

Confessions of an Incurable Romantic*

Writing my intellectual autobiography is an assignment which I have long postponed, primarily out of fear. Such a retrospective self-appraisal would inevitably make me stand naked in front of myself, influence the rest of my career and impart a sense of, hopefully premature, semi-closure. A gentle reminder by the editors has now made me grit my teeth, lay my trepidations aside, and commence.

I was born in Cernowitz, Roumania, in March, 1930. Amusingly enough, Joseph Schumpeter had played an unwitting role in my parents' marriage. My mother was a law student at the University of Cernowitz while Schumpeter was teaching there. She was being courted by my father, a businessman ten years her senior. She decided to reject his suit, took her qualifying exam in economics from Schumpeter, and returned to her home town. A few months later, she was informed that Schumpeter had lost her examination paper, and that she would have to take the exam again. She returned to Cernowitz, was met by my father who, in his sorrow at being rejected by her, had shaved his head and lost about fifteen pounds. Her heart went out to him and she reversed her decision.

Formative influences

The formative influences on my life and values were my parents, my early education, and the trauma of World War II. My mother, a very attractive, intelligent, and vivacious woman, never got to practice law. My father claimed that her working would ruin his credit rating and the

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quota on Jewish lawyers imposed by the Roumanian government in the thirties meant that by choosing to practice she would take bread out of the mouth of a Jewish male *pater familias*. So she concentrated her boundless energy and ambition on me, a single child. She was determined (poor woman) that I would be as attractive as possible given my original, rather unpropitious, endowments and that I would have the career that circumstances had conspired to rob her of. Her efforts imbued me with a view of the perfectibility of individuals and society that have dominated my teaching and research.

My father was a socialist businessman, a not unusual paradox among East European Jewish businessmen at the time. He had been studying at the University of Kiev, in the Ukraine, when the Russian Revolution broke out, and was a Zionist Menshevik, a socialist reformer. When the Bolsheviks won, he fled to Roumania. He had been scheduled to be shot, but the officer in charge of the firing squad turned out to be a friend of my uncle's and enabled him to escape. From my father I gained my commitment to social reform, my compassion for the poor, and my sense of outrage at social conditions that generate mass poverty and mass deprivation.

Despite being Jewish, my early education in Roumania was at a French Catholic nun's school, Notre Dame de Sion. The Jews are said to own "guilt" and the Catholics to have a lifetime lease on it. My early education therefore left me with a mammoth sense of primordial guilt, that was later reinforced by the guilt of the survivor of the Holocaust. The expiation of this guilt through the only mechanism it can be expiated — service to humanity — has been a primary driving force in my life.

World War II left an indelible mark, even though I escaped comparatively unscathed. My father had had the foresight and courage to leave Roumania in 1939 for Palestine, so that the entire nuclear family survived intact. The main impact of the War on me was the wrenching break in personal attachments involved in becoming a refugee, and the experience of mass religious hatred. I remember my father telling me when I was six that I might be reviled for being Jewish, but that I should be proud of this fact. Since I knew not what being Jewish was, and had been taught by the nuns that pride is sin, this talk left me totally bewildered. The War imbued me with a sense of rootlessness, a suspicion of mass ideologies, a sense of the impermanence of any state, a lack of attachment to possessions, and a sense of personal worthlessness. It also induced a sense of belonging nowhere and everywhere, that

is the mark of the cosmopolitan, and a feeling of "There but for the grace of God go I" towards the less fortunate, akin to Rawl's initial state of ignorance. On the positive side, I learned that the only thing one can rely on is one's human capital — one's knowledge, skills, and character — because all else can be taken away at the bellow of a demagogue. My later fascination with stochastic shocks, with nonlinear dynamics, and socio-political view of economic development also have their roots in my World War II experience.

There was never any doubt in my mind that I had to become an intellectual. It was my only comparative advantage (I was an uncoordinated, rolly-polly and cross-eyed youngster), and my parents and world view had predisposed me to see in education my only potential for achieving a moderately stable and socially productive life. After finishing high school in Palestine and fighting in the Israeli war of independence, I enrolled in 1949 as an undergraduate at the University of California at Berkeley. I chose business administration with a minor in public administration not because of my interest in these subjects — had I let myself pursue my own inclinations I would have studied French and German literature and art history — but rather because of my perception of the primary needs of the nascent State of Israel, the furthering of whose interests I was dedicated to. But this was not to be! Shortly after coming to Berkeley, I met my husband, an American physics PhD candidate, fell in love, married, and stayed. In making the decision to marry and stay, I felt very guilty at putting my personal happiness ahead of my duty to Israel, whom I felt I was betraying by not returning there.

