

From Engineering to Economics *

I. Introduction

Since this essay was first suggested to me by the editor of this series, my respect for autobiographical work has increased tremendously. I have spent long hours attempting to weave a homogeneous whole out of quite a heterogeneous mix of conceptual and chronological threads. To begin, in 1940 my professional life was launched as a petroleum engineer. I then proceeded by easy stages through branches of engineering and meteorology. Eventually I changed careers and went on to teach economics and to work on the economic problems of developing countries for most of my life.

In 1952, I had to decide on whether to resign from government service and take up an academic career. In the end I did both. I now believe that combining government and academic work is not only personally rewarding, but also beneficial for the quality of both professions. Since then I have alternated fairly equally among teaching, research, and government service. This sequence led to a degree of geographical specialization, mostly in "Newly Industrialized Countries": in my case, southern Italy, Greece, Turkey, South Korea, Israel, Taiwan, Colombia, and Chile. The academic phases of my life were spent mainly at Stanford (1952-61) and Harvard (1965-70; 1982-present); the governmental phases with the U.S. Economic Cooperation Administration (1949-53), the U.N. Economic Commission for Latin America (1957-58), the U.S. Agency for International Development (1961-65) and the World Bank (1970-1982).

Among the conceptual clusters that run through these phases, I have been intensively involved with production functions, interindustry

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The author collaborated with Hans Peter Lankes in relating the events described in this article. He extends special thanks to David Bell, Moshe Syrquin and Lance Taylor for their contributions as well as to his family, friends and many colleagues at the Harvard Institute for International Development for their valuable suggestions.

structure, programming and disequilibrium models, structural transformation, and development economics. Again, these topics are closely related. Most of my work under these headings shows some signs of engineering methodology. This is particularly the case of the first two, where specific assumptions are made about the nature of technology. More generally, my research has been cast methodologically within quantitative and structural approaches to economic policy, reflecting the early influence of Jan Tinbergen, Simon Kuznets and Wassily Leontief on my mode of reasoning.

Beyond that, the issues I worked on and the particular shaping they assumed were clearly products of their period. My efforts to resolve the difficulty of reconciling concepts and chronology are deeply embedded within the general evolution of policy making in developing countries during my professional lifetime. In a 1984 paper, "The Evolution of Development Planning", I attempted to trace the pattern of that evolution to the interaction among three factors that include accumulating experience of developing countries; a broadening of their economic and social objectives; and developing analytical techniques to evaluate alternative policies.

A sketch of the evolution of development planning is presented in Table 1. Even though I will not discuss it here, it represents a topical reference for much of the following.

II. Harvard: The Analysis of Production

Unlike most contributors to this series, I did not initially set out to become an economist or anything close to it. Instead I took undergraduate programs in mathematics, physics and engineering and then shifted to meteorology through the happenstance of wartime demands.

In 1935 I graduated from Hotchkiss, a solid New England prep school, and I expected to go to Yale along with many of my classmates. The family doctor advised otherwise, however, and I ended up at the University of Arizona. I have always been grateful for this bit of good luck, since Arizona's climate more than made up for any academic deficiencies. Even though the average course may have left something to be desired, it proved possible to put together a

TABLE 1

A SKETCH OF THE EVOLUTION OF DEVELOPMENT PLANNING

Rough dates	Analytical Techniques			LDC Experience
	Objectives & Constraints	Macro	Micro	
1950-1965	<i>Internal Constraints</i> Investment Savings Skills	<i>Closed Economy</i> Harrod-Donnar Input-output	Investment Criteria	Import substitution Low export growth Import limits
1960-1970	<i>External Constraints</i> Trade limit Aid growth	<i>Open Economy</i> Linear Programming Semi-input-output	Project Evaluation Efficiency prices	Increased export orientation Accelerated growth Growing inequality
1968-	<i>Multiple Objectives</i> Growth and equity	<i>CGE Models</i> Market simulation Additional instruments	Welfare prices Poverty focussed investments	Growing interdependence Oil shocks and instability
1975-	<i>Coping with Interdependence</i> External capital limits Structural disequilibrium	Models of structural adjustment Global modelling	Sources of productivity growth	Advantages of flexibility Lag of poor countries Rising unemployment

Source: HOLLIS B. CHENERY, "The Evolution of Development Planning", *HHID Development Discussion Paper No. 58*, December 1983.

major in mathematics and physics. More important, the Arizona's climate worked wonders on improving my sinusitis.

I graduated from the University of Arizona in 1939 with a respectable Bachelor of Science degree but it was clear that there was no future for me in pure science. In searching for alternatives, I identified engineering, my father's profession, and economics as preferred choices. By 1941 when the war broke out I had completed a second Bachelor of Science degree in petroleum engineering at the University of Oklahoma. The outbreak of war was enough to eliminate engineering as a career (at least as it is practiced by engineers) from further consideration. I did spend a year on the economics of engineering, however, which later proved to be quite rewarding.

This academic sequence had a rather eccentric effect on my professional career. My first job in 1941 was experimenting with the production of high-octane gasoline in an Oklahoma refinery. In 1942 I enlisted in the U.S. Air Force and the years from 1943 to 1945 were spent forecasting the weather for the Fifteenth Air Force in Italy.

Back in the U.S. in 1945, I finally made the switch to economics by registering in a Masters Program at the University of Virginia. When I received my Masters degree, one of my professors suggested that I apply to Harvard. I wrote to the admissions committee and received a letter from Professor Harold Burbank saying that Harvard was overcrowded and that I really was not properly prepared for graduate work. They suggested that if I really wanted to come, however, I should do so. Thus encouraged, I did.

Engineering Production Functions

In the permissive atmosphere of postwar Harvard I decided to reverse the normal sequence of graduate study and to make a virtue of my engineering background. The obvious starting point was the economist's concept of a production function that had been elaborated by Paul Samuelson in 1947. I spent some weeks trying to derive engineering equivalents of Samuelson's formulation, which I duly presented to a seminar directed by Wassily Leontief. Although my mathematics were hardly up to the task, the idea itself seemed promising. Leontief subsequently agreed to supervise my Ph.D. thesis on *Engineering Bases of Economic Analysis*, which was completed in 1949. In the same year I summarized its main ideas in my first article,

"Engineering Production Functions", published by the *Quarterly Journal of Economics*.

The essential difference between the engineer and the economist lies in the variables that each uses to describe the process of production. In the simple example of gas transmission that I used to illustrate the two approaches, the inputs into the engineering production function include pumps, pipe, energy sources, and skilled labor. The main outputs are the movement of natural gas over varying distances and at different pressures. Where the physical principles underlying the design of capital goods such as a pipeline have been well studied, the resulting "design laws" can be used to construct a production function linking inputs and outputs. The economist's cost function can then be derived by replacing some of the engineering variables with economic variables which conceptualize output as a function of types of capital goods, labor and raw materials.

The type of engineering production functions developed for the natural gas industry also shed some light on the pattern of investment over time. The accepted view was based on the acceleration principle, in which investment expands in proportion to the increase in demand. In industries where economies of scale are significant, however, the investment planning problem should incorporate the trade-off between the cost of carrying excess capacity and the lower unit production costs it permits. In a model with demand growth specified exogenously, a capacity cost function with constant elasticity, and the objective of minimizing the cost of production at the level of demand, I showed that it is more efficient to build capacity ahead of the growth of demand in order to take advantage of scale economies.

I tested this hypothesis for several industries for which capacity data were available. The central phenomenon was described as "optimal overcapacity" in "Overcapacity and the Acceleration Principle" published in *Econometrica* in 1952. Earlier in 1948 Richard Goodwin had developed a similar concept which he called a "flexible accelerator".

In later years, Alan Manne expanded the idea of optimal overcapacity in a planning context to include issues like stochastic demand and the choice between domestic production and imports. The same ideas also turn out to be of central importance in development planning in an interindustry or "linkage" context. In a 1959 article on the interdependence of investment decisions, I drew on

economies of scale and their impact on optimal investment over time to formalize and strengthen the proposition of development economists like Paul Rosenstein-Rodan, Ragnar Nurkse and others, that coordination among investment decisions can help realize pecuniary externalities and increase growth. I restated this idea for the coordination among sub-groups of industries, however, relying on inter-industry linkages, and not with a focus on aggregate demand as in the economy-wide "big push" theories.

