’t do that without appearing to have made a mistake in 1949. So they floated. And then they left this out and there was a big discussion with the IMF. And anyway, Canada had all through the 1950’s had a floating exchange rate. It didn’t float very much. It floated upwards. It went up to a premium against the Americans and then they had to talk it down and they had to … they didn’t like the way it was working and so they ended up going back to fixed rates. But I was definitely saying you needed a model to study flexible exchange rates. But what models existed to study? There is in the open economy there was the matrix multiplier type, two country multiplier and matrix multiplier type model, and these models had no monetary policy in them, they just assumed interest rate … and money wasn’t in it. And then there was this very good paper, excellent, very important paper by Laursen and Metzler which had two economies, two markets, goods and services markets and the balance of payment equilibrium with flexible exchange rates.  So that was the first model you had of a flexible exchange rate system, but it was in the context no money was in it, but you had flexible exchange rates and you … they have to be interpreted as real exchange rates. But it was a very ingenious paper and a very important paper. And that paper assumed no capital movements because they had controls over capital movements in the assumption in that paper. Well obviously, in the Canadian context that didn’t work, because after Canada had a floating exchange rate, after a while it took away its controls, there was no function to have … controls anymore And you had perfect capital mobility effectively between Canada and the United States except for the two currencies which always prevents an ideal situation from developing, you had capital that was completely free to move between Canada and the United States. So this was a natural IFM modelling, fixed or flexible exchange rates I model world of capital mobility. I agree with you, it was the … Canadian background there was quite important. |
| Q18 | You mentioned your other important contribution, the so-called theory of optimum currency areas. Instead of asking the question “Should a nation have floating for fixed exchange rates?” you re-formulated the question and said “In which areas should people chose, regardless of nation states, fixed, floating rates, or should they form a monetary union?” And of course this idea about monetary union has become more and more important over the years, and the most important development recently of course is the European Monetary Union has been established. Do you think this was a good idea to create the European Monetary Union? Your theory of optimum currency areas emphasised the importance of high labour mobility within an area that had a common currency. And many people believe that Europe does not yet have that kind of labour mobility. And many people think that Europe is not the optimum currency area by your definition. What is your reflection about that? What do you think about the idea of European Monetary Union? |
|  | Yes, it just is for the background on that and the paper on optimum currency areas and the first paragraph I think ends with something to the effect that we talked about the argument for flexible exchange rates being based on money illusion etc., and then it said this paper casts doubt on one of the alternatives to adjusting the terms of trade through flexible exchange rates. And so this paper has to be looked upon as really a kind of criticism of flexible exchange rates and with the conclusion that unless currency areas were contiguous with regions defined by factor mobility. That was the point. So you can look at that in two ways. I was … here again the Canadian influence is important. I first gave that paper in 1957 at a faculty seminar at UBC British Columbia, and what I did was to argue that flexible exchange rates between Canada and the United States wouldn’t help British Columbia if there was a decline in demand for lumber products in here at terms of trade problems for British Columbia or something of that nature. Canada/US flexibility wouldn’t help. You’d have to have a separate currency for BC in order to apply … British Columbia, in order to apply the arguments for flexible exchange rates and that would be true in any country with multi-regions or with zones in the country of inflexibility.  So it was really the genesis of the paper was in large part that flexible exchange rates aren’t going to do what people think they’re going to do because in between Canada and the United States this wasn’t … and I still believe that it doesn’t really, it isn’t a good system between Canada and the United States and between other countries that have money regions. If you have flexible exchange rates between Italy and elsewhere it doesn’t help the problem between the North and South of Italy. It doesn’t help the problem in the different parts of Spain, the different parts … etc., etc. So you could if the argument for flexible exchange rates were really valid you really should separate countries up into smaller and smaller regions. The only trouble is that as you do that you lose more and more of the functions of money, and as the regions get smaller and smaller the basic argument for flexible exchange rates begins to break down because as the region gets smaller it becomes more open, and then you can no longer assume that workers have this money illusion that they’re going to not see the link between the exchange rate and domestic prices and their wage rates. So that was the argument. |
| Q18 | So what will happen with the dollar now when you get the European currency? Will that out-compete the dollar in world economy or is dollar so strong that it will stay as a dominating currence in currency world? |
|  | We have the short run aspects of this and the intermediate and long run aspects. The long run growth of the dollar area and the Euro area will depend in a large part on the growth of population in the economies. I can see certainly that right now you can think of the GDP area, the transactions area of the dollar is now about … US GDP’s about 9 trillion, and the EU 11 countries have a GDP of about 7 trillion, but as the other countries come into the Euroland, as Sweden and Britain, Denmark and Greece, and I think they will come in maybe three or four years, we don’t know, but I think they will come in, and as countries in central and eastern Europe start joining either by currency boards or coming completely into the Union, by the year in 12 years time that Euroland is going to be considerably larger than the dollar area of the United States itself. Now of course the dollar area is likely to grow because there’s a growing movement in Latin America to link some more currencies to the dollar, just as the Euro area is going to grow. But certainly in getting up to say a dozen years from now, you can imagine that most central banks would want to hold their assets about evenly balanced between Euros and dollars.  So there’s going to be a big demand if you get to that steady state of equilibrium, a big annual demand for Euros because the euro is very underweight in terms of portfolios of all around the world, compared to what the equilibrium certainly will be in a few years. So you could imagine a demand, an annual demand for Euros even with no growth of dollars of something like 50 or 100 billion dollars every year and this is going to have a big impact on exchange rates, that is going to add greatly to the strength of the euro against the dollar. |
| ID | 0853 |
| Biographical | I was born in a University campus and seem to have lived all my life in one campus or another. My family is from Dhaka – now the capital of Bangladesh. My ancestral home in Wari in “old Dhaka” is not far from the University campus in Ramna. My father Ashutosh Sen taught chemistry at Dhaka University. I was, however, born in Santiniketan, on the campus of [Rabindranath Tagore](https://www.nobelprize.org/nobel_prizes/literature/laureates/1913/index.html)‘s Visva-Bharati (both a school and a college), where my maternal grandfather (Kshiti Mohan Sen) used to teach Sanskrit as well as ancient and medieval Indian culture, and where my mother (Amita Sen), like me later, had been a student. After Santiniketan, I studied at Presidency College in Calcutta and then at Trinity College in Cambridge, and I have taught at universities in both these cities, and also at Delhi University, the London School of Economics, Oxford University, and Harvard University, and on a visiting basis, at M.I.T., Stanford, Berkeley, and Cornell. I have not had any serious non-academic job.  My planned field of study varied a good deal in my younger years, and between the ages of three and seventeen, I seriously flirted, in turn, with Sanskrit, mathematics, and physics, before settling for the eccentric charms of economics. But the idea that I should be a teacher and a researcher of some sort did not vary over the years. I am used to thinking of the word “academic” as meaning “sound,” rather than the more old-fashioned dictionary meaning: “unpractical,” “theoretical,” or “conjectural.”  During three childhood years (between the ages of 3 and 6) I was in Mandalay in Burma, where my father was a visiting professor. But much of my childhood was, in fact, spent in Dhaka, and I began my formal education there, at St. Gregory’s School. However, I soon moved to Santiniketan, and it was mainly in Tagore’s school that my educational attitudes were formed. This was a co-educational school, with many progressive features. The emphasis was on fostering curiosity rather than competitive excellence, and any kind of interest in examination performance and grades was severely discouraged. (“She is quite a serious thinker,” I remember one of my teachers telling me about a fellow student, “even though her grades are very good.”) Since I was, I have to confess, a reasonably good student, I had to do my best to efface that stigma.  The curriculum of the school did not neglect India’s cultural, analytical and scientific heritage, but was very involved also with the rest of the world. Indeed, it was astonishingly open to influences from all over the world, including the West, but also other non-Western cultures, such as East and South-East Asia (including China, Japan, Indonesia, Korea), West Asia, and Africa. I remember being quite struck by Rabindranath Tagore’s approach to cultural diversity in the world (well reflected in our curriculum), which he had expressed in a letter to a friend: “Whatever we understand and enjoy in human products instantly becomes ours, wherever they might have their origin… Let me feel with unalloyed gladness that all the great glories of man are mine.”  Identity and violence I loved that breadth, and also the fact that in interpreting Indian civilization itself, its cultural diversity was much emphasized. By pointing to the extensive heterogeneity in India’s cultural background and richly diverse history, Tagore argued that the “idea of India” itself militated against a culturally separatist view, “against the intense consciousness of the separateness of one’s own people from others.” Tagore and his school constantly resisted the narrowly communal identities of Hindus or Muslims or others, and he was, I suppose, fortunate that he died – in 1941 – just before the communal killings fomented by sectarian politics engulfed India through much of the 1940s. Some of my own disturbing memories as I was entering my teenage years in India in the mid-1940s relate to the massive identity shift that followed divisive politics. People’s identities as Indians, as Asians, or as members of the human race, seemed to give way – quite suddenly – to sectarian identification with Hindu, Muslim, or Sikh communities. The broadly Indian of January was rapidly and unquestioningly transformed into the narrowly Hindu or finely Muslim of March. The carnage that followed had much to do with unreasoned herd behaviour by which people, as it were, “discovered” their new divisive and belligerent identities, and failed to take note of the diversity that makes Indian culture so powerfully mixed. The same people were suddenly different.  I had to observe, as a young child, some of that mindless violence. One afternoon in Dhaka, a man came through the gate screaming pitifully and bleeding profusely. The wounded person, who had been knifed on the back, was a Muslim daily labourer, called Kader Mia. He had come for some work in a neighbouring house – for a tiny reward – and had been knifed on the street by some communal thugs in our largely Hindu area. As he was being taken to the hospital by my father, he went on saying that his wife had told him not to go into a hostile area during the communal riots. But he had to go out in search of work and earning because his family had nothing to eat. The penalty of that economic unfreedom turned out to be death, which occurred later on in the hospital. The experience was devastating for me, and suddenly made me aware of the dangers of narrowly defined identities, and also of the divisiveness that can lie buried in communitarian politics. It also alerted me to the remarkable fact that economic unfreedom, in the form of extreme poverty, can make a person a helpless prey in the violation of other kinds of freedom: Kader Mia need not have come to a hostile area in search of income in those troubled times if his family could have managed without it.  Calcutta and its debates By the time I arrived in Calcutta to study at Presidency College, I had a fairly formed attitude on cultural identity (including an understanding of its inescapable plurality as well as the need for unobstructed absorption rather than sectarian denial). I still had to confront the competing loyalties of rival political attitudes: for example, possible conflicts between substantive equity, on the one hand, and universal tolerance, on the other, which simultaneously appealed to me. On this more presently.  The educational excellence of Presidency College was captivating. My interest in economics was amply rewarded by quite outstanding teaching. I was particularly influenced by the teaching of Bhabatosh Datta and Tapas Majumdar, but there were other great teachers as well, such as Dhiresh Bhattacharya. I also had the great fortune of having wonderful classmates, particularly the remarkable Sukhamoy Chakravarty (more on him presently), but also many others, including Mrinal Datta Chaudhuri (who was also at Santiniketan, earlier) and Jati Sengupta. I was close also to several students of history, such as Barun De, Partha Gupta and Benoy Chaudhuri. (Presidency College had a great school of history as well, led by a most inspiring teacher in the form of Sushobhan Sarkar.) My intellectual horizon was radically broadened.  The student community of Presidency College was also politically most active. Though I could not develop enough enthusiasm to join any political party, the quality of sympathy and egalitarian commitment of the “left” appealed to me greatly (as it did to most of my fellow students as well, in that oddly elitist college). The kind of rudimentary thinking that had got me involved, while at Santiniketan, in running evening schools (for illiterate rural children in the neighbouring villages) seemed now to be badly in need of systematic political broadening and social enlargement.  I was at Presidency College during 1951 to 1953. The memory of the Bengal famine of 1943, in which between two and three million people had died, and which I had watched from Santiniketan, was still quite fresh in my mind. I had been struck by its thoroughly class-dependent character. (I knew of no one in my school or among my friends and relations whose family had experienced the slightest problem during the entire famine; it was not a famine that afflicted even the lower middle classes – only people much further down the economic ladder, such as landless rural labourers.) Calcutta itself, despite its immensely rich intellectual and cultural life, provided many constant reminders of the proximity of unbearable economic misery, and not even an elite college could ignore its continuous and close presence.  And yet, despite the high moral and ethical quality of social commiseration, political dedication and a deep commitment to equity, there was something rather disturbing about standard leftwing politics of that time: in particular, its scepticism of process-oriented political thinking, including democratic procedures that permit pluralism. The major institutions of democracy got no more credit than what could be portioned out to what was seen as “bourgeois democracy,” on the deficiencies of which the critics were most vocal. The power of money in many democratic practices was rightly identified, but the alternatives – including the terrible abuses of non-oppositional politics – did not receive serious critical scrutiny. There was also a tendency to see political tolerance as a kind of “weakness of will” that may deflect well-meaning leaders from promoting “the social good,” without let or hindrance.  Given my political conviction on the constructive role of opposition and my commitment to general tolerance and pluralism, there was a bit of a dilemma to be faced in coordinating those beliefs with the form of left-wing activism that characterized the mainstream of student politics in the-then Calcutta. What was at stake, it seemed to me, in political toleration was not just the liberal political arguments that had so clearly emerged in post-Enlightenment Europe and America, but also the traditional values of tolerance of plurality which had been championed over the centuries in many different cultures – not least in India. Indeed, as Ashoka had put it in the third century B.C.: “For he who does reverence to his own sect while disparaging the sects of others wholly from attachment to his own, with intent to enhance the splendour of his own sect, in reality by such conduct inflicts the severest injury on his own sect.” To see political tolerance merely as a “Western liberal” inclination seemed to me to be a serious mistake.  Even though these issues were quite disturbing, they also forced me to face some foundational disputes then and there, which I might have otherwise neglected. Indeed, we were constantly debating these competing political demands. As a matter of fact, as I look back at the fields of academic work in which I have felt most involved throughout my life (and which were specifically cited by the Royal Swedish Academy of Sciences in making their award), they were already among the concerns that were agitating me most in my undergraduate days in Calcutta. These encompassed welfare economics, economic inequality and poverty, on the one hand (including the most extreme manifestation of poverty in the form of famines), and the scope and possibility of rational, tolerant and democratic social choice, on the other (including voting procedures and the protection of liberty and minority rights). My involvement with the fields of research identified in the Nobel statement had, in fact, developed much before I managed to do any formal work in these areas.  It was not long after [Kenneth Arrow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1972/index.html)‘s path-breaking study of social choice, Social Choice and Individual Values, was published in New York in 1951, that my brilliant co-student Sukhamoy Chakravarty drew my attention to the book and to Arrow’s stunning “impossibility theorem” (this must have been in the early months of 1952). Sukhamoy too was broadly attracted by the left, but also worried about political authoritarianism, and we discussed the implications of Arrow’s demonstration that no non-dictatorial social choice mechanism may yield consistent social decisions. Did it really give any excuse for authoritarianism (of the left, or of the right)? I particularly remember one long afternoon in the College Street Coffee House, with Sukhamoy explaining his own reading of the ramifications of the formal results, sitting next to a window, with his deeply intelligent face glowing in the mild winter sun of Calcutta (a haunting memory that would invade me again and again when he died suddenly of a heart attack a few years ago).  Cambridge as a battleground In 1953, I moved from Calcutta to Cambridge, to study at Trinity College. Though I had already obtained a B.A. from Calcutta University (with economics major and mathematics minor), Cambridge enroled me for another B.A. (in pure economics) to be quickly done in two years (this was fair enough since I was still in my late teens when I arrived at Cambridge). The style of economics at the-then Cambridge was much less mathematical than in Calcutta. Also, it was generally less concerned with some of the foundational issues that had agitated me earlier. I had, however, some wonderful fellow students (including Samuel Brittan, Mahbub ul Haq, Rehman Sobhan, Michael Nicholson, Lal Jayawardena, Luigi Pasinetti, Pierangelo Garegnani, Charles Feinstein, among others) who were quite involved with foundational assessment of the ends and means of economics as a discipline.  However, the major debates in political economy in Cambridge were rather firmly geared to the pros and cons of Keynesian economics and the diverse contributions of Keynes’s followers at Cambridge (Richard Kahn, Nicholas Kaldor, Joan Robinson, among them), on the one hand, and of “neo-classical” economists sceptical of Keynes, on the other (including, in different ways, Dennis Robertson, Harry Johnson, Peter Bauer, Michael Farrell, among others). I was lucky to have close relations with economists on both sides of the divide. The debates centred on macroeconomics dealing with economic aggregates for the economy as a whole, but later moved to capital theory, with the neo-Keynesians dead set against any use of “aggregate capital” in economic modelling (some of my fellow students, including Pasinetti and Garegnani, made substantial contributions to this debate).  Even though there were a number of fine teachers who did not get very involved in these intense fights between different schools of thought (such as Richard Stone, Brian Reddaway, Robin Matthews, Kenneth Berrill, Aubrey Silberston, Robin Marris), the political lines were, in general, very firmly – and rather bizarrely – drawn. In an obvious sense, the Keynesians were to the “left” of the neo-classicists, but this was very much in the spirit of “this far but no further”. Also, there was no way in which the different economists could be nicely ordered in just one dimension. Maurice Dobb, who was an astute Marxist economist, was often thought by Keynesians and neo-Keynesians to be “quite soft” on “neo-classical” economics. He was one of the few who, to my delight, took welfare economics seriously (and indeed taught a regular course on it), just as the intensely “neo-classical” A.C. Pigou had done (while continuing to debate Keynes in macroeconomics). Not surprisingly, when the Marxist Dobb defeated Kaldor in an election to the Faculty Board, Kaldor declared it to be a victory of the perfidious neo-classical economics in disguise (“marginal utility theory has won,” Kaldor told Sraffa that evening, in commenting on the electoral success of a Marxist economist!)  However, Kaldor was, in fact, much the most tolerant of the neo-Keynesians at Cambridge. If Richard Kahn was in general the most bellicose, the stern reproach that I received often for not being quite true to the new orthodoxy of neo-Keynesianism came mostly from my thesis supervisor – the totally brilliant but vigorously intolerant Joan Robinson.  In this desert of constant feuding, my own college, Trinity, was a bit of an oasis. I suppose I was lucky to be there, but it was not entirely luck, since I had chosen to apply to Trinity after noticing, in the handbook of Cambridge University, that three remarkable economists of very different political views coexisted there. The Marxist Maurice Dobb and the conservative neo-classicist Dennis Robertson did joint seminars, and Trinity also had Piero Sraffa, a model of scepticism of nearly all the standard schools of thought. I had the good fortune of working with all of them and learning greatly from each.  The peaceful – indeed warm – co-existence of Dobb, Robertson and Sraffa was quite remarkable, given the feuding in the rest of the University. Sraffa told me, later on, a nice anecdote about Dobb’s joining of Trinity, on the invitation of Robertson. When asked by Robertson whether he would like to teach at Trinity, Dobb said yes enthusiastically, but he suffered later from a deep sense of guilt in not having given Robertson “the full facts. ” So he wrote a letter to Robertson apologizing for not having mentioned earlier that he was a member of the Communist Party, supplemented by the statement – I think a rather “English” statement – that he would understand perfectly if in view of that Robertson were to decide that he, Dobb, was not a fit person to teach Trinity undergraduates. Robertson wrote a one-sentence reply: “Dear Dobb, so long as you give us a fortnight’s notice before blowing up the Chapel, it will be all right.”  So there did exist, to some extent, a nice “practice” of democratic and tolerant social choice at Trinity, my own college. But I fear I could not get anyone in Trinity, or in Cambridge, very excited in the “theory” of social choice. I had to choose quite a different subject for my research thesis, after completing my B.A. The thesis was on “the choice of techniques,” which interested Joan Robinson as well as Maurice Dobb.  Philosophy and economics At the end of the first year of research, I was bumptious enough to think that I had some results that would make a thesis, and so I applied to go to India on a two-years leave from Cambridge, since I could not – given the regulation then in force – submit my Ph.D. thesis for a degree until I had been registered for research for three years. I was excitedly impatient in wanting to find out what was going on back at home, and when leave was granted to me, I flew off immediately to Calcutta. Cambridge University insisted on my having a “supervisor” in India, and I had the good fortune of having the great economic methodologist, A.K. Dasgupta, who was then teaching in Benares. With him I had frequent – and always enlightening – conversations on everything under the sun (occasionally on my thesis as well).  In Calcutta, I was also appointed to a chair in economics at the newly created Jadavpur University, where I was asked to set up a new department of economics. Since I was not yet even 23, this caused a predictable – and entirely understandable – storm of protest. But I enjoyed the opportunity and the challenge (even though several graffitis on the University walls displayed the “new professor” as having been just snatched from the cradle). Jadavpur was quite an exciting place intellectually (my colleagues included Paramesh Ray, Mrinal Datta Chaudhuri, Anita Banerji, Ajit Dasgupta, and others in the economics department). The University also had, among other luminaries, the immensely innovative historian, Ranajit Guha, who later initiated the “subaltern studies” – a highly influential school of colonial and post-colonial history. I particularly enjoyed getting back to some of the foundational issues that I had to neglect somewhat at Cambridge.  While my thesis was quietly “maturing” with the mere passage of time (to be worthy of the 3-year rule), I took the liberty of submitting it for a competitive Prize Fellowship at Trinity College. Since, luckily, I also got elected, I then had to choose between continuing in Calcutta and going back to Cambridge. I split the time, and returned to Cambridge somewhat earlier than I had planned. The Prize Fellowship gave me four years of freedom to do anything I liked (no questions asked), and I took the radical decision of studying philosophy in that period. I had always been interested in logic and in epistemology, but soon got involved in moral and political philosophy as well (they related closely to my older concerns about democracy and equity).  The broadening of my studies into philosophy was important for me not just because some of my main areas of interest in economics relate quite closely to philosophical disciplines (for example, social choice theory makes intense use of mathematical logic and also draws on moral philosophy, and so does the study of inequality and deprivation), but also because I found philosophical studies very rewarding on their own. Indeed, I went on to write a number of papers in philosophy, particularly in epistemology, ethics and political philosophy. While I am interested both in economics and in philosophy, the union of my interests in the two fields far exceeds their intersection. When, many years later, I had the privilege of working with some major philosophers (such as John Rawls, Isaiah Berlin, Bernard Williams, Ronald Dworkin, Derek Parfit, Thomas Scanlon, Robert Nozick, and others), I felt very grateful to Trinity for having given me the opportunity as well as the courage to get into exacting philosophy.  Delhi School of Economics During 1960-61, I visited M.I.T., on leave from Trinity College, and found it a great relief to get away from the rather sterile debates that the contending armies were fighting in Cambridge. I benefited greatly from many conversations with [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economics/laureates/1970/index.html), [Robert Solow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1987/index.html), [Franco Modigliani](https://www.nobelprize.org/nobel_prizes/economics/laureates/1985/index.html), Norbert Wiener, and others that made M.I.T such an inspiring place. A summer visit to Stanford added to my sense of breadth of economics as a subject. In 1963, I decided to leave Cambridge altogether, and went to Delhi, as Professor of Economics at the Delhi School of Economics and at the University of Delhi. I taught in Delhi until 1971. In many ways this was the most intellectually challenging period of my academic life. Under the leadership of K.N. Raj, a remarkable applied economist who was already in Delhi, we made an attempt to build an advanced school of economics there. The Delhi School was already a good centre for economic study (drawing on the work of V.K.R.V. Rao, B.N. Ganguli, P.N. Dhar, Khaleq Naqvi, Dharm Narain, and many others, in addition to Raj), and a number of new economists joined, including Sukhamoy Chakravarty, Jagdish Bhagwati, A.L. Nagar, Manmohan Singh, Mrinal Datta Chaudhuri, Dharma Kumar, Raj Krishna, Ajit Biswas, K.L. Krishna, Suresh Tendulkar, and others. (Delhi School of Economics also had some leading social anthropologists, such as M.N. Srinivas, Andre Beteille, Baviskar, Veena Das, and major historians such as Tapan Ray Chaudhuri, whose work enriched the social sciences in general.) By the time I left Delhi in 1971 to join the London School of Economics, we had jointly succeeded in making the Delhi School the pre-eminent centre of education in economics and the social sciences, in India.  Regarding research, I plunged myself full steam into social choice theory in the dynamic intellectual atmosphere of Delhi University. My interest in the subject was consolidated during a one-year visit to Berkeley in 1964-65, where I not only had the chance to study and teach some social choice theory, but also had the unique opportunity of observing some practical social choice in the form of student activism in the “free speech movement.” An initial difficulty in pursuing social choice at the Delhi School was that while I had the freedom to do what I liked, I did not, at first, have anyone who was interested in the subject as a formal discipline. The solution, of course, was to have students take an interest in the subject. This happened with a bang with the arrival of a brilliant student, Prasanta Pattanaik, who did a splendid thesis on voting theory, and later on, also did joint work with me (adding substantially to the reach of what I was trying to do). Gradually, a sizeable and technically excellent group of economists interested in social choice theory emerged at the Delhi School.  Social choice theory related importantly to a more widespread interest in aggregation in economic assessment and policy making (related to poverty, inequality, unemployment, real national income, living standards). There was a great reason for satisfaction in the fact that a number of leading social choice theorists (in addition to Prasanta Pattanaik) emanated from the Delhi School, including Kaushik Basu and Rajat Deb (who also studied with me at the London School of Economics after I moved there), and Bhaskar Dutta and Manimay Sengupta, among others. There were other students who were primarily working in other areas (this applies to Basu as well), but whose work and interests were influenced by the strong current of social choice theory at the Delhi School (Nanak Kakwani is a good example of this).  In my book, Collective Choice and Social Welfare, published in 1970, I made an effort to take on overall view of social choice theory. There were a number of analytical findings to report, but despite the presence of many “trees” (in the form of particular technical results), I could not help looking anxiously for the forest. I had to come back again to the old general question that had moved me so much in my teenage years at Presidency College: Is reasonable social choice at all possible given the differences between one person’s preferences (including interests and judgments) and another’s (indeed, as Horace noted a long time ago, there may be “as many preferences as there are people”)?  The work underlying Collective Choice and Social Welfare was mostly completed in Delhi, but I was much helped in giving it a final shape by a joint course on “social justice” I taught at Harvard with Kenneth Arrow and John Rawls, both of whom were wonderfully helpful in giving me their assessments and suggestions. The joint course was, in fact, quite a success both in getting many important issues discussed, and also in involving a remarkable circle of participants (who were sitting in as “auditors”), drawn from the established economists and philosophers in the Harvard region. (It was also quite well-known outside the campus: I was asked by a neighbour in a plane journey to San Francisco whether, as a teacher at Harvard, I had heard of an “apparently interesting” course taught by “Kenneth Arrow, John Rawls, and some unknown guy.”)  There was another course I taught jointly, with Stephen Marglin and Prasanta Pattanaik (who too had come to Harvard), which was concerned with development as well as Policy making. This nicely supplemented my involvements in pure social choice theory (in fact, Marglin and Pattanaik were both very interested in examining the connection between social choice theory and other areas in economics).  From Delhi to London and Oxford I left Delhi, in 1971, shortly after Collective Choice and Social Welfare was published in 1970. My wife, Nabaneeta Dev, with whom I have two children (Antara and Nandana), had constant trouble with her health in Delhi (mainly from asthma). London might have suited her better, but, as it happens, the marriage broke up shortly after we went to London.  Nabaneeta is a remarkably successful poet, literary critic and writer of novels and short stories (one of the most celebrated authors in contemporary Bengali literature), which she has combined, since our divorce, with being a University Professor at Jadavpur University in Calcutta. I learned many things from her, including the appreciation of poetry from an “internal” perspective. She had worked earlier on the distinctive style and composition of epic poetry, including the Sanskrit epics (particularly the Ramayana), and this I had got very involved in. Nabaneeta’s parents were very well-known poets as well, and she seems to have borne her celebrity status – and the great many recognitions that have come her way – with unaffected approachability and warmth. She had visits from an unending stream of literary fans, and I understand, still does. (On one occasion, arrived a poet with a hundred new poems, with the declared intention of reading them aloud to her, to get her critical judgement, but since she was out, he said that he would instead settle for reading them to me. When I pleaded that I lacked literary sophistication, I was assured by the determined poet: “That is just right; I would like to know how the common man may react to my poetry.” The common man, I am proud to say, reacted with appropriate dignity and self-control.)  When we moved to London, I was also going through some serious medical problems. In early 1952, at the age of 18 (when I was an undergraduate at Presidency College), I had cancer of the mouth, and it had been dealt with by a severe dose of radiation in a rather primitive Calcutta hospital. This was only seven years after Hiroshima and Nagasaki, and the long-run effects of radiation were not much understood. The dose of radiation I got may have cured the cancer, but it also killed the bones in my hard palate. By 1971, it appeared that I had either a recurrence of the cancer, or a severe case of bone necrosis. The first thing I had to do on returning to England was to have a serious operation, without knowing whether it would be merely plastic surgery to compensate for the necrosis (a long and complicated operation in the mouth, but no real threat to survival), or much more demandingly, a fresh round of efforts at cancer eradication.  After the long operation (it had lasted nearly seven hours) when I woke up from the heavy anaesthesia, it was four o’clock in the morning. As a person with much impatience, I wanted to know what the surgeon had found. The nurse on duty said she was not allowed to tell me anything: “You must wait for the doctors to come at nine.” This created some tension (I wanted to know what had emerged), which the nurse noticed. I could see that she was itching to tell me something: indeed (as I would know later) to tell me that no recurrence of cancer had been detected in the frozen-section biopsy that had been performed, and that the long operation was mainly one of reconstruction of the palate to compensate for the necrosis. She ultimately gave in, and chose an interesting form of communication, which I found quite striking (as well as kind). “You know,” she said, “they were praising you very much!” It then dawned on me that not having cancer can be a subject for praise. Indeed lulled by praise, I went quietly back to my post-operative sleep. In later years, when I would try to work on judging the goodness of a society by the quality of health of the people, her endorsement of my praiseworthiness for being cancer-free would serve as a good reference point!  The intellectual atmosphere at the LSE in particular and in London in general was most gratifying, with a dazzling array of historians, economists, sociologists and others. It was wonderful to have the opportunity of seeing Eric Hobsbawm (the great historian) and his wife Marlene very frequently and to interact regularly with Frank and Dorothy Hahn, Terence and Dorinda Gorman, and many others. Our small neighbourhood in London (Bartholomew estate, within the Kentish Town) itself offered wonderful company of intellectual and artistic creativity and political involvement. Even after I took an Oxford job (Professor of Economics, 1977-80, Drummond Professor of Political Economy, 1980-87) later on, I could not be budged from living in London.  As I settled down at the London School of Economics in 1971, I resumed my work on social choice theory. Again, I had excellent students at LSE, and later on at Oxford. In addition to Kaushik Basu and Rajat Deb (who had come from Dehli), other students such as Siddiq Osmani, Ben Fine, Ravi Kanbur, Carl Hamilton, John Wriglesworth, David Kelsey, Yasumi Matsumoto, Jonathan Riley, produced distinguished Ph.D. theses on a variety of economic and social choice problems. It made me very proud that many of the results that became standard in social choice theory and welfare economics had first emerged in these Ph.D. theses.  I was also fortunate to have colleagues who were working on serious social choice problems, including Peter Hammond, Charles Blackorby, Kotaro Suzumura, Geoffrey Heal, Gracieda Chichilnisky, Ken Binmore, Wulf Gaertner, Eric Maskin, John Muellbauer, Kevin Roberts, Susan Hurley, at LSE or Oxford, or neighbouring British universities. (I also learned greatly from conversations with economists who were in other fields, but whose works were of great interest to me, including Sudhir Anand, Tony Atkinson, Christopher Bliss, Meghnad Desai, Terence Gorman, Frank Hahn, David Hendry, Richard Layard, [James Mirrlees](https://www.nobelprize.org/nobel_prizes/economics/laureates/1996/index.html), John Muellbauer, Steve Nickel, among others.) I also had the opportunity of collaboration with social choice theorists elsewhere, such as Claude d’Aspremont and Louis Gevers in Belgium, Koichi Hamada and Ken-ichi Inada in Japan (joined later by Suzumura when he returned there), and many others in America, Canada, Israel, Australia, Russia, and elsewhere). There were many new formal results and informal understandings that emerged in these works, and the gloom of “impossibility results” ceased to be the only prominent theme in the field. The 1970s were probably the golden years of social choice theory across the world. Personally, I had the sense of having a ball.  From social choice to inequality and poverty The constructive possibilities that the new literature on social choice produced directed us immediately to making use of available statistics for a variety of economic and social appraisals: measuring economic inequality, judging poverty, evaluating projects, analyzing unemployment, investigating the principles and implications of liberty and rights, assessing gender inequality, and so on. My work on inequality was much inspired and stimulated by that of Tony Atkinson. I also worked for a while with Partha Dasgupta and David Starrett on measuring inequality (after having worked with Dasgupta and Stephen Marglin on project evaluation), and later, more extensively, with Sudhir Anand and James Foster.  My own interests gradually shifted from the pure theory of social choice to more “practical” problems. But I could not have taken them on without having some confidence that the practical exercises to be undertaken were also foundationally secure (rather than implicitly harbouring incongruities and impossibilities that could be exposed on deeper analytical probing). The progress of the pure theory of social choice with an expanded informational base was, in this sense, quite crucial for my applied work as well.  In the reorientation of my research, I benefited greatly from discussions with my wife, Eva Colorni, with whom I lived from 1973 onwards. Her critical standards were extremely exacting, but she also wanted to encourage me to work on issues of practical moment. Her personal background involved a fine mixture of theory and practice, with an Italian Jewish father (Eugenio Colorni was an academic philosopher and a hero of the Italian resistance who was killed by the fascists in Rome shortly before the Americans got there), a Berlinite Jewish mother (Ursula Hirschman was herself a writer and the brother of the great development economist, Albert Hirschman), and a stepfather who as a statesman had been a prime mover in uniting Europe (Altiero Spinelli was the founder of the “European Federalist movement,” wrote its “Manifesto” from prison in 1941, and officially established the new movement, in the company of Eugenio Colorni, in Milan in 1943). Eva herself had studied law, philosophy and economics (in Pavia and in Delhi), and lectured at the City of London Polytechnic (now London Guildhall University). She was deeply humane (with a great passion for social justice) as well as fiercely rational (taking no theory for granted, subjecting each to reasoned assessment and scrutiny). She exercised a great influence on the standards and reach that I attempted to achieve in my work (often without adequate success).  Eva was very supportive of my attempt to use a broadened framework of social choice theory in a variety of applied problems: to assess poverty; to evaluate inequality; to clarify the nature of relative deprivation; to develop distribution-adjusted national income measures; to clarify the penalty of unemployment; to analyze violations of personal liberties and basic rights; and to characterize gender disparities and women’s relative disadvantage. The results were mostly published in journals in the 1970s and early 1980s, but gathered together in two collections of articles (Choice, Welfare and Measurement and Resources, Values and Development, published, respectively, in 1982 and 1984).  The work on gender inequality was initially confined to analyzing available statistics on the male-female differential in India (I had a joint paper with Jocelyn Kynch on “Indian Women: Well-being and Survival” in 1982), but gradually moved to international comparisons (Commodities and Capabilities, 1985) and also to some general theory (“Gender and Cooperative Conflict,” 1990). The theory drew both on empirical analysis of published statistics across the world, but also of data I freshly collected in India in the spring of 1983, in collaboration with Sunil Sengupta, comparing boys and girls from birth to age 5. (We weighed and studied every child in two largish villages in West Bengal; I developed some expertise in weighing protesting children, and felt quite proud of my accomplishment when, one day, my research assistant phoned me with a request to take over from her the job of weighing a child “who bites every hand within the reach of her teeth.” I developed some vanity in being able to meet the challenge at the “biting end” of social choice research.)  Poverty, famines and deprivation From the mid-1970s, I also started work on the causation and prevention of famines. This was initially done for the World Employment Programme of the [International Labour Organization](https://www.nobelprize.org/nobel_prizes/economics/laureates/1969/index.html), for which my 1981 book Poverty and Famines was written. (Louis Emmerij who led the programme took much personal interest in the work I was trying to do on famines.) I attempted to see famines as broad “economic” problems (concentrating on how people can buy food, or otherwise get entitled to it), rather than in terms of the grossly undifferentiated picture of aggregate food supply for the economy as a whole. The work was carried on later (from the middle of 1980s) under the auspices of the World Institute of Development Economics Research (WIDER) in Helsinki, which was imaginatively directed by Lal Jayawardena (an old friend who, as I noted earlier, had also been a contemporary of mine at Cambridge in the 1950s). Siddiq Osmani, my ex-student, ably led the programme on hunger and deprivation at WIDER. I also worked closely with Martha Nussbaum on the cultural side of the programme, during 1987-89.  By the mid-1980s, I was collaborating extensively with Jean Drèze, a young Belgian economist of extraordinary skill and remarkable dedication. My understanding of hunger and deprivation owes a great deal to his insights and investigations, and so does my recent work on development, which has been mostly done jointly with him. Indeed, my collaboration with Jean has been extremely fruitful for me, not only because I have learned so much from his, imaginative initiatives and insistent thoroughness, but also because it is hard to beat an arrangement for joint work whereby Jean does most of the work whereas I get a lot of the credit.  While these were intensely practical matters, I also got more and more involved in trying to understand the nature of individual advantage in terms of the substantive freedoms that different persons respectively enjoy, in the form of the capability to achieve valuable things. If my work in social choice theory was initially motivated by a desire to overcome Arrow’s pessimistic picture by going beyond his limited informational base, my work on social justice based on individual freedoms and capabilities was similarly motivated by an aspiration to learn from, but go beyond, John Rawls’s elegant theory of justice, through a broader use of available information. My intellectual life has been much influenced by the contributions as well as the wonderful helpfulness of both Arrow and Rawls.  Harvard and beyond In the late 1980s, I had reason to move again from where I was. My wife, Eva, developed a difficult kind of cancer (of the stomach), and died quite suddenly in 1985. We had young children (Indrani and Kabir – then 10 and 8 respectively), and I wanted to take them away to another country, where they would not miss their mother constantly. The liveliness of America appealed to us as an alternative location, and I took the children with me to “taste” the prospects in the American universities that made me an offer.  Indrani and Kabir rapidly became familiar with several campuses (Stanford, Berkeley, Yale, Princeton, Harvard, UCLA, University of Texas at Austin, among them), even though their knowledge of America outside academia remained rather limited. (They particularly enjoyed visiting their grand uncle and aunt, Albert and Sarah Hirschman, at the Institute for Advanced Study in Princeton; as a Trustee of the Institute, visits to Princeton were also very pleasurable occasions for me.) I guess I was, to some extent, imposing my preference for the academic climate on the children, by confining the choice to universities only, but I did not really know what else to do. However, I must confess that I worried a little when I overheard my son Kabir, then nine years old, responding to a friendly American’s question during a plane journey as to whether he knew Washington, D.C.. “Is that city,” I heard Kabir say, “closer to Palo Alto or to New Haven?”  We jointly chose Harvard, and it worked out extremely well. My colleagues in economics and philosophy were just superb, some of whom I knew well from earlier on (including John Rawls and Tim Scanlon in philosophy, and Zvi Griliches, Dale Jorgenson, Janos Kornai, Stephen Marglin in economics), but there were also others whom I came to know after arriving at Harvard. I greatly enjoyed teaching regular joint courses with Robert Nozick and Eric Maskin, and also on occasions, with John Rawls and Thomas Scanlon (in philosophy) and with Jerry Green, Stephen Marglin and David Bloom (in economics). I could learn also from academics in many other fields as well, not least at the Society of Fellows where I served as a Senior Fellow for nearly a decade. Also, I was again blessed with wonderful students in economics, philosophy, public health and government, who did excellent theses, including Andreas Papandreou (who moved with me from Oxford to Harvard, and did a major book on externality and the environment), Tony Laden (who, among many other things, clarified the game-theoretic structure of Rawlsian theory of justice), Stephan Klasen (whose work on gender inequality in survival is possibly the most definitive work in this area), Felicia Knaul (who worked on street children and the economic and social challenges they face), Jennifer Ruger (who substantially advance the understanding of health as a public policy concern), and indeed many others with whom I greatly enjoyed working.  The social choice problems that had bothered me earlier on were by now more analyzed and understood, and I did have, I thought, some understanding of the demands of fairness, liberty and equality. To get firmer understanding of all this, it was necessary to pursue further the search for an adequate characterization of individual advantage. This had been the subject of my Tanner Lectures on Human Values at Stanford in 1979 (published as a paper, “Equality of What?” in 1980) and in a more empirical form, in a second set of Tanner Lectures at Cambridge in 1985 (published in 1987 as a volume of essays, edited by Geoffrey Hawthorne, with contributions by Bernard Williams, Ravi Kanbur, John Muellbauer, and Keith Hart). The approach explored sees individual advantage not merely as opulence or utility, but primarily in terms of the lives people manage to live and the freedom they have to choose the kind of life they have reason to value. The basic idea here is to pay attention to the actual “capabilities” that people end up having. The capabilities depend both on our physical and mental characteristics as well as on social opportunities and influences (and can thus serve as the basis not only of assessment of personal advantage but also of efficiency and equity of social policies). I was trying to explore this approach since my Tanner Lectures in 1979; there was a reasonably ambitious attempt at linking theory to empirical exercises in my book Commodities and Capabilities, published in 1985. In my first few years at Harvard, I was much concerned with developing this perspective further.  The idea of capabilities has strong Aristotelian connections, which I came to understand more fully with the help of Martha Nussbaum, a scholar with a remarkably extensive command over classical philosophy as well as contemporary ethics and literary studies. I learned a great deal from her, and we also collaborated in a number of studies during 1987-89, including in a collection of essays that pursued this approach in terms of philosophical as well as economic reasoning (Quality of Life was published in 1993, but the essays were from a conference at WIDER in Helsinki in 1988).  During my Harvard years up to about 1991, I was much involved in analyzing the overall implications of this perspective on welfare economics and political philosophy (this is reported in my book, Inequality Reexamined, published in 1992). But it was also very nice to get involved in some new problems, including the characterization of rationality, the demands of objectivity, and the relation between facts and values. I used the old technique of offering courses on them (sometimes jointly with Robert Nozick) and through that learning as much as I taught. I started taking an interest also in health equity (and in public health in particular, in close collaboration with Sudhir Anand), a challenging field of application for concepts of equity and justice. Harvard’s ample strength in an immense variety of subjects gives one scope for much freedom in the choice of work and of colleagues to talk to, and the high quality of the students was a total delight as well. My work on inequality in terms of variables other than incomes was also helped by the collaboration of [Angus Deaton](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2015/deaton-facts.html) and James Foster.  It was during my early years at Harvard that my old friend, Mahbub ul Haq, who had been a fellow student at Cambridge (and along with his wife, Bani, a very old and close friend), returned back into my life in a big way. Mahbub’s professional life had taken him from Cambridge to Yale, then back to his native Pakistan, with intermediate years at the World Bank. In 1989 he was put in charge, by the United Nations Development Programme (UNDP), of the newly planned “Human Development Reports.” Mahbub insisted that I work with him to help develop a broader informational approach to the assessment of development. This I did with great delight, partly because of the exciting nature of the work, but also because of the opportunity of working closely with such an old and wonderful friend. Human Development Reports seem to have received a good deal of attention in international circles, and Mahbub was very successful in broadening the informational basis of the assessment of development. His sudden death in 1998 has robbed the world of one of the leading practical reasoners in the world of contemporary economics.  India and Bangladesh What about India? While I have worked abroad since 1971, I have constantly retained close connections with Indian universities, I have, of course, a special relation with Delhi University, where I have been an honorary professor since leaving my full-time job there in 1971, and I use this excuse to subject Delhi students to lectures whenever I get a chance. For various reasons – personal as well as academic – the peripatetic life seems to suit me, in this respect. After my student days in Cambridge in 1953-56, I guess I have never been away from India for more than six months at a time. This – combined with my remaining exclusively an Indian citizen – gives me, I think, some entitlement to speak on Indian public affairs, and this remains a constant involvement.  It is also very engaging – and a delight – to go back to Bangladesh as often as I can, which is not only my old home, but also where some of my closest friends and collaborators live and work. This includes Rehman Sobhan to whom I have been very close from my student days (he remains as sceptical of formal economics and its reach as he was in the early 1950s), and also Anisur Rehman (who is even more sceptical), Kamal Hossain, Jamal Islam, Mushairaf Hussain, among many others, who are all in Bangladesh.  When the Nobel award came my way, it also gave me an opportunity to do something immediate and practical about my old obsessions, including literacy, basic health care and gender equity, aimed specifically at India and Bangladesh. The Pratichi Trust, which I have set up with the help of some of the prize money, is, of course, a small effort compared with the magnitude of these problems. But it is nice to re-experience something of the old excitement of running evening schools, more than fifty years ago, in villages near Santiniketan.  From campus to campus As far as my principal location is concerned, now that my children have grown up, I could seize the opportunity to move back to my old Cambridge college, Trinity. I accepted the offer of becoming Master of the College from January 1998 (though I have not cut my connections with Harvard altogether). The reasoning was not independent of the fact that Trinity is not only my old college where my academic life really began, but it also happens to be next door to King’s, where my wife, Emma Rothschild, is a Fellow, and Director of the Centre for History and Economics. Her forthcoming book on Adam Smith also takes on the hard task of reinterpreting the European Enlightenment. It so happens that one principal character in this study is Condorcet, who was also one of the originators of social choice theory, which is very pleasing (and rather useful as well).  Emma too is a convinced academic (a historian and an economist), and both her parents had long connections with Cambridge and with the University. Between my four children, and the two of us, the universities that the Sen family has encountered include Calcutta University, Cambridge University, Jadavpur University, Delhi University, L.S.E., Oxford University, Harvard University, M.I.T., University of California at Berkeley, Stanford University, Cornell University, Smith College, Wesleyan University, among others. Perhaps one day we can jointly write an illustrated guide to the universities.  I end this essay where I began – at a university campus. It is not quite the same at 65 as it was at 5. But it is not so bad even at an older age (especially, as Maurice Chevalier has observed, “considering the alternative” ). Nor are university campuses quite as far removed from life as is often presumed. Robert Goheen has remarked, “if you feel that you have both feet planted on level ground, then the university has failed you.” Right on. But then who wants to be planted on ground? There are places to go. |
| Autobiographical |  |
| Podcast |  |
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| Biographical | I was born in New York, New York, on July 31, 1944, the middle child between two sisters, Stephanie and Vanessa. I grew up in Hastings-on-Hudson, a village of about 8000 outside the city, in a house that Vanessa and her family live in today. My father, born in Philadelphia the son of immigrant parents, was a professor of sociology at Columbia University. He is now University Professor Emeritus at Columbia, having meanwhile been awarded the National Medal of Science for founding the sociology of science and for his contributions to sociological knowledge such as the self-fulfilling prophecy and the focus group. My mother, from a multigenerational southern New Jersey Methodist/Quaker family, stayed at home. She died in 1992. My mother’s mother and many (at one time 25) cats completed the household that shared my childhood.  Hastings was a mixed middle-class and blue-collar town with local employment dominated by a wire and cable company and a chemical plant. Despite this composition and the town’s small size, the local public school provided a fine education opportunity. In a graduating class of only some 90, I nevertheless was able to take mathematics through the calculus and five years of science (two in physics including a MIT-designed course). I was a good student but not at the top of my class. I played varsity football and ran track, neither with great distinction. Among my classmates were the sons of the Columbia physicists and Nobel laureates, [James Rainwater](https://www.nobelprize.org/nobel_prizes/physics/laureates/1975/index.html) and [Jack Steinberger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1988/index.html). Other long-time Hastings residents were the laureate in medicine, [Max Theiler](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1951/index.html), and the laureate in economics, [William Vickrey](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1996/index.html), as well as the sculptor Jacques Lipchitz.  School work and intellectual interests such as music and the arts were not especially important to me while I was growing up, although mathematics, my favorite subject, was fun. Baseball was my first passion: I played sand lot and Little League, and rooted for the Brooklyn Dodgers. Around age 11, that passion began to turn toward cars. On my bedroom wall, I put a large sheet of paper with 1800-plus numbers: one to be crossed out each day until I would be old enough for my driver’s license. As I had known all the batting averages and pitching records of big league baseball players, so I came to know the horsepower, cubic inches of engines size, and other detailed specifications of just about every automobile in the post-war era. Going to auto shows and stock car races and handing tools to older, amateur buffs working on their cars were outlets for my passion until, at age 15, I bought and rebuilt my first car. After getting my driver’s license, I built street hot rods which I raced at drag strips in upstate New York and Long Island. I thought that I would become an automobile engineer when I grew up. Indeed, while in college, I spent two summers working for Ford in its headquarters in Dearborn, Michigan: one as an engineer in advanced vehicle design and the other in the Lincoln-Mercury division trying to figure out optimal importing patterns for the English Ford. Other than working in a local cemetery after school and in the summer during high school and a summer spent in information technology at IBM, these automotive jobs were the only full-time, non-academic ones I have had.  Both of my parents played important roles in my early life of learning. My father introduced me to baseball, poker, magic, and the stock market (only magic didn’t take root). And books of every kind were everywhere. He said nothing directly about expected academic performance. There was no need to. Simply by self-exemplification, he set the standards for work effort and for clarity of thought and expression. I had the normal father-son tensions as a teenager, but we subsequently became very close: for more than 30 years, we have talked to each other, at least once, nearly every day. My mother taught me caring and sensitivity towards the feelings of others, animals as well as humans. She gave me much good, practical advice for getting through life. One such counsel in particular I have applied often and in varied arenas: “First show them that you can do it their way, so that you earn the right to do it your way.”  One week before my 17th birthday, I had a blind date with June Rose, a television actress on network soap operas, a model, and a regular on the popular Dick Clark’s Saturday night American Bandstand show from New York. We were married five years later, one week after my graduation from Columbia. We devoted much energy to and derived enormous pleasure from raising three wonderful and talented children, Samantha J., Robert F., and Paul J. June and I separated in 1996.  My arrival at college marked the beginning of serious focus on academic matters. Just one day after entering Columbia College I switched to the Engineering School. With its small and flexible program and fine faculty, it was a great place for an undergraduate to explore mathematics and its uses. I took several undergraduate and graduate mathematics courses, both applied and pure: my tastes were however clear, preferring partial differential equations to real analysis and the calculus of variations to functions of a complex variable. Along with a number of engineering courses including drawing, I also took Columbia’s famous Contemporary Civilization course, humanities, one introductory course in economics (using Samuelson’s Economics), a graduate course in mathematical sociology, and two general studies night courses in accounting and stock market investments. The C- or D received in my English literature course in my sophomore year did not help my grade point average. The paper onGulliver’s Travels written for that course, however, became my first published article (in the Journal of the History of Ideas).  After Columbia, I went west to pursue a Ph.D. in applied mathematics at the California Institute of Technology. My time at Cal Tech (1966-67), brief as it was, added significantly to my stock of mathematics. Even more valuable to me was its creed of placing students from the outset in a research framework, “playing” with their subject instead of merely passively learning the material. Sometime during the year, I decided to leave Cal Tech (and mathematics) to study economics. Although he thought it was a bizarre idea, Gerald Whitham (the department head) provided generous help. I applied to half a dozen good departments, but only one, M.I.T., accepted me, and it gave me a full fellowship.  My decision to leave applied mathematics for economics was in part tied to the widely-held popular belief in the 1960s that macroeconomics had made fundamental inroads into controlling business cycles and stopping dysfunctional unemployment and inflation. Thus, I felt that working in economics could “really matter” and that potentially one could affect millions of people. I also believed that my mathematics and engineering training might give me some advantage in analyzing complex situations. Most important in my decision was the sense that I had a much better intuition and “feel” into economic matters than physical ones. Nowhere was that more apparent to me than in the stock market.  As early as 8 or 9 years of age, I developed an interest in money and finance, even at play. I created fictitious banks such as the RCM Savings of Dollars and Cents Company. I gladly balanced my mother’s check book. As already noted, my father introduced me to the stock market. At 10 or 11, I drew up an “A” list of stocks, and bought my first one, General Motors. In college, I spent time doing some trading, learning tape watching, and hearing the lore of the market from retail traders in brokerage houses. In late 1963, I had my first experience in what is now known as “risk arbitrage”. The trade surrounding the merger of Friden Company and Singer Company involved buying Friden shares and shorting Singer shares in a ratio of 1.75-to-1. The current difference in value between the two would become a “sure-thing” profit, provided the merger went through. Fortunately for me, it did. At Cal Tech, many mornings I would get to a local brokerage house at 6:30 am (9:30 am in New York) for the opening of the stock market, spend a couple of hours watching the tape and trading, and then go to my classes. In addition to stocks, I traded warrants, convertible bonds, and over-the-counter options. Although I did apply mathematical skills, my valuation approaches were essentially ad hoc. Nevertheless, I learned much from those varied transactional experiences about markets and institutions which proved useful in my later research. For instance, in discovering specialized banks that would legally finance my convertible bond positions at 85 percent of their value (leveraging terms considerably superior to the 50 percent financing offered by standard margin accounts), I learned early on that the “institutional rigidities,” often postulated as inviolate in academic financial-market modeling, can be more flexible and permeable in their real-world counterparts.  When I arrived at MIT in the fall of 1967, I discovered why they had admitted me when no other institution had: Harold Freeman, statistician and member of the economics department from pre-Samuelson days. Harold had recognized some of the mathematicians who had written my letters of recommendation and convinced the department to take a flyer. Now in the role of first-year advisor, he saw my proposed, “traditional” course plan and told me “…you follow that and you’ll leave here by the end of the term out of boredom … go take [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/index.html)‘s mathematical economics course.” I did. Not only did I get to interact with Paul Samuelson, but I met the then second-year students, Stanley Fischer and Michael Rothschild. I learned economics from Paul’s Foundations and wrote a term paper on an optimal growth model with endogenous population changes which was later published in 1969. As a result of our meeting in his course, Paul hired me as his research assistant that spring. Quite a yield from a single course!  In the course of my work for Paul, we discovered shared interests and some common knowledge about the stock market, warrants and convertible securities. I found out that my “after/before-hours” interest in such things could also be a legitimate part of my day-hours devoted to research. In the summer of 1968, we began a joint effort to advance Paul’s 1965 theory of warrant pricing, which was subsequently published in 1969. Later in October, I would have my first experience presenting in a formal seminar. My co-author decided that I, the second-year grad student, and not he, the Institute Professor, would give our paper at the inaugural session of the MIT-Harvard Mathematical Economics seminar. With a full audience of Harvard economics faculty including [Kenneth Arrow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1972/index.html), [Wassily Leontief](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1973/index.html), and Hendrik Houthakker, it was surely a memorable baptism.  The research with Paul on warrant pricing introduced me to the expected utility maxim and its application to optimal portfolio selection in a static framework. As a consequence of that effort, I began to think about combining the static theory of portfolio selection with the intertemporal optimization of lifetime consumption under certainty found in the growth-model literature. Ignorant of the important work underway by Nils Hakansson and Hayne Leland, then graduate students elsewhere, I attacked the problem of dynamic portfolio theory in a continuous-time framework without having the benefit of their discrete-time formulations. Despite all the mathematics courses that I had taken, l had seen neither stochastic dynamic programming nor the Ito calculus, both of which turned out to be key mathematical tools needed for this research. Instead, driven by “need,” I found them and learned them on my own. Presented first at a Harvard-MIT graduate student seminar in November 1968, my paper on lifetime consumption and portfolio selection under uncertainty was published the following August as a companion paper to one by Paul investigating the effect of age on portfolio risk tolerance.  Despite having Paul Samuelson, [Franco Modigliani](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1985/index.html), [Robert Solow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1987/index.html), Frank Fisher, Robert Bishop, Evsey Domar, Peter Diamond, Peter Temin, and Ed Kuh as teachers, I must confess that my focus was more on research than classes from the beginning (and my course grades reflected that). However, no one could have had a better introduction to economics than I did, with these great teachers and the opportunity to live in Paul Samuelson’s office as his assistant for two and a half years. Three of the five chapters of my thesis were published before the thesis was submitted. A fourth, “Optimum Consumption and Portfolio Rules in a Continuous-Time Model,” was presented at the Second World Congress of the Econometric Society in August 1970 and was published in 1971. During those most productive days of graduate study, I also developed much of my 1970 working paper on equilibrium asset pricing and the pricing of the capital structure of the firm which formed the core of my later papers on the intertemporal capital asset pricing model, the rational theory of option pricing, and the pricing of corporate debt.  Paul nominated me to be a Junior Fellow at Harvard. After being rejected, however, I had no choice but to get a job. I spent the fall and winter of 1969 interviewing only with departments of economics, but I ended up taking an appointment to teach finance at M.I.T.’s Sloan School of Management. It was Franco Modigliani, with a foot in both the department and Sloan, who made the invitation and who convinced me that I could teach there even though I had no formal training in finance. I had been a student of Franco’s and my research on optimal lifetime consumption and portfolio selection supported his Life-Cycle Hypothesis. Our relationship, however, became even stronger in the years after I joined Sloan. I was especially honored and greatly touched when he invited me to be the speaker at the traditional American Economics Association luncheon honoring him for receiving the 1985 Nobel Prize. That occasion (the remarks later published in Economic Perspectives) provided the opportunity to express, albeit inadequately, the deep respect and affection I hold for Franco. Some years later, he made a physically very demanding trip to Rome specifically to be the speaker at the National Academy of Lincei on the occasion celebrating my receiving the International INA-Accademia Nazionale dei Lincei Prize.  It was in the process of interviewing for the job at Sloan that I met Myron Scholes, a recent arrival to the faculty from the University of Chicago. As I note in the accompanying lecture, the story of my meeting Myron Scholes and Fischer Black and our subsequent interactions and collaborations are well described in Myron’s Nobel Lecture, in our joint tribute in memory of Fischer Black, in Fischer’s own writings, and in the book by Peter Bernstein. Thus, even with its obvious central importance to my professional life, both its academic and practitioner parts, there is no need to repeat it here.  When I joined the finance group at M.I.T., the faculty consisted of assistant professors Myron Scholes, Stewart Myers, and Gerry Pogue and senior professors Daniel Holland and Franco Modigliani. Dan specialized in tax matters and Franco was involved in many things everywhere. As a consequence, de facto, the junior faculty “ran” everything in the group with respect to both teaching and research. It was a wonderful environment of benign neglect in which all of us could grow. I enjoyed teaching from the start. My first experience was in 1969 as one of four graduate students (Karen Johnson, David Scheffman, and Jeremy Siegel were the others) who team-taught the second monetary course usually taught by Paul Samuelson. I began, however, full-time teaching at Sloan in the fall of 1970 with two regular courses in the Master’s degree program in management: general finance and advanced capital markets. In the basic course involving both capital markets and corporate finance, I taught [Markowitz](https://www.nobelprize.org/nobel_prizes/economics/laureates/1990/index.html)–[Tobin](https://www.nobelprize.org/nobel_prizes/economics/laureates/1981/index.html) portfolio theory, the [Sharpe](https://www.nobelprize.org/nobel_prizes/economics/laureates/1990/index.html)(-Lintner-Mossin) Capital Asset Pricing Model, and the Modigliani-[Miller](https://www.nobelprize.org/nobel_prizes/economics/laureates/1990/index.html) theorems, learning much of the material barely before I presented it to the students. For my own sense of security and to provide some evidence of preparation to the students, I gave them detailed (handwritten) teaching notes for each class. Lecturing from them saved me from having to write everything down on the blackboard and saved the students from having to copy what I would have otherwise written. This in turn left more time for in-class discussion. These notes became so popular that I prepared similar ones for every course I taught at Sloan, long after I had learned the subject matter. In the capital markets course that first year, I introduced intertemporal portfolio selection, Black-Scholes type option pricing and hedging and its application to the pricing of corporate liabilities. I did so not as an “outlet” for presenting my research interests but because I thought that the training would be quite useful in practice. I believed that teaching this material, even before it was published, was more important for the M.S. students than for the Ph.D. students since non-academic jobs were not likely to provide the same opportunity as academics to keep up with the research literature after graduation. At Sloan in those days, M.S. and Ph.D. students in finance took the same courses, and so both groups were exposed to this research long before its publication.  Throughout the 18 years I spent at the Sloan School, it was a stimulating and happy place to do research. I shall always owe a great debt to my brilliant colleagues there: Myron Scholes and Fischer Black, Franco Modigliani, Stewart Myers, John Cox, Chi-fu Huang, Terry Marsh, Richard Ruback, Douglas Breeden (unfortunately, only as a visitor), and from the Economics Department, Stanley Fischer and Paul Samuelson. The tenure and the tenor of the finance area varied considerably and it was small, rarely having more than a half dozen members at a time. But the quality of mind, diversity of thought, devotion to the subject of finance, and genuine affection for one another were reliable constants throughout those years.  I see my research interests as fitting into three regimes of roughly equal lengths across time: 1968 to 1977, 1977 to 1987, and 1988 to the present, with a reflective year 1987-1988. The first period was my most productive one for basic research, in terms of both the number of papers produced and the originality and significance of contribution. The central modeling theme was continuous-time stochastic processes with continuous-decision-making by agents. Locating this modeling approach within mathematical economics, I see my models falling in the middle range between simple models (e.g., one or two-periods with a representative agent) designed to give insights (associated by some with the “M.I.T. School”) and full general equilibrium models on a grand scale involving an arbitrary number of agents with general preferences and production technologies (often associated with the “Berkeley School”). Compared with discrete-time dynamic formulations, the continuous-time models are mathematically more complex. But by explicitly setting the trading interval and modeling the evolution of the system as diffusion processes, the derived results of the continuous-time models were often more precise and easier to interpret than their discrete-time counterparts. As a consequence, these theoretical models combined enough richness and tractability to be applied normatively for decisions in practice and positively in empirical tests. Of course, all models involve abstractions from complex reality. I see the relative importance of my contribution to be more in selecting “good” abstractions than in introducing and applying mathematical power.  My first decade’s research focus on developing dynamic models of optimal lifetime consumption and portfolio selection, equilibrium asset pricing, and contingent-claim pricing shifted in the 1978-1987 period to applications of those models. A series of papers examined the various risk-bearing roles of pay-as-you-go and funded social security and defined-benefit, defined-contribution, and integrated private-sector pension plans. I also applied option-pricing theory to the valuation of deposit insurance, market-timing information, corporate investment decisions, and implicit labor contracts. I worked on models for estimating expected returns on the market portfolio, for fitting dividend and earnings behavior, and for testing of investor market-timing skills. A number of papers including my Presidential address to the American Finance Association examined the rationality of capital-market prices and the effects of market imperfections on equilibrium asset prices. With Stanley Fischer, I also wrote on issues common to macroeconomics and finance. Others whom I was fortunate to publish with during this period were Zvi Bodie, Mathew Gladstein, Roy Henriksson, Alan Marcus, Terry Marsh, Scott Mason and, for the first time, Myron Scholes. In contrast, during the prior decade, I had had only two co-authors: (three papers with) Paul Samuelson and (one with) Marti Subrahmanyam.  In 1987, I took my first-ever sabbatical year to write a book based on my work in continuous-time finance. Peter Dougherty, then an editor at Basil Blackwell, had suggested a book nearly a year before that would use my previously published papers as the core. As it happened, I had been thinking about putting my ideas on the subject together and Peter’s expressed interest served as a catalyst. M.I.T. graciously provided my full salary for the year and the Harvard Business School kindly offered an office in which to work. It was a most enjoyable and productive time. With no other commitments, I wrote nearly every day with no limit on length and no set deadline for either any piece or the whole. Earlier writings were corrected and, in some cases, significantly expanded. Five new chapters were created incorporating my cumulative thoughts in the fields of optimal portfolio selection, option pricing, financial intermediation, and general equilibrium theory. I even took pleasure in developing my own extensive index and bibliography.  This reflective year was a watershed, both for my research and for where it would take place. In effect, Continuous-Time Finance was the crowning synthesis of my earlier work. Its Chapter 14 on intermediation and institutions, however, represented a bridge to a new direction of my research. From that time until the present, I have focused on understanding the financial system with special emphasis on the dynamics of institutional change. In particular, I am studying the role of financial technology and innovation in driving changes in financial institution and market design, the management of financial-service firms, and the regulatory and the accounting systems. There is, however, continuity of this line of inquiry with the past: Fischer’s, Myron’s and my derivative-security research provided much of the foundation for the contracting and security-design technology that is central to the extraordinary wave of real-world financial innovation of the past two decades.  My decision to move from M.I.T. to the Harvard Business School in 1988 was significantly influenced by this turn in my research interests. Although it was a difficult decision to make, I have never since doubted that it was the right one. Both the institution and my colleagues have treated me in extraordinary fashion. A prime exemplifying case: shortly after my joining the HBS faculty, then-Dean John McArthur resigned from the George Fisher Baker professorship in order to give it to me. Giving the name chair of the founding-sponsor of the School-known as the “Dean’s Chair” since the beginning–to a newly arrived professor of mathematical finance who had not yet taught a single HBS student, was a towering symbol and statement of confidence and support. That recognition meant all the more to me because it was given after I had happily accepted the School’s offer and this timing made it altogether clear that it was not part of any negotiation. Now, a decade later, I am especially pleased to become the first John and Natty McArthur University Professor and to have the Baker chair become once again the “Dean’s Chair.”  