

THE FALL AND RISE OF DEVELOPMENT ECONOMICS

This is not exactly a paper about Albert Hirschman.

In the first place, I am unqualified to write such a paper. My acquaintance with Hirschman's works is very limited. In essence, the Hirschman I know is the author of The Strategy of Economic Development and little else. So I am in no position to write about his larger vision.

Furthermore, while I am a great admirer of The Strategy of Economic Development, I do not think that it was helpful to development economics. That may sound paradoxical, but I'll try to explain what I mean as I go along. To put it briefly, however, I regard the intellectual strategy that Hirschman adopted in writing that book as an understandable but wrong response to what had become a crisis in the field of economic development. Perversely, the very brilliance and persuasiveness of the book made it all the more destructive.

If this paper is not about Hirschman, what is it about? It is some reflections on two intertwined themes. One is the strange history of development economics, or more specifically the linked set of ideas that I have elsewhere (Krugman 1993) called "high development theory". This set of ideas was and is highly persuasive as at least a partial explanation of what development is about, and for a stretch of about 15 years in the 1940s and 1950s it was deeply influential among both economists and policymakers. Yet in the late 1950s high development theory rapidly unravelled, to the point where by the time I studied economics in the 1970s it seemed not so much wrong as incomprehensible. Only in the 1980s and 1990s were economists able to look at high development theory with a fresh eye and see that it really does make a lot of sense, after all.

The second theme is the problem of method in the social sciences. As I will argue, the crisis of high development theory in the late 1950s was neither empirical nor ideological: it was methodological. High development theorists were having a hard time expressing their ideas in the kind of tightly specified models that were increasingly becoming the unique language of discourse of economic analysis. They were faced with the choice of either adopting that increasingly dominant intellectual style, or finding themselves pushed into the intellectual periphery. They didn't make the transition, and as a result high development theory was largely purged from economics, even development economics.

Hirschman's Strategy appeared at a critical point in this methodological crisis. It is a rich book, full of stimulating ideas. Its most important message at that time, however, was a rejection of the drive toward rigor. In effect, Hirschman said that both the theorist and the practical policy-maker could and should ignore the pressures to produce buttoned-down, mathematically consistent analyses, and adopt instead a sort of muscular pragmatism in grappling with the problem of development. Along with some others, notably Myrdal, Hirschman didn't wait for intellectual exile: he proudly gathered up his followers and led them into the wilderness himself. Unfortunately, they perished there.

The irony is that we can now see that high development theory made perfectly good sense after all. But in order to see that, we need to adopt exactly the intellectual attitude Hirschman rejected: a willingness to do violence to the richness and complexity of the real world in order to produce controlled, silly models that illustrate key concepts.

This paper, then, is a meditation on economic methodology, inspired by the history of development economics, in which Albert Hirschman appears as a major character. I hope that it is clear how much I

admire his work; he is not a villain in this story so much as a tragic hero.

THE FALL AND RISE OF DEVELOPMENT ECONOMICS

The glory days of "high development theory" spanned about 15 years, from the seminal paper of Rosenstein Rodan (1943) to the publication of Hirschman's Strategy (1958).

Loosely, high development theory can be described as the view that development is a virtuous circle driven by external economies -- that is, that modernization breeds modernization. Some countries, according to this view, remain underdeveloped because they have failed to get this virtuous circle going, and thus remain stuck in a low level trap. Such a view implies a powerful case for government activism as a way of breaking out of this trap.

It's not that easy, of course -- just asserting that there are virtuous and vicious circles does not qualify as a theory. (Although Myrdal (1957) is essentially a tract that emphasizes the importance of "circular and cumulative causation" without -- unlike Hirschman (1958), which is often treated as a counterpart work -- providing much in the way of concrete examples of how it might arise). The distinctive features of high development theory came out of its explanation of the nature of the positive feedback that can lead to self-reinforcing growth or stagnation.

