PART 9: POLICY REFORM, STABILIZATION, STRUCTURAL ADJUSTMENT AND GROWTH

Introduction

We noted in our preface that since the publication of the first two volumes of the *Handbook of Development Economics*, the paradigm of development that was dominant for a long time after the Second World War and which emphasized a state-directed, inward-oriented, import-substituting industrialization as the appropriate strategy of development was finally dethroned with many countries undertaking reforms aimed at liberalizing their economies from the shackles of state control. Enough data and experience with structural adjustment and stabilization in several countries have since become available to permit a serious evaluation of their impact. We noted also that interest in theories of long-run growth revived in the late eighties and a burgeoning theoretical and empirical literature on growth has since emerged.

The chapters that follow examine, from an analytical and empirical perspective, the important issues thrown up by the experience of developing countries, particularly since 1980, but also earlier. These include: a comparison of alternative development strategies, macroeconomic stabilization and microeconomic structural adjustment, poverty and the poor during the process of development, stabilization and adjustment, credibility, sequencing and the political economy of reforms. Also an assessment of recent contributions to the literature on growth theory and empirics from the perspective of the economics of development is included. The chapters are:

Handbook of Development Economics, Volume III, Edited by J. Behrman and T.N. Srinivasan

Chapter 44. Vittorio Corbo and Stanley Fischer: Structural Adjustment, Stabilization and Policy Reform: Domestic and International Finance

Chapter 45: Dani Rodrik: Trade and Industrial Policy Reform

Chapter 46: Pranab Bardhan, "The Contribution of Endogenous Growth Theory to the Analysis of Development Problems: An Assessment"

The dethronement of the dominant paradigm and the elevation to a higher status, if not enthronement, of openness, competition and the market in development is best illustrated by India, the earliest articulator and adopter of, and the last among major developing countries to abandon, the dominant paradigm. The Indian case is worth stating in some detail not only because of India's large share of the population and of the poor in the developing world, but also, as Krueger (Chapter 40) points out, because of the significant influence of Indian thought and experience on development. Further, Indian experience also illustrates and confirms some of the analytical results presented in some of the chapters about the reform process.

The foundations of post-independence Indian planning for economic development were laid in the late thirties and early forties, predominantly by the National Planning Committee of the Indian National Congress constituted in 1938 under the chairmanship of Jawaharlal Nehru. Also, other groups spanning the entire political spectrum including businessmen, labor unions and followers of Mahatma Gandhi put forward their own development plans in the early forties. Remarkably all groups agreed not only on the overarching objective of poverty eradication, but also on the dominant role the state had to play in achieving the objective. Indeed, that the strategy for economic development should be articulated through a national development plan and implemented through state-directed planning was also widely accepted.

The National Planning Committee completed most of its work prior to the arrest in 1940 of Nehru by the colonial government. Nehru (1946) provides a fascinating account of the committee's plan. It clearly shows that many, though not all, post-war debates among development economists and in international organizations about objectives, strategy, roles of industrialization, the state and foreign trade were anticipated by the committee.

The committee declared the overarching objective of development was

> . . . to insure an adequate standard of living for the masses; in other words, to get rid of the appalling poverty of the people. There was lack of food, of clothing, of housing, and of every other essential requirement of human existence. To remove this lack and insure an irreducible minimum standard for everybody, the national income had to be greatly increased, and in

addition to this increased production there had to be a more equitable distribution of wealth.[1]

We fixed a ten-year period for the plan, with control figures for different periods and different sectors of economic life. Certain objective tests were also suggested:

(1) The improvement of nutrition – a balanced diet having a calorific value of 2400 to 2800 units for an adult worker.

(2) Improvement in clothing from the then consumption of about 15 years to at least 30 yards per capita per annum.

(3) Housing standards to reach at least 100 square feet per capita. Further, certain indices of progress had to be kept in mind:

(a) Increase in agricultural production
(b) Increase in industrial production
(c) Diminution of unemployment
(d) Increase in per capita income
(e) Liquidation of illiteracy
(f) Increase in public utility services
(g) Provision of medical aid on the basis of one unit for 100 population
(h) Increase in the average expectation of life [Nehru (1946), pp. 402–403]

The committee deemed industrialization to be the primary instrument of development and asserted that the problems of poverty and unemployment, of national defence and of economic regeneration in general, cannot be solved without industrialization. Promotion of large scale manufacturing, heavy industries, electric power and scientific research were thought to be essential: "The three fundamental requirements of India, if she is to develop industrially and otherwise, are: a heavy engineering and machine-making industry, scientific research institutes, and electric power . . . an attempt to build up a country's economy largely on the basis of cottage and small-scale industries is doomed to failure". (p. 416)

The committee assigned a dominant role for state ownership and regulation in the development of industrial, agricultural and financial sectors:

[1] Disquiet expressed in the post-independence Parliament about the distribution of the income growth in the first two five-year plans was to lead Nehru to appoint a committee in 1960 under the chairmanship of the eminent statistician and planner, Professor P. C. Mahalanobis, to study the Distribution of Income and Levels of Living. The committee reported in 1964 long before the World Bank woke up to distributional issues.

