

PROCEEDINGS OF THE WORLD BANK
ANNUAL CONFERENCE
ON
DEVELOPMENT ECONOMICS
1992

Supplement to THE WORLD BANK ECONOMIC REVIEW
and THE WORLD BANK RESEARCH OBSERVER

EDITORS

Lawrence H. Summers and Shekhar Shah

EDITORIAL CONSULTANT

Elinor Berg

BANK

The World Bank Annual Conference on Development Economics is a forum for discussion and debate of important policy issues facing developing countries. The conferences emphasize the contribution that relevant policy, empirical, and basic economic research can make to understanding development processes and to formulating sound development policies. Conference papers are written by researchers in and outside the World Bank. The conference series was started in 1989.

The *Proceedings of the World Bank Annual Conference on Development Economics* is published as a special supplement to the World Bank's professional economics journals, *The World Bank Economic Review* and *The World Bank Research Observer*. All current subscribers receive this supplement to the journals. The Editorial Boards of the *Review* and the *Observer* do not review the *Proceedings*, nor are the papers selected from the conference and published here subject to the peer review process through which regular submissions to these journals must pass. Summaries are included of selected floor discussions and the roundtable discussion; they attempt to convey the sense and substance of what was discussed, interventions by participants from the floor, and responses by panelists. They have not been reviewed by the authors, the discussants, or the participants.

The views and interpretations expressed in this *Proceedings* volume do not necessarily represent the views of the World Bank or its member countries and should not be attributed to the World Bank or its affiliated organizations.

© 1993 The International Bank for Reconstruction and Development / THE WORLD BANK. All rights reserved. Manufactured in the United States of America.

First printing March 1993

ISSN 1014-7268

ISBN 0-8213-2211-7

Material in this journal is copyrighted. Requests for permission to reproduce articles should be addressed to the Office of the Publisher, the World Bank, Washington, DC 20433, U.S.A. The World Bank encourages dissemination of its work and will normally give permission promptly and, when the intended reproduction is for noncommercial purposes, without asking a fee. Permission to copy portions for classroom use is granted through the Copyright Clearance Center, 27 Congress Street, Salem, MA 01970, U.S.A.

The World Bank Economic Review is indexed in *Current Contents / Social & Behavioral Sciences*, the *Social Sciences Citation Index*, the *Journal of Economic Literature*, and the *Public Affairs Information Service*. *The World Bank Research Observer* is indexed in the *Journal of Economic Literature*, the *Standard Periodical Directory*, the *Public Affairs Information Service*, Inc., and, online, by the *Economic Literature Index* and *DIALOG*. It is available in microform through University Microfilms, Inc., 300 North Zeeb Road, Ann Arbor, MI 48106, U.S.A.

PROCEEDINGS OF THE WORLD BANK
ANNUAL CONFERENCE
ON
DEVELOPMENT ECONOMICS
1992

Supplement to THE WORLD BANK ECONOMIC REVIEW
AND THE WORLD BANK RESEARCH OBSERVER

Introduction <i>Lawrence H. Summers and Shekhar Shah</i>	1
Opening Remarks <i>Lewis T. Preston</i>	7
KEYNOTE ADDRESS The State and Economic Development: A Mexican Perspective <i>Pedro Aspe and José Angel Gurria</i>	9
GROWTH AND DEVELOPMENT THEORIES	
Toward a Counter-Counterrevolution in Development Theory <i>Paul Krugman</i> Comment, <i>Joseph E. Stiglitz</i> 39 Comment, <i>Lal Jayawardena</i> 51 Floor Discussion 59	15
Two Strategies for Economic Development: Using Ideas and Producing Ideas <i>Paul M. Romer</i> Comment, <i>Kaushik Basu</i> 93 Comment, <i>Marcelo Selowsky</i> 99 Comment, <i>T. N. Srinivasan</i> 103 Floor Discussion 111	63

LABOR MARKETS AND DEVELOPMENT

- Labor Market Institutions and Policies: Help or Hindrance
to Economic Development? 117**

Richard B. Freeman

Comment, *Victor E. Tokman* 145

Comment, *Kwadwo A. Tutu* 151

Floor Discussion 153

- Labor Market Adjustment in Transitional Economies 157**

Jan Svejnar

Comment, *Marek Góra* 169

- Information Flows and Discrimination in Labor Markets
in Rural Areas in Developing Countries 173**

Andrew D. Foster and Mark R. Rosenzweig

Comment, *Nancy Birdsall* 205

Floor Discussion of Svejnar and Foster-Rosenzweig Papers 209

TECHNOLOGY

- Knowledge, Property, and the System Dynamics
of Technological Change 215**

Paul A. David

Comment, *Ashok Desai* 249

Comment, *Morris Teubal* 253

- Accumulating Technological Capability in Developing
Countries 257**

Martin Bell and Keith Pavitt

- Technology Gaps between Industrial and Developing
Countries: Are There Dividends for Latecomers? 283**

Howard Pack

Comment, *John Page* 303

Comment, *Jorge M. Katz* 307

Comment, *Carl Dahlman* 313

Floor Discussion of Bell-Pavitt and Pack Papers 317

INTERNATIONAL CAPITAL FLOWS

International Capital Flows to Latin America: What Is the Promise? 323

Pedro-Pablo Kuczynski

Comment, *Albert Fishlow* 337

Comment, *Mario I. Blejer* 341

Floor Discussion 345

Capital Flows to Developing Countries: Implications from the Economies in Transition? 349

Susan M. Collins

Comment, *András Inotai* 371

Comment, *Ishrat Husain* 379

Conversion of Official Bilateral Debt: The Opportunities and the Issues 383

Stephany Griffith-Jones

Comment, *Jonathan Eaton* 403

Floor Discussion of Collins and Griffith-Jones Papers 409

ROUNDTABLE DISCUSSION 413

The Health of the Public: A Public Responsibility?

*Julio Frenk, Eyitayo Lambo, W. Henry Mosley,
and Uwe Reinhardt*

Toward a Counter-Counterrevolution in Development Theory

Paul Krugman

During the 1940s and 1950s a distinctive set of ideas emerged in development economics that stressed the importance of increasing returns and pecuniary external economies arising from the effects of market size. Unfortunately, the economists who proposed these ideas were at first unable, and later unwilling, to codify them in clear, internally consistent models. At the same time the expected standard of rigor in economic thinking was steadily rising. The result was that development economics as a distinctive field was crowded out of the mainstream of economics. Indeed, the ideas of "high development theory" came to seem not so much wrong as incomprehensible.

This paper argues that in light of new developments in industrial organization, international economics, and growth theory, the old development economics now looks much more sensible than it seemed during the "counterrevolution" against interventionist development models. While development economics has been used to justify some highly destructive economic policies, there is a valid and useful set of core ideas that can be usefully resurrected. Thus this paper calls for a "counter-counterrevolution" that restores some of the distinctive focus that characterized development economics before 1960.

Once upon a time there was a field called development economics—a branch of economics concerned with explaining why some countries are so much poorer than others and prescribing ways for poor countries to become rich. In the field's glory days in the 1950s, the ideas of development economics were regarded as revolutionary and important and commanded both great intellectual prestige and substantial real-world influence. Moreover, development economics attracted creative minds and was marked by a great deal of intellectual excitement.

That field no longer exists. There are, of course, many excellent people who work on the economics of developing countries. Some of the problems they work on are essentially generic to all countries, but there are also issues uniquely characteristic of poorer countries, and in this sense there is a field that focuses on the economics of underdevelopment. But it is a diffuse field: those who work on

Paul Krugman is professor of economics at the Massachusetts Institute of Technology.

© 1993 The International Bank for Reconstruction and Development / THE WORLD BANK

the economics of agriculture in the developing countries, for example, have little (if any) overlap with those who work on developing country trade in manufactures, who in turn hardly talk to those who focus on the macroeconomics of debt and hyperinflation. And very few economists would now presume to offer grand hypotheses about why poor countries are poor or what they can do about it. In effect, a counterrevolution has swept development economics away.

This paper argues that the counterrevolution went too far. I will argue that during the 1950s a central core of ideas emerged regarding external economies, strategic complementarity, and economic development that remains intellectually valid and that may continue to have practical applications. This set of ideas, which I will refer to as "high development theory,"¹ anticipated in a number of ways the cutting edge of modern trade and growth theory. But high development theory was virtually buried, essentially because the founders of development economics failed to make their points with sufficient analytical clarity to communicate their essence to other economists, and perhaps to themselves. Recent changes in economics now make it possible to reconsider what the development theorists said and to regain the valuable ideas that have been lost. In other words, this paper calls for a counter-counterrevolution in development theory.

It is somewhat awkward to present a paper that is to some extent a history of thought at a conference on current research. But development is one of those fields (growth, trade, and regional economics are others) in which recent concepts can lead to a rediscovery of the validity of discarded insights. And by linking old ideas with the new, we may avoid repeating old mistakes and get a chance to make new ones instead.

I. A MOTIVATING EXAMPLE: THE BIG PUSH REVISITED

The Big Push paper of Rosenstein-Rodan (1943) has inspired many interpretations. Some economists read it as essentially Keynesian, a story about interactions between the multiplier and the accelerator. Rosenstein-Rodan himself seems to have had some more or less Keynesian idea about effective demand in mind, with (as we will see) considerable justification. Other economists saw it as an assertion that growth must be somehow "balanced" in order to be successful. Indeed, Hirschman (1958) cast *The Strategy of Economic Development* as an argument with—and refutation of—Rosenstein-Rodan (and others of the balanced-growth school), which I will argue was both a misunderstanding and self-destructive. Yet other economists tried to generate low-level equilibrium traps by invoking such mechanisms as interactions among income, savings, and population growth (see, for example, Leibenstein 1957; Nelson 1956). Such

1. It will become apparent that what I identify as "high development theory" is essentially the nexus among the external economy/balanced growth debate, the concept of linkages, and the surplus labor doctrine. This theory's golden age began with Rosenstein-Rodan (1943) and more or less ended with Hirschman (1958). Obviously this nexus does not cover all of what was happening in the field of development economics even at that time, but it is the core of what I believe needs to be recaptured.

mechanisms can also justify a Big Push, but they are very far from the spirit of the original story.

Fairly recently, however, Murphy, Shleifer, and Vishny (1989) offered a formalization of the Big Push that is quite close to the original spirit and also quite revealing about the essential aspects of high development theory. I offer a slightly streamlined presentation of their model and then turn to what it tells us.

The Big Push Model of Murphy, Shleifer, and Vishny

The model examines an economy that is closed to international trade. The economy can be described by assumptions about factor supply, technology, demand, and market structure. This model can serve as a motivating example to explain both the elements of high development theory and why that theory failed to establish a secure place in the mainstream of economics.

Factor supply. The economy is endowed with only a single factor of production—labor—in fixed total supply L . Labor can be employed in either of two sectors: a “traditional” sector, characterized by constant returns, or a “modern” sector, characterized by increasing returns. Although the same factor of production is used in the traditional and modern sectors, it is not paid the same wage. Labor must be paid a premium to move from traditional to modern employment. We let $w > 1$ be the ratio of the wage rate that must be paid in the modern sector to that in the traditional sector.

Technology. It is assumed that the economy produces N goods, where N is a large number. We choose units so that the productivity of labor in the traditional sector is unity in each of the goods. In the modern sector unit labor requirements are decreasing in the scale of production. For simplicity, decreasing costs take a linear form. Let Q_i be the production of good i in the modern sector. Then if the modern sector produces the good at all, the labor requirement will be assumed to take the form

$$(1) \quad L_i = F + cQ_i$$

where $c < 1$ is the marginal labor requirement. Note that for this example it is assumed that the relationship between input and output is the same for all N goods.

Demand. Demand for the N goods is Cobb-Douglas and symmetric. That is, each good receives a constant share $1/N$ of expenditure. The model will be static, with no asset accumulation or decumulation; expenditure thus equals income.

Market structure. The traditional sector is assumed to be characterized by perfect competition. Thus, for each good there is a perfectly elastic supply from the traditional sector at the marginal cost of production; given our choice of units, this supply price is unity in terms of traditional sector labor. By contrast, a single entrepreneur is assumed to have the unique ability to produce each good in the modern sector.

How will such a producer price? Given the assumption of Cobb-Douglas demand and a large number of goods, she will face unit-elastic demand. If she were an unconstrained monopolist, she would therefore raise her price without limit. But potential competition from the traditional sector puts a limit on the price: she cannot go above a price of 1 (in terms of traditional labor) without being undercut by traditional producers. So each producer in the modern sector will set the same price—unity—as would have been charged in the traditional sector.