In that article, external economies were not specified in the traditional Marshallian sense of costs and benefits of production not adequately reflected in the price system, but in a dynamic sense, referring to the potential effect of one investment on the profitability of another and therefore reflecting dynamic disequilibrium. In a context of permanent competitive general equilibrium – with prices reflecting demand and supply not only in present but also future markets and absent economies of scale – the pecuniary effects of one investment on another are part of the mechanism by which the market coordinates action among investors. When markets are not complete and the continuous adjustments needed to maintain competitive equilibrium are not assumed to take place – as is the case in the typical developing country – the private profitability of an investment viewed individually can understate its social desirability because feedbacks from the improved profitability of linked industries are not taken into account. These issues of coordinated movement over time, including the effects of economies of scale, were later taken up by Larry Westphal and again by neoclassical growth economists in an attempt to formalize the debates of the 1950s.

During my dissertation period, Leontief encouraged and motivated me and I felt comfortable with his engineering focus. As a result of my 1949 paper on engineering production functions I was invited to join Leontief's research group. At that time they were studying the structure of the American economy in the framework of his input-output system. (Later in 1952, together with Mathilda Holzman, I did empirical work on the input side of the system.)

Back in 1948, however, the European Recovery Program was underway and about the same time at Harvard I had completed the course requirements for a Ph.D. I was greatly tempted to postpone work on my thesis in order to join the Economic Cooperation Administration (ECA) in Paris which was in charge of implementing the Program. I soon discovered that this temptation was shared by

other budding economists. Fortunately my other thesis advisor, Ed Mason, persuaded me that the ECA would continue to be in business a year later, and this turned out to be the case.

III. The Marshall Plan and Italy: Applied Input-Output Analysis

The end of the war in Europe and the Far East left virtually all the participants in a state of economic disequilibrium. During most of the Marshall Plan period from 1948 to 1952 this condition was reflected in a series of bottlenecks that developed in basic industries, infrastructure, and above all foreign exchange.

The objectives of the Recovery Program were stated in terms of growth, structural change and viability. The richer countries – notably Britain, France, and the Netherlands – were identified as needing balance of payments support, while the poorer countries – Greece, Portugal, Turkey, and southern Italy – required loans and grants. These categories formed the basis for aid programs in other parts of the world and during later periods.

Unfortunately the data and the analytical tools needed to cope with these problems had not been elaborated. Neither the reigning Harrod-Domar approach nor neo-classical orthodoxy provided for foreign exchange as a source of growth. In fact, both implied perfect substitution of domestic and foreign savings at the margin. The Marshall Plan stipulated the basis for a variety of analytical work focussing in particular on new methods for planning, such as input-output analysis and linear programming. Many of the leading economists were drawn into it, including future Nobel Prize winners Ragnar Frisch, Jan Tinbergen, Richard Stone and Wassily Leontief. Intellectually I found that I had entered a fertile field wherein the chief innovation was a focus on economic structure.

In 1949 I arrived in Paris directly from Harvard for my first assignment with the ECA and worked for a year on loan programs for recovering countries. This was a fortunate learning opportunity for me. I was quickly immersed in the new kinds of problems typified by program lending and the use of counterpart funds. But for me the most fruitful phase began with my appointment as chief economist for both the State Department and ECA in Italy. It was during this

period that, together with Paul Clark, Vera Cao Pinna, and others, I began to experiment with the use of interindustry models to heighten the case for program lending.

The Italian Model

Earlier attempts to analyze the external requirements of European recovery had focussed on the need for imports by the countries receiving aid in the form of loans and grants authorized under the Recovery Program. These studies were essentially accounting exercises that tested the consistency between future demand and supply in major economic sectors. As the data gathered by the O.E.C.D. (then known as the O.E.E.C.) were improved, research groups such as our Italian one as well as groups in the Netherlands, the United Kingdom, the United States, Norway, and other countries began to construct input-output systems to improve these projections and to test the feasibility of alternatives. If the analyst maintained Leontief's assumption that input-output coefficients remain fairly constant for technological reasons, the input-output table could be used as a basis for projections or even for exercises in linear programming.

Since the basic model is "underdetermined" in final demands and sectoral production, the planner has a choice of specifying exports, imports or domestic production in each sector on the basis of additional assumptions that can be used to "close" the model. According to the Tinbergen framework of policy planning, these additional variables can be treated either as constraints such as a limit to the increase in agricultural production, or as proxies for policy instruments such as a required increase in exports. The values of the instrument variables can be varied systematically to determine the feasible range of combinations of policy variables.

Very little was published on input-output or interindustry analysis before the early 1950s although work was in progress in several countries. The existing state of the art came to light at the first European conference on input-output relations held by the Netherlands Economic Institute in 1950. A second conference took place in Bellagio in the same year. Some of the most original debates of the latter conference occurred while participants were crossing stormy Lake Como en route between our conference and our dinner lo-

cations. Usually it was Frisch and Leontief battling over the use of input-output systems in programming exercises.

In 1954, Paul Clark, Vera Cao Pinna and I presented "The Structure and Growth of the Italian Economy" at a conference in Varenna. Earlier in 1952, we had published a summary version in Italian that had been jointly financed by the Italian and American governments. We anticipated that its principal uses would be to test the balance among sectors in demand, investment and production.

This model had several distinctive features stemming from its data sources and intended uses. Due to the importance of the balance-of-payments constraint, it put much more attention on tradable commodities than non-tradables and on manufacturing rather than agriculture. The emphasis on trade was reflected in a rectangular structure in which there were many more commodities than sectors of production. When needed, separate constraints on sub-sectors such as natural gas or canned fruit could be added to offset the lack of price effects, which led to an association between this formulation and later "two-gap" models.

A later study, *Interindustry Economics*, by Paul Clark and me made use of the Italian research to develop a general approach to country-wide analysis. I also considered the advantages that would come from examining investment alternatives in an input-output framework. Since that time input-output analysis became a regular feature of the work of SVIMEZ (Associazione per lo Sviluppo dell'Industria nel Mezzogiorno) and other organizations specializing in the problems of the Italian Mezzogiorno.

The Difficult Days Before the Computer

During the 1950s input-output solutions required long hours to invert matrices of modest size. (Today computers would manage the task in seconds or minutes.) The high cost of compiling data by hand led to efforts to take advantage of quasi-triangular structures and other special features of the input-output system. Luckily for the analyst, circular feedbacks between industries were relatively infrequent, particularly in less developed countries in which the manufacturing of intermediate inputs was limited. Changes in final demand ripple through the economy from industries with higher to lower intermediate demand coefficients, limiting the need for endless

iterations. The tediousness of working with large databases by hand did have its advantages because the analyst became completely familiar with the ins and outs of the empirical characteristics of the underlying economies.

In 1958 I wrote a paper with Tsunehiko Watanabe that compared the interindustry structures of the United States, Italy, Norway and Japan. We ended up with a categorization of industries that was similar to the ones used today (without input-output frameworks) and was devised partly to limit computational deadweight. In order to generate triangular matrices, we classified industries by their degree of dependence on intermediate inputs versus value added and on intermediate demand versus final demand.

In spite of these difficulties, by the early 1960s input-output studies had been completed for a number of middle and lower income countries as well as for the U.S., Britain, the Netherlands, Japan and other advanced economies. The growing availability of input-output tables generated a number of spin-offs. Multi-country studies of the interindustry structure revealed sufficient similarities in the triangular structure of production to encourage further comparative analysis of technology and demand in the input-output framework, both among countries and over time. Input-output systems were also applied to examining investment alternatives in an interindustry framework and to demonstrating the rewards from coordination of investment decisions among linked industries. Finally, they have been used in the calculation of shadow prices for project evaluation.