> "The very essence of this planning was a large measure of regulation and co-ordination. Thus while free enterprise was not ruled out as such, its scope was severely restricted. In regard to defense industries it was decided that they must be owned and controlled by the state. Regarding other key industries, the majority were of opinion that they should be state-owned, but a substantial majority of the committee considered that state control would be sufficient. Such control of these industries, however, had to be rigid". (p. 403)

> "Agricultural land, mines, quarries, rivers and forests are forms of national wealth, ownership of which must vest absolutely in the people of India collectively . . . We, or some of us at any rate, hoped to evolve a socialized system of credit. If banks, insurance, etc. were not to be nationalized, they should at least be under the control of the state, thus leading to a state regulation of capital and credit. It was also desirable to control the export and import trade. By these various means a considerable measure of state control would be established in regard to land as well as in industry as a whole, though varying in particular instances, and allowing private initiative to continue in a restricted sphere". (p. 404)

Last, but not the least, foreign trade was viewed, not as an engine of growth, but of economic imperialism and autarkic development was extolled:

> "The objective for the country as a whole was the attainment, as far as possible, of national self-sufficiency. International trade was certainly not excluded, but we were anxious to avoid being drawn into the whirlpool of economic imperialism. We wanted neither to be victims of an imperialist power nor to develop such tendencies ourselves". (p. 403)

The development thought and strategy as formulated by Nehru's pre-independence committee governed India's development plans and policies in the first four and a half decades (1947–1991) since independence. An elaborate regulatory framework was instituted to implement the plans. The regulations covered the whole spectrum: the scale, technology, and location of any investment project other than relatively small ones were regulated; permission was needed to expand, relocate, change the output or input mixes of operating plants; critical inputs, particularly imported ones, were allocated; access to domestic equity markets and debt finance was controlled; some vital consumption goods were subject to complete or partial price controls; almost automatic and made-to-measure protection from import competition was granted to domestic producers in many "priority" industries, including in particular the equipment producers.

The crucial aspect of all these regulations was that they were essentially discretionary rather than rule-based and automatic. Although some principles and priorities were to govern the exercise of these regulatory powers, these were largely non-operational for two reasons. *First*, it was impossible, even in theory, to devise a set of principles or rules for all the myriad categories of regulations that were mutually consistent and in consonance with the multiple goals of the industrial policy framework, which in themselves were not entirely consistent. *Second*, the problem of translating whatever rules there were into operational decisions was a problem of Orwellian dimensions. The allocative mechanism was largely in the form of quantitative restrictions unrelated to market realities. A chaotic incentive structure and the unleashing of rapacious rent-seeking and political corruption were the inevitable outcomes.

The achievements under more than four decades of planning and inward-oriented development were modest: incidence of poverty (with a very modest poverty line) went down from over 50 percent of the population in the mid-fifties to about a third in the late eighties; real GDP grew at an average rate of less than 4 percent between 1950–1951 and 1992–1993; self-sufficiency was achieved in a number of commodities, including notably foodgrains, but at very modest levels of per capita consumption; life expectancy at birth increased from 32 years in 1951 to 61 years in 1992 and infant mortality fell from around 150 per 1000 live births to 79 during the same period; a diversified, but internationally uncompetitive, industrial structure was established, but the share of industry in GDP rose only modestly from about a sixth in 1950–1951 to a fifth in 1992–1993, and its share in employment rose far less; gross domestic saving and investment as a share of GDP more than doubled from about 10 percent in 1950–1951 to about 25 percent in 1992–1993; public sector became dominant in several industries, accounting for over 25 percent of GDP and 25 percent of capital stock; India's share in world exports fell from 2 percent in the late forties to about 0.6 percent in the nineties.

Even though the systemic failures of the Indian development strategy was already evident in the mid-sixties and were documented by, among others, Bhagwati and Desai (1970), Bhagwati and Srinivasan (1975) and more recently Bhagwati (1992), their arguments in favour of reforms were largely ignored by the government until a major macroeconomic (fiscal and balance of payments) crisis hit the economy in the wake of the Gulf War and brought it close to default on external debt in June 1991. The government of Prime Minister Rao that took power on June 21, 1991 recognized the *systemic and long-term* failures of India's development strategy and embarked on major reforms by dismantling the strangulating regulatory apparatus governing investment, foreign trade and the financial sector.

The Indian experience brings out, in an especially clear way, the reluctance to abandon cherished beliefs about the virtues of and the need for the state to assume a dominant role in the economy long after their failures had become evident. It also suggests that while macrostability (such as that India enjoyed until the eighties) in itself is not sufficient to generate sustained and rapid growth if micro distortions are massive, a severe macroeconomic crisis could trigger overdue systemic reforms. While the Indian development strategy needed drastic revision, its overarching objective, viz. poverty eradication, still remains as valid as it was in 1938 when the National Planning Committee began its work. Achieving credibility of a reform programme that drastically changes the development strategy pursued for over four decades and challenges long-held beliefs shared across the political spectrum is not easy.

Krueger examines the evolution of thought on development policy, the experience with the application of policies that challenged received wisdom and thought, and the two-way interaction between thought and policy. With this background, she examines a number of studies with individual aspects of economic policy and experience as their focus. She begins with the ideas that were part of the dominant paradigm described above, since, in her view, the experience with the policies that followed from it is still a factor affecting policies and thinking about development. She then reviews the few sustained successes (largely in East Asia), and many aborted successes and outright failures, from a policy perspective. This review leads her to argue that the association of outward-orientation with the successes and inward-orientation with failures was seen by policy makers in countries that failed as calling for policy reform. She concludes her chapter with a review of the theory of and evidence on alternative approaches to policy reform and of the crucial considerations of political economy in policy formulation, reform and execution.

