

file

Reynolds

ECONOMIC GROWTH CENTER

YALE UNIVERSITY

Box 1987, Yale Station
New Haven, Connecticut

CENTER DISCUSSION PAPER No. 57

THE CONTENT OF DEVELOPMENT ECONOMICS

Lloyd G. Reynolds

October 1, 1968

Note: Center discussion Papers are preliminary materials circulated to stimulate discussion and critical comment. References in publications to Discussion Papers should be cleared with the author to protect the tentative character of these papers.

[Note: This is a draft of a paper for the A. E. A. December meetings. Comments are solicited, but to be useful they should reach me by November 1.]

The Content of Development Economics

Economic development is an activity in search of an intellectual framework.

The impetus to development studies was the appearance after 1945 of many new nation-states, with low income levels and primitive economic systems, able for the first time to assert their needs in international forums; and the political and commercial rivalry among the richer nations, which inclined them to make some positive response to these needs.

But a domain of practical activity does not constitute a scientific study; and the basis for this new subject remains unclear. Opinions range all the way from those who take the existence of development economics for granted to those who deny that there is any such thing.

Let me stake out a centrist position in this spectrum. Those of us who work on the less developed economies cannot yet claim the coherence of subject matter, accepted analytical apparatus, and wealth of empirical observations which characterize the older economic specialties. Courses and texts in economic development do not show the homogeneity of courses in international economics or public finance. There is, however, both the potentiality and the emerging outline of a true specialty, which will assume an increasingly solid place in the curriculum. It is not a passing fad which will go away if we wait a decade or two. It will not go away because the less developed economies will not go away, nor will their problems become less interesting and important.

This emerging specialty will not, however, be symmetrical with the older functional or sectoral specialties. Its domain is a certain range of national economies in certain regions of the world. Work on these economies requires mastery of existing macro-and micro-economic tools, together with new tools which remain to be invented. In this respect development economics resembles the study of socialist economies, or the study of economic history, rather than a specialty confined to one sector of the economy. It interpenetrates with the functional specialties rather than standing alongside them.

Two preliminary comments

Having asserted that economic development is at least a potential subject, I am duty bound to say what this subject is about. But before this, let me make two ground-clearing remarks.

One can argue that analysis of the LDC's consists mainly of applying standard economic tools in familiar ways. I understand that one leading graduate school has implemented this view by abolishing separate courses in economic development and substituting a "development section" in the standard courses on public finance, labor economics, industrial organization, and so on.

There is substantial truth in this argument. In shifting from the American economy to that of Chile or Pakistan we do not throw away our present tool-kit. Nor can one dissent from the view that the standard functional courses should have a larger non-Western content. Such courses can gain in generality and power by being stretched to cover the LDC's and the socialist countries. But I do not think this is all that needs to be done. There is a place for special courses on socialist economies, which view them in the round and explore such distinctive features as the planning mechanism. There is room for similar specialized treatment of the less developed countries, along lines which I hope to indicate a bit later on.

While familiar theoretical concepts provide a starting point for analyzing a less developed economy, it is only a starting point. The institutional and behavioral peculiarities of the LDC's require a significant amount of new tool construction. For example, Seers has shown that economic fluctuations in the LDC's are usually induced by swings in export receipts rather than in domestic investment; and that an understanding of their impact on the economy requires analysis of the main export products. Again, Brazilian inflation really

is different from American inflation, analytically as well as empirically. Peasant households cannot be presumed to behave like Midwestern farm households. A student of the less developed economies has to be partly a tool-builder, or tool-adaptor. People who have worked on this kind of economy for a long time are best prepared to refashion theory in useful directions.

It is also a misconception to think that one can handle the LDC's by tucking them into a general course on growth theory. Early economic growth in poor countries is a sufficiently different phenomenon from growth in mature industrial economies that it is misleading to package them under the same label. The standard assumptions of modern growth theory--one or two products, free mobility of factors, competitive pricing, constant returns to scale, well-behaved production functions, a closed economy--are quite fanciful even for the developed countries. To extend such assumptions to the LDC's does not seem very plausible.