IV. Stanford 1954-60: The Quantitative Approach to Development Planning

By 1953, I had left the ECA to join the faculty at Stanford University. My years at Stanford were fairly evenly divided among teaching, research and development consulting. At that time there was no accepted curriculum in development, although some universities offered graduate students opportunities to work in the field or to support advisory teams. In 1955 Ken Galbraith taught the first seminar in development at Harvard with later participation from Ed

Mason and other recruits from Harvard's Development Advisory Service. Paul Baran and I taught at Stanford.

As it was taught in the United States, the field of development economics emerged as a by-product of America's aid programs and, more generally, as a means of coming to terms with the post-war economy. The Keynesian revolution was in full swing stimulating a new synthesis of theory and policy. The war-torn economies of Western Europe were being rebuilt under the auspices of the Marshall Plan and a form of economic cooperation developed that suited the European participants.

Structuralism

The considerable mobility and exposure to country experience among practitioners in this new field was one of the factors responsible for divergences of development economics from mainstream economic theory. If one were to give a simple definition of development economics in its early stages, it would probably center on the notion that "structure matters", in *addition* to endowments and prices, although their importance was often underrepresented in practice. While the structuralist approach was admittedly not a neatly defined category, it did attempt to identify specific rigidities, lags, and other characteristics of the structure of developing economies that affect economic adjustments and the choice of development policy. A common theme in most of this work was the failure of the equilibrating mechanisms of the price system to produce steady growth or a desirable income distribution. Although this supposition was periodically questioned during subsequent decades, it has reemerged consistently in the wake of international economic disturbances that developing economies have found difficult to absorb. As I described in my article, "Development Policies for Southern Italy", if structure shows some rigidity and disequilibria arise among segments of the economy, then the change in structure itself can generate growth.

The methodology of structural analysis evolved from applying a set of rather intuitive hypotheses by Paul Rosenstein-Rodan, Ragnar Nurkse, Raul Prebisch, Gunnar Myrdal and W. Arthur Lewis to models of increasing empirical validity and analytical rigor. In contrast to neo-classical theory, structuralist analyses need not lead to

immediate policy conclusions. To deduce policy advice, they have to be embedded in explicit general equilibrium frameworks. For this purpose, most researchers have used either neo-classical models with particular structural features added or linear Leontief input-output models that exclude substitution. Either way, there is a natural affinity between structuralist *a priori* hypotheses and empirical and quantitative methods in development policy and planning.

The Stanford Project for Quantitative Research in Economic Development (SPQRED) and the Constant Elasticity of Substitution (CES) Function

My own contributions to the field focussed on applying quantitative methods by emphasizing the empirical analysis of structural hypotheses and regularities in development and by linking up development planning and mathematical programming. Beginning in 1953 I applied these techniques to a variety of areas within the framework of the SPQRED. For the next 10 years the SPQRED, organized by Henk Houthakker, Mo Abramovitz and me, provided the base for a series of comparative studies financed by the Ford Foundation. The conceptual framework was taken from Kuznets' research program and we also tried to combine insights from the interindustry work described previously with more specialized econometric analyses. This included Houthakker's comparative analyses of the structure of demand and Watanabe's test of the Constancy of Input-Output coefficients.

The best known work of the Center was "Capital-Labor Substitution and Economic Efficiency". By the time it was published in 1961 the article had spread beyond its original concern with development. The main participants in this research program were Kenneth Arrow, Bagisha Minhas and myself from Stanford and Bob Solow from MIT. Henk Houthakker, with whom I shared an office, was doing methodologically related work on consumption functions at the time, while others in the group were dealing with production systems. Henk would have been a natural member of the CES research team, but somehow there was a feeling that four people were already more than enough to generate one article. My main role was to balance the econometric studies so that they could be combined as a coherent whole. Whenever possible this objective was furthered by

the use of an input-output framework. Minhas and I developed the empirical side of the work while Arrow and Solow developed most of the theory.

In my opinion this study was most notable for its blend of theory and empiricism. Its point of departure was the uncomfortable co-existence of the assumptions of zero or unit substitution in economic theory (including the Harrod-Domar growth model and Samuelson's assumptions about the ranking of factor proportions in international trade) with the empirical evidence of widely varying degrees of substitutability in different types of production. Technological alternatives were numerous and flexible in some sectors and limited in others, and uniform substitutability was most unlikely. The difference in elasticities was confirmed by direct observation of capital-labor proportions which showed much more variation among countries in some sectors than in others.

Our mathematical formulation of a CES production function was tested against the fragmentary available information on the direct use of capital. In fact, by generating a relatively large and useful database on capital for his Ph.D. thesis, Minhas had stimulated the research that followed.

The Art of Collaboration

This research on the CES production function was the product of four people working together. Instead of competing for priorities we worked in cooperation to extend the existing methodologies and apply them to a theoretical frontier. In my opinion, the form of collaboration that evolved really allowed us to broaden the field and delve into a whole range of new issues. The whole was, indeed, greater than the sum of the parts. In over thirty-five years of professional life I have worked with a wide variety of colleagues more than half of the time. These temporary alliances were always rewarding and often led to enduring friendships. Although I am not aware of any systematic study of collaboration in economics, it is clear that the availability of collaborators is an important part of any research environment and deserves acknowledgment in a retrospective assessment.

Why do scholars collaborate? From casual observation I would suggest three typical situations that lead to joint research:

- (i) the traditional relationship of professors to more junior colleagues;
- (ii) the willingness and ability to share knowledge of different countries, industries or techniques which can contribute to comparisons of country experience;
- (iii) the complementarity of skills or techniques as in the combination of empirical and theoretical skills.

The main advantage of collaboration is to make possible research that might not otherwise be done. In my case this typically involved my working with a more theoretical partner and supplying empirical background and policy implications. To cite an extreme case, when Hirofumi Uzawa and I wrote our article on "Non-Linear Programming in Economic Development" in 1958, it was Uzawa who supplied the mathematics while I provided the words. The truly collaborative effort lay in the design of the analysis which drew on previous work of both Uzawa and myself. Other components of the research environment did play an important part in engendering this particular instance of collaboration. In 1954, Ken Arrow and I had shared an office at Stanford and he had advised me on mathematics and on how to go about recruiting the best research assistance. It was his suggestion that I work with Uzawa and that our study be undertaken.

Perhaps the most popular research mode in development economics is the comparative analysis of a selected group of countries. This approach had been pioneered by Kuznets with a research assistant as collaborator. Early examples of this type of collaboration in my own research experience resulted in joint papers with Watanabe and Lance Taylor. In 1975 when Moshe Syrquin collaborated with me in writing *Patterns of Development*, we were able to test a number of Kuznets' hypotheses and relied more heavily on econometric techniques. This has been a successful collaboration that has continued to the present. In fact, this article has been a major collaborative effort with family members, friends and colleagues.

The Harvard Advisory Group

While I was teaching at Stanford, I continued to maintain links with Harvard and travelled back and forth between the two coasts on

a regular basis. In 1954, I received an offer to join the newly established Harvard Advisory Group then working in Pakistan. This group was headed by Ed Mason and included several former colleagues such as Wassily Leontief whom I was happy to see again.

In the aftermath of his contribution to the creation of the Marshall Plan, Mason announced that he intended to shift his field of specialization from Industrial Organization to Economic Development. The contents of this shift remained to be determined but the Advisory Group in Pakistan was his first significant involvement with the reality of conditions in the underdeveloped countries.

When Mason died in 1992 at age 93, he was honored as one of the principals in the genesis of development assistance policies during the 1950s. He was one of the first to recognize that the U.S. as a developed nation has a responsibility to assist the less developed world. He was a founder of the Harvard Advisory Group for developing countries that later matured into the Harvard Development Advisory Service and ultimately evolved into the Harvard Institute for International Development.

Back in 1954, my own work with the Harvard Group in Pakistan began with a 3-month visit to Karachi. I was assigned to work on the first draft of Pakistan's embryonic Five-Year Plan scheduled to begin in 1955. With the assistance of a young Pakistani colleague, I concentrated principally on industrial structure and we were able to prepare a sketchy input-output table based on data collected from a limited census on manufactures. Although we had found these data in a basement awaiting tabulation, one of the main lessons of those early days in the effort to apply development planning methods was that countries like Pakistan were desperately short of reliable data.