For nearly a decade, I have enjoyed developing the new work on the financial system: to begin with, on my own, but then quite soon after, in a delightful, productive and multi-faceted collaboration with Zvi Bodie, professor of finance at Boston University, whom I have known since the early 1970s when he was a student in the M.I.T. department of economics. Together we have developed the idea of using a “functional” perspective to analyze and to predict financial institutional change over time and to provide a better understanding of contemporaneous institutional differences across geopolitical borders. Zvi and I have refined and applied this idea in a series of working papers, published articles, and book chapters. In 1992, Zvi and my HBS colleague and my former MIT student, Carliss Baldwin, led the way to the creation of the Global Financial System project at the Harvard Business School. The project which involves several of my finance colleagues (and Zvi) working together with senior management from 15 major global financial-service firms has considerably expanded the research effort devoted to applying the functional approach to the financial system and to the management of financial institutions. Whether published as jointly or singly authored, my work with Zvi in this area has always been collaborative. Conceived at the outset as a parallel development to our research, but completed only now, is our textbook on basic finance that applies this perspective and presents the subject as a set of principles much like first-courses in economics and the physical sciences.  Throughout the last 30 years of academic research, I have been involved in finance practice. The vast bulk of my research has been in mathematical finance theory, but I believe that my involvement in practice has shaped that research and in turn has been shaped by it, this interplay to the benefit of both. A targeted instance can be found in the section of my 1973 rational option pricing paper on pricing and hedging the risk of the “down-and-out” call option. I became aware of such instruments in the early 1970s only as a consequence of a consulting assignment from a firm that was issuing them in Asia. In the decades since, the down-and-out option has become the prototype example of the exotic option, which is now a large, mainstream class of financial-product offerings.  My first consulting experience was in 1969 for a southern California bank on the pricing of warrants. Ironically, had the “equal-yield-for-equal sigmarisk” model I developed ad hoc for them been taken to its continuous-trading limit, it would have led to the Black-Scholes pricing formula but of course without any of the rigorous foundation underlying that formula which includes the key hedging insight of Black and Scholes. Myron Scholes and I began working together on consulting projects shortly after I joined the Sloan faculty. In 1972, we were engaged by Mathew Gladstein of the options department of Donaldson, Lufkin, & Jenrette to develop option pricing and hedging models for the over-the-counter market and later for the new Chicago Board Options Exchange. In 1973, Leo Pomerance, head of the DLJ options department, became the first chairman of the CBOE. Myron and I learned much from our DLJ experience and indeed, our first publications together in 1978 and 1982 evolved directly from a mutual-fund project which DLJ helped us to underwrite.  As a consequence of the extraordinary decline in the stock market in 1973-74, Myron and I had an idea for a mutual-fund product that would provide significant downside protection to the investor while at the same time affording significant exposure to upside movements in the stock market. The strategy designed to do this was to purchase a diversified portfolio of call options with 10 percent of the assets and to invest the balance of the assets in short-term money-market instruments, with those proportions rebalanced every 6 months. Since the strategy involved option buying instead of option writing, many cautioned us that the SEC might see it as “too speculative” for the first option-based mutual fund in the United States and thus, would create serious roadblocks. They were wrong: the staff of the SEC apparently understood the fund’s relatively conservative design and approved it with essentially no delays. The strategy was thus implemented with the creation of Money Market/Options Investment, Inc., an open-ended mutual fund, which went effective in February 1976. The portfolio’s subsequent performance fit the simulations projected for it. Nevertheless, and despite being a direct predecessor to the portfolio-insurance products of the 1980s and the various successful “floor” products offered around the globe in the 1990s, MM/OI was not a commercial success. Still, it was for us a broadening experience about the multi-dimensional aspects of setting up a new financial entity. As Myron is fond of observing, “Sometimes the early bird gets the worm … and sometimes, it gets frozen.”  For the rest of the 1970s and much of 1980s, I kept my hand in practice, serving on a few mutual fund boards and being elected a trustee of College Retirement Equities Fund. During the early 1980s, several of my former students from M.I.T., most of them Ph.D.s, were attracted to Salomon Brothers, the global investment bank, by John Meriwether and under his leadership, they helped build an enormously successful proprietary trading group focused on arbitrage in the fixed-income markets. The core financial technology used in the group was based on the Black-Scholes-Merton derivative-security models, but it was highly refined to take account of much greater empirical detail and practical market experience. In 1988, one of those former students, Eric Rosenfeld, and Thomas Strauss, then President of Salomon, came to see me about becoming a special consultant to the Office of the Chairman. They made me an offer I couldn’t refuse: unlike the simple model-building/product-design role of my past consultancies, this one also called for a role as trusted advisor (with technical skills) to the CEO on business matters of the firm and on the direction of institutional change in the global financial system. One could hardly imagine a better fit to inform, and to be informed by, my then-new direction of research on the functional perspective. Over the next four years, I learned much about the operations and management of a global intermediary, and trust that I contributed something as well. I definitely strengthened old friendships with former students and developed new friendships with their colleagues, and Myron and I got back together in practice when he joined Salomon from Stanford.  In early 1993, John Meriwether who had left Salomon in 1991, Eric Rosenfeld who had recently left and James McEntee, former chairman and cofounder of Carroll-McEntee, the primary government-bond dealer, and John’s long-time friend, had the idea of building a new firm to undertake fixed-income arbitrage on a global basis. The thought of working with John and Eric once again and having a hand in building a large-scale financial firm from scratch was exciting and I immediately volunteered to help out.  Over the ensuing months, one at a time, senior members of John’s old group left Salomon and showed up ready to get involved in what became Long-Term Capital Management (LTCM): Victor Haghani arrived in the spring; Gregory Hawkins and Myron Scholes (bringing us back together in practice once again) in the summer; William Krasker in the fall; and Richard Leahy and Larry Hilibrand at year end. The last of the founders, David Mullins, who joined LTCM in late winter, never worked at Salomon. With a Ph.D. from M.I.T. and 15 years on the finance faculty at Harvard Business School before going into government service, he exemplifies the strong M.I.T. and HBS connections among the founders of LTCM: seven of the eleven founders (and nine of the current sixteen principals of the firm) were either graduates, or on their faculties, or both.  This small group of founding principals, together with a few key early employees, put together and tested the financial, telecommunication, and computer technologies, hired the strategists and operations people to run them, designed the organizational structure of the business, executed the complex contractual agreements with investors and counterparties, found and outfitted physical quarters in both the United States and London, and helped to raise over $1 billion from investors. The design and development efforts along each of these dimensions attempted to marry the best of finance theory with the best of finance practice. It all came together in February 1994 when the firm began active business. Today, LTCM has 180 employees, a third office in Tokyo, and its capital has grown considerably.  It was deliciously intense and exciting to have been a part of creating LTCM. For making it possible, I will never be able to adequately express my indebtedness to my extraordinarily talented LTCM colleagues.  The distinctive LTCM experience from the beginning to the present characterizes the theme of the productive interaction of finance theory and finance practice. Indeed, in a twist on the more familiar version of that theme, the major investment magazine, Institutional Investor characterized the remarkable collection of people at LTCM as “The best finance faculty in the world. “  In long retrospect, unexpected roads happily traveled.   |  |  | | --- | --- | | Personal | | | Born: | July 31, 1944  New York, New York | | Address: | Harvard Business School  Morgan Hall 397  Soldiers Field  Boston, MA 02163 | |  |  | | Education | | | B.S., Columbia University (Engineering Mathematics), 1966 | | | M.S., California Institute of Technology (Applied Mathematics), 1967 | | | Ph.D., Massachusetts Institute of Technology (Economics), 1970 | | |  | | | Honorary Degrees | | | Masters of Arts, Harvard University, 1989 | | | Doctor of Laws, University of Chicago, 1991 | | | Prof esseur Honoris Causa, Hautes Etudes Commerciales (Paris), 1995 | | | Doctoris Honoris Causa, University of Lausanne, 1996 | | | Doctoris Honoris Causa, University Paris-Dauphine, 1997 | | | Honorary Doctor, National Sun Yat-sen University, 1998 | | |  | | | Academic Appointments | | | John and Natty McArthur University Professor, Graduate School of Business Administration, Harvard University, 1998- | | | George Fisher Baker Professor of Business Administration, Graduate School of Business Administration, Harvard University, 1988-1998 | | | Invited Professor of Finance, Faculté des Sciences Economiques, Université de Nantes, June 1993 | | | Visiting Professor of Finance, Graduate School of Business Administration, Harvard University, 1987-1988 | | | J. C. Penney Professor of Management, A. P. Sloan School of Management, Massachusetts Institute of Technology, 1980-1988 | | | Assistant Professor of Finance, 1970-73, Associate Professor, 1973-74; | | | Professor 1974-80, A.P. Sloan School of Management, Massachusetts Institute of Technology | | | Instructor, Department of Economics, Massachusetts Institute of Technologv, 1969-1970 | | | Research Assistant to Paul Samuelson, Massachusetts Institute of Technology, 1968-1970 | | |  | | | Other Professional Appointments | | | Principal, co-founder, Limited Partner, Long-Term Capital Management, L.P. (1993-) | | | Research Associate, National Bureau of Economic Research, 1979- | | | Trustee, College Retirement Equities Fund (1988-1996) | | | Director, Travelers Investment Management Company (1987-1991) | | | Director, ABT Investment Series (1983-1988) | | | Director, ABT Utility Income Fund (1982-1988) | | | Trustee, ABT Growth and Income Trust (1982-1988) | | | Director, Nova Fund (1980-1988) | | |  | | | Elected Societies and Positions | | | Member, Tau Beta Pi, Columbia University, 1965 | | | Member, Sigma Xi, Massachusetts Institute of Technology, 1970 | | | Director, American Finance Association, 1982-84; 1987-88 | | | Fellow, Econometric Society, 1983 | | | Fellow, American Academy of Arts and Sciences, 1986 | | | President, American Finance Association, 1986 | | | Vice President, The Society for Financial Studies, 1993-96 | | | Member, National Academy of Sciences, 1993 | | | Senior Fellow, International Association of Financial Engineers, 1994 | | | Fellow, Institute for Quantitative Research in Finance (“Q Group”), 1997 | | | Honorary Member, the Bachelier Finance Society, 1997 | | |  | | | Awards | | | 1964 | Faculty Scholar Award, Columbia University | | 1971-72 | Salgo-Noren Award for Excellence in Teaching, Massachuset Institute of Technology | | 1977-78 | Graduate Student Council Teaching Award, Massachuset Institute of Technology | | 1983 | Leo Melamed Prize, University of Chicago | | 1985, 1986 | First Prize, Roger Murray Prize Competition, Institute for Quantitative Research in Finance | | 1989 | Distinguished Scholar Award, Eastern Finance Association | | 1993 | International INA – Accademia Nazionale dei Lincei Prize National Academy of Lincei, Rome | | 1993 | FORCE Award for Financial Innovation, Fuqua School of Business, Duke University | | 1993 | Financial Engineer of the Year Award, International Association of Financial Engineers | | 1997 | The Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel | | 1998 | Inducted, Derivatives Hall of Fame, Derivatives Strategy | |  |  | | Selected Lectures | | | 1975 | Distinguished Speaker Lecture, Western Finance Association | | 1985 | Mortimer Hess Memorial Lecture, Association of the Bar of the City of New York | | 1988 | 12th Annual Lecture, Geneva Association, Paris | | 1992 | Scholl Chair in Finance Distinguished Speaker Lecture, DePaul University | | 1993 | Lecture, Discussion Meeting on Mathematical Finance, The Royal Society, London | | 1993 | Keynote, 10th International Conference in Finance, Association Française de France | | 1994 | AEA/AFA Speaker, Allied Social Sciences Meetings | | 1994 | Speaker, International Monetary Conference, London | | 1995 | Lecture, Newton Institute Seminar, Isaac Newton Institute for Mathematical Sciences, Cambridge | | 1995 | Keynote, 12th International Conference in Finance, Association Française de France | | 1995 | Keynote, 25th Anniversary, Financial Management Association | | 1996 | Oxford University Press and Massachusetts Institute of Technology, Sloan School of Management Distinguished Lectures in Business, Massachusetts Institute of Technology, Cambridge | | 1996 | Donor’s Lecture, London Business School, London | | 1996 | Inaugural, Dean’s Research Seminar, Harvard Business School | | 1996 | Faculty Inaugural Session, University of Lausanne | | 1996 | Paolo Baffi Lecture on Money and Finance, Bank of Italy, Rome | | 1997 | Edgar Lorch Memorial Lecture, Sigma Xi, Columbia University | | 1998 | Lionel McKenzie Lecture, University of Rochester | | 1998 | Martin H. Crego Lecture, Vassar College | | 1998 | I.E. Block Community Lecture, Society for Industrial and Applied Mathematics | |  |  | | Advisory and Editorial Boards | | | Current: | | | International Board of Scientific Advisers, Tinbergen Institute (1995-) | | | Advisory Board, Brookings-Wharton (1997-) | | | Advisory Board, International Journal of Theoretical & Applied Finance (1997-) | | | Advisory Board, Center for Global Management and Research, George Washington University (1996-) | | | Advisory Board, European Finance Review (1997-) | | | Advisory Board, Journal of Financial Education (1995-) | | | Advisory Board, Review of Derivatives Research (1993-) | | | Advisory Board, Japan Financial Economics Association (1993-) | | | Advisory Board, Mathematical Finance (1989-) | | | Editorial Board, Finance India (1988-) | | | Associate Editor, Journal of Fixed Income (1991-) | | | Associate Editor, Journal of Banking and Finance (1977-1979, 1992-) | | |  | | | Past: | | | Advisory Board, The New Palgrave Dictionary of Money and Finance (1989-1992) | | | Selection Editor, Papers and Proceedings, Journal of Finance, July 1986 | | | Co-Editor, Journal of Financial Economics (1974-1977) | | | Associate Editor, Financial Review (1992-1997) | | | Associate Editor, Geneva Papers on Risk and Insurance (1989-1996) | | | Associate Editor, Journal of Financial Economics (1977-1983) | | | Associate Editor, Journal of Money, Credit and Banking (1974-1979) | | | Associate Editor, Journal of Finance (1973-1977) | | | Associate Editor, International Economic Review (1972-1977) | | | Founding Committee, Review of Financial Studies (1986) | | |  | | | Publications, Cases and Unpublished Papers | | | Books | | | Finance, with Zvi Bodie, New Jersey: Prentice-Hall, 1998. | | | The Global Financial System: A Functional Perspective, with D. 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| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0854 |
| Interview |  |
| Q18 | Professor Robert Merton, very welcome to this interview. It’s a pleasure to have you with us. Today during your speech you outlined something that I found very interesting. You were saying that households and individuals have had, over the last couple of years, to take much more serious economic decisions about how their, for example, pensions would look like in the future and so on. Would you please tell us a little bit more about what the consequences have been and what consequences you foresee in the future, for example for households and financial institutions? |
|  | The trend has been, starting now in the Anglo-Saxon world but moving really to the rest of the world, as the ageing has taken place and retirement and pensions and so forth have become a much bigger issue. Recognizing that the structures that are used in the past, either from the Government or from corporations, of funding pensions in particular, have been just too costly. It’s not that there’s anything wrong with them, it’s just it wasn’t recognized how expensive they were. So there’s been a move really over the last I would say 20 years of putting more and more responsibility on households to make major financial decisions; decisions that they hadn’t had to make in the past. Decisions that they’re not equipped to make at the moment, and with all respect for most households, I don’t think they could be expected to become educated to make them in the future. Very much like you wouldn’t ask people to perform their own medical services.  You know you can’t get something from nothing …  And as a consequence of that I see a shift, a trend, that’s going to, I think, come, in which you’re going to move from this sort of separate pieces, that everybody is asked to assemble their own financial plan or financial coverage, to more integrated solutions that are simpler for us to understand and do what we really want as individuals to do that. Now in particular, when you see the baby boomers coming to retirement, they’re going to discover, many of them, that they do not have, with the combination of social security and their private savings, they will not have adequate savings in order to generate an annuity, which is what will look after them in retirement, adjusted for inflation, so that they don’t have to give up their lifestyle as they age, that would be adequate to cover that. You know you can’t get something from nothing.  But one /- – -/ use of modern financial technology would be to develop instruments that get the most of what people do have for them. And one such instrument would be reverse mortgages. What that essentially allows the owner, who has accumulated wealth in their house, but wants to keep to live in as part of their living style, an opportunity to in effect be able to continue to live in their house throughout their retirement until their death, with no fear of being evicted because they couldn’t repay the loans, but to be able to borrow out a substantial part of the value of the house and use that to purchase additional annuities so that they could live at a higher level. That’s one such example.  Another would be combining treatment for long-term care and a life annuity in one package. And it just turns out, without getting too technical about it, that if someone has the need for long-term care, they’re less likely to live as long, so by providing both contracts the provider, the insurance company or the corporate provider, can reduce its risk and therefore charge a lower fee than if they were sold separately. So we keep looking for use of financial innovations as a way to address in this case the retirement part of the lifecycle, but really to apply it throughout the lifecycle that all people in all places go through. |
| Q18 | Could this be a way of reducing the risk in a way? I mean people have been quite upset, if you look at Sweden, for example. We’re only now starting to realize that a large group of us will not have a pension that we thought we would have, because things have shifted and we were forced or made to believe that we had to make certain decisions. Do you think the risk for the individual will be less? |
|  | My hope, what I’m trying to build, so of course it’s not finished yet, but which I think is quite feasible within the technology, is to develop instruments, tools, programmes and contracts that will make it possible for individuals to have a relatively risk-free economic retirement.  Does it mean that financial institutions need to become more risk-prone or more ethical about the way they do business, for example? You know who’s going to change here?  Robert Merton: I think part of it is the development of technology and more efficient allocation of resources, which doesn’t require beyond that that either party gives something up. I think on the case of the institutions – they will have to work harder. They will either have to be more creative or they’ll have to be at more risk or find ways to get risk-borne. From an economist’s point of view that makes sense. It’s much more sensible to expect institutions to find a more efficient way to distribute risk than to impose it on the individual person. So, I think we can make definite improvements there by moving in that direction. The ethical nature of things of course in every field and every profession sadly there aren’t ethical things. But I think for the most part people who work in the financial services industry are really trying to do a good job and do the right thing.  You made a likeness between a car manufacturer, and you know you just can’t have a car which has one door and looks the same color. So the financial institutions are going to have to be more, as you said, more innovative.  Robert Merton: Yes, as it is now the institutions, by leaving the risk and the decision on the individual household, make life simpler for themselves, because the household’s taking much more of the risk and they don’t have to make the kinds of commitments. It opens it up, therefore, for many more people to be advisors on finance, because you don’t really have to take that commitment. It is clearly more complex for the producer, so there’s a kind of paradox. The easier, simpler to understand and more comfortable you make it for your customer, the more complex you make it for yourself.  And in the case of an automobile analogy, you know it’s easier to make one door on a car rather than four, and if you produce all cars the same color you don’t have to have as complex a painting choice or anything. But of course that doesn’t take account of the needs of the actual customer. And in this industry, as well as the auto industry and the other industries, I think in the end they will find competitively they will have to do what’s right for the customer. So, there is greater complexity, but I think on the whole it’s a more efficient and effective system, and I really am quite hopeful that significant improvements will be made. And I wouldn’t leave this by saying that over the last 20 years in many ways there have been very great improvements in what is offered in the financial services. It’s just that there’s been this underlying trend of putting more responsibility on the household for things that they really aren’t in a position to make good decisions about. |
| Q37 | I just want to go back a little bit in time and ask you something about your childhood, because you said you were looking forward all the time. When did you realize that you had a very good sense for economy and finance and, you know, the maths around it? Did you know that already as a child? I read somewhere that you liked baseball and you didn’t really care that much for maths? |
|  | Well, I liked math as my academic subjects. But you’re quite right, I think as a child I did the minimum amount of work I had to do in school; with the exception really of math – I enjoyed really. I liked baseball and other things. At the same time, when I was a pretty young age, I used to create fictitious companies, including a fictitious bank. I think it was called the RCM Dollars and Savings Company, of which I went and tried to get deposits and make investments. And at a relatively early age, ten or so, I invested my first share of stock. And I used to follow, look at companies and so forth. But throughout the whole period, and indeed right through my college years, while I was involved in the stock market, always interested in finance, I never thought of it as a full-time job. You know I always thought that was something for after hours. And it was really fairly late in my graduate study that I decided actually to move into economics.  I read somewhere that you said you wanted to do something that really mattered?  Robert Merton: That was a big factor in my decision. As an undergraduate at Columbia, I went to the engineering school, I had a great deal of training in engineering and mathematics as well as subdiversified training. And then I went to the California Institute of Technology to do my PhD in applied math. And I went there and I took all the courses I needed and passed my qualifying exams. So I was really making a decision about doing research for my dissertations. So I finally came to the point where I really had to focus on what I was going to do. And I found myself … two things happening: One: I was following the stock market and doing things before hours – not interfering with my studies but doing that.  And the other is I started to look at the range of problems that people were working on, using mathematics and the applied fields, and it ranged from plasma physics problems to water waves in a tank and fluid mechanics. And none of those really excited me too much. This was also in 1967, so middle-late part of the ’60s. And we had had the view, at least we – I was not in a congress at the time – that we had solved macroeconomic problems; that we had the Council of Economic Advisors and under Kennedy and then Johnson there was a sense that we really had solved macro problems. And I looked at that and said, you know, “If you could do something in the field of economics,” which intrigued me anyway, “you might have an influence, even if you did a little something, on an awful lot of people.” And that’s exciting. And so there was a combination of a sense that you really could do something, and then my idea that I sort of had a feel for the stock markets and things of that sort – economics, I had this mathematics training.  … there was a bit of serendipity or a bit of luck, I guess you would put it …  And then I’ll confess there was a bit of serendipity or a bit of luck, I guess you would put it, I read a book on mathematical economics. I can’t remember the author and as you’ll see it’s good that I can’t. And I decided it was so terrible that I thought even I, if I went into this field with my mathematics, could probably do something. Had I read [Robert Solow](https://www.nobelprize.org/prizes/economic-sciences/1987/solow/facts/)‘s or [Paul Samuelson](https://www.nobelprize.org/prizes/economic-sciences/1970/samuelson/facts/)‘s mathematical works I might never have come into the field. So, it was a combination of all that that I made this complete break where I left Caltech, I applied to a number of economics departments, who of course I had no economics. All but one turned me down. The one that accepted me was MIT, which most would say was the best department in the world at that time. And they gave me a full fellowship so my decision was easy. And then I went to MIT. |
| Q3 | Did your parents influence you? I know that your father is a highly acclaimed academic and I believe your mother also is in the academic field. In which way did they influence you? I’m sure in some way or another. |
|  | Well of course. I mean as we all have happen, being in a household with 10,000 books and things going on you absorb things that are not … But I think in the case of my father, in terms of the things that influenced me, he never pressed me to go into academics or pressed me to go to a field, and indeed my behavior was largely to move as far the other direction. I don’t think that’s uncommon with people with very successful parents. And so for a long time I think what I really absorbed from him was a sense of a work ethic; a sense that if you are going to do something it should be of top quality. And of course he being in a major academic environment, he himself being very eminent in a field, it just seemed standards that were set. I mean I was very comfortable. I met Nobel Laureates and their equivalents in the other fields and just, you know, listened. So there was that as background.  In my mother’s case she taught me something, and one thing I often repeat to people is she gave me the dictum that I should do it their way, whoever they are who set standards, and do it so well that there’s no question that I could do exactly what they were asking. And then once I did that, I had the right to do it my way. And I, to some degree, followed that for a good part of my life. But of course later my mother died, about 15 years ago, my father died last year, but my father and I were, you might say, best friends for the last 40 years. So we interacted; almost did some joint research.  He must have been very proud when you were given the prize?  Robert Merton: Oh yes. And he was very happy. In fact he came to Stockholm with all of the rest of my family, but he was my significant other at the table. And he was very happy and he always signed his letters, even before the prize, as the ‘father of the economist’. I would sign as the ‘son of the sociologist’. |
| Q7 | How did you react when you got a phone call? |
|  | That’s a question there. I was actually leaving my apartment for an early flight to New York and the phone rang. And as we often do, the first moment was, “I better not answer it because I’ll be late for the plane.” Then I said, “Oh, I’d better answer it, you know, say I’m rushing.” Well of course when the phone came and they said who they were and they said they had some interesting news for me I never left my apartment, I missed the plane. But after getting it I guess I just sat down. The phone started ringing and I quickly stopped answering. And I started thinking about it and it was just, you know, unbelievable. I guess the only feeling that came to mind was I said, “Well now I’m going to have to be a little more careful what I say, because I go downstairs now and whatever I say will be all over the world. And if it’s something really dumb it’s going to really reflect not on me only but also on my institution. It’s going to cost the Nobel Foundation.” And the committee had made their decision. So I felt a little more responsible of what I had to give when I said things.  Did you restrict yourself?  Robert Merton: Oh, not really. It was the thought, but no, I think I felt a certain additional responsibility – it also created some – because people might actually listen. |
| Q18 | The fields of economics is often to people from the outside very theoretical, but really it’s about daily life. How is it possible to translate theory into daily life and explain it? You know it’s so much part of our lives. How do you go about it when you meet people who might not have any knowledge when you explain a theoretical problem? |
|  | Well, as in every field there are some problems that only someone in the field can find interest in. They really are very technical. However, at the main stream of things, I think one should really be able to explain what’s interesting and what’s important about just about every aspect of economics. The trick is to find out the background of the person you’re talking to and try to find a frame of reference. But if you really do understand the problems and what they’re about I think there’s no reason at all that you can’t explain. At least the substance of it and why it’s important. The execution may be more difficult to explain. |
| Q14 | Has it helped you to move in these two worlds? I mean, you’ve done both academic work and you’ve done a lot of more practical work one could say as well, as a consultant, for example. How important is that, to be able to move between these two? |
|  | It turns out from a professional and a personal point of view it’s been very important. As it happens, although I was at MIT on the faculty full-time for 18 years and then at Harvard for another 16, so I’ve always been in full-time an academia, I always found it was both beneficial for my research and beneficial for the other work to be involved in the practicing community. And I did some serious consulting when I was still a graduate student, but certainly within a year or two of my becoming a faculty member I became involved in practicing a very material way. And I could show in fact in the paper that was cited for the prize on option pricing, in that paper is a section I did which was an application of the technology we had developed to a thing called the “down-and-out” option, which was pretty esoteric. But it’s turned out subsequently to be the prototype for a whole industry called exotic options, which are used all over the world and have been.  I wouldn’t have known of the existence of that contract to put in my research paper, let alone solve it, how to price it and so forth, if I hadn’t been involved in practice. So there really has been a feedback that, as a theorist the art of the science is making judgments about the abstraction from complex realities. When you build a model the world looks very complicated, so you have to simplify. And it’s the art of how you simplify which I think, to a large extent, influences how people see your models and what you have developed. And I felt the confidence in my abstractions that – in the area I was working – that as a consequence of my being involved in practice I felt the confidence of being able to say, “This abstraction’s okay. This is close enough to reality, this is the way it really happens. This isn’t.” And that, in no one specific case but as building the whole mosaic of models that you build and how you explain them and what’s an interesting problem, I felt very influenced by having the practice.  On the more personal level it was stimulating to be working with world-class minds in academia; MIT, Harvard, you know, University of Chicago and so forth. And then be able to in some sense change clothes and go from Cambridge to New York, which is not a long distance in space, and be involved with very intelligent, interesting and motivated and talented people of a very different sort. And the combination of that just from a personal point of view is quite stimulating and still is. |
| Q13 | Well, I have to confess to a bias at my part of economics, financial economics, I think this has, in the past, one of the great excitements about it is that doing the most complex mathematical theories, and empirical work, you could take that same work and it’s turned out to have direct implications in practice. Not just inside. Direct implications in practice. So you could have sort of both. And that isn’t traditional within economics actually. But it’s true in finance or has been. Looking forward, I see the opportunities with that background, financial economics, to expand and grow what you can do with that. I think that the tools of financial economics are going to be more widely used in what are traditional macroeconomic fields, monetary theory, fiscal policies. If you want to end up running a central bank you’d better understand finance, you’d better understand the contracts. It’s as important as learning about interest rates and the more traditional macro.  The opportunities: finance deals with risk. Risk permeates everything we do, and has always and it will continue. And the explicit understanding of risk, how to measure it and how to corral it or manage it, and designing instruments that make that possible, it is a very challenging thing but it’s not going to go out of business. And finally, I would point out for the longer term that, you know, for someone starting a career, you’re in a circumstance because of the evolving financial technology and physical technologies, that not only do the emerging market countries have to revise their financial systems and institutions, but so does Japan, the second largest economy. It completely has to redo it. It’s not going to happen overnight of course. But so does Euro Europe, because while you have monetary union here you have not rationalized or designed the institutions that are going to permit that a true unification. No question that’s going to happen. No question it’s going to take rebuilding.  The excitement for the individual who’s going into the field now, at least I see it, is that there’s already enormous technology, market proven technologies, that can all be brought to bear to address these whole new problems. And, as I discovered in my own career, some ideas I had were adopted within a couple of years into mainstream practice. That was the options model. A second idea was related I had dealing with rescue debt. Took 26 years before it’s now become widely adopted. The difference between the two was not somehow that people didn’t recognize the good contribution in the second term, because they did. It was need.  Practice response to the things that are most needed. It doesn’t have the luxury of looking at every idea and developing it. I believe that the need in the case of Eurolan, the need case of Japan and then of course all the emerging markets and whatever you want to define China to be, which is kind of in a class by itself, all point to both a need and the availability of the technology to expand and develop in whole ways that hadn’t been done before. Really, if you want a metaphor for rebuilding the financial systems and the opportunities, in the past people had looked to the most successful economies and tried to model from them, adapt them. Whether it’s the US or Britain and so forth. But in this rapidly changing environment of technology it’s a little bit metaphoric. The same as if you were going to build a telephone system today because you had none, you wouldn’t adopt the one that’s in the United States with telephone calls. You would leapfrog the technology and build up a /- – -/, digital. And the same thing is true of financial systems. It’s not that the US had a lousy phone system, in fact one of the ironies they had the best one. But because they had the best one it doesn’t make sense to tear it all down and rebuild it.  So here, when you have this need of developing, of redoing these financial systems, the sort of bright light to it is because of all the technology that’s evolved is the opportunity to leapfrog the best systems which exist today substantially; if you look at it the right way; if you’re forward-looking not looking backwards, because if you’re looking for help it’s not in the past.  … if I was starting over I’d be very excited. I’m excited now. I don’t plan ever to retire …  And this creates wonderful opportunity for young people coming along, whether in academia to do research. Just think of you know all the things that one could do. I mean, if I was starting over I’d be very excited. I’m excited now. I don’t plan ever to retire. But if I was starting over I think it’s a wonderful situation to be getting into. I mean because even though there may surely be delays, we know we sadly have wars and depressions – or at least recessions, that will dislocate changes. But if one of them looks at what has to happen over the next several decades, from a career point of view, both in terms of satisfaction and being in demand and having a chance to do something that could really influence a lot of people, you could look at it afterwards and say, “I was a part of that,” not a bad feeling. There may be some others that you could name, but I think it has a great deal of opportunity. And that’s what keeps me going now. |
| Q72 | That’s lovely. I just want to pass on the question about the third world and the developing world, which is really a continuation of what you’ve said. They have been going about financing and building up in a very old-fashioned way, taking risks that have not been at all profitable and of course having been so badly hit by war and conflict and natural disasters. But how do you see the developing world making the leapfrog, because it’s certainly there, the natural resources, the human resources? The capacity is enormous and yet they are so far behind. |
|  | Of course it’s a great challenge and to sit here and suggest it’ll all take care of itself would be not the case. But that said, if we measure it in relative terms, relative to where we have been and what the opportunities or hope might be for the future, in my area of finance the new technologies, this new way of doing things, of being able to really much more efficiently distribute risk, to find ways which we’ve evolved of stripping risk and separating from the other qualities, of investment projects or a variety of other economic activities – gives us the opportunity to allow these countries to develop better the resources they have, because they can in effect take more risks, not for themselves because, that’s one of the problems – they are not rich enough, wealthy enough, to be able to take much risk – so therefore you know that’s where the rather poor, low return opportunities. The hope here is they’re being able to distribute the risk elsewhere in the world, which it should be. Very much analogist to what I discussed moments ago about the households.  There are better ways to distribute lots of the risk than to impose them all on these smaller countries. So if we find ways, and there are ways, to distribute it that gives the opportunities for them to develop their resources, which inherently may have more risks, but they can shed them efficiently, and also have with them somewhat higher returns; that’s one element. A second element is on the ongoing basis it’s possible to substitute for the very concentrated risks, which are inherent in a small country. I mean there are only typically one or two major industries that can be developed or sources, whether it’s agriculture or natural resources, oil even, that you can now get rid of those risks, those concentrated risks, while retaining the good parts of developing what are the comparative advantages in those countries? You could say, “What’s different now?” Some technological difference. If you look at how countries, developing countries and certainly third world countries, get financed at all, it’s been done by bank loans, it’s also been done by governments and World Bank support and so forth, but to the extent you have private sector financing it’s almost all been done by banks. It’s been done as a consortium and it’s typically been loans, loans in some foreign currency. Typically short-term loans, which is a prescription frankly for disasters. You know that may have been the best way to do it at the time, but it sets up a situation for a crises.  The idea here is by bringing financial technology, opening up the financial world, all the kinds of these new technologies /- – -/, financial contracts as we call them, and so forth that are papers, derivatives and so forth, is it offers a multiple chance to these countries for getting rid of risks and for getting investment. So if one of the channels is clogged or something goes wrong with its banks, they have other channels; they’re not shut off. Every country will benefit by development of this financial technology, but the smaller developing countries and the poorer countries actually will, I believe, benefit disproportionately because it gives them a potential seat at the table. Now it’s true they need education – not of everyone, I mean education of people to be able to make use of these markets. But that’s feasible. There is no reason at all that these countries can’t have the best that they have.  I know the World Bank used to have a programme, which I think they still do, where they train them in how to deal with this. So you know it does require education. But the opportunity, the idea of having a place at the table of the world capital markets, without only being able to go through indirectly through the one channel which are banks, I think you know holds great hope in my mind. And so in a way it’s going to be inevitable that at least the first beneficiaries of a lot of the technology are the more developed countries, because that’s where the resources are, but ultimately I think the incremental percentage benefits are going to be much greater for these smaller countries and that’s really a hope in my mind. It’s a hope but it’s a hope with some reason to be hopeful. |
| ID | 0855 |
| Biographical | I was born in Timmins, Ontario, Canada on July 1, 1941. My father had ventured to Timmins, a relatively prosperous gold-mining region, to practice dentistry during the depression. My mother and her uncle established a chain of small department stores in and around Timmins. The death of her uncle resulted in a family dispute, my first exposure to agency and contracting problems. To my benefit, my mother then devoted her time to raising her two sons. At the age of ten, we moved 500 miles south to Hamilton, Ontario.  I was a good student, ranking near the top of my class. Soon after we arrived in Hamilton, my life changed dramatically. My mother developed cancer. She died a few days after my sixteenth birthday. Another shock awaited me. I developed scar tissue on each of my corneas that impaired my eyesight. It was difficult to read for extended periods of time. I learned to think abstractly and to conceptualize the solution to problems. Out of necessity, I became a good listener–a quality appreciated by subsequent associates and students. Luckily, at age twenty-six, a successful cornea transplant greatly improved my vision.  Through my parents and relatives I became interested in economics and, in particular, finance. My mother loved business and wanted me to work with her brother in his book publishing and promotion business. During my teenage years, I was always treasurer of my various clubs; I traded extensively among my friends; I gambled to understand probabilities and risks; and worked with my uncles to understand their business activities. I invested in the stock market while in high school and university through accounts set up first by my mother and then by my father. I was fascinated with the determinants of the level of stock prices. I spent long hours reading reports and books to gleam the secrets of successful investing, but, alas, to no avail.  Because of my mother’s death, I decided to remain in Hamilton attending McMaster University for my undergraduate studies. Although the McMaster University entrance committee thought that I would concentrate in Physics or Engineering, I stuck mostly to the liberal arts, majoring in Economics. At McMaster, Professor McIver, a University of Chicago graduate in economics, worked closely with me and directed me to read and understand the works of [George Stigler](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1982/index.html) and [Milton Friedman](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1976/index.html), two subsequent Nobel Prize winners in Economics. I was impressed. Upon graduation in 1962, I had considered attending law school but instead, I decided to follow my mother’s wishes and join my uncle in his publishing business. I would do so, however, if I could first attend graduate school at the University of Chicago.  Intuitively, I knew that if I wanted to grow and achieve my potential, I should attend a school where I could learn from and work with those who were the best and who could bring out the best in me. And that has become a cornerstone of my career. During that first year in Chicago, I met a few classmates, who would become life-long friends, and from whom I have learned a tremendous amount over the years. Michael Jensen and Richard Roll, both in the Ph.D. program in Finance, who have become world-renowned scholars in their own right, were significant contributors to my growth in understanding of finance and economics. Also, I credit Jack Gould for helping me clarify many of the finer points of economic reasoning.  The summer after my first year at Chicago changed the direction of my life forever. I decided that I would not return to my uncle’s firm. Instead, although I had never programmed before, I secured a junior computer-programming position at the school through the kindness of Dean Robert Graves. During my first few days on the job, several professors asked for computer-programming assistance on their research projects. I was able to fend them off by arguing that the senior programmers would soon be on scene to assist them. They never did show up. By the third day, I could no longer fend off the aggressive professors seeking programming assistance. On confronting Dean Graves, he informed me that I, a novice, was the only “programmer” left. He pointed me to the computer facility some six blocks from the school, and I was on my way. I spent the next four and one-half months falling in love with computers and with the researchers that I met that summer. I must have been one of the first computer nerds; I worked all hours of the day and night. But by the end of that summer, I was becoming a computer wizard, a skill that I would continue to develop over many years. If Chicago had had a computer science school or if computer science had been a more developed field, I might have become a computer scientist.  A more powerful force, however, had taken hold of me that summer: the love of economics and economic research. I absorbed how my professors created and addressed their own research. This was empowering. They enjoyed the process. From time to time I ventured to ask them to explain their research, and occasionally I made suggestions to improve the research design. Lester Telser and Peter Pashigian were two of my clients. [Merton Miller](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1990/index.html) and Gene [Fama](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2013/), two financial-economics professors, were clients as well. Either because of my scholastic qualities or because he did not want to lose me as a programmer, Merton Miller suggested that I enter the Ph.D. program. I did and I came to love economics and its young new branch, which has come to be called Financial Economics. Chicago provided me with a wonderful learning environment. Miller and Fama were blazing ahead in financial economics. Stigler was leading the way in information economics. Friedman was fighting on in the macro-economic front.  I became interested in relative asset prices and the degree to which arbitrage prevented economic agents from earning abnormal profits in security markets. My Ph.D. dissertation attempted to determine the shape of the demand curve for traded securities. Since risk and return characteristics distinguish one security from another, the extent of the market was far greater than that of the individual stock. It was new information that would cause a change in the price of the security, information that was signaled by a large sale by an informed trader.  In addition, I worked on measures of risk and the effect of differential risk on security returns in a paper with Merton Miller. I studied the relation between accounting and market-determined measures of risk in another paper with William Beaver and Paul Kettler.  After essentially finishing my Ph.D. dissertation in the fall of 1968, I became an Assistant Professor of Finance at the Sloan School of Management at MIT. Paul Cootner, [Franco Modigliani](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1985/index.html), and Stewart Myers became my colleagues. During my first year at the Sloan School I met Fischer Black, then a consultant working for Arthur D. Little, in Cambridge. We started collaborating on many research projects. It was an extremely productive relationship.  Although Paul Cootner unfortunately left the Sloan School in 1969, Robert Merton joined our group at that time. Essentially, because Franco Modigliani was involved in large macro projects, the young assistant professors controlled the development of the financial economics program at the Sloan school. Stewart Myers greatly influenced my thinking in the area of corporate finance, and Franco Modigliani on macro and asset-pricing models.  Robert Merton, Fischer Black and I were interested in asset pricing and derivative pricing models. It was through many interactions that we developed and extended the field of contingent-claims pricing. During my years at the Sloan school, I worked on testing the capital asset pricing model with Fischer Black and Michael Jensen, and developing the option pricing technology with Fischer Black, while continuing to work with Merton Miller.  Although I knew that I would miss working on a day-to-day basis with Robert Merton, I returned permanently to the Graduate School of Business at the University of Chicago after visiting for the 1973-74 academic year. Fischer Black took his first position in academics as a Professor at the University of Chicago in 1972. I wanted to return to Chicago and, in particular, work with Fischer Black, Gene Fama and Merton Miller. It was an important period in the life of the school and I had the opportunity to interact with many interesting colleagues. Although Robert Merton was successful in luring Fischer back to Boston in 1974, I resisted and remained at Chicago. During my Chicago years, I started to work on the effects of taxation on asset prices and incentives. For example, I studied the effects of the taxation of dividends on the prices of securities in three papers, one with Fischer Black and two others with Merton Miller. Merton Miller and I studied the interaction of incentives and taxes in executive compensation. Robert Hamada and I addressed capital structure issues with taxation, and George Constantinides and I studied the effects of taxes on the optimal liquidation of assets.  I became heavily involved with the Center for Research in Security Prices at the University of Chicago between 1973 – 1980. This led to the development of large research data files of daily security prices. Joe Williams and I wrote a paper on the estimation of risk parameters employing nonsynchronous data.  In 1981, I visited Stanford University and became a permanent faculty member in the Business School and the Law School in 1983. The period at Stanford was a time of significant learning for me. My close colleagues in the Business School included [William Sharpe](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1990/index.html), James Van Horne, and a host of up and coming younger professors most notably Jeremy Bulow, Anat Admati, Paul Pfleiderer and Michael Gibbons. My close colleagues in the Law School included Ronald Gilson and Kenneth Scott. With Jeremy Bulow, I wrote several papers on pension planning. Most important, I was fortunate to work with and become a close friend of Mark Wolfson. We wrote several articles together on investment banking and incentives. We developed a new theory of tax planning under uncertainty and information asymmetry. Many of our published articles on these topics were rewritten and incorporated into our book, *Taxes and Business Strategy: A Planning Approach* that was published in 1992.  In 1990 my interests shifted back to the role of derivatives in financial intermediation. I became a special consultant to Salomon Brothers, Inc. and continued on as a managing director and co-head of its fixed-income-derivative sales and trading group, while still conducting research and teaching at Stanford University. In 1994, I joined with several colleagues, many from Salomon Brothers, to become a principal and co-founder of a firm called Long-Term Capital Management. By applying financial technology to practice, I have achieved a better understanding of the evolution of financial institutions and markets, and the forces shaping this evolution on a global basis. My research papers in the last few years have focused on the interaction and evolution of markets and financial institutions.  I have received honorary doctorate degrees from three universities. University of Paris-Dauphine awarded one to me in 1989. McMaster University awarded me another in 1990, and Katholieke Universiteit Leuven awarded me my third in 1998. I am fortunate to have two wonderful daughters, Anne and Sara, and a son-in-law, Anne’s husband Seth. They have added tremendous joy to my life. My fortunes have also risen in the last few years for I have found Jan. She completes my life. We plan to be married on October 4th 1998 and enjoy each other’s company and insights for many years to come. Although I do not have time for many hobbies, I do enjoy skiing and golf, two sports that allow me to be outdoors in both winter and summer. |
| Autobio |  |
| Podcast |  |
| Telephone Interview |  |
| Interview | Myron Scholes discusses the concomitant benefits of having poor eyesight as a child, the importance of reading the classics (6:36), the view that led to his choice of PhD topics (8:22), the necessary cross-pollenization of theorists and empirical economists (13:16), and the option pricing model he helped devise while working as an assistant professor at MIT (16:39). He then explains how this model was accepted among practitioners before academics (27:15), why communication and perturbation are essential to scientific research (36:12), why he retired from teaching (43:29) and why “risk is fine” (50:01). |
| ID | 0856 |
| Biographical | Newton Stewart is a town of two thousand people in the beautiful centre of Galloway, in the southwest of Scotland. My father came there in 1934, newly married, to be a teller in one of the six banks. In 1936 I was born, in a cottage across the river in the neighbouring village of Minnigaff. Three years later we moved to Newton Stewart proper, my brother was born, and, coincidentally, my conscious life began. Though later, about 1950, we moved to the coastal village of Port William, eighteen miles south, I went to school in Newton Stewart, travelling latterly by the school bus. While at primary school I was apparently quite quick at mental arithmetic, and also acquired glasses. If you need glasses it is hard to enjoy football (association football is the main sport in the area). Without them, I was not good at guessing where the ball was. That, and various childhood illnesses, gave me time to read, which suited me fine. To tell the truth, I would not have been very good at football anyway. I once took a catch in the annual cricket match.  To my relief, I passed the “control”, the examination at age eleven to decide who could go on to the high school, the Douglas Ewart. In these days there were prizes every year. With much parental encouragement, I tried to win them, and as far as I remember was rather successful. But I must have had some sense that this drive to win is somewhat ignoble. When a friend beat me in chemistry I recollect being scolded at home for accepting defeat with equanimity. By the age of fourteen I had acquired a strange enthusiasm for mathematics, having managed to acquire a book called Teach Yourself Calculus, and done so. When he found out, the head mathematics teacher somehow gave me individual tuition during classes, and I raced ahead. At the same time the music teacher, who was also my piano teacher, provided books like Hogben’s Mathematics for the Million. In the school bus, I tried to read my mathematics teacher’s university books. This was much more fun than trying to come top. When asked by the Rector (the headmaster) what I wanted to do in life, I gave the obvious answer: be a professor of mathematics. Mr Geddie sounded appropriately sceptical.  In Scotland, unlike England, one does a wide range of subjects all through school, and for the final school examination: English, mathematics, science, French, Latin, history, in my case. Oddly enough this examination is taken in the penultimate year, at age sixteen, and the final school year is devoted to odds and ends, except that there were two special mathematics papers that one could do in that final year. I took them a year early, successfully, catching the attention of the inspector from the Scottish Education Department who suggested I should try for the Cambridge scholarship. No doubt we had heard of Cambridge, but none of us, teachers, friends or relatives, knew what this mysterious scholarship examination was, nor why going to Cambridge might be such a Good Thing. It emerged that Cambridge was not for young Scots, in the normal way, since the government grants provided to pay university fees and some subsistence, could not be used outside Scotland (the English, though, were allowed to come to Scottish universities, and many did). The one exception was the scholarship. If someone won a scholarship to Oxford or Cambridge in the college scholarship examinations, a supplementary grant would then be given. In Glasgow and Edinburgh there were schools that prepared some of their pupils for entrance to Oxford and Cambridge, but that was another world.  The examination was held in December. The suggestion to take it was made, I believe, in June. We were not aware that in English schools those preparing for the examinations would specialize for two or three years, doing, for example, nothing but mathematics. We, my mathematics teacher and I, got past examination papers and tried to do them. It was exciting, but even in late November I found I could not always solve all the questions in the time available. We did not know that rather less was expected. There was no immediate happy outcome. The weekend I should have gone to Cambridge to take the examination I was rushed to the nearest serious hospital, seventy miles away, with peritonitis. In 1954, I went to Edinburgh, which I did not mind then, and do not regret now.  Somehow I argued my way into starting the Edinburgh mathematics course in its second year, thus shortening the normal four-year Scottish degree to three years. Also, and I suspect this was the more remarkable achievement, I persuaded the authorities to let me take philosophy in my first year. It was regarded as morally dangerous to take philosophy at the beginning of one’s university course, but I had a cousin who was doing philosophy at Glasgow (and still is); and on long country walks he had infected me with it. I did not do any other philosophy courses at university (though I went to some lectures later in Cambridge), but that remained an important basis for much of what I tried to do later in a subject that used to be part of the Moral Sciences tripos in Cambridge, economics.  In those years at Edinburgh, mathematics was easy, and needed little time. Having obtained a number of individually small scholarships through various examinations, I had just a little more money than the minimum, and could afford the new Penguin books as they came out. The university library had lovely open shelves and easy access. I could afford to go to some concerts, which were cheap at that time, and the National Gallery of Scotland is a wonderful introduction to painting. There were plenty of university societies too, and being far from home I threw myself into them too, debating, philosophy, but not then much politics; and endless talk in Cowan House, a long disappeared hall of residence with, I think, selective admission. At any rate, it was a great group of people.  Although mathematics then seemed easy, I am sure I was getting careless. But as usual I was lucky, and got the Napier medal in the final examination. Earlier that year, I had, at last, taken the Cambridge scholarship examination, as had a series of other Edinburgh mathematics students, and thereby earned a further grant to go on and do yet another undergraduate degree. In 1957, at the age of twenty-one, I left Scotland, like many before and since, an excited member of the Scottish diaspora, with my birthday present, a typewriter. In the summer, I had taught myself to touch-type.  The mathematics undergraduate degree at Cambridge had three parts, or triposes. For a degree it was sufficient to do Part I and Part II: the famous “wranglers” are those who are in the first class in Part II. The best mathematicians did Part II in their second year and then Part III in their third. Those of us who came from Edinburgh (and other universities whose preparation was deemed insufficient for an immediate plunge into research) did Part II in the first year, Part III in the second. In Trinity, the College I had joined, two of us from Edinburgh enjoyed being taught together by some of the best mathematicians one could hope to find. It was exciting, and not yet too difficult. We duly became wranglers.  In Cambridge you can go to lectures in any subject, without formality. I took full advantage. In the summer I wrote a mathematical essay for a prize. The subject I chose was game theory, and I didn’t make much of it. My Edinburgh friend got the prize. Then came Part III mathematics, with subjects on the borders of current research. That was hard, and there was a lot I wanted to do besides mathematics. The result was good enough to allow me to go on to research had I wanted to do so, but I did not. Social science, perhaps even sociology, beckoned. Peter Swinnerton-Dyer, in mathematics, guided me to Piero Sraffa, in economics. In any case it was indeed economics I wanted to do, because I kept discussing it with economist friends, and they didn’t make sense to me; and because poverty in what were then called the underdeveloped countries, seemed to me what really mattered in the world, and that meant economics.  How could I possibly get finance for a third undergraduate degree? Fortunately Cambridge had an arrangement whereby you did a part of the final undergraduate examination in one year, called it the Diploma in Economics, and treated it as an initial year of graduate work. Still, money had to be found. All students in a Cambridge college have a Tutor, who looks after them in a general way, administratively not academically. Each Tutor has many students. Somehow mine managed to get the Department of Scientific and Industrial Research to give me a three year award to do a Ph.D. in economics. They had an interesting incentive contract: I did not have to take a Ph.D., but if I did not, then I had to write a thesis of equivalent length. I didn’t enquire what the sanctions were, I just got on and did it. Somehow the examiners at the end of the first year were fooled into thinking I knew some economics. Economics takes a while to learn, even if much of it is in a way quite simple. It is simple to be wrong as well as to be right, and it is none too easy to distinguish between them.  David Champernowne, newly returned from a Chair in Oxford to a Cambridge readership and a teaching fellowship in Trinity, was my first teacher. In Oxford people did not appreciate science fiction and computers, so he had returned to what he regarded as the centre of economics. Being taught on one’s own or with one other seems extraordinarily wasteful and expensive, but I have benefited immensely from it, and still enjoy doing it as a teacher. If it works, it is not usually by the simple transmission of information. David started by telling me to read Keynes’s General Theory, I think because he had just been re-reading it. That may not have been the best advice, but it did no great harm, and one day I hope to finish it. Needless to say, I also relied a great deal on fellow students, who in effect taught me most generously. They were cooperative times. My reading remained haphazard, and the lectures and seminars of the notable Cambridge names, Kahn, Kaldor and Joan Robinson were highly idiosyncratic, but it was a stimulating time.  [Richard Stone](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1984/index.html) was my official supervisor, to guide my research, and as soon as the first year was over he involved me in the Growth Project that he was beginning, a project to simulate realistically the long-term growth of an economy, particularly the UK. The guiding star was indicative planning, a forgotten notion. Stone pointed me to Ramsey on optimal saving (an interest of David Champernowne’s too) and at some point in all this I had discovered [Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/index.html) and mathematical economics. I tried writing research papers, which were largely rubbish. Dick passed something, I forget what, to Frank Hahn, just lured to Cambridge by Kaldor. Frank somehow saw merit amid the rubbish, and was encouraging. He became another unofficial supervisor and great mentor. I ought to have been feeling lost and confused that year, but I was engaged and happy, and got married to Gill, just finishing her teacher training in Cambridge. So we set off to Scotland for a honeymoon, with my typewriter, and I set about doing a Fellowship dissertation for Trinity. Considering I had made no discoveries, it seems a daft enterprise. Oddly enough I still have a copy: Contributions to a theory of economic planning. Without looking inside, I am sure there were no contributions. It was submitted for the October competition, and was unsuccessful. I remember no disappointment, just surprise when I learned that the thesis had not been so far from success. Writing a couple of hundred pages was a great way of learning anyway. There was little mathematics in it.  I was thinking about planning, which was the main theme of Stone’s Growth Project. Having a mathematical culture, I suppose I expected that at some point there would be a real idea, an inspiration, and one day in November 1961 it came. Uncertainty seemed to me unduly neglected, so I tried to think about how the amount of uncertainty should affect the optimal rate of saving in an economy. I thought of a neat way of modelling the question. Contrary to what everyone else seemed to think (then), I showed that quite commonly, greater uncertainty is a reason for saving more, not less. I started using Wiener processes and discovered the Ito calculus for myself. Of course it would have been more sensible to learn the techniques that were already known, but I didn’t know where to find them.  The thesis could have been finished at the end of that academic year, but two things happened. Nicholas Kaldor wanted a research assistant to help with writing a paper on growth, a continuation of a notable series. David Champernowne put us together. Nicky was no mathematician, so I was what he needed. In the end he generously made me a co-author. The paper is a bit mixed up, but our long discussions were a wonderful experience, as he tried to make sense of economic growth, and I tried to make sense of him. For a month or so, it was full time, and the thesis languished.  Then [Amartya Sen](https://www.nobelprize.org/nobel_prizes/economics/laureates/1998/index.html) suggested and arranged that I go to India for a year, with the India Project run by Paul Rosenstein-Rodan for the MIT Center for International Studies. Rosie said I must first go to MIT for the summer “to acclimatize”, and Gill and I had our only period of mild impoverishment living for three months in a basement in Somerville, Massachusetts, followed by a remarkably and inappropriately luxurious eight months in India. But it was a good summer: I met Paul Samuelson and [Bob Solow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1987/index.html), and gave a seminar at MIT on optimum growth under uncertainty. They spotted a mistake (which had not been in the thesis, I must add) but were nevertheless encouraging. Probably I am mistake-prone, but have learned to live with it. In September, we continued on our way to New Delhi.  It was never clear quite what I was supposed to be doing on the India Project, particularly after an initial period helping with a rightly abortive input-output exercise. I thought a lot, and wrote many little papers, particularly about investment appraisal, and efficiency wages. Some years later I remembered that I had worked out the theory of efficiency-wage equilibria in 1962 on our way from MIT to India, on the long, long flight from San Francisco to Tokyo, and wrote it up as a paper in the early seventies, a paper I still rather like. The work on investment appraisal, including ideas about uncertainty, led on, after a lapse of years, to work with Ian Little on criteria for cost-benefit analysis in developing countries. I fear I did not do as much for the Planning Commission as had been hoped or intended. I learned an immense amount, both from the country and from its many fine economists. In these days Jagdish Bhagwati, T. N. Srinivasan and Sukhomoy Chakravarty were all there too, and Amartya Sen was about to return.  Before I had left Cambridge, Nuffield College, Oxford offered me a research fellowship for which I had not applied. Trinity retaliated almost instantly by offering me a teaching fellowship in economics to be taken up in 1963, when Sen would be leaving to return to India. I accepted the Trinity job. It seems ridiculous, but I have never had a job I applied for. When I do apply, I don’t get it, but that is a small sample. While in India, I was told I had been given a university assistant lectureship. That meant I would not have to do so much individual teaching in Trinity, and would have to give lectures.  When we got back from India, two things had to be done. It was time to write the thesis, and to have a child. We did. Catriona was born in a College flat in Trinity Street, and I cooked duck a l’orange for the only time in my life since Gill couldn’t very well cook the celebratory dinner. The thesis was duly submitted in September 1963, on Optimum Accumulation Under Uncertainty. Wonderfully, [Ken Arrow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1972/index.html) was visiting Cambridge that year, and was one of my two examiners. He tried very hard to find the mistakes and failed. But I have still never been able to solve the main problem the thesis addressed, at least to my satisfaction. I published only one small paper on the subject, much later.  Ken Arrow had already been thinking about investment choices under uncertainty, and I found that what I had worked out in India had already been done better. Bob Solow was there that year too. Growth and capital were the main subjects of discussion. I wrote up, and greatly improved, the easy part of the thesis, without uncertainty, at much the same time that Cass, [Koopmans](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1975/index.html) and others were developing optimum growth theory beyond the Ramsey level. Now writing slowed to a crawl, whether because of the demands of teaching, or rising standards; and because I could not prove what I guessed about the uncertainty case. Speaking of crawl, Fiona was born in 1966. Fortunately the stimulus of teaching took me in some new directions, as I thought increasingly about general welfare economics, conceived as a general theory of economic policy. An examination question about optimal taxes caused immense trouble among the examiners, since Joan Robinson would not believe the result. It should not have been in an examination paper, of course, but it was the beginning, on my side, of the work on optimal taxation that Peter Diamond and I did in the next few years, after he came on a six-month visit to Cambridge. I followed the main principle for academic success: get a good co-author (and also the second: get another). The still-continuing collaboration with Peter has been at the centre of the work, his influence on the sole-authored papers immense too.  That in turn led to thinking about nonlinear tax schedules, and what we still call optimal income tax theory, which I discuss in the Prize Lecture. But that step, towards a more general conception of relationships between principal and agent in economic contracts, came after I had essentially left Cambridge. Oxford had a professorship of economics, which had to be in mathematical economics or in econometrics. David Champernowne had held it. Now it was vacant and proving hard to fill. They decided that some baby-snatching was in order, and offered it to me in 1968. At that time, thirty-two seemed quite young for a professor. Cambridge was still a place to be, with [James Meade](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1977/index.html) and Dick Stone, and good new people, often recruited by Dick; but Frank Hahn had already left, and Cambridge was increasingly suffering from shrill doctrinal, almost religious, squabbles (mainly then with the rest of the economic world) . It was time to go. I briefly toyed with MIT and LSE, both standing higher than Oxford, but we were small-town people. At that time, Ian Little, at Nuffield, had already got me to do a manual on Cost Benefit Analysis with him. Paradoxically, the Oxford choice probably meant I would not specialize too severely in mathematical economics. It also meant that I would deal entirely with graduate students. It was immensely helpful to have that simplification in what had become a too complex academic life.  In the intervening sabbatical term at MIT, between Cambridge and Oxford, work on nonlinear incentive relationships began. That year, or the next, the first version of the optimum income tax paper went round, but mathematical justifications took another year and too many pages. In the end much of the rigorous justification was published only many years later. I never learned not to publish in a book: it can take a very long time to appear. Of course it can be quick too. The mimeoed version kept vanishing from the Nuffield library, so at least it was being read, or looked at.  Already several superb PhD students had come to me as supervisor, for example, Azizur Rahman Khan and Partha Dasgupta, and I had taught David Newbery as an undergraduate. From that time on I found myself almost invariably with at least one, often several research students of the highest class. The Oxford environment seemed to make that happen. I have always supervised research much more diverse than what I do myself, and by no means all of them worked in the principal/agent or welfare economics field. Some became colleagues. It was only quite loosely a school of optimal taxes and welfare and incentives. I am proud that in due course industrial economics and game theory flourished in Oxford. Even they are not unconnected with incentive and contract theory, but there is no doctrinal connection, no common catechism. I have long lost count of the number of my students who hold full professorships, but I like to think they are numerous as well as able.  There came a time when it seemed best to make a last change, to seek new stimulus. In November 1993, Gill died, five years after cancer was first diagnosed. Catriona and Fiona had grown, married, and gone. A Cambridge Chair was offered, and in 1995 I moved, and moved into Trinity. There is still work to be done.  When that curious English publication, Who’s Who, first asked me for an entry, my normal inclination to brighten up dark corners led me to list as my recreations “playing the piano, reading detective stories and other forms of mathematics, travelling, listening”. I did not suppose that anyone would have much reason to read it, but in these last two months it seems many have, and, looking at it again, I find no reason to change it, though I should now add other reading and computer programming. Everything is to be interpreted there in the broadest sense, as at least those (few) who have heard me play the piano may agree.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1996, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1997  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.  James A. Mirrlees died on 29 August 2018. |
| Autobiographical |  |
| Podcast |  |
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| Interview |  |
| Q6 | I just want to start off with some memories from 1996, the day that you were told that you were to receive the Economy Prize. What were your thoughts? I don’t know whether the telephone rang or whether you were told in a different way, but what do you think? Was it a huge surprise? |
|  | So the telephone rang and I got the message. And I politely suggested that it didn’t sound very likely and I needed some proof. But fortunately Assar Lindbeck was there too, so there was somebody I knew who could actually tell me it was true. So I thought it was very surprising. That was the first thought. Then I suppose I kind of wondered what was going to be the little problems of the day that followed from all of that. Was I going to be able to get away from journalists?  And were you? Not really.  James Mirrlees: No, I really didn’t try to. But Cambridge wasn’t awfully good at organising things. I did have a little press conference with some journalists, but most of them who wanted to get in touch with me were unable to do so, because I was somewhere else when they were trying to get hold of me.  You quietly disappeared somewhere.  James Mirrlees: They got me into the wrong place really and we weren’t very good at communicating around the place. Difficult to organise. But then I decided it was really quite a nice feeling. I was appreciated in a way I didn’t think I was. So it was nice to know my work seemed actually to be worthwhile. |
| Q4 | Has it changed your working life in the sense that you are now demanded to answer questions about all sorts of problems that have to do with world economy? |
|  | Occasionally, but I’m not usually asked by people who really need to know the answers. I suppose I haven’t been quite that kind of economist on the whole. But I find I have to give a lot of lectures on general topics to a much more general audience than I was ever used to talk to. And you feel an obligation to do that kind of thing. And you have to think about problems that are seen by ordinary people as really major economic problems, but on which I haven’t done professional work. It’s been quite a challenge and it’s also quite interesting a lot of the time. |
| Q5 | I read that you were very good at maths as a child, but it wasn’t a straight road. You were also taking philosophy at university and you were interested in developmental issues, and you certainly liked, I read, to supervise research of the students that you worked with, you know. What was your main driving force during this development from your student years and forward? What was the main driving force for you? |
|  | I suppose that I wanted to know everything. It must have been something like that; a sort of a lot of curiosity. So that wasn’t just curiosity to explain things, but curiosity to know what people had said. So I spent an awful lot of time reading in that period.  The force for that was just because one of the advantages of the Scottish university system is you do some subjects beside your major subject. And I did that and some English literature and some physics, although mathematics was the major subject. So I’ve the sense that somehow going through Edinburgh, looking back, I should really have been trying to do some serious mathematics research then. But I didn’t, I just enjoyed doing the mathematics and learning more about it. So a lot of the time it was just learning. And then somehow in Cambridge there was this immense shift of deciding that I didn’t quite see the direct point of mathematics, at least for me, then. So I wanted to do something that I thought would be more useful.  And then I really got this feeling that there were problems I wanted to solve …  And I’m sure I must also have felt that it would be more interesting. And then I really got this feeling that there were problems I wanted to solve, I really wanted to do new things. And that I suppose was because there are just one or two papers I read that seemed to have a tremendous excitement about them. They would pose a problem, an interesting problem, and could actually solve it. They just seemed lovely examples of how you can get a fresh thought; something you would have no idea that you could really say anything sensible about. And one of them was about optimal taxes, which is what I then got very interested in professionally to run. |
| Q18 | I know that you went to India. That was a sidetrack maybe. But what did it give you, because you have said that you wanted to do something useful and something new, and at that time there was this whole idea of helping the developing world to greater economy, to, you know, narrow the gap? |