In most versions of high development theory, the self-reinforcement came from an interaction between economies of scale at the level of the individual producer and the size of the market. Crucial to this interaction was some form of economic dualism, in which "traditional" production paid lower wages and/or participated in the market less than the modern sector. The story then went something like this: modern methods of production are potentially more productive than traditional ones, but their productivity edge is large enough to compensate for the necessity of paying higher wages only if the market is large enough. But the size of the market depends on the extent to which modern techniques are adopted, because workers in the modern sector earn higher wages and/or participate in the market economy more than traditional workers. So if modernization can be gotten started on a sufficiently large scale, it will be self-sustaining, but it is possible for an economy to get caught in a trap in which the process never gets going.

The clearest and simplest version of this story is in the original paper by Rosenstein Rodan (1943) himself. In that 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 went 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 of dualism, embedded in the idea that these workers can be drawn from unemployment or low paying agricultural employment.

I regard Rosenstein Rodan's Big Push story as the essential high development model. Admittedly, some of the classics of high development theory differed in their emphasis from this central vision. On one side, Arthur Lewis's famous "Economic development with unlimited supplies of labor" emphasized dualism while ignoring the role of economies of scale and circular causation. On the other side, some authors, notably Fleming (1954), argued that owing to the role of intermediate goods in production -- what Hirschman would later memorably dub forward and backward linkages -- self-reinforcing development could conceivably occur even without dualism.

There were also disputes over the nature of the policies that might be required to break a country out of a low-level trap. Rosenstein Rodan and others appeared to imply that a coordinated, broadly based investment program -- the Big Push -- would be required. Hirschman disagreed, arguing that a policy of promoting a few key sectors with strong linkages, then moving on to other sectors to correct the disequilibrium generated by these investments, and so on, was actually the right approach. Indeed, Hirschman structured his book as an argument with what he called the "balanced growth" school. He did not acknowledge that he had far more in common with Rosenstein Rodan and other "balanced growth" advocates like Nurkse (1953) than any of them had with the way that mainstream economics was going.

For mainstream economics was, by the late 1950s, becoming increasingly hostile to the kinds of ideas involved in high development theory. Above all, economics was going through an extended period in which increasing returns to scale, so central to that theory, tended to disappear from discourse.

It may not be obvious just how crucial economies of scale were to high development theory. One of the characteristics of the writing of many of its expositors was a certain vagueness that makes it hard to know exactly what the essence of their arguments were -- a vagueness that, as we will soon see, was no accident. Still, if reads carefully, one finds that increasing returns are invariably crucial to the argument.

Consider, for example, what may have been Hirschman's most cited concept, that of "linkages." Some crude followers of Hirschman have identified these directly with having a lot of entries in the input-output table.⁽¹⁾ But Hirschman's own discussion makes it clear that the idea involved the interaction between market size and economies of scale.

In Hirschman's definition of backward linkages the role of market-size 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 at least minimum economic scale. The strength of an industry's backward linkages is to be measured by the probability that it will in fact 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 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.

So economies of scale were crucial to high development theory. Why did that present a problem? Because economies of scale were very difficult to introduce into the increasingly formal models of mainstream economic theory.

THE EVOLUTION OF IGNORANCE

A friend of mine who combines a professional interest in Africa with a hobby of collecting antique maps has written a fascinating paper called "The evolution of European ignorance about Africa." The paper describes how European maps of the African continent evolved from the 15th to the 19th centuries.

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 15th 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 travellers' 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 15th 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 18th 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 19th century darkest Africa had been explored, and mapped accurately. In the end, the rigor of modern cartography led to infinitely better maps. But there was an extended period in which improved technique actually led to some loss in knowledge.

Between the 1940s and the 1970s something similar happened to economics. A rise in the standards of rigor and logic led to a much improved level of understanding of some things, but also led for a time to an unwillingness to confront those areas 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 re-explored.

Economics has always been unique among the social sciences for its reliance on numerical examples and mathematical models. David Ricardo's theories of comparative advantage and land rent are as tightly specified as any modern economist could want. Nonetheless, in the early 20th century economic analysis was, by modern standards, marked by a good deal of fuzziness. In the case of Alfred Marshall, whose influence dominated economics until the 1930s, this fuzziness was deliberate: an able mathematician, Marshall actually worked out many of his ideas through formal models in private, then tucked them away in appendices or even suppressed them when it came to publishing his books. Tjalling Koopmans, one of the founders of econometrics, was later to refer caustically to Marshall's style as "diplomatic": analytical difficulties and fine points were smoothed over with parables and metaphors, rather than tackled in full view of the reader. (By the way, I personally regard Marshall as one of the greatest of all economists. His works remain remarkable in their range of insight; one only wishes that they were more widely read).