Nevertheless, the first Pakistan Five-Year Plan accomplished a great deal. I agree with Ed Mason that the Plan broke new ground in giving top priority to agriculture at a time when the current fashion was to emphasize heavy industry. It also focussed upon the importance of public management and recognized the significance of rapid population growth. The process of planning itself was useful to the Pakistanis because it led to the generating and structuring of economic information and provided a consistent framework for defining priorities and identifying available instruments for development policy.

Personally, like most of those who were part of the Harvard Group in Pakistan, I learned a great deal from the experience.

During the course of several visits to Pakistan, I gained an understanding of the harsh realities that people from developing countries must work within. This experience was of great value to me in my later work.

Economic Commission for Latin America (ECLA)

During my Stanford years ECLA (known in Spanish as CEPAL) asked me to teach classes in development economics in Santiago. I spent three consecutive winters in Chile and soon grew accustomed to the rhythm of constant travel and was able to adjust easily from one hemisphere to another. I was able to use the time spent on the boat trip from San Francisco to Santiago to work on research without interruption. Once my young daughter, Teresa, was asked by a passenger what her father did for a living and she answered, "He puts little boxes into little squares". This was an accurate reply as I was working on ways to solve a non-linear input-output model and had actually developed something resembling non-linear programming. It was this research that led to my joint paper with Uzawa.

The planning course that I taught in Santiago became known for the high quality of its student participation. At that time there were few opportunities for students to study in this kind of program and each ECLA member country sent one student, usually a young official selected from the best and brightest. One of those outstanding students was David Ibarra, who later became Mexico's Finance Minister, and there were others who developed equally impressive careers. Many of the students had an engineering rather than an economics background and I ended up presenting course materials in a technical way by experimenting with existing planning models and attempting to extend them.

When I first came to Santiago, I barely spoke any Spanish. I did speak Italian, however, and at some point my students asked me to try lecturing in that language because they thought it would prove more beneficial for all of us. A real camaraderie developed among the students that led to enduring networks among this group of Latin American economists during the decades that followed.

I also served as an advisor to Raul Prebisch during the time when the battle lines with structuralism had been drawn not only in

the field of economic theory, but also in the politics of North and South. A leftist school of structuralism was quite prominent at ECLA and created a degree of animosity in relations with the United States. In the opinion of many more politically aware structuralists, such as Oswaldo Sunkel or Celso Furtado, my approach was too quantitative and empirical to be considered truly structuralist.

Prebisch himself was much less dogmatic than some have described him. Like Robert McNamara, whom I did not meet until the end of the 1960s, Prebisch was an extremely dominating personality. Like McNamara, he was intellectually very curious and could easily spend six hours at a stretch discussing an idea he found interesting and important enough to want to understand thoroughly. He was also interested in the neoclassical side of the story. He believed that the American government had exerted too much influence in too many institutions and occasionally I found myself to be the only American he wanted to have around him during important planning sessions. In one instance, before he gave a speech to the American Economic Association, he asked both Al Harberger and me to exchange views with him.

V. USAID 1961-65: Trade and the Programming of Foreign Assistance

Development economics was a product of the postwar period and in 1950 there were still very few courses in economic development and still fewer institutions devoted to development policy or research. By 1960, however, John F. Kennedy had been elected to the Presidency and his new administration set up the U.S. Agency for International Development (USAID) to administer the government's foreign aid program. I was offered the job of Chief Economist and later my friend, David Bell, became Administrator of the Agency. Emile Despres took over my teaching duties at Stanford and I was able to accept this new challenge.

As an arm of the U.S. government administration, USAID was obviously not free from political interference and Congress did interfere in certain projects, for instance, dealing with population control. For the most part, however, I was involved in several

research projects where I had a great deal of independence and excellent collaborators.

Program Lending and Two-Gap Methodology

An earlier premise of development policy that derived from the Harrod-Domar model of growth stated that capital and skilled labor constitute the key factors limiting growth. This premise had come to be overshadowed by the crucial importance of the balance of payments constraint and, thereafter, trade strategies and the importance of foreign financial assistance moved to the forefront of the discussion. During its early years, USAID spent at least 50% of its resources on program lending that could probably be best defined as non-project lending. This meant that loans were not connected to individual investments but to the overall plans and policies of the country. As in the Marshall Plan analysis, which was almost all program lending, the productivity of the provision of foreign exchange was of direct policy relevance. It is interesting to note that the renewed interest in program lending in the late 1970s and 1980s has had a somewhat different rationale, that is smoothing out adjustment to shocks and promoting dialogue on policy reform. These same elements were also evident in USAID strategies during the 1960s although, at that time, the main focus was on growth.

We had some money for research projects for improving the methods of programming capital inflows. Under the auspices of USAID Michael Bruno and I developed the "two-gap" methodology during the time that I consulted to the Bank of Israel in 1959. In addition to the elements that limit growth in an advanced country setting, our basic proposition was that the availability of foreign resources and other factors should be taken into account as potential constraints in developing countries. Back at USAID Alan Strout and I felt that even if sufficient savings were available, a country might not be able to transform them rapidly into the imported investment goods needed for growth because of limits to rapid export expansion. A developing country was often characterized by limited structural flexibility in the short- and medium-term and might not be able to change its productive structure sufficiently to meet the changing patterns of demand. This picture was very much in line with the thinking at ECLA at that time.

The economy characterized above can be described by a set of linear constraints for a period of, say 5 to 10 years, and only in the long run did neo-classical growth patterns hold, that is, only with the passage of time can we assume that domestic and foreign savings are substitutes at the margin. In a country attempting to transform its economy and grow without external assistance, any one of the mentioned limits could form a ceiling for growth, and the other factors will end up being underutilized.

At USAID we concluded that the effectiveness of foreign assistance could be measured by its use in relieving shortages in skills, savings and imported commodities. We were able to calculate the value of a capital inflow over short periods by its contribution to the increase in output resulting from the fuller use of domestic resources which this capital inflow made possible. By contrast, two-gap models were rather pessimistic about the possibilities for growth in the absence of large amounts of foreign resource inflows.

During the early 1960s Taiwan, Israel, Korea, Brazil, and Mexico had developed a sufficient industrial base to enable them to progress from an initial strategy of import substitution to the promotion of manufactured exports. The differences between structuralist and neoclassical analyses diminished once a diversified productive structure was achieved and these countries became less dependent on a handful of primary exports. To the extent that the export promotion strategy was made possible or successful because of the previous period of import substitution, I believe that the dichotomy between inward- and outward-oriented strategies has perhaps been overemphasized. The two strategies should rather be viewed as successive elements in an overall strategy designed to bring about changes in the structure of both production and trade.

In a 1961 article "Comparative Advantage and Development Policy", I advocated cautious support for certain types of import substitution policies under certain structural conditions. I outlined a series of assumptions about the theory of economic development that differed strongly from traditional trade theory. They included the basic structuralist tenet that factor prices do not necessarily reflect opportunity cost in a dual economy; that there might be externalities in production that result in changes in the quality and quantity of factors of production; that economies of scale can be important relative to the size of existing markets; and - the foundation of the theories of balanced growth - that complementarity among commodities is dominant in both producer and consumer markets.

Given these conditions, under which comparative advantage can change dynamically and endogenously, even perfect markets might not generate optimal outcomes in free trade. However, I did caution that non-optimality of free trade does not necessarily imply the optimality of trade intervention. By 1984 Anne Krueger had challenged my argument and argued that some of the structural rigidities can be accentuated by barriers to trade and that some of the potential dynamic factors that I had identified could actually be promoted by an openness of the trade regime and stymied by protection.