Krueger's discussion of early ideas on development recapitulates the dominant paradigm, in particular its distrust of the private sector, markets and foreign trade and investment, and its exaggerated notions of what state interventions in the economy could achieve. She points out that the outcome of these ideas "was a fairly tightly interconnected set of economic policies. In country after country, an "economic plan" was drawn up (p. 2506). The plan set targets for investment to generate desired growth of income, estimated likely domestic savings, with the difference between required investment and available savings being the required external capital inflow. The analytical framework for deriving investment was the one-sector Harrod–Domar model and a simple Keynesian aggregate savings function for estimating available savings. Often the plan set detailed targets for the output of and investment in individual sectors, these being derived using input–output tables and simple capital–output ratios. Plans also included proposals to extend public sector involvement in economic activities, either by nationalization of private enterprises or creation of new public enterprises, particularly for producing import substitutes including intermediates. The financial sector was repressed to ensure credit was available at low cost to finance the public sector activities and favored private sector enterprises. Krueger points out that "there does not appear to have been very much thought given to the question either of how governments would ensure that the public sector activities specified in plans would be carried out or how private sector output and investment targets would be met" (pp. 2506–2507). Thus, the policy instruments to achieve plan targets (which, to begin with, were based on optimistic technical assumptions) were rarely fully specified and, unsurprisingly, achievements fell short of targets in plan after plan.

Yet, as Krueger points out, many countries achieved rapid rates of growth in the 50's and 60's, in part because of the favorable external environment of rapid growth of the global economy and in part because initial import substituting investments were in light consumer industries in which developing countries were not at a comparative disadvantage. But this growth could not be sustained for several reasons: import substitution was extended at high cost in many countries into activities in which they had no comparative advantage; interventions in and biases against agriculture created an excess demand for food and adversely affected the supply of export crops; the resort to monetary expansion to finance the growing public sector led to inflation; and the growth in import intensity of production in the face of slowly growing export earnings and other foreign exchange receipts resulted in periodic balance of payments crises. Interestingly enough, the problems and periodic crises did not, until the eighties, lead to rethinking of the policies and government control of the economy but rather to intensification of controls.

Krueger contrasts the above dismal experience with the success story of East Asia. Initially East Asian rapid growth was attributed to their special circumstances including massive foreign aid to Korea and Taiwan. As their growth was not only sustained but accelerated even after aid flows diminished, it became clear that good policies, particularly with respect to the external sector, rather than peculiar circumstances, explained East Asian success in achieving rapid growth, equitable sharing of the fruits of such growth, and the flexibility of the economy to withstand external shocks (e.g. the two oil shocks) without having to sacrifice growth to any significant extent. By the same token, inappropriate policies with respect to foreign trade and exchange rate, macroeconomy, agriculture and the financial sectors individually and in combination, explain the stalled growth of many other developing countries in the late seventies and eighties, according to Krueger.

The reform processes initiated in the eighties are briefly discussed by Krueger. Since the other chapters go more deeply and extensively into them (see below), it is worth focusing instead on her analysis of the political economy. As a prelude she briefly touches on the hoary topic of the proper role of government in development. She distinguishes three types of government actions: first, the things governments *must do* because the private sector cannot (e.g. maintenance of law and order, a system of property rights and contract laws and a stable macroeconomic framework); second, additional activities that governments *should* do that may enhance development efforts (e.g. provision of public health services, education and physical infrastructure such as roads, ports, railway and communication); third, the role of government in "picking winners".

While there is a consensus on the need for the first two types of government action, the third is very controversial. Some see the third role as explaining the success of East Asia (other than Hong Kong) through purposive government action in choosing industries and exports and ensuring their success through industry-specific interventions. Others, while acknowledging extensive government intervention in East Asia, nonetheless attribute the success to the uniformity of the exchange rate, combined with the discipline international markets provided in an outward-oriented economy. This discipline "severely constrained the scope of bureaucratic intervention and provided strong signals when policy mistakes were made" (p. 2542). This is in contrast to the inward-oriented economy of India where industry-specific protection was granted on a made-to-measure and permanent basis. Mistakes in investment that resulted in unprofitable operation of firms, instead of being corrected by letting them go bankrupt, were allowed to continue through budgetary support of losses. She quotes a study by Lee (1992) that government's attempt to "pick winners" was not that successful in Korea: those industries that had received less government support had experienced more growth in total factor productivity than those that did. The debate on the contribution of industry-specific government intervention per se in contrast to that of the provision of an environment conducive to growth by way of an excellent human and physical infrastructure and a roughly uniform set of incentives across activities in explaining East Asian success is still open.

Krueger raises two important aspects of the political economy of reform. First are the set of "circumstances in which policy reform is undertaken, the factors that appear to result in a higher probability of sustained reform, and ways in which reforms can be undertaken that reduce the likelihood of strengthening the opposition" (p. 2543). The second is "the extent to which it is appropriate to regard political reactions and pressures as exogenous, and the extent to which political-economic interactions render economic policies partially endogenous". (p. 2543)

Research on the first aspect focused on the role of potential gainers and losers from the reforms and on finding ways in which their balance could be tilted in favour of reforms. Although such a traditional interest-group analysis is useful, Krueger finds that it underplays four important features of policy reform. First, the policies to be reformed because of their adverse impact on growth were adopted in the first place for idealistic motives and in fact enjoyed broad support rather than that of narrow interest groups. This is best seen in the evolution of the Indian development strategy described earlier. Second, once certain policies are in place, those benefitting from them form a group with a vested interest in their continuation. The opposition to privatization of inefficient and loss-making public enterprises in India and elsewhere amply illustrates this. And even those who recognize that the policies have adverse effects often cling to the belief that it is not the policies that are at fault but particular ways of implementing them. Until recently this was common in India. Third, a crisis of major proportions can realign the power of various interest groups and thus provide a window of opportunity for introducing reforms. The initiation of far-reaching reforms in India after a major macroeconomic crisis in 1991 illustrates this. Fourth, it is simplistic to view the government as a Platonic guardian intent on maximizing social welfare. The nature of the state and the extent of its autonomy from interest group pressures would influence whether any reforms are undertaken, and if they are, the types of reform attempted and their outcomes. Bardhan (1984) provides an illuminating analysis of this phenomenon in India.