|  | Yes. It became clear I wanted to be a development economist. I mean I said I wanted to work on the economics of poor countries. And I’d actually say that I don’t think that was so much about narrowing the gap as about increasing their incomes, which means economic growth, which is really my prime interest. So it was very fortunate that I was able to go to India. It was arranged for me by [Amartya Sen](https://www.nobelprize.org/prizes/economic-sciences/1998/sen/facts/), who’s also a Nobel Prize Winner, because we knew one another at that stage. And I was just a research student, he was a young research fellow but he had these contacts. And it was a marvellous opportunity to have to do with the Planning Commission there. I didn’t feel that it was entirely useful for them, because I suppose I had a real sense that I didn’t know quite what to do.  Looking back I don’t quite know why that should have been so. But you know there’s problems, you can see all the poverty and so on, but it’s quite another matter to know what you can do about it. And some people are rather good at knowing that. But anyway I wasn’t. And I spent a lot of the year really trying to think my way through to that.  And I think it worked quite well, because I suppose I decided that the main issue was how people should decide what to do sort of rather indirect, and got into the idea that instead of planning you should have a systematic way of deciding what kind of new projects to start. In other words a much more piecemeal thing, much more like Western economy. Which at that stage in India was not. India thought that you could actually deal with economic problems by building a model and then seeing what it said you should do and doing it.  And it didn’t really work did it?  James Mirrlees: No, no. No, you can’t just say do this and it happens. Or it may happen in a much too expensive and wasteful way. But it actually took me a while to really understand how wasteful the whole process was. |
| Q18 | There has to be incentives for people to pay tax somehow. When I look at the optimal taxation model, the idea is that you need to feel that you’re getting something back you know so that you actually pay, if I’m not simplifying it too much. Would you just tell us a little bit about the way you see the taxation system? From that point of view it may be forwards. You know where are we today, the so-called Western world? And certainly not all people feel that they are you know wanting to pay as much tax because they don’t feel they’re getting back what they should get. |
|  | Yes, I suppose that there are lots of excuses about tax and I know it makes people rather unhappy.  I suppose that I’m forced to see that high tax rates do induce quite a lot of people to find ways of avoiding them …  When I had the sort of little film interview that they do after the prize as part of a general film, the cameraman on that occasion said to me as we were walking across the court in Trinity, “Since the taxes you pay are going to be spent on things that the Government does, which are good things like the health service and education and so on which you surely value too, doesn’t that mean that you should be very happy to pay taxes and there should be no incentive problem?” So I thought that was a nice argument and I liked it and I wish that people felt like that about taxes. But of course the work I’ve done has mainly been recognising that people do not feel like that about taxes and that it apparently even got to the point of discouraging quite a lot of people from earning as much as they might have, which in particular I mean they no longer pay as many taxes as they should. I suppose that I’m forced to see that high tax rates do induce quite a lot of people to find ways of avoiding them, or evading them.  And I think that’s also part of it. In the Western countries, still the total tax take is pretty high and I think on the whole it’s going up again in most countries. And that’s for perfectly straightforward reasons that more and more is needed for things that, at least in Europe, the state does, like education, which is necessarily something that gets more expensive, and very notable in Britain the health service. The rate at which we spend on health is going up a great deal, and I think that’s pretty much the picture in the European countries as well. And actually I’m not as concerned as lots of people are about the marginal tax rates which they think are quite high. I think in effect in most of the European countries the total marginal tax rate is over 50 percent; that’s to say add on other taxes like VAT to the income tax. So yes, people are paying rather a large proportion of their income above the basic to the Government to use for this and that. But I suppose I don’t quite understand why they don’t think they’re getting a lot of it back, because actually a lot of the benefits do accrue to the better off as well. And then there are lots and lots of people in the country who are not so well off. And I would think clearly a majority of the population are getting or can expect to get more out of the system than they’re putting in on taxation. So I think that’s what it’s about. It’s about ensuring that the things that are needed for everybody, certain basic things which cannot be conveniently left to people to buy for themselves, should be available for everyone, but they are mainly going to have to be paid for by the better off. |
| Q24 | How do you see the issue around the ageing population in the Western world, and the fear that there will not be enough tax coming in to actually pay for pension schemes and for the care of the older population and so on and so forth, including education and all the other things that we are used to that the state would provide for the population in general? |
|  | Yes. I mean certainly for the main continental European countries there is quite a big issue there. I suppose I believe it’s a bit exaggerated because economic output, which is what we are needing here, gets created by capital as well as by people, and that’s even true of things like medical services and to some extent of education. So that you need workers of course but you also need other things like capital. And so you can ensure that there is more output available in due course by building up capital by saving.  So I do think that these demographic changes imply that it’s rather important for the European economies to do more saving. And indeed some of them have moved over to doing that, but I think not rapidly enough; they’re not doing enough of that. And some of the birth rates are really remarkably low. And this will have a variety of effects. What people are worrying about now is the period when there will be a lot of retired people and not all that many people working. I must say that I still think while people might find themselves, an average person, might be paying nearly half their income over taxes and so on, that’s very different from paying 90 percent. And when you think what it’s being paid for, health, education and pensions, these seem rather valuable things.  Well of course.  James Mirrlees: So I think it’s a little strange people should be quite so troubled about that. But of course this is against the backdrop of a situation where these European economies have not been growing very fast. Incomes have been rising a little but not a lot. I’d like to be able to say, “Well, you know maybe what’s going to happen is that the workers in 20 years time will not be able to spend more on current consumption than the workers today can do.” Which after all isn’t bad. And I think it should be close to that. In other words, people will be spending more of their income on taxes in order to pay the current pensions. And that part will grow quite a lot. So they look at the proportion and so on. So I suppose I think that people should look back and think that you know ‘Why should I be spending more on consumption than my parents,’ who were probably pretty happy with it. |
| Q72 | But is there a lack of dialogue, maybe, between politicians who are now forced to make the savings and, you know, more opportunistic politicians, maybe, who rather, you know, use people’s fear? I mean it’s difficult to discuss these issues, because they are quite complex and rather, you know, people are really just looking into their own pockets and not seeing the bigger picture. |
|  | Yes, and yet in a way I think people may sense what’s at issue here. Suppose you were to increase savings substantially, this would be a way of putting down people’s consumption now, like increase the taxes now rather than have them in the future. And probably people have a sense that that’s what’s at issue. And you shouldn’t always think of things in terms of proportions. It’s just absolutely saying, “Ah, now it looks as though the current generation should now cut its standard of living significantly in order that its children should in the future be able to enjoy a standard of living, consumption level …, probably quite a bit higher than the current generation.” And I say that partly because I actually think that the rate of economic growth is going to accelerate again. It’s not going to stay at 1½ percent.  You are fairly positive?  James Mirrlees: Yes, I don’t see why not. I mean there are some problems I know, but that’s partly looking at the overall picture of the European economies where I mean in an area like where we’re sitting now, which is pretty much the centre of a rather large circle where unemployment is low and economic growth is going on quite well, it’s just there are these other regions where it’s not. And it’s a very interesting question as to why say Western France, Southern Italy, Northern, Eastern Germany are performing quite badly. |
| Q13 | If we look at the world at large, we have this idea, although we see problems, that economic growth is possible, it will continue. But if you look at the world at large, the amount of natural resources is limited for example. Can we expect a continued growth though, I mean if you look at the whole picture? What is necessary? |
|  | It’s very interesting to look at it like that, and I think it wouldn’t be a bad idea if somebody were to try to picture what the pattern of our consumption might be likely to be towards the end of the century, certainly because of the limits on the natural resources. We should be doing much less travelling then or our grandchildren should be. You know planes should become very expensive because they’re using fuel. I dare say that there will methods brought in. But things like heating and air-conditioning and so on I’m sure we’ll be able to deal with by other methods other than hydrocarbons.  … I think it wouldn’t be a bad idea if somebody were to try to picture what the pattern of our consumption might be likely to be towards the end of the century …  Transport I suspect will get quite a lot more expensive. So we will spend our time in different ways. I think perhaps more holidays at home or closer to home again. And since we think of going away to the other side of the world for our holidays as somehow characteristic of the world we’ve moved into we may think this is a great loss. It’s a loss, but there are going to be other tremendous compensations; I don’t see why not. Certainly resources will put some limit on it. Of course I’m aware I remember a paper that I wrote years ago with a friend on natural resources. We remarked on how there had been all these tremendous concerns about how Britain was going to be in a pretty bad way because it soon run out of trees. And what will we do then? Of course that was just as economic growth was beginning and it turned out that it was no issue. Now, one of these times this sort of typical economist’s optimism will prove to be unjustified. The world’s been pretty good at coming up with new ways of doing things.The need will create new disciplines, new ideas, new questions to solve. Do you see …  James Mirrlees: Well, yes and it has done that and in a way it’s a little mysterious what’s happened. Like we would have thought by now that probably fusion energy would be generating most of our electricity and so on, and it hasn’t happened at all and there’s really no prospect that it will. So things are not going the way we might have expected. But I still believe they will. |
| Q10 | Eventually, yes. If you were a young economist today, or you were to advise a person to go into this field, what to look at to be very busy? What would you if you were to start afresh again? |
|  | I would think of different things each day more or less because I think I don’t have a persistent large vision about what exactly one should do. But it seems to me that the way that we reason, and how that leads to what we do, which sounds more like psychology than economics, but it’s a very economic kind of activity and this involves the sort of information we get from others. I think that as yet we have not found good ways of measuring that and recording that objectively.  So I really believe that the development of theories or models, whichever way you want to call it, of human behaviour is just beginning, and that there’s a lot of scope, both empirically in doing descriptive measurement of things of a kind that people have simply not done up to now – the way we form expectations and the way we work out our decisions, that’s just desperately needing to be done. And there’s actually a tremendous intellectual challenge in connecting these up with issues of economic policy … interested in about how taxes operate, how people respond to them, how we think about issues, the sort that you’ve been raising earlier. |
| ID | 0857 |
| Biographical | William S. Vickrey was born in Victoria, British Columbia, in 1914. His elementary and secondary education were in Europe and the United States, with graduation from Phillips Andover Academy in 1931. He received a B.S. in mathematics from Yale in 1935, followed by graduate work in economics at Columbia University from 1935 to 1937, when he received the M.A. degree. He then worked for the National Resources Planning Board in Washington and the Division of Tax Research in the U.S. Treasury Department.  A conscientious objector during World War II, he spent part of his alternate service designing a new inheritance tax for Puerto Rico. Columbia University awarded him the Ph.D. in economics in 1948. His doctoral dissertation, Agenda for Progressive Taxation, was reprinted as an “economic classic” in 1972.  In 1946 he began his teaching career at Columbia University as a lecturer in economics. He became a full professor in 1958 and was named McVickar Professor of Political Economy in 1971. He was chairman of the Department of Economics from 1964 to 1967 and retired as McVickar Professor Emeritus in 1982.  A long career of research covered a large range of subjects. The first of many involving efficient pricing of public utilities done in 1939 and 1940 for the Twentieth Century Fund dealt with electric power. In 1951, he studied transit fares in New York City for The Mayor’s Committee on Management Survey. He was a member of the 1950 Shoup mission that developed a comprehensive program for revising the tax system of Japan. He lectured widely and served as a consultant in the United States and overseas and to the United Nations.  He was elected to the National Academy of Sciences and in 1992 served as president of the American Economic Association. He was a Fellow of the Econometric Society and received an honorary degree from the University of Chicago in 1979.  He was a founding member of Taxation, Resources, and Economic Development and was a member of many professional and civic organizations and an active supporter of organizations promoting world peace. He belonged to The Religious Society of Friends.  A 1994 volume, Public Economics Cambridge University Press, contains a complete bibliography; it lists eight books, 139 articles, 27 reviews, and 61 unpublished articles and notes.  He was married to Cecile Thompson in 1951. They lived in Hastings-on-Hudson in New York. He died in October 1996.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1996, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1997  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.  William Vickrey died on October 11, 1996. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0857 |
| Interview |  |
|  |  |
| ID | 0858 |
| Biographical | I was born in 1937, in Yakima, Washington, the oldest child of Robert Emerson Lucas and Jane Templeton Lucas. My sister Jenepher was born in 1939 and my brother Peter in 1940. My parents had moved to Yakima from Seattle, to open a small restaurant, The Lucas Ice Creamery. The restaurant was a casualty of the 1937-38 downturn, and during World War II our family moved to Seattle, where my father found work as a steamfitter in the shipyards and my mother resumed her earlier career as a fashion artist. My brother Daniel was born in Seattle in 1948.  My parents were admirers of President Roosevelt and the New Deal. Their parents and most of our relatives and neighbors were Republicans, so they were self conscious in their liberalism and took it as emblematic of their ability to think for themselves. The idea that one could decide for oneself what kind of person to be, and that one ought to think about these decisions, was not limited to politics. I remember discussions, with my mother especially, of religion (she was a liberal protestant), of decor (she favored hardwood floors and oriental rugs), even on how to choose what kind of cigarette to smoke.  After the war, my father found a job as a welder at a commercial refrigeration company, Lewis Refrigeration. He became a craftsman, then a sales engineer, then sales manager, and eventually president of the company. He had no college degree and no engineering training, and learned the engineering he needed from the people he worked with and from handbooks. I remember many technical and managerial discussions with him, as well as our ongoing political arguments. When I took calculus in high school, he enlisted my help on a refrigeration design problem he was working on – and actually used my calculations! It was my first taste of real applied mathematics, and an exciting one.  I attended Seattle Public Schools, graduating from Roosevelt High School (where my parents had graduated in 1927) in 1955. I was good at math and science, and it was expected that I would attend the University of Washington in Seattle and become an engineer. But by the time I was seventeen I was ready to leave home, a decision my parents agreed to support if I could obtain a scholarship. MIT did not grant me one but the University of Chicago did. Since Chicago did not have an engineering school, this ended my engineering career. But when I began the 44 hour train trip “back east” to Chicago, I was pretty sure something interesting would turn up.  What to do instead? I took some mathematics at Chicago, but lost interest soon after my courses got past the material I had half learned in high school. I did not have the nerve to major in Physics, which is what you did at Chicago in those days if you thought you could make it. The real excitement for me was in the liberal arts core of the Chicago College, courses from the Hutchins era with names like History of Western Civilization, and Organization, Methods, and Principles of Knowledge. Everything in these courses was new to me. All of them began with readings from Plato and Aristotle, and I wanted to learn all I could about the Greeks. I took a sequence in Ancient History, and became a history major. Though I had no real idea what a professional historian does, I had learned that one can make a living by pursuing one’s intellectual interests and writing about them. I began to think about an academic career.  I obtained a Woodrow Wilson Doctoral Fellowship, and entered the graduate program in History at the University of California. With no Greek or French and minimal Latin and German, I was in no position to pursue my classical interests, so I began work at Berkeley with little more than an open mind. The most exciting modern historian I had read at Chicago had been the Belgian historian Henri Pirenne, whose account of the end of the Roman era stressed the continuity of economic life in the face of major political disruptions. For me, Pirenne’s shift of focus away from emperors and dreary Merovingian kings and on to the daily lives of private citizens was novel and exciting, and fit my sense of what was important. At Berkeley, I took courses in Economic History and audited an economic theory course. I liked economics at once, but it was obvious that to apply it with any confidence I would need to know much more than I could pick up on the side as a history student. I decided to move into economics and, since there appeared to be no hope of financial support from Berkeley’s Economics Department, I returned to Chicago. During the rest of that academic year I took some undergraduate economics at Chicago and one or two graduate courses, to prepare for my real start as a graduate student the next fall.  It was lucky for me that one of my undergraduate texts referred to [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/index.html)‘s Foundations of Economic Analysis as “the most important book in economics since the war.” Both the mathematics and the economics in Foundations were way over my head, but I was too ambitious to spend my summer on the second most important book in economics, and Samuelson’s confident and engaging style kept me going. All my spare time that summer went in to working through the first four chapters, line by line, going back to my calculus books when I needed to. By the beginning of fall quarter I was as good an economic technician as anyone on the Chicago faculty. Even more important, I had internalized Samuelson’s standards for when an economic question had been properly posed and when it had been answered, and was in a position to take charge of my own economic education.  In the fall of 1960, I began [Milton Friedman](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1976/index.html)‘s price theory sequence. I had been looking forward to this famous course all summer, but it was far more exciting than anything I had imagined. What made it so? Many Chicago students have tried to answer this question. Certainly Friedman’s brilliance and intensity, and his willingness to follow his economic logic wherever it led all played a role. After every class, I tried to translate what Friedman had done into the mathematics I had learned from Samuelson. I knew I would never be able to think as fast as Friedman, but I also knew that if I developed a reliable, systematic way for approaching economic problems I would end up at the right place.  Friedman’s course ended my long career as a conscientious, near-straight A student. Now if a course did not promise to be a life-changing experience, I lost interest and attended only sporadically. I accumulated many C’s, but also a lot of time to pursue what I found interesting. I took my first rigorous analysis courses, and a statistics course using Volume I of William Feller’s An Introduction to Probability Theory and Its Applications. I still pick up Feller’s book from time to time, as I do Samuelson’s, just for the pleasure of the author’s company.  There was also plenty of interesting economics going on at Chicago. My interest in probability and statistics stemmed from an interest in econometrics, stimulated by courses of Zvi Griliches and Gregg Lewis. Donald Bear, a new Assistant Professor from Stanford, taught a valuable mathematical economics course, and gave valuable encouragement to technically inclined students. Arnold Harberger’s sequence in public finance was a lasting influence on me too. My thesis, which used data from U.S. manufacturing to estimate elasticities of substitution between capital and labor, was written under Harberger and Lewis, and was part of a larger project of Harberger’s analyzing the effects of various changes in the U.S. tax structure.  There was a terrific collection of students at Chicago in the early 1960s. My closest friends were Glen Cain, Neil Wallace, Sherwin Rosen, and G.S. Maddala, and there were many others who now have international reputations. For many of us, the shock wave of Friedman’s libertarian-conservative ideas forced a rethinking of our whole social philosophy. Intense student discussions ranged far beyond technical economics. I tried to hold on to the New Deal politics I had grown up with, and remember voting for Kennedy in 1960. “Nixon? Bob, you couldn’t,” my sister had said, and she was right (for then!). But however we voted, Friedman’s students came away with the sense that we had acquired a powerful apparatus for thinking about economic and political questions.  In 1963 Richard Cyert, the new Dean of the Graduate School of Industrial Administration at Carnegie Institute of Technology (now Carnegie-Mellon University), offered me a faculty position. I had met Allan Meltzer and Leonard Rapping at my job seminar there, and I knew GSIA would be a stimulating and congenial place for me. GSIA’s leading intellectual figure was [Herbert Simon](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1978/index.html). Although Simon was no longer working in economics when I came to Carnegie, he was always ready to talk about economics (or any other area of social or management science) at lunch or coffee. He gave all of us at GSIA the feeling of being in the major leagues, and helped us to outgrow the sense that all the important work was going on at Chicago or Cambridge.  Once my thesis was finished, I began theoretical work on the decisions of business firms to invest in physical capital and in improved technology. Dale Jorgenson had served on my Chicago thesis committee, and his work on investment had stimulated me. I spent a lot of time in my first years at Carnegie Tech learning the mathematics of dynamical systems and optimization over time, and trying to see how these methods could best be applied to economic questions. Economists of my cohort all over the world were engaged in this enterprise in the 1960s, and I remember exciting conferences on this theme at Chicago and Yale, led by Hirofumi Uzawa.  During my years there, Carnegie-Mellon had a remarkable group of economists interested in dynamics and the formation of expectations. Foremost, of course, was John Muth, my colleague in my first three years there. Morton Kamien and Nancy Schwartz had come from Purdue about the time I came from Chicago. Dick Roll, a student of [Eugene Fama](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2013/)‘s at Chicago, brought the ideas of efficient market theory to GSIA. Thomas Sargent came to Carnegie-Mellon from Harvard in the middle of writing his thesis, and I remember the discussions he and Roll had about interest rates (that none of the rest of us could follow). Morris DeGroot taught a course in statistical decision theory that influenced Edward Prescott, and through Ed, me. John Bossons and later Michael Lovell studied direct evidence on expectations. It would be hard to think of a better group of colleagues, given my interests in economic dynamics.  At Carnegie I became involved in two collaborations, both of which bore immediate fruit and also influenced my thinking for years afterward. One of these was a project with Leonard Rapping, my closest friend and colleague at that time, in which we undertook to provide a neoclassical account of the behavior of U.S. wages and employment from 1929 to 1958. The paper was a bolder step into new territory than I would have taken then on my own, and the project never would have been undertaken or completed without Leonard’s confidence and his expertise in labor economics.  Edward Prescott had come to GSIA as a doctoral student in the same year I joined the faculty, and we were immediate friends. A few years later, when Ed had become a faculty member at Penn, I enlisted his help on a theoretical project I had begun on the dynamics of an imperfectly competitive industry. That problem defeated us, but in the course of failing to solve it we found ourselves talking and corresponding about everything in economic dynamics. In a couple of years we learned large chunks of modern general equilibrium theory, functional analysis, and probability theory, and wrote a paper, “Investment under Uncertainty,” that reformulated John Muth’s idea of rational expectations in a useful way. During this brief period my whole point of view of economic dynamics took form (along with Ed’s), in a way that has served me well ever since.  David Cass, who came to Carnegie-Mellon in 1971, had earlier aroused my interest in Samuelson’s overlapping generations model of a monetary economy. At about the same time, [Edmund Phelps](https://www.nobelprize.org/nobel_prizes/economics/laureates/2006/index.html) convinced me that Rapping’s and my model of labor supply needed to be situated in a general equilibrium context. These influences, combined with much that I had learned working with Prescott, came together in my paper, “Expectations and the Neutrality of Money,” which was completed in 1970 and published in 1972. The role of this paper, certainly the most influential of my writings, is one of the subjects of my Nobel lecture. In May, 1995, Rao Aiyagari organized a 25th Anniversary Conference for this paper, sponsored by the Federal Reserve Bank of Minneapolis. This occasion ranks high among the professional pleasures and honors I have received.  In 1974 I returned to Chicago as a faculty member. In 1980 I became the John Dewey Distinguished Service Professor at Chicago, the position I hold today. Chicago has been a marvellous place for me, as I knew it would be from my student experiences, and I have been stimulated by colleagues and graduate teaching into research on monetary theory, international-trade, fiscal policy, and economic growth: all the basic topics in macroeconomics. But the main features of one’s approach to science, like the main features of one’s personality more generally, are set early on. For me, the influences of my parents, my undergraduate and graduate years at Chicago, and my years at Carnegie Mellon were critical, so it is these influences I have focused on here.  I have had a rewarding personal life, intertwined with the intellectual life that I have described in these notes. Rita Cohen, also an undergraduate at Chicago, and I were married in New York in August, 1959, just before I began graduate studies at Berkeley. Our son Stephen was born in Chicago in September, 1960. Our son Joseph was born in Pittsburgh in January, 1966. Steve is now a securities trader at the Chemical Bank in New York. Joe is a graduate student in History at Boston University, and his wife Tanya is a resident at Beth Israel Hospital in Boston. Rita and I were separated in 1982, and divorced several years later.  Since 1982 I have lived with Nancy Stokey, who is now a colleague of mine at Chicago. We have collaborated in papers on growth theory, public finance, and monetary theory. Our monograph, Recursive Methods in Economic Dynamics, was published in 1989. Since then, our collaboration has been a domestic one only . We have an apartment on Chicago’s north side, and a summer house on Lake Michigan, in Door County, Wisconsin.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1995, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1996  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.  Robert E. Lucas Jr. passed away on 15 May 2023. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0858 |
| Interview |  |
|  |  |
| ID | 0859 |
| Biographical | I was born in Budapest, Hungary, on May 29, 1920. The high school my parents chose for me was the Lutheran Gymnasium in Budapest, one of the best schools in Hungary, with such distinguished alumni as John von Neumann and [Eugene Wigner](https://www.nobelprize.org/prizes/physics/1963/wigner/facts/). I was very happy in this school and received a superb education. In 1937, the year I graduated from it, I won the First Prize in Mathematics at the Hungary-wide annual competition for high school students.  My parents owned a pharmacy in Budapest, which gave us a comfortable living. As I was their only child, they wanted me to become a pharmacist. But my own preference would have been to study philosophy and mathematics. Yet, in 1937 when I actually had to decide my field of study, I chose pharmacy in accordance with my parents’ wishes. I did so because Hitler was in power in Germany, and his influence was steadily increasing also in Hungary. I knew that as a pharmacy student I would obtain military deferment. As I was of Jewish origin, this meant that I would not have to serve in a forced labor unit of the Hungarian army.  As a result, I did have military deferment until the German army occupied Hungary in March 1944. Then I did have to serve in a labor unit from May to November 1944.  In that November the Nazi authorities finally decided to deport my labor unit from Budapest to an Austrian concentration camp, where most of my comrades eventually perished. But I was lucky enough to make my escape from the railway station in Budapest, just before our train left for Austria. Then a Jesuit father I had known gave me refuge in the cellar of their monastery.  In 1946 I re-enrolled at the University of Budapest in order to obtain a Ph.D. in philosophy with minors in sociology and in psychology. As I got credit for my prior studies in pharmacy, I did get my Ph.D. in June 1947, after only one more year of course work and after writing a dissertation in philosophy.  From September 1947 to June 1948 I served as a junior faculty member at the University Institute of Sociology. There I met Anne Klauber, a psychology student who attended a course I was teaching and who later became my wife. But in June 1948, I had to resign from the Institute because the political situation no longer permitted them to employ an outspoken anti-Marxist as I had been.  Yet Anne did go on with her studies. But she was continually harassed by her Communist classmates to break up with me because of my political views, but she did not. This made her realize, before I did, that Hungary was becoming a completely Stalinist country, and that the only sensible course of action for us was to leave Hungary.  Actually we did so only in April 1950. We had to cross the Hungarian border illegally over a marshy terrain, which was less well guarded than other border areas. But even so, we were very lucky not to be stopped or shot at by the Hungarian border guards.  After waiting in Austria for our Australian landing permits for several months, we actually reached Sydney, Australia, on December 30, 1950. On January 2, 1951, Anne and I got married. Her unfailing emotional support and her practical good sense have always been a great help to me.  As my English was not very good and as my Hungarian university degrees were not recognized in Australia, during most of our first three years there I had to do factory work. But in the evening I took economics courses at the University of Sydney. (I changed over from sociology to economics because I found the conceptual and mathematical elegance of economic theory very attractive.) I was given some credit for my Hungarian university courses so that I had to do only two years of further course work and had to write an M.A. thesis in economics in order to get an M.A. I did receive the degree late in 1953.  Early in 1954 I was appointed Lecturer in Economics at the University of Queensland in Brisbane. Then, in 1956, I was awarded a Rockefeller Fellowship, enabling me and Anne to spend two years at Stanford University, where I got a Ph.D. in economics, whereas Anne got an M.A. in psychology. I had the good fortune of having [Ken Arrow](https://www.nobelprize.org/prizes/economic-sciences/1972/arrow/facts/) as advisor and as dissertation supervisor. I benefitted very much from discussing many finer points of economic theory with him. But I also benefitted substantially by following his advice to spend a sizable part of my Stanford time studying mathematics and statistics. These studies proved very useful in my later work in game theory.  In 1958 Anne and I returned to Australia, where I got a very attractive research position at the Australian National University in Canberra. But soon I felt very isolated because at that time game theory was virtually unknown in Australia. I turned to Ken Arrow for help. With his and [Jim Tobin’s](https://www.nobelprize.org/prizes/economic-sciences/1981/tobin/facts/) help, I was appointed Professor of Economics at Wayne State University in Detroit. Then, in 1964, I became at first Visiting Professor and then Professor at the Business School of the University of California in Berkeley. Later my appointment was extended also to the Department of Economics. Our only child Tom was born in Berkeley.  In the early 1950s I published papers on the use of von Neumann-Morgenstern utility functions in welfare economics and in ethics and on the welfare economics of variable tastes.  My interest in game-theoretic problems in a narrower sense was first aroused by [John Nash’s](https://www.nobelprize.org/prizes/economic-sciences/1994/nash/facts/) four brilliant papers, published in the period 1950-53, on cooperative and on noncooperative games, on two-person bargaining games and on mutually optimal threat strategies in such games, and on what we now call Nash equilibria.  In 1956 I showed the mathematical equivalence of Zeuthen’s and of Nash’s bargaining models and stated algebraic criteria for optimal threat strategies.  In 1963 I extended the Shapely value to games without transferable utility and showed that my new solution concept was a generalization both of the Shapley value and of Nash’s bargaining solution with variable threats.  In a three-part paper published in 1967 and 1968, I showed how to convert a game with incomplete information into one with complete yet imperfect information, so as to make it accessible to game-theoretic analysis.  In 1973 I showed that “almost all” mixed-strategy Nash equilibria can be reinterpreted as pure-strategy strict equilibria of a suitably chosen game with randomly fluctuating payoff functions.  I also published a number of papers on utilitarian ethics.  I published four books. One of them, Rational Behavior and Bargaining Equilibrium in Games and Social Situations (1977), was an attempt to unify game theory by extending the use of bargaining models from cooperative games also to noncooperative games. Two books, Essays on Ethics, Social Behavior, and Scientific Explanation (1976), and Papers in Game Theory (1982), were collections of some of my journal articles. Finally, A General Theory of Equilibrium Selection in Games (1988) was a joint work with [Reinhard Selten](https://www.nobelprize.org/prizes/economic-sciences/1994/selten/facts/). Its title indicates its content.  Let me add that in 1993 and 1994 I wrote two, as yet unpublished papers, proposing a new theory of equilibrium selection. My 1993 paper does so for games with complete information, while my 1994 paper does so for games with incomplete information. My new theory is based on our 1988 theory but is a much simpler theory and is in my view an intuitively more attractive one.  I am a member of the National Academy of Sciences, and a Fellow of the American Academy of Arts and Sciences and of the Econometric Society, as well as a Distinguished Fellow of the American Economic Association. In 1965-66 I was a Fellow of the Center for Advanced Study in the Behavioral Sciences at Stanford. I have an honorary degree of Doctor of Science from Northwestern University. After my retirement from my university, Reinhard Selten edited a volume in my honor with the help of H. W. Brock. It has the title, Rational Interaction.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1994, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1995  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.  John C. Harsanyi died on August 9, 2000. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0859 |
| Interview |  |
|  |  |
| ID | 0860 |
| Biographical | My beginning as a legally recognized individual occurred on June 13, 1928 in Bluefield, West Virginia, in the Bluefield Sanitarium, a hospital that no longer exists. Of course I can’t consciously remember anything from the first two or three years of my life after birth. (And, also, one suspects, psychologically, that the earliest memories have become “memories of memories” and are comparable to traditional folk tales passed on by tellers and listeners from generation to generation.) But facts are available when direct memory fails for many circumstances.  My father, for whom I was named, was an electrical engineer and had come to Bluefield to work for the electrical utility company there which was and is the Appalachian Electric Power Company. He was a veteran of WW1 and had served in France as a lieutenant in the supply services and consequently had not been in actual front lines combat in the war. He was originally from Texas and had obtained his B.S. degree in electrical engineering from Texas Agricultural and Mechanical (Texas A. and M.).  My mother, originally Margaret Virginia Martin, but called Virginia, was herself also born in Bluefield. She had studied at West Virginia University and was a school teacher before her marriage, teaching English and sometimes Latin. But my mother’s later life was considerably affected by a partial loss of hearing resulting from a scarlet fever infection that came at the time when she was a student at WVU.  Her parents had come as a couple to Bluefield from their original homes in western North Carolina. Her father, Dr. James Everett Martin, had prepared as a physician at the University of Maryland in Baltimore and came to Bluefield, which was then expanding rapidly in population, to start up his practice. But in his later years Dr. Martin became more of a real estate investor and left actual medical practice. I never saw my grandfather because he had died before I was born but I have good memories of my grandmother and of how she could play the piano at the old house which was located rather centrally in Bluefield.  A sister, Martha, was born about two and a half years later than me on November 16, 1930.  I went to the standard schools in Bluefield but also to a kindergarten before starting in the elementary school level. And my parents provided an encyclopedia, Compton’s Pictured Encyclopedia, that I learned a lot from by reading it as a child. And also there were other books available from either our house or the house of the grandparents that were of educational value.  Bluefield, a small city in a comparatively remote geographical location in the Appalachians, was not a community of scholars or of high technology. It was a center of businessmen, lawyers, etc. that owed its existence to the railroad and the rich nearby coal fields of West Virginia and western Virginia. So, from the intellectual viewpoint, it offered the sort of challenge that one had to learn from the world’s knowledge rather than from the knowledge of the immediate community.  By the time I was a student in high school I was reading the classic “Men of Mathematics” by E.T. Bell and I remember succeeding in proving the classic Fermat theorem about an integer multiplied by itself p times where p is a prime.  I also did electrical and chemistry experiments at that time. At first, when asked in school to prepare an essay about my career, I prepared one about a career as an electrical engineer like my father. Later, when I actually entered Carnegie Tech. in Pittsburgh I entered as a student with the major of chemical engineering.  Regarding the circumstances of my studies at Carnegie (now Carnegie Mellon U.), I was lucky to be there on a full scholarship, called the George Westinghouse Scholarship. But after one semester as a chem. eng. student I reacted negatively to the regimentation of courses such as mechanical drawing and shifted to chemistry instead. But again, after continuing in chemistry for a while I encountered difficulties with quantitative analysis where it was not a matter of how well one could think and understand or learn facts but of how well one could handle a pipette and perform a titration in the laboratory. Also the mathematics faculty were encouraging me to shift into mathematics as my major and explaining to me that it was not almost impossible to make a good career in America as a mathematician. So I shifted again and became officially a student of mathematics. And in the end I had learned and progressed so much in mathematics that they gave me an M. S. in addition to my B. S. when I graduated.  I should mention that during my last year in the Bluefield schools that my parents had arranged for me to take supplementary math. courses at Bluefield College, which was then a 2-year institution operated by Southern Baptists. I didn’t get official advanced standing at Carnegie because of my extra studies but I had advanced knowledge and ability and didn’t need to learn much from the first math. courses at Carnegie.  When I graduated I remember that I had been offered fellowships to enter as a graduate student at either Harvard or Princeton. But the Princeton fellowship was somewhat more generous since I had not actually won the Putnam competition and also Princeton seemed more interested in getting me to come there. Prof. A.W. Tucker wrote a letter to me encouraging me to come to Princeton and from the family point of view it seemed attractive that geographically Princeton was much nearer to Bluefield. Thus Princeton became the choice for my graduate study location.  But while I was still at Carnegie I took one elective course in “International Economics” and as a result of that exposure to economic ideas and problems, arrived at the idea that led to the paper “The Bargaining Problem” which was later published in Econometrica. And it was this idea which in turn, when I was a graduate student at Princeton, led to my interest in the game theory studies there which had been stimulated by the work of von Neumann and Morgenstern.  As a graduate student I studied mathematics fairly broadly and I was fortunate enough, besides developing the idea which led to “Non-Cooperative Games”, also to make a nice discovery relating to manifolds and real algebraic varieties. So I was prepared actually for the possibility that the game theory work would not be regarded as acceptable as a thesis in the mathematics department and then that I could realize the objective of a Ph.D. thesis with the other results.  But in the event the game theory ideas, which deviated somewhat from the “line” (as if of “political party lines”) of von Neumann and Morgenstern’s book, were accepted as a thesis for a mathematics Ph.D. and it was later, while I was an instructor at M.I.T., that I wrote up Real Algebraic Manifolds and sent it in for publication.  I went to M.I.T. in the summer of 1951 as a “C.L.E. Moore Instructor”. I had been an instructor at Princeton for one year after obtaining my degree in 1950. It seemed desirable more for personal and social reasons than academic ones to accept the higher-paying instructorship at M.I.T.  I was on the mathematics faculty at M.I.T. from 1951 through until I resigned in the spring of 1959. During academic 1956 – 1957 I had an Alfred P. Sloan grant and chose to spend the year as a (temporary) member of the Institute for Advanced Study in Princeton.  During this period of time I managed to solve a classical unsolved problem relating to differential geometry which was also of some interest in relation to the geometric questions arising in general relativity. This was the problem to prove the isometric embeddability of abstract Riemannian manifolds in flat (or “Euclidean”) spaces. But this problem, although classical, was not much talked about as an outstanding problem. It was not like, for example, the 4-color conjecture.  So as it happened, as soon as I heard in conversation at M.I.T. about the question of the embeddability being open I began to study it. The first break led to a curious result about the embeddability being realizable in surprisingly low-dimensional ambient spaces provided that one would accept that the embedding would have only limited smoothness. And later, with “heavy analysis”, the problem was solved in terms of embeddings with a more proper degree of smoothness.  While I was on my “Sloan sabbatical” at the IAS in Princeton I studied another problem involving partial differential equations which I had learned of as a problem that was unsolved beyond the case of 2 dimensions. Here, although I did succeed in solving the problem, I ran into some bad luck since, without my being sufficiently informed on what other people were doing in the area, it happened that I was working in parallel with Ennio de Giorgi of Pisa, Italy. And de Giorgi was first actually to achieve the ascent of the summit (of the figuratively described problem) at least for the particularly interesting case of “elliptic equations”.  It seems conceivable that if either de Giorgi or Nash had failed in the attack on this problem (of a priori estimates of Holder continuity) then that the lone climber reaching the peak would have been recognized with mathematics’ Fields medal (which has traditionally been restricted to persons less than 40 years old).  Now I must arrive at the time of my change from scientific rationality of thinking into the delusional thinking characteristic of persons who are psychiatrically diagnosed as “schizophrenic” or “paranoid schizophrenic”. But I will not really attempt to describe this long period of time but rather avoid embarrassment by simply omitting to give the details of truly personal type.  While I was on the academic sabbatical of 1956-1957 I also entered into marriage. Alicia had graduated as a physics major from M.I.T. where we had met and she had a job in the New York City area in 1956-1957. She had been born in El Salvador but came at an early age to the U.S. and she and her parents had long been U.S. citizens, her father being an M. D. and ultimately employed at a hospital operated by the federal government in Maryland.  The mental disturbances originated in the early months of 1959 at a time when Alicia happened to be pregnant. And as a consequence I resigned my position as a faculty member at M.I.T. and, ultimately, after spending 50 days under “observation” at the McLean Hospital, travelled to Europe and attempted to gain status there as a refugee.  I later spent times of the order of five to eight months in hospitals in New Jersey, always on an involuntary basis and always attempting a legal argument for release.  And it did happen that when I had been long enough hospitalized that I would finally renounce my delusional hypotheses and revert to thinking of myself as a human of more conventional circumstances and return to mathematical research. In these interludes of, as it were, enforced rationality, I did succeed in doing some respectable mathematical research. Thus there came about the research for “Le Probleme de Cauchy pour les E’quations Differentielles d’un Fluide Generale”; the idea that Prof. Hironaka called “the Nash blowing-up transformation”; and those of “Arc Structure of Singularities” and “Analyticity of Solutions of Implicit Function Problems with Analytic Data”.  But after my return to the dream-like delusional hypotheses in the later 60’s I became a person of delusionally influenced thinking but of relatively moderate behavior and thus tended to avoid hospitalization and the direct attention of psychiatrists.  Thus further time passed. Then gradually I began to intellectually reject some of the delusionally influenced lines of thinking which had been characteristic of my orientation. This began, most recognizably, with the rejection of politically-oriented thinking as essentially a hopeless waste of intellectual effort.  So at the present time I seem to be thinking rationally again in the style that is characteristic of scientists. However this is not entirely a matter of joy as if someone returned from physical disability to good physical health. One aspect of this is that rationality of thought imposes a limit on a person’s concept of his relation to the cosmos. For example, a non-Zoroastrian could think of Zarathustra as simply a madman who led millions of naive followers to adopt a cult of ritual fire worship. But without his “madness” Zarathustra would necessarily have been only another of the millions or billions of human individuals who have lived and then been forgotten.  Statistically, it would seem improbable that any mathematician or scientist, at the age of 66, would be able through continued research efforts, to add much to his or her previous achievements. However I am still making the effort and it is conceivable that with the gap period of about 25 years of partially deluded thinking providing a sort of vacation my situation may be atypical. Thus I have hopes of being able to achieve something of value through my current studies or with any new ideas that come in the future.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1994, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1995  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.  John F. Nash Jr. died on 23 May 2015. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0860 |
| Interview |  |
| Q6 | It has passed ten years since you received the Economics Prize. What impact has it had on your professional and maybe on your private life over the last ten years? |
|  | Well, it’s almost ten years. It has had a tremendous impact on my life, more than on the life of most Prize winners because I was in an unusual situation. I was unemployed at the time. I was in good health, but I had reached the age of 66 and beginning to get social security, but I didn’t have much of that, I had many years of unemployment before me. And so I was in a position to be very much influenced by the recognition of my earlier work came about in this way. I had become widely known, but in a sense it wasn’t officially recognised. I was quoted very frequently in the literature of economics and mathematics, but it’s quite different to get official recognition. It transformed my life. |
| Q7 | Do you have any special memories from the time, -94, when you went to Stockholm to receive the Prize? Anything particular that you would like to share with us? |
|  | I have many memories, but I don’t know what I should say. It was very remarkable to be in Stockholm in December. It’s when the days are getting very short and I was there for St Lucia’s Day, which is where there’s a little premature celebration of returning light. It’s ten days early in fact. But you could see in the afternoon it began getting dark around three o’clock and so that was really a phenomenon that I had not observed before at lower latitudes. |
| Q4 | You said in your speech, I believe, or maybe it was later in your biography, that you were hoping, by having now the age of 66, that you would achieve much more professionally. Do you feel that you have had the opportunity and the time to do that, or have your time been mainly taken up by giving lectures or travelling around, being a celebrity? |
|  | Much of my time has been taken up in that way, but I have been making progress in the other sense. I haven’t completed so much. I have a paper published on ideal money, which I hope to follow up with another paper extending the ideas, and I really feel I’ve just discovered a … It’s hard to describe it … There has been a deterioration in the quality of money that’s observable in many areas, and it brings up questions. Like you could see how money is different all of a sudden in Italy when they had the lire and now they have the euro. So they, in a revolutionary way, have gone from bad money to good money comparatively. But what about the rest of the world?  Even the dollar has some questions around today.  John Nash: Oh yes, It’s not what it was. The dollar used to be a gold standard currency. And the dollar is really good in the last century, I mean in the 19th century. |
| Q18 | I find this very fascinating what you’re talking about. What are the reasons for this, would you say, that the quality of money has sort of changed? |
|  | Well, it’s complicated. I think we might get into too much deep water here … And I don’t want to compete with [Professor Mundell](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1999/mundell-facts.html) in this media. I’m going to be listening to him and learning things when he lectures.  That’s a fascinating subject.  John Nash: Oh yes. And I have other areas of research I’m about to publish and my work really relates to game theory, a project … and I have other areas of study. I don’t know how long I will live and how much I will be able to do, but at least I am active. |
| Q37 | I was thinking that, when you were a child … I have spoken to some of the Prize winners over the last couple of years, and many of them were not at all interested in the subject that they finally got very deep into. How was it for you? Were you very interested in maths, for example? Did you feel that you have a special gift for that? |
|  | I did have an appreciation of maths and science as a child, and even at elementary school I would like to do more in mathematics than the other students were doing in one way or another. So I did have that at an early stage. Like maybe almost comparable to Mozart in music. Of course he had a father who was a musician  Was that recognised at an early stage in your case, your special gift and your talent?  John Nash: It’s recognised … my parents realised that I was mentally special and of course things developed. Where I lived the school system wasn’t really adapted for special students and so there wasn’t much to do except you go through that and then go on … from the town. |
| Q38 | Do you think it’s better now that there are maybe special schools for specially talented children so that one can really give them what they need? |
|  | There are many complicated issues coming into education. I don’t know that I want to express an opinion. You have to have education in different locations. You have the cities and you have the smaller areas that present different problems. The students can reach the level that they need to reach ultimately, and sometimes if they try to go into it too early, though they might, then they don’t do so well finally. So I don’t know if it matters so much. Of course the American education system is very inefficient in many ways compared to other countries in Europe or Japan, but it works in such a way that at least the few people who are going onto unusual careers and science can manage to get into that even though they go through an earlier stage that doesn’t give them much.  Could almost be seen as a waste of time for some?  John Nash: Oh, it is. It’s like baby sitters … Sort of like you have kindergarten when it should be the elementary school. |
| Q6 | You were very young when you made your discovery that you were given the Prize for. What was the breakthrough for you when you were looking into that problem and, briefly, how did you realise what you were achieving at the time? |
|  | Well, there were special circumstances with regard to what’s called the Nash equilibrium, and that’s the main /- – -/ topic. I happened to be a graduate student in Princeton and von Neumann and Morgenstern were residents of Princeton, and they had written the book, Theory of Games and Economic Behavior, which in turn depended on some earlier work in France, Emile Borel, and then going back even to people like Pierre de Fermat and Pascal and who studied probabilities. And so I had the opportunity to describe something different from the procedure that von Neumann and Morgenstern, and I wrote it up, but I could use much of their ideas, their concept of strategies and utility, part of which in themselves they had been adopted from previous study by Borel, and so I could develop this thing which was first called ‘Equilibrium Points in N-person Games’ and then, ‘Non-Cooperative Games’ was my final thesis title.  So it was very fortunate that I happened to be in that location. There was a seminar at the university that was concerned with work relating to the work of von Neumann and Morgenstern. So everything went together and I had also some other work relating to economics which linked in. This is what you call the bargaining problem, and that is also important, but it wasn’t cited for the Nobel Prize because I was linked with also with Harsanyi and Selten in the Prize and they were linked mostly with the equilibrium concept and everyone would think that Harsanyi also had done work in the bargaining cooperative games and Selten has done work in the experimental games. And he would talk on that here in this meeting.  id you realise at the time how big this discovery was?  I knew I had a good idea that was well worth publishing …  John Nash: I knew I had a good idea that was well worth publishing and it sort of expanded von Neumann and Morgenstern’s zero-sum game, which of course von Neumann himself had done originally. But it’s hard to know what would come of something, how the future would be, and of course as far as the Nobel Prize at that time there was nothing in economics that one couldn’t look forward to there. So that in -68 you started those, so that is entirely a matter of the fortunes of history. |
| Q10 | If you were to give advice to young students today, what fields should they look into if they are interested in economics? |
|  | With regard to the specific sub-fields of economics, I don’t know so well. I can observe the game theory is applied very much in economics. Generally, it would be wise to get into the mathematics as much as seems reasonable because the economists who use more mathematics are somehow more respected than those who use less. That’s the trend.  I don’t think exactly like a professional economist. I think about economics and economic ideas, but somewhat like an outsider. Of course von Neumann was not an economist but Morgenstern was, and they teamed together on that book. Otherwise there are a lot of trends in economics. What seems fashionable now and the general opinion might be quite different after 20 years or so. Somebody studying a career they should be prepared for changes. I think they should learn things that are good foundations but don’t necessarily depend on a current fashion or what could be considered general opinion or popular opinion. You should maybe try to learn things that would be good for all time. Unquestionable scientific value. |
| Q72 | Are there any issues in the world today that you are concerned of? For example, if we look at the disparity between the first so called First World and Third World. If we look at the political situation and what comes with it, for example, the US budget deficit, and its reasons for that … Are there any of those issues that concerns you as a scientist and that you give extra thought? |
|  | You mentioned some different things here like the question of the First World, Third World and what happened to the Second World? The US budget, those are quite different topics.  Certainly they are. There’s a broad span.  John Nash: I think they require different doctors to deal with them.  Is there anything that you are particularly concerned of or …?  John Nash: Well, these are popular themes, but you find something that people are talking about and you may find that there are differing opinions. There’s the most widely held opinion, but there are maybe some other opinion that is more scientific or more subtle. It is easy to say that there are the rich and the poor and so something should be done. But in history there are always the rich and the poor. If the poor were not as poor we would still call them the poor. I mean whoever has less can be called the poor. You will always have the 10% that have less and the 10% that have the most. But maybe comparatively they’re not so bad.  Man has existed for much longer than has this technology.  I was thinking about these things when I saw India for the first time. I think it was last year in January 2003, when driving through the countryside of India I could see areas where presumably the amount of recognised income, personal income, would be very low. But this gave me the thought, comparatively one sort of peasant or level of Indian and another one might not feel so bad. These are not the beggars on the street, but people out in the country. If they had as much as someone else in that area, if they are not so poor compared to their neighbours, and if you look through history, humans have lived under different conditions, they had to live maybe very, very primitively a few thousand years ago, so much of this living of the modern humans is very, very recent. Man has existed for much longer than has this technology. |
| Q71 | I think from what you’re saying it’s really comparative how much you need and there is a certain idea maybe in the so called Western World that we need more than we actually do need. It’s a certain greed which has kind of taken resources from so called Third World countries. Does it lead to wishes of further wealth which is maybe …? |
|  | The more in the richer countries … if they wish to consume more, this is likely to provide some business for other parts of the world. They will have maybe some imports. I mean if countries like the USA and Sweden are to import more, then that is business for other parts of the world, and maybe some choice products. But what makes it hard for the other parts of the world if the countries again like USA or Sweden can manufacture all that they need in manufactured goods, and furthermore produce all they need agriculturally so they don’t need to import anything and they have things to export.  That’s partly where we are today, isn’t it?  John Nash: There is a political issue about these subsidies for agriculture that European countries, which could import agriculture products with subsidised local agriculture and maybe produce the products there, like Germany may be growing a lot of wheat which they make into brot, and it might be we wouldn’t need to grow any wheat, they could import it from Russia or somewhere. And then that would provide business by trade. |
| Q27 | That’s right. It was partly concentrating on times which were very difficult in your life, or concentrating on that, and how do you feel about that version of your life that came across to the public and do you feel that there is a necessity for you to comment on that today? Or is it something that you left behind? |
|  | John Nash: The movie?  The movie?  … what can be called schizophrenia has an unfavourable course in history …  John Nash: The movie is easier to comment on. The movie … Of course the family … We received some money for co-operating and authorising the movie. The movie is in part an interpretation of how a case of mental illness can go or could be the nature of a case and how it could evolve. Of course most typically what you call mental illness or what can be called schizophrenia has an unfavourable course in history, in the sense that the people never really recovered to be what you could call mentally well. They become like what are called consumers of mental health organisations. They are always taking some sort of a pill. They likely are not in hospital, they get out of hospitals but they live in a form of life that’s for people who are not really highly functionable, they’re not functioning, they’re not really in the economy as self supporting people. They’re not living a normal life from an economic point of view. So in my case there was certainly a recovery of sanity and more a possibility of doing normal activities, and so it was an interesting case. And I’m not a mental patient now, I don’t take any medicine, but I have a son who unfortunately uses medication and is in this process of taking drugs and seeing psychiatrists and this sort of thing. We don’t know what the course of that will be.  But I was disturbed in this way for a very long period of time, like 25 years, and sort of starting around 30 so it was quite a portion of a life’s history.  And the movie gives an insight into how this sort of thing can go. The movie actually, at the end of it, it suggests that the person in the movie is still taking some medicine, and taking a modern type of a drug after he had tried not to take any, he’d struggled with the issue of taking. So there is a difference there. The director in the movie didn’t want to suggest that people who are living in a state with controlled mental illness, that they should stop taking their medicine. It would be dangerous to suggest this.  So it doesn’t correspond to me accurately. There had been many years since I haven’t had medicine, since 1970, that’s 35 years, well more than 30 years.  Is it important for you …  John Nash: But even before that, most of the time I didn’t really take the medicine, so there are different types of … it’s not the same. |
| Q27 | But a film which was so widely seen and I was thinking that has it been important for you to be able to give your version of your life as well or is it something that you feel you can … because when somebody goes in and make a film about somebody’s life then you might need to say: That’s the film, but reality is a little bit different. |
|  | The film is artistic and it doesn’t describe accurately the nature of the delusional thinking that was my history, and it interprets … It has someone who sees imaginary persons, sees that different persons are actually present there /- – -/ and that is not, that’s not even typical in schizophrenia. But that interprets the idea of delusions. More typically a person may hear voices, they’re talking with spirits or something which are not there. This is the form of more typical delusions. But you can’t illustrate that in a movie very well. I mean if the movie shows someone who can be seen, then the person seeing the movie can better understand it. This can occur in mental illness but it’s the less typical form. |
| Q3 | It’s fascinating to talk to you and I’m really happy that you have spoken to us today. Is there any concerns … I was talking earlier about young students and somebody said to me that the teachers are those who open up doors, but that are the students who have to walk through them themselves. Did you have any teacher through your period at university which was particularly a role model to you? How has it been for you? |
|  | I certainly had some good teachers who were very helpful to me and influential. For example, in economics I only took one economics course and I was an undergraduate study in Pittsburgh at what is now called Carnegie Mellon, but by coincidence the person who taught the course, it was a course in international economics, and by coincidence this was someone who came from Austria. So there’s actually to consider Austrian economics is like a different school than typical American or British. So I was by coincidence influenced by an Austrian economist which may have been a very good influence. |
| ID | 0861 |
| Biographical | I was born in Breslau on October 5th, 1930. At that time, Breslau, now called Wroclaw, belonged to Germany and only German was spoken there. After the second world war Breslau became Polish and the original German population was almost completely replaced by a Polish one. I have never visited Wroclaw after the war. Heavy fighting destroyed most of the town in which I grew up and most of the familiar places of my youth look different now.  When I was born my father owned a business called a “reading circle”; folders containing an assortment of magazines were lent to customers for one week, then recollected and lent out again. The older the folder, the lower was the fee. This was a florishing branch of industry. My father had built up his business in spite of the fact that he became blind at young years and had only three years of school education. Already in the mid-thirties he had to sell his firm because of his Jewish origin. Jews were forbidden to run a business connected to the press. My father did not belong to any religious community and my mother was a protestant. Originally my parents intended to let me grow up without any attachment to a particular religion in order to give me the opportunity to decide for myself later in my life. However, under the prevailing political circumstances it seemed to be better to have me baptized as a protestant. The ceremony is one of my early memories. Much later as a young man I left the protestant church and became unattached to any religion again. Unlike several other relatives my father did not become a victim of the holocaust, since he died after a serious illness already in 1942 before the worst of the terror began.  It was not easy for me to live as a half-Jewish boy under the Hitler regime. When I was 14 I had to leave high school and the opportunity to learn a trade was denied to me. The only career open to me was that of an unskilled worker. Fortunately it turned out that this did not matter much since after about half a year my mother, my brothers, my sister, and I left Breslau on one of the last trains before all outbound railway traffic stopped.  My situation as a member of an officially despised minority forced me to pay close attention to political matters very early in my life. Moreover I found myself in opposition to the political views shared by the vast majority of the population. I had to learn to trust my own judgment rather than official propaganda or public opinion. This was a strong influence on my intellectual development. My continuing interest in politics and public affairs was one of the reasons why I began to be interested in economics in my last high school years.  After we left Breslau we were refugees, first in Saxonia, then in Austria and finally in Hessia. Until schools opened again in 1946 I worked as a farm boy, first in Austria and later in the village in Hessia where we lived. In 1947, we moved to Melsungen, a small town in which I went to high school until 1951. In these years I developed a strong interest in mathematics. When we still lived in the village near Melsungen, I had to walk to school which took about three and a half hours there and back. During these walks I occupied my mind with problems of elementary geometry and algebra. I still like to hike in forested hills and to think while walking.  When I finished high school, it was clear to me that I would study mathematics, even if I also considered economics and psychology. It took me relatively long to reach my master’s degree in mathematics. My studies were not sufficiently concentrated on this goal. One of the reasons was that I went to many lectures which had nothing to do with my study of mathematics. However, it later turned out that some of these extracurricular activities became important for my career. I studied mathematics at the university of Frankfurt from 1951 to 1957. Until I completed my “Vordiplom”, the intermediate examination which roughly corresponds to the bachelor’s degree, I also had to study physics. Originally I considered to take astronomy as a minor for my master’s degree and I actually spent much time trying to get some knowledge of this field but now almost everything is forgotten. What finally turned me away from astronomy was that I became more and more involved in game theory and economics. I am grateful to the Natural Science Faculty of Frankfurt University for the decision to permit mathematical economics as a minor for the master’s degree in order to enable me to be the first one to take this choice.  My first contact with game theory was a popular article in Fortune Magazine which I read in my last high school year. I was immediately attracted to the subject matter and when I studied mathematics I found the fundamental book by von Neumann and Morgenstern in the library and studied it. Somewhat later I saw the announcement of a student seminar for economists on game theory, headed by Professor Ewald Burger who taught advanced mathematical courses but also mathematics for economists. I participated in the seminar and Ewald Burger gave me the chance to write a master’s thesis in cooperative game theory. He was a man of extraordinary mathematical erudition and an excellent teacher. I owe much to his guidance and to his patient advise.  My master’s thesis and later my Ph.D. thesis had the aim of axiomatizing a value for e-person games in extensive form. This work made me familiar with the extensive form, in a time when very little work on extensive games was done. This enabled me to see the perfectness problem earlier than others and to write the contributions for which I am now honored by the prize in memory of Alfred Nobel.  After I had received my master’s degree in 1957, I was hired by Professor Heinz Sauermann, an economist at the University of Frankfurt am Main, who employed me for ten years in various assistant positions. It was my task to do research funded by Deutsche Forschungsgemeinschaft, the German counterpart of the National Science Foundation. At first I was supposed to apply decision theory to the theory of the firm, but soon I became involved in economic laboratory experimentation. Fortunately the referees of Sauermann’s research proposals approved of this new research direction. This made it possible to finance a small group of young people doing experimental research. Sauermann had about 15 assistants and only two to four of them were involved in experiments. I became something like a foreman of this small detachment. Reinhard Tietz, Volker Haselbarth, Otwin Becker, Klaus Schuster and others belonged to it for longer or shorter periods.  Heinz Sauermann was a remarkable man. He was one of the first to propagate Keynesianism in Germany. In spite of a lack of mathematical training he encouraged his students to do work based on formal models. He always had a good feeling for the trends of the field and therefore was very successful in suggesting the right problem areas to those who did research under his supervision. Moreover he was an excellent administrator and scientific organizer, who did much for the propagation of experimental economics. I owe much to him.  In 1959, I married Elisabeth Langreiner, who for all the years since then helped me to become the person I am now. We would have liked to have children but we do not have any. We both belong to the Esperanto movement and this is how we met. The international language Esperanto has still an important influence on our life.  My first publication was a journal article with the title “Ein Oligopolexperiment” (an oligopoly experiment) written together with Heinz Sauermann and published in 1959. When we began to do experimental economics at Frankfurt, such a field had not yet existed. My attempts to learn some psychology while I studied mathematics had made me acquainted to experimental techniques. I had listened to lectures of the gestalt psychologist Edwin Rausch, who was a careful experimenter, and I had participated in psychological experiments as a subject. Therefore it seemed natural to me to try an experimental approach to oligopoly.  In 1961, I received my Ph.D. in mathematics at the University of Frankfurt am Main. Shortly afterwards Oskar Morgenstern made it possible for me to participate in a game theory conference at Princeton. In the late 50s – I do not remember the year – he had given a talk at Frankfurt and my remarks in the subsequent discussion must have impressed him. In the following years he sometimes asked me to meet him when his travels brought him to Frankfurt. He also gave me financial support for staying several weeks longer at Princeton after the game theory conference. My short visit to Princeton was important for my life since it gave me the opportunity to interact with R.J. Aumann and M. Maschler who were members of Morgenstern’s research group at that time.  Around 1958, I became aware of [H.A. Simon](https://www.nobelprize.org/prizes/economic-sciences/1978/simon/facts/)‘s seminal papers on bounded rationality and was immediately convinced by his arguments. I tried to construct a theory of boundedly rational multigoal decision making. Together with Heinz Sauermann, I worked out an “aspiration adaptation theory of the firm” which was published as a journal article in 1962. After the Princeton conference in 1961, I visited Pittsburgh for two days in order to establish contacts with H.A. Simon and his associates. The problem of bounded rationality has occupied my mind for a long time but unfortunately with less success than I had hoped for. More and more I came to the conclusion that purely speculative approaches like that of our paper of 1962 are of limited value. The structure of boundedly rational economic behavior cannot be invented in the armchair, it must be explored experimentally.  In the early 60’s I had run experiments on an oligopoly game with demand inertia. A game theoretical analysis of this model proved to be too difficult but I was able to solve a simplified version. I found a natural equilibrium but the game has many other equilibria. In order to describe the distinguishing features of my solution, I defined subgame perfectness. My paper, Ein Oligopolmodell mit Nachfrageträgheit (An Oligopoly Model with Demand Inertia) was published in 1965. At that time I did not suspect that it often would be quoted, almost exclusively for the definition of subgame perfectness. Very soon it became clear to me that the perfectness problem is not completely solved by this concept. Therefore in a paper published in 1975, I defined a refined notion of perfectness, now often referred to as trembling hand perfectness.  In 1965, I was invited to a game theory workshop at Jerusalem which lasted for three weeks and had only 17 participants, but among them all the important researchers in game theory, with few exceptions. Game theory was still a small field. We had heated discussions about Harsanyi’s new theory of games with incomplete information. This was the beginning of my long cooperation with [John C. Harsanyi](https://www.nobelprize.org/prizes/economic-sciences/1994/harsanyi/facts/). Not long after the conference I became a member of a group of game theorists hired by the research firm MATHEMATICA to work on projects for the Arms Control and Disarmament Agency. The group often met for several days near Washington D.C.. I cooperated with John C. Harsanyi on bargaining under incomplete information, but I also did other work on models of nuclear deterrence. The group did not produce anything of practical value for the Arms Control and Disarmament Agency, but nevertheless it was very successful because important theoretical advances, e.g. in the analysis of repeated games of incomplete information by Aumann, Maschler, and Stearns were made there.  In Germany the Ph. D. is not yet the last formal requirement for a career as a university teacher. In addition to this, one is expected to work towards a “habilitation”. For this purpose one presents a habilitation thesis, often a monography of an area of research. The habilitation is a permission to lecture independently. In my case the habilitation thesis was a monography on multiproduct pricing. In the academic year of 1967/68 I was visiting full professor at the business school of the University of California, Berkeley. I had completed my habilitation thesis shortly before I left to Berkeley and was habilitated when I came back. In 1970 my habilitation thesis was published as a book.  In 1969, I accepted an offer of the Free University of Berlin, where I was a full professor of economics until 1972. My wife and I liked to live in West Berlin. In these years Germany experienced a period of student unrest, which made teaching difficult and sometimes impossible. The student movement was especially strong at the Free University, but this was not the reason why I moved to the University of Bielefeld in 1972. I was attracted by plans to create a big Institute of Mathematical Economics. However, these plans could not be realized since it finally turned out that the money was not available. Eventually a small institute with only three professors was established. I was not unhappy with this solution since I succeeded to convince the appointment committee that all positions should be held by game theorists. The positions were filled by Joachim Rosenmuller, Wulf Albers, and myself. The concentration on game theory gave us a chance to get some international reputation.  My years at the University of Bielefeld were a productive time. My experimental research continued but I mainly worked on game theory and its application to industrial organization and other areas. After John Harsanyi and I had completed our work on bargaining under incomplete information we decided to attack the problem of selecting a unique equilibrium for every game. He twice came to Bielefeld for a year and I often visited Berkeley for short periods of one or two months. It took us about 18 years to construct a reasonable general theory of equilibrium selection in games. In this time we considered many ideas and rejected two fairly well worked out approaches. Our book of 1988 only describes the theory we finally agreed upon.  On my frequent visits to Berkeley I also had a cooperation with Tom Marschak which resulted in a book on multiproduct pricing published in 1974. I also did experimental work on bargaining under incomplete information together with Austin Hoggatt and his younger associates. In the basement of Barrows Hall at the University of California, Berkeley, Austin Hoggatt had built up the first computerized laboratory for experimental economics. There our experiments were run.  In the twelve years I spent at Bielefeld, I began a close cooperation with Werner Guth, who in some sense is one of my students, even if we never held positions at the same university. We worked on applications of the equilibrium selection theory by John Harsanyi and myself, long before it had reached a final form, but we also did research on other problems like wage bargaining in the framework of a business cycle model. Also other people who later became university professors sometimes came to Bielefeld to seek my advise, namely Ulrike Leopold from Graz, Joel Moulen from Lyon, and Eric van Damme from Eindhoven. Ulrike Leopold also worked on the application of equilibrium selection theory and I wrote some papers together with her. Joel Moulen did Ph.D. work on cooperative game theory and became a professor of mathematics at Jaounde, Kameroun. Eric van Damme needed very little advise and is now a well known game theorist.  One of my students, Jon-ren-Chen, a Taiwanese who was my assistent for many years, has never worked on game theory. He does applied econometric research on international trade and development. He was habilitated at Bielefeld and is now a Professor of Economics at Innsbruck. A student of mine, Rolf Stoecker, who was a promising young experimentalist left me after his Ph.D., joined an insurance company and became its chief executive after 5 years. Later something similar happened to me again in Bonn. My assistent Gerald Uhlich who had done important experimental work on coalition bargaining left the university after his Ph.D. and became the second man in a furniture textile factory. Nevertheless I still nourish the hope that some of the students who now work on experimental economics under my guidance will become university teachers.  At the University of Bielefeld, cross fertilization between different fields is favored by the existence of a unique institution, the center for interdisciplinary research. Talks given there brought me into contact with biologists who made me aware of applications of game theory to biology. A young mathematician, Peter Hammerstein, who had a junior position as a statistical advisor in the biology department made me accquainted with the notion of evolutionary stability. From that time on I developed a strong interest in biological game theory. One of my contributions to this field is the investigation of evolutionary stability in extensive games. However, I also wrote other papers in this area, some with Peter Hammerstein and others with Avi Shmida, a botanist at the Hebrew University of Jerusalem, with whom I cooperate on theoretical models of pollination of flowers by bees. Peter Hammerstein is now a well established theoretical biologist. Another student of mine, Franz Weissing, also started a career as a university teacher of biology.  I find it very interesting to cooperate with scientists in different fields who have little mathematical training but much substantial knowledge. My first experience of this kind was my cooperation with the political scientist Amos Perlmutter with whom I developed the scenario bundle method, a systematic way of constructing simple game models of concrete international conflicts. Unfortunately the results of our research have never been formally published. It is the advantage of this kind of interaction that the judgement of the expert on the empirical facts is not yet contaminated by mathematical models. I had a similar experience with Avi Shmida, even if he as a natural scientist is not quite as unmathematical.  I am grateful to Avi Shmida, not only for his scientific cooperation but also for another reason. Before I came into contact with theoretical problems in botany I hardly could distinguish any flower from any other one. However, I felt that I could not really do work on pollination problems without learning at least a little of the art of recognizing wild flowers. Since then I always carry a flower book on my hikes, except in the winter. I enjoy my often frustrated efforts to identify wild flowers. This activity has opened my eyes to the astonishing diversity and the marvelous beauty of flowering plants.  In 1984, I moved to the University of Bonn, where I am a Professor of Economics since then. I liked the interdisciplinary atmosphere at the University of Bielefeld, but I wanted to build up a computerized laboratory for experimental economics and Bonn was willing to offer me much better conditions in this respect. I came back to Bielefeld for the time from October 1, 1987, to September 30, 1988, in order to act as the organizer of a research year on “game theory in the behavioral sciences” at the center for interdisciplinary research. The cooperation of an international group of participants with backgrounds in economics, biology, mathematics, political science, psychology, and philosophy finally resulted in four volumes on “game equilibrium models” published in 1991.  At the University of Bonn my work and that of most of my assistants is concentrated on experimental economics. It is our goal to help to build up a descriptive branch of decision and game theory which takes the limited rationality of human behavior seriously. The financial support of the Deutsche Forschungsgemeinschaft in the framework of the Sonderforschungsbereich (special research unit) 303 enables us to work in this direction.  In 1991, it was discovered that both, I and my wife, suffer from diabetes. Probably we had this disease for some time without knowing it. As as consequence of diabetes my wife lost both legs up to the knee. Therefore she is now bound to the wheelchair. Moreover her eyesight has become very bad. Nevertheless she does many things in the house, even if everything takes much longer than it used to. She cooks and takes care of our three cats and, what is most important, she maintains a cheerful attitude towards life. We have learnt to adjust to our situation.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1994, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1995  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.  Reinhard Selten died on 23 August 2016. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0861 |