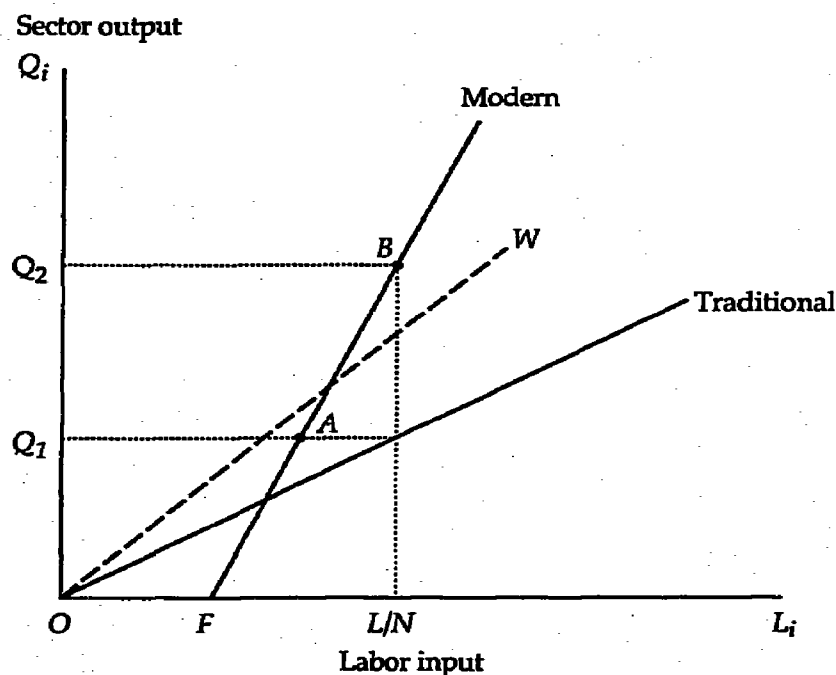
We can now ask the question, will production actually take place in the traditional or the modern sector? To answer this, it is useful to draw a simple diagram (figure 1). On the horizontal axis is the labor input, L_i , used to produce a typical good. On the vertical axis is that sector's output, Q_i . The two solid lines represent the technologies of production in the two sectors: a ray from the origin for the traditional sector, a line with a slope of $1/c$ for the modern sector.

From this figure it is immediately possible to read off what the economy would produce if all labor were allocated to either the modern or the traditional sector. In either case L/N workers would be employed in the production of each good. If all goods are produced traditionally, each good would have an output Q_1 . If they are all produced using modern techniques, the output is Q_2 . As drawn, $Q_2 > Q_1$; this will be the case provided that

$$(2) \quad \frac{(L/N) - F}{c} > L/N$$

that is, as long as the marginal cost advantage of modern production is suffi-

Figure 1. *The Big Push*



ciently large, or fixed costs are not too large, or both. Since this is the interesting case, we focus on it.

But even if the economy could produce more using modern methods, this does not mean that it will. Production must be profitable for each individual entrepreneur in the modern sector, taking into account the necessity of paying the premium wage w , as well as the decisions of all other entrepreneurs.

Suppose that an individual firm starts modern production while all other goods are produced using traditional techniques. The firm will charge the same price as that charged by traditional makers and hence will sell the same amount. Because there are many goods, we may neglect any income effects and suppose that each good continues to sell Q_1 . Thus, this firm would have the production and employment illustrated by point A.

Is this a profitable move? The firm uses less labor than would be required for traditional production but must pay that labor more. Draw in a ray from the origin whose slope is the modern relative wage w ; OW in the figure is an example. Then modern production is profitable given traditional production elsewhere if and only if OW passes *below* A. As drawn, this test is of course failed: it is not profitable for an individual firm to start modern production.

In contrast, suppose that all modern firms start simultaneously. Then each firm will produce Q_2 , leading to production and employment at point B. Again, this will be profitable if the wage line OW passes below B. As drawn, this test is satisfied.

Obviously there are three possible outcomes.² If the wage premium $w - 1$ is low, the economy always "industrializes"; if it is high, it never industrializes; and if it takes on an intermediate value, there are both low- and high-level equilibria.

One would hardly conclude from this model that the existence of multiple equilibria is likely; even given the assumptions, such multiple equilibria will occur only for some parameter values. And it is easy to critique the plausibility of the assumptions. Yet the model can serve as a useful jumping-off point for thinking about development models.

Some Analytical Implications

The Big Push model may be viewed as a minimalist demonstration of the potential role of pecuniary external economies for development, of the necessary conditions for such external economies, and of what a model of external economies must include.

External economies. It is clear that when there are two equilibria in this model, the movement from one to the other involves meaningful external economies. This is true even if one takes the wage premium for the modern sector to represent payment for the disutility of modern life; that is, if one

2. Actually four, if one counts the case in which (2) is not satisfied, so that the economy actually produces less using modern techniques. In this case it clearly stays with the traditional methods.

regards the gain in wages when workers move from traditional to modern jobs as having no welfare significance. Even in that case the industrialized equilibrium leaves workers indifferent while generating profits that would otherwise not exist. If one instead offers some kind of efficiency wage or surplus labor argument that places at least partial value on the rise in wages, the case is that much stronger.

But there are no technological external economies in the model. Why do pecuniary external economies matter here?

Necessary conditions. Two conditions are necessary to generate external economies in this model. First, there must be economies of scale in production. This is obvious from the geometry: if there were no fixed costs in the modern sector, the profitability of modern firms would not depend on how many other firms were using modern techniques.

Second, the modern sector must be able to draw labor out of a traditional sector that pays lower wages. I would like to stretch the point a bit here and think of the essence of the condition as the availability of an elastic supply of labor to the modern sector—labor that would not be employed in equally productive occupations otherwise. (This is what gives the model its vaguely Keynesian feel.) It is thus the interaction between internal economies of scale and elastic factor supplies that gives rise to *de facto* external economies.

Modeling. A final point is crucial. In writing a coherent model of the Big Push, it is necessary to deal with the problem of market structure. As long as there are unexhausted economies of scale in the modern sector, which are crucial to the whole argument, one must face up to the necessity of modeling the modern sector as imperfectly competitive. In the Murphy, Shleifer, and Vishny (1989) formulation, imperfect competition is dealt with by assuming a set of limit-pricing monopolists. This works well here, although, as we will see, it is not always an adequate device. The point is, however, that one must deal with the issue somehow. To the extent that there is anything to high development theory, it is intimately bound up with imperfect competition. If one tries to fudge that issue, as many economists have, one ends up with mush.

There are, unfortunately, no general or even plausible tractable models of imperfect competition. The tractable models always involve some set of arbitrary assumptions about tastes, technology, behavior, or all three. This means that in order to do development theory one must have the courage to be silly, writing down models that are implausible in the details in order to arrive at convincing higher-level insights.

This is not a new lesson. Trade theorists learned it more than a dozen years ago, when they realized that a reconstruction of trade theory to take account of increasing returns would necessarily involve abandoning all pretense of generality; growth theorists learned the same lesson a few years later. High development theory faltered because it did not take the same leap.

II. THE ELEMENTS OF HIGH DEVELOPMENT THEORY

This section offers an interpretive summary of the main elements of high development theory. It is not a literature survey: I base the discussion on only a few authors and must admit that I am imposing more coherence on their views, both across authors and within the work of an individual author, than an unbiased reader is likely to discover on reading their works. So this is, in a way, a statement of what high development economics could or should have been, rather than a portrait of what it was.

Economies of Scale and External Economies

A casual reading of the development literature suggests that there is a dividing line around 1960. Before 1960 writers on development generally assumed that economies of scale were a limiting factor on the ability to establish profitable industries in developing countries and that in the presence of such economies of scale pecuniary external economies assumed real welfare significance. They seem, however, to have been unaware of the degree to which economies of scale raise problems for explicit modeling of competition and of the extent to which the drive for formalism was pushing economics toward explicit models.³ After 1960, by contrast, economists working on development had been trained in the formalism of constant-returns general equilibrium; they did not so much reject the possibility that economies of scale might matter as simply fail to notice it.

The Big Push model presented above is one in which economies of scale at the plant level and an elastic supply of factors of production interact to yield pecuniary external economies with real welfare significance. In retrospect, it is remarkable how clearly similar stories were presented in many papers from the era of high development theory—and also how unaware many of the authors seem to have been of the extent to which their conclusions depended crucially on the non-neoclassical assumption of significant unexploited scale economies.

We begin with Rosenstein-Rodan (1943). In his seminal paper he illustrated his argument for coordinated investment by imagining a country in which 20,000 (!) “unemployed workers . . . are taken from the land and put into a large new shoe factory. They receive wages substantially higher than their previous income *in natura*.” Rosenstein-Rodan then goes on to argue that this investment is likely to be unprofitable in isolation but profitable if accompanied by similar investments in many other industries. Both crucial assumptions are clearly present: the assumption of economies of scale, embodied in the assertion that the factory must be established at such a large scale, and the assumption that these workers can be drawn elastically from among the unemployed or poorly paid agricultural workers.

3. In the field of economic geography, in which some of the tradition of high development theory survives, it appears that the problems raised by economies of scale for market structure are still not appreciated. See, for example, Dicken and Lloyd (1990), an excellent and clear-headed survey that nonetheless blithely ignores the problems of market structure.

Some—although not all—subsequent development writers invoked economies of scale as crucial to external economies. In the best papers the basic story comes through clearly. Fleming (1955) presented an analysis of the nature of external economies in development that focused on the interaction between factor supply and scale economies and that also, unlike Rosenstein-Rodan, points out clearly that the case for coordination falls apart without both assumptions.

Hirschman (1958) is not usually thought of as a thinker preoccupied with non-convexities. Nevertheless, his explanation of the concept of backward linkages explicitly invokes the importance of achieving minimum economic scale, and his discussion of forward linkages alludes to the role of scale as well, although more vaguely. I would argue, then, that a central concept of high development theory circa 1958 was the idea that economies of scale at the level of the individual plant translated into increasing returns at the aggregate level through pecuniary external economies. Admittedly some of the literature of the time does not seem to agree with my argument that scale economies were a key element of the theory. Nurkse (1952), while accepting that indivisibilities play a role in virtuous circles of development, denies that they are essential. Scitovsky (1954), in making the clear distinction between technological and pecuniary external economies, makes the now classic point that in competitive equilibrium it is actually efficient to ignore pecuniary external effects. When he searches for reasons to soften this conclusion, he provides only a single paragraph on scale effects, then turns to an extended discussion of expectational errors. Lewis's (1955) text on economic growth seems fairly innocent of the whole idea of external economies; indeed, the term does not even appear in the index. And Myrdal's (1957) exposition of the role of "circular and cumulative causation" sounds as if it must surely include a major role for economies of scale, but I have been unable to find in his work a single—even indirect—reference to their role. Indeed, when he offers an example of the process of circular causation, the external economies occur through the tax rate rather than through any private market spillover.

So it may be giving too much credit to our intellectual forerunners to think of 1950s development theory as involving a general appreciation of the way economies of scale at the level of the individual plant can aggregate to strategic complementarity at the level of the economy. But at least some theorists seem to have understood the point quite clearly.

Factor Supply

Probably the most famous paper in the literature of development economics is Arthur Lewis's (1954) "Economic Development with Unlimited Supplies of Labor." In retrospect it is hard to see exactly why. One interpretation of Lewis's argument is that the shadow price of labor drawn from the agricultural sector in developing countries is zero—or at least low—so that the social return to investment in industry exceeds its private return. It was pretty obvious even early on, however, that this was a fragile basis for the justification of protection and the promotion of industry.

Why then was Lewis so influential? One reason was probably that the surplus labor story, unlike many of the other development stories emerging at the time, could be formalized with relative ease; thus it gave economists a way to follow the mainstream's increasing emphasis on rigor and formalism while continuing to work on development. But even though Lewis himself made no reference to the external economy/development literature, his defense of the surplus labor idea also helped shore up one of the key ideas of that literature. The assumption that additional labor in the manufacturing sector could come out of rural underemployment was, as already noted, central to Rosenstein-Rodan (1943), and Fleming (1955) observed that in the absence of such an assumption industrial investments would be substitutes instead of complements.

Rosenstein-Rodan and Lewis stressed the elasticity of labor supply as a key factor in development. Other authors, such as Nurkse (1952), stressed the elasticity of capital supply, which also has a potential role in Murphy, Shleifer, and Vishny's (1989) two-period version of the Big Push. Krugman (1990) raises the possibility of multiple equilibria, and Hirschman (1958) emphasized at length the extent to which investment opportunities could elicit additional savings. Again, it may be stretching the point, but many development theorists in the 1950s seem to have been aware that elasticity of factor supply was also crucial to an external economy story of development.

It may be worth pointing out that in regional economics and economic geography, it is entirely natural to assume high elasticity of factor supply to a particular region, since factors of production may be attracted from other regions. This is one reason why the tradition of high development theory remained alive much longer among geographers than among economists; development stories such as those of Pred (1966) continued to seem natural and plausible.

Backward and Forward Linkages

The idea of linkages is one of the greatest sources of confusion in thinking about both the theory of development and development in practice. Hirschman (1958) introduced the term and presented it as something quite new. Later commentators have taken him at his word. Thus Little (1982) insists that since other authors had already explored at some length (if with some confusion) the possible role of pecuniary external economies, Hirschman's linkage concept must have crucially involved a nonpecuniary element. Yet in Hirschman's definition of backward linkages, as already mentioned, the role of pecuniary externalities linked to economies of scale is quite explicit: an industry creates a backward linkage when its demand enables an upstream industry to be established at minimum economic scale. The strength of an industry's backward linkages is to be measured by the probability that it will push other industries over the threshold.

Forward linkages are also defined by Hirschman as involving an interaction between scale and market size; in this case the definition is vaguer, but it seems to involve the ability of an industry to reduce the costs of potential downstream

users of its products and thus, again, push them over the threshold of profitability.

Seen in this way, the concepts of forward and backward linkages seem quite straightforward—and also less distinctive to Hirschman. Fleming (1955), in particular, argued that the “horizontal” external economies of Rosenstein-Rodan were less important than the “vertical” external economies that result when intermediate goods are produced subject to scale economies, which sounds awfully close to linkage theory.