Endogeneity of policy is an area of active research. Krueger draws attention to the well-known and understandable phenomena that

> "once a political decision is made to undertake a particular economic policy or set of policies, market forces are set in motion which can thwart the policy or make it operate in ways which were not initially intended. The effects of the policy can also influence the alignment of political forces supporting and opposing the policy. Those interactions can then lead to changes in the initial policy, further market reactions, and so on". (p. 2544)

Indeed, this way of posing the problem at once suggests a sequential-game-theoretic approach to its analysis, as indeed some have attempted. Rodrik (Chapter 45) presents illustrative examples of such attempts. Krueger concludes, correctly, that

> "While analysis of the political economy of policy determination is still in its infancy, lessons already learned are sufficient to indicate that one cannot regard the political factors determining the choice of economic policies as entirely exogenous, nor can all economic policies be treated as stable steady states; interactions between political and economic markets are an important part of the process of policy formulation and execution". (p. 2546)

It should cause no surprise that eradication of mass poverty has been the overarching objective of development (at least in the rhetoric, if not in practice) of many developing countries from early on. As was noted earlier, India's National Planning Committee declared this objective more than five decades ago. What is surprising of course is the late discovery of the poor and poverty by multilateral development institutions such as the World Bank! Lipton and Ravallion (Chapter 41) trace the history of ideas about mass poverty and the realization that poverty is not an immutable human condition but one amenable to change through secular economic growth and through social and economic policy. They point out that until about 1750 in contemporary industrialized countries (and until the end of the Second World War in developing countries) there was hardly any growth in output per person and, as such, "moves to reduce poverty by peaceful redistribution proved politically inviable. In such a world poverty did not seem curable" (p. 2555). Adam Smith (1937) was aware of this. After pointing out that "No society can surely be flourishing and happy, of which the far greater part of the members are poor and miserable" (p. 79), he concludes that

> ". . . it is in the progressive state, while the society is advancing to the further acquisition, rather than when it has acquired its full complement of riches, that the condition of the laboring poor, of the great body of the people, seems to be the happiest and the most comfortable. It is hard in the stationary, and miserable in the declining state". (p. 81)

Thus, Smith did not think in terms of either equity versus growth or even equity with growth along the lines of the rather sterile discussion among development economists of recent vintage, but in terms of growth as a necessary dynamic force towards poverty alleviation.

The suggestions of Lipton and Ravallion that the Western European transition from poverty "drove the colonization process . . . (that) reproduced European progressive economies, and associated changes in poverty problems, led to important experiments in anti-poverty policy . . . relevant to poor countries today" and that it is precursor to a transition in less developed countries in the past half century, "to a stronger civil society, to a progressive economy, to modern demography and to more consensual states" (p. 2559) seem rather hyperbolic. But they are not entirely without foundation.

Lipton and Ravallion argue that import-substituting "forced draft planned industrialization offered little for the poor. Growth was often retarded; even when not, it brought few gains for the poor" (p. 2634), and it was financed in large part by extracting an agricultural surplus at the expense of the poor. They see a turn away from this towards rural development and investment in physical and human infrastructure in the 70's. They find that "many of the arguments that adjustment – relative to non-adjustment – had unambiguously hurt the poor were implausible. But so were some of the high expectations of supply-side response to adjustment, and hence to a rapid transition to a more favorable growth path". (p. 2634)

Lipton and Ravallion see a more balanced and realistic consensus emerging in the late 1980's as to how poverty can be most effectively reduced.

> "In this view, the main role of the state is to facilitate provision of privately under-supplied goods (infrastructure, but also social equity itself) in an otherwise market driven economy. With neutral incentives, growth in such an economy is seen as being in the best interests of the poor, who are intensive suppliers of the main factor of production likely to benefit, labor. Growth in private-sector economic activity is a key part of this story, both as an instrument for income poverty reduction, and as one of the means of financing public support where it is needed. But it is only a part. As much emphasis is given to successful public action, in the areas where it is called for". (p. 2635)

They suggest that the enduring topics of poverty research include

> ". . . the political economy of poverty reduction; country-incentive issues in pro-poor aid policies; the costs and benefits to the poor of asset redistribution; the ways initial distribution affects the type of growth, and hence final distribution; the extent to which poverty considerations should influence macroeconomic and trade policies; complementarities between fighting chronic poverty and fighting vulnerability to poverty; the status of the so-called "special poverty groups" (women, children, remote areas); environmental effects (positive and negative) of poverty and its reduction; the impacts of developed country policies on distribution within developing countries". (p. 2635)

They also identify two important roles for public action:

> "One involves fostering the conditions for pro-poor growth, particularly in providing wide access to the necessary physical and human assets, including public infrastructure. The other entails helping those who cannot participate fully in the benefits of such growth, or who do so with continued exposure to unacceptable risks. Here there is an important role for interventions aiming by various means to improve the distribution of the benefits of public expenditures on social services and safety nets in LDCs". (pp. 2637–2638)

Lipton and Ravallion's comprehensive discussion of poverty measurement, dimensions and characteristics of the poor and the interaction among growth, inequality and poverty ably summarizes a rich literature to which the two authors themselves have contributed significantly.

One of the robust features of the development process is the transformation of the economy from a structure in which the share of value added by agriculture in gross domestic product exceeds 50 percent and the share of the labour force dependent on agriculture for gainful activity exceeds two thirds, to one in which the two shares fall below 10 percent. Thus at early stages of development not only agriculture looms large in value added and gainful employment but also it shelters a disproportionate share of the poor consisting of landless agricultural laborers and marginal farmers. Arable land is obviously the crucial input in agricultural and livestock activities. Until cost of transporting agricultural commodities over long distances and storing them for extended periods of time fell, historically domestic (even local) agriculture supplied the food and fibre needs of most populations. It is not surprising therefore that the terms and conditions under which a right to owner cultivate land and the organization of agricultural production have been crucial in determining the pace and character of the above-mentioned structural transformation and progress towards alleviation of poverty. For example, the egalitarian character of the growth process in Japan, Korea and Taiwan has been attributed to their having had a thorough-going land reform, albeit imposed on them by "outsiders", soon after the end of the Second World War.