I sailed through undergraduate school, shifted to economics in graduate school, and obtained my PhD six years after having entered as an undergraduate. My graduate education was sadly lacking. At the time, Berkeley was very weak in economic theory and in mathematical training. Robert Dorfman was the only ray of light in the graduate program and I shudder to think what I would have become had I not been able to benefit from his tutelage. I supplemented the graduate program in economics by taking courses in statistics, mathematics, and in agricultural economics, where, together with Arnold Zellner, Zvi Griliches and Yair Mundlak, I learned my econometrics from George Kuznets. I also benefitted greatly from the influence of my husband, Frank Adelman, who taught me a view of scientific method involving a continual iterative interaction between theory and experimental or statistical "stylized facts" that is natural to applied physicists but still not to economists.

Research

From the perspective of a historian of doctrine, the research process appears planned *a priori*; from the perspective of the author, it appears as a series of unplanned choices that are guided by personal interest and a sense of the importance of the issues, and are made in response to opportunities, both external and self-generated. Although both perspectives are correct, I shall adopt the latter in this narrative.

My early research was eclectic, but there were a few common threads, arising from my values and early personal experiences: concern with dynamics, both cyclical and long run; concern with stochastic processes; and concern with aggregation procedures. "The Dynamic Properties of the Klein Goldberger Model", "Business Cycles, Endogenous or Stochastic?", "A Stochastic Analysis of the Size Distribution of Firms", my first book, and my work on hedonic index numbers (1961) were all facets of these concerns. There were also some methodological predilections, which have stayed with me throughout my entire career, and which reflect my scientific predispositions: a view of the world as an interdependent system; a view of the world as real and of scientific research as holding up a, hopefully non-distorting, mirror to it; and an inner compulsion to contribute to the elucidation of real world issues that affect the welfare of a large proportion of the world's population.

The Klein-Goldberger paper arose when my husband, a physicist, one day expressed a desire to try programming a simple problem and asked whether there was anything in economics that might be suitable. I suggested the Klein Goldberger model. This was before the days of Fortran (1955); all programming was in machine language. I remember spreading out a large sheet of paper on the floor, with a map of the computer memory, and keeping track of the location of individual variables after every operation. Nevertheless, when we ran the problem, there was only one error in the code! After we finished the computer runs, my husband taught me a valuable lesson. He said: "Now it's up to you to milk the results". The writing of the paper was excruciating: we composed it jointly and fought over every word in every sentence, finishing only one or two paragraphs per night. This paper, which confirmed the Frisch hypothesis of random origin of business cycles, has been identified as one of the best 20 articles in *Econometrica*, and achieved the status of a "classic" in business cycles and in simulation of economic systems.

My first book, *Theories of Economic Growth and Development* (Stanford Press, 1961), was originally written as the development-theory section of an undergraduate text on economic development joint with L. Mears and A. Pepelassis. The publisher, Mc Graw Hill, objected that my section was at a more advanced level than the rest of the book and insisted that it be taken out. I then revised it, making the text more lucid, but, when it came to seeking a publisher, I became racked with doubt. It seemed to me that the book contained little that was original, and that when I was describing the interactions of socio-cultural and institutional features of societies with their economic development I did not know what I was talking about. So, for a few months after finishing the revision, I held the manuscript. But this troubled me. Paul Baran, my then colleague at Stanford, noticed that I was upset and asked me why. When I blurted out my concerns, he said: "It's all very simple, Irma. Let the market decide! Send the book to a few publishers, and see whether they take it". Amusing advice from the then only Marxist economist teaching at an American university...

This book, with its associated doubts, set the stage for one of my consistent lines of research: how the economic growth of nations is affected by and, in turn, affects economic and political institutions, and socio-cultural structures and values; and how institutions and economic structures and choices affect the diffusion of benefits from economic and institutional change. I felt the need to understand these processes better and to base my understanding on empirically generated hypotheses and stylized facts. This is the line of research with which Professor Cynthia Taft Morris became associated.