It is also possible to offer simple formal models illustrating the concepts of forward and backward linkages. Indeed, the Murphy, Shleifer, and Vishny Big Push model can be seen as essentially driven by the backward linkages among goods; each good produced in the modern sector enlarges the markets for all other goods.

Forward linkages are a little more difficult to model. They ordinarily arise in the context of industries producing intermediate goods (although not always, as described below); this means that a more complex structure than that presented in the model above is required. Also, the limit-pricing assumption that makes imperfect competition easy in the Big Push model immediately rules out any forward linkage, since cost savings are never passed on to downstream consumers.

There are, however, slightly but not much more difficult models in which both forward and backward linkages do appear. In particular, in models of economic geography (Krugman 1991) it is quite natural to try to model concentrations of population by assuming that factor mobility interacts with economies of scale at the plant level to generate external economies.

In the simplest such model, a geographic concentration of manufacturing is held together for two reasons: firms want to locate close to the large market provided by other firms' workers (a backward linkage), and workers want to live close to the supply of goods provided by other firms (a forward linkage). The appendix to this paper presents this simplest model (again in a somewhat streamlined form compared with its initial version) and shows how the algebra naturally gives rise to terms that can be identified with forward and backward linkages.

As in the case of Lewis, it is slightly puzzling that Hirschman's work had such an impact. What he seems to have offered by way of distinct analysis were two hints about development planning. First, the focus on linkages involving intermediate goods rather than final demand suggested that development efforts could focus on a few strategic industries rather than seek an economywide Big Push; hence Hirschman's view that he was an opponent of Rosenstein-Rodan and Nurkse, even though they were far closer to one another in world view than any of them was to the emerging views of mainstream economics. Second, Hirschman's discussion seemed to suggest that appropriate critical industries could be identified by examining input-output tables, an exciting suggestion for the quantitatively oriented planner.

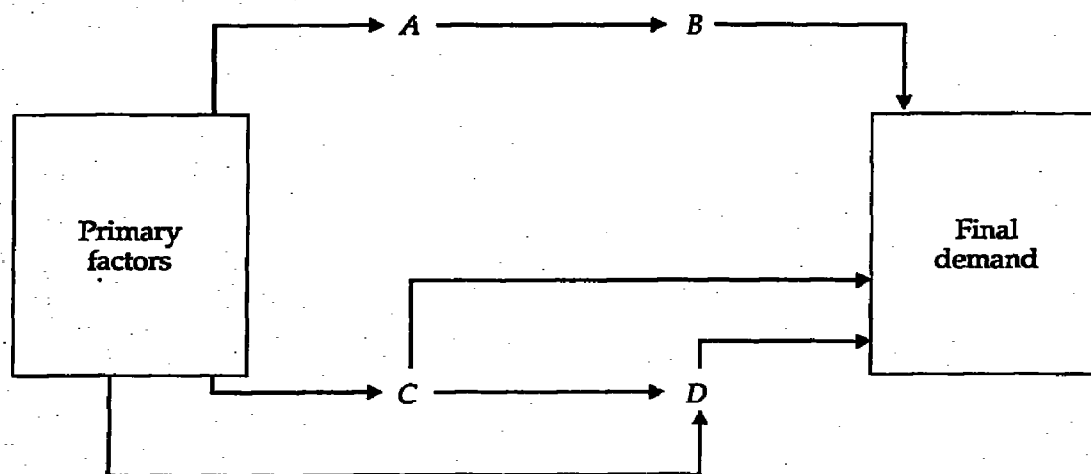
In fact, the concept of linkages, even as Hirschman presented it, implied no such thing. Consider the input-output structure illustrated by the arrows in figure 2. Imagine that there are four industries—*A*, *B*, *C*, and *D*. *A* is a pure intermediate-good industry, and *B* makes no direct use of factors of production. By contrast, *C* sells part of its output directly to final demand, while *D* uses primary inputs as well as inputs from *C*. Any classification using the input-output table will suggest that *A* has stronger forward linkages than *C* and that *B* has stronger backward linkages than *D*. But what if *A* and *B* are characterized by constant returns, while *C* and *D* are characterized by economies of scale? Then it could easily be that there is a coordination problem for *C* and *D*—that neither industry will emerge unless assured of sufficient scale of the other—without any corresponding problem for *A* and *B*. In other words, lots of entries in the input-output table tell the analyst little about which industries might actually play a catalytic role for the economy.

In general, it seems best to regard “linkages” as simply a particularly evocative phrase for the strategic complementarities that arise when individual goods are produced subject to economies of scale. This, in effect, argues that Hirschman’s distinctive contribution was more one of style than of substance, a point to which I will return below.

Summary

I have argued that a number of works in development economics written during the 1950s contained, more or less explicitly and more or less self-consciously, a theory in which strategic complementarity played a key role in development, in which external economies arose from a circular relationship whereby the decision to invest in large-scale production depended on the size of the market, and in which the size of the market depended on the decision to invest. Whatever the practical relevance of this theory, it made perfectly good logical sense.

Figure 2. *Input-Output Linkages among Four Industries*



Yet this development theory was subsequently abandoned to such an extent that classic papers in the field began to seem, as the physicist Wolfgang Pauli used to say, "not even wrong"—simply incomprehensible. We next turn to the reasons for that abandonment.

III. THE DECLINE OF HIGH DEVELOPMENT THEORY

Why did development economics fade away? One can, with some justification, offer the cynical explanation that the field waned with its funding. After all, development economists were most often consulted or given positions of influence in connection with the disbursement of foreign aid. As foreign aid became increasingly unpopular with the electorates of rich nations and as the real value of such aid not only failed to keep pace with gross world product but actually declined, development economics became a much less exciting career. One may also argue that development economics was discredited by a lack of practical success. After all, compared with the hopes of the 1950s and the 1960s, the performance of most developing countries has been dismal. (Indeed, the polite phrase "developing country" itself has become an embarrassment when it must be used in such sentences as "Per capita income in the developing countries of Sub-Saharan Africa has declined steadily since the mid-1970s.") It is unfair to blame Western economists for more than a small fraction of this failure but, as described below, the ideas of development economics were too often used to justify policies that in retrospect impeded rather than encouraged growth. Where rapid economic growth did occur, it happened in ways that were not anticipated by the development theorists.

Yet neither declining external demand for development economists nor their practical failures fully explain the collapse of the field. Purely intellectual problems were also extremely important. During the years when high development theory flourished, the leading practitioners failed to turn their intuitive insights into clear-cut models that could serve as the core of an enduring discipline.

Failures of Formalism

From the point of view of a modern economist, the most striking feature of the works of high development theory is their adherence to a discursive, non-mathematical style. Economics has, of course, become vastly more mathematical over time. Nonetheless, development economics was archaic in style even for its own time. Of the four most famous works, Rosenstein-Rodan's was approximately contemporary with Samuelson's formulation of the Heckscher-Ohlin model, while Lewis, Myrdal, and Hirschman were all roughly contemporary with Solow's initial statement of growth theory.

The problem was not that development economists were peculiarly incapable mathematically. Hirschman made a significant contribution to the formal theory of devaluation in the 1940s, while Fleming helped create the still-influential Mundell-Fleming model of floating exchange rates. Moreover, development practitioners were at the same time generating mathematical planning models—

first Harrod-Domar-style growth models, then linear programming approaches—that were actually quite technically advanced for their time.

So why wasn't high development theory expressed in formal models? Almost certainly for one basic reason: the difficulty of reconciling economies of scale with a competitive market structure.

The examples of the Big Push model and the linkage model show that models in the spirit of high development theory need not be very complicated. They must, however, somehow deal with the problem of market structure. This essentially means making some peculiar assumptions that allow one to exploit the bag of tricks that industrial organization theorists developed for thinking about such issues in the 1970s. In the 1950s, although the technical level of development economists was actually high enough to allow them to do the same thing, the bag of tricks wasn't there. So development theorists were placed in an awkward bind, with essentially sensible ideas that they could not quite express in fully worked-out models. And the drift of the economics profession made the situation worse. In the 1940s—and even in the 1950s—it was still possible for an economist to publish a paper that made persuasive points verbally without tying up all the loose ends. After 1960, however, the response to a paper like Rosenstein-Rodan's would have been: "Why not build a smaller factory (for which the market is adequate)? Oh, you're assuming economies of scale? But that means imperfect competition, and nobody knows how to model that, so this paper doesn't make any sense." It seems safe to say that such a paper would have been unpublishable any time after 1970, if not earlier.

Some development theorists responded by getting as close to a formal model as they could. This is to some extent true of Rosenstein-Rodan, and certainly of Fleming (1955), which gets painfully close to being a full model. But others at least professed to see a less formal, less disciplined approach as a virtue. It is in this light that one needs to see Hirschman and Myrdal. These authors are often cited today (by me among others) as forerunners of the recent emphasis in several fields on strategic complementarity. Their books, however, actually marked the end, not the beginning, of high development theory. Myrdal's central thesis was the idea of "circular causation." But the idea of circular causation is essentially already there in Young (1928), not to mention Rosenstein-Rodan, or Nurkse, who in 1952 referred repeatedly to the circular nature of the problem of getting growth going in poor countries. So Myrdal was, in effect, encapsulating an already extensive and familiar set of ideas rather than a new departure. Similarly, Hirschman's idea of linkages was more distinctive for the effectiveness of the term and the policy advice that he derived loosely from it than for its intellectual novelty; in effect Rosenstein-Rodan was already talking about linkages, and Fleming very explicitly had both forward and backward linkages in his discussion.

What marked Myrdal and Hirschman was not so much the novelty of their ideas but their stylistic and methodological stance. Until their books were published, economists doing high development theory were trying to be good mainstream economists. They could not develop full formal models, but they got as

close as they could to the increasingly model-oriented mainstream. Myrdal and Hirschman abandoned this effort and eventually took stands on principle against any effort to formalize their ideas.

One imagines that this was initially liberating for them and their followers. Yet in the end it was a vain stance. Economic theory is essentially a collection of models. Broad insights that are not expressed in model form may temporarily attract attention and even win converts, but they do not endure unless codified in a reproducible and teachable form. You may not like this tendency; certainly economists tend to be too quick to dismiss what has not been formalized (although I believe that the focus on models is basically right). Like it or not, however, the influence of ideas that have not been embalmed in models soon decays. This was the fate of high development theory. Myrdal's effective presentation of the idea of circular and cumulative causation and Hirschman's evocation of linkages were stimulating and immensely influential in the 1950s and early 1960s. By the 1970s (when I was a student of economics) they had come to seem not so much wrong as meaningless. What were these guys talking about? Where were the models? High development theory was not so much rejected as simply bypassed.

The exception proves the rule. Lewis's surplus labor concept was the model that launched a thousand papers—even though surplus labor assumptions were already standard among development theorists, the empirical basis for assuming surplus labor was weak, and the idea of external economies/strategic complementarity is surely more interesting. The point was, of course, that precisely because he did not mix economies of scale into his framework, Lewis offered theorists something they could model using available tools.

Practical Failures

Development theorists were unable to formulate their ideas with the precision required by an increasingly model-oriented economic mainstream and were thus left behind. Although I believe this to be the main explanation of what went wrong, it is also true that practical failures and empirical evidence had something to do with the decline of development economics.

Little (1982) has pointed out that development theorists in the 1950s were by and large not optimists. Indeed, they were generally more pessimistic than turned out to be warranted (except in Africa). But their pessimism was largely based on doubts about the ability of countries to carry out the coordinated effort that they regarded as central to industrialization.

What actually happened was that most developing countries were quite successful at developing industrial bases, but since these industries were highly inefficient, industrialization turned out not to have much to do with development. This called into question the whole idea that the problem of coordinating investments in the face of external economies was a major part of the underdevelopment story.

The successes of development have also been an embarrassment for high development theory. Most versions of that theory tacitly assumed that firms would

produce only for the domestic market, a view that seemed sensible given the trade pessimism of the time. The great success stories have, however, involved export-oriented industrialization.

Another major embarrassment for development has been the realization of the extent to which the rhetoric of development theory has been used to cover poorly conceived or even corrupt policies. This is a familiar subject, acerbically surveyed in Little (1982). The calculation of effective rates of protection, whatever the method's flaws, revealed levels that were often absurdly high, some cases of negative value added at world prices, and highly variable rates of protection across industries that were difficult to justify. Country studies of trade policy revealed a heavy preference for complex administrative regulations that were evidently more costly than tariffs and, moreover, failed to yield revenue (Little, Scitovsky, and Scott 1970). Studies of repressed financial systems showed similar irrationalities (see, for example, McKinnon 1973). And it became apparent that the incentives provided by administratively generated rents were becoming major objectives of both legal and illegal economic activity (Krueger 1974).

These observations of bad policies based on old development economics provided a key argument in what amounted to the formation of a new orthodoxy. Little (1982) has argued that in the 1950s what he calls a "structuralist" theory of economic development was in effect an orthodoxy. I find it hard to link the ideas of high development theory as described above with his definition of the structuralist view, which is that developing country markets are rigid and need to be pushed into action by government. But it is certainly true that in the 1950s market failures were seen as pervasive, and the case for intervention was taken to be not so much an empirical observation as an obviously true conclusion from obviously true theory. By about 1980 a belief in the efficacy of free trade and free markets for developing countries had similarly taken hold, its intellectual credibility underpinned by the demonstration of market efficiency in neoclassical general equilibrium theory. This orthodoxy also effectively denies that there is anything special about the situation of developing countries compared with those of richer nations. The poor are no different from you and me—they just make less money.