Binswanger et al. (Chapter 42) offer a fascinating account of the evolution of land rights and modes of production (e.g. slash and burn cultivation, communal ownership, manorial estate, junker estate, family farms) from the pre-Christian era to the present. They argue that it is much too simplistic and ahistorical to suggest that increasing land scarcity (in part driven by the increasing population relative to arable land) "leads to better definition of rights, which are then traded in sales and rental markets that are equally accessible to all players. The outcome should be the allocation of land to the most efficient use and users" (p. 2664) In their view:

> "rights over land and the concentration of ownership observed in most developing countries at the end of World War II are outgrowths of power relationships. Landowning groups used coercion and distortions in land, labor, credit, and commodity markets to extract economic rents from the land, from peasants and workers, and more recently from urban consumer groups or taxpayers. Such rent-seeking activities reduced the efficiency of resource use, retarded growth, and increased the poverty of the rural population". (p. 2664)

Land reforms are necessary, they suggest, to ensure that efficient small family farmers cultivate most of the land, since land markets are unlikely to bring this about. They describe the vicissitudes of land reforms in market and non-market economies and the subversion of the ideal of small family farms into large commercial farms in the former and large collectives in the latter. They establish that small farms are indeed efficient by examining the case for scale economies in agriculture and finding that such economies are exceptions. They attribute the failure of the land market to bring about an efficient distribution of land to a number of factors including covariance of risks, imperfections in credit markets, distortions in commodity markets and government subsidization of large farms. They review the theoretical and empirical literature on land lease arrangements and reiterate that tenancy and share cropping are not as inefficient as a naive Marshallian analysis would suggest. In fact they are optimal responses to incomplete or distorted markets for labour, credit and risk sharing and spending. Clearly government regulation of tenancy or banning it outright would have perverse efficiency and equity effects.

Binswanger et al. provide a thorough discussion of land policy. While recognizing that clear land titles and registration provide the necessary institutional framework for an efficient land market, they warn that in practice titling can lead to greater concentration of land ownership and dispossession of groups that have enjoyed land rights under a customary system. They then suggest a number of steps that could be taken to avoid these undesirable effects and reduce the cost of titling programs.

Although since the days of Henry George the virtues of a land tax have been recognized, in the absence of up-to-date land records indicating the size, value and ownership status of each tract of land, its productive capacity and profitability, designing and administering an efficient land tax is virtually impossible. Even where reasonably complete and up-to-date land record system and a tax structure based on it once existed, as in parts of British India, the political economy of post-independence India ensured that the taxes were repealed or allowed to be inflated away. Nonetheless Binswanger et al. find it useful to have flat or mildly progressive land taxes based on rough classification of land.

Regulations on land sales by imposing ceilings and floors on land holdings on balance are found to be inefficient. At the same time, even if the absence of a floor results in fragmentation of holdings, Binswanger et al. do not think land consolidation programs are likely to be cost-effective. As mentioned earlier, Binswanger et al. are against restrictions on land rents on grounds of efficiency and equity. They argue that unless the distortions that drive land prices above the capitalized value of profits from cultivation are removed, redistributive land reforms would fail since small farmers will have an incentive to sell out to large farmers and the environment would continue to favour large ownership holdings. The decidedly mixed experience with redistributive land reforms around the world confirms their assessment.

It was almost an article of faith in the early literature on development that investment in creating physical infrastructure (or in social overhead capital as it was called then) was lumpy (and hence subject to scale economies) and its supply and use had external effects. For both these reasons it was argued that less than socially optimal investment in infrastructural services would come about unless the government intervened. Given the rudimentary fiscal system of most developing countries, public subsidization of private investment in infrasturctural services was deemed infeasible so that the government itself had to invest and produce.

Jimenez (Chapter 43) analyzes issues relating both to physical infrastructure (services that enhance the productivity of physical capital) and human infrastructure (i.e. health, education and nutrition that enhance the productivity of labour). His survey of recent studies confirm the continuing critical importance of infrastructure for economic growth and for poverty alleviation. He finds that the positive link between measures of infrastructure and development is fairly robust across studies and methodologies. Micro level studies, despite problems with data and methodology, broadly confirm the aggregate results. The key role of government in financing and the supply of infrastructure is reemphasized by the recent literature, for the same reasons as in the early literature, viz. externalities, scale economies and public goods characteristics, are significant in infrastructure. However, Jimenez finds that in practice infrastructure investment has been inefficiently allocated in that projects with high social returns do not always get priority over those with low returns; services that are being provided are not often provided at the least cost; inappropriately priced; and their distribution is inequitable (i.e. the poor do not get an adequate share). Jimenez suggests that infrastructural investments are subject to "capture" by special interest groups and this is in part why projects that have higher social returns do not always receive funding. Examples are larger investments in lower yielding tertiary education than in higher yielding primary education, relative neglect of rural infrastructure, underinvestment in operations and maintenance and underspending in non-wage categories.

