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CENTER DISCUSSION PAPER No. 44

IS ECONOMIC DEVELOPMENT A SUBJECT?

Lloyd G. Reynolds

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Is Economic Development a Subject?

Lloyd G. Reynolds

A striking feature of postwar economics has been the "development boom." In 1945 anyone scanning library shelves for a book on economic development would have found only Schumpeter. There was probably not a single university course under this heading. Today there are dozens of such courses, several standard textbooks, scores of monographs, hundreds of articles and government reports. Economic development is among the two or three most popular specialties for students of economics.

Yet it is not at all clear that economic development constitutes a distinct subject. Specialists in older branches of our science view this intruder in the curriculum with considerable reserve. There is good reason for this skepticism. Work on the less developed countries has been heavily policy-oriented rather than scientifically oriented, more concerned with projecting the future than with understanding the present and the past. Nor is there any body of theory about early economic growth, comparable to the analytical tools available to the student of public finance or international trade.

It may be useful, therefore, to look at the work of the past twenty years in perspective. Is economic development a subject, actual or potential? If potential, what shape can be discerned in the present embryo?

A brief comment is needed on terminology. Geographically, development economists are concerned with the countries of Latin America, Asia, and Africa (excepting South Africa and Japan). The least bad term for this group of countries is perhaps "less developed countries" or LDC's, which seems to have come into increasing use. The remaining nations--Australasia, South Africa, Japan, North America, Europe including the U. S. S. R.--should then be termed "more developed countries"(MDC's). This is, of course, a rank ordering rather than a simple dichotomy. Nations toward the top of the LDC list will

gradually move over into the MDC category (as, for example, Israel, Greece, Yugoslavia, Mexico). Nor is the ranking simply, or even primarily, in terms of per capita output. We place Japan in the MDC group not because its per capita output is high, but because its politico-economic organization is "modern" and because its economic growth has been sustained over a long period.

Students of long-term change in the MDC's usually say they are analyzing economic growth, while work on the LDC's is usually labeled economic development. This is not a satisfactory distinction. Surely poor countries as well as rich countries may experience economic growth. It does not seem useful to say that the British economy was developing between 1750 and 1800, but growing between 1900 and 1950. The term "development" is ambiguous also because of its strong activist connotations.

It seems best, therefore, to use growth to cover scientific analysis and development to cover policies aimed at initiating or accelerating growth. Growth (or its absence) is something one can measure, describe and try to explain. Development, which involves target-setting and policy formation, is something one tries to achieve. An understanding of how economic growth occurs is naturally useful in framing development policies. But confusion of the two activities can only work mischief.

The problem of this paper can now be posed more precisely: does economic analysis of today's LDC's, including (but not limited to) study of early economic growth in these countries, constitute a distinct kind of work within economics?

We shall approach the problem from three directions: first, do we now have plausible hypotheses about early economic growth in the LDC's? Second, what lines of research are presently underway or might usefully be undertaken, on these economies? Third, does this work involve merely application of established tools of "Western economics," or does it involve a good deal of new tool-building? A finding that fresh theorizing is necessary

would strengthen the view that here is indeed a valid scientific specialty.

I. Old Dogmas and New Hypotheses

The roots of the "development boom" were not primarily intellectual. Problems preceded theory, as has typically been true in economics. Between 1945 and 1960 a dozen colonial areas in Asia and two dozen in Africa achieved independent status. The United Nations provided a forum in which these new nations, along with older nations of Asia and Latin America, could voice their problems and needs. The dramatic income gap between richer and poorer nations, which has continued to widen over the past generation, pricked many consciences in the richer countries. Political rivalry among the industrial nations, and particularly between the United States and the U. S. S. R. , placed the poor countries in a favorable position to bargain for economic assistance.

The spectacular economic recovery of Western Europe in the wake of Marshall Plan aid encouraged a facile assumption that American capital could work equal miracles in other parts of the world. As loans and grants to Europe tapered off, loans and grants to the less developed countries were phased into an expanding U. S. aid programme. Almost by oversight the United States found itself in the business of promoting economic growth throughout the world, but with little knowledge of the economies which were supposedly to be transformed. As the 'fifties wore on there was an increasing flow of loans and grants from Britain, Western Europe, the U. S. S. R. and the East European countries, and from international organizations.

More or less simultaneously, there developed during the nineteen fifties a set of ideas which helped to rationalize these ongoing programs while also passing in some academic circles as a theory of early economic growth. The cardinal points of the older orthodoxy may be set out as follows:

1. There is a category of "underdeveloped countries," sufficiently similar to warrant a general diagnosis and prescription.
2. The people of these countries, or key leadership groups in these countries, have a strong interest in raising per capita output.
3. The factor limiting the rate of economic growth is typically a shortage of capital.
4. The relation between increments of capital and increments of output can be taken as reasonably constant, which enables one to transform output targets into capital requirements.
5. Domestic saving is typically insufficient to finance a desirable growth rate.
6. The gap between domestic savings and required capital formation can, however, be filled by capital transfers from abroad.
7. Given adequate aid at the outset, a country's need for aid will eventually diminish and it will "take off" into self-sustained growth.

This set of ideas was optimistic, policy-oriented, preoccupied with capital requirements, and fortified by casual empiricism. It visualized a future in which one after another of the poor nations will have "taken off," propelled by relatively short bursts of aid from the richer countries. Whatever its policy uses, its scientific effect was stultifying, for it announced that we already knew the answers when in fact serious research had scarcely begun.

This view of the world appears most clearly in the writings of W.W. Rostow and Paul Rosenstein-Rodan. In a remarkable article published in 1961,¹ Professor Rosenstein-Rodan projected the feasible growth rate of 81 LDC's (including such well-documented regions as

¹ Paul Rosenstein-Rodan, "International Aid for Underdeveloped Countries," Review of Economics and Statistics, May 1961, pp. 107-138.