What was ironic was that a competitive neoclassical orthodoxy settled in on the development front just as that orthodoxy was breaking up in other fields.

IV. RECENT THEORETICAL DEVELOPMENTS AND HIGH DEVELOPMENT THEORY

We can now see that whatever bad policies may have been implemented in the name of high development theory, the theory itself makes quite a lot of sense. Indeed, in some ways it was a remarkable anticipation of ideas that would come to analytical fruition thirty years later in the fields, for example, of international trade and economic growth. This section reviews these developments and asks where we go from here: how do we recapture the insights of development economics, and what good might they do for policy formulation?

International Trade

One of the main empirical criticisms of models of high development theory is their emphasis on domestic rather than international markets and their neglect of international trade. So it is somewhat ironic that the initial rehabilitation of the idea that economies of scale, external economies, and strategic complementarity are important outside the field of industrial organization came from the theory of international trade.

The literature on the "new trade theory" is much too extensive to cite. It is, however, possible to summarize the main lines of thought. The initial models, many of which are summarized in Helpman and Krugman (1985), invoke economies of scale at the level of individual firms to explain national specialization in individual products and hence the observed large volumes of intra-industry trade. In these initial models, economies of scale at the firm level did not aggregate to yield any kind of strategic complementarity, and thus there were few parallels with what I call high development theory.

In subsequent developments, however, many of the themes of high development theory have in effect reentered. Trade theorists noted (Ethier 1982) that increasing returns in the production of intermediate goods generate external economies at the level of final goods. If intermediate goods are nontradable, these *de facto* external economies are country-specific, and one easily generates examples of multiple equilibria (Helpman and Krugman 1985, ch. 11).

Nor need one assume nontradability. Models in which there are nonprohibitive transport costs can also yield external economies; the model in the appendix to this paper is an example. And indeed in some such models the likelihood and welfare significance of multiple equilibria can actually increase when transport costs fall and trade increases (Krugman 1991, ch. 3). It is often argued that Big Push stories could only be valid in an autarkic economy. What the models suggest is that as long as some things (perhaps only factors of production) are costly to trade, the possibility of country-specific external economies remains.

But are such external economies of real practical importance for international trade? Systematic empirical testing is difficult. At least among business observers, however, external economy stories of international specialization are widely accepted. Porter's (1990) analysis of international competition can be viewed in large part as a return to traditional development themes in the context of an advanced country.

In the academic field of international trade, recent work has increasingly shifted to the analysis of long-run growth (Grossman and Helpman 1991). This move in trade is part of the broader movement known as "new growth theory."

Growth

The new growth theory was created by Romer (1986) and Lucas (1988). It attempts to get away from the conventional Solow result that most long-term

per capita growth arises from exogenous technical progress, through three related hypotheses: (1) social returns to investment are higher than private returns because of external economies; (2) capital broadly defined is a much larger share of input than conventionally measured; and (3) technical progress is largely an endogenous result of market-driven research and development.

Of these three hypotheses, the first is clearly in the same spirit as that of high development theory. In Romer's initial formulation, external economies were treated as purely technological in form. (Fashions in plausibility change: Scitovsky did not hesitate to ridicule Marshall's "bucolic" examples and proclaim the "scarcity of pure technological external economies," and indeed most 1950s theorists seem to have regarded pecuniary externalities as the only interesting case.) In later papers, however, Romer imported techniques from the trade literature to show how growth driven by external economies could arise from economies of scale at the plant.

Given the immense interest in new growth theory, is there any point in revisiting old development theory? Or to put it another way, even if modern theorists have reinvented the wheel, didn't they get it better this time around?

I think the answer is no. New growth theory has been preoccupied with a different question than high development theory: how to explain the persistence of growth rather than how to get it started. And it is notable that new growth models tend to assume that the economy has only one sector, or that all sectors are symmetric. By contrast, high development theory had a core preoccupation with the difference between modern sectors that were presumed to be characterized by economies of scale and traditional sectors that were not. Even within the modern sector, the linkage concept implied a search for key industries. So while the philosophy of new growth theory is in essence a rediscovery of high development theory, it has not returned to the same questions.

The most essential difference, of course, is that despite its abstract approach, high development theory was intended as a guide to policy, while new growth theory was not. The latter theorists only try to explain the world, while their predecessors thought they could change it. The point is that if the underlying economic assumptions of high development theory have once again been legitimized, one can ask the same questions again.

Policy Implications

It is not my intention to offer a clarion call for interventionist trade and industrial policies, much less for a return to import substitution. Indeed, a proper interpretation of the insights from old development theory (and new trade and growth theories) suggests that the costs of fragmenting markets are high precisely because markets are not characterized by constant returns and perfect competition.

Instead, I want to offer two more modest suggestions. First, this survey of intellectual developments may serve as a caution against carrying a free-market

orthodoxy too far. It makes considerable sense for the World Bank and other multilateral agencies to push very hard for liberal policies in developing countries, given the demonstrated tendencies of these clients to engage in economically irrational interventions. But in the back of our minds we should remember that it is not true that economic theory "proves" that free markets are always best: there is an intellectually solid case for some government promotion of industry—one that has often seemed empirically plausible to sophisticated observers. In other words, don't get caught up too much in the orthodoxy of the moment.

It is worth recalling that in the course of a generation, the ideas of high development theory went from being regarded as self-evident to being regarded as logically meaningless. They were actually ideas that were sensible and coherent, but they were of more limited application than their creators imagined. The lesson here is that both casual empiricism and intellectual narrow-mindedness will lead one badly astray.

The second suggestion is that we need a reorientation of research. Research on trade and industrial policy in developing countries is still dominated by the agenda of the counterrevolution that began in the 1960s: the horrors of import substitution and the distortions imposed by government policy. This is valuable work, and governments continue to give it opportunities to be useful. Yet it is probably time once again to focus on market as well as government failures.

APPENDIX. A SIMPLE MODEL OF FORWARD AND BACKWARD LINKAGES

The Murphy, Shleifer, and Vishny (1989) Big Push model essentially captures, in a minimalist way, the idea of backward linkages. The way it solves the problem of oligopoly pricing, however (by limit pricing to the level set by a traditional, constant-returns technology), rules out any forward linkages. The purpose of this appendix is to show in a minimalist way how a somewhat different formulation can produce both backward and forward linkages.

The example uses regional concentration rather than the transition from traditional to modern production. This is largely because in Krugman (1991) I have already worked out this model (although this is a streamlined exposition). One may also argue, however, that the regional version is more plausible than the application to the whole transition to modernity. Indeed, Myrdal (1957) chose to begin his discussion with the case of relative regional development within countries rather than international disparities, presumably because he too regarded that case as easier to sell. High development theory lived on in the work of regional scientists such as Pred (1966) well after it had declined to obscurity in mainstream economics. And as a practical matter, regional disparities within developing countries are significant issues. The main point of this appendix is, however, to provide an illustrative model on which to hang the discussion in the text.

Assumptions of the Model

We consider a country that has two regions, East and West, and produces two kinds of goods, agricultural and manufactured. Agricultural production is homogeneous, produced under constant returns and perfect competition. Manufactures consist of a number of differentiated products, each produced subject to economies of scale, with a monopolistically competitive market structure.

Everyone in the economy is assumed to share the same tastes. Welfare is a Cobb-Douglas function of consumption of agricultural goods and a manufactures aggregate:

$$(A.1) \quad U = C_M^\pi C_A^{1-\pi}.$$

Note that given this functional form, π is the share of expenditure that falls on manufactures.

The manufactures aggregate is in turn a CES function of consumption of individual manufactured goods, of which there is a large number, not all of them actually produced:

$$(A.2) \quad C_M = \left[\sum_i c_i^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}}.$$

As long as a large number of manufactured goods is produced, this functional form ensures that the elasticity of demand for any individual good is simply σ .

There are two factors of production, each of which is specific to a particular sector. "Farmers" produce agricultural goods and "workers" produce manufactured goods; farmers cannot become workers or vice versa. To save notation we choose units so that there is a total of $1 - \pi$ farmers and π workers (this choice of units leads to the result that the wages of farmers and workers are equal in equilibrium).

The geographic distribution of farmers is taken as fixed, with $(1 - \pi)/2$ farmers in each region. Workers move to whichever region offers them a higher real income.

Farmers produce their goods under constant returns to scale. The economies of scale in manufacturing take the form of a linear cost function, in which a fixed cost in manufacturing labor must be incurred in order to produce any individual variety of manufactures:

$$(A.3) \quad L_{Mi} = \alpha + \beta x_i.$$

Finally, we assume that there are costs of transporting manufactured goods between the two regions. These take Samuelson's "iceberg" form, in which only a fraction of a good that is shipped arrives (so that transport costs are incurred in the good shipped). We let $\tau < 1$ be the fraction of a manufactured good shipped that actually arrives. Transport of agricultural goods is assumed to be costless, an assumption made for analytical convenience; it ensures that the wage rates of farmers and the prices of agricultural goods are the same in the two regions.

Pricing and Competition

Since there is a large number of potential manufactured goods, each of them produced subject to economies of scale, there is no reason for any two firms to try to produce the same good; the market structure of manufactures will therefore be one of monopolistic competition.

The producer of any one good will face an elasticity of demand σ . Her profit-maximizing price is therefore a constant markup over marginal cost,

$$(A.4) \quad p_i = \frac{\sigma}{\sigma - 1} \beta w$$

where w is the wage rate of manufacturing workers.

If there is free entry, however, profits will be driven to zero. The zero-profit condition may be written

$$(A.5) \quad (p - \beta w)x = \alpha w.$$

Note that with zero profits, price equals average cost. But this means that the ratio of average cost to marginal cost—which is one measure of economies of scale—is simply $\sigma/(\sigma - 1)$. Thus equilibrium economies of scale are a function only of σ , so that σ , even though it is a parameter of tastes rather than technology, nonetheless acts as a sort of inverse index of the importance of increasing returns.

The zero-profit and pricing conditions together imply that the output of a representative manufacturing firm is

$$(A.6) \quad x = \frac{\alpha(\sigma - 1)}{\beta}.$$

Consider a region with a resident labor force of L_M workers; the number of manufactured goods that region will produce is

$$(A.7) \quad n = \frac{L_M}{\alpha + \beta x} = \frac{L_M}{\alpha \sigma}.$$

Sustainability of a Core-Periphery Pattern

We now ask the following question: is a situation in which all manufacturing is concentrated in one region, leaving the other region with only agriculture, an equilibrium? Since it doesn't matter which region we choose, we examine the sustainability of an equilibrium with East as the manufacturing core and West as the agricultural periphery.

As we will see in a moment, there are two "centripetal" forces tending to keep a manufacturing core in existence and one "centrifugal" force tending to pull it apart. Holding the core together are the desire of firms to locate close to the larger market and the desire of workers to have access to the goods produced by other workers. It is these two forces that may be thought of as corresponding, respectively, to backward and forward linkages.

These forces can only be seen analytically by examining their tension against the third force, which tends to break the core apart: the incentive of firms to move out to serve the peripheral agricultural market. What we will do is derive a criterion that determines whether the backward and forward linkages are strong enough to sustain an established core.

We begin by noting that given our choice of units, the wage rates of workers and farmers will be equal. That is, a share of expenditure π is spent on manufactured goods (including those goods that "melt" in transit) and (since profits are zero) ends up as wages of workers; but we have also chosen units so that a fraction π of the population consists of workers. Given this choice of unit, the wage rates will necessarily be equal.

Now ask how the incomes of the two regions compare. East has half of the farmers, who receive a share $(1 - \pi)/2$ of total income, plus all of the workers, who receive a share π . Let total income be unity; then the income of East is

$$(A.8) \quad Y^E = (1 + \pi)/2.$$

West has only its immobile farmers, who receive a share $(1 - \pi)/2$ of income; so the income of West is

$$(A.9) \quad Y^W = (1 - \pi)/2.$$

This situation, in which all manufacturing is concentrated in East, will be sustainable if it is unprofitable for any firms to enter in West. So we must determine whether it is profitable for an individual firm to "defect" by commencing production in West.

The brief answer is that it will be profitable to defect if a firm that moves to West can afford to pay workers a higher real wage than they are currently receiving in East.

Let n be the (large) number of firms currently producing in East. Then the sales of each of these firms will be

$$(A.10) \quad s^E = \pi/n.$$

If a firm were to try to start production in West, it would need to attract workers. Suppose that to do this it must pay a wage that is w times the wage that East's firms are paying. Then, because the profit-maximizing price is a constant markup over marginal labor cost, such a firm will charge an f.o.b. price that is also w times as high as that of East's firms.