Jimenez reviews the analytics as well as the practice of pricing of publicly provided infrastructure. While properly cautioning against deriving general conclusions from studies with disparate and problematic methods and data of varying quality, he finds increasing empirical support for the conceptual arguments for increasing prices of infrastructural services. Empirical evidence strongly suggests that subsidies currently being offered could be directed more towards the poor and moderate increases in user fees are feasible and desirable. However, more research is needed to assess the feasibility of measures to protect the poor and for operationalizing the idea of raising user fees. Jimenez's analysis supports the broad conclusion reached by the World Bank (1994) in its report on infrastructure:

> "The potential for improving performance in infrastructure provision and investment is substantial, as is the quantity of resources devoted to infrastructure. Thus, both the need and the broad direction for reform are clear. Additional investment will obviously be needed – but more investment will not in itself avoid wasteful inefficiencies, improve maintenance, or increase user satisfaction. Achieving these improvements will require three broad actions: applying commercial principles to infrastructure operations, encouraging competition from appropriately regulated private sector providers,

> and increasing the involvement of users and other stakeholders in planning, providing, and monitoring infrastructure services. These adjustments call not only for policy changes, but also for fundamental institutional changes in the way that the 'business' of infrastructure is conducted". (p. 109)

The report also discusses the strengths and weaknesses (as well as measures to augment strengths and reduce the weaknesses) of four broad options regarding infrastructure: (a) public ownership and public operation, (b) public ownership and private operation, (c) private ownership and private operation, and (d) community and user provision. It concludes, correctly, that the choice among these options will depend on what is needed and what is possible in each country.

Chapter 44 by Corbo and Fischer is devoted to an analysis of structural adjustment and stabilization (SAS) programs. They point out that

> "In the 1980s many developing countries faced a combination of severe balance of payments problems, high and variable inflation, slow growth, and high unemployment. These problems emerged from the cumulative effects of weak national policies and institutions that combined with a drastic and unfavorable change in external conditions (terms of trade shocks, interest rate shocks, a worldwide recession, and a severe reduction in commercial bank lending) to lead to the debt crisis". (p. 2846)

Their definition of structural adjustment is rather broad

"Structural adjustment is a process of market-oriented reform in policies and institutions, with the goals of restoring a sustainable balance of payments, reducing inflation, and creating the conditions for sustainable growth in per capita income". (p. 2847, emphasis in original)

While concentrating on the experience since the early eighties, they briefly discuss earlier adjustment programs as well, even though they were not called as such: the term "structural adjustment" came into common use only in the 1980's after the World Bank proposed lending for structural adjustment. This new form of lending introduced in 1979, was to

> "support a program of specific policy changes and institutional reforms designed to reduce the current account deficit to sustainable levels; assist a country in meeting the transitional costs of structural changes in industry and agriculture by augmenting the supply of freely usable foreign exchange; act as a catalyst for the inflow of other external capital to help ease the balance of payments situation". (Ernest Stern, then Senior Vice President of the World Bank, as quoted by Corbo and Fischer, p. 2851)

Structural Adjustment Loans (SAL's) were complemented by Sectoral Adjustment Loans (SECALS) to support reforms in particular sectors. The International Monetary Fund introduced its own lending programs for structural adjustment in the 1980's by creating the Structural Adjustment Facility and the Extended Structural Adjustment Facility.

Corbo and Fischer point out

> "Adjustment programs were not confined to developing countries: New Zealand undertook a radical adjustment program starting in the 1980s; and the OECD increasingly laid stress on efficiency-oriented reforms in its member countries ... Nor was structural adjustment in the developing countries confined to those receiving financial support from the IFIs: China pursued its reform program with strong World Bank intellectual and financial support, but without the benefit of Bank adjustment lending until late in its adjustment process". (p. 2852)

They suggest that typically countries enter into an SAS programme after experiencing a severe balance of payments crisis. The crisis usually arises from one of two sources: unfavorable external shocks and pursuit of unsustainable policies. As they point out, correctly, countries would have to adjust in *some way* to the crisis, whether or not funding from the World Bank, IMF or other sources is available. The question then is whether such funding delays the adjustment process and makes it more expensive to undertake in the future, or whether it reduces the burden of adjustment by providing funds to countries at a lower cost than the cost at which they themselves would have been able to acquire the funds. The lowered cost presumably arises, not from subsidization by the international financial institutions, but from imperfections in the international capital market. But the argument based on capital market imperfections is relevant for lending for *any* project earning the relevant social rate of return and not just to adjustment lending. Lending for policy reform raises another difficult issue of its rationale in the case of a country that would not have undertaken such reform in the absence of such lending or, alternatively, that of a country that would have undertaken the reform even in the absence of such lending.

A policy is defined as unsustainable by Corbo and Fischer "when it cannot continue forever" (p. 2858). They distinguish between *economic* unsustainability (arising from the infeasibility to meet government's intertemporal budget constraint if policies do not change) and *political* unsustainability because of political infeasibility to continue with the policies. They also analyze economic unsustainability in the context of debt dynamics. They discuss issues relating to the *timing* of the crisis, i.e. when it will occur given unsustainable policies, and the political economy of the *timing* of the response to the crisis, i.e. whether stabilization will be delayed. They (also Srinivasan (1993a)) illustrate the economics of stabilization using the well-known dependent economy model of Salter (1959). This analysis demonstrates the need for two instruments, expenditure reduction and expenditure switching away from traded to non-traded goods, for restoring equilibrium once financing the external deficits is no longer possible. Thus a reduction in absorption and a real devaluation are needed.

Their model of the dynamics of disinflation in high inflation countries (such as those in Latin America) enables them to sort out the differences between the orthodox stabilization programs that focus on absorption and real devaluation and heterodox programs that in addition have wage-price controls as parts of the policy package.

Corbo and Fischer provide an illuminating discussion of the credibility and the optimal speed of implementing an adjustment and reforms package (e.g. big bang versus gradualism, to use the popular terminology). They correctly point out that if the package is fully credible and this is common knowledge to all participants, then the first-best solution is to implement it immediately. But if administrative constraints or political feasibility of removing particular distortions is not established, then either the package is no longer credible or one enters the realm of the second best so that no general recommendation can be made.