High development theorists followed Marshall's example. 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 high development 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 Robert Solow's initial statement of growth theory.

As in Marshall's case, this was not because development economists were peculiarly mathematically incapable. 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, the development field itself was at the same time generating mathematical planning models -- first Harrod-Domar type growth models, then linear programming approaches -- that were actually quite technically advanced for their time.

So why didn't high development theory get expressed in formal models? Almost certainly for one basic reason: high development theory rested critically on the assumption of economies of scale, but nobody knew how to put these scale economies into formal models.

The essential problem is that of market structure. From Ricardo until about 1975, what economists knew how to model formally was a perfectly competitive economy, one in which firms take prices as given rather than actively trying to affect them. There is a standard theory of the behavior of an individual monopolist who faces no comparably-sized competitors, but there is no general theory of how oligopolists, firms who have substantial market power but also face large rivals, will set prices and output. Still less is there any general approach to modeling the aggregate behavior of a whole economy largely peopled by oligopolistic rather than perfectly competitive industries.

Since the mid 1970s economists have broken through this barrier in a number of fields: international trade, economic growth, and, finally, development. The way they have done this is essentially by making some peculiar assumptions that allow them to exploit the bag of tricks that industrial organization theorists developed for thinking about such issues in the 1970s. (We'll see an example of the power and limitations of this kind of intellectual trickery below, when I present a quick formal version of the Big Push story). In the 1950s, although the technical level of the leading development economists was actually quite high enough to have allowed them to do the same thing, the bag of tricks wasn't there. So development theorists were placed in an awkward bind, with basically 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, an attempt to publish a paper like Rosenstein Rodan's would have immediately gotten a grilling: "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 the case for Fleming (1954), 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 rather than an awkward necessity. 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. In fact, however, their books 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 Allyn Young (1928), not to mention Rosenstein Rodan, and Nurkse in 1952 referred repeatedly to the circular nature of the

problem of getting growth going in poor countries. So Myrdal was in effect providing a capsulization of an already extensive and familiar set of ideas rather than a new departure. Similarly, Hirschman's distinctive 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, 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, trying to keep close to the increasingly model-oriented mainstream. Myrdal and Hirschman abandoned this effort, and eventually in effect took stands on principle against any effort to formalize their ideas.

One imagines that this was initially very 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. And this was the fate of high development theory. Myrdal's effective presentation of the idea of circular and cumulative causation, or Hirschman's evocation of linkages, were stimulating and immensely influential in the 1950s and early 1960s. By the 1970s (when I was myself 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? And so 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.

METAPHORS AND MODELS

I have just acknowledged that the tendency of economists to emphasize what they know how to model formally can create blind spots; yet I have also claimed that the insistence on modeling is basically right. What I want to do now is call a time out and discuss more broadly the role of models in social science.

It is said that those who can, do, while those who cannot, discuss methodology. So the very fact that I raise the issue of methodology in this paper tells you something about the state of economics. Yet in some ways the problems of economics and of social science in general are part of a broader methodological problem that afflicts many fields: how to deal with complex systems.

It is in a way unfortunate that for many of us the image of a successful field of scientific endeavor is basic physics. The objective of the most basic physics is a complete description of what happens. In principle and apparently in practice, quantum mechanics gives a complete account of what goes on

inside, say, a hydrogen atom. But most things we want to analyze, even in physical science, cannot be dealt with at that level of completeness. The only exact model of the global weather system is that system itself. Any model of that system is therefore to some degree a falsification: it leaves out some (many) aspects of reality.

How, then, does the meteorological researcher decide what to put into his model? And how does he decide whether his model is a good one? The answer to the first question is that the choice of model represents a mixture of judgement and compromise. The model must be something you know how to make -- that is, you are constrained by your modeling techniques. And the model must be something you can construct given your resources -- time, money, and patience are not unlimited. There may be a wide variety of models possible given those constraints; which one or ones you choose actually to build depends on educated guessing.

right in the way that quantum electrodynamics is right. At a certain point you may be good enough at predicting that your results can be put to repeated practical use, like the giant weather-forecasting models that run on today's supercomputers; in that case predictive success can be measured in terms of dollars and cents, and the improvement of models becomes a quantifiable matter. In the early stages of a complex science, however, the criterion for a good model is more subjective: it is a good model if it succeeds in explaining or rationalizing some of what you see in the world in a way that you might not have expected.