To find the sales of the defecting firm, we note that in West's market, the c.i.f. price of East's good will be $1/\tau$ times its f.o.b. price. Since the elasticity of substitution is σ , this means that in West's market, the value of the sales of the firm will be $(w\tau)^{-(\sigma-1)}$ times the sales of a representative Eastern firm. By similar reasoning, in East's market the sales ratio will be $(w/\tau)^{-(\sigma-1)}$. Thus the relative sales of a Western firm will be

$$(A.11) \quad S_W/S_E = S = \frac{1}{2} w^{-(\sigma-1)} [(1 + \pi) \tau^{\sigma-1} + (1 - \pi) \tau^{1-\sigma}].$$

How high a wage can such a firm afford to pay? It is easiest to think of the firm as using an operating surplus to cover fixed costs. The operating surplus,

in the Dixit-Stiglitz model, is proportional to sales. The fixed cost is incurred in labor, so the maximum relative wage that the firm can afford to pay is defined by

$$(A.12) \quad S = w.$$

This gives us the wage equation

$$(A.13) \quad w = \left[\frac{1 + \pi}{2} \tau^{\sigma-1} + \frac{1 - \pi}{2} \tau^{1-\sigma} \right]^{1/\sigma}.$$

The final step is to ask whether this maximum nominal wage implies a real wage higher or lower than the real wage in East. Since this is a lone defecting firm, all manufactured goods (except for its own negligible contribution) would have to be imported. Recall that only a fraction τ of a good that is shipped arrives. The price of manufactured goods in West will therefore be $1/\tau$ times as high as that in East. The overall price index, which is a geometric average of manufactures and agricultural goods, will thus be $\tau^{-\pi}$ times as high. The maximum relative real wage a defecting firm can pay is thus

$$(A.14) \quad \omega = w\tau^{\pi} = \tau^{\pi} \left[\frac{1 + \pi}{2} \tau^{\sigma-1} + \frac{1 - \pi}{2} \tau^{1-\sigma} \right]^{1/\sigma}.$$

A concentration of all manufacturing in East is an equilibrium only if this relative real wage is less than one (that is, a defecting firm could not profitably induce labor to move). We can immediately note that if manufacturing were a small part of the economy (π close to zero), concentrated manufacturing would never be an equilibrium. Equation A.14 would reduce to

$$(A.15) \quad \omega = \left[\frac{1}{2} \tau^{\sigma-1} + \frac{1}{2} \tau^{1-\sigma} \right]^{1/\sigma}$$

which is always greater than one because of Jensen's inequality. This result arises because of the attraction of dispersing production toward the dispersed rural market.

A comparison of (A.14) and (A.15) reveals immediately the potential role of a large manufacturing sector to generate linkages that lead to concentration. In the case with large π , the first term becomes less than one and is smaller the larger is π . This first term represents the attraction to workers of locating close to the existing manufacturing concentration to have access to the goods it produces; in effect it measures a forward linkage. At the same time, a large π shifts the weights on the average inside the brackets from the second term, which exceeds one, toward the first term, which is less than one, and therefore reduces the size of this second term as well. This effect occurs because the larger the manufacturing sector, the larger the market in the region that gets the manufacturing. That is, this effect arises because of the attraction of producing near the market provided by other firms—in effect a backward linkage.

If the forward and backward linkages are strong enough, which they will be if π is large enough, there will be enough strategic complementarity to lead to concen-

trated manufacturing. Krugman (1991) shows that this depends on the degree of economies of scale; concentration is also more likely when scale economies are stronger.

Finally, we may note that if concentration in East is an equilibrium, so is concentration in West, because the regions are symmetric. Thus strong forward and backward linkages imply multiple equilibria.

This model is, of course, very simplified compared with the ideas of linkage that were in the minds of most development theorists. In particular, there are no intermediate goods and thus no input-output structure. But it does show in the simplest fashion that these concepts do at least make sense.

REFERENCES

- Dicken, Peter, and Peter Lloyd. 1990. *Location in Space*. London: Harper and Row.
- Ethier, Wilfred. 1982. "National and International Returns to Scale in the Modern Theory of International Trade." *American Economic Review* 72:389-405.
- Fleming, J. M. 1955. "External Economies and the Doctrine of Balanced Growth." *Economic Journal* 65 (June):241-56.
- Grossman, Gene M., and Elhanan Helpman. 1991. *Innovation and Growth in the Global Economy*. Cambridge, Mass.: MIT Press.
- Helpman, Elhanan, and Paul Krugman. 1985. *Market Structure and Foreign Trade*. Cambridge, Mass.: MIT Press.
- Hirschman, Albert O. 1958. *The Strategy of Economic Development*. New Haven, Conn.: Yale University Press.
- Krueger, Anne. 1974. "The Political Economy of the Rent-Seeking Society." *American Economic Review* 64(3):291-303.
- Krugman, Paul. 1990. "Endogenous Innovation, International Trade, and Growth." In Paul Krugman, ed., *Rethinking International Trade*. Cambridge, Mass.: MIT Press.
- . 1991. *Geography and Trade*. Cambridge, Mass.: MIT Press.
- Leibenstein, Harvey. 1957. *Economic Backwardness and Economic Growth*. New York: Wiley.
- Lewis, W. A. 1954. "Economic Development with Unlimited Supplies of Labor." *Manchester School of Economic and Social Studies* 22 (2):139-91.
- . 1955. *The Theory of Economic Growth*. London: Allen and Unwin.
- Little, I. M. D. 1982. *Economic Development*. New York: 20th Century Fund.
- Little, I. M. D., Tibor Scitovsky, and Maurice Scott. 1970. *Industry and Trade in Some Developing Countries*. Oxford, U.K.: Oxford University Press.
- Lucas, Robert E., Jr. 1988. "On the Mechanics of Economic Development." *Journal of Monetary Economics* 22:2-42.
- McKinnon, Ronald. 1973. *Money and Capital in Economic Development*. Washington, D.C.: Brookings Institution.
- Murphy, Kevin M., Andrei Shleifer, and Robert Vishny. 1989. "Industrialization and the Big Push." *Journal of Political Economy* 97 (October):1003-26.
- Myrdal, Gunnar. 1957. *Economic Theory and Under-developed Regions*. London: Duckworth.
- Nelson, R. R. 1956. "A Theory of the Low-Level Equilibrium Trap in Underdeveloped Economies." *American Economic Review* 46 (December):894-908.

- Nurkse, Ragnar. 1952. "Some International Aspects of the Problem of Economic Development." *American Economic Review* 42 (May):571-82.
- Porter, Michael. 1990. *The Competitive Advantage of Nations*. New York: Free Press.
- Pred, Allan. 1966. *The Spatial Dynamics of US Urban-Industrial Growth*. Cambridge, Mass.: MIT Press.
- Romer, Paul. 1986. "Increasing Returns and Long-Run Growth." *Journal of Political Economy* 94:1002-37.
- Rosenstein-Rodan, Paul N. 1943. "Problems of Industrialization of Eastern and South-Eastern Europe." *Economic Journal* 53 (June-September):202-11.
- Scitovsky, Tibor. 1954. "Two Concepts of External Economies." *Journal of Political Economy* 62 (April):143-57.
- Young, Allyn. 1928. "Increasing Returns and Economic Progress." *Economic Journal* 38 (December):527-42.

COMMENT ON "TOWARD A COUNTER-COUNTERREVOLUTION
IN DEVELOPMENT THEORY," BY KRUGMAN

Joseph E. Stiglitz

I wholeheartedly agree with the main points that Professor Krugman raises so eloquently. I would like to raise two related issues. First, I do not think Krugman's interpretation of the intellectual history of development economics is quite right. Second, although Krugman has identified two factors that represent important critiques of the neoclassical paradigm and form the basis for the construction of a "new view," his vision is too narrow: there are equally important factors that he has ignored. In brief, Krugman argues that:

- High development theory left the mainstream of economics.
- The reason for this was that "development theorists were unable to formulate their ideas with the precision required by an increasingly model-oriented economic mainstream, and were thus left behind."
- Attention was diverted by ideas like Lewis's (1955) surplus labor model that could be easily formalized.
- Real-world events, such as the failure of industrialization, "called into question [the idea that] coordinating investments in the face of external economies was a major part of the underdevelopment story."
- The resurrection of high development theory can be attributed to the development of simple models of increasing returns.

Each of these propositions is debatable. To take the first, whether an idea is or is not in the mainstream depends on what river you are sitting beside. The mainstream looks quite different depending on whether one is viewing it from the banks of the Charles (that is, from the Massachusetts Institute of Technology), the Cam (Cambridge), or the Cherwell (Oxford), let alone from the shores of Lake Lagunita (Stanford University). At these institutions—and others—scholars never stopped talking about the importance of externalities, returns to scale, imperfect competition, and technological change and the relationships among them. Research continued on modeling not only the endogeneity of market structure but also "endogenous growth," with theoretical and empirical

Joseph E. Stiglitz is professor of economics at Stanford University. He is indebted to Joshua Gans, Mark Gersovitz, and Andrés Rodríguez for helpful comments on the issues raised here.

© 1993 The International Bank for Reconstruction and Development / THE WORLD BANK

work aimed at understanding the determinants of the transfer, absorption, development, and adaptation of new technologies. These ideas were and continue to be a major focus of academic research and a standard part of the graduate curriculum.

At Cambridge, for instance, throughout the 1950s, 1960s, and 1970s Kaldor (1970, 1972) emphasized three of the elements that Krugman stresses—increasing returns, imperfect competition, and technological change. With Mirrlees (Kaldor and Mirrlees 1969), he provided a formal model of growth theory that captured some of his ideas. Kaldor recognized the profound policy implications of these ideas, and they provided the theoretical foundations for the selective employment tax enacted by the Labour government during his tenure as economic adviser.¹

At Stanford, Arrow (1962) developed one of the central versions of what would later be called a model of endogenous growth. Uzawa (1963, 1965) developed another, at Stanford and Chicago.² Many other formal models were constructed and published, including the well-known paper by Inada (1969), which actually used the word “endogenous.”

To be sure, we were not satisfied with the models offered. The results, particularly those pertaining to steady states, were highly sensitive to the special parameterizations, and one of the objectives of the research program was to explore these sensitivity issues. (Inada 1969 illustrates this line of analysis.) And we were aware that with increasing returns, markets would be imperfectly competitive, and we needed to model those imperfections. Krugman is right in identifying the advances in the theory of imperfect competition of the 1970s as providing a crucial building block. But he fails to mention the other problem, raising the interesting question of the extent to which progress can be attributed to a lowering of standards—a willingness to work with special (should I say *ad hoc*?) consequential parameterizations, which generated results that were not robust.

The 1970s and 1980s were marked by advances in the modeling of externalities, technological progress, and returns to scale. Major strands of research on evolutionary modeling were associated with Nelson and Winter (1982) and Dosi and others (1988); the analysis of network externalities was undertaken by David (1987) and Arthur (1985, 1988, 1989) and the work on the microeconomics of technological progress by Dasgupta and Stiglitz (1980a, 1980b) and Stiglitz (1988). Aoki (1970) formalized the concept of Marshallian externalities, and Greenwald and Stiglitz (1986) provided a general framework for the analysis of externalities. Although they focused on incomplete markets and

1. Kaldor (1970) explores the implications of these ideas in the context of regional development. See also Kaldor (1972).

2. Modeling “endogenous” technical change was a major thrust of research in this period. See, for instance, the collection of essays by Shell (1967); Atkinson and Stiglitz (1969); Bardhan (1970); and Teubal (1967).

imperfect information, their framework was equally applicable to economies with tax distortions and imperfect competition. They showed that what might be thought of as pecuniary externalities essentially always mattered, as long as the economy was not (constrained) Pareto efficient, and that in these circumstances the economy was essentially *never* constrained Pareto efficient.

Indeed, not only did Krugman ignore major strands of theoretical work; he also ignored major empirical research projects that were exploring some of the central issues of high development economics, such as the Economic Commission for Latin America (ECLA) program under Jorge Katz (see Katz 1987).

Not only was research on these ideas under way, but policies were also informed by these perspectives. I have already referred to the selective employment tax in Great Britain. Certainly current writings on the policies pursued in Japan (see Komiya, Okuno, and Suzumura 1988), the Republic of Korea (see Amsden 1989; Pack and Westphal 1986), and Taiwan (China) (see Wade 1990) suggest that these economic theories were an important part of the intellectual background for those programs.

In short, in my reading of intellectual history, high development economic theory never died; it was alive and well, and the rest of the world may have taken little note of its absence on the banks of the Charles.

I would like to agree with Krugman concerning the importance of theory and models for shaping the direction of the profession. Yet I remain unpersuaded of the dominant role assigned by Krugman, for several reasons.

The first is perhaps a normative rather than a positive argument: that we can write down a model of a phenomenon proves almost nothing. It does not make the idea right or wrong, important or unimportant. It is—at most—a test of certain logical relations, of the consistency of certain ideas. Formalizing ideas is extremely important for quite another set of reasons: it leads to better and more concise debates and to precise and more useful questions!

Second, there were formal models available. Many of us had published models with all the characteristics that Krugman would like—simplicity, elegance, and rigor. The lack of such models simply cannot account for the temporary demise of high development theory—if that had happened.

Conversely, had Rosenstein-Rodan (1943) succeeded in formalizing his ideas, I doubt that those ideas would have been any more palatable. In his model the income effects associated with increasing returns leave the economy stuck in a low-level equilibrium. As Krugman points out, the problem arises from a lack of demand, but once we open the economy to international trade, this argument loses its force.³

I also take issue with Krugman's contention that what accounts for the central role of surplus labor in the 1970s is not the importance of surplus labor but the

3. There are contexts in which a more subtle version of the argument might be relevant: income effects are obviously important for nontraded goods, and there may be spillovers between the returns to scale for nontraded intermediate goods used to produce traded and nontraded final goods.

ease of modeling it. The model was successful because it described central aspects of the development process, including the reallocation of labor from the low-productivity rural sector to the high-productivity urban sector and the high rates of capital accumulation that were facilitated by low wages. These are still important aspects of the development process, although they are far from the whole story.⁴

I would submit that a far more plausible explanation for the seeming demise of high development theory is that the same currents that led to the dominance of free market ideology in the United Kingdom and the United States were reflected—at least in the United States—in the dominance of those ideas in certain intellectual circles. In short, it was as much the market demand for ideas as the supply of models that was crucial.