Laos and Nepal!) from 1961 to 1976. These growth rates were then converted into capital requirements by using a standard 3:1 incremental capital-output ratio. Future domestic saving in each country was then projected. Estimates of future marginal savings rates were based on 1961 information concerning average savings rates,¹ which was apparently accepted as reliable. Finally, domestic saving was deducted from capital requirements to yield the "necessary capital inflow."

This kind of political arithmetic is doubtless necessary in government departments. It is surprising only that it should have been presented as a serious piece of economic research.

The Rostow "take-off" concept requires little comment, because it has already come under heavy and justified criticism. As regards the MDC's, things seem simply not to have happened as Rostow surmised. To be sure, careful reading of the original article² reveals that he committed himself to little in the way of testable hypotheses. On the few points where he did venture quantitative generalizations--rates of capital formation, rates of output increase--the evidence runs counter to his hypotheses. Professor Kuznets' conclusions on this matter carry weight:

¹"In the majority of cases the marginal savings rate was assumed as roughly twice as high as the average rate" (p. 136). Since the article appeared in 1961, the 1961 base-year "data" presented on national output, population, savings and other variables cannot have been actual measurements. As nearly as one can determine from footnotes, the 1961 figures were projections by the author from actual data for 1957, drawn mainly from the U.N. National Accounts Yearbook, 1959.

² W.W. Rostow, "Take-off into Self-Sustained Growth," Economic Journal. Subsequently, the doctrine was enlarged and somewhat vulgarized in a small book (W.W. Rostow, The Stages of Economic Growth, Cambridge: Cambridge University Press, 1960. Still later, the issues were discussed thoroughly and quite critically at a conference organized by the International Economic Association (see W.W. Rostow (ed.), The Economics of the Take-Off, New York: Martin's Press, 1963.

"The capital formation rates, if they rise, climb at a sustained rate and for a much longer period than the two or three decades of take-off. Rates of growth of total product, if they show any long-term acceleration (and those for only a few countries do within the period beginning with the take-off stage) increase slowly and certainly over a longer period than the short span of the take-off. . . I can only conclude that the available evidence lends no support to Professor Rostow's suggestions."¹ This view is reinforced by the detailed analysis of early economic growth in Britain, France, and other countries presented at the I. E. A. conference.

As regards the LDC's, Rostow asserted, on the basis of a few years' data from the early 'fifties, that some of them were "attempting take-off." Such a statement surely has little meaning. The experience of repeated take-offs and landings in some countries since 1950 suggests a helicopter rather than a jet aircraft simile. Bicanic' notion that nations creep painfully over the threshold of economic development is even more appropriate.

If the earlier orthodoxy has collapsed, what remains? What do we actually know about early economic growth? Surely very little. We know little even about early growth in the older industrial countries, on which economic historians have been working for generations. We have little idea how far--if at all--conclusions drawn from 18th and 19th century growth are applicable to a quite different range of economies in the late twentieth century. We certainly do not have a long enough record for today's LDC's to determine which of them have embarked on a sustained growth path and how this happened.

This is not to say that we are without ideas. It will be useful to advance a few hypotheses about early economic growth in today's LDC's--ideas which at some points run directly counter to the traditional doctrine. Let it be clear, however, that these are merely hypotheses, which can be tested only by much additional research.

¹ Simon Kuznets, "Notes on the take-off," in W. W. Rostow (ed.), op. cit.

1. Economic growth is not homogeneous. The countries which we label LDC's are quite heterogeneous. No generalizations about economic structure and behavior apply equally to all. If one looked carefully at the MDC's in the era when their accelerated growth began, their heterogeneity would doubtless appear equally great. Consider England in 1750, the U.S.A. in 1830, Japan in 1870. Growth itself gradually smooths out many differences and produces considerable resemblance among "mature" economies; but this is not true in the incipient stages.

If countries enter on economic growth with differing internal structures and under different external circumstances, it follows that what happens in the early stages of growth will differ from one case to the next. True, output per capita rises, capital formation rises as a percentage of national product, and so on. But this is purely definitional--this is what we mean by economic growth. It does not indicate that the initial factor endowment or the stimuli to growth, or the leading and lagging sectors, or the attendant institutional transformation were similar from case to case. One should not expect, then, to arrive at a single theory of early economic growth.

2. Economic growth is gradual. It is not accomplished by a single "big push," nor is it compressed into a Rostovian take-off period of two to three decades. Rather, output per capita at first rises slowly, sometimes almost imperceptibly. The growth rate then gradually increases, and so does the capital formation rate, though there is no indication of a close relation between increments of capital and output. This acceleration continues for perhaps 50 to 75 years before the growth rate, the capital formation rate, and (possibly) the rate of population increase settle down on a kind of plateau. To a mediaeval historian 75 years may seem a short period; but it is much longer than the "take-off" periods visualized in much of the development literature.

Reasons for the inevitability of gradualness are not hard to find. In a new country it may take decades to establish secure political leadership, orderly procedures for the transfer of power, internal law and order, and other prerequisites for economic progress. Gradualness is inherent also in the time required to lay down the physical infrastructure of a modern economy, and in the subsequent lag before other productive activities have "grown up" to the point of utilizing these facilities fully. Perhaps equally important is the slow turnover of human populations. It is a truism that the most important product of economic modernization is a different kind of person, and that this different person is required for effective operation of the new facilities. If one starts today to educate all children aged 6, it will be twenty years before these children have reached peak productive efficiency. The higher the occupational level, the longer the gestation period. It may be thirty or forty years before highly-educated business managers, political leaders, agriculturalists and civil servants have taken over from their less well educated forbears.