I first met Cynthia Taft Morris in Washington D.C., in the summer of 1962, when we were both Research Associates at the Brookings Institution. We both had just moved to Washington, following, like the Biblical Ruth, our husbands' careers, and were both a little disoriented by the need to build new professional bases for ourselves. Our work together that summer was the beginning of a lifetime friendship and association. After the summer, Cynthia Morris combined teaching at American University with part-time work at the Agency for International Development (AID), in the research division headed by Hollis Chenery. And I started teaching at Johns Hopkins University, in Baltimore, and was brought by Hollis Chenery into his research division with a vague mandate to roam through the AID files and find something researchable. I found the AID country reports — monographs generated by AID offices in the field as annual reports on their respective

countries. This was before the days of general data banks; indeed, even before the days of published comparable figures on per capita GNP! The reports were variable in quality and reliability, but had undergone some vetting before being sent to Washington, and, at least in principle, were uniform in coverage. They were treasure-troves of up to date information concerning political and socio-cultural country situations together with quantitative and descriptive information on industry, agriculture, investment and international trade. Naturally, the information had to be cross-checked, especially for political bias and lack of comparative experience with other developing countries, but nevertheless offered an invaluable starting point. I became very excited about the potential of these country reports for generating information usable in research on interactions of economic social and political facets of economic development. Also, in reading psychological literature, I had come across the use of factor analysis. This technique seemed to offer an ideal statistical vehicle for exploratory research on interactions about which there were no validated theories. I asked Cynthia Morris whether she would be interested in collaborating with me on this project. And so, *Society, Politics, and Economic Development — A Quantitative Approach* (Johns Hopkins Press, 1967) was born.

A word about our professional collaboration may be in order: Cynthia Morris had polio as a teenager and has been on crutches ever since. As a result, her mobility has been limited. She therefore informed me early in our collaboration that she did not wish to be involved in presenting papers at conferences, professional meetings, etc.; that task would be up to me. Unfortunately, her less visible role has led the profession to underestimate her contributions to our joint work.

In 1965, when the major work on *Society, Politics, and Economic Development* was done, it occurred to us that it would be interesting to see how applicable the hypotheses generated by this research on contemporary development were to the historical development process during the period of the Industrial Revolution. This research was especially appealing to Cynthia Morris, whose training was as an economic historian with a strong institutional bent. She then started working on gathering comparable information on 23 countries for the period 1850-1914. In 1972, I met Herman Wold, at a talk on the methodology of partial least squares and, more generally, on soft modelling that he gave at the World Bank. I got very excited by his philosophy and approach, rare among mainstream statisticians, to which I resonated. Ever since finishing *Society, Politics, and Economic*

Development, I had been looking for distribution-free methods of melding partial prior specification with sample information. (While my philosophy was Bayesian, there were two reasons I could not go the strict Bayesian route: I did not want to specify a specific prior distribution, especially with the type of discrete, ranked data that characterized my work on interactions between social, political, and institutional features of societies and their development patterns. I also wanted to deal with interdependent systems, and this is still difficult with present Bayesian techniques.) Herman Wold's approach seemed to be the answer. I started working with him in the early stages of the development of the partial least squares approach, and through him became aware of the work of Svante Wold, on disjoint principal components models. It is this latter approach that Cynthia Morris and I used in our historical work. Little did we know, when we started this research in 1965, that it would be 23 years before our historical work could culminate in a book describing what role institutional and political forces had played in inducing the very diverse economic responses of individual countries to the challenges and opportunities offered by the early Industrial Revolution in Great Britain! Our book, *Comparative Patterns of Economic Development, 1850-1914* (Johns Hopkins Press) appeared only in 1988. In this book, I finally succeeded in persuading Cynthia Morris to put her name as first author, both as a means of reflecting our relative contributions to this work and as a means of partially rectifying the general misconceptions about her contributions to our past joint research.

Of course, during the twenty three years that it took to complete this book, there were several detours on the way. The most important was our joint and my separate work on income distribution in developing countries. In 1969, the Agency of International Development came under fire from the US Congress for not paying enough attention to the spread of benefits from its projects. (It would appear that international junkets by Senators and Congressmen have some uses!) Cynthia and I were asked to undertake a study of the breadth of participation, both economically and politically, by developing countries' populations in the development process. The result was *Economic Growth and Social Equity in Developing Countries* (Stanford Press, 1973). While generally taken as having confirmed Kuznets' U-hypothesis, our results confirmed a J-hypothesis. We found that the share of income accruing to the poor first declines rapidly, then less rapidly, then less rapidly, and then, depending on the policy choices

made, either levels off (the J) or starts increasing (the U). Politically, as the indigenous middle class and urbanization increase, and as education and communication improve, the influence on policy of non-elite groups starts extending to the middle class and to workers in the modern sector. But we found that the greater political participation of these groups does not redound to the benefit of the poor. Indeed, the middle class benefits at the expense of both the poor and the rich.