Krugman is correct in his contention that real world events, such as the failure of the planning paradigm, reinforced these currents, but they do not fully account for them. I say this for two reasons. First, the critique of the neoclassical paradigm was far broader than its omission of increasing returns and externalities. In the 1970s we realized not only that the informational assumptions that underlay that model were implausible but that *all* the results of the model were highly sensitive to these assumptions (see, for instance, Stiglitz 1985). But a careful analysis of the implications of imperfect and costly information provided a critique of both the free market and the planning paradigms (see Stiglitz 1992). Krugman seems to suggest that once the planning paradigm was rejected, the only alternative was the free market paradigm. There were alternatives available, and to explain which alternatives the profession focused on, one has to look elsewhere.

Second, not only is Krugman's view of the intellectual alternative incorrect; his analysis ignores the debates about the success of the East Asian economies, which was based, according to some interpretations, on selective government intervention, consistent with the new insights of microeconomic analysis (see Amsden 1989; Komiyama, Okuno, and Suzumura 1988). Amsden cites Kaldor (1970), while Itoh and others (1991) cite papers from the 1970s and early 1980s, well before the formal models that Krugman would like to credit with the resurgence of high development economics were written.⁵

4. Krugman seems to be unaware of the work that originally established Lewis's reputation as an economist (see, for example, Lewis 1949): the importance of overheads (nonconvexities and increasing returns), which he stressed throughout the 1970s and 1980s in courses on development economics at Princeton.

5. This is not the only evidence that intellectual developments outside economics help us understand the dominant ideas in economics. How else could we account for the prevailing fashion of the time: the emphasis on models assuming full employment? Surely memories are not so short as to relegate the Great Depression to ancient history. Were economists so confident about the new era that the economic downturns in 1982 and 1991, accompanied by rising unemployment, came as a total surprise? What about the persistent unemployment in Europe in the 1980s? Here was an area in which simple models with alternative explanations were available.

THE VISION

Krugman takes far too narrow a view of the development process and of what is wrong with both the standard neoclassical and the planning paradigms. I have already illustrated one limitation of his vision: If the central problems were those of externalities and increasing returns, the planning process would have been an appropriate remedy. But that assumption ignored information problems, which are now recognized to be central. Evidently, governments are not well equipped to identify projects and motivate project managers. But these were not the issues on which the planning mechanism focused, and, not surprisingly, it did not resolve them.

Financial Institutions

Indeed, the question of who gets funding and how it is used is the essential problem addressed by financial institutions in capitalist economies. They provide the institutional "solution" to the information problem. How, when, and whether they work is certainly part of the development story. Recent research in macroeconomics has emphasized the markedly different consequences of debt and equity for risk; it has identified failures in both aspects of the capital market (the presence of debt and equity rationing). There is here another link between an elastic labor supply and economic growth. Earlier literature emphasized the importance of capital accumulation; the new literature emphasizes the form in which capital is accumulated—equity versus debt (see Greenwald, Kohn, and Stiglitz 1990). Equity is viewed as being more powerful. Low wages result in high profits and the accumulation of *equity* capital, thus facilitating the growth process. Krugman's failure to mention the importance of these institutions in the growth process is perhaps the best example of what I mean when I say that a broader vision is required.

Political Economy

In interpreting the general problem of government interventions to correct market failures, Krugman refers to problems of political economy. To be sure, these problems are important. But his analysis of the issues is both incomplete and misleading. As noted earlier, political economy problems are not the only source of the failure of the planning paradigm. Moreover, rent-seeking behavior is, at the very least, an incomplete explanation for the failure of public sector enterprises. Krugman fails to note the existence—let alone the importance—of rent-seeking in modern managerial capitalism (see Shleifer and Vishny 1989; Edlin and Stiglitz 1992). And finally, ascribing to political problems the failure to develop does not explain the differences in regional development that have characterized virtually all countries at various stages of their growth. (See Greenwald, Levinson, and Stiglitz 1992 for a discussion of how localized knowledge of capital markets can explain patterns of regional development.) Nor can the allusion to political economy problems explain the many successful govern-

ment enterprises. They may represent a minority of all such enterprises, but there are enough successes to make it plausible that success is not just a matter of luck.

Externalities and Increasing Returns

There is no single explanation of why countries grow or fail to grow. Increasing returns, externalities, and learning by doing may be—and undoubtedly are—important, but modeling them in a way that provides insights into the development process requires more care than has typically been taken, and many of the models formulated to date simply miss the essential issues.

Consider, for instance, the modern rendition of the Big Push argument, at least as interpreted by Murphy, Shleifer, and Vishny (1989). I have already suggested that those arguments, based on income effects, have dubious plausibility (in their present formulations) when applied to economies that face trading opportunities.⁶

Or consider the argument originally modeled by Aoki (1970) and incorporated in Romer's (1986) growth model—that we can reconcile learning by doing with competitive behavior when learning is external to the firm (and internal to the country). If the spillover to other firms is less than 100 percent (and it is hard to believe that those outside the firm learn everything) any time there is learning by doing, competition will be imperfect (see Dasgupta and Stiglitz 1988).

Or take the argument that what is important are “aggregate increasing returns.” That suggests that large economies have a distinct advantage over small economies; it does not explain how a small economy could grow into a big economy. The essential problem—from both an analytic and a policy perspective—is to identify the nature of the externalities that are not internalized by markets and the sources of the returns to scale.

Coase (1960) went too far when he (or his disciples) asserted that all externalities could be internalized; yet many can be. Indeed, a primary theme of Chandler's (1977) classic study is that firms are an alternative to markets and succeed in internalizing certain externalities to solve failures of coordination. (See Sah and Stiglitz 1989 for a discussion of “diffuse externalities” that are relatively unamenable to internalization; see also Stiglitz 1991.)

Similarly, it makes a great deal of difference whether the locus of increasing returns is within an industry or within the broader economy. In the former case even a small economy can, by specializing, avail itself of increasing returns; surely there are industries in which the minimum efficient scale of production is relatively small.

6. Or consider the argument that because early innovators get to choose the product in which they then specialize, they can choose a product with a better learning curve. In an international context these effects are essentially undone by changes in relative prices (Skeath 1989). Indeed, if we focus, for simplicity, on the case of unitary price elasticities, price effects will precisely undo output effects, so that income rates of growth will be the same in all countries.

When the economies of scale are spread more broadly, one must ask how they arise. And offsetting these economies of scale are diseconomies of scale—congestion economies. Regional economics provides some insight into these issues. We see agglomerations, economic activity that is not dispersed. Yet we also see viable communities, with high per capita income, that are relatively isolated geographically and are relatively small, certainly under a million population. These communities are, of course, part of larger communities, but what are the effective barriers? If there were none, everyone would be equally a part of the world economy and could take advantage of whatever economies of scale were relevant at this highly aggregate level. But costs of communication and transport help delimit the scope of communities. These costs, in turn, have implications for patterns of development; at certain stages of development and for certain products, they may be larger. Unfortunately, models with aggregate increasing returns to scale give us absolutely no insight into the relevant issues.

One attraction of models with economies of scale and externalities is that using models with nonconvexities and externalities makes it easy to construct multiple equilibria, as Krugman effectively illustrates. (See also Sah and Stiglitz 1989; Stiglitz 1987, 1991; Murphy, Shleifer, and Vishny 1989). And it is tempting to try to interpret the differing situations in which industrial and developing countries find themselves as reflecting these different equilibria. But again, we hardly need nonconvexities and externalities to generate multiple equilibria. Solow (1956) showed us how we could do that with his simple model; all we need is to have savings rates or reproduction rates depend (in a particular way) on the capital-labor ratio. These models were inadequate because some of their central implications—such as convergence in the rate of growth of income per capita and equalization of factor prices⁷—seemed counterfactual.

Differences in Technology

There is, fundamentally, only one way to resolve the paradox that all factors receive lower returns: the “effective” technologies in the two countries are different. There are two reasons that this might be so. If economies of scale are significant, larger economies are better off. For reasons already cited, I find this

7. Stiglitz (1970) and Inada (1968) extend the standard theory to the context of growth. The implications for factor prices across countries remain even after human capital is introduced; they are simply a consequence of the negative slope of the factor price frontier. For instance, if interest rates are equalized, it must be the case that if unskilled wages are lower in one country, skilled wages are higher. The critical assumption, of course, is that all countries face the same technology. By the same token, in international trade models with factor price equalization, such as that cited by Helpman and Krugman (1985)—as in earlier models of local public goods with free migration of labor and goods (for example Stiglitz 1977)—one can easily obtain asymmetric multiple equilibria; yet welfare of all those of a given ability is identical in all communities and countries. Such models, accordingly, have little to contribute to our understanding of the development problem.

explanation—at least as it is usually presented—at best incomplete and at worst misleading or wrong.⁸

The second reason that technology may be less effective in a developing country is simply differential knowledge. To economists who are used to assuming that everyone has access to best-practice technology, this explanation is anathema; it is too simple, it is ad hoc, or it leaves unexplained why countries lack access to best-practice technology. Yet once we recognize that information is costly to obtain and disseminate, that firms in industrial countries may have strategic reasons for withholding their most advanced technology, and that local conditions make necessary adaptation of the technology for the particular country, the explanation of differential knowledge makes perfect sense (see Gans 1989). That it is common sense is a virtue, not a vice.

The developing countries provide a rich set of facts and phenomena to be explained. The challenge for economic theory is to devise models that accommodate as many of these as possible. Doing so will, as Krugman rightly says, take us back to what he calls high development economics, but it is a vision of high development economic theory which, although it incorporates externalities and nonconvexities, is richer and more complex than one that incorporates those features alone.

REFERENCES

- Amsden, A. E. 1989. *Asia's Next Giant: South Korea and Late Industrialization*. New York: Oxford University Press.
- Aoki, Masahiko. 1970. "A Note on Marshallian Process under Increasing Returns." *Quarterly Journal of Economics* 84(1) (February):100–12.
- . 1987. "An Incentive-Compatible Approximation of Nash-like Solution under Nonclassical Environments." In Thomas Graves, Ray Radner, and Stanley Reiter, eds., *Information, Incentives and Economic Mechanisms: Essays in Honor of Leonid Hurwicz*. Minneapolis: University of Minnesota Press; London: Blackwell.
- Arrow, K. J. 1962. "The Economic Implications of Learning by Doing." *Review of Economic Studies* 29 (June):155–73.
- Arthur, W. B. 1985. "Competing Techniques and Lock-in by Historical Events: The Dynamics of Allocation under Increasing Returns." Stanford University.

8. One variant of the model attempts to identify the source of the returns by focusing explicitly on issues of nontradability (Rodríguez 1992). It shows how a *small* economy—open to trading many, but not all, goods and services—may be caught in a low-level equilibrium in which there are no incentives for capital to flow into the country and in which both skilled and unskilled laborers receive low wages. It rests on the reasonable hypothesis that there are nonconvexities in the production of intermediate goods, some of which (such as services) are essentially nontradable, and that the range of the intermediates that are available depends on the pattern of production of final goods. Countries that produce to their current comparative advantage (based on their current supply of these intermediate goods, not the underlying factor prices) may produce final goods that do not generate demand for the large variety of intermediate goods needed to produce complex goods at competitive costs. But these intermediate goods form the basis of industrialization. What is attractive about this kind of modeling is that it goes well beyond an appeal to aggregate economies of scale or externalities.