It is often said that poor nations today are determined to develop more rapidly than their predecessors, and optimistic projections are often embodied in "perspective plans." But future projections are less persuasive than past accomplishments. Where are the LDC's which have succeeded in modernizing their economies and achieving a sustained growth rate of, say, 2 percent per capita per year in less time than was required by the richer nations? One can point to Israel and Taiwan, but these are special cases. Both countries imported large quantities of human capital--administrators, business men, technicians, teachers--in the first case from Europe, and in the second from mainland China. Both countries received foreign funds which, relative to their small populations, were very large. In order not to grow, they would have had to be remarkably wasteful and inept. The growth of these two countries resembles the postwar reconstruction of Japan and Western Europe which,

given a rich endowment of human capital plus substantial imports of physical capital, were able to restore their productive capacity in a remarkably short time.

3. Economic growth depends mainly on internal effort. The classic cases of almost completely self-financed development are Britain, Japan, and more recently the U. S. S. R. Some of the European countries, such as Sweden, received limited amounts of capital from abroad. The most substantial nineteenth-century capital movements, however, were to the frontiers of settlement in the United States and the British Dominions. This was part of a vast transfer of human beings as well as capital goods, combined with continuous settlement of new land and exploitation of additional natural resources. Had the foreign capital component been missing, expansion might have been slower, but it is very unlikely that it would have been stopped. The willingness of British bankers to market American railroad bonds can scarcely be considered the key to the dramatic expansion of the American economy.

It would be useful to compare contemporary growth rates in the LDC's (which are themselves difficult to determine¹) and to correlate these with various measures of foreign capital inflow. Even if such an exercise yielded a positive relation, one could not infer that the foreign capital was the source of more rapid growth. It is more likely that countries with a superior institutional framework and internal leadership, which are able on this account to grow faster than others, are considered superior credit risks and are able to attract larger amounts of foreign funds. Capital typically flows toward those who need it least.

¹ Estimates of gross national product by urban economists and statisticians are not reliable in a primarily agricultural economy where production data are poor and most of the product is non-marketed. It may even be that the concept of GNP, which was developed under modern American conditions, is a "rich country gadget." Greater importance should be attached to physical output series for major products, and in the absence of these no firm conclusions can be drawn.

4. Economic growth involved a diversified increase in output across a broad front. Some lines of production naturally grow faster than others, and there may even be a sensational spurt in one field (British cotton goods, 1780-1820, or Swedish timber output, 1830-1880). But these are not "leading sectors" in the sense of an engine pulling an inert mass. Rapid growth in one sector both requires and encourages growth in related activities. Unless conditions in the economy are broadly favorable to growth, as they were in the British and Swedish cases, expansion in a single sector will prove abortive. The colonial type of enclave economy is no exception. While a substantial rate of expansion may be achieved within the enclave, this typically fails to communicate itself to the mass of the population and to activate a general growth process.

It is now generally recognized that increases in agricultural output are an indispensable feature of early economic growth. The industrialization of the MDC's was typically accompanied or preceded by substantial agricultural progress. In today's LDC's, too, the behavior of agriculture is the best single test of growth capacity. The reason is not just that higher agricultural output is essential in an expanding economy, but also that inability to activate agriculture is symptomatic of weaknesses in government leadership and administration. Anyone can order a steel mill, but the intransigent problems of agriculture are a crucial test of innovational ability.

5. Output expansion is accomplished initially by absorbing previously unused resources. In the conventional view economic growth is being held back by resource scarcities, and particularly by a shortage of capital. It is more accurate to say that, in a stagnant economy, the resources needed for a higher level of output are present but under-utilized. There is often cultivable land which is not already under cultivation. Many workers may be unemployed or underemployed, and can be induced to work longer hours in industry or

agriculture. Management and entrepreneurship may also be present but under-utilized. Nothing is more striking in the early growth history of the advanced countries than the ease with which merchants moved over into manufacturing, banking and other new activities as opportunities for profit appeared. They ended up working harder, making larger profits, contributing more to production.

There are also indications that many LDC's have unused saving capacity. It is not that investment is being held back by unwillingness to save. Rather, people who could save perceive only limited investment opportunities attended by high risk. If the outlook changes and the prospective yield of capital rises, saving will be undertaken. There is also unused saving potential in the fiscal mechanism. Lewis has argued that no country is too poor to devote, say, 20 percent of national output to public sector purposes. If actual revenues are small, and if the proportion of revenue devoted to investment is also small, the main reason may be weak government and poor public administration.

The view that capital shortage is not a major barrier to early economic growth is supported also by the experience of the MDC's. Close students of early industrialization in Britain and Western Europe are of the opinion that finance was not a serious limitation on industrial development. Habbakuk, for example, states that perceived investment opportunities typically generated the necessary capital, rather than vice versa.¹ Deane and Cole ~~exp~~ress the opinion, with respect to early eighteenth-century England, that "the limiting factors to an increase in capital formation seem to have operated more from the side of investment than from the side of saving."²

¹ See in particular his essay on "The historical experience on the basic conditions of economic progress," in L. Dupriez (ed.), Economic Progress (Louvain: Institut de Recherches Economiques et Sociales, 1955)

² Dupriez (ed.), op. cit., p. 260

If there is any basic scarcity in the LDC's it is a scarcity of leadership, of ability to innovate in both the private and public sectors. This limitation has been heavily, and in our view correctly, emphasized by Hagen and Hirschman.