VI. Patterns of Development

In 1965 Harvard invited me to return as professor of economics and I accepted. I returned at a period of high interest in issues of economic development among students. With a million dollars in funding from USAID and the National Science Foundation we were able to reactivate the Project for Quantitative Research in Economic Development. A highly motivated group of recent graduate students that included Sam Bowles, Art MacEwan, Lance Taylor, Moshe Syrquin, Carl Gotsch and Larry Westphal were soon attracted to the Project. They had perceived a lack of hard numbers in the field of development economics and were quickly engrossed in the rigorous quantitative treatments the project was applying to it.

Many of the students and younger faculty on the project reflected the radicalization on campus in the late 1960s and, in fact, most of them were leftists. Personally I was not much interested in politics and perhaps this provided an environment for intellectual expansion. Over time, a Marxist study group was organized around Bowles, MacEwan, Gotsch and Tom Weiskoff and while the Dean advised me that the university was concerned about Bowles and MacEwan, they both received the Wells Prize for their excellent dissertations.

Intellectually, I would view my work at Harvard as expanding on a set of previously formulated ideas with the help of colleagues and students. First, Larry Westphal and I elaborated the increasing

returns and input-output nexus in a series of integer programming models. Then, the model I had elaborated with Uzawa led to papers with Bill Raduchel and ultimately fed into the development of computable general equilibrium (CGE) models. Art MacEwan and I continued two-gap work in a linear programming set-up with inputs from Robert Dorfman. Sam Bowles extended industrial planning in a linear programming framework to education. And finally Moshe Syrquin and Lance Taylor and I participated in the further elaboration of the "Patterns" approach to the analysis of development.

My interest in the process of industrialization extends back to my participation in the study of Italy in the early 1950s. Since then I have pursued the study of industrialization and other related development phenomena employing a methodology that evolved through the interaction of model formulation and empirical testing. This evolution covered four phases: *a)* a definition of the role played by industrialization in development in a formal interindustry model; *b)* an analysis of intercountry variations in the principal structural elements; *c)* the use of interindustry models of individual countries to explain the role of industrialization; and *d)* a generalization of the results through policy simulations.

The Input-Output Model of Industrialization

The generalizations of the findings of country-based models were based on studies of the uniformities of the underlying relations. Such studies were undertaken at the Stanford Project, including work by Houthakker and Williamson, the already mentioned analysis of interindustry flows that I did with Watanabe in 1958, as well as a study of patterns of production and imports in 1960. The latter started from a Walrasian framework and derived from it a reduced-type relation between the growth of sectoral output and the growth of per capita income. Another feature of this paper was a decomposition analysis of the proximate sources of industrialization, separating the effects of income expansion or demand from changes in the structure of import substitution. This paper together with the works of Kuznets and others conclusively established the association of the levels of income and industrialization across countries and over time.

Patterns of Development: An Econometric Approach

I continued to elaborate the ideas and the approach of the 1960 paper along two main lines. First, the econometric estimation of patterns of development across larger samples of countries and, secondly and more important over time as the relevant data became available, the refinement of the "sources of growth" methodology.

The early comparative studies of Kuznets were based on pure cross-section analysis which made the interpretation of results as growth equations a matter of speculation. The relation between time-series and cross-section estimates of industrial patterns was examined with Lance Taylor in 1968 and later with Moshe Syrquin in 1975. With some exception noted in those studies, the findings tended to justify the use of cross-section estimates as a means of describing the "stylized facts" of development.

In the 1975 study Moshe and I extended the econometric approach to a large set of the processes that characterize economic growth and were centered around the most likely to be included in a minimal definition of the structural transformation: accumulation of physical and human capital and shifts in the composition of demand, trade, output, and factor use. Also included were some socio-economic processes, such as urbanization, demographic transition and changes in income distribution, which appeared to be correlated with the level of development. The results of this study did not support a dichotomy between less developed and developed countries. Instead they suggested the concept of a transition from an economic structure representative of low income levels to one typical for high income countries. I continued to describe this transformation of the economic structure most elaborately in *Industrialization and Growth: A Comparative Study* with Sherman Robinson and Moshe Syrquin in 1986.

Application of the Industrialization Model to Individual Countries (Japan)

Refinement of the industrialization model has come from its application to a number of countries. In our attempts to generalize from the historical evolution of economic structures, industrializing countries received particular attention because they provided an opportunity to observe substantial changes over periods of several decades. There were a dozen or so countries in this category, in-

cluding Japan, Taiwan, Korea, Thailand, India, Colombia, Turkey and Israel. For each of these countries there have been historical studies covering 20 to 30 years in which the Leontief model provided a basis for comparisons. My initial attempt described the role of industrialization in Japan for the period from 1914 to 1954.

In 1956 I was invited to teach modelling in a Summer Program run by Stanford in Japan to give the Japanese a sampling of contemporary work being done in different fields. The Japanese Planning Office was by that time engaged in a large-scale effort to put together an input-output matrix and to improve historical series. The availability of these basic data made it possible to explore the relations between growth and structural change over a forty year period. To do so, I invited two Japanese scholars, Tsunehiko Watanabe and Shuntaro Shishido, to join with me in organizing such a study. The first result was to compare the historical results of the Kuznets type to the cross-country patterns of development. We subsequently analyzed the differences in productivity growth by sector, drawing on Leontief's work on the American economy. Later on, the Japanese results provided an independent test of the cross-country estimates of the CES production function.

The multisectoral growth model specified for the Japanese study proved to be particularly useful in the study of industrialization in general. The nearly universal phenomenon of rising shares of industrial output in GNP was traced to changes in trade patterns combined with changes in the composition of demand. Repetition of similar studies in other industrializing countries reinforced the conclusions drawn from the Japanese results.

Following the elaboration of the industrialization model as applied to Japan I attempted to combine the large samples and diverse experience captured in the econometric approach with the deeper structural specification afforded by a country-specific model. The result was a model of industrialization patterned after the Japanese model but calibrated around cross-country estimated relations and designed to answer questions related to the transformation of resource allocation in the course of the transition.

A preliminary version of this study was read at the Rome World Congress of the Econometric Society. In this early version, I tried to combine an input-output model focussed on the interrelations among demand, trade, production and employment, with a price-endogenous model with neoclassical specifications used to analyze the possibilities

for substitution among both commodities and factors. But the greater availability of data on commodity flows led to a more detailed specification of the input-output model and only to a more aggregated and illustrative treatment of the price-endogenous model.

Bill Raduchel and I expanded on my earlier approach with Uzawa for a treatment of substitution in an empirical interindustry framework. This work together with Johansen's influential 1960 study ultimately fed into the computable general equilibrium models (CGE) developed by Irma Adelman and Sherman Robinson in 1978 and later by Lance Taylor and Frank Lysy in 1979 at the World Bank, which in turn led to scores of applied CGE models all around the world.

Model-based Simulations of Structure and Policies

As mentioned above, the availability of data on commodity flows for a large number of countries led to my putting more emphasis on the input-output model of industrialization. The comparative or historical studies based on input-output tables can be regarded as an extension of structural analysis in which some parts of the economy are held constant. This corresponds to the idea of a second-best economy characterized by the imposition of constraints on the mobility of specific factors such as labor or natural resources. I have found this line of analysis intriguing because it provides a way of linking up technological findings, such as the relative growth of productivity in agriculture and manufacturing, with hypotheses as to international trade and consumer demand. Together with Moshe Syrquin in the 1980s, I applied this cross-country model to explore issues related to typical or expected changes in industrial structure, to identify the proximate sources of industrial growth and change, and to analyze the main sources of diversity in the patterns of structural change. This led us to establish a typology of development strategies that formed a basis for the analysis of the effects of different initial conditions and policies.

Patterns of Growth

A basic hypothesis of the comparative analysis of the structural transformation is that changes in structure and economic performance

are interrelated. Around 1970, at the PQRED at Harvard and in collaboration with Chris Sims and Hazel Elkington I undertook a study designed to go beyond the static associations of structure with the level of development. We intended to explore the major sources of differences in growth patterns and the relationship between the level of development and the rate of growth. Our approach combined suggestions from growth theory as to the likely sources of growth as well as from the development literature on structural change. Interestingly enough, some of the issues we analyzed were convergence, human capital, and scale effects that figured prominently in the "new" growth theory elaborated by Robert E. Lucas in 1988 and Paul Romer in 1986. The econometric results were not very conclusive but clearly suggested the importance of considering measures of human capital and exports as well as systematic differences in the underlying relations between less and more developed countries. A decade later I suggested that the Development Research Center at the World Bank which was under my jurisdiction at that time do some work along these lines. The result was Gershon Feder's contribution to *Industrialization and Growth* which gave a thorough presentation of the impact of export growth on output growth and of the likely channels of transmission.