- . 1988. "Competing Technologies: An Overview." In Giovanni Dosi, ed., *Technical Change and Economic Theory*. London: Frances Pinter.
- . 1989. "Competing Technologies, Increasing Returns and Lock-in by Historical Events." *Economic Journal* 99:116–31.
- Atkinson, A. B., and J. E. Stiglitz. 1969. "A New View of Technological Change." *Economic Journal* 79:573–78.
- Bardhan, Pranab. 1970. *Economic Growth, Development, and Foreign Trade: A Study in Pure Theory*. New York: Wiley Interscience.
- Chandler, Alfred. 1977. *The Visible Hand*. Cambridge, Mass: Belknap Press of Harvard University Press.
- Coase, Donald. 1960. "The Problem of Social Cost." *Journal of Law and Economics* 3:1–44.
- Dasgupta, Partha, and J. E. Stiglitz. 1980a. "Industrial Structure and the Nature of Innovative Activity." *Economic Journal* 90 (June):266–93.
- . 1980b. "Uncertainty, Market Structure and the Speed of R&D." *Bell Journal of Economics* 11(1) (Spring):1–28.
- . 1988. "Learning by Doing, Market Structure and Industrial and Trade Policies." *Oxford Economic Papers* 40:246–68.
- David, Paul. 1987. "Some New Standards for the Economics of Standardization in the Information Age." In Partha Dasgupta and Paul Stoneman, eds., *Economic Policy and Technological Performance*. Cambridge, U.K.: Cambridge University Press.
- Dosi, Giovanni, ed. 1988. *Technical Change and Economic Theory*. London: Frances Pinter.
- Edlin, Aaron, and J. E. Stiglitz. 1992. "Discouraging Rivals: Managerial Rent Seeking and Economic Inefficiencies." Stanford University, Department of Economics, Stanford, Calif.
- Gans, Joshua S. 1989. "Knowledge of Growth and the Growth of Knowledge." *Information Economics and Policy* 4:201–24.
- Greenwald, Bruce C., and Joseph E. Stiglitz. 1986. "Externalities in Economies with Imperfect Information and Incomplete Markets." *Quarterly Journal of Economics* 101:229–64.
- Greenwald, Bruce C., Meir Kohn, and Joseph E. Stiglitz. 1990. "Financial Market Imperfections and Productivity Growth." *Journal of Economic Behavior and Organization* 13 (June):321–45.
- Greenwald, Bruce C., A. Levinson, and J. E. Stiglitz. 1992. "Capital Market Imperfections and Regional Economic Development." Paper prepared for CEPR conference on Finance and Development in Europe, Santiago, Spain, December 1991.
- Helpman, Elhanan, and Paul R. Krugman. 1985. *Innovation and Growth in the Global Economy*. Cambridge, Mass.: MIT Press.
- Inada, K-I. 1968. "Free Trade, Capital Accumulation and Factor Price Equalization." *Economic Record* 44 (September):28–37.
- . 1969. "Endogenous Technical Progress and Steady Growth." *Review of Economic Studies* 36(1):99–107.
- Itoh, Motoshige, and others. *Economic Analysis of Industrial Policy*. San Diego, Calif.: Harcourt Brace Jovanovich.
- Kaldor, Nicholas. 1970. "The Case for Regional Policies." *Scottish Journal of Political Economy* 17:337–48.
- . 1972. "The Irrelevance of Equilibrium Economics." *Economic Journal* (December):1237–55.
- Kaldor, Nicholas, and J. A. Mirrlees. 1969. "A New Model of Economic Growth." *Review of Economic Studies* 82 (June):174–92.

- Katz, Jorge. 1987. "On the Microeconomics of Technical Progress." *Technology Generation in Latin American Manufacturing Industries*. Basingstoke, U.K.: Macmillan.
- Komiya, Ryutaro, Masahiro Okuno, and Kotaro Suzumura. 1988. *Industrial Policy of Japan*. San Diego, Calif.: Academic.
- Lewis, W. A. 1949. *Overhead Costs*. New York: Holt Rinehart.
- . 1954. "Economic Development With Unlimited Supplies of Labor." *Manchester School of Economics and Social Studies* 22(2):139–91.
- . 1955. *The Theory of Economic Growth*. London: Allen and Unwin.
- Murphy, Kevin M., Andrei Shleifer, and Robert W. Vishny. 1989. "Industrialization and the Big Push." *Journal of Political Economy* 97(5):1003–26.
- Nelson, Richard R., and Sidney G. Winter. 1982. *An Evolutionary Theory of Economic Change*. Cambridge, Mass.: Belknap Press of Harvard University Press.
- Pack, Howard, and L. E. Westphal. 1986. "Industrial Strategy and Technological Change: Theory versus Reality." *Journal of Development Economics* 22:87–128.
- Rodríguez, Andrés. 1992. "The Big Push in a Small Open Economy." Stanford University. Department of Economics, Stanford, Calif.
- Romer, Paul. 1986. "Increasing Returns and Long-Run Growth." *Journal of Political Economy* 94(5):1002–37.
- Rosenstein-Rodan, Paul N. 1943. "Problems of Industrialization in Eastern and South-eastern Europe." *Economic Journal* 53 (June–September):202–11.
- Sah, Ray, and J. E. Stiglitz. 1989. "Sources of Technological Divergence between Developed and Less Developed Countries." In Guillermo A. Calvo and others, eds., *Debt, Stabilization and Development: Essays in Memory of Carlos Diaz-Alejandro*. Oxford, U.K.: Blackwell.
- Shell, Karl, ed. 1967. *Essays on the Theory of Optimal Economic Growth*. Cambridge, Mass.: MIT Press.
- Shleifer, Andrei, and Robert W. Vishny. 1989. "Management Entrenchment: The Case of Manager-Specific Investments." *Journal of Financial Economics* 25:123–39.
- Skeath, Susan. 1989. "Learning, Price Effects, and Income Growth." Princeton University, Princeton, N.J.
- Solow, Robert M. 1956. "A Contribution to the Theory of Economic Growth." *Quarterly Journal of Economics* 70(1):101–8.
- Stiglitz, J. E. 1970. "Factor Price Equalization in a Dynamic Economy." *Journal of Political Economy* 78 (May–June):456–89.
- . 1977. "Theory of Local Public Goods." In M. S. Feldstein and R. P. Inman, eds., *The Economics of Public Services*. New York: Macmillan.
- . 1985. "Information and Economic Analysis: A Perspective." *Economic Journal* 95, supplement (March):21–41.
- . 1987. "Learning to Learn: Localized Learning and Technological Progress." In Partha Dasgupta and Paul Stoneman, eds., *Economic Policy and Technological Performance*. Cambridge, U.K.: Cambridge University Press.
- . 1988. "Technological Change, Sunk Costs, and Competition." *Brookings Papers on Economic Activity* 3:883–947.
- . 1991. "Social Absorption Capability and Innovation." Paper prepared for Korean Development Institute, 20th Anniversary Symposium, Seoul, June.
- . 1992. "Whither Socialism?: Perspectives from the Economics of Information." Wicksell Lectures presented in Stockholm, May.
- Teubal, Morris. 1967. "Inflation, Unemployment, and Economic Growth." Ph.D. thesis. University of Chicago, Chicago, Ill.

- Uzawa, Hirofumi. 1963. "On a Two-Sector Model of Economic Growth II." In J. E. Stiglitz and Hirofumi Uzawa, eds., *Readings in the Modern Theory of Economic Growth*. Cambridge, Mass.: MIT Press.
- . 1965. "Optimal Technical Changes in an Aggregative Model of Economic Growth." *International Economic Review* 6 (January):12–31.
- Wade, Robert. 1990. *Governing the Market: Economic Theory and the Role of Government in East Asian Industrialization*. Princeton, N.J.: Princeton University Press.
- Wright, Gavin. 1987. "The Economic Revolution in the American South." *Journal of Economic Perspectives* 1 (Summer):161–78.

COMMENT ON "TOWARD A COUNTER-COUNTERREVOLUTION
IN DEVELOPMENT THEORY," BY KRUGMAN

Lal Jayawardena

Professor Krugman seeks to explain the disappearance of "high development theory," which spanned the period between 1943 and 1958 and was associated with such names as Rosenstein-Rodan, Fleming, Nurkse, Myrdal, Scitovsky, and Hirschman. He argues that its eclipse, despite the continuing relevance of its insights to the ever-present problem of accelerating development, occurred because its pioneers were unable "to turn their intuitive insights into clear-cut models that could serve as the core of an enduring discipline."

Krugman draws out the policy implications that follow from his legitimization of the underlying economic assumptions of high development theory in a manner that would carry conviction to mainstream economic theorists. There seem to be two implications: first, a caution against carrying a free-market orthodoxy too far because there is an intellectually solid case for some government promotion of industry and, second, a need to shift the research on trade and industrial policy away from its focus on government failure or the horrors of import substitution and the distortions imposed by government policy and toward a concern with market failure.

The difficulty confronting anyone asked to comment on Krugman's paper is that there is not a great deal in it with which one can disagree. My principal reservation is whether the failure of high development theory to model its results was the sole reason for its disappearance. I therefore faxed one of the few surviving members of that generation, Albert Hirschman, to check out my intuition. His faxed response is worth quoting:

My reaction to the Krugman paper is, of course, that I am delighted: delighted to be rehabilitated and to be present, unlike most other contributors to high development theory, at our collective rehabilitation at the hands of Krugman. It certainly is true that the failure of the proponents of "high development theory" to speak the modeling language contributed to the loss of influence of their ideas in the 1970s and 1980s. But here I agree

Lal Jayawardena is assistant secretary-general, United Nations, and director, World Institute for Development Economics Research, Helsinki.

© The International Bank for Reconstruction and Development / THE WORLD BANK

with you that this is not the whole story. His is essentially what is called an "internalist" account in the history and sociology of science. I tried to supply the "externalist" side of the story in my article on "The Rise and Decline of Development Economics."

Hirschman (1981), in providing his "externalist" explanation, argues that development economics as formulated in the period of high development theory was characterized by two basic ingredients: a rejection of the *monoeconomics* claim, which argued that the same economic laws were applicable to both developing and industrial countries, and an assertion of the *mutual benefit* claim, which held that economic relations between these two groups of countries could be handled in ways that would yield gains to both. The available competing approaches to economics were situated differently with respect to each of these ingredients. Orthodox economics asserted both the monoeconomic and mutual benefit claims, while the neo-Marxist economic theories, which gained ground later, rejected both claims. Hirschman traces the decline of development economics to the consequences of the "strange alliance of neo-Marxism and monoeconomics." In brief, the neoclassical right faulted it for having forsaken the true principles of efficient resource allocation prescribed by monoeconomics in that it, for example, promoted inefficient import-substituting industrialization in developing countries. And for the neo-Marxist left, exemplified by economists such as Paul Baran (1952), development economics was not sufficiently radical.

Even more fatal, in Hirschman's view, were the political disasters that struck many developing countries after the 1960s. It was the resulting self-doubt and failure to mount a counterattack against the "unholy alliance of neo-Marxists and neoclassicists" that doomed development economics. At the same time, growing disenchantment with development assistance undermined interest in the "mutual interests" idea—in spite of attempts by such international groups as the Brandt Commission (Independent Commission on International Development Issues 1980) to keep the theme alive. (Paradoxically, the current concern about the global environment might help reinstate the mutual benefit claim for adequate resource transfers to developing countries and lead to a revival of interest in development economics.)

Krugman's thesis has also been underlined in Williamson's (1990) paper, which states the "Washington consensus" on policy reform. Having outlined the five elements that constitute the consensus—a balanced budget, relative price correction (principally a competitive exchange rate), liberalization of trade and foreign investments, privatization, and domestic market deregulation—Williamson adds:

A striking fact about the list of policies on which Washington does have a collective view is that they all stem from classical mainstream economic theory, at least if one is allowed to count Keynes as a classic by now. None of the ideas spawned by the development literature, such as the "big push,"

balanced or unbalanced growth, surplus labour, or even the two-gap model, plays any essential role in motivating the Washington consensus. This raises the question as to whether Washington is correct in its implicit dismissal of the development literature as a diversion from the harsh realities of the dismal science.

What is significant is that the only reaction to this view during the conference came from Stanley Fischer, chief economist of the World Bank at the time, who argued against an "active industrial policy" in developing countries despite its success "on balance . . . in parts of East Asia" (Williamson 1990).

INDUSTRIAL POLICY IN EAST ASIA

Any lack of rigor in articulating high development theory did not prevent its determined and fruitful application in East Asia, particularly in Japan and the Republic of Korea, whose planning models embodied its insights. Unlike the cases of South Asia and Latin America, there is little evidence to support the view that high development theory was invoked in East Asia in support of *inefficient* import-substituting industrialization. *Efficient* import substitution, in contrast, endeavored to promote selected infant industries with the potential to compete as exporters in international markets. The success of East Asian interventionist strategies in "picking winners" has sparked an intense debate within the World Bank as to whether the standard trade liberalization component of the Washington consensus is appropriate for developing countries or should be modified to provide both "functional" (nondiscriminatory) and "selective" (discriminatory) government intervention.

The key insight of high development theory was that the coordination implicit in the communication of price signals was not sufficient where significant economies of scale existed. Thus, investment decisions required explicit coordination by a planning authority to achieve efficient allocation of resources. The implication of this insight was that while the price mechanism could be relied on to take care of the *production* problems of an economy, the allocation of *investment* could not be left to the price mechanism but would require state intervention.

This insight was incorporated at a very early stage in East Asian (specifically, Korean) planning and was implemented through planning models (Westphal 1979) for those sectors in which substantial economies of scale existed. These essentially took the form of numerically specified general equilibrium models. With allowance for economies of scale and following a modified input-output approach, these programming models were used to determine the optimal pattern of investment when the composition of final demand depends only on per capita income and the main choices are between domestic production and imports. Note that from a policy perspective a methodology *was* available to arrive at meaningful conclusions on the problem of vertical (intertemporal) and horizontal (intersectoral) interdependence within the framework of an econ-

omywide model that explicitly allowed for economies of scale in specified sectors.

The rapid development of the Republic of Korea, Taiwan (China), Singapore, and Hong Kong has led development economists to draw quite opposite policy conclusions as to the underlying reasons. Those concerned with market failure point to successful industrial policy and trade protection. Those concerned with government failure point to the adoption of sensible macroeconomic policies—low inflation, competitive exchange rates, a commitment to small government, and an insistence on guiding (rather than supplanting) a market mechanism with an educated labor force and entrepreneurial skills. The traditional World Bank view cited earlier falls within the second group, although there are indications in the current internal debate that the first view is gaining prominence (The issues in this debate are summarized in World Bank 1991.)