| Interview |  |
| Q6 | Professor Selten, very welcome to this interview here in Lindau. My very first question would really be about the day ten years back when the telephone rang and you were told that you were to be given the Economy Prize in Memory of Alfred Nobel, and you were to share it with Nash and Harsanyi. How did you react, what did you feel? |
|  | There was no telephone ringing. I was in the supermarket with my wife and we were shopping. So nobody was at home when the telephone was ringing. And usually you are called before it, but that since there was, since we were away so long, they then announced it, even if I was not yet knowing it.  So you didn’t know it?  Reinhard Selten:Didn’t know it, but it was already announced. So when I came home then, there are … a lot of people before my house and I thought: What happened? Maybe something bad happened here? Somebody broke in or something like that? Now people are collecting there for some reason, I didn’t know what. And I stopped the car and I went out of the car and somebody came to me and said I congratulate you. Gave hand. Congratulations, I said, for what? So will you explain to me?  What did you think then? Did you think it was hoax?  … I opened the door and then everybody is there, all journalists invading my house …  Reinhard Selten:No, I said to him Yes, I have to get out first my wife from the car and the wheelchair, to bring my things in, and I opened the door and then everybody is there, all journalists invading my house and so on. And it was so hectic that I didn’t have much time to think about it at all, not, I was just trying to cope with this situation.  Then the telephone was always ringing and all this and so … I didn’t really believe it, not at first. Not that it was a hoax, but I knew that it was true, but you don’t get emotionally adjusted to it immediately, no. You see that it is reported in television, but it has all an unreal flavour which for me something I couldn’t get really adjusted to. Took some time. And I only got adjusted to it when we went in Stockholm to the City Hall where there was this …  The big banquet?  Reinhard Selten:Yes, but actually the moment when we came near by car to the City Hall, then I began to feel that this is real … It’s the banquet and all of. But before, I mean, of course I knew that this had happened, I mean, it was intellectually clear, but it was not emotionally accepted still, no. |
| Q7 | Has it affected your life in a way that you at times maybe would even regret taking a lot of time away from your research? |
|  | Yes, it is a little like this because my presence is respected at many locations and I have to travel more than I would like, because, well, sometimes you like to do it, but in many cases you do it in order not to disappoint some people whom you think you should not disappoint and so, not that it is not good, you know, but I have difficulty to do my work no? I really get under pressure also at home. |
| Q5 | What was it in the game theory that really fascinated you? I’m not sure, I just want to make a simple assumption here, that when you deal with game theory you’re really looking into predicting human actions? |
|  | No, this was not … well, maybe predicting human actions is also a goal of game theory, but it is more the question what would rational players do in a game? Maybe players are not always rational. I mean, we know now that they are far from rational very often; I mean they are still boundedly rational, but not really rational. But nevertheless it is an important problem to think about what rational players would do in the game, how would they interact, yes? I mean, to have the game theory concerned with the definition of rationality. Regardless of whether people follow rationality or not you have to know what it is, yes?  And that was the real question of rational game theory. It had to be pursued even if I had … I mean I had done early experimental work and I knew that game theory would not … I mean, sometimes would be predictive certain, other cases it would not predict correctly human behaviour, you wouldn’t expect it. It always predicts human behaviour, but it was for a long time I was adhering to the idea of bounded rationality, I was convinced … ever since I have read the work of Herbert Simon in the late 50’s I was convinced about the idea of bounded rationality, but nevertheless I felt very much compelled to explore the definition of rationality in game situations. Was very important question because it’s not really clear what rationality should be in such interactive situations. |
| Q1 | It seems that it continued to fascinate you over a number of years because you have, which I found very interesting, worked with a lot of researchers, scientists from other academic disciplines. What made you want to do that? |
|  | I was always fascinated by the idea to do something about the real world, about science, about applications to the real world, but I wanted to rely on the expertise of people in the fields. So I worked together very successfully with the political scientist who was a real expert on the Middle East, Amos Perlmutter, and then I worked also very well with Avi Shmida, who is a biologist. Then also this fascinated us by new ideas and concepts, I mean this biological game field fascinated me also and I came into contact with it and this was … |
| Q39 | How far can one take it? You had also looked into, for example, creating a game theory around, for example, real conflicts, international conflicts, which I found very, very interesting. |
|  | I did several types of work about several investigations of application of game theory to international relations. And this one thing which is of special importance I think, is the scenario bundle method which I developed together with Amos Perlmutter and that was, I think, quite interesting for you. I mean for a long time I didn’t do anything about it but just recently I did something together with the Security Office of the Austrian Defence Ministry, Mr Reiter, he had made a study of the Kosovo conflict and a book was published about this though this is still …  Would you please mention the title of the book, I think that could be very interesting if you remember?  Reinhard Selten: It was something like Zur Lösung des Kosovo-Konfliktes\*, it’s a German book. |
| Q24 | Can one apply your theories on Germany today to look into the future or the different ways of, I mean, for example, a lot of Western countries today are dealing with problems of a slow economic growth, we have aging populations, we have less jobs available? |
|  | We could look, I mean in all that we see lots of problems are and you have to go to economic data and this is not the problem of game theory, that is the problem of usual economic analysis which can do a lot about it. It has been done work on this. But what you could do, you could make a strategic analysis, even apply the scenario bundle method to the internal policy, to the parties, the political forces in Germany.  For example you ask the question what will happen in politics, not so much what will happen in 10, 20 years in political reforms, but whether the reforms will come about at all and you know what will be the consequences of the loss which are now made and so on. But it is difficult to make good predictions about this, because there is one weak point in all this. This was also in our earlier efforts and this is that political mass movements are difficult to predict. So our first implication of this was done to the Persian Gulf and this was in ’76, and then my experts at that time all said that a revolution in Iran is very implausible, very unlikely, will not happen and they had also good reasons for that. And it then happened.  Then Khomeini came to power very surprisingly and not only my experts but all secret services, everybody was really surprised. I mean, if some people said afterwards say they have known this before it’s just a hindsight by us. I mean you always find people who have known everything before allegedly but I don’t think so. I mean nobody expected such things and there are unexpected consequences, mass movements which you don’t predict. |
| Q37 | Already at an early age the course of your childhood and the difficulties that your Jewish background created growing up in Germany you were deprived of opportunities and that it was hard at times but you have said in your biography that it made you act and think independently as well. Which way has that shaped you, do you think, if you would like to elaborate? |
|  | I was always sceptical about authority, about things which were told by authorities, because I was living in a country and in a time where the authority was utterly wrong, in my view. And therefore I distrusted, I feared authority, I also fear it today. I am in a very, very fearful, I mean maybe more than other people, but I distrust authority. That makes me more independent and also some part of rebellious, I mean what … once said in … about me that I am a maverick, I’m a maverick.  Because also in science I took always some points of view which were somewhat oppositional. And I found the force to present oppositional views, to keep them. I was not feeling the pressure to conform to the general view about certain things. It was in science and it’s important to have an independent mind, to be able to follow your own ideas. |
| Q73 | Is it important to have a strong family, a wife who also stands behind you or with you during times of opposition or criticism? Has that been for you in that way? |
|  | Yes, my wife was important, also when we were young she was helping me, even in my scientific work she was helping me and so that was important. But she never pushed me to do this or that, you know what I mean? It’s not like that. I was pushed because I was always delivered from this …, science was for me like a drug. I am addicted to research, let’s say. |
| Q10 | I would like to just move on a little bit, as I know that I would very much like students who are listening to this interview to get advice from you, what field should they get into? If they really want to keep this in the future? |
|  | I mean they should … I don’t want to give too specific advice, but it depends also on their character. I mean, if they want to have a sure and long career, then they should stay in the mainstream and try to be very quick and to do something and get their job. But if they’re more willing to take a little more risk they should enter a very young field and grow with it rather than to enter into an older field to learn a lot in this older field, to make a lot of investment and then go there with the mainstream.  What would such a young field be?  But that’s actually the challenge, that you don’t know how to attack the problems.  Reinhard Selten:For example bounded rationality is still very young, because there’s very much which we don’t know. I mean it’s very difficult. Usually these young things are very difficult and then people always say Oh, what can you do? I mean, we don’t know what we can do here. But that’s actually the challenge, that you don’t know how to attack the problems. And these are the worthwhile problems where there is still everything is confused and you don’t know how to attack it and how to yes? |
| ID | 0862 |
| Biographical | I was born in New York City in 1926, four years after my parents and my brother migrated to the United States from the city of Odessa in Russia. Although they arrived in New York penniless, my parents scraped together enough savings to establish the first of several small businesses just after I was born. Despite the hard times of the Great Depression and the modest financial circumstances in which we lived, they created a joyful household and they encouraged my brother and me to be optimistic about the future.  My parents’ reverence for learning encouraged both my brother and me toward academic pursuits. In many ways, however, it was my brother who was the main intellectual influence on me until he joined the armed forces in 1941. Almost six years my senior in age and nine years ahead of me in school, he inspired me with his intellectual brilliance. I still remember the intense discussions by my brother and his college classmates about the social and economic issues of the Depression that I overheard as I lay in my bed, supposedly asleep, in the next room.  My education in the public schools of New York City between 1932 and 1944 was an excellent preparation for a life in science. Because of the Depression, these schools were able to attract a remarkably talented and dedicated collection of teachers who encouraged their students to strive for the highest levels of accomplishment. That environment led me to aspire to a career in science, and also kindled my love for literature and history.  My professional training began at Cornell University (BA 1948) and continued at Columbia University where I obtained my MA (1960), and at Johns Hopkins University, where I obtained my Ph.D. (1963). It was at Cornell that my scientific interests shifted from physics and chemistry to economics and history. The switch in focus was precipitated by the widespread pessimism about the future of the economy during the second half of the 1940s, when forecasts about the imminent return to the massive unemployment of the Great Depression were rife.  I began my graduate training with the naive belief that by combining the study of history and economics I would quickly discover the fundamental forces that had determined technological and institutional changes over the ages and that such knowledge would point to solutions to the current problems of economic instability and inequity. As I became aware of how little was actually known about these large processes and their interconnections, I began to focus on more discrete issues: What did we really know about the role of the factory system in economic and institutional change during the nineteenth century? What was the nature and the magnitude of the contribution of particular new technologies, such as railroads or steel mills, to economic growth? I also concluded that to answer such questions, much greater use had to be made of quantitative evidence, so I set out to master the most advanced analytical and statistical methods that were then taught in the economics department. It was only later that I discovered that the training program I had worked out for myself was unorthodox for an economic historian.  The two teachers who influenced me the most during my year at Columbia were [George J. Stigler](https://www.nobelprize.org/prizes/economic-sciences/1982/stigler/facts/), who taught the graduate microeconomics sequence, and Carter Goodrich, who taught the sequence in American economic history. Stigler made microeconomic theory come alive. He emphasized not its elegance but its applicability to a wide range of issues in economic policy. He continually moved between theory and evidence, carefully considering the empirical validity for the assumptions that theorists made about the slope or other aspects of the shape of key functions. He often considered when, with what model, and under what implicit assumptions one could draw a particular inference from a given body of data.  Goodrich impressed me not only with his knowledge of the literature of American economic history, but with his willingness to identify the gaps in the profession’s collective knowledge of key issues. By the end of the course one not only had a good grasp of what was known about the process of American economic growth, but a list of potential projects. It was to Goodrich that I turned for advice on my master’s thesis. He was then engaged in research for his book, Government Promotion of Canals and Railroads and raised a number of issues that puzzled him about the financing, riskiness, and benefits of the Union Pacific Railroad. These questions became the subject matter of my master’s thesis, which was also my first published book. Although Goodrich did not himself make use of the new mathematical and statistical methods of economics, he encouraged me to do so. He also suggested that, given my substantial interests and quantitative approaches to economic history, [Simon Kuznets](https://www.nobelprize.org/prizes/economic-sciences/1971/kuznets/facts/) at Johns Hopkins was probably the best economist to guide my future training.  The teachers who taught me the most at Johns Hopkins, aside from Simon Kuznets, were Abba Lerner and Fritz Machlup in microeconomic theory; Evsey Domar in macroeconomic theory and the theory of economic growth; T.C. Liu in mathematical economics, and two teachers of mathematical statistics and of sampling design in the School of Public Health.  Simon Kuznets, who supervised my doctoral dissertation, was by far the most influential figure in my graduate training. Soft spoken and of moderate stature, one did not have to be in his class very long to discover that he was a towering intellect, erudite not only in economics, but also in history, demography, statistics, and the natural sciences. His course in economic growth covered the history of technological change during the modern era, demography and population theory, and the use of national income aggregates for the comparative study of economic growth and of the size distribution of income. It was not until some years later that I realized the course presented the substance of the research that later appeared in a series of 10 supplements to Economic Development and Cultural Change, and in his 1966 monograph, Modern Economic Growth: Rate, Structure, and Spread – the work for which he was awarded the third Nobel Prize in economics. Kuznets’s course was valuable not only for the substance of the material but also for the way that he used the material to transmit the art of measurement. He repeatedly demonstrated that the central statistical problem in economics was not random error but systematic biases in the data, and he conveyed a number of powerful approaches to coping with that problem, particularly emphasizing the role of sensitivity analysis.  By the time I left residence at Johns Hopkins, I had worked out a two-pronged research strategy that I thought could keep me going for a decade or more. The first was to measure the impact of key scientific and technological innovations, key governmental policies, and key environmental and institutional changes on the course of economic growth. The second was to promote the wider use of the mathematical models and statistical methods of economics in studying the complex, long-term processes that were the focus of economic historians. In my mind these two objectives were closely interrelated. The best argument for the new methods was the demonstration that in the study of particular issues, such as the contribution of railroads to economic growth, these methods were superior to traditional approaches. The new methods made it possible to lay out the key analytical issues in a manner that made them amenable to measurement, to identify the categories of evidence needed to resolve the points at issue, to develop techniques of measurement that were suitable for both the issues and the available evidence, and to assess the robustness of the results.  Several factors made the realization of my research program possible. One was the willingness of university administrators to provide me with a generous share of the limited research funds at their disposal, a sine qua non for work that was both labor and computer intensive. Even when I was still an unproven new assistant professor at Rochester, Lionel W. McKenzie provided several research assistants, a computer programmer, and all of the computer time I could use. Deans D. Gale Johnson and Robert McC. Adams made similar investments in my research at Chicago during the 1960s and early 1970s at levels that reflected as much their estimates of my promise as of accomplishments. This type of support was continued at Harvard by Henry Rosovsky during the last half of the 1970s.  Except for a small grant from the Social Science Research Council (SSRC) when I was still a student at Johns Hopkins, my work on railroads was supported exclusively from university funds. Since my later projects were based on ever-larger data sets, obtained primarily from manuscript sources at archives, these projects could not have been carried out without the generous support of foundations, particularly the National Science Foundation (NSF) and the National Institutes of Health (NIH), but to a significant degree also such private foundations as the Ford Foundation, the Exxon Educational Foundation, and the Walgreen Foundation Endowment Fund. University funding still remained crucial since it took considerable outlays of funds to bring a large project to a point that could win approval from peer review committees.  Another key factor was the plunging cost of data processing made possible by rapid advances in computer hardware and software. These technological developments made it feasible to work with ever-larger data sets. By linking together the data on individuals and households from a wide range of archival sources, data sets could be customized for particular economic issues. The sources include the manuscript schedules of decennial censuses, probate records, military and pension records, genealogies, tax rolls, death certificates, and public health records.  Still another important factor in making such research feasible was the cooperation of offcials at the U.S. National Archives and of the Genealogical Library of the Church of Jesus Christ of Latter-Day Saints in Salt Lake City. The Genealogical Library is especially valuable because it is a depository for vast quantities of records from all over the United States, and from many other countries, relevant to economic, social, and biomedical research. Although collected for religious reasons, officials of the Library have made their holdings available to the scientific community, providing a resource that would otherwise have required enormous sums of money to reproduce.  No single organization has contributed more to the study of long-term economic growth than the National Bureau of Economic Research (NBER). The long-term approach figured prominently in NBER research programs conducted between the late 1930s and the late 1960s. That work, which was conducted mainly at the macro level, was a continuation of the Bureau’s pioneering work in the development of national income accounts and related measures of macroeconomic behavior. However, during the 1970s the Bureau’s work on long-term growth processes had waned. When Martin Feldstein became President of the NBER in 1977 he decided to undertake a new program on the long-term Development of the American Economy (DAE), and asked me to be its program director.  I appointed an executive committee consisting of Lance E. Davis, Stanley L. Engerman, Robert M. Gallman, Claudia D. Goldin, Clayne L. Pope, and myself to chart the direction of the new program. After reviewing the Bureau’s past work, and the new direction it was taking under Feldstein’s leadership, the committee sought to identify a set of current policy issues to which the DAE could contribute. In the course of this review we consulted with Simon Kuznets, [Douglass C. North](https://www.nobelprize.org/prizes/economic-sciences/1993/north/facts/), Richard A. Easterlin, and Moses Abramovitz, among others.  After more than a year of investigation, we concluded that to understand the sources of the long-term decline in saving and investment rates, the factors influencing the rate of technological change, or the long-term shifts in the demographic structure of the population and the labor force, we needed to know much more about microeconomic behavior than was known at the time. Research at the microeconomic level, however, had been inhibited by the absence of suitable data. The DAE, therefore, turned its attention to the problem of constructing new data sets capable of illuminating the relationship between the current and the past behavior of families and firms.  The executive committee launched a series of pilot projects investigating the feasibility of creating several representative data sets consisting of intergenerationally linked families. Such data sets would open up entirely new possibilities for examining the interaction of economic and cultural factors and their mutual influence on such variables as the saving rate, the rate of female entry into the labor force, fertility and mortality rates, the inequality of the wealth distribution, migration rates, and rates of economic and social mobility. These data sets could not be created from a single set of records but required the linking of several different types of archival records. The executive committee also began a pilot study on the feasibility of constructing data sets based on firm records that would permit the analysis of the way that firms respond to the changing technological opportunities that are open to them, as well as to the changing institutional and legal environment in which they must operate. Dealing with such issues required the development of representative sets of firm records stretching over long periods of time that not only contained information on the decision-making processes of these firms, but also on the economic consequences of the decisions.  The DAE’s review of the pilot projects concluded that the design of portable computers for data retrieval, and of software to manipulate large files, had developed to the point where the creation of such microeconomic data sets was feasible. A score of projects were set out by 1980 and investigators to lead them were chosen. Claudia Goldin, who became the director of the DAE in 1991, reported that there are now some forty DAE research associates. Since the start of the DAE, they have created over fifty longitudinal and cross-sectional data sets that span the period from the late 1700s to the present. These data sets have formed the basis for scores of papers, several conference volumes and a number of monographs.  My ability to work on the problem of creating and studying large lifecycle and intergenerational data sets reached a new level in 1981 when Richard N. Rosett, then Dean of the Graduate School of Business at The University of Chicago, invited me to succeed George J. Stigler as the Charles R. Walgreen Professor of American Institutions. In addition to the unusual research fund endowed by Walgreen, Rosett offered to establish a Center for Population Economics (CPE) that would focus on the interaction of economic, demographic, and biological processes over life-cycles and generations. The invitation was enthusiastically supported by Hanna Gray, who was then the President of The University of Chicago. The generous support of the CPE has been continued by John P. Gould, who succeeded Rosett as Dean, by Robert S. Hamada, the current Dean, and by Hugo F. Sonnenschein, President of The University of Chicago.  Without the resources of the Walgreen Chair and the CPE the current research projects on which I reported in the Prize Lecture would not have been possible. The data on health conditions, for example, comes from a project called Early Indicators of Later Work Levels, Disease, & Death which is tracing nearly 40,000 Union Army men from the cradle to the grave. It takes over 15,000 variables to describe the life-cycle history of one of these men. These life-cycle histories are created by linking about a score of data sets. It took more than half a decade of work to investigate the potential of these data sets, work out procedures for data retrieval and file management, and to establish the feasibility of the enterprise in our own minds.  The site committee of the National Institutes of Health which reviewed the original project proposal in 1986 agreed that such a project could in principle make a significant contribution to an understanding of the process of aging, but they were skeptical about the quality of some of the data, about whether the software and programming procedures we had developed by that time were adequate for the management of such a large data set, and about whether the project could be completed within the proposed budget. To resolve these doubts it was necessary to draw a six percent subsample which linked together all of the separate sources and which demonstrated the effectiveness of the software by analyzing the information in the subsample. It took an additional four years to complete the second phase of the justification of the project. Thus nearly a decade of preliminary research, much of it funded by Walgreen and the CPE, was required before the project was accepted by the peer reviewers of NIH and NSF.  No individual has done more to help me pursue a career in science than my wife of forty-five years. I met Enid Cassandra Morgan during the election campaign of 1948 when she was a Sunday school teacher, a leader of the youth organizations of St. Phillips Episcopal Church, and the head of Harlem Youth for the election of Henry Wallace. Over the years Enid has been both my most confident supporter and my keenest critic. During my graduate training her earnings contributed significantly to the income of our family. When I was an assistant professor she combined care of the children with many hours of unpaid labor as a research assistant in library archives. She helped boost my self-confidence when my unorthodox findings provoked controversy and criticisms, and she often provided insightful suggestions for the improvement of my lectures, papers, books, letters, and research proposals.  Throughout the years she has been the overseer of my social conscience, pulling me back to reality when she saw that my preoccupation with the abstract aspects of scientific issues had led me to extenuate their deeply human aspects. I also benefitted greatly from her experiences as Student Counselor, Dean of Students, and Director of Student Life at Rochester, Harvard, and Chicago. She has helped me to understand the administrator’s point of view and to improve what she and my sons refer to as “people skills”.  My sons, Michael and Steven, have shared in the joys and the tribulations of being raised by academic parents. They have encouraged me to adhere steadfastly to scholarly principles in the face of unfair criticisms. They have read my papers and books, offered helpful suggestions, and sometimes helped substantially in the process of editing, teaching me how to say more with fewer words.  One aspect of the plunging cost of data processing has been the emergence of large-scale collaborative projects in economic history. Such projects have been promoted partly by economies of scale in the retrieval and cleaning of the data sets and partly by the wide range of skills required to manipulate, analyze and interpret the data. There were, for example, thirty five contributors to the three technical volumes of Without Consent or Contract, many of them former students who are now distinguished senior investigators. The research team for the Early Indicators project is even larger. It has been my good fortune to have had access not only to the pool of talented students at Chicago, but also to those at Harvard and Rochester. In both the slavery and aging projects these students were often far ahead of the senior investigators in recognizing major unanticipated findings, in proposing novel approaches to the analysis of the data, in discovering new data sets, and in offering probing criticisms.  It is known far and wide among economic historians that much of the credit for the success of my research enterprises goes to Marilyn Coopersmith who has worked with me for more than a quarter of a century. She was the administrative assistant of the DAE program from its inception until 1991, and she has been the associate director of the CPE since 1981. She is not only an effective coordinator but has been a diligent researcher and a friend to a legion of graduate research assistants, who often turned to her for help in overcoming bureaucratic obstacles.  The companionship of scholars and the thrill of continuous learning are two wonderful aspects of a life in science. When one is engaged with students who are both very curious and very bright, it is never quite clear who is teaching whom. I have also had the good fortune of collaborating with senior investigators who are all exceptional teachers with enthusiasm for their work and with great patience for the bewilderment of novices. Their guidance greatly facilitated my efforts to train myself for research involving the interconnections between economics, demography, and the biomedical sciences. James Trussell tutored me as I tried to master the mathematical models of demography and the art of applying them to incomplete data. J.M. Tanner has spent numerous hours teaching me the fundamentals of the branch of medicine called auxology (the study of human growth), looking at our data and helping to interpret them, guiding me through basic texts, calling my attention to the latest relevant papers, and reading and criticizing my work. I received a similar education from Nevin S. Scrimshaw in epidemiology (particularly of infectious diseases), in nutrition, and in some aspects of both physiology and clinical medicine. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0862 |
| Interview |  |
| Q49 | Has it in any way had any impact on your life, that they were immigrants? |
|  | That’s a good question. It certainly made me very much aware of America as a home for immigrants and my father believed that America was the land of opportunity. I think that was a widespread feeling among the immigrants that families I knew in New York, they fled difficult circumstances and found opportunity in America, although it wasn’t always easy.  Was it so for your family? They left Russia.  … all the money that they took with them from Odessa was spent saving my father’s life …  Robert Fogel: They left Russia. My father got sick in Constantinople and all the money that they took with them from Odessa was spent saving my father’s life and they had to borrow money to pay for passage to the United States and they had to borrow money when they reached Ellis Island to send a telegram to an older cousin who was already in the United States and saying, please come and get us, so they literally started off penniless.  After about five years my father started his first business and so, he arrived in 1922, that was about 1927, I was born in 1926, and he had several businesses and he started a third one in 1933 during the Depression and that turned out to be very successful. He and his brother started the business and seven years later he had 100 employees. Many young people in high school were of a radical persuasion and I used to tell my father that capitalisation was a corrupting system and he would be astonished and he’d say America was the land of opportunity and he’d say look at me and I said, well he was the exception that proved the rule and then he’d go down the list of other people in the New York City meat business, who were also immigrants and who were much more successful than he had been, so he did live in a world in which a lot of people came in very poor and became well to do in 15 or 20 years. |
| Q3 | I would like to come back to that, looking at it from today’s point of view, but just staying a little bit with your background and your school years. Was it important for your parents and were they influencing you that you would achieve academically? I believe you started off with physics and chemistry and yet you ended up with eonomics and history. |
|  | Right. That was not my parent’s influence, that was sort of the influence of the times because when I was in high school, I still thought of myself as becoming a physicist or a chemist and it was only when I went to college and there was so much discussion that we might have a new depression in the United States, there was a great deal of fear that the economy would not be able to employ, have full employment and so I became very curious about how the economy works and why it should be so problematic and I also discovered I loved history, it was a great story.  You said somewhere in your biography that it was a bit naive, you said you wanted to change the world.  Robert Fogel: Yes.  Do you still?  Robert Fogel: Well, like many young people I thought the reason we had so many problems was that there was an absence of goodwill and all you needed was for people to have goodwill and we were the generation that finally had goodwill. One of the most startling events in my life was when my older son was about 16 and he blamed me for all the troubles of the world. So I, I felt like telling him, oh no, I was just like you when I was your age, I wanted to change the world too. So if young people don’t have these dreams when they’re young, don’t ever have them, so I think it’s part of having you developing humanitarian perspective and learning that it isn’t quite so easy, that goodwill is not enough. You have to have good fortune and you have to have people on the other side who have goodwill. |
| Q18 | Yes. Staying there for a moment, you certainly went straight ahead into looking into history and economics and develop theories that eventually gave you the Prize. In which way have the work that you have done over the years and then the Prize, affected the way we can make people understand that we can foresee certain parts of development, for example? |
|  | Most of my work in recent years has been in the health area, the economics of health and the economics of aging and in that area we try to forecast the answer to questions like, what will happen to the health of people 20 or 30 years from now and what will happen to the cost of care, given that a much larger proportion of the population will be elderly? And if you’re going to forecast 20 or 30 or 40 years into the future, you have to have some idea about how things have been changing. So it’s impossible not to be historical, you have to go back and look at the record, at least 30 or 40 years, sometimes 100 years, in order to understand the process of change. So having a historical point of view I think has made me better able to identify the kinds of evidence that we need for forecasting. |
| Q72 | Will people who have the power to take decision, for example politicians, always acknowledge the need to do this kind of research, or would they rather ignore the historical facts because it might be easier to just leave it? |
|  | Well, if you take the people in Congress, the people who are the experts within Congress, on let’s say issues of aging and healthcare, they pay a lot of attention to the technicians, they don’t make policy independent of what the technical people are discovering or telling them, so I think they are very carefully listened to and they usually have on their staffs, people who are well trained in these fields and who have good ties to academic specialists. In the United States I think there is a pretty good interchange and my impression of most other countries is that it works, at least most other countries that I’ve visited and had a chance, it works similarly. Politicians realise they need to know what the facts are and that requires experts and they look to the experts to give them the information they need, so that they can make policy. |
| Q67 | If one looks back to your work and some of the books that you have published, they have been met with criticism because they haven’t exactly fitted into the model of what was believed to be the truth, for example the history of slavery and its impact on the US economy. Why is that so difficult for people to accept, I mean the way you were met with this criticism, for example, what did it do to your research work and why was it so controversial? |
|  | First of all I did a lot of this work with Stanley Engerman, who was at the University of Rochester and when we first started looking at the evidence, we didn’t believe it. We thought that we must be doing something wrong and what was different in our work was, that we went back to records that couldn’t have been examined before because you needed high speed computers to digitise the information, so we were lucky to be around when high speed computers and their use was becoming cheap enough so that you could do very large projects. I once estimated what it would have cost to do that stuff if we had to do it in the old-fashioned way and it would have been about $100 million. So we were able to look at what had actually happened, looking at huge quantities of data and a much different picture than what all of us had believed up until then. So we were the first but that’s what generated the controversy. If someone else had been first, I would have been criticising them but we were lucky enough to be in the right place at the right time. |
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| Q5 | I want to come to a question which has more to do with politics really. The way the United States today is acting in the Middle East. The way the US economy has been affected over the last couple of years, the huge budget deficit. Does it scare you, what’s going on at the moment in terms of the US involvement? |
|  | Well, let me take the economy first. The President has very little effect on the economy. If you want to put blame or credit, the main person who influences the business cycle is the Head of the Federal Reserve Bank. The fiscal policy used to be thought to be an important instrument for managing the economy but that was several decades ago, it is no longer in the United States considered to be an important or even an effective instrument. In the case of the Bush tax cuts, normally when Congress passes tax cuts, in order to influence the business cycle, they spend so much time arguing about the legislation that by the time it’s passed, the recession is over and it just adds to the inflationary danger as you’re in a strong expansion. In this case the economy was somewhat more sluggish and so the tax cuts probably had a positive effect, but they’re not decisive in the way in which the economy is unfolding.  The main factor influencing the American economy is the rate of technological change and that’s the reason that because the output per worker has been very high and has been growing very rapidly and which means you can do more and more with less and less people at work, so that’s what’s made the growth in jobs in the United States sluggish. So it’s really factors outside of politics. I would say in general, the main role of the government is to create circumstances in which the rate of change in technology can proceed as smoothly as possible and there is also a role that the government has to play in equity, that not all people benefit from technological change, some lose their jobs while others are lucky to be in the right place at the right time and their incomes increase very rapidly, so there are equity issues in which the government has to play a role. |
| Q36 | What you are describing at the moment, you know and there is a fear. I know that your topic here in Lindau will be talking about high performing Asian economies, you know the kind of competition we are already seeing obviously from that part of the world. |
|  | Right and it is creating problems. It’s not so much an issue that’s been popularised by the election campaign, that we’re outsourcing a lot of jobs because, not only are we outsourcing jobs but Japan and Mexico and other countries are outsourcing jobs to the United States. It’s called international trade and international trade, like everything else, has winners and losers. The consumers benefit from trade but not all the producers do, some producers benefits, other find that they can’t compete and that creates problems of equity. So the government has to play a role in making it possible for people who are not in the favourite industries to be able to find better opportunities than they now have. It is an international question too, that is, the rich countries of the world have to make at least modest efforts. Modest efforts would be, let’s say, 1% of their gross national product, should be contributed to assisting poor countries. You can’t make a poor country rich with that kind of programme.  China is becoming prosperous at a rapid rate because of what China is doing, not because what countries and OECD are doing, but there are things like the HIV AIDS epidemic that we can sit by and say, isn’t it too bad? A small amount of resources can be very effective. So those critical things, we can do a lot to save lives. We can also make our technological knowledge available, both by being open to educating the children of Third World countries and also, as has been done, not entirely unselfishly, in starting joint enterprises in poor countries which is a way of transferring our technology, that is the technology of the richer countries of the world, to the poorer countries. So those are the way to do it, but there have to be people in those countries that seize the opportunity. If they’re involved in bitter tribal conflicts, nothing we do will permit them to get out of the difficulties that they have economically, health wise and in other respects. |
| ID | 0863 |
| Biographical | I was born in Cambridge, Massachusetts – not because my family had any connection with higher education, but because my father was a manager at the Metropolitan Life Insurance Company in a nearby town and Cambridge was the nearest hospital – in 1920. In the ensuing years we moved a number of times as a result of my father’s business. First Connecticut and then, when he became head of the Metropolitan’s Canadian office, Ottawa. Because my mother believed in education broadly construed, we also lived in Europe and I went to school at the Lycée Jacquard in Lausanne, Switzerland, in 1929-30. My brother and sister are both older than I am and were born before my father went off to World War I.  I went to elementary school in Ottawa, and then to a private secondary school. When we moved back to the United States in 1933, I went to private schools in New York City and on Long Island, and then completed my high school education at the Choate School in Wallingford, Connecticut. While I was there I became deeply interested in photography, and indeed the most noteworthy event in my early life was winning first, third, fourth and seventh prizes in an international competition for college and high school students.  Our family life was certainly not intellectual. My father had not even completed high school when he started as an office boy working for the Metropolitan Life Insurance Company, and I am not sure that my mother completed high school. Nevertheless, she was an exciting person, intelligent, intellectually curious, and she played an important part in my intellectual development. My aunt and uncle were, and in the case of my aunt (Adelaide North) still is, a powerful influence. They introduced me to classical music and my aunt continues to be, to this day, a very special person in my life.  When it came time to go to college, I had been accepted for Harvard when my father was offered the position of head of the Metropolitan Life Insurance Company office on the west coast, and we moved to San Francisco. Because I did not want to be that far from home, I decided to go instead to the University of California at Berkeley. While I was there my life was completely changed by becoming a convinced Marxist and engaging in a variety of student liberal activities. I was opposed to World War II, and indeed on June 22, 1941 when Hitler invaded the Soviet Union I suddenly found myself the lone supporter of peace since everybody else had, because of their communist beliefs, shifted over to become supporters of the war. My record at the University of California as an undergraduate was mediocre to say the best. I had only slightly better than a “C” average, although I did have a triple major in political science, philosophy, and economics. I had hoped to go to law school, but the war started, and because of the strong feeling that I did not want to kill anybody, I joined the Merchant Marine when I graduated from Berkeley. We had been to sea only a short time when the Captain called me up on the bridge and asked me if I could learn to navigate since most of the officers had had only rudimentary education, and we needed to get from San Francisco to Australia. I became navigator and enjoyed it very much. We made repeated trips from San Francisco to Australia, and then to the front lines in New Guinea and the Solomon Islands.  What the war did was give me the opportunity of three years of continuous reading, and it was in the course of reading that I became convinced that I should become an economist. Then the last year of the war I taught celo-navigation at the Maritime Service Officers’ School in Alameda, California; I took up photography again and had a difficult decision as to whether to become a photographer or go into economics. In the summer of 1941 I had worked with Dorothea Lange, head of the photographic division of the Farm Security Administration, travelling with and photographing migrants through the central valley of California. Now Dorothea tried to persuade me to become a photographer. Her husband, Paul Taylor, who was in the economics department at the University of California, tried to persuade me to become an economist. He won.  I went back to graduate school with the clear intention that what I wanted to do with my life was to improve societies, and the way to do that was to find out what made economies work the way they did or fail to work. I believed that once we had an understanding of what determined the performance of economies through time, we could then improve their performance. I have never lost sight of that objective.  I cannot say that I learned much formal economics as a graduate student in Berkeley. My most influential professors were Robert Brady; Leo Rogin, a Marxist and a very influential teacher of history of economic thought; and M. M. Knight (Frank Knight’s brother) who certainly was agnostic, to say the least, about theory, but who had a wonderful knowledge of the facts and background in economic history. He became my mentor and my thesis advisor at Berkeley. But while I learned by rote most of the theory I was supposed to know, I did not acquire a real understanding of theory. It was not until I got my first job, at the University of Washington in Seattle, and began playing chess with Don Gordon, a brilliant young theorist, that I learned economic theory. In the three years of playing chess every day from noon to two, I may have beaten Don at chess, but he taught me economics; more important he taught me how to reason like an economist, and that skill is still perhaps the most important set of tools that I have acquired.  I had written my dissertation on the history of life insurance in the United States and had had a Social Science Research Council Fellowship to go to the east coast and do the spade work. That turned out to be a very productive year. I not only sat in on Robert Merton’s seminars in sociology at Columbia, but also became deeply involved in the Entrepreneurial school of Arthur Cole at Harvard. The result was that Joseph Schumpeter had a strong influence upon me. My early work and publications centered around expanding on the analysis of life insurance in my dissertation and its relationship to investment banking.  I next turned to developing an analytical framework to look at regional economic growth and this led to my first article in the Journal of Political Economy, entitled “Location Theory and Regional Economic Growth”. That work eventually led me to developing a staple theory of economic growth.  I was very fortunate that at a meeting of the Economic History Association I come to know Solomon Fabricant, who was then director of research at the National Bureau of Economic Research; and in 1956-57 I was invited to spend the year at the Bureau as a research associate. That was an enormously important year in my life. I not only became acquainted with most of the leading economists who passed through the bureau, but spent one day a week in Baltimore with [Simon Kuznets](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1971/index.html) and did the empirical work that led to my early major quantitative study of the balance of payments of the United States from 1790 to 1860.  I married for the first time in 1944. During my graduate training my wife taught school, providing our major source of support. We had three sons, Douglass, Christopher, and Malcolm, born between 1951 and 1957. After the boys were in school my wife became a successful politician in the Washington State legislature.  Between my year at the National Bureau and 1966-67, when I went off to Geneva as a Ford Faculty Fellow, I did my major work in American economic history, which led to my first book, The Economic Growth of the United States from 1790 to 1860. It was a straightforward analysis of how markets work in the context of an export staple model of growth.  By this time (1960) there was a substantial stirring to try to change and transform economic history. The year that I was at NBER, the Bureau and the Economic History Association had the first joint quantitative program on the growth of the American economy, a conference that was held at Williamstown, Massachusetts, in the late spring of 1957. This meeting was really the beginning of the new economic history, but the program coalesced when Jon Hughes and Lance Davis, two former students of mine who had become faculty members at Purdue, called the first conference of economic historians interested in trying to develop and apply economic theory and quantitative methods to history. The first meeting was held in February of 1960. This program was highly successful and the reception that it received amongst economists was certainly enthusiastic. Economics departments very quickly became interested in having new economic historians, or, as we came to call ourselves, cliometricians (Clio being the muse of history). Therefore, as I developed a graduate program jointly with my colleague Morris David Morris at the University of Washington we attracted some of the best students to do work in economic history, and during the 1960s and early 70s the job market was very responsive and our students were easily placed throughout the country.  In 1966-67 I decided that I should switch from American to European economic history, and therefore, when I received the above-mentioned grant to live in Geneva for a year, I decided to re-tool. Re-tooling turned out to change my life radically, since I quickly became convinced that the tools of neo-classical economic theory were not up to the task of explaining the kind of fundamental societal change that had characterized European economies from medieval times onward. We needed new tools, but they simply did not exist. It was in the long search for a framework that would provide new tools of analysis that my interest and concern with the new institutional economics evolved. The result was two initial books, one with Lance Davis, Institutional Change and American Economic Growth, and the other with Robert Thomas, The Rise of the Western World: A New Economic History.  Both books were early tentative attempts to develop some tools of institutional analysis and apply them to economic history. Both were still predicated on neo-classical economic theory, and there were too many loose ends that did not make sense: such as the notion that institutions were efficient (however defined). Perhaps more serious, it was not possible to explain long-run poor economic performance in a neo-classical framework. So I began to explore what was wrong. Individual beliefs were obviously important to the choices people make, and only the extreme myopia of economists prevented them from understanding that ideas, ideologies, and prejudices mattered. Once you recognize that, you are forced to examine the rationality postulate critically.  The long road towards developing a new analytical framework involved taking all of these considerations seriously: to develop a view of institutions that would account for why institutions produced results that in the long run did not manage to produce economic growth; develop a model of political economy in order to be able to handle and explain the underlying source of institutions. Finally, one had to come to grips with why people had the ideologies and ideas that determined the choices they made.  In Structure and Change in Economic History (1981) I abandoned the notion that institutions were efficient and attempted to explain why “inefficient” rules would tend to exist and be perpetuated. This was tied to a very simple and still neo-classical theory of the state which could explain why the state could produce rules that did not encourage economic growth. I was still dissatisfied with our understanding of the political process, and indeed searched for colleagues who were interested in developing political-economic models. This led me to leave the University of Washington in 1983 after being there for 33 years, and to move to Washington University in St. Louis, where there was an exciting group of young political scientists and economists who were attempting to develop new models of political economy. This proved to be a felicitous move. I created the Center in Political Economy, which continues to be a creative research center.  The development of a political-economic framework to explore long-run institutional change occupied me during all of the 1980s and led to the publication of Institutions, Institutional Change and Economic Performance in 1990. In that book I began to puzzle seriously about the rationality postulate. It is clear that we had to have an explanation for why people make the choices they do; why ideologies such as communism or Muslim fundamentalism can shape the choices people make and direct the way economies evolve through long periods of time. One simply cannot get at ideologies without digging deeply into cognitive science in attempting to understand the way in which the mind acquires learning and makes choices. Since 1990, my research has been directed toward dealing with this issue. I still have a long way to go, but I believe that an understanding of how people make choices; under what conditions the rationality postulate is a useful tool; and how individuals make choices under conditions of uncertainty and ambiguity are fundamental questions that we must address in order to make further progress in the social sciences.  In 1972 I married again, to Elisabeth Case; she continues to be wife, companion, critic and editor: a partner in the projects and programs that we undertake.  I would be remiss if I left the impression that my life has been totally preoccupied with scholarly research. True, it has been the fundamental focus of my life, but it has been intermingled with a variety of activities that have complemented that central preoccupation and enriched my life. I continue to be a photographer; I have enjoyed fishing and hunting with a close friend; and have owned two ranches, first in northern California and then in the state of Washington. I learned to fly an airplane, and had my own airplane during the 1960s. I have always taken seriously good food and wine. In addition, music has continued to be an important part of my life.  My wife and I now live in the summers in northern Michigan in an environment which is wonderfully conducive to research, and where most of my work in the last 15 years has been done. I work on research all morning. In the afternoons I hike with my dog, play tennis or go swimming. In the evening, as we are only 16 miles from the National Music Camp at Interlochen, we may listen to music two or three nights a week. It is a wonderful place for that mixture of research and leisure which has made my life such a rich experience.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1993, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1994  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.  Copyright © The Nobel Foundation 1993 **Addendum, May 2005** Since receiving the Nobel Prize in 1993 I have continued my research trying to develop an analytical framework that would make more sense out of long-run economic, social and political change. With that objective in mind, I have gone much more deeply into cognitive science and attempted to understand the way in which the mind and brain work and how that relates to the way in which people make choices and the belief systems that they have. Clearly these underlie institutional change and therefore are a necessary prerequisite to being able to develop a theory about institutional change. I have also attempted to integrate political, economic and social theory since, obviously, a useful theory of economic change cannot confine itself purely to economics but must try to integrate the social sciences and integrate them also with cognitive science. The result is a recently published book by Princeton University Press entitled Understanding the Process of Economic Change.  One result of these interests has been to establish jointly with Ronald Coase, who won the Nobel Prize in 1991, the International Society for the New Institutional Economics. Its first meeting was held in 1997 here in St. Louis, and subsequent to that it has become a thriving international organization with meetings all over the world. The new institutional economics has become such a significant addition to the social sciences that I have been asked to elaborate on it all over the world, particularly in China where there is much enthusiasm about the implications of the new institutional economics applied to solving problems of the Chinese political economic future. In 1995 the University of Beijing formally opened a research center in economics at which I gave the opening address. I also have served as adviser in applying the new institutional economics to economic development in Asia and Latin America and in Eastern Europe. One result of all of this was to establish here at Washington University in St. Louis a center for the new institutional social sciences which attempts to integrate, both at the level of teaching and in research, the social sciences.  In addition, because I feel very strongly that we must reorient the social sciences to attempt to confront these issues and to be more oriented toward policy problems, I held a meeting in the fall of 1994 of leading social scientists from political science, economics, and sociology to attempt to plan how the social sciences should evolve over time. This initial meeting was successful and successive meetings are planned for future years and at other universities to attempt to build on this development.  And finally, as a result of being asked to participate in the Copenhagen Consensus, which was an attempt to get a number of economists to confront leading issues around the world, I participated in what turned out to be a very interesting attempt to explore and resolve problems as varied as HIV and aids, malnutrition, clean water, etc., to come up with policy recommendations that would move towards solving such problems. I continue to be involved in all of those things at this time.  Douglass C. North died on 23 November 2015. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0863 |
| Interview | Douglass C. North talks about who has had the biggest influence on his life; his choices in life (3:31); why some countries are rich and some poor (6:10); his sources of inspiration (8:45); and his work on institutions (9:58). |
|  |  |
| ID | 0864 |
| Biographical | I was born in Pottsville, Pennsylvania, a little coal mining town in Eastern Pennsylvania, where my father owned a small business. He had first gone into business for himself after leaving Montreal and his family for the United States when he was only sixteen-years old. He moved many times in the eastern United States before settling in Pottsville in the mid-1920s. My two sisters, Wendy and Natalie, and brother, Marvin, were also born there. However, when I was four or five we moved to Brooklyn, New York, where my father became a partner in another business.  I went to elementary school and high school in Brooklyn. I was a good student, but until age sixteen was more interested in sports than intellectual activities. At that time I had to decide between being on the handball and math teams since they met during the same time period. It was indicative of my shift in priorities that I chose math, although I was better at handball.  My father had left school in Montreal after the 8th grade because he was eager to make money. My mother – whose family emigrated from Eastern Europe to New York City when she was six months old – also left after the 8th grade because girls were not expected to get much education. There were only a few books in our house, but my father kept up with the political and financial news, and my older sister read a lot. After my father lost most of his sight, I had the task of reading him stock quotations and other reports on financial developments. Perhaps that stimulated my interest in economics, although I was rather bored by it.  We had many lively discussions in the house about politics and justice. I believe this does help explain why by the time I finished high school, my interest in mathematics was beginning to compete with a desire to do something useful for society. These two interests came together during my freshman year at Princeton, when I accidentally took a course in economics, and was greatly attracted by the mathematical rigor of a subject that dealt with social organization. During the following summer I read several books on economics.  To be financially independent more quickly, I decided at the end of my first year to graduate in three years, a seldom used option at Princeton. I had to take a few extra courses during the next year, and I chose reading courses in modern algebra and differential equations for the summer afterwards. For the equations course, I was given a set of unpublished lectures that emphasized existence proofs and uniqueness of solutions to differential equations. I learned a lot about such proofs, but very little about actually solving one of these equations. Still, my heavy investment in mathematics at Princeton prepared me well for the increasing use of mathematics in economics.  I began to lose interest in economics during my senior (third) year because it did not seem to deal with important social problems. I contemplated transferring to sociology, but found that subject too difficult. Fortunately, I decided to go to the University of Chicago for graduate work in economics. My first encounter in 1951 with [Milton Friedman](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1976/index.html)‘s course on microeconomics renewed my excitement about economics. He emphasized that economic theory was not a game played by clever academicians, but was a powerful tool to analyze the real world. His course was filled with insights both into the structure of economic theory and its application to practical and significant questions. That course and subsequent contacts with Friedman had a profound effect on the direction taken by my research.  While Friedman was clearly the intellectual leader, Chicago had a first class group of economists who were doing innovative research. Especially important to me were Gregg Lewis’s use of economic theory to analyze labor markets, [T.W. Schultz](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1979/index.html)‘s pioneering research on human capital, Aaron Director’s applications of economics to anti-trust problems, and industrial organization more generally, and L.J. Savage’s research on subjective probability and the foundation of statistics.  I published two articles in 1952, based on my research at Princeton. But I realized shortly after arriving in Chicago that I had to begin to learn again what economics is all about. I published nothing else until an article written with Friedman and a book based on my Ph.D. dissertation came out in 1957. The book contains the first systematic effort to use economic theory to analyze the effects of prejudice on the earnings, employment and occupations of minorities. It started me down the path of applying economics to social issues, a path that I have continued to follow.  The book was very favorably reviewed in a few major journals, but for several years it had no visible impact on anything. Most economists did not think racial discrimination was economics, and sociologists and psychologists generally did not believe I was contributing to their fields. However, Friedman, Lewis, Schultz, and others at Chicago were confident I had written an important book. Support by the people I respected so highly was crucial to my willingness to persevere in the face of much hostility.  After my third year of graduate study I became an Assistant Professor at Chicago. I had a light teaching load and could concentrate mainly on research. However, I felt that I would become intellectually more independent if I left the nest and had to make it on my own. After three years in that position, I turned down a much larger salary from Chicago to take a similar appointment at Columbia combined with one at the National Bureau of Economic Research, then also located in Manhattan. I have always believed this was the correct decision, for I developed greater independence and self-confidence than seems likely if I remained at Chicago.  For twelve years I divided my time between teaching at Columbia and doing research at the Bureau. My book on human capital was the outgrowth of my first research project for the Bureau. During this period I also wrote frequently cited articles on the allocation of time, crime and punishment, and irrational behavior.  At Columbia I began a workshop on labor economics and related subjects–anything that interested us was “related.” This involved transplanting the workshop system of supervising doctoral research from Chicago – where it originated. After a few years, Jacob Mincer joined the Columbia department and became co-director of the workshop. We had a very exciting atmosphere and attracted most of the best students at Columbia. Both Mincer and I were doing research on human capital before this subject was adequately appreciated in the profession at large, and the students found it fascinating. We were also working on the allocation of time, and other subjects in the forefront of research.  I married for the first time in 1954, and have two daughters from that marriage, Judy and Catherine. To provide a better family atmosphere I lived in the suburbs and commuted to Columbia and the Bureau. Eventually, I began to tire of commuting and decided either to move into New York or to leave Columbia for another university. I also was beginning to feel intellectually stale.  My decision to leave was hastened by the student riots in 1968. I believed that Columbia should take a firm hand and uphold the right to free inquiry without student intimidation. The central administration wanted to do this, but it was incompetent, and was opposed by many faculty who behaved no better than the students.  In 1970, I returned to Chicago, and found the atmosphere there very stimulating. The department was still powerful, especially after it had added [George Stigler](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1982/index.html) and Harry Johnson. Stigler and I soon became close friends, and he had a very large effect on my subsequent intellectual development. We wrote two influential papers together: a controversial one on the stability of tastes, and an early treatment of the principle-agent problem. Stigler also renewed my interest in the economics of politics; I had published a short paper on this subject in 1958. In the 1980s I published two articles that developed a theoretical model of the role of special interest groups in the political process.  But mainly I worked on the family after returning to Chicago. I had much earlier used economic theory to try to understand birth rates and family size. I now began to consider the whole range of family issues: marriage, divorce, altruism toward other members, investments by parents in children, and long term changes in what families do. A series of articles in the 1970s culminated in 1981 in A Treatise on the Family . Since I continued to work on this subject, a greatly expanded edition was published in 1991. I have tried not only to understand the determinants of divorce, family size, and the like, but also the effects of changes in family composition and structure on inequality and economic growth. Most of my research on the family, and that by students and faculty at Chicago and elsewhere, was presented at the Workshop in Applications of Economics that Sherwin Rosen and I run.  For a long time my type of work was either ignored or strongly disliked by most of the leading economists. I was considered way out and perhaps not really an economist. But younger economists were more sympathetic. They may disagree with my analysis, but accept the kind of problems, studied as perfectly legitimate. During the past ten years I have received much tangible evidence of this shift in professional opinion, including the presidency of the American Economic Association, the Seidman Award, and the first social science Award of Merit from the National Institute of Health.  In 1983, the Sociology Department at Chicago offered me a joint appointment. I was happy to accept because this was an outstanding department. Its invitation to me gave a signal to the sociology profession that the rational choice approach was a respectable theoretical paradigm. James Coleman and I shortly thereafter began an interdisciplinary faculty seminar on rational choice in the social sciences that has been far more successful than we anticipated.  Until 1985, I had published only technical books and technical articles in professional journals. At that time, I was surprised by being asked to write a monthly column for Business Week magazine. Since I feared that I could not write for a general audience, I was inclined to turn the offer down. Finally, however, I agreed to do some columns on an experimental basis. It was a wise decision, for I was forced to learn how to write about economic and social issues without using technical jargon, and in about 800 words per column. Doing this has enormously improved my capacity to discuss important subjects briefly and in simple language. The pressure of having to do a column every month also makes me stay abreast of many subjects that interest the business and professional readers of the magazine.  I married for the second time in 1980 to Guity Nashat – my first wife died in 1970. This gave me two stepsons, Michael and Cyrus, to go with two daughters. Guity is the one who overcame my reluctance to do the Business Week columns. She is an historian of the Middle East with professional interests that overlap my own: on the role of women in economic and social life, and the causes of economic growth. The personal and professional compatibility she provides has made my life so much better.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1992, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1993  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.  Gary S. Becker died on 3 May 2014. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0864 |
| Interview |  |
|  |  |
| ID | 0865 |