Their analysis of evaluations of structural adjustment programs (according to Corbo and Fischer, the World Bank committed as much as $41 billion to 258 adjustment loans between 1980 and 1991) is again very insightful. They reiterate an important and difficult to solve methodological issue in such evaluations, an issue that has been often recognized but less frequently acted upon in the literature. An *ex ante* evaluation of any proposed programme is difficult, if not impossible, given the multiplicity of its objectives, some of which are often vaguely defined at best, and the difficulty of controlling for myriad other factors besides the adjustment programme that would influence the course of the economy. An *ex post* evaluation that limits itself to the question whether the economy was performing better in some well defined sense after adjustment is difficult, if not altogether infeasible, primarily because the relevant comparison, namely, with a counterfactual scenario of a continuation pre-adjustment disequilibria, distortions and weaknesses without adjustment is not easy to construct (Srinivasan, 1988, 1993a).

A simplistic but common "before and after" approach (e.g. Cornia et al. (1987)) that compares the pre-and post-adjustment path of the economy can reveal nothing about the effect of an adjustment programme. Corbo and Fischer discuss results from the so-called "control group" and modified control group approaches. In the control group approach one proxies the relevant counterfactual for an adjusting country with the performance of a "control group" of countries which are similar but did not undertake adjustment. However, it is not easy to ensure that the "control group" is appropriate in the sense that they did not undertake adjustment but had pre-adjustment situations comparable to that of the country that undertook adjustment and experienced similar trends, as the adjusting countries did, during and after the period of adjustment, in exogenous variables, including in particular those relating to foreign trade and payments. On the other hand, counterfactual simulations from an econometric model of the adjusting country are not free of problems either unless the model tracks the pre-adjustment unsustainable disequilibrium path well and the methodology of comparison adequately captures the fact that the single *realized* actual path of the economy during adjustment and beyond has to be compared with a *distribution* of counterfactual paths.

Some might consider the conclusion of Corbo and Fischer that "The statistical cross-sectional studies provide an aggregate and reasonably consistent picture of the effectiveness of adjustment programs" (pp. 2889–2890) as somewhat stronger than warranted, given the above methodological problems. On the other hand, they are right in their conclusions that the research on evaluation "has not empirically evaluated the economics behind the programs; nor has it taken the extent of compliance into account with any care" (p. 2890) and

> "The absence of agreed-upon analytic or econometric models to analyze some of the basic problems of adjustment is striking. For instance, the analysis of sequencing problems is still underdeveloped. The important issue of the distributional impact of adjustment has received attention in computable general equilibrium models . . . but it is fair to say that this work has not yet had a wide impact. It is also striking how few empirical generalizations are yet widely accepted. In part this is because of the difficulty of constructing counterfactuals, a problem that lies at the heart of the econometric issues . . . In part it reflects the very broad and imprecise questions often asked in the analysis of adjustment, e.g. should the financial sector be deregulated before the trade sector. Almost surely the answer is "it depends", but we do not as yet have sufficient evidence or analytics to know precisely on what it depends. One thing we do know is that it depends also on political factors . . . That of course adds both interest and complexity to the analysis of adjustment". (p. 2917)

Rodrik (Chapter 45) also focuses on structural adjustment. He uses the term *structural adjustment* more narrowly than Corbo and Fischer to denote *policies* "aimed at improving an economy's efficiency and its long-term growth" (p. 7), thereby excluding the macroeconomic stabilization policies covered by Corbo and Fischer from his discussion, while recognizing the importance of the latter for the success of the former.

He identifies two common features of the 1980's from the perspective of developing countries:

> "First, much of the developing world, including a majority of countries in Latin America and Africa, became engulfed in a debt and macroeconomic crisis of major proportions. Per capita income scarcely grew, and, in many countries, declined over the course of the decade. It became commonplace to call this the 'lost decade' for development.
>
> But maybe not all was lost. For the second major feature of the decade was that in scores of countries, the inward-oriented, import-substituting policies of the past came under critical scrutiny from policy makers – often from the same government leaders who had enthusiastically espoused and implemented the older policies. By the end of the decade, the anti-export and anti-private enterprise bias of the prevailing policy regimes was largely discredited. Public enterprise, industrial promotion, and trade protection were out; privatization, industrial deregulation, and free trade were in". (p. 2927)

His chapter presents the theory (particularly from the recent literature), evidence, and their interplay in reviewing the available knowledge about the consequences of these reforms. His focus is confined to trade and industrial policies since the chapter by Corbo and Fischer covers macroeconomic stabilization issues; although, as he notes, it is not always easy to draw clear distinctions between stabilization and structural measures.

Rodrik, following Thomas et al. (1991, p. 11), terms structural adjustment as "changes in relative prices and institutions designed to make the economy more efficient, more flexible, and better able to use resources and so to engineer sustainable long-term growth" (p. 2929) and structural adjustment policies as those aimed at improving an economy's efficiency and its long-term growth.

The trade policies to be reformed "were directed at licensing and other quantitative restrictions, high and extremely differentiated tariff rates, export taxes, and burdensome bureaucratic requirements and paperwork" (p. 2930). The targets of reform in industrial policy were "inefficient and loss-making public enterprises, entry and exit restrictions on private enterprise, price controls, discretionary tax and subsidy policies, and soft-budget constraints" (p. 2930).

Rodrik identifies "four basic arguments in favour of "market-oriented policy reform: (i) economic liberalization reduces static inefficiencies arising from resource misallocation and waste; (ii) economic liberalization enhances learning, technological change, and economic growth; (iii) outward-oriented economies are better able to cope with adverse external shocks; (iv) market-based economic systems are less prone to wasteful rent-seeking activities. While all four of these arguments are used widely, it is the last three that have dominated the discussion on structural adjustment policies" (p. 2932).