We were deeply shocked by our findings. Up to then we had believed in the benign view of economic development offered by modernization-scholars and in the trickle-down hypothesis imbuing mainstream writings on economic development. Were it not for the function-free statistical technique we adopted for our study, and were it not for our inductive empirical approach, we would have adopted an *a priori* specification confirming the modernization-cum-trickle-down theories. We would then have ascribed the poor statistical fit to poor data and small sample size. It is also fortunate that we undertook an arduous effort to obtain direct information on income distribution, despite the virtual lack of published studies. Of over 200 books and more than 1000 articles on individual countries published in the previous ten years, we found income-distribution information in only one — Samuel Barber's study of South Africa — for 1948! We found a list, prepared by the United Nations Statistical Office, of income distribution studies in developing countries that had been carried out but had not been published, and then proceeded to use the leverage or AID field offices to obtain the studies themselves. We were also fortunate to find the comparative study by Christian Morrisson, written as his PhD dissertation, with income-distribution estimates for Sub-Saharan African countries. This data, though of lesser reliability than the data for the more developed countries, played a critical role in generating the initial decline of the income share of the poorest. We submitted our report to AID and then did not publish the results for two years since we feared that our findings would be used as an argument to curtail resources for foreign-assistance rather than redirect resources to more poverty-oriented projects and programs. We felt free to publish our findings only after we were convinced they would do no harm: by 1973 the decline in foreign-assistance was already underway and new evidence concerning increasing urban unemployment despite rapid growth was making it clear that all was not well with the development process.

Our findings in this book led to the second major strand in my work: that dealing with income distribution and poverty, both descrip-

tively and from a policy viewpoint. Two articles, "On the State of Development Economics" and "Development Economics - A Reassessment of Goals", summarize the effect my shock had on my research. In the former I argued that the fundamental failure of development economics had its roots in several methodological deficiencies: the failure to take a sufficiently broad systems-approach; the failure to monitor results adequately; the pervasive search for panaceas and for simplicity and simple guidance rules; and insufficient humility and insufficient professionalism in our approach to development. In the latter article, I argued that the goals of development should become the creation of the social and material conditions for the realization of human potential by all. This goal should replace the goal of self-sustained growth; rather, growth should be viewed as an instrument for the achievement of poverty reduction — a goal to which I referred as "depauperization". Mark Blaug (1985) calls this paper my most readable and controversial article.

I joined the World Bank in 1971 and started a research program aimed at seeing whether an approach to economic development policy exists that would spread more of the benefits of development to the poor. This was genuinely an open question, since the history of the early phases of the Industrial Revolution in developed countries had also exhibited a decrease in the share of income accruing to the poor. It seemed to me that finding such an approach would require generating a computer-laboratory in which experiments with policies and programs could be carried out and evaluated. This laboratory should represent how economic actors interact in an actual economy, portray the governmentally set rules for markets and behavior, incorporate all the instruments for intervention and all the variables that are important in mediating the impact of the economy, of governments, and of the rest of the world on the poor. Having seen how the simple (simplistic?) *a priori* models that identified single-cause development prime-movers or bottlenecks had led the development-policy formulating community into advocating a seriously flawed development process, I rejected the methodology of specifying a two or three sector model with one or two classes of actors, solving it for its comparative statics implications, and then basing policy recommendations on these findings. Rather, I argued for building a complex but realistic computer-model and then simplifying it *a posteriori*, on the basis of sensitivity experiments. This returned me to the methodology of digital simulation, introduced into economics by my first major published article, on the Klein-Goldberger

model. I asked Sherman Robinson, who was then an Assistant Professor at Princeton, to join me in this research. (I had first met Sherman Robinson when, as a PhD candidate at Harvard, he asked me for the *Society, Politics, and Economic Development* data for use in his dissertation. When he finished, he sent me a copy of his dissertation, and I was impressed with him.)