| Biographical | My father, a methodical man, recorded in his diary that I was born at 3:25 p.m. on December 29th, 1910. The place was a house, containing two flats of which my parents occupied the lower, in a suburb of London, Willesden. My father was a telegraphist in the Post Office. My mother had been employed in the Post Office but ceased to work on being married. Both my parents had left school at the age of 12 but were completely literate. However, they had no interest in academic scholarship. Their interest was in sport. My mother played tennis until an advanced age. My father, who played football, cricket and tennis while young, played (lawn) bowls until his death. He was a good player, played for his county and won a number of competitions. He wrote articles on bowls for the local newspaper and for Bowls News.  I had the usual boy’s interest in sport but my main interest was always academic. I was an only child but although often alone, I was never lonely. When I learnt chess, I was happy to play the role of each player in turn. Lacking guidance, my reading (in books borrowed from the local public library) was undiscriminating and, as I now realize, I was unable to distinguish the charlatan from the serious scholar. My mother taught me to be honest and truthful and although it is impossible to escape some degree of self-deception, my endeavours to follow her precepts have, I believe, lent some strength to my writing. My mother’s hero was Captain Oates, who, returning with Scott from the South Pole and finding that his illness was hampering the others, told his companions that he was going for a stroll, went out into a blizzard and was never heard of again. I have always felt that I should not be a bother to others but in this I have not always succeeded.  Aged 11, I was taken by my father to a phrenologist. What the phrenologist said about my character was, I feel sure, determined less by the shape of my skull than by the impressions he derived from my behaviour. Out of the various printed summaries of character in his booklet, that chosen for “Master Ronald Coase” started: “You are in possession of much intelligence, and you know it, though you may be inclined to underrate your abilities.” This printed summary also included the following remarks: “You will not float down, like a sickly fish, with the tide… you enjoy considerable mental vigour and are not a passive instrument in the hands of others. Though you can work with others and for others, where you see it to your advantage, you are more inclined to think and work for yourself. A little more determination would be to your advantage, however.” In the written comments, the pursuits recommended were: “Scientific and commercial banking, accountancy. Also, horticulture and poultry-rearing as hobbies.” Added were some comments about my character: “More hope, confidence and concentration required – not suited for the aggressive competitive side of business life. More active ambition would be beneficial.” It was also noted that I was too cautious. It was hardly to be expected that this timid little boy would one day be the recipient of a Nobel Prize. That this happened was the result of a series of accidents.  As a young boy I suffered from a weakness in my legs, which necessitated, or was thought to necessitate, the wearing of irons on my legs. As a result I went to the school for physical defectives run by the local council. For reasons that I do not remember I missed taking the entrance examinations for the local secondary school at the usual age of 11. However, as the result of the efforts of my parents I was allowed to take the secondary school scholarship examination at the age of 12. The only thing I now remember is that at the oral examination I caused some amusement by referring to a character in Shakespeare’s Twelfth Night as Macvolio. However, this lapse was not fatal and I was awarded a scholarship to go to the Kilburn Grammar School. The teaching there was good and I received a solid education. I particularly remember our geography teacher, Charles Thurston, who introduced us to Wegener’s hypothesis on the movements of the continents long before it was generally accepted and who also took us to lectures at the Royal Geographical Society, one of which, on river meanders, discussed the effect of the earth’s rotation on the course of rivers. I took the matriculation examination in 1927, which I passed, with distinction in history and chemistry.  It was then possible to spend the two years after matriculation at the Kilburn Grammar School studying for the intermediate examination of the University of London as an external student, which covered the work which would have been taken during the first year at the University as an internal student. I then had to decide what degree to take. The answer was in fact determined by one of those accidental factors which seem to have shaped my life. My inclination was to take a degree in history, but I found that to do this I would have to know Latin and having arrived at the Kilburn Grammar School at 12 instead of 11, there had been no possibility of my studying Latin. So I turned to the other subject in which I had secured distinction and started to study for a science degree, specialising in chemistry. However, I soon found that mathematics, a requirement for a science degree, was not to my taste and I switched to the only other degree for which it was possible to study at the Kilburn Grammar School, one in commerce. Although my knowledge of the subjects on which I was examined was rudimentary, I managed to pass the intermediate examinations and went to the London School of Economics in October, 1929 to continue my studies for a Bachelor of Commerce degree. I took a hodgepodge of courses for Part I of the final examination, which I passed in 1930.  For Part II, I specialised in the Industry Group. I then had an extraordinary stroke of luck, another accidental factor which would affect everything I was to do subsequently. Arnold Plant, who had previously held a chair at the University of Cape Town, South Africa, was appointed Professor of Commerce (with special reference to Business Administration) at the London School of Economics in 1930. I attended his lectures on business administration but it was what he said in his seminar, which I started to attend only five months before the final examinations, that was to change my view of the working of the economic system, or perhaps more accurately was to give me one. What Plant did was to introduce me to Adam Smith’s “invisible hand”. He made me aware of how a competitive economic system could be coordinated by the pricing system. But he did not merely influence my ideas. My encountering him changed my life. I passed the B. Com, Part II final examination in 1931, but having taken the first year of University work while still at school and three years residence at the London School of Economics being required before a degree could be awarded, I had to decide what to do in this third year. Among the subjects studied for Part II, the one I had found most interesting was Industrial Law and what I had decided to do was to study in this third year for the degree of B.Sc. (Econ), with Industrial Law as my special subject. Had I done so I would undoubtedly have gone on to become a lawyer. But that was not to be. No doubt as a result of Plant’s influence, the University of London awarded me a Sir Ernest Cassel Travelling Scholarship and although I did not know it, I was on the road to becoming an economist.  I spent the academic year 1931-32 on my Cassel Travelling Scholarship in the United States studying the structure of American industries, with the aim of discovering why industries were organized in different ways. I carried out this project mainly by visiting factories and businesses. What came out of my enquiries was not a complete theory answering the questions with which I started but the introduction of a new concept into economic analysis, transaction costs, and an explanation of why there are firms. All this was achieved by the Summer of 1932, as the contents of a lecture delivered in Dundee in October 1932, make clear. These ideas became the basis for my article “The Nature of the Firm”, published in 1937, cited by the [Royal Swedish Academy of Sciences](http://www.kva.se/) in awarding me the 1991 Alfred Nobel Memorial Prize in Economic Sciences. The delay in publishing my ideas was partly due to a reluctance to rush into print and partly to the fact that I was heavily engaged in teaching and research on other projects. I held a teaching position at the Dundee School of Economics and Commerce from 1932 to 1934, at the University of Liverpool from 1934 to 1935 and at the London School of Economics from 1935 on. At the London School of Economics I was assigned a course on the economics of public utilities in Britain. In 1939, the Second World War broke out and in 1940 I entered government service doing statistical work, first at the Forestry Commission and then at the Central Statistical Office, Offices of the War Cabinet. I returned to the London School of Economics in 1946. I then became responsible for the main economics course, “The Principles of Economics”, and also continued with my research on public utilities, particularly the Post Office and broadcasting. I spent nine months in 1948 in the United States on a Rockefeller Fellowship studying the American broadcasting industry. My book, British Broadcasting: A Study in Monopoly, was published in 1950.  In 1951, I migrated to the United States. I went first to the University of Buffalo and in 1959, after a year at the Center for Advanced Study in the Behavioral Sciences, I joined the economics department of the University of Virginia. I maintained my interest in public utilities and particularly in broadcasting and during my year at the Center for Advanced Study in the Behavioral Sciences, I made a study of the Federal Communications Commission which regulated the broadcasting industry in the United States, including the allocation of the radio frequency spectrum. I wrote an article, published in 1959, which discussed the procedures followed by the Commission and suggested that it would be better if use of the spectrum was determined by the pricing system and was awarded to the highest bidder. This raised the question of what rights would be acquired by the successful bidder and I went on to discuss the rationale of a property rights system. Part of my argument was considered to be erroneous by a number of economists at the University of Chicago and it was arranged that I should meet with them one evening at Aaron Director’s home. What ensued has been described by [Stigler](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1982/index.html) and others. I persuaded these economists that I was right and I was asked to write up my argument for publication in the Journal of Law and Economics. Although the main points were already to be found in The Federal Communications Commission, I wrote another article, The Problem of Social Cost, in which I expounded my views at greater length, more precisely and without reference to my previous article. This article, which appeared early in 1961, unlike my earlier article on “The Nature of the Firm”, was an instant success. It was, and continues to be, much discussed. Indeed it is probably the most widely cited article in the whole of the modern economic literature. It, and The Nature of the Firm were the two articles cited by the Royal Swedish Academy of Sciences as justification for awarding me the Alfred Nobel Memorial Prize. Had it not been for the fact that these economists at the University of Chicago thought that I had made an error in my article on The Federal Communications Commission, it is probable that The Problem of Social Cost would never have been written.  In 1964, I moved to the University of Chicago and became editor of the Journal of Law and Economics. I continued as editor until 1982. Editorship of the journal was a source of great satisfaction. I encouraged economists and lawyers to write about the way in which actual markets operated and about how governments actually perform in regulating or undertaking economic activities. The journal was a major factor in creating the new subject, “law and economics”. My life has been interesting, concerned with academic affairs and on the whole successful. But, on almost all occasions, what I have done has been determined by factors which were no part of my choosing. I have had “greatness thrust upon me”.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1991, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1992  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.  Ronald H. Coase died on 2 September 2013. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0865 |
| Interview |  |
|  |  |
| ID | 0866 |
| Biographical | I was born in Chicago in 1927, the only child of Morris and Mildred Markowitz who owned a small grocery store. We lived in a nice apartment, always had enough to eat, and I had my own room. I never was aware of the Great Depression.  Growing up, I enjoyed baseball and tag football in the nearby empty lot or the park a few blocks away, and playing the violin in the high school orchestra. I also enjoyed reading. At first, my reading material consisted of comic books and adventure magazines, such as The Shadow, in addition to school assignments. In late grammar school and throughout high school I enjoyed popular accounts of physics and astronomy. In high school I also began to read original works of serious philosophers. I was particularly struck by David Hume’s argument that, though we release a ball a thousand times, and each time, it falls to the floor, we do not have a necessary proof that it will fall the thousand-and-first time. I also read The Origin of Species and was moved by Darwin’s marshalling of facts and careful consideration of possible objections.  From high school, I entered the University of Chicago and took its two year Bachelor’s program which emphasized the reading of original materials where possible. Everything in the program was interesting, but I was especially interested in the philosophers we read in a course called OII: Observation, Interpretation and Integration.  Becoming an economist was not a childhood dream of mine. When I finished the Bachelor’s degree and had to choose an upper division, I considered the matter for a short while and decided on Economics. Micro and macro were all very fine, but eventually it was the “Economics of Uncertainty” which interested me–in particular, the Von Neumann and Morgenstern and the Marschak arguments concerning expected utility; the Friedman-Savage utility function; and L. J. Savage’s defense of personal probability. I had the good fortune to have [Friedman](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1976/index.html), Marschak and Savage among other great teachers at Chicago. [Koopmans’](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1975/index.html) course on activity analysis with its definition of efficiency and its analysis of efficient sets was also a crucial part of my education.  At Chicago I was invited to become one of the student members of the Cowles Commission for Research in Economics. If anyone knows the Cowles Commission only by it influence on Economic and Econometric thought, and by the number of Nobel laureates it has produced, they might imagine it to be some gigantic research center. In fact it was a small but exciting group, then under the leadership of its director, T. Koopmans, and its former director, J. Marschak.  When it was time to choose a topic for my dissertation, a chance conversation suggested the possibility of applying mathematical methods to the stock market. I asked Professor Marschak what he thought. He thought it reasonable, and explained that Alfred Cowles himself had been interested in such applications. He sent me to Professor Marshall Ketchum who provided a reading list as a guide to the financial theory and practice of the day.  The basic concepts of portfolio theory came to me one afternoon in the library while reading John Burr Williams’s Theory of Investment Value. Williams proposed that the value of a stock should equal the present value of its future dividends. Since future dividends are uncertain, I interpreted Williams’s proposal to be to value a stock by its expected future dividends. But if the investor were only interested in expected values of securities, he or she would only be interested in the expected value of the portfolio; and to maximize the expected value of a portfolio one need invest only in a single security. This, I knew, was not the way investors did or should act. Investors diversify because they are concerned with risk as well as return. Variance came to mind as a measure of risk. The fact that portfolio variance depended on security covariances added to the plausibility of the approach. Since there were two criteria, risk and return, it was natural to assume that investors selected from the set of Pareto optimal risk-return combinations.  I left the University of Chicago and joined the RAND Corporation in 1952. Shortly thereafter, George Dantzig joined RAND. While I did not work on portfolio theory at RAND, the optimization techniques I learned from George (beyond his basic simplex algorithm which I had read on my own) are clearly reflected in my subsequent work on the fast computation of mean-variance frontiers (Markowitz (1956) and Appendix A of Markowitz (1959)). My 1959 book was principally written at the Cowles Foundation at Yale during the academic year 1955-56, on leave from the RAND Corporation, at the invitation of [James Tobin](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1981/index.html). It is not clear that Markowitz (1959) would ever have been written if it were not for Tobin’s invitation.  My article on “Portfolio Selection” appeared in 1952. In the 38 years since then, I have worked with many people on many topics. The focus has always been on the application of mathematical or computer techniques to practical problems, particularly problems of business decisions under uncertainty. Sometimes we applied existing techniques; other times we developed new techniques. Some of these techniques have been more “successful” than others, success being measured here by acceptance in practice.  In 1989, I was awarded the Von Neumann Prize in Operations Research Theory by the Operations Research Society of America and The Institute of Management Sciences. They cited my works in the areas of portfolio theory, sparse matrix techniques and the SIMSCRIPT programming language. I have written above about portfolio theory. My work on sparse matrix techniques was an outgrowth of work I did in collaboration with Alan S. Manne, Tibor Fabian, Thomas Marschak, Alan J. Rowe and others at the RAND Corporation in the 1950s on industry-wide and multi-industry activity analysis models of industrial capabilities. Our models strained the computer capabilities of the day. I observed that most of the coefficients in our matrices were zero; i.e. , the nonzeros were “sparse” in the matrix, and that typically the triangular matrices associated with the forward and back solution provided by Gaussian elimination would remain sparse if pivot elements were chosen with care. William Orchard-Hayes programmed the first sparse matrix code. Since then considerable work has been done on sparse matrix techniques, for example, on methods of selecting pivots and of storing the nonzero elements. Sparse matrix techniques are now standard in large linear programming codes.  During the 1950s I decided, as did many others, that many practical problems were beyond analytic solution, and that simulation techniques were required. At RAND I participated in the building of large logistics simulation models; at General Electric I helped build models of manufacturing plants. One problem with the use of simulation was the length of time required to program a detailed simulator. In the early 1960s, I returned to RAND for the purpose of developing a programming language, later called SIMSCRIPT, which reduced programming time by allowing the programmer to describe (in a certain stylized manner) the system to be simulated rather than describing the actions which the computer must take to accomplish this simulation. The original SIMSCRIPT compiler was written by B. Hausner; its manual by H. Karr who later co-founded a computer software company, CACI, with me. Currently SIMSCRIPT II.5 is supported by CACI and still has a fair number of users.  I am sorry I cannot acknowledge all the people I have worked with over the last 38 years and describe what it was we accomplished. As each of these people know, I often considered work to be play, and derived great joy from our collaboration.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1990, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1991  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.  Harry M. Markowitz died on 22 June 2023. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0866 |
| Interview |  |
|  |  |
| ID | 0867 |
| Biographical | I was born in Boston, Massachusetts on May 16, 1923, the only child of Joel and Sylvia Miller. My father, an attorney, was a graduate of Harvard University (A.B. 1916) and in that one respect, at least, I followed in his footsteps, entering Harvard in 1940 and graduating in 1943 (A.B., magna cum laude, Class of 1944). My main interest, however, was in economics, not law. One of my college classmates – indeed we were in the same section of the introductory survey course, Economics A – was [Robert M. Solow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1987/index.html), the laureate in Economics for 1987.  During the war years I worked as an economist first in the Division of Tax Research of the U.S. Treasury Department and subsequently in the Division of Research and Statistics of the Board of Governors of the Federal Reserve System. In 1949, I decided to return to graduate school and chose Johns Hopkins University in Baltimore primarily because Fritz Machlup was then a leading member of its small, but very distinguished faculty.  My first academic appointment after receiving my doctorate from Hopkins in 1952 was Visiting Assistant Lecturer at the London School of Economics for 1952-1953. From there I went to Carnegie Institute of Technology (now Carnegie-Mellon University) whose Graduate School of Industrial Administration was the first and most influential of the new wave of research-oriented U.S. business schools. Among my colleagues at Carnegie were [Herbert Simon](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1978/index.html) (Economics Laureate 1978) and [Franco Modigliani](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1985/index.html) (Economics Laureate 1985). Modigliani and I published the first of our joint M&M papers on corporation finance in 1958 and we collaborated on several subsequent ones until well into the mid-1960’s.  In 1961, I left Carnegie for the Graduate School of Business at the University of Chicago where I have been ever since except for a one-year visiting professorship at the University of Louvain in Belgium during 1966-1967. At Chicago, where I am currently Robert R. McCormick Distinguished Service Professor, most of my work continued to be focussed on corporate finance until the early 1980’s when I became a public director of the Chicago Board of Trade. My research interests since then have shifted strongly towards the economic and regulatory problems of the financial services industry, and especially of the securities and options exchanges. I am currently serving as a public director of the Chicago Mercantile Exchange where I had served earlier as Chairman of its special academic panel to conduct the post-mortem on the Crash of October 19-20, 1987.  I continue to be an activist supporter of free-market solutions to economic problems, very much in the tradition of my fellow Chicago laureates, [Milton Friedman](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1976/index.html) (1976), [Theodore Schultz](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1979/index.html) (1979) and [George Stigler](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1982/index.html) (1982).  The untimely death in 1969 of my first wife, Eleanor, the mother of my then 3 young daughters, was a heavy personal blow. I have since remarried and my wife Katherine and I divide our time between a Hyde Park townhouse during the week, and a country retreat on a working farm (though not worked by us) in Woodstock, Illinois on the weekends. Like some other weekend retreaters my hobby has become brush-cutting and maintenance generally, plus a little gardening. Unlike some of my more athletic fellow laureates, however, the closest I get to recreational exercise these days is watching the Chicago Bears from my season-ticket seats (17 years now) in the south-end zone of frigid Soldier Field.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1990, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1991  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.  Merton H. Miller died on June 3, 2000. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0867 |
| Interview |  |
|  |  |
| ID | 0868 |
| Biographical | I was born on June 16, 1934 in Boston, Massachusetts. At that time my parents had completed their undergraduate educations – my father in English literature, my mother in science. My father was then employed at Harvard University, working in the placement office.  In 1940, world events led to the activation of my father’s National Guard unit and a move to Texas. The subsequent outbreak of World War II required further moves to northern California and finally to southern California.  The majority of my pre-college education was completed in the public schools of Riverside, California, which were excellent. I benefitted there from stimulating teachers and challenging curricula.  In 1951 I enrolled at the University of California at Berkeley, with a plan to major in science en route to a medical degree. A year of the associated courses convinced me that my preferences lay elsewhere. To change both curriculum and locale I transferred to the University of California at Los Angeles with a declared major in Business Administration.  In my first semester at UCLA I took Accounting and Economics–two courses that were required for the Business degree. Both had a major influence on my career. The accounting course dealt primarily with bookkeeping, while the economics course focused on microeconomic theory. I found bookkeeping tedious and light on intellectual content. But I was greatly attracted to the rigor and relevance of microeconomic theory. Hence, I changed my major to Economics. I have since learned to appreciate Accounting on both pragmatic and intellectual grounds, but am delighted that my first brush with it helped turn me towards the field in which I have worked happily throughout my professional life.  I took two degrees in Economics at UCLA before serving in the Army. I received the Bachelor of Arts degree in 1955 and the Master of Arts degree in 1956. While working for the former I was named to Phi Beta Kappa.  Two professors at UCLA had a profound influence on my career.  I was fortunate to serve as a research assistant for J. Fred Weston, a professor of finance in the Business School, and also to take courses from him. Fred first introduced me to the work of [Harry Markowitz](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1990/index.html) and to the rest of the challenging and rigorous research that was beginning to revolutionize finance. As part of my PhD program I was subsequently able to take a field in finance with Fred, greatly broadening my understanding of the subject.  Armen Alchian, a professor of economics, was my role model at UCLA. He taught his students to question everything; to always begin an analysis with first principles; to concentrate on essential elements and abstract from secondary ones; and to play devil’s advocate with one’s own ideas. In his classes we were able to watch a first-rate mind work on a host of fascinating problems. I have attempted to emulate his approach to research ever since. When I returned to pursue the PhD degree, I took a field in microeconomics with Armen and he also served as chairman of my dissertation committee.  After a short period in the Army, I joined the RAND Corporation in 1956 as an Economist. RAND was an almost ideal place for anyone interested in performing research that was both aesthetically pleasing and also pragmatic. During this period path-breaking work in computer science, game theory, linear programming, dynamic programming and applied economics was being done at RAND, both by permanent staff and visitors from major universities. The atmosphere was collegial and the schedule flexible. Most research projects were chosen by the investigators, and additional work on more fundamental issues was encouraged and generously supported. Among other things, I learned computer programming at RAND. Professional editors and colleagues also helped me improve my communication skills, both written and oral.  While at RAND I pursued a PhD degree in Economics at UCLA. I received the degree in 1961. After completing my field examinations in 1960 I began work on a dissertation concerning the economics of transfer prices. At the suggestion of Armen Alchian, my preliminary results were reviewed by another faculty member who had previously done research on the subject. He thought that I should consider some other topic. Fred Weston suggested that I might see if Harry Markowitz, who was then at RAND, had any ideas. He had, and I proceeded to work closely with him on the topic Portfolio Analysis Based on a Simplified Model of the Relationships Among Securities. Although Harry was not on my committee, he filled a role similar to that of dissertation advisor. My debt to him is truly enormous. The dissertation was approved in 1961, at which time I received the PhD degree.  In the dissertation I explored a number of aspects of portfolio analysis based on a model first suggested by Markowitz. At the time I called it the “single index model”, although it is now generally termed a “one-factor model”. Key is the assumption that security returns are related to each other solely through responses to one common factor. In the dissertation I addressed both normative and positive results. The final chapter, A Positive Theory of Security Market Behavior, included a result similar to that now termed the security market line relationship of the Capital Asset Pricing Model, but was obtained in the limited environment in which returns are generated by a one-factor model.  In 1961 I moved to Seattle to take a position in Finance at the School of Business at the University of Washington. Once settled, I prepared a paper summarizing the normative results from my dissertation; the paper was subsequently published in Management Science in 1963. More importantly, I began work on a generalization of the equilibrium theory contained in the final chapter of the dissertation. By the fall of 1961 I had discovered that a very similar set of results could be obtained without making any assumptions about the number of factors influencing security returns. I first presented this approach at the University of Chicago in January 1962. Shortly thereafter I submitted a paper on the subject to the Journal of Finance. An initially negative report from a referee plus a change in editorship delayed publication until September of 1964. Both in content and title, this paper provided much of the basis for what is now termed the Capital Asset Pricing Model (CAPM).  The CAPM is built using an approach. familiar to every microeconomist. First, one assumes some sort of maximizing behavior on the part of participants in a market; then one investigates the equilibrium conditions under which such markets will clear. Since Markowitz had provided a model for the requisite maximizing behavior, it is not surprising that I was not alone in exploring its implications for market equilibrium. Sometime in 1963, I received an unpublished paper from Jack Treynor containing somewhat similar conclusions. In 1965, John Lintner published his important paper with very similar results. Later, Jan Mossin published a version that obtained the same relationships in a more general setting.  I was at the University of Washington from 1961 through 1968, with the exception of a year spent on leave at RAND. At Washington I taught a wide-ranging set of subjects, covering material from the fields of microeconomics, finance, computer science, statistics, and operations research. As is so often the case, I found that the best way to learn a subject was to teach it. Hopefully, the students did not suffer overmuch from their participation in the process.  My research during this period was as eclectic as my teaching. I worked on extensions of the CAPM and empirical tests of its implications. I also published books on the economics of computers (based on research supported by RAND) and on computer programming.  My years at Washington were busy but highly productive. While I relied heavily on colleagues at RAND and at other universities during this period, I was fortunate to have interested and supportive colleagues in Seattle–most importantly, George Brabb, Stephen Archer and Charles D’Ambrosio.  In 1968, I moved to the University of California at Irvine to participate in an experiment involving the creation of a School of Social Sciences with an interdisciplinary and quantitative focus. For various reasons the expectations of many who participated in the experiment were not fulfilled, leading some of us to go elsewhere. I was fortunate to be invited to take a position at the Stanford University Graduate School of Business, to which I moved in 1970. Before doing so, however, I completed a book, Portfolio Theory and Capital Markets , summarizing both normative and positive work in these areas.  My years at Stanford have been all that anyone with interests in both research and teaching could have desired. Throughout, I have had the benefit of stimulating colleagues and students. Much of my knowledge of finance was gained when I participated in a team of three, teaching the first PhD seminar in the field at Stanford in the early 1970’s. Alan Kraus, Bob Litzenberger and I shared not only our experience and knowledge but also an interest in sailing–a sport in which we indulged fairly frequently.  I also learned a great deal from two colleagues, now departed, in the 1970’s. Alex Robichek combined a traditionalist’s view of finance with a thirst for new ideas; Paul Cootner came to the field with totally fresh and innovative views. Both placed a premium on useful theory. Both contributed much, through research and teaching. Their premature deaths caused a tremendous loss for the field of finance, for Stanford and for me.  Other finance colleagues, presently or formerly at Stanford, from whom I learned much include Anat Admati, Doug Breeden, John Cox, Darrell Duffie, Allan Kleidon, Mike Gibbons, Jack McDonald, George Parker, Paul Pfleiderer, [Myron Scholes](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1997/scholes-facts.html), and Jim Van Home. Finance students with whom I worked closely included Marcus Bogue, Guy Cooper, Krishna Ramaswamy, and Howard Sosin.  In 1973 I was named the Timken Professor of Finance at Stanford.  In the 1970s I concentrated most of my research effort on issues connected with equilibrium in capital markets and the implications thereof for investors’ portfolio choices. Following the passage of key legislation in the U.S. in 1974, I began to study the role of investment policy for funds designed to fulfill pension obligations. I also wrote a textbook, Investments, designed to include institutional, theoretical and empirical material in a form accessible to students in undergraduate and graduate programs. The first edition, published in 1978, met with considerable success. The book, now co-authored by Gordon Alexander, is currently in its fourth edition. I am especially gratified by the fact that a number of universities still consider it appropriate for its intended purpose. A variant, Fundamentals of Investments, also coauthored with Gordon Alexander, published in 1989, has also been well received.  In the course of preparing and revising the Investments text, I found it necessary to extend prior theory, create new theory, and perform new empirical analyses. Perhaps the most fruitful example of this activity is the creation of the binomial option pricing procedure, first published in the 1978 edition of Investments. It provides a discrete-state analogue of the Black-Scholes procedure which assumes a continuous time setting. Given today’s computer power, the binomial procedure offers a practical method for evaluating instruments with complex embedded options, and is widely-used.  During this period I served as a consultant first to Merrill Lynch, Pierce, Fenner and Smith and then to Wells Fargo Investment Advisors. In each case my goal was to help put into practice some of the ideas of financial economics.  At Merrill Lynch I was involved primarily in designing services for estimating beta values on a continuing basis for a large set of common stocks and for providing risk-adjusted portfolio performance measurement.  At Wells Fargo I helped with the creation of index funds, passive portfolios tailored to meet investor objectives, estimation of Security Market Lines (and Planes) using forecasts of future cash flows, assessment of portfolio risk, choice of optimal portfolios to track selected indices, and so on. In my opinion, the people at Wells Fargo at the time were among the most creative and innovative in the industry. From them I learned much about the real world of investment. Such knowledge informed my teaching and research in countless ways. Undoubtedly, my greatest debt in this connection is to Bill Fouse, whose vision made Wells Fargo such an exciting and stimulating organization at the time.  I spent the 1976-1977 academic year at the National Bureau of Economic Research as a member of a team studying issues of bank capital adequacy under the direction of Sherman Maisel. My focus was on the relationship between deposit insurance and default risk. The results, published in the Journal of Financial and Quantitative Analysis in 1978, supported the notion of risk-based insurance premia. Empirical work with Laurie Goodman also showed that market values of securities of financial institutions can reveal important information about capital adequacy. The NBER project strongly advocated greater concern with the risk of financial institutions and warned that a system of fixed insurance rates and de facto unlimited coverage with imperfect monitoring and enforcement procedures provides dangerous incentives for those running such institutions to take excessive risk. Would that our results had been heeded by those concerned with savings and loan institutions in the United States in the subsequent decade!  In the latter part of the 1970s I developed a simple yet effective method for finding approximate solutions to a class of portfolio analysis problems. The procedure, described in a Stanford working paper and in my textbook, has been widely implemented, although final publication of the paper describing the algorithm was delayed until 1987, due to confusion at a journal that had planned to publish it.  In 1980 I was elected President of the American Finance Association. I chose as the topic of my Presidential Address, Decentralized Investment Management. My goal was to provide some structure for analyzing the widespread custom of large institutional investors to divide funds among a number of professional investment managers. The subject is interesting both theoretically and practically, and my work on it continues.  In the 1980s I continued to work on issues relating to pension plan investment policy. A theoretical paper on the subject with J. Michael Harrison was completed in 1983. I also became interested in the return-generating process in the U.S. equity market, a subject pioneered by Barr Rosenberg, then at the University of California at Berkeley. This led to an empirical paper on factors in New York Stock Exchange security returns, published in 1982. I also began to focus much of my effort on asset allocation – the allocation of an investor’s funds among major asset classes. To make both the ideas and the technology more widely available, I prepared a package that included a book, optimization software and databases, under the title, Asset Allocation Tools. First published in 1985, it is now offered both by the original publisher and by Ibbotson Associates in conjunction with their much larger set of databases.  In 1983, I helped Stanford establish a program in international investment Management, offered jointly, initially, with the International Management Institute in Geneva, and later, with the London Graduate School of Business. The program, extending over a week, is designed for senior investment professionals wishing to obtain a thorough grounding in financial economic theory and the associated empirical research. I served as Co-Director of the program through 1986 and have participated in subsequent years. Independently, I also helped create a three-week program for the Nomura School of Advanced Management, designed to bring much of the same material to investment professionals in Japan, and taught in the program for five years. I also assisted Sidney Cottle, of Financial Research Associates, in preparing seminars designed to communicate the results of recent research to investment practitioners.  In 1986, I took a two-year leave from Stanford to found Sharpe-Russell Research, a firm chartered to perform research and to develop procedures to help pensions, endowments and foundations select asset allocations appropriate to their circumstances and objectives. Supported by several major pension funds and by the Frank Russell Company, and assisted by a talented group of professionals, I was able to bring previous results from the field of financial economics to bear on these important issues and to provide new theoretical and empirical material of relevance. Subsequent to this period, the firm’s charter was broadened to include consulting for pensions, endowments and foundations in the area of asset allocation. Published work resulting from these activities covered the areas of integrated asset allocation, dynamic strategies for asset allocation, factor models for evaluating manager styles and performance, and liability hedging.  In 1989, I chose to change status, becoming Timken Professor Emeritus of Finance at Stanford, in order to devote more of my time to research and consulting activities at William F. Sharpe Associates, as my firm is now known. While this involves giving up regular teaching, I have the great fortune to be able to continue to participate in the intellectual life of the school. In addition, I can pursue research with a fine group of colleagues and to provide assistance to (and learn from) a highly sophisticated group of clients.  It has been my great good luck to be able to work with a number of organizations in the investment industry. I served as a Trustee of the College Retirement Equities Fund from 1975 through 1983 and currently serve a trustee for the Research Foundation of the Institute of Chartered Financial Analysts, a committee member for the Institute of Quantitative Research in Finance, and a member of the Council on Education and Research of the Institute of Chartered Financial Analysts. I also serve as a Strategic Advisor for Nikko Securities’ Institute of Investment Technology and the Institutional Portfolio Management division of the Union Bank of Switzerland.  I have also received awards from diverse constituencies. I am especially proud to have been the recipient of the American Assembly of Collegiate Schools of Business award for outstanding contribution to the field of business education in 1980 and the Financial Analysts’ Federation Nicholas Molodovsky Award for outstanding contributions to the [finance] profession in 1989.  In the course of this long and demanding career, I have enjoyed the influence and example of my parents and step-parents, all of whom pursued further education in mid-career. My father retired as a college president, my mother as an elementary school principal, and my step-father as a public defender. They taught me by example the joys associated with learning and with communicating the results of that learning to others.  I am also fortunate to have two fine children, Deborah and Jonathan, now grown. Both share a love of learning and of communicating knowledge to others, although they have chosen fields far removed from my own. In 1986 I married my wife Kathryn, an accomplished painter, who shares both my personal and my professional life – the latter in her capacity as Administrator of William F. Sharpe Associates. Without her help, encouragement, and support I truly could not have accomplished what I have in the last five years. We enjoy sailing, opera and Stanford football and basketball games, especially when the weather is good, the music well performed and the opponents vanquished. |
| Autobiographical |  |
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| Telephone  interview | 0868 |
| Interview |  |
| Q6 | Very pleased to see you here. How did you react that morning, I think it would have been, when they called you and said that you had been awarded the Economic Prize in Memory of Alfred Nobel? |
|  | Well, it was eventful. My wife and I were at a conference in Arizona and the phone rang about four o’clock in the morning, and by coincidence there had been some chap, in Belgium I believe, who was trying to get me to speak at a conference or something, and he’d been calling at very strange times, and so my first reaction when I first heard someone on the other end of the line was that it was this guy from Belgium. And then, you know, I was immediately reassured, and so I was pretty sure … and then, of course, your second thought is it’s a hoax, one of your friends or colleagues, and so I was 99% sure it was real.  After we’d finished the chat we turned on CNN and within about five minutes the first announcement came across, and it had many inaccuracies in terms of there were three of us, but nonetheless there was enough there that we were pretty sure that it was real. So needless to say we were both elated and we ordered room service and sat on the balcony looking at the sun coming up across the desert before everything broke loose and became crazy. But it was … and then I had not even been paying attention to the fact that this was the date for the announcement, so it was very much a surprise, a very welcome one. |
| Q7 | Any particular memory from the celebration in Stockholm that you would like to share with us? |
|  | Yes, we took an extended family, and my father was not well, but all of his doctors gathered together to find ways to get him there and in good health, and he absolutely revelled in it. He marvelled about everything, and as it turned out there were 11 in our party ultimately, and it turned out all of us got colds and were sick to varying degrees, except my father who went strong the entire time through and just had the time of his life, as did all of us, it was truly magical, there’s no other word for it. |
| Q6 | In which way has it changed your life, if it has, professionally? Privately? After the award? |
|  | I think there’s a period, and talking to others I think this happens a lot, in which you almost are unable to function because you feel that people expect so much, you know, out of the next paper. The standards are so much higher because of the prize that it’s almost impossible to write a paper and feel it’s good enough to submit for a publication, so there’s some of that. One gets over that. You certainly, especially in financial economics where there is a tie to the real world and to the world of commerce, you tend to get invited to, you know, ever so many lectures or talks or whatever you want to call them, and you have to have the good sense to limit that and to get back to work if you will. So it’s difficult, there’s no danger of being lionised by your colleagues, because your colleagues know exactly who you are, so that doesn’t change at all.  And so I think it’s basically trying to determine what balance you’re going to strike between, you know, among the things that you’ve always done. Some people feel that they have a /- – -/ and that they should use the position to go out and try to effect policy. Some, to some extent, commercialise it, you know, or take commercial advantage, let me say, of the position, but I think the majority of prize winners try to keep doing pretty much what they’ve been doing, with probably more focus on things that are for the public good perhaps, than they might have put otherwise. |
| Q26 | Have you felt that you had to kind of use this award in a way that would benefit more people? |
|  | My research had been moving towards … My research has always had a substantial, pragmatic aspect towards helping people make sensible investment decisions, trying to understand the determination of asset prices and helping people then understand that and then make good decisions. Conditional on that, and I’d been focused heavily on institutional decision making by large pension funds for example, because that was the major vehicle, public and private pension funds. That was the major vehicle for saving and investment and sort of life time consumption planning, if you will. Up to the early 1990’s, but then as the shift accelerated towards putting more onus on the individual to make saving and investment decisions for retirement. I wanted to turn my research in that direction because there was going to be a huge need to help individuals do it right and sensibly. So that was the direction my research would have taken in any event. And it seemed to me then and it seems to me even more so now that that’s an area in which financial economics has to really help people.  In the Western World particularly? Or right across the board do you think?  William F. Sharpe. Well certainly in the Western developed world and many in the, you know, in the middle ground, I mean the Mexico’s and Chile’s of the world, in the countries that are really just beginning to develop, who are probably quite a way from that, but you’re certainly starting to see this as an issue in China for example. But then where you will in the process, so I think just increasingly that’s, rightly or wrongly, you know, the social programmes are covering a certain level, but even the social programmes, in Sweden for example, are now adding a substantial amount of decision making on the part of the individual.  Which has been very difficult for many individuals to accept, I think.  William F. Sharpe: I’m absolutely certain and there are some very serious issues as to how much latitude you want to give people, at least for a minimalist level of protection, there are some very, very important social issues, yes. |
| Q24 | Do you think we are at some kind of turning point there? I mean with the Western World and governments in the Western World has to rethink? |
|  | I think it needs to be thought through much more carefully. My belief is that the driving force is the demographic shift towards so many more older people per younger person, basically, in so many of our countries, and we need to figure out an appropriate way to share risk across those generations and of course within the generations as well, and I don’t think we’re anywhere near the point of having understood or thought through, especially at a political level, what the trade offs are. And so I think far more needs to be done to inform public policy, but there’s a lot of fundamental, financial, economics work to be done as well.  In the meantime they need to try to take what we think we understand about asset prices and bring it to bear on the decisions that individuals are making and are increasingly making and help them one way or the other to make those decisions intelligently. |
| Q36 | It’s a fascinating field and so much still to be done, I would like to come back to that a little bit further down in our interview, but as a child, were you thinking that you would go into this field? I know that you like sailing for example, I don’t know whether that was later in your life, but as a child were you interested in maths, for example? |
|  | No, well I mean, you know, if you go straight through high school as I was, I enjoyed the technical courses, chemistry and physics and mathematics to an extent. My mother wanted me to be a doctor, so I started out in college in a pre-medical programme, didn’t like chemistry in college, for whatever reason, and shifted to a business major, knowing nothing about business majors or business for that matter. My parents had both been educators, and so the first course I took was the required beginning economics course and until my sophomore year I didn’t know anything about economics, economics was not in the high school curriculum in California in my day, and I loved this course, you know, I loved …  Why?  William F. Sharpe: I loved the beauty of it, I loved the logic of the theory, I loved the practical aspects of it and it just, I just really enjoyed it, and I thought, well, I don’t really know what economists do, I suppose I’ll work for a government or something, but I had to take more of this, so I switched to an economics major. And never looked back. |
| Q37 | I can understand that it must be fascinating. The other thing that you did was also to … you were early going into computers and learning about computer programming. Did you at that time foresee in which way we would be depending on computers, particularly in the financial field? |
|  | Absolutely not no. Again, I think, I went into computer programming partly out of necessity, but I was at a very exciting place in RAND corporation, where some of the most important early work on the computer programming and computer software, if you will, and algorithms was being done. And I’d learned programming both to help me in my own research so I could do my own research processing, but also it was, sort of, like the economics experience. I just loved the logic and the practicality of it and the fact that, you know, it’s very [Pavlovian](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1904/pavlov-facts.html) in that you do something here and then you get an immediate response to the stimulus, so you get rewarded very, very quickly if you do it right. So how much of it, my interest in programming, was pragmatic and how much was just, you know, liking the logic and the practicality of it, I can’t tell you, but I continue to programme to this day, many, many hours. Again, it’s very rewarding. |
| Q18 | And the financial market, I mean, we can sit anywhere in the world and do huge transactions or small transactions, does it scare you or is it a useful tool in today’s financial market? |
|  | I would say “yes” to both questions. It’s frightening, on the other hand it’s extremely useful, and there’s so much we can do to allocate risk officially across regions, across individuals, across institutions. But with that comes danger. You have to learn how to use the tools and you have to build the institutions that can at least minimise, never probably avoid, the risks associated with people making honest mistakes and people doing dishonest things, because both happen. And they’re very, very powerful incentives in the financial world to do things dishonestly, and so it’s very dangerous, but it’s such a force for good if used appropriately, that I think we’ve just got to find a way to minimise the bad things and be able to enjoy the good aspects. |
| Q9 | Every theory that you’re coming up with you should be your own devil’s advocate … Have you continued to have that, brought that with you in your daily life? |
|  | Yes indeed. Because of the jetlag I was awake for several hours last night and working on some theory that I’ve turned my attention to, and I was doing exactly that: Let’s see, I think I can make the following simplification and then say, you know, if I were the reviewer, if somebody had given that paper with that method and I were the reviewer, whose job was to find fault with it, what would I try to find that was wrong with the argument or wrong with the algorithm. I think that’s absolutely essential. Question not only everybody else’s work, but question your own work as you do it, let alone after it’s done. |
| Q18 | The model that you got the prize for, so to speak, the capital asset pricing model, has been part of many students’ text books over the year. Today, how valid is it? And obviously it’s used a lot, but do you see that it needs to be revised at times? How do you look at that theory that you then brought up, so to speak? |
|  | That’s a question that it’s hard to answer in less than 200 pages, but let me take a crack at it. The original capital asset pricing model made a great many, very simplifying assumptions as theories do, but especially the first theory that tries to tackle a particular area and in some ways it was, again others were working within similar areas, I’m not saying that I was the only one, but it was really simple, it was simple, and then that’s a great strength, but it’s also a great weakness. In the period since the early 1960’s people have looked at a lot more elements of what happens in the real world when asset prices are determined. My most recent work has actually started from a different place, started from the [Arrow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1972/arrow-facts.html)–[Debreu](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1983/debreu-facts.html) view of the world, which is different than the [Markowitz](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1990/markowitz-facts.html) view of the world, and tried to explore a lot of the aspects of quote reality. In that setting, that exploration, if you look at the simpler cases that motivated the CAPM, produces the CAPM, in the more complex cases it produces something that would be more nuance than have more complexity.  What continues to come through pretty strongly, sort of the fundamental, economic insight that the risk that is rewarded with higher expected return, it was generally risk associated with doing badly in bad times, that’s sort of the key insight, and that is preserved, not perfectly, but reasonably well in quite broad settings, where you take into account many other aspects: people have jobs, people have houses, people differ in their predictions of the future etc. And so I’ve been working in that area, I have a book in draft form exploring some of that, using simulation technology actually. Computer programming and stuff. So yes, is the CAPM too simple? Yes. Can we do better? Yes. Have we done better? Yes. That is not to say that it’s a simple matter to empirically differentiate among the possible candidates for a broader approach, but I don’t tend to think of the alternatives as replacements for the CAPM, but more realistic models building off the same basic idea of prices being determined by people coming into market and interacting with one another and doing trades and setting prices until equilibrium’s determined. |
| Q70 | … It’s more a complex economic situation in the world today since the first model was presented or the first draft or the model or the version. |
|  | That’s interesting, it raises an interesting point. There was a period in the late 1980’s in which I was working with some models which took into account the stylised fact that it was probably difficult or costly or possibly impossible for people to take so called short positions, to take negative positions in assets. And I’ve gotten some rather nice results, but I more or less, and I described some of that in my Nobel speech, but to some extent I’ve convinced myself that because of the modern developments in risk sharing, with derivatives and exotic financial instruments. Probably the world is closer to the simple CAPM in some ways than it was when I did the CAPM, because now it is possible to take different positions, vis à vis risk, using financial derivatives where before it was very difficult for people who wanted to do that in certain circumstances, to take short positions and engage and get their broker to load them stock etc. So to some extent, I think that the increase in financial technology, broadly construed, has actually brought us closer to some of the “perfect world” in some of the simpler economic models, be they Arrow-Debreu or CAPM type. |
| Q22 | You have yourself gone between the economic world and the so called real world, working as a consultant, starting up your own firm. How important has that been for you, to be able then to test and to verify and to learn as well, I presume, from the so called real world? |
|  | In financial economics at least, I certainly won’t speak for other fields, but in financial economics I think that each side can inform and improve the other. I think that it’s really helpful when you’re doing academic research, to know something about the instruments that are available and some of the transactions costs if you will, and institutional impediments in the real world, and also some of the motivations, if you will, of the people who are players in the real world. I think academic research can be informed first of all as you can focus on unimportant problems, and then you have a better notion of what’s important, and you can build more realistic models of the people who are interacting, creating the worlds that you’re trying to understand.  I think the real world can considerably inform the academic research …  I think the real world can considerably inform the academic research, and of course I have to think that the academic research can be of huge value in the real world, I do think that, and that’s been my experience, and I think the experience of many. I think in fact it’s a bit of a conceit for a financial economist, but I think we think, many of us, that we have had more impact on actual practice than a number of fields both in economics and perhaps in other disciplines. I don’t know about other disciplines, and certainly I think that in many of fields of what’s called business, administration of business education, I think the impact of economists on the financial practice in the world has been really quite profound.  I think, whether you want to do academic work only, or you want to do practical application only, spending time on both sides of the street is helpful. My personal circumstances that I, what I really want to do is understand the world and help people, you know, make better decisions in the world, so given that particular utility function, it was imperative that I spent time on both sides, and what I’ve been trying to do over the course of my career is get the balance right. Sometimes it’s too much on one side, sometimes it’s too much on the other side and so it’s a delicate balancing act. |
| Q21 | What advice do you give to young students who are looking into the field of research? People that you have met and that you are meeting, maybe right now? Are there any particular fields that one should look into? Given what you said earlier about the way that the individual today, particularly in the Wstern World, has to really think about ones finances and particularly in the  pension schemes, for example. |
|  | I think there are two issues. If you’re advising a PhD student as what to do and the student wants to have an academic career, there are certain things which will give you a better chance of going to a top university or research institution, and they tend to be more abstract, they tend to be more technical, they tend to be in areas that are particularly demanding, technically demanding, at least that’s my experience. On the other hand, if you want to do something that’s going to change the world in important ways, then you do want to look at the big problems, that are both big and hard, and potentially problems that you can contribute to solving …  For example?  William F. Sharpe: Well, again my personal preference at the moment is helping individuals or helping societies decide how to balance the demands of those who want to consume, be they old, retired people or young working people, and those who have to produce. So how are we going to take the amount of production that we can get out of the society and distribute it among the people in the society? And that has of course a temporal aspect as the traditional way is that you work when you’re young and then you consume less than you produce, and then when you’re old you consume more than you produce, and how we’re going to work all that out given huge uncertainties and particular uncertainties on the mortality, healthcare side which are, I mean grave uncertainties for the individual but even societal.  And that of course has huge implications for distribution of production and consumption across the globe between the less developed world, where we’re still getting population increases, and the developed world where the population at least, add some immigration, is either stagnant or actually going to be decreasing apparently. So there are huge issues to be dealt with there, it cuts across many, many fields of economics and many fields that aren’t economics, and so that’s an area that I find fascinating and is clearly important and the area that I spend most of my time thinking about. But that doesn’t mean everybody should. |
| Q72 | That does lead me to the last question really, and it’s been fascinating to listen to you, and that is if we just look at US economy today, for example, the huge budget deficit and obviously taking on in a politically huge commitment outside America, I’m thinking about Iraq, for example. How volatile is the world economy when we have all these uncertain factors, and when you can see one of the largest economies in the world having such huge economical problems as we are now seeing in America? |
|  | First let me give you a caveat, macroeconomics and monetary economics, for that matter international economics are not my fields, so my opinion is not a very informed opinion, I’ll give it to you anyway. I’m very disturbed obviously, as is everyone, with what’s going on, things that you mentioned. I’m not at all convinced that our responses politically or economically have been the best responses to the situations that we’ve been in the last few years, and I’m speaking of the United States. In fact, I have fairly strong opinions to the contrary, but again, I don’t want to say that those are deeply informed opinions from a professional standpoint, but I think the general issue of the increased uncertainty, one of the fascinating issues, and it’s a sort of a knee jerk reaction when you’re asked the question, is to think about allocating that risk across the global world.  Take a trivial example. Let’s say we know that there will be a level x of terrorist incidents across the globe, but we don’t know where they’ll be or perhaps what form they’ll take, but the law of large numbers is such that we can predict, with quite some accuracy in some abstract way, the level. Then presumably we could set up various kinds of insurance or financial contracts across the globe to pool that risk so that obviously the people who are directly and personally affected get the pool of risk, of being injured or killed. But nonetheless you can ensure some of the economic effects, so it’s possible that with good risk pooling procedures, and we’re better at that now than we used to be for all the reasons we’ve spoken about, that we can ameliorate the impact of that on any given person rather than having a situation where you’re attacked and suffer badly and I’m not attacked and I’m fine.  … the idea of global terror insurance is something that comes naturally to mind …  We can pool the risk so we’ll both suffer to some extent but not differentially other than the direct personal costs of attacks, so the idea of global terror insurance is something that comes naturally to mind for somebody thinking about risk sharing and risk pooling. That can help ameliorate the uncertainty and perhaps dampen the impact in terms of a global meltdown. But nonetheless it’s terribly concerning even with the best risk sharing in the world, and of course from a human standpoint everything that’s going on. And I’m worried, obviously you alluded to the issue of deficit financing, I’m worried the inner generational issues, which I think are not trivial at all, and have not been thought through that well by politicians, and certainly have not been brought to the level in political discourse, at least not in the United States, to allow anything like an informed decision. I despair of politically informed decisions given the nature of our politics at least, on much of anything, so I think there’s much more that needs to be done, and it’s a very frightening time. Very frightening. And I think economics can help! Can’t solve all the problems by any means but I think it can help. Good economics. |
| Q24 | Because when you see the huge disparity in the world, I mean, between the developed and the developing world, there’s another huge concern, of course, which we could … I mean, what is feeding, what is feeding the terrorist attacks that we are seeing as well, and that’s a huge political question obviously but I think as well not economical question. |
|  | I mean even if you take, you know, the classic economic person argument where everyone cares only about his or her welfare, you know, narrowly construed, which is a terrible view to have to take, but even if you take that view, I think, you know, what you’re suggesting I believe makes great sense. It undoubtedly is worthwhile for people in the highly developed countries to do more than they’re doing now, to redress some of the disparity between highly developed and highly undeveloped or far less developed countries, just as a matter of pure self interest. Now, of course, if you add to that some concern for other people, then you’d get an even stronger case, so I do not think, and I will say mea culpa especially in the case of the United States. I don’t’ think we’re doing anywhere near enough from a humanitarian developmental aspect, strictly from a self interest standpoint, and of course I would think we should do more than that, but that’s my personal preferences. Shall we. |
| ID | 0869 |
| Biographical | The field of econometrics is concerned with estimating economic relations and testing whether postulated relations conform fully with reality. In an article in Econometrica in 1943 and in his doctoral thesis entitled, The Probability Approach in Econometrics (1944), Haavelmo showed that the results of many of the methods used thus far had been misleading. Earlier methods did not sufficiently account for the fact that real economic development is determined by interaction of a multitude of economic relations and that economic laws are not strictly rigorous.  In his thesis, Haavelmo presented a new and pathbreaking approach to the estimation of economic relations by applying methods used in mathematical statistics. His work established the foundations for a new field of research which came to dominate the study of estimating complex economic relations.  In his review of Haavelmo’s doctoral thesis, the British Nobel laureate [Richard Stone](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1984/index.html) wrote that it was a brilliant contribution to econometrics which would have a revolutionary effect on the degree of success in estimating economic relations.  After he became professor at the University of Oslo, Haavelmo’s research interests turned to economic theory. His book, entitled, A Study in the Theory of Economic Evolution (1954), was a pioneering study of the possible reasons for economic underdevelopment of a country in relation to other countries, long before other economists became seriously engaged in development research.  Haavelmo also made a valuable contribution to the theory which determines the development of investments in a country. His book, entitled, A Study in the Theory of Investment (1960), introduced theories on the demand for real capital, and sluggishness in the adjustment of real capital, which have been of fundamental importance in subsequent research. Numerous theoretical and empirical studies of investment behavior have been inspired by his work.  Many of Haavelmo’s other studies, such as a monograph on environmental economics which appeared long before such research came into existence, have been an inspiration to other researchers.  Haavelmo has had a decisive influence on economics in Norway – not only as a researcher, but also as a teacher. During his active years at the Institute of Economics at the University of Oslo, he was the leading teacher in the field. He covered numerous areas of economic theory and many of his students and assistants received their first instruction in authorship by writing expositions based on his lectures – under stimulating guidance. No less inspiration was given to the many research recruits for whom Haavelmo served as advisor.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1989, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1990  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.  Trygve Haavelmo died on July 26, 1999. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0869 |
| Interview |  |
|  |  |
| ID | 0870 |
| Biographical | My youth I was born May 31, 1911, in Paris. My parents owned a small cheese shop, and my maternal grandfather was a carpentry worker. I thus came from what is commonly known as the working class.  In August 1914, my father was called to war, and then taken prisoner. He died in captivity in Germany on March 27, 1915. My youth, indeed my entire life, was deeply marked by this, directly and indirectly.  Albeit in often difficult conditions, I was nevertheless able to pursue my secondary studies. I received my high school baccalaureate diploma in Latin and Science in 1928, then my two baccalaureate diplomas in Mathematics and Philosophy in 1929. Throughout my college career I was generally first in my year in almost all subjects, including French and Latin as well as Mathematics.  Fascinated by History, I wanted to apply to the Ecole des Chartes, but on the insistence of my mathematics teacher I entered the special mathematics class in order to prepare for the Ecole Polytechnique, which I entered in 1931. I graduated first in my class in 1933, which is commonly considered to be a “summum” in France. Indeed, the Ecole Polytechnique, together with the Ecole Normale Superieure, are the top of French education in the sciences.  My choice of a government administration upon graduation was the “Corps National des Mines”, not because of any particular vocation, but simply because each year the top graduates of the Ecole Polytechnique (three in my class) always chose this government service because of the career possibilities it opened up in the country’s large industrial enterprises.  After a year of military service, first in the Artillery School at Fontainebleau and then in the Alpine Army, and two years at the Ecole Nationale Superieure des Mines in Paris, I started as an engineer in the mines public service in October 1936.  My professional career In 1937, at the age of twenty six, I found myself in charge of the Nantes Mines and Quarries Service, which included five of the 89 French “departments”, and also put in charge of a number of controls, in particular that of the general and local railway system.  In 1939, I was called back to the Alpine Army on the Italian front, and was given command of a heavy artillery battery in the area of Briancon. But the real war only lasted two weeks, from June 10, 1940, when Italy declared war on France, until June 25, 1940, the date of the armistice.  Released from service, I took up my old position in Nantes in July 1940 in the German occupation zone. From October 1943 to April 1948 I was director of le Bureau of Mines Documentation and Statistics in Paris.  From January 1941 to April 1948 I simultaneously carried out my administrative functions and published my first works: two fundamental works, A la Reserche d’une Discipline Economique, (In Quest of an Economic Discipline), and Economic et Interet (Economy and Interest, 1947); and three minor works, Economic Pure et Rendement Social (Pure Economics and Social Efficiency, 1945), Prolégomenes a la Reconstruction économique du Monde (Prolegomena for the World Economic Reconstruction, 1945), and Abondance ou Misère (Abundance or Misery, 1946), as well as various news articles. Throughout this period I worked very hard, at least eighty hours per week.  From April 1948 on, I was relieved of all administrative duties and was able to devote all my time to teaching, research, and writing for publication. I was professor of Economic Analysis at the “Ecole Nationale Superieure des Mines ” from 1944 on, and Director of a research unit at the “Centre de la Recherche Scientifique ” (C.N.R.S.) from 1946 on. At various times I held teaching positions at other institutions, such as the Institute of statistics at the University of Paris (1947-1968), the Thomas Jefferson Center of the University of Virginia as a Distinguished Visiting Scholar 1958 – 1959), the Graduate Institute of International Studies in Geneva (1967-l970), and the University of Paris-X (1970-1985).  I retired from the civil service on May 31, 1980, but, thanks to the Ecole Nationale Supérieure des Mines and the Centre National de la Recherche Scientific, I have been able to keep some means for working and to continue to in teaching, research, and writing.  I have received many awards for my works (fourteen scientific prizes from 1933 till 1987). The most important was the Gold Medal of the National Center for Scientific Research (C.N.R.S.), the most distinguished honour in French Science ( as a rule there is only one Gold Medal every year for all sciences). It was awarded to me in 1978 for my lifetime work, the first, and, so far, the only time an economist has ever received this honour.  My involvement in applied economics and politics In addition to the above activities I have undertaken economic studies for both private and nationalized firms, and for theEuropean Economic Community.  Throughout the years following World War II and until the formation of the European Economic Community in 1958, I was very active as a national or international rapporteur at many of the international conferences aiming to establish an European community. I also took part in various international conferences with the view of the foundation of an Atlantic community and I was rapporteur at the “NATO in Quest of Cohesion” international conference organized in 1964 in Washington by the Center of Strategic Studies of Georgetown University.  Finally, from 1959 to 1962, I was also founder and general delegate to the Movement for a Free Society, a liberal (in the European sense of the term) para-political organization.  My contributions to economic science My contributions to the fundamental Economic Science have essentially focused on five fields, all concerned with the research of the conditions for a maximum efficiency of the economy and with the analysis of the corresponding determining factors of the distribution of income. I have given a broad outline of these contributions in my Nobel Lecture.  My work in applied economics On a national level and in close connection with my work in economic analysis, I was led to study, more particularly, four areas of applied economics: economic management, the distribution of income and taxation, monetary policy, and the economy of energy, of transport and of mining research.  From the point of view of the management of the economy, the demonstration of the equivalence of states of maximum efficiency and states of equilibrium of an economy of markets (markets in the plural) is naturally of great import. It shows indeed that any economy whatsoever, whether collectivist or private property, must be organized on the decentralized basis of an economy of markets in order to be efficient and to use at best the scarce resources at its disposal.  What are then the conditions of implementation of an economy of markets? What are the ethical questions raised by such an implementation? Can the techniques of an economy of markets and the ethical aspirations of our time be reconciled? What are the monetary conditions of growth? What are the conditions of full employment? Such are the questions which I tried to answer. My major conclusion was that both the economic and ethical objectives of our time can be reached at the same time only if the institutional framework within which the economy works is appropriately reformed, and I have tried to specify the principles for such a reform.  On the international level, the active part I took in various organisations such as the European Union of Federalists, the European Movement, the Movement for an Atlantic Union, and the European Economic Communities, together with my lecturing for several years at the Institute of International Studies in Geneva, have led me to study thoroughly, in various works and memoirs, the international factors of economic development, the liberalisation of international trade, the monetary conditions of international economic relations, and economic unions.  In my study of the factors of development, as well as in that of the various economic systems, I was led to make numerous researches on the compared real income and productivity of France, the Soviet Union and the United States, to study in detail the economies of these countries, and to analyse the possible causes of the productivity differences observed. This analysis shows that the main explanatory factors are their systems of economic organisation together with the institutional framework within which they operate.  At the same time, my contacts with administrative and industrial circles led me to study, in my memoirs on the economy of energy, of transport and of mining research, three series of questions which I was asked on several times. What must the energy policy of investments, exploitation and price be in order to be considered effectively satisfactory? According to what principles must a rational coordination and tariff policy of transports be established? What is the optimal strategy to adopt for the mining research of mineral deposits? All these problems led me to study very diverse and concrete questions, and to reflect on numerous aspects of economic theory, econometrics and operational research. The – often new – solutions which I gave them gave rise to many debates in engineering circles and led many engineers to study economic theory and to apply it to their respective fields.  For my 1952 memoir on mining research, published in English in 1957, I was awarded The Lanchester Prize 1958 of the Johns Hopkins University and the Operations Research Society of America for the outstanding paper, on Operations Research, published in 1957.  All my works in applied economics are closely linked to my works in economic analysis. Theoretical analysis naturally led me to applications, and the study of concrete questions has led me to reflect on the theoretical foundations from which it was possible to provide satisfactory answers.  I have been constantly driven by the conviction that a man of science cannot fail to take an interest in the fundamental problems of his time. I have of course never ceased to think that, whether as an adviser or a teacher, the economist as such should not take a stand on individual ends which often are contradictory. The ends to pursue belong to the field of politics and it is in fact the essential task of political systems to define them through overall compromises. But precisely, on the economic level, the economist’s role is to examine whether the ends defined through such comprises are actually compatible with each other and whether the means used to reach them are really the most appropriate.  On the whole, on the level of the analysis as well as on the level of applied economics, my work has endeavoured to rethink the role of economic liberty and of an economy of markets as regards the search for efficiency and the achievement of the ethical objectives of our time, and to contribute to a thorough study of the questions raised by the economic organisation of societies.  There is no doubt that my works in applied economics have been influenced by a philosophy of liberal inspiration (in the European sense) along the lines of Alexis de Tocqueville, Leon Walras, Vilfredo Pareto, and John Maynard Keynes, to name but a few. But, whatever this influence may have been, I have constantly endeavoured to keep my analyses on as objective and as scientific a level as possible. In fact, all my works in applied economics are particularly marked by two characteristics, the first being that they are always founded on a thorough theoretical analysis, the second that they are constantly preoccuppied with the quantitative aspects of the questions studied.  My two parallel interests During my whole career since 1936, I have had two parallel interests to which I have never ceased to devote an important part of my activity: history and physics.  My research on the history of civilizations It is in the course of my secondary studies that I first was passionated for history. That passion has never left me since.  From 1961 to 1968 I wrote the first version of a general book, “Essor et déclin des civilisations-Facteurs economiques” (Rise and Fall of Civilizations – Economic Factors), which I have continued to improve and develop at different times over the past twenty years. This work, as ambitious as it is daring, tries to draw out permanent regularities, particularly quantitative, from the history of civilizations, dealing with economic systems, standards of living, technology, monetary phenomena, demographic factors, inequality and social classes, the respective influences of heredity and environment, international relations, exogenous physical influences on human societies, and political systems.  My research on the economic and social factors of the history of civilizations has been extremely enlightening for me. Nothing can be more formative than the study of the history of facts, doctrines and economic thought. Whether it be economic systems, the evolution of real income, monetary phenomena, demography, international relations, ideologies, or the interactions of these factors and their relationships of cause and effect, nothing can be more significant than their analysis.  My work in theoretical and experimental physics My involvement in physics dates from my reflections on physics, mechanics, and astronomy courses at the Ecole Polytechnique. Had the National Centre for Scientific Research existed in 1938, I would have devoted myself to the study of physics and would not have become an economist.  But there again, over the past fifty years, while pursuing my activities as an economist, I have never stopped reflecting and working at various times on the problems involved in the elaboration of a unified theory of gravitation, electromagnetism, and quanta.  On the experimental level, and as a by-product of this theoretical research, I conducted, from 1952 to 1960, experiments on the anomalies of the paraconical pendulum (a short pendulum, about one meter long, suspended by a steel ball), anomalies the existence of which I proved. For these experiments I received the 1959 Galabert Prize of the French Astronautical Society, and I was laureate in 1959 of the United States Gravity Research Foundation.  My main idea at the start was that a link could be established between magnetism and gravitation by observing the movements of a pendulum consisting of a glass ball oscillating in a magnetic field. Of all the observations made in 1952 and 1953 I was not able to draw any definitive conclusion. Through certain experimental devices, I obtained positive effects, but with other devices I obtained no effect whatsoever. A much stronger magnetic field would have been necessary, but it was unrealizable in my laboratory with the available means.  But in the absence of any magnetic field other than that of the earth, I observed, in the course of continuous observations, pursued over periods of about one month from 1954 to 1960, very remarkable anomalies in the movement of the paraconical pendulum, to wit essentially the existence of a significant periodicity of the order of 24 hrs 50 min. Identical results were found in June-July 1958 in two laboratories, some 6 km away from each other, one in a basement, the other in an underground quarry.  At the same time, I observed in the second half of July 1958 a correspondence between the anomalies in the movement of the paraconical pendulum and the anomalies observed in the optical sightings on a fixed sighting mark through a fixed telescope.  Finally during the total eclipses of the sun on June 30, 1954, and October 22, 1959, quite analogous deviations of the plane of oscillation of the paraconical pendulum were observed.  In fact, all these phenomena are quite inexplicable within the framework of the currently accepted theories.  With regard to all these results as well as to their analysis I can make a prediction: if, without interruption, for at least one month, in the same place and at the same time, observations of the movement of the paraconical pendulum are made, together with optical sightings such as those I made, as well as a repetition of the Michelson-Morley (1887) and Miller (1925) experiments, the purpose of which was to display the movement of the earth relatively to the “ether”, it will be found that the effects observed by Miller in 1925 correspond to the anomalies in the movement of the paraconical pendulum and the anomalies of the optical sightings which I observed.  All my researches in theoretical and applied physics which, at first sight, appear to be remote from my main activity as an economist, have, in reality, enriched me with valuable experience.  These researches, which constantly presented all kinds of very great difficulties, have led me to reflect on the nature of our knowledge, the nature of experience and theory, the difficulties of experimentation and the interpretation of results, and the scientific method in general.  I have been particularly struck by the identity of problems relating to the construction of models and the meaning of empirical data in economics and physics. Nothing has been more instructive for me than this confrontation between two apparently so dissimilar sciences.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1988, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1989  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.  Maurice Allais died on 9 October 2010. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0870 |