Notice that I have not specified exactly what I mean by a model. You may think that I must mean a mathematical model, perhaps a computer simulation. And indeed that's mostly what we have to work with in economics. But a model can equally well be a physical one, and I'd like to describe briefly an example from the pre-computer era of meteorological research: Fultz's dish-pan.

Dave Fultz was a meteorological theorist at the University of Chicago, who asked the following question: what factors are essential to generating the complexity of actual weather? Is it a process that depends on the full complexity of the world -- the interaction of ocean currents and the atmosphere, the locations of mountain ranges, the alternation of the seasons, and so on -- or does the basic pattern of weather, for all its complexity, have simple roots?

He was able to show the essential simplicity of the weather's causes with a "model" that consisted of a dish-pan filled with water, placed on a slowly rotating turntable, with an electric heating element bent around the outside of the pan. Aluminum flakes were suspended in the water, so that a camera perched overhead and rotating with the pan could take pictures of the pattern of flow.

The setup was designed to reproduce two features of the global weather pattern: the temperature differential between the poles and the equator, and the Coriolis force that results from the Earth's spin. Everything else -- all the rich detail of the actual planet -- was suppressed. And yet the dish-pan exhibited an unmistakable resemblance to actual weather patterns: a steady flow near the rim evidently corresponding to the trade winds, constantly shifting eddies reminiscent of temperate-zone storm systems, even a rapidly moving ribbon of water that looked like the recently discovered jet stream.

What did one learn from the dish-pan? It was not telling an entirely true story: the Earth is not flat, air is not water, the real world has oceans and mountain ranges and for that matter two hemispheres. The unrealism of Fultz's model world was dictated by what he was able to or could be bothered to build -- in effect, by the limitations of his modeling technique. Nonetheless, the model did convey a powerful

The important point is that any kind of model of a complex system -- a physical model, a computer simulation, or a pencil-and-paper mathematical representation -- amounts to pretty much the same kind of procedure. You make a set of clearly untrue simplifications to get the system down to something you can handle; those simplifications are dictated partly by guesses about what is important, partly by the modeling techniques available. And the end result, if the model is a good one, is an improved insight into why the vastly more complex real system behaves the way it does.

When it comes to physical science, few people have problems with this idea. When we turn to social science, however, the whole issue of modeling begins to raise people's hackles. Suddenly the idea of representing the relevant system through a set of simplifications that are dictated at least in part by the available techniques becomes highly objectionable. Everyone accepts that it was reasonable for Fultz to represent the Earth, at least for a first pass, with a flat dish, because that was what was practical. But what do you think about the decision of most economists between 1820 and 1970 to represent the economy as a set of perfectly competitive markets, because a model of perfect competition was what they knew how to build? It's essentially the same thing, but it raises howls of indignation.

Why is our attitude so different when we come to social science? There are some discreditable reasons: like Victorians offended by the suggestion that they were descended from apes, some humanists imagine that their dignity is threatened when human society is represented as the moral equivalent of a dish on a turntable. Also, the most vociferous critics of economic models are often politically motivated. They have very strong ideas about what they want to believe; their convictions are essentially driven by values rather than analysis, but when an analysis threatens those beliefs they prefer to attack its assumptions rather than examine the basis for their own beliefs.

Still, there are highly intelligent and objective thinkers who are repelled by simplistic models for a much better reason: they are very aware that the act of building a model involves loss as well as gain. Africa isn't empty, but the act of making accurate maps can get you into the habit of imagining that it is. Model-building, especially in its early stages, involves the evolution of ignorance as well as knowledge; and someone with powerful intuition, with a deep sense of the complexities of reality, may well feel that from his point of view more is lost than is gained. It is in this honorable camp that I would put Albert Hirschman and his rejection of mainstream economics.