Moreover, from a LDC standpoint, these models do not pose the right questions. They take the values of all the interesting variables as given. The model then helps us to determine the rate of steady-state growth and whether, if the economy is off this equilibrium path, it will or will not converge toward it. Comparative dynamics is the name of the game. In a LDC, however, the interesting questions are what determines the rate of increase in factor supplies, the speed and direction of technical progress, and the increase of total factor productivity. The givens of the standard growth problem must be investigated as variables.

My second preliminary comment is that there is considerable confusion in the literature over whether the center of gravity of development economics lies in positive economics or in normative economics. Here I find myself an old-fashioned Keynesian. "Economics does not furnish a body of settled conclusions immediately applicable to policy." Our

central task is to understand the economic mechanism of the less developed countries. We are scientists before we are engineers. Economists may or may not have comparative advantage as policy advisers. Where we clearly have comparative advantage is in building a systematic body of knowledge about how the economy operates, which improves our power to predict the consequences of policy decisions.

Models of Early Economic Growth

I pass now to the constructive part of my task. Viewing development economics as a branch of positive economics, what is it really about? What kinds of work on the LDC's may yield a better understanding of their operation, and lay a firmer basis for policy-making? Let me suggest briefly four useful lines of activity: construction of growth models adapted to the less developed countries; empirical analysis of early economic growth in particular countries; comparative cross-sectional studies; and micro-analysis of economic behavior.

I have already suggested that most of recent growth theory has little relevance to the LDC's, and that a different species of theory is required. In my judgment, the most useful "LDC growth model" produced to date is the Fei-Ranis model. Its authors would be the first to agree, however, that we stand only at the beginning of this kind of activity. Looking down the path which they have helped to open, I visualize several lines of development.

First, we all realize that the universe of LDC's is very heterogeneous. There are numerous "types" of LDC, though no one can yet say confidently what we mean by a "type." One obvious basis of classification is by relative factor availabilities. Many LDC's are "labor surplus economies," but others are not. The numerous countries which still have open frontiers might be termed "land surplus economies." There are even a few oil-rich countries which can be considered "capital-surplus economies." Size and openness is

another important dimension, along which one can array LDC's from Jamaica to India. Seers has suggested an output-mix classification, based partly on the importance of industry relative to primary production, partly on the diversification of output within each of these categories. In any event, the fact of heterogeneity is evident. So we should not expect any one growth model to have equal explanatory power for every kind of LDC. Rather, we should look toward a variety of models tailored to particular circumstances.

Next, we need to experiment with a finer subdivision of sectors. Two sectors are certainly better than one, but four may be better than two. I have suggested elsewhere¹ that it would be useful to distinguish the public sector, "modern" private industry, traditional urban activities (trade, services, handicrafts), and agriculture. My mathematician friends tell me that four-sector models are extremely difficult, and may not be susceptible of general solution. But it may still be useful to tinker with models which come closer to reality, even if we can only develop illustrative special cases or run simulation experiments.

Third, trade and capital movements are central facts of life in most of the LDC's. There is general recognition that we really need open-economy models, which portray the interaction of growth and trade. Here the existence of a long tradition of trade theory is an advantage. On the other hand the extreme simplification of most trade theory, leading to limited ability to predict trade flows, is a considerable weakness. Work in this area can perhaps contribute as much to trade theory as to building better models of economic growth.

Finally, let me note that virtually all growth models --developed or underdeveloped-- assume that growth is already underway "before the curtain rises." They do not tackle the

¹ "Development with Surplus Labor: Some Complications," Oxford Economic Papers, July 1969.

intriguing question of how growth gets started in a previously stagnant economy. This problem obviously involves non-economic variables, some of which are difficult to quantify; and so economists tend to hold back from it. But there is little indication that political scientists, social anthropologists, or others are going to do the job for us. So I believe we should be venturesome enough to make some forays into this borderline area. Let's have a little "economists' imperialism."

The Movement of Economies Through Time

A second task is analysis of how particular economies have grown (or failed to grow) over time. There are 40 to 50 LDC's of some size and consequence. Each of these provides a laboratory in which one can test theoretical models of early economic growth.

The Economic Growth Center at Yale initiated a major program in this area in 1961, which is now (hopefully) approaching completion. We selected 25 LDC's, reasonably well distributed by continents, and including most of the larger and more consequential countries. Within the foreseeable future, we shall have a shelf of books analyzing the growth experience of these countries, with particular attention to the post-1945 period, but going back in some cases to 1900 or beyond.