| Interview |  |
|  |  |
| ID | 0871 |
| Biographical | I was born in Brooklyn, New York on August 23, 1924, the oldest of three children. My parents were themselves the children of immigrants. They had to earn a living as soon as they finished secondary school. So my sisters, my cousins and I were the first generation of our family to attend a university. I was educated, and well educated, in the neighborhood public schools of New York City. I was good at school from the very beginning, but not very intellectual until my last year in high school. Then one of those teachers who make a difference taught me to read the great 19th century French and Russian novelists, and to take ideas seriously. I won a scholarship to Harvard College and arrived there in September 1940.  Like many children of the Depression, I was curious about what made society tick. My first studies were in sociology (with Talcott Parsons) and anthropology (with Clyde Kluckhohn) as well as elementary economics. By the end of 1942, when I had turned 18, it seemed that there were more urgent and exciting matters than what I was doing, so I left the university and joined the U.S. Army. I served briefly in North Africa and Sicily, and then from the beginning to the end of the war in Italy, until I was discharged in August 1945.  I think that those three years as a soldier formed my character. I found myself part of a tight-knit group, doing a hard job with skill and mutual loyalty, led by one of the most remarkable men I have ever known, who never wavered from the path of humor and decency. Twice again I have had similar experiences: in Walter Heller’s Council of Economic Advisers (along with [James Tobin](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1981/index.html), Kermit Gordon and Arthur Okun) and, for most of my adult life, in the Economics Department at M.I.T. Day in and day out, that is the best sort of environment. The only other thing one could ask for is a warm and happy family life, and that I have certainly had.  Upon returning to Harvard in 1945, now married to the reader and writer of all those V-letters, I chose, almost casually, to go on with economics. By a piece of good luck, [Wassily Leontief](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1973/index.html) became my teacher, guide and friend. I learned from him the spirit as well as the substance of modern economic theory. He was also responsible for my introduction to empirical work: as his research assistant I produced the first set of capital-coefficients for the input-output model.  Somehow – the memory is lost – I became interested in statistics and probabilistic models. In those days, the teaching of statistics at Harvard was, to put it kindly, eccentric. I learned a lot from Frederick Mosteller, whose appointment was in the Department of Social Relations. Eventually he advised me to study more intensively in a place where that was possible. So, in 1949-50, I spent a fellowship year at Columbia University, in the lectures of Abraham Wald, Jacob Wolfowitz and T.W. Anderson, along with my fellow student and friend, Jack Kiefer. During that year I was also working on my Ph.D. thesis, an exploratory attempt to model changes in the size distribution of wage income using interacting Markoff processes for employment-unemployment and wage rates. The thesis was awarded the Wells Prize at Harvard, which offered publication in book form and $500 (in 1951 prices!) upon completion. When I reread the thesis, however, I thought that I could do it better. But I never returned to that work and the thesis remains unpublished (and the check uncashed).  Just before going off to Columbia I was offered and accepted an Assistant Professorship in the Economics Department at M.I.T. I have never had or wanted any other job. M.I.T. hired me primarily to teach courses in statistics and econometrics. In the beginning I fully intended to make my career along those lines. It did not turn out that way, probably for a geographical reason. I was given the office next to [Paul Samuelson’s](https://www.nobelprize.org/nobel_prizes/economics/laureates/1970/index.html). Thus began what is now almost 40 years of almost daily conversations about economics, politics, our children, cabbages and kings. That has been an immeasurably important part of my professional life. I suppose it was inevitable that I should drift back into “straight” economics, where I discovered an instinctive macroeconomist struggling to get out.  The M.I.T. Economics Department has been a wonderful place to teach and work. It has provided me – and not only me – with sharp and delightful colleagues, and with a long line of spectacular students. I estimate that if I had neglected the students, I could have written 25 percent more scientific papers. The choice was easy to make and I do not regret it.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1987, Editor Wilhelm Odelberg, [Nobel Foundation], Stockholm, 1988  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.  Copyright © The Nobel Foundation 1987 **Addendum, May 2005** The original autobiography left me teaching at MIT in 1987. I retired in 1995, although Samuelson and I still occupy neighboring offices there, and still compare notes. By 1995 there was no compulsory retirement age, but I thought the Department would be better off with another young teacher and researcher.  During the past decade I have continued to work – though inevitably more slowly and less energetically – on targets of opportunity, especially a long collaboration with Frank Hahn on macro-theory. We found ourselves disaffected – to put it mildly – with the assumptions and methods that had become standard in short-to-medium-run macroeconomics. Most of all we rejected the representative-agent models that more or less impose optimal properties on observed trajectories. Our own try at proposing alternative models has not attracted attention, possibly because it tends to be opaque and messy.  Thirty years ago I was one of the founding directors of the Manpower Demonstration Research Corporation, a non-profit research group that has done pioneer work in the rigorous experimental testing of policy interventions that aim at improving the employment and earning power of disadvantaged groups like high-school drop-outs, welfare mothers, etc. The organization has flourished, both intellectually and practically, as the prime source of validated information about the actual effectiveness of such policies. I am currently chairman of its Board of Directors.  I have also participated in several studies conducted by the McKinsey Global Institute aimed at explaining the sometimes large international differences in performance of certain industries. The idea was to combine the use of the usual data with information gleaned from acquaintance with individual firms in the industry. Martin Baily and I have written an article summarizing this work.  More recently I have succeeded the great sociologist Robert K. Merton as Foundation Fellow of the Russell Sage Foundation. My first project there was an attempt to account for the extraordinary success of the U.S. economy during the years 1995-2000. This resulted in a book (The Roaring Nineties) edited and introduced by Alan Krueger and me. My current activity is as one of the originators, planners and advisors of an absorbing comparative study of the nature and institutional background of low-wage work in the U.S. and a few European countries, and their differing labor-market outcomes. It is too soon to know what generalizations will emerge.  One of the by-products of involvement in these projects is a reinforced skepticism about the way that labor and product markets are handled by the currently fashionable style in the macroeconomics of the short and medium run. The traditional assumptions are sometimes useful, or even necessary, but have to be treated very gingerly and not simply given the benefit of the doubt. The long-run growth-theory enterprise strikes me as being in better shape. It went through a phase of uncritical acceptance of “easy” routes to endogeneity, but may be coming back to a more historical view. There seem to be many opportunities for further progress. **Copyright © The Nobel Foundation 2005** Robert Solow died on 21 December 2023. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0871 |
| Interview |  |
| Q18 | Your lecture here in Lindau has been about low wage workers in high income countries. That is a typical Western World scenario. In Germany, for example, the government is suggesting that jobless people should stay on the social benefits, but above that get a job and only get paid one euro an hour. Is that a way to move ahead, to try to solve this enormous problem of the joblessness and the less work that is available to people in this part of the world? |
|  | Something of that sort, I think, has to be done. The background for this is very important. We are all, both in Europe and in North America, looking forward to a time when our populations will have aged, when there will be fewer people of working age, compared with the size of the population to be supported. So labour will be scarce and it is important to have people who can work, working. In Europe this is compounded by the fact that even now there is quite a lot of unemployment, much more unemployment than in the United States for instance and more than Europe has had in the past.  So, how to get people who are now unemployed back into the labour force and back into jobs? If a civilised country like German, like Sweden, like any, would like to limit extreme poverty, then people who are unemployed and have no income have to be provided for, some way. And if they are provided for relatively generously, they may have no motive to work, even if jobs were available, because their benefits may amount to as much as they could earn in a labour market and they come without the effort of getting up early in the morning and going to work and doing perhaps some unpleasant work and coming home tired in the evening. So, this creates a problem.  One way to deal with that problem is to somehow, even for people whose earning power is very low, to make their work pay them enough so that it is preferable to work than to sit with benefits. In Germany, the proposal is that people who have been unemployed, perhaps for a long time, and have been receiving social benefits, if they go to work, they find a job and go to work, then they continue to receive their benefits, but they receive an additional one euro and hour or two euros an hour on top of that. That does make work more profitable, make work yield more to a person than simple benefits, but it’s a small amount and there is something a little insulting about the one euro an hour that you get for your work.  In the United States we have tried to attack the same problem, even though we have less unemployment than Europe, by what is called the Earned Income Tax Credit and it works this way: If you work a certain number of hours a week, a minimum of a certain number of hours a week, whether it is 20 or 30, I don’t remember, it doesn’t matter, and if you earn less than a certain amount, you fill out a tax return. But instead of paying a tax, you get a payment back from the tax authorities, enough to raise your income, often by a substantial fraction, by as much as 1/4 or 1/3, which is a little better than a euro an hour or two euros an hour. And this is a way of letting the labour market work and asking employers to pay only what the job normally brings in the market, but then putting on top of that, a negative tax, a rebate, a credit from the Federal Treasury which can bring the income for such a person to perhaps a decent level and this has been a solution to the problem in the US and something like it. I’m not sure that the German device is the best, but something like it could be used elsewhere. |
| Q18 | Could there be another solution to the so called Western problem of ageing population, less people working. There are millions of people in the so called Third World who really would like to come and earn a living in this part of the world, but they are not really welcome. |
|  | That would be a solution, but a solution that brings obviously its own problems. The arrival of immigrants from poor countries, Third World countries, into Europe or into the United States, does two things for the ageing population. First of all it brings a number of young people of working age and secondly, those people normally have more children than Swedes or Germans or Americans are now having, so they will help in the future with the population. From that point of view, immigration is a solution to the problem. The difficulty is that many Americans, Europeans, resist immigrants. They see their culture changing, they see the streets look different than the way the streets used to look, you know, from some points of view that is an additional benefit, a little variety is a good thing. A street full of everyday Americans is not so interesting as a more colourful street, but it does create political resistance and it’s impossible to know how that will come out. I presume what will happen is that there will continue to be immigration but rather less than there might be or there might be use for in terms of working population. |
| Q46 | Is it so that we in the Western World have created a situation where our material standard is so high, our demands are so high that we really are not able to forsake anything, any longer. I mean, what is the solution? Must we forsake anything to make this world become a bit more, less divided between the rich and the poor? |
|  | That would be a very nice outcome, but I do not think it’s likely to happen. You know, there has hardly been a time in the past 200 years when you could not have said something similar, even when standards of living in the West were much lower than they are now. They were better than they had been and they were better than in poorer countries and the possibility was always there that the rich people of the Western countries would tax themselves, so to speak, to benefit the poor elsewhere. I do not think mankind has a very great record at caring seriously about people far away, whom they do not know, who speak a different language, who live a quite different life, so I suspect that if a solution to that enormous world inequality was a dreadful thing, I think the solution is more likely to come from raising incomes abroad in Africa, Latin America, Asia and elsewhere, than from generating sacrifices from Europeans and Americans, who like their comforts and I do not think one can blame them for liking their comforts. So I think our efforts look to be focussed on trying to raise standards of living elsewhere through primarily the work and investment of the people there. |
| Q24 | Can we expect this idea of continued economic growth when we know that there is a limitation to the world’s natural resources though? |
|  | I am a little more optimistic than most people about that, for these reasons. We know that as incomes rise, at least this has been true in all of the Western World, what people want to spend their incomes on is services, rather than material goods. This is often put amusingly by describing ourselves as moving toward a weightless economy, an economy in which the gross domestic product will not weigh as much as it does now and that is clearly happening. Now education is much less resource using than driving an automobile and banking, financial services, is much less resource using than food production. But in fact, as we get better off, we spend a much smaller fraction of our income on food and a much larger fraction on education, on financial services and on services generally, so there is some tendency, an almost automatic tendency for countries as they get more wealthy to behave in a somewhat more sustainable way.  The second thing is, we have not yet made a real effort in the technologically advanced Western World to encourage renewable sources of energy. We are still an oil and natural gas based civilisation. Now we could, I don’t know exactly when, because a lot of technological progress is needed and that is always uncertain, but sooner or later we will be shifting noticeably toward energy derived, well all of it comes from the sun one way or another, but from wind or from direct sunlight and so we will be able to conserve energy resources much more than we do now. Even automobiles, you know, can be run without the use of petroleum based fuels, so with a little more emphasis on renewable resources, we still of course, by the way, in Germany, in Sweden, in the US, we produce an incredible amount of waste.  My pet complaint is packaging. Everything one buys comes in packages …  My pet complaint is packaging. Everything one buys comes in packages which first of all, it takes an engineer to get them open and I have spent a whole aeroplane trip trying to open the little envelope with peanuts and don’t succeed and then we simply throw away the wrappings. If we never wrap them so much in the first place, we could also … So between conservation and the consumption of services, I think the situation is serious but not desperate and our governments could probably encourage the conservation of materials much more than they do, either through regulation or through taxation or things like that. So, we will have to work our way in that direction but I don’t think it’s hopeless, I don’t think the situation is hopeless. |
| Q70 | That’s right, yes. I was wondering your opinion. Is there a time to look into alternative ideas around economic thinking, you know, environmental economists, developmental issues? Do you think we’re going to see a different kind of prize takers in the future? |
|  | I think that the Royal Swedish Academy of Sciences has already begun that process. Of course one of the earlier Nobel Prizes in Economics went to development economists, [Arthur Lewis](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1979/lewis-facts.html) and [Theodore Schultz](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1979/schultz-facts.html), but [Amartya Sen](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1998/sen-facts.html) was a very recent prize winner and he is clearly unorthodox in the sense that he is concerned with problems that most economists don’t think about. He’s a person who seems to be as much read by philosophers as he is by economists. I think that there may very well be soon a prize given to environmental economists, I think there’s every reason to believe that the tendency is in that direction and I think that’s on the whole a good thing and I would like to see it continue.  It may also be that economics, science of economics itself, will turn more in the direction of economic development and environmental preservation and things of that sort. There’s a very interesting history. In the 1950s or 1960s, development economics, the study of development, the evolution of poor countries was very popular amongst graduate students. Then it kind of lost popularity, I think because there was not much success in generating new ideas and if the new ideas aren’t there, then young students are not likely to be attracted to it. There has been a revival of development economics, at least there has been in my own university, at MIT, and I think, although I’m not sure about other places and I think it’s because there is some intellectual ferment, there are new ideas. There are new kinds of data available so one can ask questions that previously could not possibly be answered because the basic information was lacking and that seems to be changing and I think that’s a good change. |
| Q71 | Is there a realisation as well, some kind of ethical even or moral realisation that maybe the financial economy or the financial market is not the full solution to the problem that the world is facing? |
|  | Yes, but I think that the ethical imperative has always been there. I think that economists, academics and their students have always felt that there was some obligation first of all to advocate policies that will help poor countries lift themselves out of poverty and some obligation to think about those questions and try to learn more about what it takes to improve situations and you know, there have been some notable successes and the biggest poor country of all, China, now appears to be advancing at a fairly rapid rate, so I really think the problem has been, at least in academic circles, discouragement. No-one wants to spend their life working on a problem that is too hard to solve. For one thing, it is not easy to get promoted when you’re doing that, but for another, you get your pleasures from academic work in economics by working on problems that you can just barely solve, that are hard, so you feel good if you have succeeded, but that are not so hard that you can’t get anywhere.  What I’m hoping is that there will be this kind of intellectual advance and one other source of discouragement about this in the past has been the feeling, and I think this is what was in your mind, that merely trying to provide funds for poor countries, the sort of thing that the World Bank has been doing for 50 years now, is now adequate. It no doubt helps, or perhaps there is doubt that it always helps, but the main thing is that the feeling has grown that it will take something more than simply providing funds to allow or to find a route, a way in which poor countries can raise their standards of living, so there is some of that as well.  Now, the question that I think is still a little dodgy is, to what extent deep down most Europeans and Americans think that success in a poor country means becoming more like us or whether there is some other route to development which does not involve our sort of consumer oriented production, oriented economy. I don’t think that there is any general answer to that question, I think you find out by trying and seeing what happens and seeing whether other countries, currently poor countries, like India, like China, like Indonesia, can find what will be development from their point of view in the sense that those people will feel better off, but that will not simply amount to being like Americans or Germans or Swedes. One can think about it. If I wanted to write about that I might be just inclined to write a novel, except that I know I’m incapable of writing a novel, but that’s the sort of thing that you find out experimentally, I guess.  It’s very interesting. I just want to briefly touch on this: South Africa, who has an enormous challenge ahead of spreading its wealth and really making people, who are discluded of any kind of decision taking for economic wealth to kind of, spread their risk. They’re dealing a lot with Asia, for example, in their trade and it has certainly been of some great benefit for the macro economic situation in the country, so they are also looking into different ways, not just the traditional.  Professor Robert Solow: Yes, I didn’t know that about South Africa, of course I know what the problems in South Africa have been, but I think that the idea of diversifying both their economy and their trading partners, that sounds very interesting and I think it’s likely to succeed.  Mmm … and I think that scares certain more established economies like the United States and Europe as they see how South Africa are diversing.  Professor Robert Solow: Oh, I hope it doesn’t, because the possible benefits are very great and any possible costs to the rich countries are bound to be small compared with the amount of wealth that we now have. |
| Q26 | We just want to go back a little bit to that day in 1987 … I don’t know if you got a phone call or how you got to know that you were to receive the prize, but I very much liked your speech at the banquet, when you said “In the last seven weeks I’ve been asked to solve X amount of countries economical problems” and you said “Of course I know how to answer that, but I’m not going to tell you now”. How did you feel and what has it come with to get this honour or this prize? |
|  | Well, of course, you know, it’s a very great satisfaction, and the satisfaction is that it suggests that you’re admired by the very people whose admiration you care most about, namely other people in your profession. So, I mean, there’s no doubt that it’s a great boost for the ego, there’s no way out of that. There is however that the remark I made on that staircase in Stockholm had the ring of truth to it. There is a feeling that if you have won the Nobel Prize in Economics, you must know the answer to every question and that persists. It’s now 1987, 2004, that’s 17 years ago, but when I was meeting, here in Lindau, with a roomful of graduate students, they started off by asking questions that I thought I might have some chance of answering, but every so often, someone asked me a question of such cosmic significance that I wouldn’t know where to start answering it. And I suppose they still had the feeling, you know, here is someone who has a Nobel Prize in Economics, good Lord, he must be able to answer any question we ask.  That can be a little embarrassing at times, but it’s probably good for one’s psychological health every so often to have to say “I don’t know” or “I haven’t the slightest idea” or “I wouldn’t even know how to begin thinking about that question”. There is, you know, just an enormous respect for the Nobel Prize which means it must have been conducted in a very intelligent way so that one still has the feeling that … I have to be very careful about what I say because anyone who hears what I say may be inclined to believe that it’s true and one doesn’t want to mislead anyone, so there is that, there’s no question about it, yes. |
| ID | 0872 |
| Biographical | |  |  | | --- | --- | | Born: | October 3, 1919, Murfreesboro, Tennessee, USA | | Civil Status: | Married (to Ann Bakke Buchanan on 5 October 1945), no children | |  |  | | Education | | | 1948 | Ph. D., University of Chicago | | 1941 | M. S., University of Tennessee | | 1940 | B. A., Middle Tennessee State College | |  |  | | Current position and title | | | General Director, Center for Study of Public Choice, and Harris University | | | Professor, George Mason University, Fairfax, Virginia, USA | | |  | | | Past positions | | | 1969 – 1983 | Virginia Polytechnic Institute | | 1968 – 1969 | University of California, Los Angeles | | 1956 – 1968 | University of Virginia | |  |  | | Honors and awards | | | 1984 | Frank Seidman Distinguished Award in Political Economy | | 1984 | Doctor, h.c., University of Zurich | | 1983 | Distinguished Fellow, American Economic Association | | 1982 | Doctor, h.c., University of Giessen | |  |  | | Principal publications (books only) | | | Liberty, Market and State, Wheatsheaf, 1985\* | | | The Reason of Rules (with G. Brennan), Cambridge University Press, 1985\* | | | The Power to Tax (with G. Brennan) Cambridge University Press, 1980)\* | | | What Should Economists Do?, Liberty Press, 1979 | | | Freedom in Constitutional Contract, Texas A & M University Press, 1978\* | | | Democracy in Deficit (with R. Wagner), Academic Press, 1977\* | | | The Limits of Liberty, University of Chicago Press, 1975\* | | | Cost and Choice, Markham Press, 1969 | | | Demand and Supply of Public Goods, Rand McNally, 1968 | | | Public Finance in Democratic Process, University of North Carolina Press, 1967 | | | The Calculus of Consent, with G. Tullock; University of Michigan Press, 1962\* | | | Fiscal Theory and Political Economy, University of North Carolina Press, 1960\* | | | Public Principles of Public Debt, Richard D. Irwin, 1958 | |   \*Books most directly relevant to Nobel Award. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0872 |
| Interview |  |
|  |  |
| ID | 0873 |
| Biographical | I was born in Rome, Italy, the son of Enrico Modigliani and Olga Flaschel. My father was a leading pediatrician in the city and my mother was a volunteer social worker.  My school performance in the early years was good though not outstanding. Then, in 1932, a major trauma occurred. My father died as a consequence of an operation. I suddenly realized how deeply I loved and admired him and at 13 my whole world seemed to collapse. After this event my school performance for the next 3 years became spotty until I moved to Liceo Visconti, the best high school in Rome, and the challenge proved healthy and I seemed to blossom. Encouraged, I decided to skip the last year of the Liceo, passed the required difficult exams and entered the University of Rome at 17 (two years ahead of the norm).  My family hoped that I would follow in my father’s steps, entering a career in medicine. I was torn for a while, but finally decided against it because of my low tolerance level for sufferings and blood. Instead I chose law which in Italy, opens the way to many career possibilities. In my second year I decided to enter a national competition sponsored by the student organization (I Littoriali della Cultura) in the area of economics. To my surprise I won first prize and, although now I would hesitate to recommend that first essay as a significant contribution to economics, clearly, it served the purpose of establishing my current interest in economics. Unfortunately, under fascism, teaching in this field was dismal, and only with the advice of the few good economists I knew personally, and especially of Riccardo Bachi, I began on my own to read the English and Italian classics.  The Littoriali had put me in contact with young antifascists, and my political opposition to the regime began then. My involvement with my future wife, Serena Calabi, and her remarkable father, Giulio, who was a long standing antifascist also contributed. In 1938 the Italian racial laws were promulgated and at the invitation of my future in laws, I joined them in Paris where, in May 1939, Serena and I were married. I enrolled at the Sorbonne but found the teaching there uninspiring and a waste of time, so I spent my time studying on my own and writing my thesis at the Bibliotheque St. Genevieve. In June 1939 I returned briefly to Rome to discuss my thesis and receive my degree of Doctor Juris from the University of Rome. Shortly after this, fearing that Europe was going to be soon engulfed in a bloody war, we applied for an immigration visa for the U.S. and arrived in New York in August 1939, a few days before the beginning of World War II.  It became apparent that our stay in the U.S. would be a long one and I immediately began thinking on how best to pursue my interest in economics. I had the great luck of being awarded a free tuition fellowship by the Graduate Faculty of Political and Social Science of the New School for Social Research, an institution freshly created to give haven to the European scholars who were victims of the three fascist dictatorships. Thus in fall 1939, I started on a routine that was to last three years, of studying at night from 6 – 10, while working during the day selling European books to support my family which soon included our first son: Andre. I worked hard but, nonetheless, remember that period as an exciting one, as I was discovering my passion for economics, thanks also to excellent teachers, including Adolph Lowe and above all Jacob Marschak to whom I owe a debt of gratitude beyond words. He helped me develop solid foundations in economics and econometrics, some mathematical foundations, introduced me to the great issues of the day and gave me, together with his unforgettable kindness, constant encouragement. In particular I owe to him that blend of theory and empirical analysis, theories that can be tested and empirical work guided by theory – that has characterized a good deal of my later work. Marschak also provided me with an experience that contributed to my development, by inviting me to participate in an informal seminar which met in New York around 1940-41, whose members included, among others, Abraham Wald, [Tjalling Koopmans](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1975/index.html) and Oscar Lange.  I consider that my formal training ended in 1941 when Marschak left the New School to join the University of Chicago, and I obtained my first teaching job as an instructor at New Jersey College for Women. My first published article in English, “Liquidity Preference and the Theory of Interest and Money”, Econometrica, Vol. 12, No. 1, January 1944, which is also, substantially, my doctoral dissertation, and which I regard as one of my major contributions, appeared some two years later. The result of discussions in Marschak’s seminar and of a running debate with Abba Lerner, it purports to integrate the Keynesian “revolution”, then generally regarded as a total break with the past, with the mainstream of classical economics.  In 1942 I became an instructor in economics and statistics at Bard College, then a residential college of Columbia University, and came to appreciate the unique qualities of life in an American college campus, especially the intimate association with first rate students. In 1944 I returned to the New School as a Lecturer and a Research Associate at the Institute of World Affairs where together with Hans Neisser, I was responsible for a project whose results were eventually published in National Income and International Trade. During this period I also made my first contribution to the study of saving, which has since come to be known as the Duesenberry-Modigliani hypothesis.  In fall 1948 I left New York, having been awarded the prestigious Political Economy Fellowship of the University of Chicago as well as offered the opportunity of joining, as a Research Consultant, the Cowles Commission for Research in Economics, then the leading institution in its field. Shortly after my arrival I accepted an attractive position at the University of Illinois as director of a research project on “Expectations and Business Fluctuations”. However, I remained in Chicago through the academic year 1949-50, greatly benefiting from my association with the Cowles Commission, staffed and visited by people like Marschak, Koopmans, [Arrow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1972/index.html), [Simon](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1978/index.html), at a time when the profession was absorbing two important revolutions, one centering on the theory of choice under uncertainty, initiated by von Neuman and Morgenstern, and the other on statistical inference from non-experimental observations, inspired by [Haavelmo](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1989/index.html).  My association with the University of Illinois lasted only till 1952 because of internal strife. During that brief time, I befriended a brilliant young graduate student, Richard Brumberg. With his collaboration we laid the foundations for what was to become the “Life Cycle Hypothesis of Saving”. It was elaborated in 1953 and 1954 in two papers, one dealing with individual behaviour and the other with aggregate saving. After we had both left the University of Illinois, Brumberg had gone to complete his Ph.D. at the Johns Hopkins University and I joined Carnegie Institute of Technology, now Carnegie-Mellon University. The “aggregate” paper was only published in 1980 in my Collected Papers because the shock of Brumberg’s untimely death in 1955 sapped my will to undertake the revisions and condensation that would have been required for publication in one of the standard professional journals.  My association with Carnegie, which lasted until 1960, was a very productive one. In addition to completing the two basic papers setting the foundations for the “Life Cycle Hypothesis”, I collaborated on a book dealing with the problem of optimal production smoothing, and wrote the two essays with Miller on the effect of financial structure and dividend policy on the market value of a firm. I also published a paper with E. Grunberg on the predictability of social events when the agent reacts to prediction, which later was to provide one of the pillars for the “theory of rational expectations”. All of these contributions represented, to some extent, the coming to fruition of seeds started during my research on “Expectations and Business Fluctuations”.  In 1960 I was a visiting professor at the Massachusetts Institute of Technology, to which I returned after a year at Northwestern University, and where I have remained ever since. Supported by this unique institution and its unique colleagues, I have pursued the interests developed earlier in macroeconomics, including criticism of the monetarist positions, generalizations of the monetary mechanism and empirical tests of the” Life Cycle Hypothesis”. I have also branched out into new areas and, in particular, international finance and the international payment system, the effects of and cures for inflation, stabilization policies in extensively indexed open economies, and into various fields of finance such as credit rationing, the term structure of interest rates and the valuation of speculative assets.  In the late sixties I also had a major responsibility for designing a large scale model of the U.S. economy, the MPS, sponsored by the Federal Reserve Bank and still utilized by it. Finally, I have participated actively in the debate over economic policies both in Italy and the U.S., concentrating lately on the deleterious effects of the huge public deficits.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1985, Editor Wilhelm Odelberg, [Nobel Foundation], Stockholm, 1986  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.  Franco Modigliani died on September 25, 2003. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0873 |
| Interview |  |
|  |  |
| ID | 0874 |
| Biographical | I was born in London on 30 August 1913, the only child of Gilbert and Elsie Stone. My school days were spent first at Cliveden Place Preparatory School and then at Westminster School, which I attended from 1926 to 1930. At Westminster, I was on the classical side: my father, who was a barrister, destined me for the law, and for this, a classical education was deemed indispensable. As a result, I learnt little mathematics beyond elementary arithmetic, algebra and geometry and was rather bored. I expect I could have had a more interesting education if I had shown more interest in what I was taught, but as a boy, my passion was model-building; not mathematical models but models of trains and boats, an activity in which my father was a skilled and enthusiastic collaborator.  In 1930, my father was appointed a High Court judge in Madras. When he was about to leave for India, he consulted the school about what was to be done with me. I think it would be a very good thing if he were to accompany you, said the headmaster, he doesn’t seem to be doing much good here. So I had a year’s break in India between school and university.  From 1931 to 1935, I was an undergraduate at Cambridge in my father’s old college, Gonville and Caius, which was particularly strong in medicine and the law. However, after two years of law I switched to economics, much to my father’s disappointment. At that time the world was in the depth of the great depression and my motive for wanting to change subject was the belief, bred of youthful ignorance and optimism, that if only economics were better understood, the world would be a better place.  My college did not have an economist among its Fellows, and so, for my weekly supervisions, I was sent to Richard Kahn at King’s College. This was a piece of great good fortune, as Kahn was not only a brilliant theorist but also a stimulating and encouraging supervisor. Another of my teachers to whom I owe much was Colin Clark, who was lecturer in statistics and who became a close friend. Finally, there was Keynes, who was in the habit of giving a short course of lectures on whatever book he happened to be writing; at that time, the book on the stocks was The General Theory. I was invited to become a member of his Political Economy Club which met in his rooms at King’s. He was kind to me as he was to all young people, but it was only later that I got to know him well.  Unlike my school performance, my undergraduate career had been uniformly successful, and after I had taken my degree in 1935, my college offered me a research studentship. But while I was much tempted by this opportunity, I had done only two years of economics and was not quite sure that I was ready for research. Furthermore, my father was anxious to see me settled in a job and so I did not take up my studentship but joined the staff of a firm of Lloyds brokers in the City. I was never cut out for a business career but I did learn a good deal about life from my brief encounter with the insurance world.  My job was not so heavy that I could not carry on with the kind of work that interested me. In 1936, I married Winifred Mary Jenkins, who had also read economics at Cambridge, and we spent much of our spare time writing on economic subjects. In particular, we were responsible for a little monthly called Trends, which appeared as a supplement to the periodical, Industry Illustrated. Colin Clark had been running it and bequeathed the task to me when he went to Australia in 1937. Following in his footsteps, we filled it every month with indicators of British economic conditions: employment, output, consumption, retail trade, investment, foreign trade, prices and so on. From time to time we would add a special article on regional employment, say, or the economic recovery of Germany, or the American stock market; in short, on any subject that seemed to us topical.  Trends was small and modest, nevertheless, it must have attracted some attention as, in 1939, I was asked whether I would be prepared to join the staff of the Ministry of Economic Warfare which was to be set up in the event of war. I accepted, and when, on 2 September, war did break out, I reported for duty.  I remained in the Ministry about nine months, in the section responsible for shipping and oil statistics. Then, in the summer of 1940, I was transferred to the Central Economic Information Service of the Offices of the War Cabinet, where [James Meade](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1977/index.html) was preparing the groundwork for a survey of the country’s economic and financial situation and wanted somebody to help with the statistical side. By December 1940, Meade and I had completed a set of estimates which we showed to Keynes, who was then a member of the Chancellor’s Consultative Council at the Treasury, and through his advocacy they were published as the second part of a White Paper entitled, An Analysis of the Sources of War Finance and an Estimate of the National Income and Expenditure in 1938 and 1940 which accompanied the budget of 1941. Our estimates consisted of three tables relating to the national income and expenditure, personal income, expenditure and saving, and the net amount of funds required by, and available from, private sources for government purposes. They hardly amounted to a set of national accounts but they were a beginning. In constructing the accounts, we made use of residual estimation. The balancing of the accounts, therefore, threw little light on the accuracy of the entries. But the sources for the first two tables were largely independent of those for the third, and the fact that for 1940 the sum of the first two residuals was not very different from the third, encouraged us to think that the results were not grossly inaccurate.  The Chancellor in his budget speech emphasised that the publication of official estimates of national income and expenditure should not be regarded as setting a precedent. In fact, they established themselves as an annual feature and have appeared in increasingly elaborate form ever since. At the instigation of Keynes, whose assistant I had become, I continued to be responsible for them until I left the government service at the end of the war.  The United States and Canada had also for some time been making estimates of national income and national expenditure, more detailed than ours though not cast in the form of balancing accounts, and while the three countries used similar concepts and definitions, it was clear that some adjustment would be needed to obtain reasonably comparable tables. So, in 1944, I was sent over to see how far agreement could be reached. I met my Canadian opposite number, George Luxton, in Ottawa and we travelled down to Washington for discussions with Milton Gilbert and his team at the Department of Commerce. The meetings were very friendly and the results extremely satisfactory, so that my first taste of international cooperation could not have been more encouraging.  In 1940 my marriage had been dissolved, and in 1941, I had married Feodora Leontinoff. From a background in philosophy, she had become, in 1939, the Secretary of the National Institute of Economic and Social Research which had been founded the year before. At the outbreak of war, the director and his staff had been absorbed into the Ministry of Economic Warfare, and Feodora’s initial function was simply that of caretaker. The survival of the Institute looked very uncertain, but thanks to the drive of Henry Clay and Geoffrey Crowther, and to Feodora’s energy and talent for administration, it came to life again.  In 1945, the war ended and I was chosen to be the first director of the newly-established Department of Applied Economics in Cambridge. Between leaving the government service and taking up my new post, I had a break of about three months which I spent at the Institute of Advanced Study in Princeton. I intended to use my time there writing up my ideas on a social accounting system for the measurement of economic flows, a thing I had wanted to do for years but had not had time for during the war. What happened was that, in Princeton, I met Alexander Loveday, the Director of Intelligence at the League of Nations, who wanted a paper on the problems of defining and measuring the national income and related totals for consideration by the League’s Committee of Statistical Experts. He asked me if I would undertake the work and naturally I accepted. I soon had a memorandum ready and it was discussed in Princeton while I was still there by a subcommittee convened by Loveday. Their report was eventually published by the United Nations in Geneva in 1947 under the title, Measurement of National Income and the Construction of Social Accounts, with my memorandum as an appendix.  In Europe, interest in social accounting had been growing, and I had, around that time, many fruitful exchanges with my European colleagues. The catalyst, again, was an international body. In the late 1940’s the Organisation for European Economic Cooperation was established in Paris with the initial aim of administering American aid under the Marshall Plan. It was decided, at the instigation I think of Richard Ruggles, that the national accounts would provide a useful framework for reviewing the progress of the member countries, and with this in mind, a National Accounts Research Unit was set up in Cambridge under my direction. The brief my European colleagues and I were given was, first, to produce a standard system of accounts; second, to prepare studies of the national accounts of individual countries; and, third, to train other statisticians from member countries in the appropriate techniques. It was a lively group, which included visitors from Austria, Denmark, France, Greece, the Netherlands, Norway, Sweden and Switzerland. Several reports resulted from our activity, among them, A Simplified System of National Accounts and A Standardised System of National Accounts, published by the OEEC in 1950 and 1952, respectively. The research unit lasted from 1949 to 1951, when its work was taken over by the economics and statistics section of the organisation in Paris, then directed by Milton Gilbert.  Concurrently with this work, my main research interest at the Department of Applied Economics was the analysis of consumers’ behaviour. I had made a start on this during the war at the National Institute of Economic and Social Research, as part of a large project I had in mind, for estimating the British national accounts for the interwar period, so that we should have series going back over the 1920s and 1930s comparable as far as possible with the official estimates that had been started in 1941. My first paper on the subject, The Analysis of Market Demand, was read to the Royal Statistical Society and published in its journal in 1945. After moving to Cambridge, I continued my work with the help of Deryck Rowe of the National Institute, and, eventually, two large volumes appeared, the first in 1954, and the second in 1967, under the title, The Measurement of Consumers’ Expenditure and Behaviour in the United Kingdom, 1920-1938. At the time of the publication of the first volume, I wrote a paper, applying to British data, a system of demand equations which I termed the linear expenditure system, in which the price of each commodity appeared along with income in each of the equations. The model had been devised by [Lawrence Klein](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1980/index.html) and Herman Rubin as a basis for constructing a constant-utility index of the cost of living. It is now superseded, but it had a good innings and has been used all over the world.  During the early 1950s, I made a number of trips abroad in connection with the national accounts. In 1950, I visited India with [Simon Kuznets](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1971/index.html) and J.B.D. Derksen to advise the National Income Committee on methods of estimation, and in 1952 I spent some time in Athens on a similar mission to the Ministry of Coordination.  In July of that same year, I was called to New York by the UN Statistical Office who wished to establish a standard system of national accounts and was convening a committee of experts for the purpose. I was chosen as chairman and work began. The weather was so hot that we decided to sleep by day and work by night. This proved very effective: our report was formulated, discussed and written in one month and was published by the UN with very little delay as A System of National Accounts and Supporting Tables (SNA).  In 1952, not many statisticians were familiar with national accounting and so there was no need for elaborate discussions outside the committee. The position was very different twelve years later, when the major revision of the SNA began. By that time most statistical offices were constructing national accounts and it was desirable to have a series of regional consultations if the new system was to prove acceptable. The consultative period lasted from 1964 to 1968 and the main task of explaining the revised version to committee after committee devolved on my friend Abraham Aidenoff of the UN Statistical Office. The new system appeared in 1968 as A System of National Accounts. I was responsible for writing the first four chapters and the remainder was the work of Aidenoff.  In 1955 I gave up the directorship of the Department of Applied Economics on being appointed P.D. Leake Professor of Finance and Accounting in the University. My duties in this capacity were to advance knowledge in my subject and live within five miles of the university church, two commitments which suited me very well.  Towards the end of the 1950s, stimulated by Alan Brown who had been working with me at the Department since 1952, I thought it would be a good idea to bring together various studies that were in progress at the Department and build an econometric model of the British economy. This was the start of the Cambridge Growth Project. In 1962, Alan and I published our ideas in A Computable Model of Economic Growth, the opening volume in our series, A Programme for Growth. The beginnings were comparatively modest, though the principal characteristics of the model were present from the outset: it was a disaggregated model in which several branches of production, types of commodity, consumers’ goods and services and government purposes were distinguished, and it was based on a social accounting matrix. At first, it was a static model which provided projections for a period about five years ahead, without considering the path that would be followed in reaching the projected situation. Now it is one of the largest existing models of a national economy, and under the influence of T.S. Barker, who succeeded me as director of the project, it has assumed a dynamic form: given an initial state of the economy and future values of the exogenous variables such as tax rates and the level of world trade which we do not try to model, we can solve the several thousand equations of the system iteratively year by year so as to trace the course of each of the endogenous variables into the future. The model can also be used for purposes other than forecasting. Just for the record, I should add that the team engaged on the project, though changing in composition through the years, has never numbered more than ten people.  In 1956, my wife Feodora had died after a long illness. In 1960, I married Giovanna Croft-Murray (née Saffi) who, though not formally trained as an economist, has been for the last twenty-five years, my partner in all my work. We wrote two books together, Social Accounting and Economic Models (1959) and National Income and Expenditure (1961). The latter was an expanded fifth edition of a little book Meade and I had written in 1944; it went into five more editions, the last one appearing in 1977. Giovanna played a large part in editing the twelve volumes of A Programme for Growth which described the Cambridge Growth Model up to 1974, and threw herself with particular enthusiasm into the work on social demography and demographic accounting which I began in 1965.  I started this work with the idea of introducing education and training into the Growth Model. This never came to anything, but I continued to work on education and eventually was asked by the Organization for Economic Cooperation and Development to prepare a report on the subject for their Committee for Scientific and Technical Personnel. In this I explained what demographic accounting is, what kind of information is needed to carry it out and how it can be used as a basis for model-building. The report was illustrated by examples drawn from the British educational system and was published by the OECD in 1971 under the title Demographic Accounting and Model-Building. In 1970, the UN Statistical Office became interested in developing an integrated system of social and demographic statistics and called me in as a consultant. After preparing several drafts for the usual round of discussions, I finally wrote the report which was published by the UN in 1975 under the title, Towards a System of Social and Demographic Statistics (SSDS).  As with the revised SNA, the interpreter of the SSDS throughout the world during the period of gestation was Aidenoff. My long collaboration with him, like my collaboration with Milton Gilbert at the OEEC and with Alan Brown on the Cambridge Growth Project, was one of the many happy working relationships of my life.  In the last ten years, my interest has focused on three subjects. I have continued my work on social demography. I have tried out on the British national accounts the adjustment method on which I had written a paper in 1942 with David Champernowne and James Meade entitled, The Precision of National Income Estimates. And I have given some thought to mathematical simulation models of economic growth and fluctuation, their stability and their control.  In 1980, I retired from my university post. My retirement, however, has not severed my links with the two colleges with which I have been associated throughout my life in Cambridge: King’s College, where I have held a Fellowship since 1945, and Gonville and Caius College, where I spent my undergraduate days and where I have been an Honorary Fellow since 1976. Nor has it altered my habits much except in so far as it has enabled me to work full time where I have always preferred to work at home. Recently a period of ill health has slowed me down, but now things are improving and I have started to pick up the threads again. I look forward to a productive 1985.  From [Les Prix Nobel](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). The Nobel Prizes 1984, Editor Wilhelm Odelberg, [Nobel Foundation], Stockholm, 1985  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.  Richard Stone died on December 6, 1991. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0874 |
| Interview |  |
|  |  |
| ID | 0875 |
| Biographical | I was born in 1921 in Calais, France, the son of Camille Debreu and Fernande (née Decharne) Debreu. My father was the business partner of my maternal grandfather in lace manufacturing, a traditional industry in Calais. My paternal grandfather managed, until his retirement, the printing plant he had created in the small town of Marquise between Calais and Boulogne.  All my schooling to the Baccalauréat in 1939 took place in the College of the City of Calais. In the summer of 1939, the Second World War started for France, and instead of preparing for the entrance examination to one of the scientific grandes écoles in a lycée in Paris, I studied in an improvised Mathématiques Spéciales Préparatoires curriculum in Ambert (Puy-de-Dôme) during the academic year 1939-40. In the summer of 1940, France was divided into several zones by the German occupation forces, and I went from Ambert to Grenoble, both being in the so-called “Free Zone”. The academic year 1940-41 was spent at the Grenoble Lycée where I took the Mathématiques Spéciales curriculum.  In the summer of 1941, I was admitted to the École Normale Supérieure where I studied and lived until the spring of 1944. Those three years were an extraordinary experience in many ways. The small size of each entering class (about twenty in the Sciences, and thirty in the Humanities at that time) and the strict admission procedures helped to create a superheated intellectual atmosphere. The dark outside world of Paris under German occupation also exerted a strong containing pressure on the microcosm in the rue d’Ulm. Of all the teachers I had during that period, Henri Cartan was the most influential. Indirectly, N. Bourbaki also fashioned my mathematical taste.  I was supposed to take the Agrégation de Mathématiques in the spring of 1944 and thereby, to end my formal studies. But D-day intervened; I enlisted in the French Army, was sent to officer school in Cherchell, Algeria, and then served briefly in the French occupation forces in Germany until the end of July, 1945. Eventually, I took the Agrégation de Mathématiques at the end of 1945 and at the beginning of 1946. In the meantime, I had become interested in economics, an interest that was transformed into a lifetime dedication when I met with the mathematical theory of general economic equilibrium, founded by Léon Walras in 1874-77, in the formulation given by [Maurice Allais](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1988/index.html) in his book, *A la Recherche d’une Discipline Économique,* 1943. The two and a half years following the Agrégation were devoted to my conversion from mathematics to economics. During that period, I was an Attaché de Recherches of the Centre National de la Recherche Scientifique which showed an impressive tolerance for the absence of tangible results associated with the change from one field to another distant field.  In the summer of 1948, I attended for several weeks the Salzburg Seminar in American Studies where [Wassily Leontief](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1973/index.html) was a member of the faculty. As the year 1948 came to a close, I obtained a Rockefeller Fellowship that permitted me in 1949 to visit Harvard University, the University of California at Berkeley, the University of Chicago, and Columbia University, and during the first four months of 1950, to visit the University of Uppsala and the University of Oslo. My stay in Salzburg and my Rockefeller Fellowship brought me up to date with all the scientific developments in economics from which France had been cut off. Even more importantly, at the time of my visit to the University of Chicago in the fall of 1949, the Cowles Commission for Research in Economics offered me a position as a Research Associate. The Cowles Commission was the optimal environment for the type of research that I wanted to do, and I accepted its offer, starting an eleven-year association on June 1, 1950. In June, 1945, I had married Françoise Bled, and our two daughters, Chantal and Florence, were born in August, 1946, and in February, 1950.  The Cowles Commission in the early fifties proved itself to be more than I had hoped for. It seemed to be a focal point for mathematical economics where every recent development was discussed. The small research staff interacted in weekly meetings, in biweekly seminars, and in numerous conversations. In that exceptionally supportive environment, in which almost all my time was devoted to research, my work on Pareto optima, on the existence of a general economic equilibrium, and on utility theory made quick progress. During the last years of the Chicago period of the Cowles Commission, I took a six-month leave at Électricité de France in Paris in the summer and the fall of 1953. The theoretical article on contingent commodities that [Arrow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1972/index.html) published in that year and the applied problems created for Électricité de France by the uncertain amounts of water in hydroelectric plant reservoirs led me to the study of economic uncertainty that was eventually published as the last chapter of my monograph, *Theory of Value*, 1959.  In the summer of 1955, the Cowles Commission moved from the University of Chicago to Yale University, and in that new environment, I completed an article on market equilibrium and my monograph whose purpose was an axiomatic analysis of the theory of general economic equilibrium. I also studied several problems in the theory of cardinal utility, notably, the additive decomposition of a utility function defined on a Cartesian product of sets.  The year 1960-61 was spent at the Center for Advanced Study in the Behavioral Sciences at Stanford and devoted mostly to the complex proof that appeared in 1962 of a general theorem on the existence of an economic equilibrium. During that visit, I accepted an appointment at the University of California at Berkeley to begin on January 1, 1962. In the fall of 1961, however, I was back at the Cowles Foundation at Yale University, this time as a visitor. In that semester I started work on the core of an economy continuing that of Herbert Scarf, then at Stanford. That work later gave rise to the joint paper that we published in 1963. In the mid-sixties in Berkeley, the theory of measure spaces of economic agents that originated with a paper of Robert J. Aumann, published in 1964, became one of my main interests, as did the related problem of topologizing the set of preference relations.  In the fall of 1968, I had the first of several long leaves which took me to CORE at the University of Louvain (1968 – 69; fall, 1971; and winter, 1972), to Churchill College, Cambridge, England (spring, 1972), to the Cowles Foundation at Yale University (fall, 1976), to the University of Bonn (winter and spring, 1977), and to the CEPREMAP in Paris (fall, 1980). In the summer of 1968, I had become interested in the question of regular economies which remained unanswered until I visited the University of Canterbury in Christchurch (New Zealand) in June-July, 1969. My later research interests in Berkeley in the seventies and in the early eighties centered mainly on the study of differentiable utility functions, on the characterization of the excess demand function of an economy, on the rate of convergence of the core of an economy to its set of competitive equilibria, on the problem of least concave utility functions, and (in collaboration with [Tjalling C. Koopmans](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1975/index.html)) on the question of additively decomposed quasi-convex functions.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1983*, Editor Wilhelm Odelberg, [Nobel Foundation], Stockholm, 1984  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.  *Gerard Debreu died on December 31, 2004.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0875 |
| Interview |  |
|  |  |
| ID | 0876 |
| Biographical | I was born in Renton, a suburb of Seattle, Washington, in 1911. I was the only child of Joseph and Elizabeth Stigler, who had separately migrated to the United States at the end of the 19th century, my father from Bavaria and my mother from what was then Austria-Hungary (and her mother was in fact Hungarian). I attended schools in Seattle through the University of Washington, from which I was graduated in 1931. I spent the next year at Northwestern University.  My main graduate training was received at the University of Chicago from which I received the Ph.D. in 1938. The University of Chicago then had three economists – each remarkable in his own way – under whose influence I came. Frank H. Knight was a powerful, sceptical philosopher, at that time vigorously debating Austrian capital theory but gradually losing interest in the details of economic theory1. Jacob Viner was the logical disciplinarian, and equally the omniscient student of the history of economics. Henry Simons was the passionate spokesman for a rational, decentralized organization of the economy. I was equally influenced by two fellow students, W. Allen Wallis and [Milton Friedman](https://www.nobelprize.org/prizes/economic-sciences/1976/friedman/facts/).  The Chicago Economics Department was in intellectual ferment, although the central issues of the 1930’s were very different from those in later times. I had never before encountered minds of that quality at close quarters and they influenced me strongly. For example, Knight supervised my thesis, which was on the history of production and distribution theories from 1870 to 19152. He had a wonderfully critical mind, which, however, was not well suited to intellectual history because he could not understand, let alone excuse, the errors of earlier economists. It was perhaps a decade later before I could read Ricardo through my eyes rather than through Knight’s eyes.  My teaching began in 1936 at Iowa State College where [T. W. Schultz](https://www.nobelprize.org/prizes/economic-sciences/1979/schultz/facts/) was the department chairman. Two years later, I went to the University of Minnesota from which I was on leave for several years during the war as a member of Statistical Research Group at Columbia University. After the war, I returned to Minnesota, from which I soon moved to Brown University, and a year later, to Columbia University where I remained from 1947 until 1958. The last year, I was on leave at the Center for Advanced Study in the Behavioral Sciences, sharing a splendid year with [Kenneth Arrow](https://www.nobelprize.org/prizes/economic-sciences/1972/arrow/facts/), Milton Friedman, Melvin Reder, and [Robert Solow](https://www.nobelprize.org/prizes/economic-sciences/1987/solow/facts/). In 1958, I came to Chicago where I have remained.  In retrospect, I, no doubt, purposely avoided administrative duties, and indeed, almost all non-academic entanglements. I recall my mother asking in about 1946 what I was and I replied proudly that I was a professor. A decade later she repeated her question and I repeated my answer. “No promotion?” was her comment.  Early in my professional life, I found that many areas of economics attracted me. I started working and publishing in price theory by 1938. In 1946, I published an early work on linear programming (*The Cost of Subsistence*) which solved the problem only approximately; George Dantzig soon presented the exact solution. In the 1940s, I began empirical work on price theory, starting with a test of the kinked oligopoly demand curve theory of rigid prices. In the 1950s, I proposed the survivor method of determining the efficient sizes of enterprises, and worked on delivered price systems, vertical integration, and similar topics.  In this same period, I was on the staff of the National Bureau of Economic Research. There I worked on the service industries and used what may have been the first total factor productivity measure (in *Trends in Output and Employment*). Later books on scientific personnel (with David Blank), on capital and rates of return in manufacturing, and on the behavior of industrial prices (with James Kindahl) were also done under Bureau sponsorship. It would be remiss to fail to mention the fascinating association I had with that remarkable economist, Arthur F. Burns.  Even before I came to Chicago, I had gotten interested in the existence of dispersion of prices under conditions which economic theory said would yield a single price. That interest culminated in *The Economics of Information* (1961) and later work – indeed I am about to enter into the study of the extension of this analysis to political behavior.  It was in the 1960s that I began the detailed study of public regulation. My interests were aroused, and my faith in the cliches of the subject destroyed, as so often with other subjects, by the discussions with my friend, Aaron Director. This wonderful man is that rarest of scholars: a clear-headed, imaginative, erudite man who enjoys the task of constructing luminous and original theories but does not even write them down!  Throughout the last 40 years, I have maintained a continued interest in the history of economics (as an aside, I am a diligent book collector; my oldest son is equally active in the history of statistics, and my oldest grandson has an immense collection of comic books – leading some friends to suggest a new gene!). That subject has lost its one time appeal to economists as our science has become more abstract, but my interest has even grown more intense as the questions raised by the sociology of science became more prominent.  I met my wife, Margaret L. Mack, at the University of Chicago. We were married in 1936. She died in 1970. I have three sons, Stephen (a statistician), David (a lawyer), and Joseph (a social worker). We are a close-knit family, and each summer we gather at a cottage on the Muskoka Lakes in Canada.  1. See my appreciation “In Memoriam: Frank Knight as Teacher,” *Journal of Political Economy*, June 1973.  2. Published as *Production and Distribution Theories* (Macmillan, 1941). A complete bibliography of my work is given in *The Economist as Preacher*, University of Chicago Press, 1982  \* I wish to thank [Gary Becker](https://www.nobelprize.org/prizes/economic-sciences/1992/becker/facts/), Aaron Director, Milton Friedman, and Stephen Stigler for helpful comments.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1982*, Editor Wilhelm Odelberg, [Nobel Foundation], Stockholm, 1983  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.  *George J. Stigler died on December 1, 1991* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0876 |
| Interview |  |
|  |  |
| ID | 0877 |
| Biographical | I studied economics and made it my career for two reasons. The subject was and is intellectually fascinating and challenging, particularly to someone with taste and talent for theoretical reasoning and quantitative analysis. At the same time it offered the hope, as it still does, that improved understanding could better the lot of mankind. For me, growing up in the 1930s, the two motivations powerfully reinforced each other. The miserable failures of capitalist economies in the Great Depression were root causes of worldwide social and political disasters. The depression also spelled crisis for an economic orthodoxy unable either to explain events or prescribe remedies. The crisis triggered a fertile period of scientific ferment and revolution in economic theory. The excitement reached beginning undergraduate students like myself. In 1936, at the start of my sophomore year, a young tutor at Harvard College, Spencer Pollard, suggested we read together a new book by an English economist, J.M. Keynes, and I was hooked.  My mother and father had paved the way. Margaret Edgerton Tobin, now in her ninetieth year, was a social worker who, after a sixteen-year interruption for marriage and family, resumed her career in the relief emergency of 1932 and directed the family service agency of Champaign-Urbana, Illinois, for the next quarter century. From her first-hand accounts I learned of the human suffering of unemployment and poverty. Louis Michael Tobin (1879-1943), a journalist, was, from my early childhood, the publicity director for University of Illinois athletics. The fortunes of Illinois sports teams were a big thing in our lives, to be sure. My father also happened to be an intellectual, as learned, literate, informed, and curious as anyone I have known. Unobtrusively and casually, he was my wise and gentle teacher. In the home territory of the arch-conservative *Chicago Tribune*, our home was sprinkled with alien periodicals like the *Nation, New Republic,* and Mencken’s *American Mercury.* In our town, and among my mother’s relatives in Wisconsin, my parents, and, in time, I and my young brother too, were known for eccentric but well-argued political views. I cast the only straw vote for Roosevelt in 1932 in a poll of a sophomore high school class mostly composed of university faculty children.  I was born in Champaign in 1918. From the neighborhood elementary and intermediate schools, I went to the University High School in the twin city, Urbana. The school was operated by the university’s College of Education primarily to give its students practical training in teaching. The master teachers who guided the trainees also gave us a marvelous education. The graduates number only 30 to 40 annually, but they win many scholarships in national competition. Two alumni, [Philip Anderson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/index.html) and [Hamilton Smith](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1978/index.html), are Nobel laureates. Ironically, my award this year was announced, coincidentally, with news that the school might be closed for lack of funding.  For me, one good thing about Uni-High was that in a small school, I could earn a place on the varsity basketball team, fulfilling athletic ambitions that had seemed beyond reach in my childhood. Another was that it prepared me exceptionally well for Harvard, even though neither the school nor I ever thought that midwestern teen-agers might go to a prestigious expensive eastern college a thousand miles away. I happily took for granted that I would attend the very good local university and probably go on to its law school. Harvard was my father’s idea. By chance, President James B. Conant of Harvard was just then inaugurating national full-cost scholarships designed to diversify the geographical, scholastic, and social sources of the student body, and he was starting with the midwestern states. All this my father learned because he habitually read the New York Times in the public library. So, I wrote in June three days of entrance exams for which I had neither received nor made any special preparation. I learned the amazing good news in August, and in September 1935, on the train to Boston, I left the midwest for the first time.  Four years later I received my Harvard baccalaureate. My proud parents attended the commencement, their first trip east since their honeymoon in New York in 1916. After the outbreak of war in 1939 washed out the Wanderjahr for which I had been granted a travelling fellowship, I spent the next two years as a graduate student at Harvard. Those six years were a great experience. My fellow students, many of them my lifelong friends, were of diverse backgrounds, interests, and talents. My teachers ranged from Alfred North Whitehead, about to retire, to eager young instructors. I joined the intense political debate and activity that absorbed the campus in those critical pre-war years. In economics, Harvard, the center of the intellectual ferment of the day, was enjoying a golden age. Joseph Schumpeter, Alvin Hansen[1](https://www.nobelprize.org/prizes/economic-sciences/1981/tobin/biographical/#not1), Seymour Harris[2](https://www.nobelprize.org/prizes/economic-sciences/1981/tobin/biographical/#not2), Edward Chamberlin, Edward Mason, Gottfried Haberler, Sumner Slichter, and [Wassily Leontief](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1973/index.html) were the professors who meant most to me. In addition, there was a superb assemblage of young faculty and graduate students, [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/index.html), Lloyd Metzler, Paul Sweezy, J.K. Galbraith, Abram Bergson, Richard Musgrave, Richard Goodwin, Richard Gilbert, Lloyd Reynolds, John P. Miller and others who would be leaders of the profession in later years.  I left Harvard in the spring of 1941. Ed Mason, for whose seminar I had written a paper on the uses of statistical forecasting in economic mobilization, steered me to a job in Washington in a new agency charged with restricting civilian uses, *e.g.*, in autos and other consumer durables, of metals and other materials needed for growing defense production. Aside from Melvin De Chazeau and Arthur R. Burns, we economists were all youngsters, suddenly charged with the practical responsibilities of setting quotas and explaining them to the victims.  After the United States entered the war, I joined the Naval Reserve and spent ninety days in a Columbia University dormitory learning to be a naval officer. Among my friends were, for alphabetical reasons, Cyrus Vance and Herman Wouk. Wouk’s thinly disguised reference to me in The Caine Mutiny was until recently my main source of notoriety. I spent nearly four years as a line officer on the destroyer U.S.S. Kearny, serving eventually as gunnery officer and then navigator and executive officer (second in command). Mostly, our ship engaged in convoy escort and other anti-submarine duty in the Atlantic and Mediterranean, but we also participated in the invasions of North Africa and Southern France and in the Italian campaign. I liked and valued the experience, just because its demands and tests were so different from academics. But I rejoiced with my shipmates when, after escorting occupation troop ships to Japan, we left the ship in Charleston Navy Yard to be put in “mothballs.”  I was tempted by opportunities to return to Washington. But a timely letter from the Harvard Economics Chairman, Harold H. Burbank, persuaded me my future was in academia, and I returned to complete my Ph.D. in 1946-47. I am forever grateful to Professor Burbank, mainly for a reason that will become clear later in this story. I wrote a doctoral dissertation on the theory and statistics of the consumption function, a lasting interest of mine. In 1947 I was elected Junior Fellow of the Society of Fellows, an appointment that allowed me three years of freedom for study, research, and writing. Like my high school, the Society can claim a number of Nobels, four this very year. Harvard’s golden age in economics extended to these postwar years, when several cohorts of able and mature graduate students and junior faculty converged. I used my Junior Fellowships to catch up with the economics, especially the econometrics I had missed during the war, to collaborate on a sociological-economic book, *The American Business Creed*, and to write a number of articles in macro-economics, statistical demand analysis, and the theory of rationing. Some of this took place in 1949-50 in England at [Richard Stone](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1984/index.html)‘s Department of Applied Economics in Cambridge where I benefited especially from fruitful collaboration with Hendrik Houthakker and lively discussions with him and the late Michael Farrell.  I have been at Yale since 1950. It has been a marvelous place for research, teaching, and living. The Economics Department has grown in size and stature, helped mightily by the coming of the Cowles Foundation (previously Commission) in 1955, with its remarkable leaders, [Tjalling Koopmans](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1975/index.html) and Jacob Marschak. Under their direction at Chicago, the Cowles Commission was one of the most productive research centers in history, inaugurating modern econometrics and activity analysis. Its alumni include [Kenneth Arrow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1972/index.html), [Herbert Simon](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1978/index.html), and [Lawrence Klein](https://www.nobelprize.org/nobel_prizes/economics/laureates/1980/index.html). I was director of the Foundation at Yale from 1955 to 1961 and from 1964 to 1965.  At the time, my personal research objectives were to provide Keynesian economics with more rigorous foundations and to tighten and elaborate the logic of macroeconomic and monetary theory. My Nobel lecture is, in a sense, a summary account. Largely because of my interests, the foundation added monetary theory and macroeconomics to its previous lines of inquiry. The logistical support, research assistance, and collegial setting of the Cowles Foundation have been invaluable. Most important, I have learned from my colleagues and students. The two persons to whom I owe the most are the late Arthur Okun[3](https://www.nobelprize.org/prizes/economic-sciences/1981/tobin/biographical/#not3) and William Brainard. I taught with them and collaborated with them; I argued with them, and they were usually right. Others with whom I have worked closely and fruitfully on topics related to my lecture include David Backus, Martin Neil Baily, Willem Buiter, John Ciccolo, Walter Dolde, Harold Guthrie, Challis Hall, Koichi Hamada, Donald Hester, Susan Lepper, Jorge de Macedo, [Harry Markowitz](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1990/index.html), Donald Nichols, William Nordhaus, [Edmund S. Phelps](https://www.nobelprize.org/nobel_prizes/economics/laureates/2006/index.html), James Pierce, Richard C. Porter, Richard Rosett, Gary Smith, Craig Swan, Harold Watts, and Leroy Wehrle. Moreover, the presence on the faculty of Ray Fair, William Fellner, Raymond Goldsmith, Richard Ruggles, Robert Triffin, and Henry Wallich made Yale a stimulating environment for work in macroeconomics, money, and finance. Outside Yale, [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/index.html)[4](https://www.nobelprize.org/prizes/economic-sciences/1981/tobin/biographical/#not4), [Robert Solow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1987/index.html), and [Franco Modigliani](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1985/index.html) at M.I.T. have, to my vast benefit, shared many of my interests and viewpoints. Likewise, I learned much over the years by maintaining friendships and contact with George Katona, James Morgan and others at the University of Michigan Survey Research Center, and with the late Harry Johnson[5](https://www.nobelprize.org/prizes/economic-sciences/1981/tobin/biographical/#not5). Other intellectual debts, including those to giants of the field whose influence on me came only through their writings, are indicated in my lecture.  Yale places great stress on undergraduate and graduate teaching. I like teaching, and I do a lot of it. I never fail to learn, from the students themselves and from the discipline of presenting ideas clearly to them. A large and durable reward is the legion of friends of all ages.  Beginning in the late 1950s I have written occasional articles on current economic issues directed to lay readers, not just to professional economists. A collection of these, *National Economic Policy*, was published in 1966. I have frequently testified before committees of the Congress, and I have advised government agencies and political candidates. From 1966 to 1970 I served as Chairman of the New Haven City Plan Commission.  My principal excursion into public life was as a Member of President Kennedy’s Council of Economic Advisers in 1961-62, together with Walter Heller, Chairman, and the late Kermit Gordon[6](https://www.nobelprize.org/prizes/economic-sciences/1981/tobin/biographical/#not6). After I returned to Yale, I was an active consultant to the Council for several years. The Kennedy Council recruited a remarkable staff, including Okun, Solow, and Arrow. Our collective *magnum opus* is the 1962 *Economic Report*, a full statement of the theory and practice of the policies for stabilization and growth associated with what the press then called the “new economics”. Work at the Council was demanding, exciting, and sometimes frustrating. But our advice gradually gained a large measure of acceptance, and by the end of 1965, our basic macroeconomic goals were achieved. Alas, these victories were lost during the Vietnam war and the stagflation of the 1970s.  The greatest good fortune of my return to Cambridge in 1946 was that there, in the spring, I met Elizabeth Fay Ringo. We were married a few months later. By coincidence, Betty was a recent student of Samuelson’s at M.I.T., teaching economics at Wellesley College at the time we met. By greater coincidence, she had grown up in northern Wisconsin not far from the family retreat where I spent nearly every summer of my life. We go there still. I diverted Betty, she sometimes says rescued her, from economics. But her clarity about sense and nonsense, right and wrong, fair and unfair, poor and rich has kept my priorities straight, in my professional work as in my personal life. In our first thirty-five years I have learned many other things too, in sharing her enthusiasms for animals, especially Newfoundland dogs, baseball, fireplaces, birds, nature, fishing, dancing, and jazz. We are ardent, if mediocre, skiers, alpine and crosscountry, and tennis players. In Wisconsin we like to canoe down rivers and swim and sail on our small lake. In the 1960s, Betty returned to teaching for eight years, this time in inner-city public primary schools, infinitely more demanding and challenging than college classrooms.  Together, we raised four remarkable children, a daughter, the eldest, and three sons, and we shared the fascination, joy, and occasional anxiety of watching babies become, all too quickly, adults. Differing in personalities, interests, and talents, they all have taught us no less than we taught. Our daughter is a costume and fashion designer and a writer; two sons, both married, are lawyers; the youngest is a graduate student of physics. Our first grandchild, a girl, was born in 1981. We still live in the house we bought the first year we lived in New Haven. Our whole family regularly joins us there or in Wisconsin or at our ski chalet in Vermont.  1. J. Tobin, “Hansen and Public Policy,” *Quarterly Journal of Economics*, Vol. XC, Feb. 1976, pp. 32-37.  2. J. Tobin, Tribute at the Memorial Service for Seymour Harris, Harvard University, Dec. 12, 1974, privately printed in Berkeley, California, 1976.  3. In Memoriam, Arthur M. Okun, A Tribute by James Tobin, Washington, D.C., The Brookings Institution, 1980, pp. 1-5.  4. James Tobin, “Macroeconomics and Fiscal Policy,” forthcoming as a chapter in *Paul A. Samuelson and Modern Economics*, McGraw-Hill.  5. James Tobin, “Harry Gordon Johnson 1923-1977,” *Proceedings of The British Academy*, London, 1978, Vol. LXIV, pp. 443-458.  6. J. Tobin, Kermit Gordon (1916-1976), *Year Book*, The American Philosophical Society, 1978.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1981*, Editor Wilhelm Odelberg, [Nobel Foundation], Stockholm, 1982  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.  *James Tobin died on March 11, 2002.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0877 |