6. Economic growth involves a transformation of politico-economic institutions. The relevant economic institutions are those which affect factor and product markets. In labor markets this includes adequate training facilities, encouragement of desirable mobility, and modernized wage-setting practices. In capital markets it includes private and public banks, insurance companies and other savings institutions. In industry and commerce it means a gradual superseding of the small family business by large enterprise and professional management. In agriculture it means marketing facilities, sources of credit, availability of "modern" inputs, and technical assistance.

It is unnecessary for a country to have a full panoply of such institutions before growth can begin. It need not have a stock exchange, or a social security system, or much corporate enterprise. To a large extent development of "modern" economic institutions is a by-product of economic growth. It was so in the older industrial countries, and it will be so in the LDC's. At the same time, some minimum institutional base must exist quite early. The make-up of this minimum base is surely one of the key problems in growth economics.

The governmental structure must also be broadly favorable before economic growth can begin. But again, we do not know what this means in concrete terms. Economists have skirted the issue and political scientists have not met it head-on. Moreover, orientation of government toward economic objectives, and improvement of its technical efficiency to attain those objectives, seems to be in good measure an accompaniment of long-sustained growth. Once economic modernization is underway, the political milieu becomes modified in a way progressively more favorable to continued growth. This "virtuous circle" can be traced in societies as diverse as those of Britain, the U. S. S. R. and Japan.

Discussion of the role of government in today's LDC's is afflicted by opposing dogmas. On one side is the neo-liberal view that, if government will stand out of the way, private initiative will mobilize increased resources and direct them toward the most productive uses. On the other side is the planning technician's view that government can generate growth through administrative actions, and can predict and regulate its pace. In most LDC's, however, 80 to 90 percent of national output comes from the private sector. Here government cannot compel expansion, but must induce it by creating a structure of incentives which will lead producers to respond in the desired way.

It would be helpful if speculative and ideological discussion of these matters were replaced by careful analysis of experience. What did governments in the MDC's contribute to early acceleration of growth in those countries? What have governments in selected LDC's been doing over the past generation, and with what consequences? What kinds of action seem to have contributed to growth, and what policies have led to stagnation? There can in the end be no substitute for such a detailed, case-by-case analysis of the historical record.

II. The Positive Study of Less Developed Economies

We turn now to our second main theme. What kinds of economic research can usefully be undertaken in the LDC's? What is the potential content of the subject, viewed as a branch of positive economics? Is this content substantial enough to warrant considering this a separate specialty?

The present state of the literature suggests that, if there is a subject here, it remains to be defined. Textbooks, research monographs, and course outlines represent a wide variety of concepts and approaches. Some universities, indeed, have several "development" courses and seminars, with little in common except the name. This clearly represents a stage of pre-scientific groping and experimentation.

A striking feature of the literature is its heavily normative character. Many courses and texts embody a "how-to-do-it" approach to the subject: arguments over balanced versus unbalanced growth, optimal savings rates, criteria for allocation of investment funds, choice of technology, sources of external finance, planning methodology.

Practical problems have always been important in stimulating new developments in economic thought. But between initial problems and useful policy conclusions there has usually intervened a gradual and arduous development of positive economics; definition and measurement of important variables, analytical models intended to replicate important aspects of behavior, framing and testing of specific hypotheses, gradual emergence of a picture of the economy in operation. The puzzling thing about much early work on the LDC's is the implicit assumption that this intervening stage can be by-passed, that one can create a body of policy prescriptions "hanging in the air," unsupported by a structure of positive knowledge. It is rather as though there had been, in the United States in the 1840's, an outburst of courses and textbooks on "how to improve the American economy and make it grow faster."

If one wants to engage in something other than current policy-making, what is there to do? What lines of research into the less developed economies may yield improved understanding of their operation, and lay a firmer basis for policy-making in the future? Three main lines of work, complementary rather than competitive, suggest themselves: micro-analysis of economic behavior in the LDC's; study of total economies and their evolution over time; and building alternative models of early economic growth.

Micro-analysis of Economic Behavior

In the developed countries since the nineteen-thirties macro-economics has held the center of the stage and micro theory has been pushed into the background. The tacit

assumption is that the price mechanism works sufficiently well to warrant relative neglect of resource supplies and resource use.

In the LDC's this view is clearly not warranted. The market network is fragmentary, economic management (private and public) is sub-optimal, and innovation is laggard. This being so, detailed examination of the economy--sector by sector, market by market--becomes a matter of urgency. Little has yet been done in this direction. Scattered studies of this problem or that have lifted the corners of the veil covering a particular economy. But is there a single case in which we can see all round the economy in the way that is possible for Britain, France, Sweden, or Japan? I think not.

Agriculture, for example, is the largest industry in almost every LDC. The operation of the peasant household is central to an understanding of the economy. Several competing models appear in the literature: the "inert peasant," who cultivates traditional crops in a traditional way, and is both ignorant of and unresponsive to possibilities of technical change; the "lazy peasant," a satisficer who will work only to the extent necessary to achieve conventional standards of consumption; the "maximizing peasant," who knows the possibilities of product and factor substitution, makes correct marginal calculations, and is willing to invest to raise future output. But we do not know which of these models is most plausible, nor can we find out without more empirical study.

The system of land ownership, and the division of output between owner and cultivator, may have important effects on labor input, choice of products and techniques, and receptiveness to technical change. Proposals for changes in the tenure system are warmly debated in many countries. In most cases little is known about the economic consequences of one system or another. Yet quantitative analysis is often possible. One occasionally finds almost a laboratory situation, where the same crops are grown in the same area, under two or more tenure systems. In such cases input-output relations can

be examined, and one can ask whether land tenure per se has effects which can be segregated from those of other variables.