We first thought that the appropriate model structure for our research on income distribution would be offered by a Johansen model. But it then became clear that, since our purpose was to model structural change and large-program interventions, a model like Johansen's, that is expressed in linear rates of growth, might miss effects that are of the same order of magnitude as the impacts of the experiments themselves. Sherman Robinson suggested that we change our formulation to solving for the levels of the endogenous variables rather than, as in the Johansen model, for their rates of change. And so, the first large-scale computable general equilibrium model (CGE) was born. The model was quite large (it contained over 3000 endogenous variables); mixed neo-classical and structuralist features; incorporated non-homogeneities in investment and government behavior; had an endogenous demand for money; two loanable funds markets (one official and one unorganized); two policy regimes (a tight-money regime with fixed money supply and rationing, and a loose-money regime with fixed interest rates); and some elements of industrial organization within sectors (four firm or farm sizes, with different behavior rules and different credit and foreign exchange access by large firms). The model was applied to South Korea, a country for which we both had a feeling. The initial reactions to our model specification were skeptical. Our critics contended that we would never be able to solve the model, and that, even if we were able to solve it, we would not be able to understand what was going on in the model. We proved them wrong, however, on both counts. Our book, *Income Distribution Policy in Developing Countries: The Case of Korea* (Stanford Press), finished in 1975, appeared in 1978. In it, we were able to identify the important policy variables, explain how the model worked, and gain a feeling for the relative importance of different policy interventions. (The first general presentation of the model was in 1973, at World Congress of the Econometric Society, in Toronto. There we met with John Whalley, then a graduate student writing his dissertation with Herbert Scarf, and he asked us many questions about solution-techniques, and about the feasibility of solving other than toy-models. The first written reports on the model were in 1973 and the first

publication, giving our rather pessimistic policy conclusions based on our comparative statics experiments, was in 1975, as part of my paper calling for a shift in emphasis away from economic growth and towards poverty alleviation as the major goal of development policy.)

In the policy experiments we performed with our CGE model we found that policy interventions aimed at increasing the equality of the size distribution of income were very difficult. Of the roughly 3000 endogenous variables in the model only two, rural-urban migration and the agricultural terms of trade had a perceptible impact. The size distribution of income was exceedingly stable — even large-scale programs produced effects that altered only the second decimal of the Gini coefficient. Most interventions altered the incidence of poverty (*i.e.* the functional distribution of income), especially between the rural and urban poor and near poor, without changing the relative magnitude of poverty (*i.e.* the size distribution of income). In the absence of changes in the distribution of assets or institutions affecting the access of the poor to factor and commodity markets, only changes in development strategy, equivalent to large packages of mutually coordinated programs, could alter the relative magnitude of poverty by engendering the right kind of economic growth. Absolute poverty was easier to reduce than relative poverty. This conclusion was confirmed by our dynamic experiments and is consistent with the conclusions from models for different countries, with different closure rules, and different structural specifications (Adelman and Robinson, 1988).

After the book was finished, Sherman Robinson joined the World Bank and shifted to work on industrialization and trade with generic CGE models. He simplified the specification of the Korea-CGE model, based on the intuition gained from our sensitivity and policy experiments; improved the solution algorithm; improved the trade specification; and based the model-calibration explicitly, rather than only implicitly, on the Social Accounting Matrix (SAM) accounting framework. His work did a great deal to disseminate the use of CGE models among academic researchers and in the policy-planning community. Amusingly enough, however, with the currently renewed interest in the impact of IMF-inspired structural adjustment programs on the poor in debt-ridden developing countries, many of the monetary, macro-economic, industrial-organization, and credit-allocation mechanisms that he ripped out of the Korea-CGE model, in an effort to arrive at a simpler generic model, are being reintroduced into CGEs of the 1980s, one by one.

I continued my work on income-distribution policy and, becoming increasingly discouraged about the potential for policy-impact on development assistance and on development policy after the two oil shocks, increasingly turned to non-policy work on institutions in development and economic history.

In 1977, I was invited to hold the Cleveringa chair at Leyden. This was a chair established by the Queen of the Netherlands to commemorate the resistance of Leyden University, led by Cleveringa, a law Professor, to the Nazi order to fire all Jewish Professors. The chair was to deal with some issue affecting human rights, be staffed by a social scientist one a one-year basis, and rotate between a Dutch and a foreign Professor. I was the fourth holder of the chair, the second economist after Tinbergen. In my inaugural address, "Redistribution Before Growth - A Strategy for Developing Countries" (Martinus Nijhof, 1978) I advocated asset redistribution before, rather than after, improvements in the asset's productivity: land reform before improvements in agricultural productivity and mass primary education before a major push on industrialization. Asset redistribution before improvements in productivity would enable growth-promoting measures to go hand in hand with equity-improving measures, thereby greatly enhancing the potential for improving the lot of the poor through economic development. The profession has accepted the call for increased emphasis on primary education while ignoring the call for land reform as unrealistic. In "Beyond Export-Led Growth" (World Development, 1984) I advocated a temporary shift during the low-growth-in-world-income-and-trade period of the 1980s towards agricultural development in an open trade regime as a mechanism for accelerating domestic industrialization and increasing equity (the ADLI strategy). I used the generic CGE model of Korea developed by Sherman Robinson to demonstrate the superiority of this strategy in a low-growth world environment over export-led growth.