| Interview |  |
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| ID | 0878 |
| Biographical | Leo Byron Klein and Blanche (Monheit) Klein, both of whom were born in the American Middle West, had three children. I, Lawrence Klein, was born in Omaha, Nebraska, as were my elder brother and younger sister. My early education was in the public school system of Omaha, where, retrospectively, I realize that my high school training served me in good stead for the basic subjects of mathematics, English, foreign languages and history.  Although I was not aware of it at the time, the experience of growing up during the Great Depression was to have a profound impact on my intellectual and professional career. Collegiate life subsequently gave me a basis for understanding this experience and to develop some analytical skills for dealing with the important economic aspects of this era, as well as the exciting times that were to come – World War II, postwar reconstruction, and expansion.  An early fascination with higher mathematics at the university level blossomed into speculative thinking that could provide a basis for dealing with economic issues. The teachings of the mathematics faculty at Los Angeles City College provided me with great stimulus, and the onset of World War II, with all the associated disturbances leading up to it, made a tremendous impression on my thoughts about socio-politico-economic interrelationships.  The completion of my undergraduate training at the University of California (Berkeley) provided just the needed touches of rigor at advanced levels in both economics and mathematics. My teachers there gave me great encouragement and challenge. It came as a surprise to find that a professional society and journal (*Econometrica*) were flourishing, and I entered this area of study with great enthusiasm.  The next two steps in my training and professional development were, however, fundamental. A chance to study at M.I.T. under the rising star of the period – [Paul A. Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/index.html) – was an unforgettable experience. I successfully vied for his time and attention which were instrumental in giving me a good grasp of economics and mathematical ways of dealing with significant problems of the subject. After I completed my dissertation under Paul Samuelson, the next major step was a decision to join the econometrics team at the Cowles Commission of the University of Chicago, where the director, Jacob Marschak, gave me the challenging assignment of reviving [Jan Tinbergen](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1969/index.html)‘s early attempts at econometric model building for the United States.  At Chicago, I was in the midst of a veritable galaxy of stars: [Trygve Haavelmo](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1989/index.html), [Tjalling Koopmans](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1975/index.html), Theodore Anderson, [Leonid Hurwicz](https://www.nobelprize.org/prizes/economic-sciences/2007/hurwicz/facts/), Herman Rubin, [Kenneth Arrow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1972/index.html), Don Patinkin, Herman Chernoff, and [Herbert Simon](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1978/index.html), among others. I completed my first of a series of macroeconometric models, solidified my understanding of econometrics, learned (through endless discussion) about the functioning of the economy, and got started on several theoretical paths such as aggregation, demand systems, and prediction.  At the Cowles Commission, I met and married Sonia Adelson (my second marriage). We were anxious to visit Europe right after World War II and left for Norway in October, 1947, to spend an academic year with [Ragnar Frisch](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1969/index.html) and Trygve Haavelmo. On the way from Chicago, I spent the summer of 1947 in Ottawa, helping to build the first of a series of econometric models for the Canadian government. During 1948, I had the opportunity of visiting economists in Sweden (Herman Wold, Erik Lundberg, Erik Lindahl, and Ragnar Bentzel), on the Continent (Jan Tinbergen), and in England ([Richard Stone](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1984/index.html)). I also had stimulating contact with Poul Norregaard-Rasmussen, who was visiting Oslo, and with Jorgen Pedersen of Aarhus. When I returned to America in the Autumn, 1948, I joined the staff of the National Bureau of Economic Research at the invitation of Arthur Burns, on a post doctoral grant and was able to make some econometric studies of production functions that opened up approaches that have stood the test of time. I became interested, at that time, in the possibility of estimating the effects of wealth, especially liquid assets, on saving behavior and joined the staff of the Survey Research Center, University of Michigan (jointly with the National Bureau of Economic Research for another year) to exploit the data that were being produced by George Katona’s surveys of consumer finances. My colleague, James Morgan, worked on similar problems and gave good insight to me. At Michigan, I restarted my work on macroeconometric model building and prepared, with my student, Arthur Goldberger, the model known as the Klein-Goldberger Model which evolved into a series of generations of the Michigan Model.  After four years at Michigan, I went to Oxford, at the suggestion of Frank Burchardt of the Institute of Statistics, to work on the data from the Oxford Savings Surveys and also to build a model of the U.K. During a four year stay at Oxford, I got started on some studies in theoretical econometrics dealing with methods of statistical inference.  Finally, I returned to America to join the faculty of the University of Pennsylvania, which has been my professional home ever since 1958. There I initiated a series of models that were to become known as the Wharton Models. They continue to develop. During my years at Pennsylvania, I have traveled widely, working on modeling projects for many countries – Japan, Israel, and Mexico, in particular. I have also supervised a number of dissertations that became sustained modeling efforts for many developing and developed countries.  During the early 1960s, I decided to supplement research support for quantitative economic studies at Pennsylvania by selling econometric forecasts to private and public sector buyers. The funds derived from these sales were then plowed back into student support and more general financing of a broader research effort in the Economics Department at Pennsylvania. This enterprise grew, over the years, into the status of a nonprofit corporation and ultimately was sold to a private publishing company to function as a for-profit corporation. The funds from the sale were put into research and general teaching budgets at the university. Wharton Econometric Forecasting Associates, Inc., is now a growing enterprise with many model and other econometric facilities.  After my first visit to Japan, in 1960, to work on a joint model building project at Osaka University, I maintained a continuing interest in the country and the entire Far East. On many occasions, I returned for conferences, lecturing, or extension of original econometric studies. Jointly with Michio Morishima and Shinichi Ichimura, I started *International Economic Review* which serves as a combined publishing effort of Osaka University and the University of Pennsylvania.  A committee of the Social Science Research Council (Economic Stability – later adding “Growth” to the title) looked into the question, in 1959, of building a superior short-run econometric model of the United States. I participated fully in that venture as a principal investigator, and the project continued for more than 10 years. While no working model survived this research decade, many new results and procedures were uncovered that shaped my whole course of research for some time to come. The parallel development with the series of Wharton models continued and benefitted from the transferenee of research results from the SSRC project, which had, meanwhile, moved to the Brookings Institution.  The SSRC committee turned attention from team research for building a model of the United States to doing one for world trade in order to investigate the international transmission mechanism. Project LINK was created at a meeting at Stanford in 1968. I shared responsibilities as principal investigator with Bert Hickman of Stanford, Rudolf Rhomberg of the International Monetary Fund and Aaron Gordon of the University of California. That project became an international cooperative venture, with the central coordinating facility and software located at the University of Pennsylvania. Project LINK is still thriving after more than a decade by adding new countries, new economic processes and a longer time horizon. As in the case of the Brookings-SSRC Model Project for the U.S., Project LINK created a great deal of related and incremental research by enabling countries to initiate econometric model building projects, by extending “best practice” research to various centers, and by showing official international bodies how to interrelate different parts of the world economy.  Visits to Israel to lecture at Hebrew University and to Vienna to lecture at the Institute for Advanced Studies laid the foundations for repeated return visits, in the former case as a board member of the Falk Institute for Research in Economics, and in the latter, as a member of the Scientific Advisory Board of the Institute for Advanced Studies. I have had additional reasons for visiting Austria to participate on the research program of the International Institute of Applied Systems Analysis.  My latest research efforts have been devoted to bringing new participants into the LINK Project, modeling the centrally-planned economies of the world (especially the U.S.S.R.), introducing modern econometrics into the People’s Republic of China, and expanding the activities of the Wharton Econometric Forecasting Associates where I presently serve as a professional consultant.  Over the years, I have often consulted with public officials on economic matters, both domestic and foreign, including international bodies. On several occasions I have provided public testimony at hearings but I have remained in academia and have not taken permanent positions in government. From my student days, the concept of public service and the relationship of theoretical economics or econometrics to real world problems has appealed to me, and I have tried to follow the footsteps of my teachers in practicing economics in this way.  My wife and I have four children – Hannah, following a scientific career as a Ph.D. geneticist; Rebecca, as a teacher; Rachel, as an editor; and Jonathan, as a computer programmer.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  Copyright © The Nobel Foundation 1980 **Addendum, May 2005** Since my becoming a Laureate in 1980, shortly after my 60th birthday, I continued regular teaching activities at Pennsylvania until the end of the academic year 1990-91. In my emeritus status, I supervised many doctoral dissertations, both at Pennsylvania and other universities, as well as supervised study of pre-doctoral students. Ongoing research activities and semiannual meetings of Project LINK have been maintained on an enlarged basis at the United Nations and the University of Toronto.  I have retained my personal research base of activities at the University of Pennsylvania and several consulting/forecast assignments at Wharton Econometrics, which became a private organization with the acronym WEFA and later merged into Global Insight. That work continues in the form of special projects. For many years I served on the Jury for the annual award of Premio Jaime Primero in Spain. Laureates from economics and other fields play prominent roles. Of course there are numerous other occasions every year that bring many Laureates together for serious discussions and lectures. Shortly before 1980, reform got under way in China. My involvement in leading a team of US economists to establish scholarly relations with Chinese colleagues and their institutions attracted my attention. With Lawrence Lau of Stanford, I organized a summer workshop in econometrics at the Summer Palace in Beijing. That effect blossomed into repeated visits to China, where I became a consultant to the State Information Center. From 1979-80 onwards, extensive scholarly exchange has been maintained with China, watching, interpreting, and trying to help the economy flourish. More than 25 years later, this scholarly exchange, both in China and at Pennsylvania has been extremely rewarding.  Soviet, and later, Russian reform have been equally interesting and challenging, during a transition period, in the form of doctoral research supervision. Much of this effect has grown into new attempts at econometric model building which are coming into fruition in the early 21st century. The econometric methods that have been so important for the study of macroeconomic performance in market economies have turned out to be equally revealing for the study of the two giant transition economies, decades later.  My personal research goal since 1980 has been to improve the forecast ability of quantitative methods in economics. At the beginning of this approach, data were comparatively scarce in sectoral coverage and in frequency. This has changed drastically in the information age. The joint use of modern computer hardware and the enormous availability of data point to the need for fresh research into the subject of accuracy of econometric forecasting.  I have always believed that people have misjudged the accuracy of economic forecasting and I lectured on that subject at the centennial gathering of Laureates in 2001, in Stockholm. During the 1980s and 1990s, I researched and applied methods of high frequency economic forecasting, to be used by themselves, and for objective establishment of initial conditions for longer range forecasts from structural dynamic models that carry forward the pioneering contributions of [Jan Tinbergen](https://www.nobelprize.org/nobel_prizes/economics/laureates/1969/index.html). It is my firm belief that the only satisfactory test of economics is the ability to predict, and in crucial predictive situations such as reconversion after World War II, the settlement of the Korean War, the settlement of the Vietnam War, the abrupt economic policy switch of the Nixon Administration in August 1972, the oil shock of 1973 (forecast of a world-wide succession by LINK), the recession of 1990. In these crucial periods, econometric models outperformed other approaches, yet there is considerable room for improvement, and that is precisely what is being examined in development of high-frequency models that aim to forecast the economy, every week, every fortnight, or every month, depending on the degree of fineness of the information flow. My present research deals directly with weekly forecasting for the United States, fortnightly forecasting for China, and monthly forecasting for Russia. I have consulted on model building for Hong Kong, Japan, Mexico, and Thailand, but not in connection with regularly repeated applications.  In the case of China, I and my colleague, Suleyman Ozmucur, joined the debate about the accuracy of China’s (ex post) estimates of their growth rates. We concluded, by the methods that we used, that China’s GDP figures were not biased; the research preceded the worldwide amazement and conviction that China was growing too fast. Also, with the help of two visiting scholars from China’s State Information Center, Ms Liping Tao and Mr Huiqing Gao, in summer 2004, I estimated urban and rural linear expenditure systems, from which we estimated “true” cost of living indexes, concluding that China’s inflation rate was overstated by more than 1.0%, thus implying that China’s growth rate had been even stronger than people realized.  Other research activities have included studies of US productivity growth in the 1980s and 1990s through the use of repeated input-output tables, from which it was possible, with the help of colleagues, Vijaya Duggal and Cynthia Saltzman, to estimate separate effects of increasing returns to scale, IT hardware, IT software in specific industries.  In 1990, after the end of the Cold War, a new society was launched, Economists Against the Arms Race, and I was an early Chairman of that organization, originally known as ECAAR. It reached international scope and, under various name changes, has remained committed to opposition to arms races, support of conflict resolution, arms control, and the analysis of effect of war on economic performance.  When I retired from full-time active teaching in 1991, I occasionally taught classes at the Osaka International University, Ritsumeikan University, and Reitaku University in Japan. I continued to serve as referee for some peer review journals and edited some collections of economic works, Project LINK and a memorial volume for a colleague, Albert Ando, in addition to making separate contributions for chapters in memorial volumes for others.  In this period since 1980, I have watched 7 and 1 grandchildren grow and develop. One considerably gifted grandson was swept away by a tidal wave at age ten, but I try to get as much satisfaction as possible in tracking the growth and development of others, one of whom followed in some footsteps as a postgraduate student at Oxford and Pennsylvania. My direct issues have become professor of genetics, professor of linguistics, author, and systems analyst. My wife and I follow the lives of children, spouses, and their issue with the greatest of interest.  In addition to teaching and research analysis in an academic setting since becoming a Laureate, I have served on several boards or committees, one for a profit making corporation and several for nonprofit entities, especially scholarly organizations. Two nonprofit assignments were for the Finance Committee and the Human Rights Committee of the National Academy of Sciences (US), various committees of the American Philosophical Society, and some for Israeli scholarly or academic institutions. These assignments for work in the nonprofit sectors have been intellectually rewarding – well worth the efforts.  One particular for-profit board has been with W P Carey & Co, an investment banking company engaging in sale-leaseback real estate operations as a means of providing liquid financial capital to US and some foreign companies. The W P Carey Foundation, a separate nonprofit organization makes charitable contributions and general philanthropic awards that are financed by the earnings that W P Carey has derived from his business operations. These activities have taught me a great deal about financial business activities as well as worthy philanthropic operations. I have earned and learned, on the board activities of W P Carey & Co in addition to providing advice for the company on macroeconomic issues that affect large companies. In this respect, I gained a great deal of knowledge about providing useful economic advice in business decision making.  *Lawrence R. Klein died on 20 October 2013.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0878 |
| Interview |  |
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| ID | 0879 |
| Biographical | The adverse economic events following the First World War turned me toward economics. In the Dakotas, where I was born (April 30, 1902), I learned during my youth how hard it was for farm families to stay solvent. Farm product prices fell abruptly by more than half. Banks went bankrupt and many farmers suffered foreclosures. Was politics or economics to blame? I opted for economics.  My schooling was disrupted by the shortage of labor during World War I. It meant foregoing high school. Then, late in 1921, I entered upon a short course in agriculture at South Dakota State College. I managed to enter college in 1924 and I was permitted to complete my college work in three years. The unorthodox economics of the University of Wisconsin during those years appealed to me. Despite my lack of proper credentials I was accepted by the graduate school. My intellectual debt to Professors Commons, Hibbard, Perlman and Wehrwein is large.  My professional apprenticeship at Iowa State College from 1930 to 1943 could not have been better; the Great Depression made it so and the talented younger economists at Ames during that period made it an exciting and profitable intellectual experience. The opportunity to consolidate and interpret that experience has been ideal for me at the University of Chicago, where I have been since 1943.  In retrospect, I value highly what I have learned about the economic behavior of rural people while abroad. During the summer of 1929, I acquired location specific information in parts of the Soviet Union. In 1960 when I was president of the American Economic Association, several U.S. economists and I were guests of the Soviet Academy of Sciences. It was a grand opportunity to return to the same locations about which I had acquired information in 1929. Over the years, I have ventured frequently into many low income countries to do what I did in the Soviet Union. In general, I avoided giving lectures or attaching myself while abroad to a university. To learn what I wanted to know, I went instead to rural communities and onto actual farms. Talk with university people, government officials and U.S. personnel stationed in the country was much less rewarding for me.  In addition, and beyond this, there is the standard puffing vita. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0879 |
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| ID | 0880 |
| Biographical | I was born in St. Lucia on January 23, 1915. My parents, who were both school teachers, had immigrated there from Antigua about a dozen years before. The islands were dissimilar in religion and culture, so our family had some slight characteristics of immigrant minorities.  My progress through the public schools was accelerated. When I was seven I had to stay home for several weeks because of some ailment, whereupon my father elected to teach me so that I should not fall behind. In fact, he taught me in three months as much as the school taught in two years, so, on returning to school, I was shifted from grade 4 to grade 6. So, the rest of my school life and early working life, up to age 18, was spent with fellow students or workers two or three years older than I. This gave me a terrible sense of physical inferiority, as well as an understanding, which has remained with me ever since, that high marks are not everything.  My father died when I was seven, leaving a widow and five sons, ranging in age from five to seventeen. My mother was the most highly-disciplined and hardest working person I have ever known, and this, combined with her love and gentleness, enabled her to make a success of each of her children.  I left school at 14, having completed the curriculum, and went to work as a clerk in the civil service. My next step would be to sit the examination for a St. Lucia government scholarship to a British university, but I would be too young for this until 1932. This job was not wasted on me since it taught me to write, to type, to file and to be orderly. But this was at the expense of not reading enough history and literature, for which these years of one’s life are the most appropriate.  In 1932 I sat the examination and won the scholarship. At this point I did not know what to do with my life. The British government imposed a colour bar in its colonies, so young blacks went in only for law or medicine where they could make a living without government support. I did not want to be a lawyer or a doctor. I wanted to be an engineer, but this seemed pointless since neither the government nor the white firms would employ a black engineer. Eventually I decided to study business administration, planning to return to St. Lucia for a job in the municipal service or in private trade. I would simultaneously study law to fall back on if nothing administrative turned up. So I went to the London School of Economics to do the Bachelor of Commerce degree which offered accounting, business management, commercial law and a little economics and statistics. This training has been very helpful in the various administrative jobs I have had to do, its weakness from the standpoint of my subsequent career (which was then inconceivable) was that it lacked mathematics.  I had no idea in 1933 what economics was, but I did well in the subject from the start, and when I graduated in 1937 with first class honours, LSE gave me a scholarship to do a Ph.D. in Industrial Economics.  In 1938, I was given a one-year teaching appointment which was sensational for British universities. This was converted into the usual four-year contract for an Assistant Lecturer in 1939. My foot was now on the ladder, and the rest was up to me. My luck held, and I rose rapidly. In 1948, at 33, I was made a full professor at the University of Manchester.  Until I went to Manchester, my field of study was industrial economics, and I published a series of articles on the subject culminating in a book in 1949. The leading practitioner of this art at LSE was Professor Sir Arnold Plant, and though he was a *laissez-faire* liberal and I a social democrat, I am indebted to him both for his incisive no-nonsense criticism and also for supporting me at crucial moments in the Appointments Committee.  My research work has been in three areas: in industrial economics, which I dropped after 1948; in the history of the world economy since 1870, which I started in 1944 and still pursue; and in development economics, which I did not begin systematically until about 1950.  I got into the history of the world economy because [Frederick Hayek](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1974/index.html), then Acting Chairman of the LSE Department of Economics suggested that I teach a course on “what happened between the wars” to give concreteness to the massive doses of trade cycle theory which then dominated the curriculum. I replied to Hayek that I did not know what happened between the wars; to which he replied that the best way of learning a subject was to teach it.  So I lectured on this subject for some years, and published a book on it in 1949. Among the questions that the book did not answer was whether the great depression of 1929 was *sui generis,* or one of a cycle stretching back into the nineteenth century. This I was determined to find out. However, data for the years before 1914 were sparse and unreliable, and I could not proceed faster than additions to the data and revisions would permit. I spent a lot of time with the data, and, between 1952 and 1957, published a stream of articles on world production, prices and trade from 1870 to 1914. However, I could never get the book done. In 1957, just as I was ready to start, I went off into administration for six years, never touching the subject. I returned to it in 1963, in my new professorship at Princeton University, to find that the four or five researchers of 1952 had now multiplied into a crowd of writers on this subject. I returned to improvement of the data and was just about ready to write my book when I went off to Barbados for four years setting up the Caribbean Development Bank. Returning to Princeton in 1974, I finally published in 1978 my account of growth and fluctuations in the world economy between 1870 and 1914. My Nobel Lecture derives from this sector of my intellectual interests.  Now for development economics. From the middle of the 1930s, I had spent time in the Colonial Office Library reading reports from the colonial territories on agricultural problems, mining, currency questions and the like, and by comparing different territories, had learnt something about the efficacy of different policies. I did some lecturing on this to colonial students at LSE in the 40s, but it was the throng of Asian and African students at Manchester that set me lecturing systematically on development economics from about 1950, following Hayek’s rule that the way to learn is to teach.  Half my interest was in policy questions, and here, my knowledge broadened in the 50s and 60s as a result of numerous visits to, and work stints in, African and Asian countries. This half led to my book on development planning published in 1966.  The other half of my interest was in the fundamental forces determining the rate of economic growth. This was the subject of my so-called classic book of 1955, and also the origin of the model to which the Nobel citation refers.  From my undergraduate days, I had sought a solution to the question of what determines the relative prices of steel and coffee. The approach through marginal utility made no sense to me. And the Heckscher-[Ohlin](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1977/index.html) framework could not be used, since that assumes that trading partners have the same production functions, whereas coffee cannot be grown in most of the steel producing countries.  Another problem that troubled me was historical. Apparently, during the first fifty years of the industrial revolution, real wages in Britain remained more or less constant while profits and savings soared. This could not be squared with the neoclassical framework, in which a rise in investment should raise wages and depress the rate of return on capital.  One day in August, 1952, walking down the road in Bangkok, it came to me suddenly that both problems have the same solution. Throw away the neoclassical assumption that the quantity of labour is fixed. An “unlimited supply of labour” will keep wages down, producing cheap coffee in the first case and high profits in the second case. The result is a dual (national or world) economy, where one part is a reservoir of cheap labour for the other. The unlimited supply of labour derives ultimately from population pressure, so it is a phase in the demographic cycle.  The publication of my article on this subject in 1954 was greeted equally with applause and with cries of outrage. In the succeeding 25 years, other scholars have written five books and numerous articles arguing the merits of the thesis, assessing contradictory data, or applying it to solving other problems. The debate continues.  Since 1957, I have spent nearly as many years in administration as in academic scholarship. First, a group of six years, 1957-1963, in which I was in turn UN Economic Adviser to the Prime Minister of Ghana, Deputy Managing Director of the UN Special Fund, and Vice-Chancellor (= President) of the University of the West Indies. Then, from 1970 to 1974, I set up the Caribbean Development Bank. These experiences broadened my understanding of development problems, without doing much to deepen it in the scholarly sense.  My wife Gladys was born in Grenada. Her father, who was an Antiguan, and my parents had known each other all their lives. She went to England in 1937 and trained as a teacher. We married in 1947 and have two daughters, Elizabeth and Barbara. My travels have meant much separation, but mutual love has supported the family in all its endeavours.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  *Sir Arthur Lewis died on June 15, 1991.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview |  |
| Interview |  |
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| ID | 0881 |
| Biographical | I was born in Milwaukee, Wisconsin, on June 15, 1916. My father, an electrical engineer, had come to the United States in 1903 after earning his engineering diploma at the Technische Hochschule of Darmstadt, Germany. He was an inventor and designer of electrical control gear, later also a patent attorney. An active leader in professional and civic affairs, he received an honorary doctorate from Marquette University for his many activities in the community. My mother, an accomplished pianist, was a third generation American, her forebears having been ’48ers who immigrated from Prague and Köln. Among my European ancestors were piano builders, goldsmiths, and vintners but to the best of my knowledge, no professionals of any kind. The Merkels in Köln were Lutherans, the Goldschmidts in Prague and the Simons in Ebersheim, Jews.  My home nurtured in me an early attachment to books and other things of the intellect, to music, and to the out of doors. I received an excellent general education from the public elementary and high schools in Milwaukee, supplemented by the fine science department of the public library and the many books I found at home. School work was interesting but not difficult, leaving me plenty of time for sandlot baseball and football, for hiking and camping, for reading and for many extracurricular activities during my high school years. A brother, five years my senior, while not a close companion, gave me some anticipatory glimpses of each stage of growing up. Our dinner table at home was a place for discussion and debate – often political, sometimes scientific.  Until well along in my high school years, my interests were quite dispersed, although they were increasingly directed toward science – of what sort I wasn’t sure. For most adolescents, science means physics, mathematics, chemistry, or biology – those are the subjects to which they are exposed in school. The idea that human behavior may be studied scientifically is never hinted until much later in the educational process – it was certainly not conveyed by history or “civics” courses as they were then taught.  My case was different. My mother’s younger brother, Harold Merkel, had studied economics at the University of Wisconsin under John R. Commons. Uncle Harold had died after a brief career with the National Industrial Conference Board, but his memory was always present in our household as an admired model, as were some of his books on economics and psychology. In that way I discovered the social sciences. Uncle Harold having been an ardent formal debater, I followed him in that activity too.  In order to defend free trade, disarmament, the single tax and other unpopular causes in high school debates, I was led to a serious study of Ely’s economics textbook, Norman Angell’s *The Great Illusion*, Henry George’s *Progress and Poverty*, and much else of the same sort.  By the time I was ready to enter the University of Chicago, in 1933, I had a general sense of direction. The social sciences, I thought, needed the same kind of rigor and the same mathematical underpinnings that had made the “hard” sciences so brilliantly successful. I would prepare myself to become a mathematical social scientist. By a combination of formal training and self study, the latter continuing systematically well into the 1940s, I was able to gain a broad base of knowledge in economics and political science, together with reasonable skills in advanced mathematics, symbolic logic, and mathematical statistics. My most important mentor at Chicago was the econometrician and mathematical economist, Henry Schultz, but I studied too with Rudolf Carnap in logic, Nicholas Rashevsky in mathematical biophysics, and Harold Lasswell and Charles Merriam in political science. I also made a serious study of graduate-level physics in order to strengthen and practice my mathematical skills and to gain an intimate knowledge of what a “hard” science was like, particularly on the theoretical side. An unexpected by-product of the latter study has been a lifelong interest in the philosophy of physics and several publications on the axiomatization of classical mechanics.  My career was settled at least as much by drift as by choice. An undergraduate field study for a term paper developed an interest in decision-making in organizations. On graduation in 1936, the term paper led to a research assistantship with Clarence E. Ridley in the field of municipal administration, carrying out investigations that would now be classified as operations research. The research assistantship led to the directorship, from 1939 to 1942, of a research group at the University of California, Berkeley, engaged in the same kinds of studies. By arrangement with the University of Chicago, I took my doctoral exams by mail and moonlighted a dissertation on administrative decision-making during my three years at Berkeley.  When our research grant was exhausted, in 1942, jobs were not plentiful and my military obligations were uncertain. I secured a position in political science at Illinois Institute of Technology by the intercession of a friend who was leaving. The return to Chicago had important, but again largely unanticipated, consequences for me. At that time, the Cowles Commission for Research in Economics was located at the University of Chicago. Its staff included Jacob Marschak and [Tjalling Koopmans](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1975/index.html) who were then directing the graduate work of such students as [Kenneth Arrow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1972/index.html), Leo Hurwicz, [Lawrence Klein](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1980/index.html), and Don Patinkin. Oscar Lange, not yet returned to Poland, [Milton Friedman](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1976/index.html), and [Franco Modigliani](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1985/index.html) frequently participated in the Cowles staff seminars, and I also became a regular participant.  That started me on a second education in economics, supplementing the Walrasian theory and Neyman-Pearson statistics I had learned earlier from Henry Schultz (and from Jerzy Neyman in Berkeley) with a careful study of Keyne’s *General Theory* (made comprehensible by the mathematical models proposed by [Meade](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1977/index.html), [Hicks](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1972/index.html), and Modigliani), and the novel econometric techniques being introduced by [Frisch](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1969/index.html) and investigated by the Cowles staff. With considerable excitement, too, we examined [Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/index.html)‘s new papers on comparative statics and dynamics.  I was soon co-opted by Marschak into participating in the study he and Sam Schurr were directing of the prospective economic effects of atomic energy. Taking responsibility for the macroeconomic parts of that study, I used as my analytic tools both classical Cobb-Douglas functions, and the new activity analysis being developed by Koopmans. Although I had earlier published papers on tax incidence (1943) and technological development (1947), the atomic energy project was my real baptism in economic analysis. My interest in mathematical economics having been aroused, I continued active work on problems in that domain, mainly in the period from 1950 to 1955. It was during this time that I worked out the relations between causal ordering and identifiability – coming for the first time in contact with the related work of Herman Wold – discovered and proved (with David Hawkins) the Hawkins-Simon theorem on the conditions for the existence of positive solution vectors for input-output matrices, and developed (with Albert Ando) theorems on near-decomposability and aggregation.  In 1949, Carnegie Institute of Technology received an endowment to establish a Graduate School of Industrial Administration. I left Chicago for Pittsburgh to participate with G.L. Bach, William W. Cooper, and others in developing the new school. Our goal was to place business education on a foundation of fundamental studies in economics and behavioral science. We were fortunate to pick a time for launching this venture when the new management science techniques were just appearing on the horizon, together with the electronic computer. As one part of the effort, I engaged with Charles Holt, and later with Franco Modigliani and John Muth, in developing dynamic programming techniques – the so-called “linear decision rules” – for aggregate inventory control and production smoothing. Holt and I derived the rules for optimal decision under certainty, then proved a certainty-equivalence theorem that permitted our technique to be applied under conditions of uncertainty. Modigliani and Muth went on to construct efficient computational algorithms. At this same time, [Tinbergen](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1969/index.html) and Theil were independently developing very similar techniques for national planning in the Netherlands.  Meanwhile, however, the descriptive study of organizational decision-making continued as my main occupation, in this case in collaboration with Harold Guetzkow, James March, Richard Cyert and others. Our work led us to feel increasingly the need for a more adequate theory of human problem-solving if we were to understand decisions. Allen Newell, whom I had met at the Rand Corporation in 1952, held similar views. About 1954, he and I conceived the idea that the right way to study problem-solving was to simulate it with computer programs. Gradually, computer simulation of human cognition became my central research interest, an interest that has continued to be absorbing up to the present time.  My research on problem-solving left me relatively little opportunity to do work of a more classical sort in economics. I did, however, continue to develop stochastic models to explain the observed highly-skewed distributions of sizes of business firms. That work, in collaboration with Yuji Ijiri and others, was summarized in a book published just two years ago.  In this sketch, I have said less about my work on decision-making than about my other research in economics because the former is discussed at greater length in my Nobel lecture. I have also left out of this account those very important parts of my life that have been occupied with my family and with non-scientific pursuits. One of my few important decisions, and the best, was to persuade Dorothea Pye to marry me on Christmas Day, 1937. We have been blessed in being able to share a wide range of our experiences, even to publishing together in two widely separate fields: public administration and cognitive psychology. We have shared also the pleasures and responsibilities of raising three children, none of whom seem imitative of their parents’ professional directions, but all of whom have shaped for themselves interesting and challenging lives.  My interests in organizations and administration have extended to participation as well as observation. In addition to three stints as a university department chairman, I have had several modest public assignments. One involved playing a role, in 1948, in the creation of the Economic Cooperation Administration, the agency that administered Marshall Plan aid for the U.S. Government. Another, more frustrating, was service on the President’s Science Advisory Committee during the last year of the Johnson administration and the first three years of the Nixon administration. While serving on PSAC, and during another committee assignment with the National Academy of Sciences, I have had opportunities to take part in studies of environmental protection policies. In all of this work, I have tried – I know not with what success – to apply my scientific knowledge of organizations and decision-making, and, conversely, to use these practical experiences to gain new research ideas and insights.  In the “politics” of science, which these and other activities have entailed, I have had two guiding principles – to work for the “hardening” of the social sciences so that they will be better equipped with the tools they need for their difficult research tasks; and to work for close relations between natural scientists and social scientists so that they can jointly contribute their special knowledge and skills to those many complex questions of public policy that call for both kinds of wisdom.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  *Herbert A. Simon died on February 9, 2001.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0881 |
| Interview |  |
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| ID | 0882 |
| Biographical | I was born into an upper-middle class family in a village in the South of Sweden in April, 1899. It was a large family with seven children, a large house and a home which was very hospitable and open to friends and relatives. There was a private school which was not very particular about the knowledge entering children had acquired. Normally, they should have had three years of preparatory studies but many of the children had only two years. For some reason, I was given only a little private teaching and prep school for one year before entering the school at the age of seven. Hence, I passed the “baccalaureat” in the classical line in the city of Hälsingborg rather early. As mathematics had been my best subject at school, my parents proposed – and I accepted – studies at the University of Lund in mathematics, statistics and economics. The choice of the latter subject is said to be due to the fact that at the age of five years, I was very fond of calculating the cost of the various cakes my mother used to bake.  After two years, I obtained the degree of *fil. kand.* with the highest mark in economics. My teacher, Professor Smil Sommarin, was a fine pedagogue, a very generous person and a great admirer of Kurt Wicksell.  Having seen in a newspaper a review of a book about the economic aspects of the world war – written by professor Eli Heckscher, who was professor at the Stockholm Business School – I suggested to my parents, that I should take up studies there. This I did and was much stimulated by Heckscher’s teaching. He was always helpful and friendly although we started with a cleavage of opinion about the correct economic principles for the right time to cut trees in forestry. With the aid of differential calculus I solved a fairly obvious profit-maximization problem.  Having finished the two years’ course at the Business School, where my studies included the French and Russian languages, I moved to the philosophical faculty of Stockholm University where my teachers were Gustav Cassel and Gösta Bagge. I also took up work for the State Tariff Committee. Cassel and Bagge were not quite so stimulating lecturers as Heckscher but they sacrificed a great deal of time for private discussion, which was exceedingly fine teaching. Like Heckscher, they did not discuss my thesis before its first version was ready.  A Stimulating Club Already, in 1918, I had become a member of the “Political Economy Club” which had been created a year before. This was a small gathering of trained economists who were interested in scientific work in economics. Apart from my three main teachers, also Professor Sven Brisman, Knut Wicksell, David Davidson and half a dozen “docents” were members. The total membership was about 20 of which 4 were graduate students. One of the latter was Per Jacobson who later became head of the International Monetary Fund. The meetings of this club were certainly the most stimulating “seminar” one could imagine. One of the members opened a discussion and then followed a free exchange of opinions. The subjects were chiefly theoretical. Knut Wicksell, who was 67 years old when I became a member, was probably the most stimulating participant of all the members. I shall never forget his questioning and modest attitude even when the problems concerned his speciality, which was monetary theory and financial policy. When meeting him privately in the library, he would sometimes ask Jacobson or me if we could help him to understand a difficult problem.  In 1919, I presented a paper on the theory of inflation in the Political Economy Club which differed from that of Cassel and Wicksell in stressing that – even in a state of balance between total demand and supply – the volume of purchasing power could rise when some prices go up. Consequently, the fact that a limitation of supply during the war could lead to higher prices of many commodities would not automatically bring about a fall in the prices of other commodities. Part of the content was published two years later in the *Ekonomisk Tidskrift*. There was some similarity with one aspect of Wicksell’s last paper in 1925 which, however, covered a wider field and provided a deeper insight into monetary theory.  From the autumn of 1920, I served for one year as assistant secretary to the Economic Council which, under the chairmanship of the Minister of Finance, included nine economic leaders from banking, industry and agriculture, and Gustav Cassel as representative of economic science. One of the bankers was Mr. Marcus Wallenberg, who played such a great role in Sweden’s economic life, as have also his two sons done later during several decades.  After one year of military service in the Navy and three months studies at Grenoble, France, where the international student’s milieu and the friendliness of the teachers and many other local people made the stay extremely pleasant, I presented my thesis about international trade theory to Gustav Cassel in 1922 to obtain the degree *licentiatus philosophiae*. It followed the same lines as my doctor’s thesis in 1924 and the mathematical appendix in my later book, *Interregional and International Trade* (1933). At that time, neither Cassel nor myself knew that the construction to combine the price systems for two different countries had been used long before by the famous French economist, Cornot, as well as by Pareto and other prominent Italian economists, building on the opportunity cost idea. However, they had drawn very few practical conclusions from their fine scientific attack on the international trade problem. I soon found that the approach in Heckscher’s pioneer paper on *The Influence of Foreign Trade on the Distribution of Income* (1919), where he analysed facts behind the differences in comparative costs, could profitably be used in a mutual interdependence price system for a realistic analysis of international trade. While Heckscher regarded his reasoning as a kind of supplement to the classical comparative cost analysis, I insisted on the use of a consistent reasoning in terms of prices. Thereby, the price system of the different national economies could be joined together and the trade between them, which takes place with the aid of prices could be analysed and many modifications in terms of money expenditure could be added.  Cassel thought that I greatly exaggerated the influence of Heckscher’s paper, but I did not agree then and do not agree now. It was a very essential stimulus.  At Cassel’s suggestion, I sent a paper containing a brief version of my thesis to Professor Edgeworth who was then co-editor with Keynes for the *Economic Journal*. It presented equation systems as a basis for an analysis of the causes and effects of international trade. At that time, equations were not so popular as diagrams. Anyhow, Edgeworth sent my paper to Keynes and asked for his opinion. Keynes wrote on a piece of paper which followed the manuscript via Edgeworth back to me: “This amounts to nothing and should be refused. J.M. Keynes.” I still retain this little note as a valuable document.  At this time, as well as later, Keynes was a very busy man. He could not read all manuscripts carefully. If he had done so with my manuscript in 1922, we might have agreed more easily about the German reparation problem which we discussed seven years later in *The Economic Journal* and in several letters.  Visits to the Two Cambridges In 1922, I got a small stipend from the Swedish-American Foundation and went to Cambridge, England, for a few months and thereafter to Harvard University. In the summer, Cambridge was rather empty, but I am grateful for many pleasant talks about economics with Austin Robinson who, in the summer of 1922, seemed to be about as lonely as I was.  Visit to Harvard When the Atlantic steamer arrived in New York harbour, the health officer came on board. The examination was very brief. He looked in my eyes and then asked what I was going to do in the United States. I said I was going to study for one year. “Where” he asked. “At Harvard”, was my answer. “You are lucky. Go ahead!” It was said with a friendly smile. There was something of open arms in his attitude which was, on the whole, characteristic also of the later reception at Harward and elsewhere in the American academic world.  My main teachers were Taussig and John H. Williams in international relations, Carver in agricultural economics as well as Alleyn Young in the history of economic doctrine. I rapidly came to the conclusion – as I had done several times before – that I was lucky in getting teachers who were brilliant, friendly and stimulating. Instead of reading well-known books for the courses, I wrote some chapters on my thesis and a paper on the laws of production which, later, however, I never published. When I looked at it a couple of years later, I found that my friend, [Ragnar Frisch](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1969/index.html), had presented a superior analysis in his first non-published compendium, which later became his well-known book on the *Theory of Production.* There was no need to publish my version.  After the two autumn months at Cambridge, England, where I had an attack of eye inflammation and was – as doctors later learned – quite unnecessarily kept in bed during the second month, I returned to Sweden. It was nevertheless a very pleasant stay and the lectures by D.H. Robertson which I attended in October were stimulating. Naturally I did not get as many contacts when confined to my bed as I might otherwise have done.  Having returned to Stockholm I dictated the later part of my thesis in English. However, I found it wiser to publish the thesis in Swedish, translated it, and got my doctor’s degree and the position as docent (“Assistant Professor”) in May 1924.  Five Years in Copenhagen Already, by Christmas time, 1923, Heckscher had written, telling me that the chair in economics which had been held by the famous statistician Harald Westergaard in the University of Copenhagen was open for applications. He hinted that even if one could not say anything about the result he saw no reason why I should not send in my papers. The Danish universities have a system of arranging a “competition” when there are different applicants and no one is clearly superior. Two Danes, one Norwegian and two Swedes – the other one was Erik Lindahl – were given three months to write a thesis on “the economic effect of the 48 hours week”. We also had to give a lecture on “guild socialism” after 48 hours preparation, and two lectures on a freely-chosen subject, which, in my case was “Monetary Stabilisation”.  The outcome was that a majority of the seven judges voted for me, the minority for Erik Lindahl. So I was appointed and took up my new duties in Copenhagen in January 1925.  It would take me too long to attempt a description of the intellectual and scientific stimulus given to me in the fine university, and in Copenhagen, in general, in the five years I remained there. Coming from the south of Sweden I felt at once at home. Among the economists, Dr. L.V. Birck, who combined a sharp intelligence with an exquisite sense of humour, exercised the greatest influence on me. But I think that I learnt as much from the students and those who had recently been students as from my colleagues. To this category belonged Carl Iversen, Thorkel Christensen, Jörgen Dich and Jörgen Pedersen, all of whom later made important contributions to economics and international economic cooperation.  The trouble was that life was too pleasant and there were too many things to do. The re-writing of my thesis in English and the adding of a section on location theory could not be done as quickly as I had hoped. In 1928 I sent a version to Harvard in the competition for the David Well’s prize. Taussig sent me a kind letter and said that they had granted the prize to another economist but that they were willing to print my long manuscript in the *Harvard Economic Studies.* I was delighted. The book was finished in January 1931, when I had returned to Sweden as successor to Heckscher in the Stockholm School of Business. Several colleagues, and, particularly, Carl Iversen, made useful, more or less critical observations about the manuscript. Chiefly owing to my own correcting and revising proofs – after valuable suggestion by my colleague and relative, Tord Palander – the book did not appear until the spring of 1933.  Apart from the general approach indicated above, the book was characterised by an attempt to pay more attention to how factor supply reactions, location, taxation, social policy, and risk affect international division of labour. The static factor proportion model was only a beginning.  From January to the end of August 1931 I was busy at Geneva making a report about “The Course and Phases of the World Economic Depression”. Working conditions were practically ideal. I was helped by the members of the economic and statistical secretariat whenever I wanted it. I also had two very good assistants, Major Wright and Al Kraal. Besides, a dozen and a half good economists came twice for a couple of days of discussion around the outline I had made for the book. Nevertheless, it was hard work to get the report ready for the September Assembly as I had to go to Stockholm for lectures during four weeks in the spring and was far from having specialist knowledge about business cycles when I started in January. I also gave a lecture on a combined deficit financial policy and monetary policy as a remedy for the world depression at the Nordic Economic Conference in June 1931.  Monetary Theory and Unemployment From that time, I concentrated my attention chiefly on monetary theory and economic expansion. Utilizing earlier achievements by Knut Wicksell, Erik Lindahl and [Gunnar Myrdal](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1974/index.html), I tried to construct a model for an analysis of a process of expansion in a state of large unused resources. After writing some papers in the *Ekonomisk Tidskrift* and elsewhere, I finished my report on measures against unemployment in the spring of 1934. It was summarised in the Finally Lectures in the University of Dublin the same year. The theoretical model for a time-using process and the policy conclusions that was there presented had some similarity with Mr. Keynes’ *The General Theory of Employment, Interest and Money* which was published two years later. I want to stress that I could not have produced this book without the assistance of the previous achievements of my Swedish colleagues. To what extent a theoretical development in Stockholm went parallel to, and, in some respect, preceded the development at Cambridge is a matter which has been subject of much discussion in recent years – which still continues – particularly in the scientific journal, *History of Political Economy* and in earlier books by Landgren and Steiger.  I was asked to deliver the Marshall Lectures at Cambridge in 1936 which gave me an opportunity to summarize the Swedish theory and make some comparisons with Keynes’ work. A considerable part of the lectures was published in the *Economic Journal*, 1937, under the title, *The Stockholm Theory of Saving and Investment.* It led to a discussion with Keynes, D.H. Robertson and R.G. Hawtray, chiefly about the theory of interest.  A year before, I had published a volume on *International Economic Reconstruction* which was part of an international investigation into world economic problems organised by the International Chamber of Commerce and the Carnegie Endowment for International Peace.  In 1938, I became a member of the Swedish Riksdag. As I continued to teach at the Stockholm School of Business in the coming years, I had only little time for scientific work. The scope for political work during a world war is almost unlimited even in a neutral country.  However, I took a few weeks off to lecture at Columbia University in January 1947. A somewhat modified series of lectures was given at Oxford in the autumn of the same year. The lectures were published in 1949 under the title, *The Problem of Employment Stabilisation*.  The scientific papers I published in the 1940s and in the following twenty years were not numerous. Perhaps I should mention the comparison between the monetary theory of the Stockholm School and the “quantity theory” which was published in 1943 in Swedish in the *Ekonomisk Tidskrift* and, some years later, in the annual volume of translations edited by the International Economic Association.  Political Activity As I became leader of the Liberal Party from 1944 – and for the rest of the war, a member of the government – and as I continued to teach from 1945 to 1965 and remained party leader until 1967, only little time was available for scientific research. From 1946 until my resignation from the leadership, the Liberal Party was the leading opposition party, except for an interregnum of two years after a setback at the municipal election in 1958.  This is not the place for a discussion of my political activity as leader of a political party for 23 years. The Swedish two-chamber system made the vote in municipal elections affect the composition of “the first chamber” many years after the vote. This chamber was elected through “indirect” elections by municipal councils – one eighth of its members every year. In some periods the social democratic government owed its majority in the Riksdag to the “overrepresentation” the advantages this electoral system brought and to the support from the Communist Party. The attitude of the Liberal Party was all the time positive to social reforms but negative to nationalisation of Swedish industry or unnecessarily detailed central state control of economic life. It is worth noting that in the four decades before 1970, practically no nationalisation took place in Sweden – much less than, *e.g.*, in France, Italy and Austria. A constitutional reform in 1968 ended the two-chamber system and led to a 50-50 position in 1973, and a non-socialist majority in 1976. The earlier existence of “the first chamber” had prevented that such a majority in 1957 led to a new government.  During the greater part of the time as party leader, I contributed articles to one or two leading Swedish newspapers. All in all I published about 1200 newspaper articles in the years 1919-1977, of which around 700 appeared in the years from 1931 to 1943.  Since I left the Riksdag in 1970 I have had more time for scientific articles and lectures. I have also written newspaper articles based on some research into monetary theory and the distribution of income as well as “inflation-protected taxation” and international economic problems.  Nobel Symposium It gave me great satisfaction when the Nobel Foundation – using a grant from the Bank of Sweden – as well as grants from the Marcus Wallenberg and the Handelsbanken Research Foundations – in 1974 decided to add a symposium in economic science to the symposia in natural sciences, international peace problems, and literature which had been organized in the preceding decades. The “Prize Committee” in economic science decided to choose as subject, *The International Allocation of Economic Activity.* As chairman of the organizing committee, I was grateful for the friendly reception of our invitations. The Symposium took place in Stockholm in 1976, and a volume edited by P.O. Hesselborn, P. Wijkman and myself appeared towards the end of the following year. It contains contributions from a large part of the most prominent economists in this field. However, monetary aspects of international trade relations found no place on the agenda as several international symposia had in recent years taken up this subject for debate. On the other hand, scientists who have come from economic and social geography to a study of the international division of labour or have specialised in regional economics were well represented. One of the chief aims of the symposium was to avoid arbitrary border lines between different approaches to research into local aspects of production and trade.  To sum up a personal reaction: It has not been easy to combine scientific work, teaching, journalistic writing and political leadership. All of these types of activity have no doubt suffered from my attempts to do too many things at the same time. However, I have found it all to be a fascinating business.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  *Bertil Ohlin died on August 3, 1979.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0882 |
| Interview |  |
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| ID | 0883 |
| Biographical | Born on 23 June 1907, I was brought up in the City of Bath in England. At school – Lambrook School (1917-1921) and Malvern College (1917-1926) – my education was concentrated on the Latin and Greek languages. At the university – Oriel College, Oxford (1926-1930) – I continued my classical education until 1928. I then moved over for two years to the newly-started School of Philosophy, Politics and Economics.  My interest in economics had the following roots. Like many of my generation I considered the heavy unemployment in the United Kingdom in the inter-war period as both stupid and wicked. Moreover, I knew the cure for this evil, because I had become a disciple of the monetary crank, Major C.H. Douglas, to whose works I had been introduced by a much loved but somewhat eccentric maiden aunt. But my shift to the serious study of economics gradually weakened my belief in Major Douglas’s A+B theorem, which was replaced in my thought by the expression MV = PT.  In 1930 I was elected to a Fellowship at Hertford College, Oxford, with freedom in the first year to continue my study of economics as a post-graduate student. As a result of having lived as a child next door to his great aunt, I already knew Dennis Robertson who invited me to go to Trinity College, Cambridge, as his student for the academic year 1930/31. This resulted in the intellectually most exciting year of my life.  At Cambridge I made a close friendship with Richard Kahn and became a member of the ‘Circus’ with him, Piero Sraffa and Joan and Austin Robinson, which discussed Keynes’ *Treatise on Money* and stimulated the start of its translation into the *General Theory*. Keynes appeared at the weekends when Richard Kahn reported to him our discussions of the week and when we met on Monday evenings at the Political Economy Club in Keynes’ rooms in King’s College. Thus I abandoned the formula MV = PT for I = S.  To spend this particular year reading essays to Dennis Robertson as one’s supervisor, and, simultaneously, enjoying membership of the group round Keynes was indeed an intellectual treat.  From 1931 to 1937, I was a Fellow and Lecturer in Economics at Hertford College, Oxford. The teaching of economics as a regular subject for examination was relatively new in Oxford and we were a young group of enthusiasts, including, in addition to Eric Hargreaves, my old tutor at Oriel College, Roy Harrod, Henry Phelps Brown, Charlie Hitch, Robert Hall, Lindley Fraser and Maurice Allen. My job was to teach the whole corpus of economic theory, but there were two subjects in which I was especially interested, namely, the economics of mass unemployment and international economics. In the 1930s one was aware of two great evils – mass unemployment and the threat of war. I thought – and I still think – that the government of the United Kingdom could, in those days, have altered the course of world history by listening to Keynes on employment policy and by backing to the hilt the League of Nations for the preservation of peace. In Oxford there was a strong branch of the League of Nations Union with Gilbert Murray as its chairman and Margaret Wilson as its secretary. In 1933 Margaret and I were married. Was there some positive feedback between my interest in Margaret and my interest in international affairs?  Margaret had close links with Geneva where she had spent some years as a student while her parents had been wardens of the Quaker Hostel there and where she had gone back as secretary to Gilbert Murray. At the end of 1937 I joined the Economic Section of the League of Nations in Geneva as editor of the World Economic Survey and produced the two issues for 1937/38 and 1938/39. These were the seventh and eighth issues of the Survey, which had been preceded in 1930 by a prototype volume entitled, *The Course and Phases of the World Depression*, prepared by [Bertil Ohlin](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1977/index.html). The preparation of the World Economic Survey was backed by the more specialised volumes written by the established members of the section, including, Rasminsky, Tirana, Nurkse and Hilgerdt. Haberler, with Marcus Fleming as his assistant, had just produced *Prosperity and Depression*; and [Tinbergen](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1969/index.html), with Pollak as his assistant, followed by [Koopmans](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1975/index.html), were busy applying statistical tests to the theories outlined by Haberler. Loveday, the Director of the Economic Section, seemed to have the knack of picking his team.  After the outbreak of war, in April 1940, we left Geneva with our three children aged 4 years, 2 years and 2 weeks only to become part of the disordered refugee crowds fleeing across France from the German army. After some adventures, we reached England where I became a member of the Economic Section of the War Cabinet Secretariat. I remained a member of the section till 1947, becoming Director in 1946. Under the inspiring wartime leadership of Lionel Robbins, and in close cooperation with Keynes in the Treasury, the Section became an influential body, having the day-to-day task of giving advice at a moment’s notice on any economic problem ranging from points-rationing for foodstuffs to the price policies of nationalised industries. For myself, three major tasks stand out. First and foremost, in 1940 and 1941, [Richard Stone](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1984/index.html) and I prepared the first official estimates of the UK national income and expenditure, and we did so in a form which constituted what was, I believe, the first true double-entry social accounts prepared for any country. Second, there were the discussions and drafts leading up to the White Paper on Employment Policy of 1944 in which the UK government accepted the maintenance of employment as an obligation of governmental policy. Third, there were the discussions, state papers and conferences leading up to the post-war international financial and economic settlement, namely the International Monetary Fund, the International Bank for Reconstruction and Development and the General Agreement on Tariffs and Trade. I was especially concerned with the last of these.  In 1947 I became Professor of Commerce at the London School of Economics where Lionel Robbins was our leader in the Economics Department with its large and rich team of economic colleagues. Of these, I will mention only Professor A.W.H. Phillips to whom I owe an immense intellectual debt of gratitude for education in the treatment of dynamic systems. There I embarked on an over-ambitious project. My interest in economics has always been in the whole corpus of economic theory, the interrelationships between the various fields of theory and their relevance for the formulation of economic policy. In Oxford before the war, I had, with this interest in mind, written a short textbook entitled, *An Introduction to Economic Analysis and Policy*. It was now my intention to rewrite this work. I realised that it might be necessary to do so in more than one volume. So, as I was appointed at the LSE to teach international economics, I started on *The Theory of International Economic Policy*. It grew into my two books, *The Balance of Payments*, and *Trade and Welfare*, with their two mathematical appendices. The former examined the international relations between a number of national economies constructed on the Keynesian model; the latter applied the theory of economic welfare to international transactions.  These books took up practically the whole of my ten years at the LSE; but even so they did not cover the whole of the international problem, there being little or no reference in them to the international aspects of economic growth or of dynamic disequilibrium. My original project was over-ambitious; but the part which I did manage to cover was sufficient, eventually, to gain for me the Nobel award.  In 1957 I moved from London to the chair of Political Economy in Cambridge, which I held till 1967, when I resigned to become a Senior Research Fellow of Christ’s College, Cambridge. I relinquished that Fellowship on retirement age in 1974. During these years I set out to confine my over ambitious project, planning to write one or two volumes on the domestic aspects of economic theory and policy. So far I have written four volumes in this series: *The Stationary Economy, The Growing Economy, The Controlled Economy,* and *The Just Economy*. But even so, I have managed only to make a beginning. The frontiers of knowledge in the various fields of our subject are expanding at such a rate that, work as hard as one can, one finds oneself further and further away from an understanding of the whole. I believe this experience to illustrate the basic problem of our subject. Sane economic policy must take into account simultaneously all aspects of the economy; but a soundly-based understanding of the whole and of the relationship between its parts becomes more and more difficult, if not impossible, to attain.  How then should a seventy-year old best use his remaining years? Since 1974 I have taken time off to act as full-time chairman of a committee set up by the Institute for Fiscal Studies to examine the structure of direct taxation in the United Kingdom, the committee consisting of a number of first-rate economic theorists and of leading practitioners in tax law, accountancy and administration. I have learned as much in the last three years as in any other comparable period of my life, but with an added realisation of how little over a half century of study one has in fact managed to learn of the whole range of economic policy issues.  Such in brief outline has been my intellectual development. But whatever I may have achieved would have been impossible without the advantages of my family background: a loving mother, a father concerned only to give me the best start in life, a wife who gives unfailing support, and four, happily married children providing seven lively grandchildren.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  *James E. Meade died on December 22, 1995.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0883 |
| Interview |  |
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| ID | 0884 |