There is a large literature, mostly of a speculative character, on the possible existence of "surplus labor," "redundant labor," or "disguised unemployment" in the agricultural sector. It is doubtful that further verbal battles on this front can yield any positive product. But there is a shortage of studies in which precisely-framed hypotheses have been confronted with relevant data. Much of the verbal argument, indeed, relates to a situation which is rare in reality, that of a declining farm labor force. The common situation in the LDC's, however, is that high population growth is swelling the farm labor force. The interesting problem for study is how this growing labor force is absorbed (or not absorbed) into the rural economy, and what happens in the process to labor inputs per acre and to production methods.

There is a growing body of evidence that, where alternative crops are feasible, peasant producers are responsible to changes in relative prices. But this is a shift of production rather than an expansion of production. Much less is known about how aggregate output responds to increased income possibilities. To put the point differently: what proportion of a potential increase in output must be left with the cultivator to persuade him to produce the output? Some material incentive is required, but there is little evidence on how much.

We have not chosen agriculture for illustration because it has been especially under-investigated. On the contrary, there has probably been more careful research on agriculture in the LDC's than on any other sector. Knowledge is slight only in relation to the size and complexity of the industry. One could take any other branch of the economy--the public sector, factory industry, foreign trade--and find a large array of significant unanswered questions.

The micro-economic problems requiring investigation cut right across the economy. One interesting implication is that to proclaim oneself simply a "development economist" is not very different from calling oneself a general practitioner. Interest in the LDC's is in no way incompatible with adequate specialization in one or more functional areas of economics. One can aim at becoming a development economist and a public finance man, or an agricultural economist, or a student of international trade.

Inter-sectoral relations and economic change

Along with study of sectoral problems, there is need for some economists to view national economies in the round. How does the economy of Chile operate? How do different industries and sectors interact? What structural changes and lines of expansion are observable over time?

This is not conventional macro-economics, using only a few highly aggregated variables. A LDC is usually a quite fragmented economy, with relatively low inter-sectoral linkages, disparate sectoral growth rates, and acute problems of internal balance. Aggregate measures of national output, employment, investment, and so on are unrevealing unless accompanied by sectoral measures. These are also quite open economies in which fluctuations are induced more by external shocks than by changes in domestic investment. To analyze how external influences ramify through the economy requires detailed knowledge of its structure.

This kind of work requires dependable sectoral measures of output, factor inputs, productivity and prices. It involves analysis of inter-sectoral movements of commodities, labor and finance. Input-output tables, national income and product statements, government budgets, balance of payments accounts, and other standard measurement devices are applicable. These can be fitted together into an internally consistent set of national economic accounts, revealing the anatomy of the economy in considerable detail.

This kind of analysis becomes increasingly interesting as it is extended over time. A few years' experience tells very little. At least twenty or thirty years is required to draw significant conclusions about growth rates and structural changes in an economy. And even this may not suffice. Anyone observing Argentina over the years 1870-1914 must certainly have concluded that this economy had "taken off." From the high growth rates of 1870-1914, and the respectable growth rates of 1914-1929, it would have been quite impossible to predict the much slower growth in the years 1930-1960.

In economies where one observes a sustained rise in per capita output over several decades interesting questions arise. Which kinds of output are increasing most rapidly? How far is the output increase attributable to increased factor use, how far to "the residual"? Does the impetus seem to be mainly internal or external? How are increased resources being mobilized and applied? Are sectoral bottlenecks or external constraints holding the growth rate below what it might otherwise be?

This analysis of early economic growth necessarily involves notions about how growth occurs. While these are not yet very systematic, we do have some concepts and hypotheses which can help to order the historical record. The accumulation of longer records of experience for more countries will in turn contribute to improvement of "growth models." This interaction of theorizing and analytical description should be unusually vigorous over the next generation, as is true in any rapidly-developing area of study.

Analysis of total economies may contribute also to a useful classification of LDC's. Everyone recognizes that the universe of LDC's is heterogeneous. It is accepted that different empirical generalizations and analytical models are required for different types of economy. But no one can yet say what is meant by "type" in this connection.

It is clear that per capita income is not a very useful basis of classification. Zambia and Venezuela stand quite high on the per capita income list; but few would argue that

Zambia is more developed than Japan. Nor does current level of per capita income correlate well with growth rate. Argentina and Chile are relatively high-income countries, but their progress in recent decades has been slow.

A more interesting basis of classification is relative factor availabilities. The concept of "labor surplus economies" represents this approach. The numerous countries which still have open frontiers might be termed "land surplus economies." Venezuela, Iran, and a few other oil rich countries might be considered "capital-surplus economies" in the sense that growth is not constrained by foreign exchange availability.

But this is a rather static kind of classification. Excess supply of one or more factors is compatible with either growth or stagnation. The rate of change in excess supplies is also significant. Is an initial surplus of labor shrinking over time, or is it rising through a high rate of population growth plus a low rate of employment expansion?

Seers has suggested¹ an output-mix classification, based partly on the importance of industry relative to primary production, partly on diversification of output within each of these categories. His spectrum of economies runs from large and diversified industrial economies such as the U.S.A. or U.S.S.R., at one pole, to the one-mineral economies represented at the extreme by the "oil sheikhdom." This classification correlates rather well with size and openness of the economy. As one goes down the spectrum, exports become increasingly the lever of the economy. For one-crop or one-mineral economies detailed analysis of the major export industry is crucial.

Still another approach, emphasized particularly by Myint, runs in terms of the development of money transactions and a market mechanism.² The process typically starts

¹Dudley Seers, "An approach to the short-period analysis of instability in primary-producing countries," OEP, Feb. 1959.

² See in particular Hla Myint, The Economics of Developing Countries (London: T. W. Hutchinson, 1964).

with sale of one or more export products for cash; but if this is all that happens, the economy remains underdeveloped. Only in so far as cash dealings, specialization in production, and trading relations spread gradually within the country can it be regarded as developing. Its degree of development is measured by how far it has moved toward full specialization, sale of products and factor services for cash, and economic integration through markets. Myint's work is rich in suggestions about how and why an economy may move, or fail to move, in this direction.