The cycle of knowledge lost before it can be regained seems to be an inevitable part of formal model-building. Here's another story from meteorology. Folk wisdom has always said that you can predict future weather from the aspect of the sky, and had claimed that certain kinds of clouds presaged storms. As meteorology developed in the 19th and early 20th centuries, however -- as it made such fundamental discoveries, completely unknown to folk wisdom, as the fact that the winds in a storm blow in a circular path -- it basically stopped paying attention to how the sky looked. Serious students of the weather studied wind direction and barometric pressure, not the pretty patterns made by condensing water vapor.

It was not until 1919 that a group of Norwegian scientists realized that the folk wisdom had been right all along -- that one could identify the onset and development of a cyclonic storm quite accurately by looking at the shapes and altitude of the cloud cover.

The point is not that a century of research into the weather had only reaffirmed what everyone knew from the beginning. The meteorology of 1919 had learned many things of which folklore was unaware, and dispelled many myths. Nor is the point that meteorologists somehow sinned by not looking at clouds for so long. What happened was simply inevitable: during the process of model-building, there

is a narrowing of vision imposed by the limitations of one's framework and tools, a narrowing that can only be ended definitively by making those tools good enough to transcend those limitations.

But that initial narrowing is very hard for broad minds to accept. And so they look for an alternative.

The problem is that there is no alternative to models. We all think in simplified models, all the time. The sophisticated thing to do is not to pretend to stop, but to be self-conscious -- to be aware that your models are maps rather than reality.

There are many intelligent writers on economics who are able to convince themselves -- and sometimes large numbers of other people as well -- that they have found a way to transcend the narrowing effect of model-building. Invariably they are fooling themselves. If you look at the writing of anyone who claims to be able to write about social issues without stooping to restrictive modeling, you will find that his insights are based essentially on the use of metaphor. And metaphor is, of course, a kind of heuristic modeling technique.

In fact, we are all builders and purveyors of unrealistic simplifications. Some of us are self-aware: we use our models as metaphors. Others, including people who are indisputably brilliant and seemingly sophisticated, are sleepwalkers: they unconsciously use metaphors as models.

THE BIG PUSH

We can now return to the story of development economics. By the late 1950s, as I have argued, high development theory was in a difficult position. Mainstream economics was moving in the direction of increasingly formal and careful modeling. While this trend was clearly overdone in many instances, it was an unstoppable and ultimately an appropriate direction of change. But it was difficult to model high development theory more formally, because of the problem of dealing with market structure.

The response of some of the most brilliant high development theorists, above all Albert Hirschman, was simply to opt out of the mainstream. They would build a new development school on suggestive metaphors, institutional realism, interdisciplinary reasoning, and a relaxed attitude toward internal consistency. The result was some wonderful writing, some inspiring insights, and (in my view) an intellectual dead end. High development theory simply faded out. A constant-returns, perfect-competition view of reality took over the development literature, and eventually via the World Bank and other institutions much of real-world development policy as well.

And yet in the end it turned out that mainstream economics eventually did find a place for high development theory. Like the Norwegians who discovered that the shapes of clouds do mean something, mainstream economics discovered that as its modeling techniques became more sophisticated some neglected insights could be brought back in.

Since this sounds rather abstract, it will be best if I explicitly present an example of how one can now do a formal treatment of the classic model of high development theory: Rosenstein-Rodan's Big Push. The treatment is a streamlined version of the exposition in Murphy, Shleifer, and Vishny (1989), and reproduces my presentation in Krugman (1993).

Our paper-and-pencil dish-pan -- our model economy -- consists of a set of assumptions about the supply of resources; technology; demand; and market structure.

Resources. The only resource in the economy is labor -- that is, we neglect the role of capital, physical or human. Labor is in fixed total supply L . It can, however, 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 quality of labor is used in the traditional and modern sectors, it is not paid the same wage. Workers 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, average labor cost is 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. Note that for this example it is assumed that the relationship between input and output is the same for all N goods.

Demand. 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? She cannot raise her price as much as she would like. The reason is that 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 45-degree line 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 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

$$[(L/N) - F]/c > L/N$$

i.e., 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 -- and also 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 drawn, 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⁽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 high development idea that countries can be caught in low-income traps, but that self-reinforcing growth is also possible, is necessarily right. Even within this model, that story is true only for some parameter values. And the specific assumptions are obviously unrealistic. Yet the model illustrates several key points about the relationship between mainstream economics and high development theory.