| Biographical | I was born July 31, 1912, in Brooklyn, N.Y., the fourth and last child and first son of Sarah Ethel (Landau) and Jeno Saul Friedman. My parents were born in Carpatho-Ruthenia (then a province of Austria-Hungary; later, part of inter-war Czechoslovakia, and, currently, of the Soviet Union). They emigrated to the U.S. in their teens, meeting in New York. When I was a year old, my parents moved to Rahway, N.J., a small town about 20 miles from New York City. There, my mother ran a small retail “dry goods” store, while my father engaged in a succession of mostly unsuccessful “jobbing” ventures. The family income was small and highly uncertain; financial crisis was a constant companion. Yet there was always enough to eat, and the family atmosphere was warm and supportive.  Along with my sisters, I attended public elementary and secondary schools, graduating from Rahway High School in 1928, just before my 16th birthday. My father died during my senior year in high school, leaving my mother plus two older sisters to support the family. Nonetheless, it was taken for granted that I would attend college, though, also, that I would have to finance myself.  I was awarded a competitive scholarship to Rutgers University (then a relatively small and predominantly private university receiving limited financial assistance from the State of New Jersey, mostly in the form of such scholarship awards). I was graduated from Rutgers in 1932, financing the rest of my college expenses by the usual mixture of waiting on tables, clerking in a retail store, occasional entrepreneurial ventures, and summer earnings. Initially, I specialized in mathematics, intending to become an actuary, and went so far as to take actuarial examinations, passing several but also failing several. Shortly, however, I became interested in economics, and eventually ended with the equivalent of a major in both fields.  In economics, I had the good fortune to be exposed to two remarkable men: Arthur F. Burns, then teaching at Rutgers while completing his doctoral dissertation for Columbia; and Homer Jones, teaching between spells of graduate work at the University of Chicago. Arthur Burns shaped my understanding of economic research, introduced me to the highest scientific standards, and became a guiding influence on my subsequent career. Homer Jones introduced me to rigorous economic theory, made economics exciting and relevant, and encouraged me to go on to graduate work. On his recommendation, the Chicago Economics Department offered me a tuition scholarship. As it happened, I was also offered a scholarship by Brown University in Applied Mathematics, but, by that time, I had definitely transferred my primary allegiance to economics. Arthur Burns and Homer Jones remain today among my closest and most valued friends.  Though 1932-33, my first year at Chicago, was, financially, my most difficult year; intellectually, it opened new worlds. Jacob Viner, Frank Knight, Henry Schultz, Lloyd Mints, Henry Simons and, equally important, a brilliant group of graduate students from all over the world exposed me to a cosmopolitan and vibrant intellectual atmosphere of a kind that I had never dreamed existed. I have never recovered.  Personally, the most important event of that year was meeting a shy, withdrawn, lovely, and extremely bright fellow economics student, Rose Director. We were married six years later, when our depression fears of where our livelihood would come from had been dissipated, and, in the words of the fairy tale, have lived happily ever after. Rose has been an active partner in all my professional work since that time.  Thanks to Henry Schultz’s friendship with Harold Hotelling, I was offered an attractive fellowship at Columbia for the next year. The year at Columbia widened my horizons still further. Harold Hotelling did for mathematical statistics what Jacob Viner had done for economic theory: revealed it to be an integrated logical whole, not a set of cook-book recipes. He also introduced me to rigorous mathematical economics. Wesley C. Mitchell, John M. Clark and others exposed me to an institutional and empirical approach and a view of economic theory that differed sharply from the Chicago view. Here, too, an exceptional group of fellow students were the most effective teachers.  After the year at Columbia, I returned to Chicago, spending a year as research assistant to Henry Schultz who was then completing his classic, *The Theory and Measurement of Demand*. Equally important, I formed a lifelong friendship with two fellow students, [George J. Stigler](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1982/index.html) and W. Allen Wallis.  Allen went first to New Deal Washington. Largely through his efforts, I followed in the summer of 1935, working at the National Resources Committee on the design of a large consumer budget study then under way. This was one of the two principal components of my later *Theory of the Consumption Function.*  The other came from my next job – at the National Bureau of Economic Research, where I went in the fall of 1937 to assist [Simon Kuznets](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1971/index.html) in his studies of professional income. The end result was our jointly published *Incomes from Independent Professional Practice*, which also served as my doctoral dissertation at Columbia. That book was finished by 1940, but its publication was delayed until after the war because of controversy among some Bureau directors about our conclusion that the medical profession’s monopoly powers had raised substantially the incomes of physicians relative to that of dentists. More important, scientifically, that book introduced the concepts of permanent and transitory income.  The catalyst in combining my earlier consumption work with the income analysis in professional incomes into the permanent income hypothesis was a series of fireside conversations at our summer cottage in New Hampshire with my wife and two of our friends, Dorothy S. Brady and Margaret Reid, all of whom were at the time working on consumption.  I spent 1941 to 1943 at the U.S. Treasury Department, working on wartime tax policy, and 1943-45 at Columbia University in a group headed by Harold Hotelling and W. Allen Wallis, working as a mathematical statistician on problems of weapon design, military tactics, and metallurgical experiments. My capacity as a mathematical statistician undoubtedly reached its zenith on V. E. Day, 1945.  In 1945, I joined George Stigler at the University of Minnesota, from which he had been on leave. After one year there, I accepted an offer from the University of Chicago to teach economic theory, a position opened up by Jacob Viner’s departure for Princeton. Chicago has been my intellectual home ever since. At about the same time, Arthur Burns, then director of research at the National Bureau, persuaded me to rejoin the Bureau’s staff and take responsibility for their study of the role of money in the business cycle.  The combination of Chicago and the Bureau has been highly productive. At Chicago, I established a “Workshop in Money and Banking”. which has enabled our monetary studies to be a cumulative body of work to which many have contributed, rather than a one-man project. I have been fortunate in its participants, who include, I am proud to say, a large fraction of all the leading contributors to the revival in monetary studies that has been such a striking development in our science in the past two decades. At the Bureau, I was supported by Anna J. Schwartz, who brought an economic historian’s skill, and an incredible capacity for painstaking attention to detail, to supplement my theoretical propensities. Our work on monetary history and statistics has been enriched and supplemented by both the empirical studies and the theoretical developments that have grown out of the Chicago Workshop.  In the fall of 1950, I spent a quarter in Paris as a consultant to the U.S. governmental agency administering the Marshall Plan. My major assignment was to study the Schuman Plan, the precursor of the common market. This was the origin of my interest in floating exchange rates, since I concluded that a common market would inevitably flounder without floating exchange rates. My essay, *The Case for Flexible Exchange Rates*, was one product.  During the academic year 1953-54, I was a Fulbright Visiting Professor at Gonville & Caius College, Cambridge University. Because my liberal policy views were “extreme” by any Cambridge standards, I was acceptable to, and able greatly to profit from, both groups into which Cambridge economics was tragically and very deeply divided: D.H. Robertson and the “anti-Keynesians”; Joan Robinson, Richard Kahn and the Keynesian majority.  Beginning in the early 1960s, I was increasingly drawn into the public arena, serving in 1964 as an economic adviser to Senator Goldwater in his unsuccessful quest for the presidency, and, in 1968, as one of a committee of economic advisers during Richard Nixon’s successful quest. In 1966, I began to write a triweekly column on current affairs for Newsweek magazine, alternating with [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/index.html) and Henry Wallich. However, these public activities have remained a minor avocation – I have consistently refused offers of full-time positions in Washington. My primary interest continues to be my scientific work.  In 1977, I retire from active teaching at the University of Chicago, though retaining a link with the Department and its research activities. Thereafter, I shall continue to spend spring and summer months at our second home in Vermont, where I have ready access to the library at Dartmouth College – and autumn and winter months as a Senior Research Fellow at the Hoover lnstitution of Stanford University.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  Copyright © The Nobel Foundation 1976 **Addendum, May 2005** In 1977, when I reached the age of 65, I retired from teaching at the University of Chicago. At the invitation of Glenn Campbell, Director of the Hoover Institution at Stanford University, I shifted my scholarly work to Hoover where I remain a Senior Research Fellow. We moved to San Francisco, purchasing an apartment in a high-rise apartment building in which we still reside. The transition of my scholarly activities from Chicago to California was greatly eased by the willingness of Gloria Valentine, my assistant at Chicago, to accompany us west. She remains my indispensable assistant.  Hoover has provided excellent facilities for scholarly work. It enabled me to remain productive and an active member of a lively scholarly community.  Initially we continued to spend spring and summer quarters at Capitaf, our second home in Vermont. However, we soon came to appreciate the inconvenience of maintaining homes a continent apart and began to look in California for a replacement for Capitaf. In 1979, we purchased a house on the ocean in Sea Ranch, a lovely community 110 miles north of San Francisco. In 1981, we disposed of Capitaf and began to spend about half the year at Sea Ranch at intervals of a week or so, spread throughout the year, rather than in one solid block. It proved a fine locale for scholarly work. The Internet plus an assistant at Hoover more than made up for the absence of a library near at hand.  After more than two wonderful decades at Sea Ranch, we sold our house to simplify our lives. We now have one home, our apartment in San Francisco.  To return to the 1970s, not long after we arrived in California, Bob Chitester persuaded us to join him in producing a major television program presenting my economic and social philosophy. The resulting effort, spread over three years, proved the most exciting adventure of our lives. The end result was *Free to Choose*, ten one-hour programs, each consisting of a half-hour documentary and a half-hour discussion. The first of the ten programs appeared on PBS (Public Broadcasting System) in January 1980. Since then, the series has been shown in many foreign countries.  When we agreed to undertake the project, little did Rose and I realize what was involved in producing a major TV series. As a first step, I gave a series of fifteen lectures over a period of nine months at a wide variety of locations. The lectures and question-and-answer sessions were all videotaped to provide the producers with a basis for planning the programs.  The filming began in March 1978 and continued for the next eight months at locations in the United States and around the world, including Hong Kong, Japan, India, Greece, Germany, and the United Kingdom – in the process generating more than six miles of video and audiotape.  Three months after the end of filming, we returned to London to view the documentaries that Michael Latham, our wonderful producer, and his associates had created from that tape and to dub the voice-overs. Another six months passed before we gathered again in Chicago where we filmed the discussion sessions – one of the most stressful weeks I have ever experienced.  One distinguishing feature of the series was that there was no written script. I talked extemporaneously from notes. When we returned to Capitaf from London with the transcripts of the final documentaries, we set to work to convert them to a book to appear simultaneously with the TV program. The book, *Free to Choose* (Harcourt Brace Jovanovich, 1980) was the bestseller nonfiction book of 1980 and continues to sell well. It has been translated into more than fourteen foreign languages.  As Rose wrote in our memoirs, “As we look back at the events chronicled in this chapter, it all seems like something of a fairy tale. Who would have dreamed that after retiring from teaching, Milton would be able to preach the doctrine of human freedom to many millions of people in countries around the globe through television, millions more through our book based on the television program, and countless others through videocassettes” (p. 503).  *Monetary Trends in the United States and the United Kingdom*, published in 1982, was the final major product of a collaboration with Anna J. Schwartz under the auspices of the National Bureau of Economic Research that lasted more than three decades. *Money Mischief* (Harcourt Brace Jovanovich, 1992) collects assorted pieces of monetary history, some of which I had published elsewhere, some of which appear first in this book.  I have continued to be active in public policy since 1977. I continued my tri-weekly column in *Newsweek* until it was terminated in 1983. Since then, I have published numerous op-eds in major newspapers. I served as an unofficial adviser to Ronald Reagan during his candidacy for the presidency in 1980, and as a member of the President’s Economic Policy Advisory Board during his presidency. In 1988, President Reagan awarded me the Presidential Medal of Freedom and in the same year I was awarded the National Medal of Science.  We have traveled extensively since 1977, including a trip through Eastern Europe in 1990, where we filmed a documentary on former Soviet satellites. The documentary was included in a shortened reissue of *Free to Choose*.  Perhaps the most notable foreign travel consisted of three trips to China: one in 1980 when I gave a series of lectures under the auspices of the Chinese government; one in 1988 when I attended a conference in Shanghai on Chinese economic development and had a fascinating session in Beijing with Zhao Ziyang, at the time, the General Secretary of the Communist Party, deposed a few months later for his unwillingness to approve the use of force on Tiananmen Square; and one in 1993 when I traveled with a group of Chinese friends from Hong Kong throughout the country. The three visits covered a period of revolutionary economic growth and development, the first stage of a shift from an authoritarian, centrally planned economy to a largely free market economy.  Ever since the 1950s, Rose and I have been interested in the promotion of parental choice in schooling through the use of vouchers. Finally, in 1996, when it became clear that our personal involvement would have to be limited, we established a foundation, The Milton and Rose D. Friedman Foundation devoted to promoting parental choice in schooling. We were fortunate in being able to persuade Gordon St. Angelo to serve as president. He has done an outstanding job. Progress toward our objective of universal vouchers has been distressingly slow, but there has been progress. The pace of progress shows every sign of speeding up, and our foundation has made a significant contribution to that progress.  In 1998, the University of Chicago Press published our memoirs, Milton and Rose D. Friedman, *Two Lucky People*.  *Milton Friedman died on November 16, 2006.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0884 |
| Interview |  |
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| ID | 0885 |
| Biographical | I was born in Petersburg (Leningrad) on 19th January 1912. My father, Vitalij Kantorovich, died in 1922 and it was my mother, Paulina (Saks), who brought me up. Some of the first events of my childhood were the February and the October Revolutions of 1917, and a one-year trip to Byelorussia during the Civil War.  My first interest in sciences and the first displays of self-dependent thinking manifested themselves about 1920. On entering the Mathematical Department of the Leningrad University in 1926, I was mainly interested in sciences (but also in political economy and modern history, thanks to the most vivid lectures of academician E. Tarle).  At the University, I attended lectures and worked in seminars of V.I. Smirnov, G.M. Fichtengolz, B.N. Delaunay; my University friends were I.P. Natanson, S.L. Sobolev, S.G. Michlin, D.K. and V.N. Faddeevs.  My scientific activities started in my second university year covering the rather more abstract fields of mathematics. I think my most significant research in those days was that connected with analytical operations on sets and on projective sets (1929-30) where I solved some N.N. Lusin problems. I reported these results to the First All-Union Mathematical Congress in Kharkov (1930).  My participation in the work of the Congress was an important episode in my life; here I met such outstanding Soviet mathematicians as S.N. Bernstein, P.S. Alexandrov, A.N. Kolmogorov, A.O. Gelfond, *et al*, and some foreign guests, among whom were J. Hadamard, P. Montel, W. Blaschke.  The Petersburg mathematical school combined theoretical and applied research. On graduating from the university in 1930, simultaneously with my teaching activities at the higher school educational institutions, I started my research in applied problems. The ever expanding industrialization of the country created the appropriate atmosphere for such developments. It was precisely at that time such works of mine, *A New Method of Approximate Conformal Mapping ,* and *The New Variational Method* were published. This research was completed in *Approximate Methods of Higher Analysis*, a book that I wrote with V.I. Krylov (1936). By that time I was a full professor confirmed in this rank in 1934, and in 1935, when the system of academic degrees was restored in USSR, I received my doctoral degree. At that time I worked at the Leningrad University and in the Institute of Industrial Construction Engineering.  The Thirties was a time of intensive development of functional analysis which became one of the fundamental parts of modern mathematics.  My own efforts in this field were concentrated mainly in a new direction. It was the systematical study of functional spaces with an ordering defined for some of pairs of elements. This theory of partially-ordered spaces turned out to be very fruitful and was being developed at approximately the same time in the USA, Japan and the Netherlands. On this subject I contacted J. von Neumann, G. Birkhoff, A.W. Tucker, M. Frechet and other mathematicians whom I met at the Moscow Topological Congress (1935). One of my memoires on functional equations was published as a result of the invitation extended to me by T. Carleman in *Acta Mathematica*. *Functional Analysis in Semiordered Spaces*, the first complete book of our contributions in this field, was published in 1950 by my colleagues, B.Z. Vulikh and A.G. Pinsker, and myself.  In those days, my theoretical and applied research had nothing in common. But later, especially in the postwar period, I succeeded in linking them and showing broad possibilities for using the ideas of functional analysis in Numerical Mathematics. This I proved in my paper, the very title of which, *Functional Analysis and Applied Mathematics*, seemed, at that time, paradoxical. In 1949, the work was awarded the State Prize and later was included in the book, *Functional Analysis in Normed Spaces*, written with G.P. Akilov (1959) .  The Thirties was also important for me as I began my first economics. The very starting point was rather accidental. In 1938, as professor of the university, I acted as a consultant for the Laboratory of the Plywood Trust in a very special extreme problem. Economically, it was a problem of distributing some initial raw materials in order to maximize equipment productivity under certain restrictions. Mathematically, it was a problem of maximizing a linear function on a convex polytope. The well-known general recommendation of calculus to compare the function values in the polytope vertices lost its force since the vertices number was enormous even in very simple problems.  But this accidental problem turned out to be very typical. I found many different economic problems with the same mathematical form: work distribution for equipment, the best use of sowing area, rational material cutting, use of complex resources, distribution of transport flows.[\*](https://www.nobelprize.org/prizes/economic-sciences/1975/kantorovich/biographical/#not) This was reason enough to find an efficient method of solving the problem. The method was found under influence of ideas of functional analysis as I named the “method of resolving multipliers”.  In 1939, the Leningrad University Press printed my booklet called *The Mathematical Method of Production Planning and Organization* which was devoted to the formulation of the basic economic problems, their mathematical form, a sketch of the solution method, and the first discussion of its economic sense. In essence, it contained the main ideas of the theories and algorithms of linear programming. The work remained unknown for many years to Western scholars. Later, [Tjalling Koopmans](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1975/index.html), George Dantzing, *et al*, found these results and, moreover, in their own way. But their contributions remained unknown to me until the middle of the 50s.  I recognized the broad horizons offered by this work at an early stage. It could be carried forward in three directions:  1) The further development of methods of solving these extremal problems and their generalization; their application to separate classes of problems;  2) A mathematical generalization of these problems such as, non-linear problems, problems in functional spaces, the application of these methods to extremal problems of mathematics, mechanics and technical sciences;  3) The spreading of the method of description and analysis from separate economic problems to general economic systems with their application to planning problems on the level of an industry, a region, the whole national economy as well as the analysis of the structure of economic indices.  Some activity took place in the first two directions (the results were published partly immediately, partly after the war), but the third one lured me most. I hope that the reasons were clarified enough in my Nobel lecture.  The studies were interrupted by the war. During the war, I worked as Professor of the Higher School for Naval Engineers. But even then I found time to continue my deliberations in the realm of economics. It was then that I wrote the first version of my book. Having returned to Leningrad in 1944, I worked at the University and at the Mathematical Institute of the USSR Academy of Sciences, heading the Department of Approximate Methods. At that time, I became interested in computation problems, with some results in the automation of programming and in computer construction.  My economics studies progressed as well. I particularly wish to mention the work done in 1948-1950 at the Leningrad Carriage-Building Works by geometrist V.A. Zalgaller under my guidance. Here the optimal use of steel sheets was calculated by linear programming methods and saved material. Our book of 1951 summarized our experience and gave a systematic explanation of our algorithms including the combination of linear programming with the idea of dynamic programming (independently of R. Bellman).  In the middle of the 50s, the interest in the improvement of economic control in the USSR increased significantly, and conditions for studies in the use of mathematical methods and computers for general problems of economics and planning became more favourable. At that time, I made a series of reports and publications and prepared the above-mentioned book for publication. It appeared in 1959 under the title, *The Best Use of Economic Resources*, and contained a broad exposition of the optimal approach to such central problems of economics as planning, pricing, rent valuations, stock efficiency, “hozraschet” problems and decentralization of decisions. Precisely at that time, I contacted foreign scholars in this field. As a particular result, thanks to the initiative of Tjalling Koopmans, my 1939 booklet was published in *Management Science*, and, somewhat later, the 1959 book was translated as well.  Some of the Soviet economists met the new methods guardedly. Together with the book, I must mention the special Conference on Mathematical Methods in Economics and Planning held by the Academy of Science. The participants of the conference were some prominent Soviet mathematicians and economists. The conference approved the new scientific direction. But this time we had obtained some positive experience of its applications.  The field attracted a number of young talented scientists, and the preparation of such hybrid specialists (mathematician-economist) began in Leningrad, Moscow, and some other cities. It is worth noting that in the newly-organized Siberian Branch of the Academy of Sciences, conditions for new scientific directions were especially favourable. A special laboratory on the application of mathematics in economics headed by Nemchinov V.S. and me was created. Its main body belonged to the Leningrad and Moscow schools. In Akademgorodok it was integrated into Institute of Mathernatics as a department.  I was elected Corresponding-Member of the Academy in 1958 and came to Novosibirsk in 1960. Out of my group in Novosibirsk, a number of talented mathematicians and economists emerged.  In spite of continual discussions and some critique, the scientific direction gained recognition more and more by both the scientific community and governmental bodies. The token of this recognition was the Lenin Prize which I was awarded in 1965.  Now I head the Research Laboratory at the Institute of National Economy Control, Moscow, where high-ranking executives are introduced to new methods of control and management. I act as consultant to various governmental bodies.  I was married in 1938. My wife, Natalie, is a physician. We have two adult children (d. and s.), both working in mathematical economy.  \* A. Tolstoy had stated this problem before me (1930). He gave an approximate method of its solution. Later, the same problem was stated by F. Hitchcock.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  *Leonid Kantorovich died on April 7, 1986.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0885 |
| Interview |  |
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| ID | 0886 |
| Biographical | I was born in 1910 in ‘s Graveland, the Netherlands, the third son of Sjoerd Koopmans and Wijtske van der Zee. Both my parents had been trained as schoolteachers and my father was principal of the (Protestant) “School with the Bible”. Our house was squeezed between the two sections of that school. The row of these three buildings was, as almost all houses in the village, sandwiched between one long street and a parallel, straight and narrow canal, marking one of the village’s boundaries. Across the street were large wooded estates, each with meadows and a large mansion. The occupants of the mansions kept aloof from the life of the village except for the employment of coachmen, gardeners, servants and contractors. Across the canal was a path for horses and an unpaved, more sparsely-settled road belonging to another village. Small freight boats traveling between Amsterdam and Hilversum were not allowed to use their motors in ‘s Graveland because the buildings abutting on the canal might be damaged by the waves so generated. Instead, men called “jagers” specialized in making their horses pull the boats for the length of the village.  Every weekday morning at nine, our living quarters and the narrow strip of garden in the back were engulfed by the sound of three different hymns sung dutifully, simultaneously, but, independently, in true Charles Ives fashion, by the schoolchildren on both sides.  The knowledge imparted by schools and the talent for its acquisition ranked high among the values of the family. Both parents worked to the limits of their strength to provide education fitting the talents of their children. The oldest son, Jan, became a minister of the Dutch Reformed Church and an influential leader in a Protestant student organization. During World War II, he showed great courage in both covertly and openly resisting encroachment on matters of Christian conscience by the racist policies of the occupying power. He died prematurely near the end of the war, a victim of a stray bullet from a nearby execution of hostages. Hendrik, the second son, became a chemical engineer who, in the last 15 to 20 years as a staff member of a semi-official consulting bureau, contributed to industrial planning in the Netherlands, in the former Dutch West Indies and in other developing nations.  When the oldest son was already receiving a university education while the second sought training in engineering, the family, in general, and I, in particular, were fortunate indeed when, at the age of 14, I was awarded a study stipend by the St. Geertruidsleen of Wijmbritseradeel in the Dutch province of Friesland from where my parents originated. This fund had been established before the Reformation to send descendants of the family of the donor to the University. But if there were no children of that family of the appropriate age, then the moneys available could be awarded to another promising person. This is what happened in my case. The St. Geertruidsleen has supported my studies up to my 26th birthday. I shall be forever grateful for its support which gave me financial, and therefore, intellectual independence and the opportunity to explore various fields of knowledge, before settling down to the particular combination of fields, to which my efforts have been devoted since.  I went to the University of Utrecht at age 17. In the first three years, my principal emphasis was on mathematics, in particular analysis and geometry, which were taught in a precise, but traditional style. Much of my time in Utrecht went directly into the studies I had undertaken. However, some of the long vacations in ‘s Graveland were devoted to broader reading. Ernst Mach, *Geschichte der Wärmelehre*, and various expositions of the theory of relativity, taught me how a whole field of science can at various junctures be on the wrong track, and how entirely new concepts may then be needed to make further progress.  The general intellectual climate in Holland required of the serious young man – or so it seemed to me – that he work himself through to a “Weltanschauung”, a consistent view of the world. After a summer of reading [Albert Schweitzer](https://www.nobelprize.org/nobel_prizes/peace/laureates/1952/index.html)‘s *Geschichte der Leben Jesu Forschung*, I greatly upset my parents by declining to confess to the Protestant faith and become a member of the church. Then followed reading in psychology and psychiatry, and conversations with two fine people in these fields. At one time I even considered switching over to psychiatry – for which I now know I would not have been suited.  Instead, in 1930, I switched my emphasis to theoretical physics – a timid compromise between my desire for a subject matter closer to real life and the obvious argument in favor of a field in which my mathematical training could be put to use. My teacher and shining example of what a scientist should be like was Hans Kramers, after the death of Ehrenfest, the leading theorist in Holland in that period, and a very humane and inspiring person with a gentle wit. His attitude and style in the application of mathematics to a substantive field have exerted a pervasive influence on all my later work. Below, I cite my one publication in quantum mechanics so that I can add here that Kramers’s generosity and my inexperience combined to prevent his being listed as co-author of that paper. He should have been, because, although the main proposition was my own idea, Kramers, besides guiding the writing, also supplied the proof!  The early thirties brought what liberal economists called the great depression and Marxist economists described as the great crisis of capitalism. It dawned on me that the economic world order was unreliable, unstable, and, most of all, iniquitous. I sought intellectual contacts and friendship with a group of socialist students and also with a small handful of communist-oriented students and unemployed workers. Thus, Karl Marx’s *Das Kapital*, Vol. I, came to be the first book in economics that I studied. While never accepting the labor theory of value, I was stirred by the famous chapter on the state of the English workers during the Industrial Revolution.  Later, in the Amsterdam period, I also had the good luck to be introduced by Kramers to his friend Jan Romein, a fine historian with a marxist outlook and, later, Professor of History at the City University of Amsterdam. I was taken into the friendship of this kind and reflective scholar and his wife, Annie. From Romein, I received a sense of the many forms of historical and political experience of mankind, and of the fragility of the more democratic forms of social and political organization.  From my explorations of Marxist thinking in my student years, I have retained a lifelong interest in the prior formulation of that fundamental part of economic theory that does not require specifying the institutional form of society to be used as a framework for the description and comparison of different economic systems.  Still in Utrecht, a physicist friend had mentioned to me that a new field called mathematical economics was being developed, and that [Jan Tinbergen](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1969/index.html), a former student of Ehrenfest, was the leader in this field in the Netherlands. This information opened the way for me to apply my mathematical training to a subject still closer to human concerns. Probably in mid-1933, Tinbergen received me cordially and included me among the small number, I moved to Amsterdam where Tinbergen was then lecturing once a week. In the first half of that year, I had the privilege of almost weekly private tutoring from him over lunch after his lecture. I have been deeply impressed by his selflessness, his abiding concern for economic well-being and greater equality among all of mankind, his unerring priority at any time for problems then most crucial to these concerns, his ingenuity in economic modeling and his sense of realism and wide empirical knowledge of economic behavior relations.  On Tinbergen’s advice, I now read Cassel, and, with a group of friends, Wicksell. I also studied econometric and statistical literature. For my doctoral dissertation, I chose, staying close to my training, a subject in mathematical statistics aimed at application in econometrics. In the fall of 1935, I spent four months in Oslo with [Ragnar Frisch](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1969/index.html), this giant of mathematical economics whose finest work tended to remain hidden for long periods in mimeographed lecture notes. At his request, I gave some lectures on the new ideas in statistics then being developed in England by R.A. Fisher, J. Neyman and others. However, I did not succeed in persuading him that probability models were useful in assessing the significance and accuracy of econometric estimates. I, in turn, departed impressed, but not persuaded by his econometric approach either.  Since my dissertation was to be presented to the Faculty of Mathematical and Physical Sciences, Kramers, who had moved to the University of Leiden, agreed to be the thesis supervisor, consulting with Tinbergen about the economic aspects. The degree was granted in November 1936 by the University of Leiden.  In Amsterdam I also met my future wife, Truus Wanningen, among a small group of students of economics whom I tutored in mathematics. Among our shared interests were economics, music, nature, love, and independence from the views and lifestyles of our parents. We married in October 1936.  For the two academic years 1936-1938, Tinbergen was called to the Financial Section of the League of Nations for what became his pioneering work on a model of business cycles in the United States. I was then asked to take over his lectures at the School of Economics in Rotterdam in those years, and, later, to succeed him in 1938 in Geneva to construct a similar model for the United Kingdom. In the two years in Geneva, I learned much from [James Meade](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1977/index.html) about the economics of welfare and the problem of optimum population. As to the project I was appointed for, it gradually became clear to me that I lacked the flair and the breadth of empirical knowledge required for the task. The project was discontinued at the outbreak of World War II.  When the war spread to Western Europe, I managed to move with my wife and our six-week old daughter to the United States in June 1940. I shall always remember the essential help given to us by Samuel Wilks of Princeton University and Mrs. Wilks in that difficult time.  The scientific fall-out from my work as a statistician for the British Merchant Shipping Mission in Washington during the war has already been described by Professor Ragnar Bentzel in his remarks at the Nobel award ceremony. My direct assignment was to help fit information about losses, deliveries from new construction, and employment of British-controlled and U.S-controlled ships into a unified statement. Even in this humble role, I learned a great deal about the difficulties of organizing a large-scale effort under dual control – or rather in this case, four-way control, military and civilian cutting across U.S. and U.K. controls. I did my study of optimal routing and the associated shadow costs of transportation on the various routes, expressed in ship days, in August 1942 , when an impending redrawing of the lines of administrative control, left me temporarily without urgent duties. My memorandum, cited below, was well received in a meeting of the Combined Shipping Adjustment Board (that I did not attend) as an explanation of the “paradoxes of shipping” which were always difficult to explain to higher authority. However, I have no knowledge of any systematic use of my ideas in the combined U.K.-U.S. shipping problems thereafter.  In mid-1944 my work at the Merchant Shipping Mission fizzled out due to another reshuffling of responsibilities, this time between the Ministry of War Transport in London and its representation in Washington. I corresponded with Jacob Marschak with whom I had had many discussions in Oxford in 1939 and in New York in 1940-41. He invited me to join the staff of the Cowles Commission for Research in Economics, affiliated with the University of Chicago. This was the beginning of a long period of close interaction, collaboration, and personal friendship with Marschak, a gentle, wise, and witty scholar who sees through pretence and timidity alike. In Chicago, Marschak created a rare kind of research environment, by shrewd selection of staff members and by a truly open style of work and discussion. Over an extended period, the focus was the construction of econometric models of the kind pioneered by Tinbergen. Since this work and the names of the participating scholars have become well-known, I shall only mention two other intellectual sources. The idea that the approximate simultaneity in the determination of different economic variables should affect the method of estimation of behavior parameters was, by my knowledge, the unique contribution of [Trygve Haavelmo](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1989/index.html). The related work in Chicago on identifiability of economic relations is the inferential counterpart of Frisch’s concept of “autonomy” of economic relations set out in a memorandum prepared in 1938 for a discussion of Tinbergen’s work for the League of Nations. As far as I know, this memorandum has not been published, but it was known to both Haavelmo and myself at that time.  My work on the transportation model broadened out into the study of activity analysis at the Cowles Commission as a result of a brief but important conversation with George Dantzig, probably in early 1947. It was followed by regular contacts and discussions extending over several years thereafter. Some of these discussions included Albert W. Tucker of Princeton who added greatly to my understanding of the mathematical structure of duality.  In 1948, I succeeded Marschak as Director of Research for a six-year period. In 1955, Mr. Alfred Cowles and other members of the Cowles family shifted their generous financial support to Yale University while, simultaneously, five members of the staff of the Cowles Commission, including myself, accepted appointments at Yale. A new Cowles Foundation for Research in Economics at Yale University was set up with [James Tobin](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1981/index.html) as Director. In most of my Yale period, my research, chiefly on optimum allocation over time, had more of a solitary character. But there was also another six-year stint as Director, 1961-1967, and a joint study with my Yale colleague, J. Michael Montias, on the description and comparison of economic systems while both of us spent the year 1968-1969 at the Center for Advanced Study in the Behavioral Sciences at Stanford, California.  My work in activity analysis and in optimal economic growth has been described in more detail by Professor Bentzel and in my Nobel lecture.  As a result of service on a committee for the National Academy of Sciences of the U.S., I have, in recent years, become interested in the application of the techniques of optimization over time in the field of the supply of energy. In part, as a result of this interest, I made a one-year visit to the International Institute for Applied Systems Analysis in Laxenburg, Austria. At IIASA I learned to see energy problems through the eyes of several different professions. I also served as leader of the Methodology Project at IIASA in the second half of 1974, succeeding George Dantzig in that function.  Our family has three splendid and, now, adult children. Their choices of professions and spouses have leaned toward the life sciences, thus enlarging the range of discussions at family reunions. Even before, but particularly after they left home, my wife and I have travelled for professional purposes to many parts of the world, with extended stays in Western Europe (1950), Italy (’65), The Soviet Union (’65, ’70), India, New Zealand and Australia (all ’69), Poland (’72) and Austria (’74). My wife has also given me and others great support, as an economic bibliographer, as a general critic of ideas and actions, and as an advisor on important decisions, including those I ultimately regard as my own. She has helped in the writing of these notes.  My wife, Truu, makes beautiful photographs and weird etchings and drawings. I have at various times written music, which has come out best when for voice, perhaps because then, the poet has already supplied the form.   |  | | --- | | Published works | | “Ueber die Zuordaung von Wellenfunktionen und Eigenwerten zu den einzelnen Elektronen eines Atoms, “*Physica* *1*, no. 2, 1934, pp. 104-113. | | “Exchange Ratios between Cargoes on Various Routes (Non-Refrigerated Dry Cargoes).” Memorandum for the Combined Shipping Adjustment Board, Washington, D.C., 1942. Publ. in *Scientific Papers of Tjalling C. Koopmans*, Springer Verlag, 1970. pp. 77-86. | | “On the Description and Comparison of Economic Systems,” (with J. Michael Montias) in *Comparison of Economic Systems*, A. Eckstein, ed., Berkeley, Univ. of California Press, 1971, pp. 27-78. | | References to other work are given at the end of the Nobel lecture and in *Scientific Papers of T.C.K.* |   From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  *Tjalling C. Koopmans died on February 26, 1985*. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0886 |
| Interview |  |
|  |  |
| ID | 0887 |
| Biographical | Gunnar Myrdal was born in Gustaf’s parish, Sweden, on December 6, 1898\*. He graduated from the Law School of Stockholm University in 1923 and began practicing law while continuing his studies at the university. He received his *juris doctor* degree in economics in 1927 and was appointed docent in political economy. From 1925 to 1929 he studied for periods in Germany and Britain, followed by his first trip to the United States in 1929-1930 as a Rockefeller Fellow. During this period, he also published his first books, including *The Political Element in the Development of Economic Theory*. Returning to Europe, he first served for one year as Associate Professor in the Post Graduate Institute of International Studies, Geneva, Switzerland. In 1933 he was appointed to the Lars Hierta Chair of Political Economy and Public Finance at the University of Stockholm as the successor of Gustav Cassel. In addition to his teaching activities, Professor Myrdal was active in Swedish politics and was elected to the Senate in 1934 as member of the Social Democratic Party. In 1938, the Carnegie Corporation of New York commissioned him to direct a study of the American Negro problem. The material which he collected and interpreted was published in 1944 as *An American Dilemma: The Negro Problem and Modern Democracy*. Having come home to Sweden in 1942, he was re-elected to the Swedish Senate, served as member of the Board of the Bank of Sweden, and was Chairman of the Post-War Planning Commission. From 1945-1947, he was Sweden’s Minister of Commerce, a position which he left to accept an appointment as Executive Secretary of the United Nations Economic Commission for Europe. In 1957, he left this post to direct a comprehensive study of economic trends and policies in South Asian countries for the Twentieth Century Fund, which resulted in *Asian Drama: An Inquiry into the Poverty of Nations and The Challenge of World Poverty. A World Anti-Poverty Program in Outline*. From 1961, he was back in Sweden and was appointed Professor of International Economics at the Stockholm University. He founded this same year the Institute for International Economic Studies at the university and is still a member of its Directorate. He was Chairman of the Board of the Stockholm International Peace Research Institute (SIPRI) and remains a board member. He was also Chairman of the Board of the Latin American Institute in Stockholm. During the academic year 1973-1974, he was visiting Research Fellow at the Center for the Study of Democratic Institutions at Santa Barbara, California, and during 1974-1975, Distinguished Visiting Professor at New York City University. Professor Myrdal is recipient of more than thirty honorary degrees beginning with Harvard University in 1938, where he gave the Godkin Lectures that year. He has received many prizes, the last one being the Malinowski Award by the Society of Applied Anthropology. He is member of the British Academy, American Academy of Arts and Sciences, [Vetenskapsakademien [the Royal Swedish Academy of Sciences]](http://www.kva.se/), Fellow of the Econometric Society, honorary member of American Economic Association.  Gunnar Myrdal is married to the former [Alva Reimer](https://www.nobelprize.org/nobel_prizes/peace/laureates/1982/index.html) who held high posts in the United Nations and UNESCO, was the Swedish Ambassador to India and became Sweden’s Minister of Disarmament and of Church. They have three grown children, two daughters, Sissela and Kaj, and one son, Jan.  A complete bibliography of his scientific publications is presently under preparation by the Royal Library of Stockholm.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  \* Gunnar Myrdal’s parents lived in Gustaf’s parish, but Gunnar was born in Skattungbyn.  *Gunnar Myrdal died on May 17, 1987.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0887 |
| Interview |  |
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| ID | 0888 |
| Biographical | Born: May 8, 1899, Vienna, Austria (son of Dr. August von Hayek, Professor of Botany at the University of Vienna and Felicitas née Juraschek)   |  |  | | --- | --- | | Education | | | 1918-1921 | Studies at University of Vienna | | 1921 | Dr. jur., University of Vienna | | 1923 | Dr. rer. pol, University of Vienna | | March 1923 –  June 1924 | Postgraduate work, New York University | |  | | | Academic Appointments | | | 1927-1931 | Director, Österreichisches Institut für Konjunkturforschung (Austrian Institute for Trade Cycle Research) | | 1931-1950 | Tooke Professor of Economic Science and Statistics, University of London (London School of Economics and Political Science) | | 1950-1962 | Professor of Social and Moral Science, University of Chicago  (Committee on Social Thought) | | 1962-1968 | Professor der Volkwirtschaftslehre, Albert- Ludwigs-Universität, Freiburg im Breisgau |   At various dates, Visiting Professor at the Universities of Stanford, Arkansas, Virginia, California (Los Angeles), Cape Town and Salzburg   |  |  | | --- | --- | | Honors and Fellowships | | | 1944 | Fellow, British Academy | | 1970 | Korrespondierendes Mitglied der Österreichischen Akademie der Wissenschaften | | 1972 | Honorary Fellow, London School of Economics | |  | | | Honorary Degrees | | | 1964 | Dr. jur. h.c., Rikkyo University, Tokyo | | 1971 | Ehrensenator der Universität Wien | | 1974 | Dr. jur. h.c., Universität Salzburg | |  | | | Others | | | 1917-1918 | War service, Austro-Hungarian Army (Italian Front) | | 1921-1926 | Legal Consultant, Austrian government, for carrying out provisions of Peace Treaty |  |  | | --- | |  | | Books Published | | *Geldtheorie und Konjunkturtheorie*, Wien, 1929, also in English as *Monetary Theory and the Trade Cycle*, London, 1933, as well as in Spanish and Japanese translations. | | *Prices and Production*, London, 1931, also in German, Chinese, French and Japanese translations. | | *Monetary Nationalism and International Stability*, London, 1937. | | *Profits, Interest, and Investment,* London 1939. | | *The Pure Theory of Capital*, London, 1940, also in Japanese and Spanish translations. | | *The Road to Serfdom*, London and Chicago, 1944, also in Chinese, Danish, Dutch, French, German, Italian, Japanese, Norwegian, Portuguese, Spanish and Swedish translations. | | *Individualism and Economic Order*, London and Chicago, 1949, also in German and an abridged Norwegian translation. | | *John Stuart Mill and Harriet Taylor*, London and Chicago, 1951. | | *The Counter-Revolution of Science*, Chicag,o 1952, also in German, Italian and an abridged French translation. | | *The Sensory Order*, London and Chicago, 1952. | | *The Constitution of Liberty*, London and Chicago, 1960, also in Spanish, German and Italian translations. | | *Studies in Philosophy, Politics, and Economics*, London and Chicago, 1967. | | *Freiburger Studien*, Tübingen 1969. | | *Law, Legislation and Liberty, vol. I, Rules and Order*, London and Chicago, 1973. | |  | | Edited by F. A. Hayek | | *Beiträge zur Geldtheorie*, Wien, 1931. | | *Collectivist Economic Planning*, London, 1935, also in French and Italian translations. | | *Capitalism and the Historians*, London and Chicago, 1954, also in Italian translation. |   From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  *Friedrich August von Hayek died on 23 March 1992*. |
| Autobiographical |  |
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| Telephone  interview | 0888 |
| Interview |  |
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| ID | 0889 |
| Biographical | I was born August 5, 1906, and spent my childhood and youth in St. Petersburg (now Leningrad) where my father was a professor of economics.[\*](https://www.nobelprize.org/prizes/economic-sciences/1973/leontief/biographical/#not) Among my early indelible memories are: the country plunged into deep mourning the day of Leo Tolstoy’s death; stray bullets whistling by during the first days of the February Revolution; Lenin addressing a mass meeting from a high tribune in front of the Winter Palace.  Entered the University of Leningrad in 1921. After studying philosophy, sociology and finally economics, I received the degree of Learned Economist in 1925.  Continued my studies at the University of Berlin with Werner Sombart and Ladislaus Bortiewicz, and received the Ph.D. degree, having submitted a dissertation on the theoretical subject “Wirtschaft als Kreislauf.” As member of the staff of the Institute for World Economics at the University of Kiel from 1927 to 1930, engaged in research on derivation of statistical demand and supply curves. This academic work was interrupted in 1929 by a twelve-month stay in China as advisor to the Ministry of Railroads. Moved to the National Bureau of Economic Research in New York in 1931 and to the Department of Economics at Harvard University in 1932. Became Professor of Economics in 1946; organized the Harvard Economic Research Project in 1948, and served as its Director until 1973; have been Chairman of the Harvard Society of Fellows since 1965.  Married Estelle Marks, who is a poet, in 1932. A daughter, Svetlana Alpers, is now Professor of the History of Arts at the University of California, Berkeley.  Having come to the conclusion that so-called partial analysis cannot provide a sufficiently broad basis for fundamental understanding of the structure and operation of economic systems, I set out in 1931 to formulate a general equilibrium theory capable of empirical implementation. Received a research grant for compilation of the first input-output tables of the American economy (for the years 1919 and 1929) in 1932. Began to make use of a large scale mechanical computing machine in 1935 and Mark I (the first large-scale electronic computer) in 1943.  After publication of *Structure of the American Economy, 1919-1929* in 1941, I continued working on the development of the input-output theory and of its various applications. The first international conference on inter-industrial relations was held at Dreibergen, Holland, in 1950; the sixth will be in Vienna in 1974.  In recent years I have been centering my attention on analysis of environmental disruption and economic growth, while maintaining, at the same time, active interest in wider problems of scientific methodology and broader issues of social and economic policies, and of evolutionary and revolutionary change.   |  | | --- | | Memberships | | American Economic Association (President, 1970) | | Econometric Society (President, 1954) | | American Philosophical Society | | American Academy of Arts and Sciences | | International Statistical Institute | | Honorary Member, Japan Economic Research Center, Tokyo | | Honorary Fellow, Royal Statistical Society, London | | Corresponding Fellow of the British Academy, 1970 | | Corresponding Member of the Institut de France, 1968. | |  | | Honorary Awards | | Order of the Cherubim, University of Pisa, 1953 | | Doctor *honoris causa*, University of Brussels, 1962 | | Doctor of the University, University of York, 1967 | | Officer of the French Legion d’Honneur, 1968 | | Bernhard-Harms Prize Economics, West Germany, 1970 | | Doctor *honoris causa*, University of Louvain, 1971 | | Doctor *honoris causa*, University of Paris I (Sorbonne), 1972. |   From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  \* Recent information sets his year of birth to 1905, in Munich.  *Wassily Leontief died on February 5, 1999.* |
| Autobiographical |  |
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| ID | 0890 |
| Biographical | I was born in 1904 at Warwick, England, where my father was a journalist on a local newspaper. I was educated at Clifton College (1917-22) and at Balliol College, Oxford (1922-26), an expensive education financed by mathematical scholarships. Thus, during my school days, and in my first year at Oxford, I was a mathematical specialist; to the mathematical training I received at Clifton, in particular, I owe a great debt. But I was not contented with mathematics; I had interests in literature and in history which I needed to satisfy. My move (in 1923) to “Philosophy, Politics and Economics”, the “new school” just being started at Oxford, was, however, not a success. I finished with a second-class degree, and no adequate qualification in any of the subjects I had studied.  Economists, in those days, were very scarce, so I did pick up a temporary lecturership at the London School of Economics and managed to get continued. I started as a labour economist, doing descriptive work on industrial relations, but, gradually, I moved over to the analytical side. Then I found that my mathematics, by that time almost forgotten, could be revived, and were sufficient to cope with what anyone (then) used in economics. By 1930, when the economics department at the London School got a new lease of life under Lionel Robbins, I had found my feet. “How wonderful it must have been in those days, when such things could be picked up with so little trouble”, my students have said to me since. They were picked up in discussion, with Robbins and [Friedrich von Hayek](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1974/index.html), with Roy Allen and Nicholas Kaldor, with Abba Lerner and with Richard Sayers – and with Ursula Webb, who, in 1935, became my wife.  By 1935, I had got so much that I needed to go away to put it together. Thus, when an opportunity arose for moving, to a university lecturership at Cambridge (and Fellowship of Gonville and Caius College), I took it. My years at Cambridge (1935-38) were mainly occupied in writing *Value and Capital* which was based on the work I had done in London, so I was not in a state to learn very much from association with Cambridge economists. From 1938 to 1946 I was Professor at the University of Manchester. It was there that I did my main work on welfare economics, with its application to social accounting. In 1946 I returned to Oxford, first as a research fellow of Nuffield College (1946-52), then as Drummond Professor of Political Economy (1952-65), and finally as a research fellow of All Souls College (1965-71).  During these latter years, I have made contributions to several branches of theoretical economics. I have written on money and on international trade, as well as on growth and fluctuations. I have also done some small pieces of applied economics, especially in relation to problems of “developing” countries, several of which I have visited in company with my wife, much of whose work has been in that field. Thus, in 1950, I was a member of a Revenue Allocation Commission in Nigeria, and in 1954, we both of us made an enquiry into the finances of Jamaica. I have been reluctant to pronounce on larger issues of practical economics since I am convinced that one should not pronounce unless one knows the facts; and to keep abreast of changing facts on a world, or even on a nation scale, is more than can be done by one whose main concern is with principles. A mere familiarity with statistics that have been prepared and digested by others is not sufficient.  We now live in the country (Porch House, Blockley, Gloucestershire) but spend a part of each week in Oxford, where we continue to do a little teaching.  I became a Fellow of the British Academy in 1942; a foreign member of the [Royal Swedish Academy](http://www.kva.se/) in 1948, of the Accademia dei Lincei, Italy, in 1952, and of the American Academy in 1958. I am an honorary fellow of Nuffield College, Oxford, since 1958 and of Gonville and Caius College, Cambridge, since 1971. I was President of the Royal Economic Society, 1960-62, and was knighted in 1964. I am an honorary doctor of several British Universities (Glasgow, Manchester, Leicester, East Anglia and Warwick) as well as of the Technical University of Lisbon. I was made (in 1971) an honorary Senator of the University of Vienna.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  *John R. Hicks died on May 20, 1989.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0890 |
| Interview |  |
|  |  |
| ID | 0891 |
| Biographical | I was born in the city of New York on August 23, 1921. My undergraduate education, at the City College in New York, was made possible only by the existence of that excellent free institution and the financial sacrifices of my parents. I was graduated in 1940 with a degree of Bachelor of Science in Social Science but a major in Mathematics, a paradoxical combination that was prognostic of my future interests. I entered Columbia University for graduate study and received an M.A. in Mathematics in June, 1941, but under the influence of the statistician-economist, Harold Hotelling, I changed to the Economics Department for subsequent graduate work.  My graduate study was interrupted, like that of many others, by World War II. From 1942-1946, I served as a weather officer in the United States Army Air Corps rising to the rank of Captain. My assignment was exclusively in the research field, and my first published paper, *On the Optimal Use of Winds for Flight Planning*, was the outgrowth of that work. The years 1946-1949 were spent partly as a graduate student at Columbia University, partly as a research associate of the Cowles Commission for Research in Economics at the University of Chicago, where I also had the rank of Assistant Professor of Economics in 1948-1949. The brilliant intellectual atmosphere of the Cowles Commission, with eager young econometricians and mathematically-inclined economists under the guidance of [Tjalling Koopmans](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1975/index.html) and Jacob Marschak, was a basic formative influence for me, as was also the summers of 1948 and subsequent years at the RAND Corporation in the heady days of emerging game theory and mathematical programming. My work on social choice and on Pareto efficiency dated from this period.  In 1949 I was appointed Acting Assistant Professor of Economics and Statistics at Stanford University and remained there until 1968, becoming eventually Professor of Economics, Statistics, and Operations Research. At various times during this period, I was a Social Science Research fellow, 1952, a Fellow of the Center for Advanced Study in the Behavioral Sciences, 1956-57, Economist on the staff of the United States Council of Economic Advisors, 1962, Executive Head of the Department of Economics at Stanford, 1953-56 and 1962-63, Fellow of Churchill College (Cambridge), 1963-64, and again in 1970, and Guest Professor, Institute for Advanced Studies, Vienna, in June, 1964, and again, 1971. In 1968, I accepted an appointment as Professor of Economics at Harvard University.  I received the John Bates Clark Medal of the American Economic Association, 1957, and I have been elected member of the National Academy of Sciences and the American Philosophical Society; also I am a Fellow of the American Academy of Arts and Sciences, the Econometric Society, the Institute of Mathematical Statistics, and the American Statistical Association. I received the honorary degrees of LL.D. from the University of Chicago, 1967, and the City University of New York, 1972, and that of Doctor of Social and Economic Sciences for the University of Vienna, 1971. With regard to professional societies, I was president of the Econometric Society in 1956 and The Institute of Management Sciences in 1963, and a President-elect of the American Economic Association for 1972.  I was married in 1947 to the former Selma Schweitzer and now have two sons, David Michael, age ten, and Andrew Seth, age 8.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  Copyright © The Nobel Foundation 1972 **Addendum, April 2005** To continue first with my formal career, I remained at Harvard University until 1979; I had been designated the James Bryant Conant University Professor in 1974. In 1979, I returned to Stanford University with the position of Joan Kenney Professor of Economics and Professor of Operations Research. I retired in 1991 and have been Professor Emeritus since.  There were several short-term appointments, including Fellow at Churchill College, University of Cambridge, in 1973 and 1986, part-time Professor at the European University Institute in 1986, Fulbright Professor at the University of Siena in 1995, and Visiting Fellow at All Souls College, University of Oxford, in 1996. I have also enjoyed a close relation with the Santa Fe Institute, being on their External Faculty since 1988 and also serving on the Science Board. I was elected to the Institute of Medicine (U.S.A.) and have chaired two of their study committees, most recently one on the economics of antimalarial drugs. I was also elected to the Pontifical Academy of Social Sciences. In 1986, the Institute for Management Sciences and Operations Research awarded me the von Neumann Prize. I served as President of several learned societies, including the International Economic Association.  My research, even before 1972, moved in directions beyond those cited for the Nobel Memorial Prize. Most of it, in one way or another, deals with information as an economic variable, both as to its production and as to its use. Two 1962 papers studied the efficiency with which the market encourages innovation and the implications of learning by doing for economic growth. In 1963 and later papers, I pointed out that the special market characteristics of medical care and medical insurance could be explained by reference to differences in information among the parties involved. Later themes included a specification of the demand for information and the implications of information as an economic input for returns to scale. Another area of study was the economics of racial discrimination.  *Kenneth J. Arrow died on 21 February 2017.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0891 |
| Interview |  |
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| ID | 0892 |
| Biographical | I was born in Russia in 1901, of Jewish parents, and came to the United States in 1922 to join my father who left Russia for the United States before World War I. My university studies began in Russia, and were completed at Columbia University (B.Sc. in 1923, M.A. in 1924, Ph.D. in 1926). It was at the graduate school at Columbia University that I first met Wesley C. Mitchell with whom I was associated for many years at the National Bureau of Economic Research, and to whom I owe a great intellectual debt.  After completion of graduate studies, I spent a year and a half as Research Fellow of the Social Science Research Council (1925-1926), in work that led to monograph (1) listed in the bibliography below. As a member of the staff of the National Bureau of Economic Research, from 1927 to the early 1960s, I worked mostly on national income and capital formation in the United States; and as Chairman of the Social Science Research Council Committee on Economic Growth (1949-1968), I worked primarily on comparative quantitative analysis of economic growth of nations. Other, largely research-oriented, activities, were: Associate Director of the Bureau of Planning and Statistics and Director of Research, Planning Committee, War Production Board, 1944-1946; Chairman of the Falk Project for Economic Research in Israel, 1953-1963; member of the Board of Trustees and honorary chairman, Maurice Falk Institute for Economic Research in Israel, 1963 to date; and Chairman, Social Science Research Council Committee on the Economy of China, 1961-1970.  As Professor of Economics and Statistics, I taught at the University of Pennsylvania, part-time, 1931-1936, and full-time, 1936-1954; as Professor of Political Economy, at the Johns Hopkins University, 1954-1960; and as Professor of Economics, Harvard University, 1960-1971.  Among the scientific societies of which I am a fellow or member are: American Economic Association (president-1954); American Statistical Association (president-1949); Economic History Association (honorary member); Econometric Society (fellow); International Statistical Institute (member); Royal Statistical Society of England (honorary fellow); American Philosophical Society (member); British Academy (corresponding fellow); [Royal Academy of Sweden](http://www.kva.se/) (member).   |  | | --- | | My major publications in the field of economic growth are: | | 1. *Secular Movements in Production and Prices*, Houghton-Mifflin, Boston and New York, 1930 | | 2. “Long-Term Changes in the National Income of the United States of America since 1870”, in *Income and Wealth of the United States: Trends and Structure*, International Association for Research in Income and Wealth, *Income and Wealth, Series II*, Bowes & Bowes, Cambridge (England), 1951 | | 3. “Quantitative Aspects of the Economic Growth of Nations”, ten long papers published either in, or as supplement to, *Economic Development and Cultural Change* (University of Chicago Press), no. I in October, 1956, no. X in January, 1967. | | 4. *Capital in the American Economy: Its Formation and Financing*, Princeton University Press for the National Bureau of Economic Research, Princeton, 1961 | | 5. *Modern Economic Growth: Rate, Structure, and Spread*, Yale University Press, New Haven, 1966 | | 6. *Economic Growth of Nations: Total Output and Production Structure*, Harvard University Press, Cambridge (USA), 1971 |   I live in Cambridge, Mass., with my wife Edith (Handler). Our son, Paul Kuznets, teaches economics at the University of Indiana; our daughter, Judith (Stein) is married to a professor of mathematics who teaches at the University of Rochester. We have four grandchildren.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  *Simon Kuznets died on July 8, 1985.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0892 |
| Interview |  |
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| ID | 0893 |
| Biographical | In this age of specialization, I sometimes think of myself as the last ‘generalist’ in economics,” wrote Paul Anthony Samuelson, Professor of Economics at the Massachusetts Institute of Technology, “with interests that range from mathematical economics down to current financial journalism. My real interests are research and teaching… ” His work in economic theory has been in modern welfare economics, linear programming, Keynesian economics, economic dynamics, international trade theory, logic choice and maximization. In terms of economic philosophy, Professor Samuelson calls himself “a ‘modern’ economist… in the right wing of the Democratic New Deal economists.”  He was born in Gary, Indiana, in 1915. He received the degree of Bachelor of Arts from Chicago University in 1935, and the degrees of Master of Arts in 1936, and Doctor of Philosophy in 1941 from Harvard University. He was a Social Science Research Council predoctoral fellow from 1935-1937, a member of the Society of Fellows, Harvard University, 1937-1940, and a Ford Foundation Research Fellow from 1958-1959. He received honorary Doctor of Laws degrees from Chicago University and Oberlin College in 1961, and from Indiana University and East Anglia University (Eng.) in 1966.  He was awarded the David A. Wells Prize in 1941 by Harvard University, and the John Bates Clark Medal by the American Economic Association in 1947, as the living economist under forty “who has made the most distinguished contribution to the main body of economic thought and knowledge.”  Even while a graduate student at Harvard, he had already won international renown and had made significant contributions to economic theory. Confronted by contradictions, overlaps, and fallacies in the classical language of economics, he sought unification – and clarification – in mathematics. In his first major work, *Foundations of Economic Analysis*, published in 1947, he demonstrated that this approach worked. He told economists that they had been practicing “mental gymnastics of a peculiarly depraved type,” and that they were like “highly-trained athletes who never run a race.” He was not claiming mathematics as the cure-all or end-all of economic analysis, but he was insisting that mathematics was essential to an understanding of what economics was all about.  His *Economics: An Introductory Analysis*, first published in 1948, has become the best selling economics textbook of all time. The textbook has sold more than a million copies and has been translated into French, German, Italian, Hungarian, Polish, Korean, Portuguese, Spanish and Arabic. It is now in its fifth edition. “The book’s emphasis on different themes has changed with the changing of the nation’s economic problems,” wrote *Business Week* in 1959. “The first edition was dominated by the end-of-the-war worry that widespread unemployment would return… later editions put growing stress on fiscal and monetary controls over inflation. In the later editions Samuelson has worked toward what he calls a ‘neoclassical synthesis’ of ancient and modern economic findings. Briefly, his synthesis is that nations today can successfully control either depression or inflation by fiscal and monetary policies… Some economists feel that Samuelson’s book… is really his greatest contribution. It has gone a long way toward giving the world a common economic language.”  He was co-author of *Readings in Economics*, published in 1955, and has co-authored numerous other works in the field. His latest book is *Linear Programming and Economic Analysis*, written in collaboration with Robert Dorfman and [Robert Solow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1987/index.html) and sponsored by a grant from the Rand Corporation. Mathematical economics is applied to practical problems in international trade, transportation and marketing, competitive strategy in business and government, industrial production, and defense planning. Such complex problems of choice can now be analysed by the mathematical economics which Professor Samuelson has developed.  He came to M.I.T. in 1940 as an Assistant Professor of Economics and was appointed Associate Professor in 1944. He served as a staff member of the Radiation Laboratory from 1944-1945, was Professor of International Economic Relations (part-time) at the Fletcher School of Law and Diplomacy in 1945. He was appointed Professor at M.I.T. in 1947 and is now an Institute Professor. He was a Guggenheim Fellow from 1948-1949.  Professor Samuelson has served widely as a consultant. He worked for the National Resources Planning Board from 1941-1943 (in charge of war-time planning for continuing full employment); the War Production Board and Office of War Mobilization and Reconstruction in 1945 (economic and general planning program); the United States Treasury, 1945-1952; the Bureau of the Budget in 1952; the Research Advisory Panel to the President’s National Goals Commission from 1959-1960; the Research Advisory Board Committee for Economic Development in 1960. He was a member of the National Task Force on Economic Education from 1960-1961 and has been a consultant to the Rand Corporation since 1949. He is an informal consultant for the United States Treasury and the Council of Economic Advisors. He is also a consultant to the Federal Reserve Bank. He was Economic Advisor to Senator, candidate, and President-elect Kennedy and was the author of the January 5, 1961 “Samuelson Report on the State of the American Economy to President-elect Kennedy.” His consultation for the government has brought him national recognition as an economic advisor. In 1965 he was elected president of the International Economic Association.  Contributing in 1958 to a symposium sponsored by the Committee for Economic Development on “What is the most important economic problem to be faced by the United States in the next twenty years?” Professor Samuelson answered, “The threat of inflation.”  “The history of the twentieth century,” he wrote, ” – America’s century! – has been pretty much a history of rising prices… inflation is itself a problem. But the legitimate and hysterical fears of inflation are – quite aside from the evil of inflation itself – likely, in their own right, to be problems. In short, I fear inflation. And I fear the fear of inflation. Avoiding inflation is not an absolute imperative, but rather is one of a number of conflicting goals that we must pursue and that we may often have to compromise. Even if the military outlook were serene – and it is not – modern democracies must expect in the future to be much of the time at, or near, the point where inflation is a concern. Our greatest economic problem will be to face that concern realistically, to weigh inflation’s quantitative evil against the evils of actions taken against it, to develop methods of adjusting to the residue of inflation which attainment of the ‘golden mean’ might involve. The challenge is great but the prognosis is cheerful.”  In an interview in 1960 with *U.S. News World Report*, Professor Samuelson talked about a new kind of inflation – what he called “cost-push.” As contrasted to the familiar kind of inflation – where too much spending power pulls up prices and wages – cost-push inflation is “a force that operates year-in and year-out, whenever we are at high employment, to push up prices. It’s a price creep, not a price gallop; but the bad thing about it is that, instead of setting in only after you have reached overfull employment, the suspicion is dawning that it may be a problem that plagues us even when we haven’t arrived at a satisfactory level of employment.”  In his report to President-elect Kennedy in 1961 on the state of the American economy, he wrote: “Various experts, here and abroad, believe that the immediate postwar inflationary climate has now been converted into an epoch of price stability. One hopes this cheerful diagnosis is correct. However, a careful survey of the behavior of prices and costs shows that our recent stability in the wholesale price index has come in a period of admittedly high unemployment and slackness in our economy. For this reason it is premature to believe that the restoration of high employment will no longer involve problems concerning the stability of prices.  “Economists are not yet agreed how serious this new malady of inflation really is. Many feel that new institutional programs, other than conventional fiscal and monetary policies, must be devised to meet this new challenge. But whatever the merits of the varying views on this subject, it should be made manifest that the goal of high employment and effective real growth cannot be abandoned because of the problematical fear that re-attaining prosperity in America may bring with it some difficulties; if recovery means a reopening of the cost-push problem, then we have no choice but to move closer to the day when that problem has to be successfully grappled with.”  In this report to President-elect Kennedy, Professor Samuelson made certain minimal policy recommendations “that need to be pushed hard even if the current recession turns out to be one that can be reversed by next summer at the latest.” He urged strong support of pledged expenditure programs, including: increasing defense expenditures and foreign aid on a basis of merit and need, vigorously pushing educational programs, high priority for urban renewal and health and welfare programs, highest priority on improving unemployment compensation, acceleration of useful public works and highway construction programs, help for depressed areas programs, and natural resource development projects.  To stimulate residential housing, he recommended reducing mortgage rates, mortgage discounts, insurance fees, and extension of maximum amortization periods, and a step-up in the Federal National Mortgage Association mortgage purchasing program. In monetary policy he specifically urged more reliance upon short term issues (to nudge a reduction in long term rates), and decisive actions to improve our international balance of payments position.  On the question of unemployment levels, Professor Samuelson made these comments in an interview with *U.S. News World Report* in December, 1960: “I think, without question, that unemployment of more than 6 per cent is something to be concerned about. You don’t push the panic button, but you don’t relax and enjoy it either… I myself don’t believe in a numbers game in which you give a maximum tolerable percentage, because I think, truly, it does vary with the times… I would hesitate to specify the figure today, but I will say this: it would be, in my mind, less than a 4 per cent figure – that is, for the period ahead. I would not, realistically, think we could hope for a 2 per cent figure in the near future, as certain European countries have been able to do. But I do think that if we are pretty zealous in this matter and insist upon getting low figures – say, 3.5 per cent – then our very success in accomplishing that may lead to a new epoch just beyond when we could hope to go below 3 per cent… “  A further question in the interview asked what degree of responsibility the government has to insure high employment. Replied Professor Samuelson: “I think I would say simply that the American people have expressed the choice that it is their concern to see that large departures from high employment will not be tolerated… I never look upon the government as something in Washington that does something to us or for us. I think of public policy as a way in which we organize our affairs, and so I do think it is part of fiscal responsibility and monetary-policy responsibility to be discontented with the sort of unemployment we had in the prewar decade, and with the sort of exuberant booms leading to crises and panics that we have had throughout the history of our capitalistic system.”  Summing up, he made this prediction for the decade: “I think the ’60s will give us the potentiality of very good growth. More and more of our social problems of the past are, in fact, being licked. So I would face the ’60s not complacently, but optimistically.”  Professor Samuelson has been active in a number of honorary and professional organizations. He is a member of the American Academy of Arts and Sciences, a fellow of the American Philosophical Society and the British Academy; he is a member and past President (1961) of the American Economic Association; he is a member of the editorial board and past-President (1951) of the Econometric Society; he is a fellow, council member and past Vice-President of the Economic Society. He is a member of Phi Beta Kappa.  He is the author of hundreds of articles in journals and magazines.  He lives with his wife and six children (including triplet boys) in Belmont, Mass.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  *Paul A. Samuelson died on 13 December, 2009.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0893 |
| Interview |  |
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| ID | 0894 |
| Biographical | I was born in Oslo, March 3, 1895, as son of the gold- and silversmith, Anton Frisch, and his wife, Ragna Fredrikke Kittilsen, who has had a great impact on my general outlook and view on life.  I was first married in 1920 to Marie Smedal. We had an only child, Ragna, who was married Hasnaoui. She has a daughter, Nadia, who, of course, in the view of her grandfather, is the most superb granddaughter in the whole world. My first wife died in 1952. In 1953, I married Astrid Johannessen whom I had known from childhood. She had passed her university degree in languages in the Oslo University, 1921. She is a daughter of the businessman and shipowner (from the time of the sailing ships), I.M. Johannessen and his wife, Julie Caspersen. They had been intimate friends of my parents for many years. Ever since our marriage, Astrid has been my unfailing companion and has sustained me devotedly in all the ups and downs of life.  My father’s gold- and silverwork firm in Oslo was established by my grandfather in 1856. Gold and silver has been a tradition in our family ever since the years around 1630 when King Christian IV of Denmark-Norway asked the Electoral Prince of Saxony to send him a team of mining specialists from Freiberg in Saxony (that had a Mining Academy) to the newly-discovered silver deposits at Kongsberg, Norway. We can trace our ancestry fairly exactly back to that time.  When I was planning my future it was more or less taken for granted that I should follow the gold and silver tradition. For that purpose, I started as an apprentice in the workshop of the famous Oslo firm, David Andersen, and at the end of the apprenticeship in 1920, I completed my handicraftsman’s probation work as a goldsmith.  After the beginning of my apprenticeship, my mother got a strong feeling that the trade would not be satisfactory for me in the long run. She insisted that at the same time as I completed my apprenticeship, I should take up a university study. We perused the catalogue of the Oslo University and found that economics was the *shortest* and *easiest* study. So, therefore, economics it became. That is the way it happened. Later on, the study of economics in the Oslo University has proceeded by leaps and bounds in the direction of a more advanced and time-consuming study (some people seem to think that, somehow, I have been instrumental in this development).  I passed my university degree in economics in Oslo, 1919. About a year later, I went abroad to study economics and mathematics in earnest. I visited France, Germany, Great Britain, the United States and Italy. During my stay of nearly three years in France, I got so familiar with the conditions there that ever since, when I get to visit France, I somehow feel that I have “come home again”. I passed my Ph.D. on a mathematical statistical subject in the Oslo University in 1926. In 1925 I was appointed Assistant Professor, in 1928, Associate Professor, and in 1931, full Professor in Oslo. I became Director of Research of the newly-established Economic Institute in the Oslo University.  In addition to these facts, I shall not have much to say about my scientific career. I am an invited member of a great number of learned societies in different countries, and have several doctorates *honoris causa*.  Of my scientific awards before the 1969 Prize in Economic Science in Memory of Alfred Nobel, I must mention the big Antonio Feltrinelli prize awarded to me in 1961 by the Accademia Nazionale dei Lincei, the old and famous Italian society of which Galileo Galilei was one of the first members.  When I think of the long list of problems of which I have in vain tried to find the solution, and think of the honours that have nevertheless been bestowed upon me, I understand with deep thankfulness to Whom all this is due: to the Lord Who has steered my steps over the years, and Who has been my refuge in the superior matters which no science can ever reach.  My hobbies have been outdoor life, including mountain climbing on a modest scale. But above all, it has been bee-keeping and queen-rearing in which I have been engaged for 57 years, with emphasis on a genetic and statistical study with a view to improving the quality of the bee. If somebody asked me if I find this occupation pleasant and entertaining, I am not sure I could honestly say yes. It is more in the nature of an obsession which I shall never be able to get rid of.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  *Ragnar Frisch died on January 31, 1973.* |
| Autobiographical |  |
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| ID | 0895 |
| Biographical | |  |  | | --- | --- | | Born | 1903, The Hague, Netherlands | | Education | 1929, Leyden University, Doctor of Physics | |  |  | | Academic appointments | | | 1933 | Professor at the Netherlands School of Economics, teaching various subjects; fulltime from 1956 onwards, when subject became Development Programming |  |  |  | | --- | --- | | Non-academic offices | | | 1929-1945 | Statistician for Business Cycle Research, Central Bureau of Statistics | | 1936-1938 | Expert, temporarily attached to League of Nations Secretariat | | 1945-1955 | Director of Central Planning Bureau of Netherlands Government | | Advisor to governments of various developing countries (United Arab Republic, Turkey, Venezuela, Surinam, Indonesia, Pakistan and other countries, occasionally) and to international organizations (European Coal and Steel Community, International Bank for Reconstruction and Development, United Nations Secretariat and other specialized and regional organizations) | | |  | | | Honors and honorary degrees | | | Member of Royal Netherlands Academy of Science and some foreign academies, honorary doctor of fifteen universities, mostly European. | | |  | | | Principal publications | | | *Business Cycles in the United States, 1919-1932*, Geneva, 1939 and New York, 1968 | | | *Business Cycles in the United Kingdom, 1870-1914*, Amsterdam, 1951 | | | *Centralization and Decentralization in Economic Policy*, Amsterdam, 1954 | | | *Economic Policy: Principles and Design*, Amsterdam, 1956 | | | *Selected Papers*, Amsterdam, 1959 | | | *The Element of Space in Development Planning* (together with L.B.M. Mennes and J.G. Waardenburg), Amsterdam, 1969 | |   From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Economics 1969-1980*, Editor Assar Lindbeck, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  *Jan Tinbergen died on 9 June 1994.* |
| Autobiographical |  |
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