The general World Bank approach to industrial policy qualifies this stance only to the extent of being moderately neoclassical; in other words, because factor and product markets are not fully efficient in developing countries, there is a role for a modest degree of government intervention. The strong preference is for functional, as opposed to selective, intervention. In sum, the general approach concentrates on getting prices right and on limiting the range of interventions in specific industrial sectors and firms.

In its microlevel approach, in contrast, the Bank takes a different view. In India, for example, it recommends “selective” interventions to support each industry, including specific measures for improving design, technology, equipment, management, and marketing. The export strategy study recommends “picking specialized and high-performance exporters” for “selective” support. The capital goods study lists institutional and technological measures to boost competitiveness. These are not neutral interventions; they can all be considered forms of “picking winners.”

The upshot of the debate was a compromise that called for selective interventions together with traditional across-the-board incentives for industrialization. Ultimately, there does not appear to be any uniform prescription for industrialization (see Taylor 1991).

Japan’s view of industrial policy is consistent with its belief in the protection of immature industries. An important recent paper by Japan’s aid agency, the Overseas Economic Cooperation Fund (OECF), challenges the conditionality of rapid trade liberalization in structural adjustment loans.

If imports are liberalized too quickly, is it possible to develop industries that will play leading roles in the next stage of economic development? Is it not necessary to protect domestic industry to some extent for a certain period of time in order to allow a viable export industry to develop? (OECF 1991)

The paper argues that conventional trade liberalization is limited to capturing the static comparative advantage typical of a developing country, which is to be

found mainly in primary products and light industry with low value added. Since the objective of developing countries is to move toward high value added production, with more sophisticated technologies and a more substantial growth potential, the OECF concludes that "sticking to simple trade liberalization based on static comparative advantage may have a negative impact on the possibility of economic development." The study argues that it is too optimistic to expect that industries with high value added will automatically emerge from the private sector in the absence of deliberate measures to foster such industries, including protection. The OECF's preferred policy prescription is to protect chosen industries for a specified period, taking precautions to avoid the harmful effects of protection.

The concern about too rapid a liberalization is also an important issue in Eastern Europe's economic transformation. Katz (1991) suggests that instead of subjecting industry to the full force of international competition, Eastern Europe might learn from East Asia's approach.

The discussion above shows that, quite independently of the formal legitimization of the underlying economic assumptions of high development theory, the successful application of its policy prescriptions in East Asia provides an empirical basis for its validity. These empirical considerations (now reinforced by Krugman) constitute a powerful case for revising the trade liberalization components of the Washington consensus, or at least excising it from adjustment loan conditionality.

The necessary revision would seem to require a somewhat less tentative approach to interventionist trade and industrial policy than is suggested in Krugman's paper. In other words, instead of merely cautioning against "carrying a free-market orthodoxy too far," the World Bank and other multilateral agencies might incorporate in structural adjustment lending specific ways of developing sound long-term industrial policies (including both functional and selective interventions) and ensuring that trade liberalization does not impede these interventions.¹

What remains to be considered is Krugman's suggested reorientation of the research agenda to focus on market failure. The World Institute for Development Economics Research (WIDER) has already made a beginning in this direction, based in part on Krugman's work on new trade theory (Helleiner 1992). A challenge to conventional wisdom is contained in a WIDER volume edited by Banuri (1991). A third area of WIDER research has begun to examine whether

1. The OECF (1991) suggests a continuing policy dialogue on industrial development between donors and developing countries. The institutional location for such a dialogue could be World Bank aid groups or United Nations Development Programme (UNDP) roundtables, which are all under a certain amount of pressure to take a longer-term view of development. The case has been reinforced both by the social costs of short-term adjustment during the 1980s and by the need to incorporate long-term environmental considerations into a country's development strategy. (See Jayawardena 1991 for a discussion of this approach and of the financing requirements for achieving a minimum rate of growth in developing countries in the 1990s.)

markets can be relied on to bring about adjustment and sustainable growth within a politically sensible timeframe without incurring extensive social costs—and, if not, what supporting mechanisms are needed to offset market failure (Taylor 1988). Other areas of research have attempted to bring together neo-classical and nonneoclassical viewpoints on the specific issue of the economic transformation of centrally planned economies (see Blanchard and others 1991; Kornai 1990; and Hansson 1992).

I have tried to do no more than outline WIDER's current work on the kind of reoriented research agenda that Krugman has suggested. I would hope that, as a follow-up, interested research institutions could pursue some form of pooling and coordination of effort in the most promising research areas.

REFERENCES

- Banuri, Tariq, ed. 1991. *Economic Liberalization: No Panacea. The Experience of Latin America and Asia*. WIDER Studies in Development Economics. Oxford, U.K.: Clarendon Press.
- Baran, Paul A. 1952. "On the Political Economy of Backwardness." In Robert I. Rhodes, ed., *Imperialism and Underdevelopment: A Reader*. New York: Monthly Review Press.
- Blanchard, Oliver, and others. 1991. *Reform in Eastern Europe. The 1990 Report of the WIDER World Economy Group*. Cambridge, Mass.: MIT Press.
- Hansson, Ardo. 1992. "Report on the conference 'Transformation from a System of Central Planning to a Market Economy,' jointly sponsored by the Government of Finland and WIDER, Helsinki, February 6–7."
- Helleiner, Gerald K. 1992. *Trade Policy, Industrialization, and Development*. WIDER Studies in Development Economics. Oxford, U.K.: Clarendon Press.
- Hirschman, Albert O. 1981. "The Rise and Decline of Development Economics." In *Essays in Trespassing: Economics to Politics and Beyond*. New York: Cambridge University Press.
- Independent Commission on International Development Issues ("Brandt Commission"). 1980. "Mutual Interests." In *North-South: A Program for Survival*. London: Pan.
- Jayawardena, Lal. 1991. "A Global Environmental Compact for Sustainable Development: Resource Requirements and Mechanisms." *WIDER Research for Action*. Helsinki.
- Katz, S. Stanley. 1991. "East Europe Should Learn from Asia." *Financial Times*. April 24.
- Kornai, János. 1990. *The Road to a Free Economy: Shifting from a Socialist System: The Example of Hungary*. New York: W. W. Norton.
- OECD (Overseas Economic Cooperation Fund). 1991. "Issues Related to the World Bank's Approach to Structural Adjustment—Proposal from a Major Partner." Occasional Paper 1. Tokyo.
- Taylor, Lance. 1988. *Varieties of Stabilization Experiences: Towards Sensible Macroeconomics in the Third World*. WIDER Studies in Development Economics. Oxford, U.K.: Clarendon Press.
- . 1991. *Income Distribution, Inflation, and Growth: Lectures on Structuralist Macroeconomic Theory*. Cambridge, Mass.: MIT Press.

- Westphal, Larry E. 1979. "Economies of Scale and Investment over Time." In Hollis Chenery, ed., *Structural Change and Development Policy*. New York: Oxford University Press.
- Williamson, John, ed. 1990. *Latin American Adjustment: How Much Has Happened?* Washington, D.C.: Institute for International Economics.
- World Bank. 1991. "Industrialization in Newly Industrializing Countries: Case Studies of Korea, India and Indonesia." Vol. 1. World Bank, Operations Evaluation Department, Washington, D.C.

FLOOR DISCUSSION OF THE KRUGMAN PAPER

Responding to Joseph Stiglitz (discussant), Krugman adapted a saying: "Those who can, do, and those who can't, worry about definitions." He and Stiglitz, being amateur economic historians, seemed to be locked into defining what constitutes "mainstream" and deciding whether things had left it or not. People used to say that everything Milton Friedman saw made him think about money; so everything Krugman saw made him think of increasing returns, and many things Stiglitz saw made him think of asymmetric information.

In response to Lal Jayawardena (discussant), Krugman said it was an open question how much the success of East Asian economies could be used to support high development theory. Many people believe that they represent a success for interventionist policies; many others believe that interventionist policies are second order and that the East Asian economies represent a success for outward-oriented policies. Either way, their success is a reproach to classical models, which can explain neither avenue to success.

One participant welcomed Krugman's rescue of important elements from development theory that might still play even a partial role in strategies for economic development. He felt that the counter-counterrevolution in development theory should include not only growth theory and the new theoretical developments emphasized in Krugman's paper but also the new themes that microeconomic research, including case studies, have shown to be important in the past twenty years. No bag of tricks yet existed to formalize these new themes.

A participant said that he fully endorsed what Stiglitz said about Krugman's selective and dated reading of the development literature. The Rosenstein-Rodan Big Push paradigm had not failed or been abandoned because of lack of formal modeling, the speaker contended, but because it was totally irrelevant once you allowed that the world was an open, not a closed, economy. Rescuing it now was not useful unless you addressed the two reasons it failed. World trade was thriving in the 1950s and 1960s, so it was pointless to talk about whether domestic demand restricted total demand or economies of scale; the minimum efficient scale for many products developing countries could produce was so small in the context of world demand that models which emphasized indefinite increases in returns were irrelevant as a basis for policymaking. Similarly, it was

The session was chaired by Vinod Thomas, chief economist, East Asia and Pacific Regional Office, the World Bank.

difficult to find indefinite externalities of the pecuniary or technological variety. Even now, the most important avenue to development is world trade, not the inward-oriented focus that Krugman's rescued development theory would suggest.

Another participant asked what in this growth theory was relevant today, when developing countries—including China and India—were more open than at any time in recent memory.

Krugman responded that many people believe that because of the potential of world trade, anything having to do with market size within a country is now irrelevant. We must keep some perspective, he said, about what is and is not trade. The Federal Reserve Bank of Boston did a systematic study of the tradable and nontradable sectors in the Massachusetts economy, characterizing sectors as serving either the local market or the national or global market. That study showed that less than 30 percent of employment was in what one would consider tradables. Even perfectly open economies such as the state of Massachusetts are engaged primarily in producing for the domestic market, he said, and these issues are relevant where the domestic market matters. But increasing returns do apply domestically—often to intermediate inputs (both intermediate goods and, more important, pools of skilled labor).

Krugman admitted that there was some fallacy of misplaced concreteness in using these models to get at their importance. He said that he would be happy to widen the range of possibilities, but he did not think one could simply dismiss their relevance. He did not believe that in the late 1960s people decided that they understood what Rosenstein-Rodan said but it was no longer relevant because they had open economies.

Larry Summers shared the speakers' general sense that things were shifting toward a more activist era, with good reason. He asked if anything in the Big Push, returns-to-scale concept did—or should—have anything to do with that shift toward activism. He said that the principal advocate of the idea that small producers in small markets are inefficient and that producers can be more efficient in large markets was Stalin, who sought to enlarge enterprises at the expense of differentiation. The idea of making markets bigger so there would be increasing returns, he suggested, was not as important a theme to activists as three others: the importance for industrial policy of "learning-by-doing" external economies; the importance of using command-and-control approaches to manage change until you have incentives in place, because you can't create or change markets if the right institutions are not in place; and the need to work around barriers that stop things from going from where they are less productive to where they are more productive (the old dualism theme).

Krugman said he was getting used to the bit about Stalin—when one deviates from the "Washington consensus," one is immediately accused of being a Stalinist, which he was not. As a matter of intellectual strategy, Krugman contended that it was important to lean against the wind regarding the tendency to emphasize somewhat "cosmic" concepts, such as the generation of knowledge and

technology. It was not that those futuristic topics were not important but that they were so much more tempting to explore than the grungy details of economies of scale in a plant or the greater likelihood of finding specialized skills in thick labor markets, which was part of the same story. We have an obligation, he said, to lean toward the less glamorous aspects of the problem because there is a strong tug the other way. In his opinion, technology as usually discussed was overrated; the crucial externalities often lay in more mundane types of interactions. As for dualism, he did not mean to preclude other options, and perhaps his bias was too narrow and concrete.

A participant from the World Bank's East Asia region said that Krugman had ignored one element in the history of economic thought: the sociology of the economics profession. In the 1970s and 1980s the agenda for development economics had been heavily influenced by the dominant ideologies of institutions such as the World Bank, not only through their funding of research but also, and more insidiously, because their resources and their grant of legitimacy affected the economics profession, especially in developing countries. A prime example is the World Bank's capacity-building initiative in Africa.

A World Bank participant made a distinction between a mathematical model and verbal or logical models, including sociological models, which tend to be forgotten because they are difficult to carry over historically. A second participant said that Krugman—and to some extent Stiglitz—stressed the importance of formal modeling as a legitimization of economic knowledge. He asked if they would agree that, given Krugman's definition, legitimate knowledge would include Euclidian geometry but would not include Darwin's theory (a nonformal, nonmathematical model that couldn't make predictions) or a great deal of nineteenth century physics and chemistry.

A participant asked Jayawardena to comment on Krugman's statement that it made sense for the World Bank and other multilateral agencies to push hard for liberal policies in developing countries, given their demonstrated tendency to engage in economically irrational interventions. Was it premature to start thinking about a counter-counter-counterrevolution? the participant asked.

Krugman closed the session by referring to the quotation about consistency being the hobgoblin of little minds. There is internal consistency, and there is consistency across different issues, he said, and nobody could accuse him or Stiglitz of having a consistent model of the world. He referred, finally, to another quote, this time from an economist, that the most reckless and dangerous theorist is the man who professes to let the facts speak for themselves. It is important to discipline oneself, he said. What he meant by a model was not necessarily a mathematical model but putting oneself, at least briefly, in an intellectual straitjacket just to be sure of actually staying in the same place for a little while.