First, it shows that it is possible to tell high development-style stories in the form of a rigorous model. The methods of mainstream economics may have created a predisposition to constant returns, perfect competition models, but they need not be restricted to such models.

Second, this example, like Fultz's dish-pan, shows that the essential logic of high development stories emerges even in a highly simplified setting. It is common for those who haven't tried the exercise of making a model to assert that underdevelopment traps must necessarily result from some complicated set of factors -- irrationality or short-sightedness on the part of investors, cultural barriers to change, inadequate capital markets, problems of information and learning, and so on. Perhaps these factors play a role, perhaps they don't: what we have just seen that a low-level trap can arise with rational entrepreneurs, without so much as a whiff of cultural influences, in a model without capital, and with everyone fully informed.

Third, the model, unlike a purely verbal exposition, reveals the sensitivity of the conclusions to the assumptions. In particular, verbal expositions of the Big Push story make it seem like something that must be true. In this model we see that it is something that might be true. A model like this makes one want to go out and start measuring, to see whether it looks at all likely in practice, whereas a merely rhetorical presentation gives one a false feeling of security in one's understanding.

Finally, the model tells us something about what attitude is required to deal with complex issues in economics. This model may seem childishly simple, but I can report from observation that until Murphy et al. published their formalization of Rosenstein-Rodan its conclusions were not obvious to many people, even those who have specialized in development. Economists tended to regard the Big Push story as essentially nonsensical -- if modern technology is better, then rational firms would simply adopt it! (They missed the interaction between economies of scale and market size). Non-economists tended to think that Big Push stories necessarily involved some rich interdisciplinary stew of effects, missing the simple core. In other words, economists were locked in their traditional models, non-economists were lost in the fog that results when you have no explicit models at all.

How did Murphy et al break through this wall of confusion? Not by trying to capture the richness of reality, either with a highly complex model or with the kind of lovely metaphors that seem to evade the need for a model. They did it instead by daring to be silly: by representing the world in a dish-pan, to get at an essential point.

CONCLUDING THOUGHTS

When I look at the Murphy et al representation of the Big Push idea, I find myself wondering whether the long slump in development theory was really necessary. The model is so simple: three pages, two equations, and one diagram. It could, it seems, have been written as easily in 1955 as in 1989. What would have happened to development economics, even to economics in general, if someone had legitimized the role of increasing returns and circular causation with a neat model 35 years ago?

But it didn't happen, and perhaps couldn't. Those economists who were attracted to the idea of powerful simplifications were still absorbed in the possibilities of perfect competition and constant returns; those who were drawn to a richer view, like Hirschman, became impatient with the narrowness and seeming silliness of the economics enterprise.

That the story may have been preordained does not keep it from being a sad one. Good ideas were left to gather dust in the economics attic for more than a generation; great minds retreated to the intellectual periphery. It is hard to know whether economic policy in the real world would have been much better if high development theory had not decayed so badly, since the relationship between good economic analysis and successful policy is far weaker than we like to imagine. Still, one wishes things had played out differently.

One would like to draw some morals from this story. It is easy to give facile advice. For those who are impatient with modeling and prefer to strike out on their own into the richness that an uninhibited use of metaphor seems to open up, the advice is to stop and think. Are you sure that you really have such deep insights that you are better off turning your back on the cumulative discourse among generally intelligent people that is modern economics? But of course you are.

And for those, like me, who basically try to understand the world through the metaphors provided by models, the advice is not to let important ideas slip by just because they haven't been formulated your way. Look for the folk wisdom on clouds -- ideas that come from people who do not write formal models but may have rich insights. There may be some very interesting things out there. Strangely, though, I can't think of any.

The truth is, I fear, that there's not much that can be done about the kind of apparent intellectual waste that took place during the fall and rise of development economics. A temporary evolution of ignorance may be the price of progress, an inevitable part of what happens when we try to make sense of the world's complexity.

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1. One US industrial policy advocate suggested that we target industries that either "provide inputs to or use inputs from a large number of other industries." I have often wondered what industry does not meet this criterion -- hand thrown pottery?

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

Figure 1