VII. The World Bank: Managing Research

Robert McNamara

I first met Robert McNamara in 1968 when he was preparing to become the President of the World Bank just after he had left the U.S. Defense Department. He came to Harvard to be briefed on the essential features of development economics. Ed Mason, one of the founding fathers of the discipline, and I met with McNamara at the Kennedy School of Government at Harvard. We started talking early in the morning and McNamara began by asking questions about the basic aspects of development. After eight hours of fairly continuous discussion Ed and I felt we had covered the groundwork thoroughly. But our star pupil was still going strong and insisted on continuing. I later recognized that this was McNamara's typical approach to learning about areas that he was unfamiliar with.

He had developed a very systematic approach to dealing with new problems during his military service in World War II as an operations researcher, his stint as President of the Ford Motor Company and later his applications in the Pentagon as the Secretary of Defense. Whether it was the production of motor cars or a military strategy or the development of a country, he preferred to look at an overall analysis that he could grasp. He then asked that the analysis be expressed in quantitative terms wherever possible. This approach gave him the basic tools to comprehend possible options within a framework. Once I understood what he wanted, the demands seemed quite reasonable. Because of my own background and experience, this approach was both comprehensible and logical. We worked well together and I consider this period to be the most satisfying and productive of my life.

Chief Economist of the World Bank

Some months after our first meeting in 1968, I received a call from McNamara who had then been in office as President of the World Bank for six months. This time the message was brief. Would I be interested in the job of Chief Economist of the Bank? I went to Washington to meet with him and quickly accepted his offer. It was quite clear that the reason he was interested in me was to establish a strong quantitative research capability in the Bank. I think he saw a fit between our work on quantitative research at Harvard and how he wanted to analyze development problems.

As Chief Economist I was responsible for several functions at the Bank. I chaired the research committee, coordinated the Bank's research program and administered its budget. I was involved in the hiring of senior economists and was a member of the Bank President's council, a small group who collectively ran the Bank. I participated in managing the Bank's economic work and in setting policies. I traveled extensively to member countries and gave speeches and lectures on the Bank's behalf. Occasionally the *World Development Report* would publish a controversial item and I handled calls from the press. In sum, the job of Chief Economist turned out to be one of the most challenging of my life.

My first task was to develop a strong research staff and I wanted a team that was technically competent. A team of outside researchers were recruited as short-term consultants to enhance the experience of existing Bank staff who had been seconded from other departments. We soon had several multinational teams in place to perform either advisory or research functions. It seemed to me that the Bank's economic analysis was very micro-oriented and I suggested that a research committee be established that would represent varying economic viewpoints. Eventually this committee came to reflect a predominantly neo-classical approach although the views of Marxists, pragmatists and even a few who were not particularly ideological were represented.

When McNamara first came to the Bank the staff was largely Anglo-American. Oxford, Cambridge and the better American universities were the training grounds for Bank staff. But he wanted a highly competent group that would more readily reflect the Bank's international membership. It turned out that there were very few problems in recruiting a truly diverse community as most people welcomed the opportunity to work with us in Washington. McNamara also persuaded his management team to accept the new affirmative action initiatives and he was responsible for hiring some outstanding women for senior positions. In time he met all of his hiring goals and did not have to sacrifice excellence to do so.

My original contract with the Bank was for 2 years which was the standard period of time for a leave of absence from Harvard. I ended up staying in Washington for twelve years which was the maximum possible before my retirement.

Characteristics of Research at the Bank

The research committee was established to set priorities for and contribute ideas to the research program. It was this committee that formulated three general objectives to provide the Bank and its members with a broader view of the development process. We wanted to develop a basis for evaluating development objectives as well as to provide analysis of the relations among growth, poverty alleviation and external borrowing. We felt it was important to analyze trends in the world economy so that the Bank's support for country development programs could be seen in a broader perspective. And, we were committed to establishing a Bank-wide re-

search program that would encompass project and sector analysis as well as macroeconomic studies. A later objective that we felt needed to be spelled out explicitly stated that the research efforts of the developing countries deserved our support in generating their own analyses. We made a lot of progress in fulfilling these objectives although I still regard the fourth and final objective as a partial failure. On the one hand it was difficult to guarantee delivery on joint research contracts with institutions in developing countries, and on the other the Bank's organizational incentives did not really favor those who genuinely tried to support developing country institutions. Experienced personnel from different parts of the Bank such as Bela Belassa, Ernie Stern, Jean Baneth, and Herman van der Tak were particularly valuable as members of research committees.

The research committee approved larger research studies while smaller studies that were often exploratory in nature were undertaken by individual departments using their own resources. Although there were fewer larger studies, there were always several groups doing research. Four or five people from my office were assigned to oversee larger projects although I sometimes ran some myself such as "Redistribution with Growth" and "Patterns of Development".

We soon began thinking about how to set up an overall Bank research program to augment the many individual efforts of Bank-wide departments. Most of the research being done at the time was neither particularly organized nor well focussed. We identified the areas of comparative advantage or strength at the Bank as a research institution and it soon became apparent that the Bank was in a much better position to do certain kinds of research than private research institutions. This had a lot to do with its ability to gain access to country policy makers and issues. It routinely prepared economic reports on virtually all developing countries and, as a result, demonstrated a unique potential for comparative analysis.

One main purpose of research was to improve the Bank's capacity to provide policy advice and we agreed that the Bank should specialize in the applied end of the research spectrum. We soon began to use specific macroeconomic modelling techniques to analyze problems and provide solutions. Nevertheless, I felt that we needed to do a certain amount of more basic research that would include intensive data collection and analysis to sustain the flow of new ideas.

The friction generated between research and operational departments became a standard feature of bureaucratic life at the Bank. I felt it important that there be a good understanding between researchers and the people working on education or agriculture or lending policies. I tried to hire people with the capacity and willingness to bridge the gap between abstract thinking and the realities facing operations personnel. The Bank had one strong mechanism which worked in favor of avoiding problems of misunderstanding and that was its policy of rotating people. A high proportion of the Bank's senior staff had started as academic economists and there were also good opportunities for researchers to apply some of the research techniques that they had studied and worked on in the field. Many researchers were persuaded to become country economists and some of the most interesting analytical work was being done by those who had been researchers and were now looking into policy applications.

Our researchers were regularly seconded by operations and I felt that the Bank could learn a great deal from these crossover experiences. It became standard practice to include members from operational departments on the research committee, as well as on the various steering groups and review panels in order to increase the influence of their concerns. We asked them to submit research topics but often because of the pressure of their work they were not able to initiate many proposals. Later short training sessions on relevant research were introduced to update operational staff.

Ernie Stern was the first head of research at the Bank and I had known him previously as a senior administrator at USAID. He eventually became Vice President in charge of Operations after holding several other positions. There was always a pull towards operational departments because the rewards in terms of management and power were greater there.

By 1974 we had introduced a system for evaluating research projects. The research committee selected 12 major projects for evaluation and a year later the Bank's Board of Directors decided to extend evaluations to all completed research projects. The operations group participated and all members were encouraged to provide frank evaluations.

Research staff were routinely offered sabbatical leave and I participated in this program. I ended up going to teach at Stanford for three months where I was able to finish writing *Structural Change and Development Policy*.

Redistribution with Growth

In 1970, one of the main issues we identified as a focus of research was income distribution. The experience of strong growth in developing countries in the 1960s had shown that GDP growth could be quite independent of the condition of the poor. In many developing countries there was renewed interest in the relation between growth and distribution. At that time there was not a great deal of theory about what determines income distribution. The data describing changes in income distribution were quite poor and, so, one of the first things we commissioned was a research project to collect available data. We identified people working in the field and invited them to a series of seminars. I chaired these seminars and McNamara regularly attended and participated. He had a reputation for being rather shy in public and usually sat in the back row unless he was forced to come to the front of the room to respond to questions. But also characteristically he asked many questions.