At the same time, Cynthia Morris and I intensified our work on economic history. After finishing the historical book in 1988, we felt that we had enough insight into the complex interactions that determined the diversity of country responses to the industrial revolution in Great Britain to be able to specify a simultaneous equation partial least squares model of 19th century economic development using Herman Wold's statistical methodology (Adelman, Lohmoller, and Morris, 1988). We are now working on a monograph comparing historical and contemporary development patterns.

Policy work

My policy work started early in my career, and I have always felt that it offered both the motivation and new insights for my research. My first involvement with policy started by accident. In 1963, AID in Washington received an urgent request from its Vietnam office for a statistician who would design a rural income-expenditure survey in the Delta. I did not quite understand why this was so urgent, but was eager to travel so I volunteered. When I came to Saigon, I was struck by two things: the Vietnamese population did not seem to be committed to the war and the security situation was much worse than depicted by either military or diplomatic communications from Saigon. I reasoned that with incorrect information, correct decisions could not be made in Washington, and, with the arrogance of youth, started on a one-woman fact-finding mission. My starting point was why the Vietnamese population was not committed to the war. I soon realized that an important part of the answer was that, with existing tenurial conditions, the rural population had a large positive incentive to keep a low level of military activity going: due to the war, most of the landlords had left the rural areas, and rents had not been collected for as much as three years. At existing rents, pacification would mean an indebtedness of about 1.5 years' output! This led me to argue for a United States supported land reform of the land-to-the-tiller variety as a higher probability alternative than the military approach to ending the war. Buying all the land of the Delta at market prices from the landlords would cost only about half the then military annual budget! Upon returning from Saigon, I spent much of my effort for about three months peddling this view to the policy establishment. I gained a hearing, but, alas, the military approach prevailed. Many years later I met the director of the Saigon AID mission again and asked him why the consumption-expenditure survey had been such a high-priority item. His answer: "The country may be burning but Washington still wants to know: what's GNP?" — a sad, but accurate, comment on bureaucracies.

My work in development planning started early, has given me some insight into methods of policy-formulation and a great deal of personal and professional satisfaction. In the 1950s and 1960s, work in economic development was by and large non-technical except in one area — that of development planning. This area, which started with Tinbergen's formulation of planning and his hierarchic view of state-economy

interactions, offered scope for the use of all techniques of econometrics and operations-research. The technical part of my soul could therefore find satisfaction in this branch of work. That period also offered scope for the influential foreign adviser. Both coincided in my work on South Korea's Second Five Year Plan, summarized in *Practical Approaches to Development Planning - Korea's Second Five Year Plan* (John's Hopkins Press, 1969).

My involvement in Korea started fortuitously. I was sitting in the office of a friend at AID in the summer of 1964, and he was complaining that his boss (Hollis Chenery) wanted him to go to South Korea, whereas he wanted to go to Turkey. I said: "I'll go!" I went under AID auspices in early 1965, wrote a critical report on the institutional setup for planning in Korea, and returned home expecting never to return. To my great surprise, my recommendations were implemented, and I was called back to assist with the work on the plan. We wound up using all the econometric and operations research techniques then known to formulate investment, credit and foreign exchange allocation for the next five year plan. The plan, initiated in 1967, involved a shift towards export-led growth, after a 50% devaluation to realign exchange rates, substantial reductions in tariffs and in the scope of protection to reduce distortions, and a doubling of interest rates to reduce inflation and increase savings. The shift towards export-led growth was a natural recommendation for an economy with highly developed human resources (a level of education three times the average for an economy of its per capita income); a very small internal market (per capita income in 1965 was about \$ 70); and a very poor natural resource base (hence high import coefficients). I was not sensitive at the time to income distribution issues, but the plan worked out very well for poverty as well, tripling the incomes of the poor in ten years, because of the very egalitarian distribution of assets. Korea had had two major land reforms in the early fifties, and had universal primary education. In 1972, I received a Presidential decoration from President Park, the Order of the Bronze Tower, for my work on the Second Five Year Plan. The citation reads: "With deep interest in the wellbeing of the Korean people, Mrs Irma Adelman, the professor at Northwestern University, has devoted her efforts with superb competence to the economic development of the Republic of Korea and thereby greatly contributed towards attaining the goals of economic self-sufficiency pursued by the Government of the Republic of Korea. Her valuable donation and service has gained for her the appreciation and admiration of the

