

# **Development, Geography, and Economic Theory**

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## Preface

This book consists of heavily revised versions of the Ohlin lectures that I gave at the Stockholm School of Economics in the fall of 1992.

Invitations to give lectures of this kind are, of course, a great honor. They are also a special privilege for those of us who occasionally find that we have things to say that fit awkwardly into the usual media of professional communication ideas that are too fuzzy for a journal article, too slight for a book, yet presume too much knowledge on the part of the audience to be published in more popular media. When you are prone to having fuzzy, slight ideas as I am a short lecture series published as a small book presents a wonderful opportunity to indulge your vice.

These particular lectures are what we might call a meditation inspired by some of the things that I have learned in the course of my main current research project, which is a reexamination of the long-neglected field of economic geography. I began that the way economists of my generation and temperament generally do: with a cute if grossly unrealistic model that seemed to me to yield some useful insights. Over the past several years I have been gradually elaborating on that original model, trying to make it

increasingly realistic, trying to bring it into confrontation with data, trying to grasp at the deeper principles that one hopes underlie the special cases I have looked at so far. This is, of course, the way that academic economists work in the late twentieth century, and I am very much a part of my intellectual culture.

In the course of this work, however, I became increasingly and uncomfortably aware that the field in which I was working had a rather strange history. Economic geography the location of activity in space is a subject of obvious practical importance and presumably of considerable intellectual interest. Yet it is almost completely absent from the standard corpus of economic theory. My main objective over the past few years has been to remedy that omission the only way I know how: by producing clever, persuasive models that in turn help inspire students and colleagues to work on the subject. But I could not help becoming interested in understanding why my profession had ignored the questions I was now having so much fun answering.

I also became aware of a somewhat different but related history in another field, economic development, where a set of ideas similar to those that I was now applying to geography had flourished briefly in the 1940s and 1950s, then were all but forgotten.

Confronted by these strange turnings in the evolution of economic thought, I have found myself playing the role of an amateur intellectual historian, reading old and neglected papers, trying to make sense of the reasons why some ideas fail despite their seeming plausibility. And at the same time I found myself trying to justify the way in which I and my friends do research even though the

limiting nature of our intellectual style was made all too obvious by my dabbings in intellectual history.

Here, then, are some meditations on the nature of economic theory. I hope that some readers will find them enlightening, and that the rest will at least find them entertaining.

# 1

## The Fall and Rise of Development Economics

A friend of mine who combines a professional interest in Africa with a hobby of collecting antique maps has written a fascinating paper on what he calls "the evolution of ignorance" about Africa. The paper describes how European maps of the African continent evolved from the fifteenth to the nineteenth centuries. <sup>1</sup>

You might have supposed that the process would have been more or less linear: as European knowledge of the continent advanced, the maps would have shown both increasing accuracy and increasing levels of detail. But that's not what happened. In the fifteenth century, maps of Africa were, of course, quite inaccurate about distances, coastlines, and so on. They did, however, contain quite a lot of information about the interior, based essentially on second- or third-hand travelers' reports. Thus the maps showed Timbuktu, the River Niger, and so forth. Admittedly, they also contained quite a lot of untrue information, like regions inhabited by men with their mouths in their stomachs. Still, in the early fifteenth century Africa on maps was a filled space.

Over time, the art of mapmaking and the quality of information used to make maps got steadily better. The



coastline of Africa was first explored, then plotted with growing accuracy, and by the eighteenth century that coastline was shown in a manner essentially indistinguishable from that of modern maps. Cities and peoples along the coast were also shown with great fidelity.

On the other hand, the interior emptied out. The weird mythical creatures were gone, but so were the real cities and rivers. In a way, Europeans had become more ignorant about Africa than they had been before.

It should be obvious what happened: the improvement in the art of mapmaking raised the standard for what was considered valid data. Second-hand reports of the form "six days south of the end of the desert you encounter a vast river flowing from east to west" were no longer something you would use to draw your map. Only features of the landscape that had been visited by reliable informants equipped with sextants and compasses now qualified. And so the crowded if confused continental interior of the old maps became "darkest Africa," an empty space.