One crucial debate occurred between Gus Ranis of Yale, who advocated a fairly orthodox view of how development works, and Dudley Seers of the Institute of Development Studies in Sussex, who argued for a more radical approach to distributional problems. Their discussion went on for an hour and a half and McNamara stayed for the entire period. Although he was rather quiet, he did ask a couple of questions. At the end of the seminar, I asked him who he thought had got the better of the debate. He had found the discussion interesting and, like me, thought that Dudley Seers presented the better case. He was attracted by a more activist approach to distributional problems although the Bank's view was more traditional. As Director of the Institute of Development Studies at the University of Sussex, Dudley invited me to participate in an international conference that was very popular and well attended. *Redistribution with Growth: An Approach to Policy* was eventually published jointly by the Institute and the World Bank from papers given by Montek Ahluwalia, C.L.G. Bell, John Duloy, Richard Jolly and me.

Early on the discussion about what the Bank ought to be doing focussed on individual countries. In 1971, Brazil was considered a critical country and typified the trickle-down approach to development in a successful setting. It was a rapidly growing country that

paid very little attention to income distribution and had deliberately adopted a policy of increasing its GNP as rapidly as possible while assuming that all sectors of the population would benefit. Our diagnosis indicated that this assumption was correct to a certain extent but it also showed that the upper income groups benefitted much more than the lower income groups. So, the first time McNamara spoke in public about distributional questions (to UNCTAD I believe), he mentioned Brazil as an example of a country which did not pay enough attention to income distribution. This remark provoked a very strong reaction from the Brazilians and quite a lot of applause from the other participants. After many internal discussions which served to sharpen our analysis, the Bank shifted its emphasis from agricultural projects to more rural development in Brazil.

Major issues for discussion were whether Bank policy ought to take account of income distribution in the choice of projects, in the way projects were designed, and in the macroeconomic dialogue. These changes in emphasis caused constant debates about the best way to design agricultural projects which were deemed potential areas for the scope of Bank influence on income distribution and on the pattern of development. Bank researchers were asking questions about the best ways to reach small farmers and to design research to determine whether small farmers were as efficient as large farmers. Some technicians asserted that there was a large trade-off and that lending money to small farmers would prove to be a much less productive strategy.

Monty Yudelman who was brought in from the O.E.C.D. to head the Bank's Agricultural Department had a broad approach to development as well as an interest in rural development and income distribution which his predecessor had not had. McNamara and Yudelman must be given credit for pushing this group to really back their claims. In the end, the results of our work proved that, under controlled conditions, small farmers could be as productive as large farmers. The subsequent evaluation of Bank loans to small farmers showed that projects designed to reach small farmers had perfectly adequate, if not outstanding, rates of return. We did not need to change the Bank's evaluation criteria to accommodate a shift in emphasis towards helping the rural poor.

The Bank's research committee then extended this kind of thinking in an effort to examine what was known as the "small is beautiful" theme to other sectors. This theme began to pervade a

substantial part of the Bank's more project-oriented research and, after three or four years, no longer needed much selling in-house. Staff were soon accustomed to the idea that there could be no trade-offs between growth and the fight against poverty.

Most of these ideas were presented in a 1974 publication *Redistribution with Growth: An Approach to Policy* wherein investment criteria are derived from a social welfare function expanded to include indices of distribution and poverty. The differences between the Bank's approach and the basic needs approach, which was developed at the International Labour Organization with a lot of exchanges of Bank research staff, were marginal in conceptual terms. In practice, however, the question of whether the Bank could maintain its project evaluation standards while adopting a focus on poverty by giving somewhat greater weight to the income of the poor was an ongoing subject of discussion.

We thought that it was perfectly feasible to find worthwhile projects in agriculture, education or housing that combined the objective of meeting basic needs but did not violate the Bank's insistence on a productive social return to the country concerned. Perhaps rightly, some advocates for the basic needs approach refused to do a calculation of the return because they felt that basic needs had a lot of external economies which could not be measured and that, as a result, the Bank's traditional cost-benefit analysis would not capture enough of the benefits.

Adjusting to the Oil Shocks

The oil shock of 1973 reactivated the basic concerns that I had about the relationship between trade and growth. The developing world was faced with the problem of adjusting its economic structure through import substitution, increasing exports and redirecting trade to accommodate the substantial worsening of its terms of trade. For most developing countries, the increase in prices for imported goods resulted in a return to a dominant trade gap and large capital inflows. These countries needed to give top priority to readjusting their economic structure in favor of higher exports which would permit a lessening of their financial dependency in the medium term. I made these points in a 1975 article, "Restructuring the World Economy" published by *Foreign Affairs*.

This situation was a challenge for both the development community and the Bank. To developing countries the cost of this one change in price was considerably greater than all the aid they were receiving from all other sources. It seemed to me that, as a development institution, we at the Bank had to focus on this new problem. Initially, we put together a task force that included energy and industrial experts as well as economists. We wanted to present an analysis of the situation that could be presented to the Bank's board of directors in three or four months time and devised the series of reports called "The Prospects for the Developing Countries". We prepared these reports for the next four years and every year it was a cliff-hanger as to whether the Board would accept our recommendations. The first year's report was strongly opposed by the American government because it was alleged to be pro-OPEC. The next year, it was the other way around when several of the OPEC countries protested that the report was pro-somebody-else.

We did not intend to publish these reports because we did not want to jeopardize their chances for obtaining the board's consent but there were regular leaks which resulted in headlines in the financial press that often inaccurately quoted some wild statements we were supposed to have made. It was in order to correct some of these ideas and to avoid the Bank having to make an official statement that I published the 1975 *Foreign Affairs* article.

World Development Report

During the late 1970s I worked with a small group that was coordinating what turned out to be a new function of the Bank staff. They believed their reports to the Bank's Board of Directors contained important and helpful information for both developing countries and aid donors and they soon began publishing them although they had not done so in the past. It was during this time that the Bank began to define a role for itself as a commentator on domestic and international issues of development policy.

By 1978, we had redesigned the "Prospects" papers to appeal to a broader audience and these were published as the *World Development Reports*. By then we had been doing the kind of analysis that the WDR was based upon for at least three years. Shortly after, a

separate unit was set up in the Bank to collect and oversee production of the *WDR*. The statistical appendix, which many regard as the best feature of this report, was actually McNamara's idea. He maintained that a collection of good data that had been examined and compiled in one place, on a comparable basis, and then published in a readable form and distributed widely would have a considerable impact on the world's thinking. I believe he was right. Whenever academics disagree on a particular issue, they will almost unanimously agree to use the Bank's numbers. The *World Development Report* is a perfect example of the kind of long-term report that the World Bank is uniquely qualified to prepare.

From his earliest days at the Bank, McNamara took a special interest in population work and gave several public lectures on the topic. By 1980 population and human resources was one of four leading categories for research funding. It was because of McNamara's interest that population projections were published in the *World Development Report*.

VIII. Looking Back: Theories and Practice

At one time one of the Bank research committees included two eminent scientists whose methodologies could hardly have been more different. As an empiricist, Simon Kuznets thought it did not make much sense to theorize without data and relied on the honesty of numbers to reach conclusions. He would abstain from any conclusions on almost any topic if they had not been sounded out empirically across every conceivable dimension. On the other hand, Sir Arthur Lewis was a man of intuition and insight, with much less use for numbers. Once when he was asked if the Bank's research budget was adequate, he replied that it was about right. He felt that the best approach was simply to have a few good people thinking.

There is no doubt that Kuznets' methods and research programs have had a lot of appeal for me. While I have followed Kuznets a long way in my methodology, I was somewhat less patient and preferred to be able to put research to rapid use. I have also felt a need to supplement the austerity of empirical work with simple but

useful theories that would not have been possible without some amount of *a priori* theorizing of the practical type that Lewis stood for.

