PROCEEDINGS OF THE WORLD BANK ANNUAL CONFERENCE
ON DEVELOPMENT ECONOMICS 1992

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

Joseph E. Stiglitz

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

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

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

Joseph E. Stiglitz is professor of economics at Stanford University. He is indebted to Joshua Cans, Mark Cersovitz, and Andrb Rodriguez for helpful comments on the issues raised here.

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

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

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

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

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

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

2. Modeling "endogenousr technical change was a major thrust of research in this period. See, for

instance,-the collection of essays by Shell (1967); Atkinson and Sriglitz (1969); Bardhan {1970); and
Teubal (1967).

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

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

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

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

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

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

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

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

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

3. There are contexts in which a more subtle version of the argument might be relevant: income effes

are obviously important for nontraded goods, and there may be spillovers between the returns to scale for nontraded intermediate goods used to produce traded and nontraded final goods.

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

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

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

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

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

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

THE VISION

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

Financial Institutions

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

PoliticalEconomy

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

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

Externalities and Increasing Returns

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

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

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

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

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

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

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

When the economies of scale are spread more broadly, one must ask how they arise. And offsetting these economies of scale are diseconomies of scalecongestion economies. Regional economics provides some insight into these issues. We see agglomerations, economic activity that is not dispersed. Yet we also see viable communities, with high per capita income, that are relatively isolated geographically and are relatively small, certainly under a million population.

These communities are, of course, part of larger communities, but what are the effective barriers? If there were none, everyone would be equally a part of the world economy and could take advantage of whatever economies of scale were relevant at this highly aggregate level. But costs of communication and transport help delimit the scope of communities. These costs, in turn, have implications for patterns of development; at certain stages of development and for certain products, they may be larger. Unfortunately, models with aggregate increasing returns to scale give us absolutely no insight into the relevant issues.

One attraction of models with economies of scale and externalities is that using models with nonconvexities and externalities makes it easy to construct multiple equilibria, as Krugman effectively illustrates. (See also Sah and Stiglitz 1989; Stiglitz 1987, 1991; Murphy, Shleifer, and Vishny 1989). And it is tempting to try to interpret the differing situations in which industrial and developing counties find themselves as reflecting these different equilibria. But again, we hardly need nonconvexities and externalities to generate multiple equilibria. Solow (1956) showed us how we could do that with his simple model; all we need is to have savings rates or reproduction rates depend (in a particular way) on the capital-labor ratio. These models were inadequate because some of their central implications-such as convergence in the rate of growth of income per capita and equalization of factor prices[7]-seemed counterfactual.

Differences in Technology

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

7. Stiglitz (1970) and Inada (1968) extend the standard theory to the contcxt of growth. The implications for factor priCes across countries remain even after human capital is introduced; they are simply a consequence of the negative slope of the factor price frontier. For instance, if interest rates are equa!ized, it must be the case that if unskilled wages are lower in one country, skilled wages are higher. The critical assumption, of course, is that all countries face the same technology. By the same token, in international trade models with factor price equalization, suCh as that cited by Helpman and Krugman (1985)-2s in earlier models of local public goods with free migration of labor and goods (for example Stiglitz 1977)-one can easily obtain asymmetric multiple equilibria; yet welfare of all those of a given ability is identical in all communities and countries. Such models, accordingly, have little to contribute to our understanding of the development problem.

explanation-at least as it is usually presented-at best incomplete and at worst misleading or wrong.8 The secondl reason that technology may be less effective in a developing country is simply differential knowledge. To economists who are used to assumlUing that everyone has access to best-practice technology, this explanation is anathema; it is too simple, it is ad hoc, or it leaves unexplained why countries * ~~~lack access to best-practice technology. Yet once we recognize that informiation is costly to obtain and disseminate, that firms in industrial countries may have strategc reasons for withholding their mast advanced techinology, and chat local conditions make necessary adaptation of the technolog'y for the particular country, the explanation of differential knowledge makes perfect sense (see Gans 1989). That **it** is common sense is a virtue, not a vice.

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

REFERENCES

Amsden, A. E. *1939. Asia's Next Giant. South Korea and Late Industrialization. New* York: Oxford Univerity Press.

Aoki, Masahiko. 1970. 'A Note on Marshaflian Process under Increasing Returns." *Quarterly Journal of Economics* 84(1) (February): 100-12.

1987. "An Incentive-Compatible Approximnation of Nash-like Solution under Nonclassical Environments.' In Thomas Graves, Ray Radner, and Stanley Reiter, *eds., Info6rmation,. Incentives and Economic Mechanisms: Essays in Honor of Leonid Hurwicz.* Minneapolis: University of Minnesota Press; London: Blackwell.

Arrow, IK. 3. **1962.** "The Economic Implications of Learning by Doing." *Revtiew of Economic Studies* 29 (June): 15S-73.

* ~~Arthur, W. B. 1985. "Competing Techniques and Lock-in by Historical Events: The Dynamics of Allocation under Increasing Returns." Stanford University.

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

1988. "Competing Technologies: An Overview." In Giovanni Dosi, ed., *Technical Change and Economic Theory.* London: Frances Pinter.

1.939. "Competing Technologies, Increasing Returns and Lock-in by Historical

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

Review of Economic Studies 82 June):174-92.

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

. 1988. "Technological Change, Sunk Costs, and Competition." *Brookings Papers on Economic Activity* 3:883-947.
1991. "Social Absorption Capability and Innovation." Paper prepared for Korean Development Institute, 20th Anniversary Symposium, Seoul, June.
1992. "Whither Socialism?: Perspectives from the Economics of Information." Wicksell Lectures presented in Stockholm, May.
Teubal, Morris. 1967. "Inflation, Unemployment, and Economic Growth." Ph.D. thesis. University of Chicago, Chicago, Ill.

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