Of course, by the end of the nineteenth century darkest Africa had been explored, and mapped accurately. In the end, the rigor of modern cartography led to much better maps. But there was an extended period in which improved technique actually led to some loss in knowledge.

Now don't get worried although I have put the word "geography" into the title of these lectures, they won't be about mapmaking, or at least not about the kind of map that can be placed on a wall. What I will be talking about is the evolution of ideas in economics specifically, with the story of the two related disciplines of development economics and economic geography.

Of course doing economics, or for that matter just about any kind of intellectual inquiry, is a kind of mapmaking.

The economic theorist is in possession of information about the economy some of it hard data, the equivalent of the work of men with sextants, some of it anecdotal, the equivalent of travelers' tales. From this mixture of reliable and unreliable evidence, plus a priori beliefs that are used not only to fill in where evidence is lacking but also in some cases to overrule the apparent evidence, the theorist attempts to put together a picture of how the economy works.

But how complete is that picture? In these lectures I will present an interpretation of the evolution of ideas in the two fields of development and economic geography. I will argue that in each of these fields, between the 1940s and the 1970s, there was a cycle somewhat similar to the story of how improved mapmaking temporarily diminished European knowledge about Africa. A rise in the standards of rigor and logic led to a much improved level of understanding of some things, but for a time it also led to an unwillingness to confront those areas that the new technical rigor could not yet reach. Areas of inquiry that had been filled in, however imperfectly, became blanks. Only gradually, over an extended period, did these dark regions get reexplored.

Why do I select these two fields? First, because of a common intellectual basis. Both development economics and economic geography experienced a flowering after World War II, resting on the same basic insight: the division of labor is limited by the extent of the market, but the extent of the market is in turn affected by the division of labor. The circularity of this relationship means that countries may experience self-reinforcing industrialization (or failure to industrialize), and that regions may experience self-reinforcing agglomeration.

What links development and geography is, however, not merely the common set of ideas that helped motivate them at one point in their history, but the specific problem that, I will argue, led to the failure of that set of ideas to become part of mainstream economic thinking.

Why do economists reject ideas? To laymen the unwillingness of academic economists to take seriously ideas that seem to them perfectly reasonable, whether they are John Kenneth Galbraith's theory of the new industrial state or George Gilder's views about wealth and poverty, is often infuriating. They can't understand the criteria; why isn't one forcefully written argument, backed by anecdotal evidence and an appeal to history, as good as another? And it is not at all uncommon for frustrated people with strong views about economics to attribute the unwillingness of the academic mainstream to listen to them or their friends to base motives to a guild mentality that refuses to consider ideas that are not from the right people or expressed in the right jargon or to political bias.

But the truth is less simple. Economists, like everyone, have their political biases, but these are by no means as strong an influence on what they are willing to consider as you might think. For example, one might have thought that strongly liberal economists like, say, James Tobin would be at least mildly sympathetic to the views of radical economists who draw their inspiration from Marx, or of heterodox economic thinkers like Galbraith. After all, in such fields as history and sociology the Marxist or post-Marxist left has long received a respectful hearing. And yet you don't find this happening: liberal economists are almost as quick as their conservative colleagues to condemn heterodox leftist ideas as foolish it was the liberal Robert

Solow, not Milton Friedman, who defended orthodoxy in the bitter "capital controversy" with British radicals.

Similarly, one might have expected to find conservative economists willing to say nice things about their political allies in the supply-side camp, and perhaps to appoint a few supply-side true believers to their departments. But in fact they do not, even at fiercely conservative departments like those at Minnesota or Carnegie-Mellon.

So is it just guild mentality? Do you have to have a Ph.D. to be listened to? Well, having a Ph.D. even having an established professional reputation is no guarantee that your economic ideas will be treated with respect. Consider John Kenneth Galbraith or Lester Thurow, both leading economists in the view of the general public, both with all the formal qualifications, both totally ignored by the academic mainstream. Or consider Robert Mundell, who is still revered for his contributions to international monetary theory, yet whose later incarnation as the father of supply-side economics has similarly been ignored. And on the other hand, a nonacademic may under some conditions receive a respectful hearing in the last few years Jane Jacobs, the maverick urban observer, has become something of a patron saint of the new growth theory.

