Developmen
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and
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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 dabblings 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.

Contents

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.[1]

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"[2]—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

$$L_i = F + cQ_i, \tag{1}$$

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.