Suggestions for a typology of LDC's are thus not lacking. But there is not yet any agreed scheme. Progress in this direction requires further country-by-country analysis of particular economies.

Theories of Early Economic Growth

During the past twenty years there has been a spate of neo-Keynesian and neo-classical growth models. A recent survey article lists upwards of a hundred contributions.¹ But most of this work is not relevant for present purposes. The standard assumptions of growth theory--one or at most two products, full mobility of factors, competitive pricing, constant returns to scale, constant elasticity of labor-capital substitution along well-behaved production functions--are quite unrealistic even for the MDC's. For the LDC's they verge on fantasy. Particularly restrictive is the common assumption of a single output and a single production function. The essence of underdevelopment is a sharp cleavage between "modern" and "traditional" production. Nor can one get round this by applying the standard growth theory only to the modern sector, leaving the much larger traditional sector in residual status. The behavior of the traditional sector as factor supplier and product demander, including its gradual transformation and annexation to the modern sector, is an integral part of early economic growth.

¹ F. H. Hahn and R. C. O. Matthews, "The Theory of Economic Growth: A Survey," Economic Journal, December 1964, pp. 779-902.

Related to this is the neglect of land and the primacy of capital in modern growth theory, which stamps it as industrially-oriented. A theory of early economic growth must explain what is happening in agriculture, which remains the largest sector of the economy for many decades after growth begins. The initial land-labor ratio, the organization of farm production, the nature of production functions and of producers' responses, the rate and factor bias of technical change, are key features of any usable growth model.

It is not even clear at this stage what one should mean by a theory of early economic growth. There are several possible views. First, theory might aim at explaining how economic growth gets started in a previously stagnant economy. What are the minimum institutional pre-requisites? Given a favorable environment, what kinds of stimuli may set the mechanism of expansion in motion? Is export-led growth a frequent or even the usual case? Study of the preconditions of growth involves non-economic variables, some of which are difficult to quantify; and so economists tend to hold back from it.¹ But there is little indication that political scientists, social anthropologists, or others are going to produce adequate theories of how economic growth begins. Economists, who in recent decades have tended to define the boundaries of their discipline more and more narrowly, should be venturesome enough to conduct some forays into this difficult area.

Second, there is theorizing of a "biological" character. This emphasizes the alternative ways in which the money economy may penetrate a system of household production, and the changes in personal behavior, economic organization, and exchange relations which occur in the process. Institutional transformation is in the center of the stage, input-output relations rather in the background. The ingenious work of Myint in this area has already been mentioned.

¹ A notable exception is Professor Everett Hagen. See his two volumes, On the Theory of Social Change: How Economic Growth Begins (Homewood, Illinois: Dorsey Press, 1962); and The Economics of Development (Homewood, Illinois: Richard D. Irwin, Inc., 1968).

Third, there are theories in which quantitative production relations play a central role. These theories are mechanical in the sense that, given one or more sectoral production functions, and given the rates of input increase, certain rates of output increase follow automatically. Economic growth has already begun "before the curtain rises," and the problem is to determine its rate and direction. This kind of work, exemplified by the Lewis and Fei-Ranis models, is attractive because of its quantitative character and the potentiality of statistical testing.

Growth models adapted to the LDC's, however, are still in an early stage of development. Theoretical work has been focused on one kind of economy--the fully settled, heavily populated, "surplus labor" economy. The assumptions used are highly simplified, and the consequences of varying them in one direction or another need to be explored. There has been little analysis of other economic situations--for example, the country with an open frontier, or the economy with both unused land and unused labor time.

Most serious, virtually all models to date have been closed-economy models. Trade and capital movements, however, are central facts of life in most of the LDC's. Analysis of the interrelations of growth and trade--on which a good deal of work has already been done by Johnson, Kindleberger, Myint, Meier and others--promises rich returns. Here the existence of a long tradition of trade theory is in one sense an advantage. On the other hand, the extremely simplified assumptions of most trade theory, leading to limited ability to predict actual trade flows, is a considerable weakness. Work in this area can perhaps contribute as much to improvement of trade theory as to building better models of economic growth.

To sum up: one can conceive of a course on the LDC's which would be basically a course in positive economics. It would focus on the three kinds of work just described: abstract models of how economic growth begins and proceeds during its early decades; analysis of the structure and development of selected LDC's, viewed as total systems; and

examination of the micro-economics of agriculture, industry, public finance, external trade, and so on in this range of economies. Much of the material covered would be relevant to economic policy; but the course would not be organized around policy issues or normative concepts.

The list of useful reading for such a course is still short. But in view of the growing volume of work on the LDC's, the literature should be richer a decade from now. Meanwhile it is important to set out the right boxes, even if some of them remain almost empty for the time being.

III. The Relevance of "Western Economics"

Economists and students from the LDC's often assert, with varying degrees of strength, that the economic theory taught in British and American graduate schools is not very relevant to their own economies. Some Western economists fall in with this assertion, while others react strongly against it. The issue is clearly important to our present concern. If a Western-trained economist can employ his usual tool-kit as effectively in Thailand as in Germany, if he in fact finds no need for additional tools, the case that study of the LDC's constitutes a distinct specialty becomes less convincing.

It is often not clear what critics of Western economics really mean. One possible meaning may be eliminated at the outset. Use of Western economic analysis is sometimes identified with a particular policy stance, with idealization of the market mechanism and a suspicion of government activity. This is simply confusion. There is no reason why such concepts as utility, preference, production possibilities, or opportunity cost should be identified with any one institutional setting. Since the work of Lange and Lerner in the 'thirties it has been accepted that the apparatus of micro-economics can be redirected toward management of a socialist system.