So what is it that makes some ideas acceptable, while others are not? The answer which is obvious to anyone immersed in economic research yet mysterious to outsiders is that to be taken seriously an idea has to be *something you can model*. A properly modeled idea is, in modern economics, the moral equivalent of a properly surveyed region for eighteenth-century mapmakers.

For the moment, let me leave on one side the question of what constitutes a "proper" economic model and

how our notion of what is proper has changed over time. (I'll say more on the subject later in this lecture and elaborate further in the third lecture). But what seems clear to me is that the reason that the development theory that emerged in the 1940s and the economic geography that emerged more or less in parallel failed to "make it" into mainstream economics was the inability of their creators to express their ideas in a way suitable for the modeling techniques available at the time. In both development and geography the crucial problem, in particular, was the inability of the field's pioneers to be explicit about *market structure* that is, about the conditions of competition in the hypothetical economies they were describing. It's a subtle problem; indeed, it is virtually impossible to explain why it is an issue at all to anyone who has not tried to engage in serious economic modeling. And yet the market structure issue proved fatal to efforts to integrate both development and geography into the mainstream of economic theory.

All this may sound fairly abstract. So let me turn to my first example: the story of the rise, fall, and resurrection of development economics.

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 with 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 address are essentially generic to all countries, but there are also issues that are characteristic of poorer countries in particular, 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 the economics of, say, Third World agriculture have little if any overlap with those who work on LDC trade in manufactures, and these 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 swept away development economics.

And yet there is now a growing sense that this counterrevolution went too far. In the last few years it has become apparent that during the 1940s and 1950s, a core of ideas emerged regarding external economies, strategic complementarity, and economic development that remains intellectually valid and may continue to have practical applications. This set of ideas which I will refer to as "high development theory" <sup>2</sup> anticipated in a number of ways the cutting edge of modern trade and growth theory.

But these ideas have had to be rediscovered. Between 1960 and 1980 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 even to each other. Only recently have changes in economics made it possible to reconsider what the development theorists said, and to regain the valuable ideas that have been lost.

## The Big Push

A good place to start our discussion is with the paper that really began the golden age of development economics: Paul Rosenstein-Rodan's "Problems of Industrialization of Eastern and South-Eastern Europe." It is a quite straightforward paper, yet it has inspired astonishingly 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 a 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, Albert Hirschman cast his celebrated *The Strategy of Economic Development* as a 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 (e.g., Leibenstein 1957, Nelson 1956); such mechanisms can also justify a Big Push, but they are very far from the spirit of the original story.

In the late 1980s, however, Murphy, Shleifer, and Vishny (1989) offered a formalization of the Big Push that is quite close to the original spirit, and that is also quite revealing about the essential aspects of high development theory. Let me offer a slightly streamlined presentation of their model, and then ask what it tells us.

Imagine, then, an economy that is closed to international trade. (This sounds archaic and way off the point in our current age of export-led economic miracles, and perhaps

it is although I'll argue later that we may be able to modify the story to make it relevant to modern economies. But in any case, for the moment let's play by the original rules.) Our hypothetical economy can be described by assumptions about factor supply, technology, demand, and market structure.

*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. 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