Korean people". But when, in 1973, President Park turned from benevolent dictator to oppressive despot, torturing and jailing the opposition, I felt I had to resign from any advisory role in South Korea, after checking with my previous Korean co-workers that my resignation would not place them in jeopardy.

My final direct involvement with policy came in 1971, when I joined the World Bank. A paper summarizing the findings of my work with Cynthia Morris on income distribution and development was circulating as a working paper at the Bank. Mc Namara's speech writer, who was looking for material on this subject, came across the paper and used it as background for Mc Namara's Chile speech. This was the speech that signaled a change in Bank policy toward emphasis on poverty alleviation in lending to developing countries.

With my change of emphasis in development policy towards income distribution and poverty, I lost all popularity with planning agencies in developing countries themselves. For a while, I was popular with international agencies with a poverty orientation: the ILO and the World Bank, in particular. But as their interest shifted towards debt and trade problems, this policy involvement stopped as well. Whatever policy influence I now have is indirect: through my academic research and policy writings.

Career issues

So far, I have not touched on how the particular issues affecting professional women — discrimination; handling the multiple demands of home, child and career; and managing two careers — impinged on my life and career. I hit discrimination against women for the time when I got my PhD, in 1955. I was totally unprepared for this. I was a foreigner to the United States, and I had not realized that, like democracy in ancient Greece, the Horatio Alger myth characterizing the United States as an open, mobile society did not apply to American women. In the fifties, discrimination against women in U.S. academia was incredible. I had graduated from a top institution, at the top of my class, in a period of high demand for college teachers. Nevertheless, when it came to entering the job market, no one would waste a recommendation on a low-probability hire. At the time, openings were

not advertised, and were publicised only through a network of personal contacts. When I applied for a teaching position at San Francisco State, the chairman suggested that I might look for a position in a local private high school! In the end, Berkeley hired me on a one-year appointment as a Teaching Associate — a position routinely given to third-year graduate students who have passed their field examinations. Then came six years, all on one-year, non-tenure-ladder appointments, at Berkeley, Mills College (a local private elite womens' college, where I became aware of the many phenomena described in Betty Friedan's *Feminine Mystique*), and Stanford. By then I had published my first book, the Klein-Goldberger article, two other articles on business cycles, my articles on sampling and hedonic index numbers, and my article on the use of Markov chains to predict the long-run size distribution of firms. The quantity and quality of my publications would have been sufficient to earn me a solid promotion to tenure in any first rate institution, had I been male. And still, I had no leg on the tenure ladder... The hardest thing during this period was to keep from getting bitter. I thank my lucky stars that I had the maturity to realize that, if I were to allow the process to make me bitter, the world would have won its fight against me, regardless of the ultimate professional outcome. I forbade myself the making of invidious comparisons with males, and ordered myself to consider myself as part of a Cairns-type non-competing group. And yet, had the process continued much longer, I would not have been able to hold out against being corroded by bitterness. Still, I was fortunate: I was employed continuously, at first-rate institutions, and worked with excellent colleagues, with whom I interacted on a par. My work-relationships with my colleagues and students were easier than those of males: women are used to interacting as equals with more senior males, my Assistant Professor colleagues did not consider me a threat, and the educator-maternal role with students came easily.

Then, I got a break: my husband became bored with his position at the Livermore Laboratory, and obtained a more challenging position in Washington, D.C. I used the Hungarian Connection (from Tibor Scitovsky, at Berkeley, to George Jaszy, at Johns Hopkins) to indicate my availability, and was offered a regular Associate Professorship at Hopkins, at the princely salary of \$ 10,000 a year, a 60% increase on my previous salary at Stanford. We moved, I met Cynthia Morris, started being exposed to policy through Hollis Chenery at AID, and could use tenured status to engage in longer-term, riskier, research which culminated in the Adelman-Morris publications. Still, salary discrimination

continued. When I complained to my chairman at Hopkins about the lack of a raise for three years despite high productivity, I was told to solicit alternative offers, as indication of my opportunity cost. Within a week, I obtained two offers, one from Maryland at 60% higher salary, and one from Northwestern, at 80% higher salary. The Northwestern position looked especially attractive, because there was an active interdisciplinary group in economic development, and because several of my would-be colleague, George Dalton, Karl de Schweinitz, and Jonathan Hughes, shared my broad-ranging institutional interests. My husband found a satisfactory position in Chicago, and, in 1966, we moved.