Setting aside this misunderstanding, a statement about the limited relevance of Western economics may mean at least four different things: (1) it may mean that quantitative relations among economic variables are different and will need to be reestimated in the LDC's; (2) it may mean that personal behavior is "less economic" in the LDC's, so that one cannot assume the usual responses to material incentives; (3) it may mean that the priority of problems is different in the LDC's, with a consequent difference in the relative importance of analytical tools; (4) it may mean that, because of structural differences in the economy and society, one has to develop new tools for explanatory and policy purposes.

The first statement is self-evidently true. For the Western economies, we know a good deal about price and income elasticities of demand, input-output relations, returns to labor and capital, consumption and investment functions, and so on. This knowledge is not directly transferable to an economy operating at a much lower income level, with different factor supplies, technology, and organization. Functional relations must be estimated anew by painstaking research, as is still being done in the MDC's. Because of the fragmentation of the less developed economies, there should be greater attention to particular sectors and industries, and greater skepticism about the stability of aggregative coefficients, than is needed in a more integrated economy.

These differences in coefficients, however, are not damaging to the logical structure of economics. The second kind of statement, which alleges non-economic behavior, would be decidedly damaging. But how convincing are such allegations?

Tests of economic rationality must be framed with care. It is not sufficient to show that individuals' preference systems are different in the LDC's. New factory workers in Kenya may give less weight than American workers to security of job tenure relative to money income. Medium-income families in Brazil may save a smaller proportion of income

than medium-income families in France or Canada. Manufacturers in Pakistan may show greater risk aversion and shorter time-horizons than their counterparts in developed countries. Such differences are readily accommodated within the framework of economic analysis.

Nor is the relevant question whether peasant producers, for example, behave as the outside observer concludes that they "ought" to behave. The peasant's subjective situation, the alternatives which appear open to him, and the considerations relevant to choice may be quite different from the situation as viewed by the educated, middle-class, and perhaps foreign observer. Given the subjective situation, the question is, first, whether material welfare is prominent among the decision criteria, and second, whether the direction of reactions is "normal," i. e. whether higher levels of material satisfaction are preferred over lower levels.

There is much evidence supporting an affirmative answer. As regards peasant producers, several research workers have concluded that, given the techniques which they know, peasants apply labor and capital to land as far as it is reasonable to do so, i. e. until marginal rates of return have fallen to a low level. Moreover, where peasants produce for market and where two or more crops are open to them, there is evidence of marked responsiveness to changes in relative product prices. If one crop becomes more advantageous than before, the proportion of acreage devoted to this crop rises with only a short time-lag.

As regards labor, there is little doubt that workers prefer higher-wage jobs to lower-wage ones. But it is sometimes asserted that a limited view of consumption possibilities sets a low ceiling to income aspirations. Once the ceiling is reached, the amount of labor offered varies inversely with the hourly wage--the labor supply curve bends backward. The writer was at some pains to test this hypothesis as regards new factory workers in

Puerto Rico, a group which is untypical only as regards the strong demonstration effect of readily available American consumer goods. For this group there was convincing evidence that income aspirations were quite elastic. Workers wanted more money, knew what they would do with it, and were willing to work longer hours to obtain it wherever factory schedules permitted.¹ Elliot Berg has reported similar findings from studies of African workers.²

There are probably two reasons for the wide currency of the backward-bending supply curve notion. First, it has long been a standard argument offered by employers, particularly foreign employers of indigenous labor, in defense of a low-wage policy. Nor is this defense at all new. Two centuries ago early English industrialists argued that higher wages would lead only to greater idleness, a conclusion which was challenged by Adam Smith.³ Second, the argument is associated with the peculiar circumstances of migratory labor in certain parts of Africa. Here the family does not accompany the worker to his place of wage employment, the wife does not become a consumer, and the normal pressures for a higher scale of household expenditure are inoperative. It is not surprising, then, that men work only long enough to acquire a few readily transportable consumer goods--bicycles, radios, etc.--or to accumulate the customary bride price in their area. On a world view, however, this system of employment is quite untypical.

The third line of attack noted above--that the priority ranking of policy issues differs as between MDC's and LDC's--is on firmer ground. The following areas, for example, seem

¹ See Lloyd G. Reynolds and Peter Gregory Wages, Productivity, and Industrialization in Puerto Rico (Homewood, Illinois: Richard D. Irwin, Inc. 1965).

² Elliot I. Berg, "Backward-sloping labor supply functions: The African case," Quarterly Journal of Economics, August 1961, pp. 468-492.

³ "A plentiful subsistence... it has been concluded, relaxes, and a scant one quickens their industry. That a little more plenty than ordinary may render some workmen idle cannot be doubted; but that it should have this effect on the greater part... seems not very probable." (Wealth of Nations, Everyman Edition, Volume I, p. 74),

to deserve a higher relative ranking in the LDC's: agricultural organization and productivity; demography and population growth; the economics of small scale industry; the micro aspects of taxation and public expenditure; and international trade and capital movements.

To the extent that economics is viewed as a policy instrument, then, there is a corresponding re-ranking of the usefulness of analytical tools. The basic tools of micro-economics are highly useful in the LDC's, whether applied to agricultural production, the economics of industry, the impact of taxation, or cost-benefit analysis of public sector projects. Western macro-economics is considerably less useful. Paradoxically, modern growth theory has little to offer to economies in which growth is the most urgent practical problem. Post-Keynesian theories of income determination and economic fluctuations are also not readily transferable to the LDC's. Thus the tendency in many Western graduate schools to emphasize a kind of macro-economics adapted to MDC institutions, while relegating micro-economics to a secondary place, is precisely the wrong thing for students from the less developed countries.