$$L_i = F + cQ_i$$

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

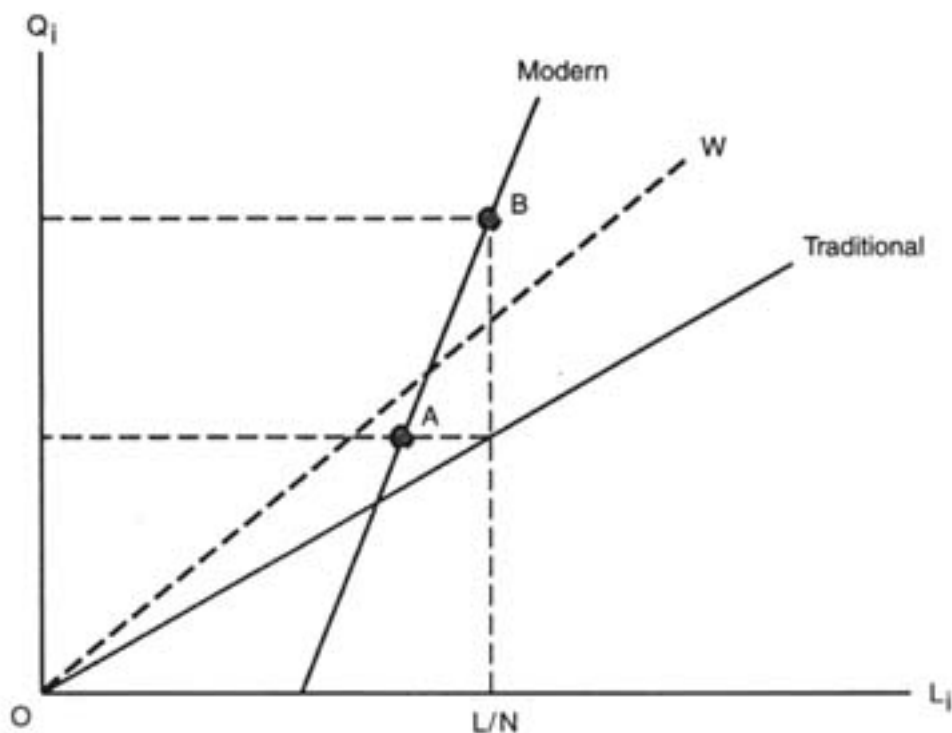


*Demand* Demand for the  $N$  goods is Cobb-Douglas and symmetric. That is, each good receives a constant share  $1/N$  of expenditure. The model will be static, with no asset accumulation or decumulation; so expenditure equals income.

*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, we draw a simple diagram (figure 1.1). 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 45-degree line for the traditional sector, a line with a slope of  $1/c$  for the modern sector.



**Figure 1.1**

From this figure it is immediately possible to read off what the economy would produce if all labor were allocated either to 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

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

that is, as long as the marginal cost advantage of modern production is sufficiently large and/or fixed costs are not too large. 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. It must be profitable for each individual entrepreneur in the modern sector to produce, taking into account the necessity of paying the premium wage  $w$  as well as the decisions of all the 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 on other goods and hence sell the same amount; since 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 I've drawn it here, this test is of course failed: it is not profitable for an individual firm to start modern production.

On the other hand, 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.<sup>3</sup> 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 highly likely, even given

the assumptions that 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, 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 being that there is 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.) So it is the *interaction between internal economies of scale and elastic factor supplies that gives rise to de facto external economies.*

*Modeling* A final point, which is crucial. To write a coherent model of the Big Push, it is necessary somehow 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 et al. 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.

Unfortunately, there are 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 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.

### **The Elements of High Development Theory**

In the last section I presented a modern version of the Big Push model as a motivating and clarifying example. Now I want to turn back to the older development literature to extract a broader set of key elements.

#### ***Economies of Scale and External Economies***

A casual reading of the development literature suggests that there is a dividing line circa 1960. Before 1960 writers on development generally assumed as a matter of course that economies of scale were a limiting factor on the ability to profitably establish industries in less developed countries, and that in the presence of such economies of scale pecuniary external economies assume 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/or of the extent to which the drive for formalism was pushing economics toward explicit models.

After 1960, by contrast, economists working on development had been trained in the formalism of constant-returns general equilibrium, and 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 may 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 key 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 unemployment or low-paying agricultural employment.

Some though not all subsequent development writers invoked economies of scale as key to external economies. In the best papers the basic story comes through very

clearly. Fleming (1954) presented an analysis of the nature of external economies in development that focuses very clearly 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 nonconvexities. Yet his explanation of the concept of backward linkages explicitly invokes the importance of achieving minimum economic scale, while his discussion of forward linkages more vaguely alludes to the role of scale as well.

So I would argue that high development theory circa 1958 did have as one of its central concepts the idea that economies of the scale at the level of the individual plant translated into increasing returns at the aggregate level via 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 then 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"



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