I was very happy at Northwestern. I liked the department, my colleagues, the size of the school, the quality of the students, the attitude of the administration, and, last but not least, the computer center. I continued my collaboration with Cynthia Morris, and would have happily stayed at Northwestern except for my husband's work-situation. His position at a research lab in Chicago proved to be unrewarding — so, again, we had to move. We spent a very happy year in 1971 at the Center for Advanced Studies in Behavioral Sciences, in Palo Alto. Influenced by Vietnam, my husband tried during this year to switch from defense-physics to work on urban social-science problems. We worked on a model of urban politics, which incorporated many novel features, but was only published in book chapters and conference proceedings. We hoped that, at the end of the year, we would be able to find joint teaching positions. But our timing was wrong: 1972 was the beginning of the academic recession, and departments were even more than usual concerned with credentialling. I tried to get us hired as a package-deal, and almost succeeded at Cornell. But, in the end, my husband's end of the deal fell through.

After the year at the Center, we moved to Washington once again. I took a job at the World Bank, my husband continued his work on the urban book, and we continued the search for joint positions. For one year, I was the major breadwinner in the family. I then learned how heavy the psychological burden of being the major breadwinner actually is. I would wake up in the middle of the night in a cold sweat, wondering what would happen to the family if I were to suffer an incapacitating accident, or be fired. I now know that what I was trying to do was wrong, even had it succeeded: my husband should have gotten a job on his own merits rather than as part of a package-deal. I felt inadequate for being unable to give him what he wanted (a

professorial position in the social sciences) and he resented my efforts and support, though asking for them. When I gave up, he found a position in his old career within a week! I did not know whether to laugh or cry.

At the end of the year at the World Bank, I took a professorial position at Maryland. Maryland was a commuter campus, and this meant that both students and faculty only came there for a purpose, retreating to home or office in Washington in between. I missed the college atmosphere typical of non-urban campuses, such as Berkeley, Stanford, or Northwestern. My husband and I were both working too hard and had no energy left to build a social life. We started drifting apart, interacting only on the level of practical problems or when going to an event at the Kennedy Center. Finally, the inevitable happened: we separated and, in 1980, divorced.

When we separated, I became a free agent, and when Berkeley's Department of Agriculture and Resource Economics enquired about my potential interest, I jumped at the opportunity. My work on poverty had made me realize the importance of agricultural development, and I was painfully aware how little I knew about agriculture, and how much more difficult agricultural development was than industrialization. I hoped that through exposure to my colleagues' work I might learn about agricultural economics, agricultural technology, and about the physical bases of agriculture. I have not been disappointed. Since joining the Department in 1979, I have learned a great deal about these issues, but I still have a great deal more to learn. Indeed, I expect agriculture-industry interactions and patterns of agricultural development to be a focus of my research in the coming years.

Thus, my response to the twin problems of discrimination against women and two careers, was high geographic mobility. I once counted that we had owned more houses than cars! We moved whenever a Pareto-optimal move was possible, and alternated in initiating moves.

My female students occasionally ask me: "When is a good time to have a child, if I want to also have a career?" My answer is: "Either in graduate school or once tenure is assured". (As a result, I wind up with a fair number of pregnant dissertation advisees...). I myself had chosen a different timing, which did not make my early career any easier. Our son was born in 1958, when I had a very precarious hold on an academic career. The most difficult parts about melding childrearing with career were the tremendous physical stamina it required, the constant guilt at not being a full-time mother, and the constant anxiety that something

might happen to him while I was at work. At the time, day care facilities were very few and of dubious quality. Therefore, for the first ten years of his life, I had live-in help. After that, I had day-help, at first five days a week, then two, then one. He was an easy child, intelligent, energetic, charming, and with a great sense of humor. Our relationship has remained close, though there have been a few rocky patches in the last ten years.

* * *

Like all autobiographies, my story is, fortunately, still unfinished. However, I do not anticipate many new departures; just a deepening of old lines of research and a continuation of my present, satisfying, personal and professional life-style.

But then, who knows?

Berkeley

IRMA ADELMAN

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