This kind of work is not easy to categorize. One might call it economic history. More precisely, one might call it quantitative, analytical, Kuznets-type economic history. We have emphasized analysis of quantitative information about these economies; and each of the published studies will contain a substantial, annotated data appendix.

Given adequate information, what would one like to find out? Agriculture usually bulks largest in the economy. So one needs to analyze agricultural output, and the distribution of output among crops and among major uses. To the extent that input data are available, one can explore how far output expansion is due to increased factor use and how far to increased factor productivity. Where factor productivity has risen, how and why did

it rise? On the income side, one would like to know how income arising in agriculture is distributed between landowners and cultivators, where these do not coincide, and how much is reinvested in agriculture or other sectors.

Just as agriculture is the largest productive sector, labor is the largest input. So one has to analyze the labor force and wrestle with concepts and measures of "unemployment" and "underemployment." The test of successful development is an employment test as well as an output test. If we cannot say whether the ratio of employed labor time to available labor time is rising or falling, we cannot tell whether the economy is meeting the employment test. Nor can we tell what is happening to man-hour output and other productivity indicators.

Economic growth and structural change imply reallocation of the labor force and a rise in labor force quality--skill level, educational level, motivation, and personal efficiency. Study of the quality dimension of the labor force leads into analysis of the educational system, viewed as the dominant influence on the supply side of the labor market. In the early stages of growth, something called "the skill constraint," or "limited absorptive capacity," is said to limit the growth rate. What is the nature of this constraint, and how is it relaxed (or not relaxed) over the course of time?

The importance of the international sector is obvious. Export performance and the reasons for it need to be analyzed. Imports are usually controlled by an elaborate structure of exchange rationing, import quotas, and tariffs, not necessarily well-rationalized or internally consistent. The way in which this structure has affected domestic resource allocation is an important subject for study. Capital movements, private and public, gross and net, need to be analyzed in terms of their productivity contribution and the current and future costs which they impose on the economy.

The public sector is still small in most LDC's, but it contains many of the levers which (in principle) a growth-minded leadership can manipulate to move the economy forward. So one should examine the growth of the main types of public expenditure. Development strategy is supposed to involve a rise in the proportion of revenue devoted to fixed investment and other developmental purposes. In some countries this has happened. In others, including some with comprehensive development plans and many statements of good intention, it has not happened. We need to know why. The tax system has to be analyzed with respect to incentive effects as well as revenue-raising power. Infrastructure industries, which are usually organized as public corporations, present important issues of investment, output, and pricing policy.

Manufacturing is also a growth sector whose expansion normally outpaces that of national output. The issues in this sector have been discussed extensively in the development literature, and little need be added. We need obviously to know much more about such matters as the sequence in which new industries normally appear in a LDC; how far this sequence can be altered by policy, and the gains and costs of so doing; the borrowing and adaptation of technology; the sources of capital and entrepreneurship; the disposition of industrial profits; the rate of growth of employment, and the interrelations among employment, productivity, and wage policy.

A picture of changes in particular sectors does not necessarily add up to an understanding of the economy. There is interaction among sectors even in a stationary economy, and in a growing economy one would expect such interaction to be increasing. This interaction will show up in intersectoral flows of commodities, labor, and finance. So mapping these flows is an essential step in analyzing the economy.

Having done this, and assuming that growth has been occurring, one can face the problem of explanation. What kinds of output have been rising relative to others? Can one

distinguish leading and lagging sectors? How important has the international sector been relative to domestic developments? How much of the output can be attributed to absorption of previously unused resources, how much to increase in factor supplies, how much to productivity improvement?

In the back of one's mind will naturally be hypotheses and models of how early economic growth may occur. But these are still sketchy, as indicated earlier, and if historical study had to wait for their perfection it would wait a long time. In this area, as in other branches of economics, progress will doubtless come through an interaction of theorizing and investigation, with crude hypotheses serving as a guide to research, which in turn suggests new or refined hypotheses.

Comparative and cross-sectional studies

Let me pass to a different kind of investigation. Suppose we want to say something, not just about what happens in Brazil, but about what happens in the LDC's generally. Or suppose we have a strong interest in a particular set of economic relationships. Then we will probably turn to testing hypotheses by cross-country econometric analysis.