The combination of the theoretical, the empirical and the practical was perhaps best embodied by Jan Tinbergen, whom I consider to come closest to my ideal of an economist. I was exposed to his way of looking at things very early on in my career as an economist. When I was living in Paris in 1949 I wrote to Tinbergen and he invited me to visit him in Rotterdam where he has his research center. We spoke at length about how best to expand research and afterwards each of us sponsored conferences on input-output analysis which was a novel topic at the time. We kept in touch over the years and I had hoped to include him as a member of the Bank's research committee but by that time it would have implied too much travel for him. His reflective approach to development planning sets the priorities straight and then proceeds to organize national efforts around fundamental goals. It has contributed to improved economic policies in many countries and has greatly influenced my own approach to development planning.

As I mentioned before, if there is a common denominator in "structuralism" – as diverse as its definitions may otherwise be – it is certainly that economic rigidities exist and matter. Those rigidities – or in Tinbergen's framework, constraints – are what motivates planning and provide a role for government. In reviewing the sketch of the evolution of development planning as described in Table 1, it appears that future rows of the diagram might well reveal that long-term planning during the last decade has not been high on anybody's agenda. In fact, I do not believe that we have reached the end of development planning or indeed of development as a process. The same themes, and probably the same methods, will come up again. What I interpret to be a leading indicator for renewed interest in this area, after a decade or more of absence, the economics of growth appear to be back on the academic agenda.

Cambridge, Ma.

HOLLIS CHENERY

REFERENCES

- ADELMAN, I. and S. ROBINSON (1978), *Income Distribution Policy in Developing Countries: A Case Study of Korea*, Stanford, CA: Stanford University Press; Oxford University Press.
- ARROW, K. with H.B. CHENERY, B. MINHAS and R. SOLOW (1961), "Capital-Labor Substitution and Economic Efficiency", *Review of Economics and Statistics*, August 1961, Volume 43, No. 3 (225-250).
- CHENERY, H.B. (1949), "Engineering Production Functions", *Quarterly Journal of Economics*, November 1949, Volume 63, No. 4 (507-531).
- CHENERY, H.B. (1952), "Overcapacity and the Acceleration Principle", *Econometrica*, January 1952, Volume 20, No. 1 (1-28).
- CHENERY, H.B. (1953), "Process and Production Functions from Engineering Data", in Leontief and others (eds.), *Studies in the Structure of the American Economy*, New York: Oxford University Press.
- CHENERY, H.B. with PAUL CLARK, and V. CAO PINNA (1953), *The Structure and Growth of the Italian Economy*, U.S. Mutual Security Agency, Rome, 1953 (published in Italian in *Industria*, 1952, No. 4; 1953, Nos. 1, 3).
- CHENERY, H.B. with T. WATANABE (1958), "International Comparisons of the Structure of Production", *Econometrica*, October 1958, Volume 26 (487-521).
- CHENERY, H.B. with H. UZAWA (1958), "Non-Linear Programming in Economic Development", in K. Arrow, and L. Hurwicz (eds.), *Studies in Linear and Non-Linear Programming*, Stanford, CA: Stanford University Press.
- CHENERY, H.B. (1959), "The Interdependence of Investment Decisions", in Abromovitz, M. et al. (eds.), *The Allocation of Economic Resources*, Stanford, CA: Stanford University Press.
- CHENERY, H.B. with PAUL CLARK (1959), *Interindustry Economics*, New York: John Wiley.
- CHENERY, H.B. (1960), "Patterns of Industrial Growth", *American Economic Review*, September 1960, Volume 50, No. 4 (624-654).
- CHENERY, H.B. (1961), "Comparative Advantage and Development Policy", *American Economic Review*, March 1961, Volume 51, No. 1 (18-51).
- CHENERY, H.B. (1962), "Development Policies for Southern Italy", *Quarterly Journal of Economics*, November 1962, Volume 76, No. 4 (517-547).
- CHENERY, H.B. with S. SHISHIDO and T. WATANABE (1962), "The Pattern of Japanese Growth, 1914-1954", *Econometrica*, January 1962, Volume 30, No. 1 (98-139).
- CHENERY, H.B. with A.M. STROUT (1966), "Foreign Assistance and Economic Development", *American Economic Review*, September 1966 (revised version of No. 31), Volume 56 (679-733).
- CHENERY, H.B. with LANCE TAYLOR (1968), "Development Patterns: Among Countries and Over Time", *The Review of Economics and Statistics*, Volume L, No. 4 (391-416).
- CHENERY, H.B. with N. AHLUWALIA, C. BELL, J. DULOY, and R. JOLLY (1974), *Redistribution with Growth: An Approach to Policy*, London: Oxford University Press.
- CHENERY, H.B. (1975), "Restructuring the World Economy", in *Foreign Affairs*, January 1975, Volume 53, No. 2 (242-263).
- CHENERY, H.B. with MOSHE SYRQUIN (1975), *Patterns of Development, 1950-1970*, New York: Oxford University Press.
- CHENERY, H.B. (1975), "The Structuralist Approach to Development Policy", in *American Economic Association Papers and Proceedings*, 65 (May 24, 1975), (310-316).
- CHENERY, H.B. (1979), *Structural Change and Development Policy*, New York: Oxford University Press for the World Bank.
- CHENERY, H.B. with MOISES SYRQUIN (1980), "A Comparative Analysis of Industrial Growth", in R.C.O. Matthews (ed.), *Measurement, History, and Factors of Economic Growth*, New York: Macmillan.
- CHENERY, H.B. (1984), "The Evolution of Development Planning", *Journal of Policy Modeling*, Volume 6, No. 2 (159-174).
- CHENERY, H.B. with S. ROBINSON and M. SYRQUIN (1986), *Industrialization and Growth: A Comparative Study*, New York: Oxford University Press.
- CHENERY, H.B. with MOSHE SYRQUIN (1989), *Patterns of Development, 1950 to 1983*, World Bank Discussion Paper 41, Washington DC: The World Bank.
- HARVARD INSTITUTE FOR INTERNATIONAL DEVELOPMENT, HARVARD UNIVERSITY, and UNIVERSITY PRESS OF AMERICA (1986), *The Harvard Institute for International Development and its Antecedents*, Edward S. Mason, Lanham, MD.
- JOHANSEN, L. (1960), *A Multisectoral Study of Economic Growth*, Amsterdam: North-Holland.
- KRUEGER, ANNE (1984), "Comparative Advantage and Development Policy Twenty Years Later", in M. Syrquin, L. Taylor, and L.E. Westphal (eds.), *Economic Structure and Performance: Essays in Honor of Hollis B. Chenery*, New York: Academic.
- KUZNETS, SIMON assisted by L. EPSTEIN and E. JENKS (1947), *National Income and Its Composition, 1919-38*, New York: National Bureau of Economic Research.
- LUCAS, ROBERT E., JR. (1988), "On the Mechanics of Economic Development", *Journal of Monetary Economics*, 22 (3-42).
- MANNE, A.S. (1961), "Capacity Expansion and Probabilistic Growth", *Econometrica*, Vol. 24 (632-649).
- ROMER, PAUL M. (1986), "Increasing Returns and Long-Run Growth", *Journal of Political Economy*, 99 (3-42).
- SAMUELSON, PAUL (1947), *Foundation of Economic Analysis*, Cambridge, MA: Harvard University Press.
- TAYLOR, LANCE and F.J. LYSY (1979), "Vanishing Income Redistributions: Keynesian Clues about Model Surprises in the Short Run", *Journal of Development Economics*, No. 1 (11-30).
- WESTPHAL, LARRY (1975), "Planning with Economies of Scale", in C.R. Blitzer et al. (eds.), *Economy-wide Models and Development Planning*, New York: Oxford University Press.
- (THE) WORLD BANK (1981), *The Research Program of the World Bank*, July 1982, Washington, DC.
- (THE) WORLD BANK/IFC ARCHIVES ORAL HISTORY PROGRAM (1983), "Transcript of an interview with Hollis Chenery by Robert Asher", January 27, 1983, Washington, DC.