This still does not answer the fourth question posed above, which is perhaps the most fundamental. Does analysis of the less developed economies require simply a reshuffling of the same instruments, a lifting of different tools from what remains essentially the same tool-kit? Or does it require also a significant amount of new tool construction? Is there a new species of "LDC economics" in process of development, or at least capable of being developed?

These are complex and difficult questions and the answers depend on the level of abstraction under consideration. Such concepts as individual preference systems or production functions are so fundamental that any kind of economic reasoning must take them as a point of departure. At this level one can argue that economics is independent of time and space. But economics does not consist solely of such basic ideas. There is a hierarchy of

theoretical constructs, ranging from the simple and general to the quite complex and specific--from, say, the concept of profit maximization to a model of investment decisions in the steel industry in contemporary United States. As theory comes closer to grappling with a specific body of phenomena, its structure becomes more elaborate, specific, and empirically oriented.

At some stage of elaboration and specialization the kind of theory required to explain a certain range of economic phenomena in a LDC--the variables to be included, the presumed relations among them, the specific hypotheses to be tested--begins to differ significantly from that which is relevant to the MDC's. Experience in having worked on similar problems in a MDC may be useful as background. But it is only background, and does not obviate the need for new theoretical constructs and new research design.

This can be illustrated from a variety of fields. We have already noted that the kind of growth theory relevant to the LDC's is considerably different from that currently being developed for the advanced industrial countries. This is true also of short-run macro-economics. The fact that fluctuations are externally induced rather than investment-induced, and that they impinge on economies with a small public sector, a primitive monetary system, and serious supply inelasticities changes both the analysis of fluctuations and the nature of stabilization measures.

While Latin American theories of "structural inflation" may be partly an apologia for fiscal laxity, they are not wholly that. Monetary processes and price behavior do differ from the corresponding processes in the MDC's, and require fresh lines of analysis.

There has been a strong reaction against standard international trade theory in the LDC's, most marked again in Latin America. While some of the counter-reasoning advanced from the LDC's may appear implausible, the deficiencies of trade theory are undoubtedly real. Work has focused on comparative advantage and optimal trade patterns at a point in time,

with given factor supplies and identical production functions in each trading nation. But the assumption of identical production functions between MDC's and LDC's is unacceptable to definition. How to import technology, and what technology to import is a major policy issue. The problem of the LDC's, as Chenery¹ and others have shown, is to define dynamic comparative advantage under conditions in which tastes, relative factor supplies and prices, and technology are all subject to rapid change. Analysis of the dynamics of trade relations, which in the MDC's may appear merely interesting, is for the LDC's a vital necessity. There is need for fresh theoretical and empirical work, conventional in the sense of being linked to a long tradition of past work, but original in placing structural change over time in the center of the picture.

In agriculture, theorizing about the production-consumption behavior of peasant households² is significantly different from the production economics of a midwestern American farm. In industry, models of the isolated profit-maximizing firm or of the interaction of firms in a competitive industry are useful but by no means sufficient. There are problems of distinguishing private from social profitability, of estimating returns to a complex of interrelated investments ten or twenty years in the future, of devising efficient sequences of investment à la Hirschman. These kinds of analysis are similar in being time-related, forward-looking, and extending beyond the bounds of a single industry. They rest in a sense on standard micro concepts. But these concepts must be manipulated in new ways to explore, not optimal resource allocation at a moment, but optimality over extended periods of time.

Thus an industrial economist, or agricultural economist, or international economist will find himself becoming a different kind of economist as he works on the structure of the

¹Hollis B. Chenery, "Comparative advantage and development policy," American Economic Review, March 1961, pp. 18-51.

²See, for example, A. K. Sen, "Peasants and dualism, with or without surplus labor," Journal of Political Economy, October 1966, pp. 425-450.

LDC's. He will have a certain expertise not possessed by those who have not strayed outside the developed world. He will necessarily have to make new theoretical contributions to achieve significant research results. In this sense there is something new about development economics.

IV. Concluding Comments

We return to the question posed at the beginning: is there here a potential subject, which may in time occupy an established position alongside older branches of economics? An affirmative answer seems warranted, but with qualifications.

1. There is not as yet any substantial body of knowledge about the less developed economies. There is not a single LDC whose economy is now understood in the way that the British or American economies are understood. This is due partly to data deficiencies but mainly to lack of careful investigation.

2. Development economics should not be construed simply as an exercise in economic engineering, directed at current policy problems. The core of the subject is, or should be, an effort to understand the operation of the less developed economies. Such an understanding, acquired through decades of careful effort, has been necessary for policy formation in the MDC's. It seems very likely that comparable effort will be required in the LDC's.

3. If this view is accepted, the "economic development" label should be replaced by a broader and more neutral title. "Early economic growth" would be better, but still too restrictive. "Structure and growth of less developed economies," or simple "economics of the less developed countries," would be in the right direction.

4. The range of possible investigation in the less developed economies is hopelessly wide. No one can prudently set up as an expert on all aspects of all LDC's. Specialization is required, either on a territorial basis or, as may seem more profitable to most economists, on a functional basis. It was hence suggested that those wishing to work in the LDC's might well start from a solid foundation in one or more functional specialties.

5. Investigation of the LDC's is not merely a matter of applying familiar analytical tools to new data. New theorizing is required both in attacking specialized research problems and in constructing general models of early economic growth. This will gradually build the solid intellectual core which is still largely lacking.

A considerable number of economists are already working along these lines, and their number seems certain to increase in the years ahead. A generation from now it should be possible to give a less qualified "Yes" answer to the topic of this paper.