To take an illustration from the developed countries: the rate of increase in money wages can be explained reasonably well by such variables as the unemployment rate, the profit rate, changes in these two rates, and changes in the consumer price index. We have data on the relevant variables for most of the industrial countries. So it is possible to derive an "international Phillips curve" from cross-country data, and this has actually been done.

It is easy to think of interesting hypotheses which might be tested in the less developed countries. For example:

(1) The hypothesis that production functions are identical among countries, which still underlies much of trade theory.

(2) The hypothesis that property income is saved while labor income is not, and that this provides the main explanation of national savings rates.

(3) The hypothesis that a country's pattern of manufacturing output is explained mainly by its population and per capita income.

(4) The hypothesis that industrial wage rates in the LDC's are pushed above the supply-price of labor by institutional pressures, that this leads employers to substitute capital and management for labor, and that this results in a slow growth of industrial employment relative to industrial output.

(5) The hypothesis that government output as a percentage of total output is positively related to a country's per capita income.

(6) The hypothesis that output of traded goods relative to non-traded goods, while it may rise initially as per capita income rises, will eventually decline.

Much of the progress in development studies over the next decade or two will probably come from cross-country analysis of this kind of problem. But progress will not be easy. I will only mention such a crass matter as data difficulties, which are still the most serious obstacle. The social marginal product of people who devote their lives to data improvement is high, and it is too bad that they receive such a low status ranking in our profession.

Beyond this, regression analysis assumes that variables are being defined and measured uniformly, and that the relations among them are of the same character throughout the universe being studied. This is less plausible for inter-country than for intra-country analysis. I am skeptical of cross-section results which include countries with very different income levels and economic structure--say, the United States and the Belgian Congo. Even the universe of less developed countries may be too heterogeneous to be treated as an entity, and we may need to analyze sub-groups of countries.

Then there is the question how far one can use cross-section results to interpret movements over time. This is a familiar problem from U. S. investigations, but it is probably more serious in cross-country studies. One may well hesitate to take cross-section findings as definitive unless they are supported by evidence from national time series.

Let me note finally that there are useful kinds of comparative study which may not be susceptible to refined statistical techniques. Take the question whether there is a Latin-American "type" of economy. This may be an unanswerable, or even a foolish question. But it may not be. This kind of question requires qualitative, institutional analysis as well as statistical comparisons.

Micro-analysis of economic behavior

Let me turn finally to what will undoubtedly, in sheer volume, be the largest kind of research activity in the less developed countries. This is detailed analysis of limited problems in a particular sector of a particular economy. After all, economic knowledge in this country has not progressed by people setting out to investigate the American economy. Rather, they have set out to investigate price-determination in the aluminum industry, or the determinants of fixed capital investment in manufacturing, or the incidence of the corporate income tax. After thousands of such studies we can begin to see the American economy in the round, in a way which we cannot yet do for Peru or Ghana.

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," the "lazy (or satisficing) peasant," the "maximizing peasant." 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 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 responsive 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 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. There is also a substantial literature on foreign trade, capital movements, and related matters. On other sectors there have been few significant pieces of research. But this means that the marginal yield of research effort is still high; and if economists behave rationally, careful micro-analysis in the LDC's should grow rapidly.

Among other things, there is here an almost limitless field for doctoral dissertations. There is no reason for a graduate student in public finance to pursue a minute and tedious study of some aspect of the Nevada sales tax when he could be studying how to extract revenue from farmers in Pakistan. The latter problem is more interesting scientifically, more important practically, and doesn't make him any less a public finance man.

So I return in conclusion to a point made at the outset: the study of economic development interpenetrates with the conventional specialties rather than standing in a watertight compartment alongside them. It is undesirable and unfeasible for development specialists to draw a jurisdictional fence around the LDC's and announce: "This is our territory. All others stay out." There is no reason why a labor economist shouldn't study wage structures and labor allocation in Chile or Zambia as well as in the United States or Sweden. Such an infusion of talent from the traditional specialties of economics will advance our understanding of the LDC's. It will also contribute to more interesting courses and monographs on public finance, industrial organization, and the rest.

This paper has been couched in research terms, because it is through the accumulation of research findings that economic development will take firmer shape as an accepted branch of economics. It has been couched in forward-looking terms because, while we are moving quite rapidly, we have not yet arrived. I hope to have given you some reason for concluding that it would not be wise to sell economic development short.