It is hard to disagree with his statement that no compelling case has been made regarding the *magnitude* of static resource misallocation costs, even though the *qualitative* "theoretical and empirical arguments for the resource misallocation costs of the import-substitution syndrome are strong" (p. 2932). He identifies a number of problems that plague the empirical studies of the *dynamic* costs of distortion. These include:

> (i) the trade-regime indicator used is typically measured very badly, and is often an endogenous variable itself; (ii) the direction of causality is not always clear, even when a policy variable is used as the trade indicator: governments may choose to relax trade restrictions when economic performance is good; (iii) openness in the sense of lack of trade restrictions is often confused with macroeconomic aspects of the policy regime, notably the exchange-rate stance; (iv) the causal mechanisms that link openness to beneficial dynamic effects are rarely laid out carefully and subjected to test themselves; this makes it very difficult for policy conclusions to be drawn". (p. 2941)

He is equally dismissive of studies that purport to show that export-oriented countries are better positioned to deal with negative external shocks than inward-oriented countries. He argues that while the informal evidence is consistent with the view "that outward oriented countries have greater flexibility in responding to shocks, or that their political economy more easily allows (and accommodates) a change in macro policies" (p. 2943), still "we lack a good understanding of how and why certain configurations of economic policy render the economy more resilient to external shocks than others" (p. 2943).

Rodrik examines the mainstream (liberalizers') and heterodox (revisionists') interpretations of the success of East Asia. He identifies the following core set of conclusions on which the two groups might agree:

> "(i) there has been a lot of government intervention and an active trade and industrial policy; (ii) but intervention has taken place above all in the context of stable macroeconomic policies in the form of small budget deficits and realistic exchange-rate management; (iii) equally important, the governments' emphasis on and unmitigated commitment to exports has helped minimize the resource costs and incentive problems that would have otherwise arisen from heavy intervention; (iv) also, intervention has taken place in an institutional setting characterized by a "hard" state and strong government discipline over the private sector; (v) furthermore, such a setting is lacking in most other developing countries. What one then does with these conclusions depends on one's predilections. Some would argue that it is possible to engineer local versions of the institutions that have made Korea's

> or Taiwan's policies so successful . . . Others would conclude that weaker governments should economize on their scarcest resource, administrative competence, and restrict their involvement in the micro-management of the economy . . . Yet others would call for an entirely hands-off approach". (p. 2948)

Rodrik then looks at the East Asian experience in light of recent developments in trade and growth theories that emphasize scale economies, imperfect competition, externalities associated with human capital, endogenous technical progress, and so on. While these theories are elegant and insightful, and are indeed seen by some enthusiasts as rationalizing the interventions of the Korean, Japanese and Taiwanese governments, which, endowed with the uncanny ability to pick winners among industries, created comparative advantage for them, Rodrik is certainly right in suggesting that "Informal case studies like these aside, there are as yet practically no direct empirical tests of the specific trade-growth linkages identified above. We need such tests to close the large gap that presently exists between the empirical work . . . and the theoretical models . . . The former is informative but largely devoid of policy content, while the latter are stimulating, but remain empirically untested". (p. 2958)

Rodrik offers an incisive summary of theoretical models of the strategy of reform, and in particular, of aspects of political economy. Though he does not say it, his comment about the applicability of recent theoretical models of trade and growth in analyzing actual experience applies to these models as well. They are suggestive, but hardly conclusive, as descriptions of the actual reform strategy or the political economy of real countries.

Rodrik is again critical of available studies of the consequences of reform as "too often sloppy in identifying precise cause-and-effect relationships". But this criticism will apply to a large number of empirical studies in economics! Nonetheless, he finds that the evidence on the supply response to price changes (on which the argument for getting prices right is predicated) with respect to exports is clear: "a credible, and lasting effort to increase the supply-price of exportables is rewarded by a large, often very quick export response" (p. 2967), and that "export performance is subject to strong hysteresis effects: it may take a big push (i.e., sizable change in incentives) to get exports out, but by the same token, once the transition is made, not much may be required to keep them going". (p. 2968)

On the static and dynamic efficiency consequences of policy reforms, Rodrik is once again critical of the methodology of most studies for the same reason as Corbo and Fischer (Chapter 44) and Srinivasan (1993a):

> "One needs a counterfactual regarding what would have happened in the absence of reform, and to disentangle the effects of the reform under

consideration from the effects of other changes in the environment. To render a welfare judgment, one needs in addition a set of shadow prices to value the change in the quantities of outputs and inputs. Even if all these obstacles are surmounted, there is the difficulty of figuring out exactly what has happened". (p. 2969)

The one study that Rodrik finds the most systematic is that of Tybout (1992) who asks: "(i) has trade liberalization led to reduced price-cost margins in import-competing sectors? (ii) has it resulted in firms taking better advantage of scale economies through industry rationalization? (iii) has it led to improvements in technical efficiency?" (p. 2970). The first question is answered affirmatively in the studies reported by Tybout, the results regarding the second question are ambiguous, but are generally favorable to the third.

Rodrik's cautious conclusions are worth reproducing:

"the benefits of price reform remain small in relation to developmental objectives, and tend to be linked to economic growth through uncertain and unreliable channels . . . relative-price distortions, and the analysis thereof, are vastly over-emphasized relative to the institutional dimensions of reform . . . South Korean and Taiwanese economies have prospered in policy environments characterized by quantitative trade restrictions, selective subsidies, and discretionary incentives bearing more than a passing superficial resemblance to those in other developing countries. What has differed, of course, is the discipline exerted by the East Asian state over private-sector groups . . . (however) countries like Mexico, Argentina, Chile, and Bolivia have travelled recently much faster and further on the road to price reform and trade liberalization than South Korea, Taiwan, and Japan before they ever did . . . So a minimal conclusion for policy makers from the available evidence would be: get prices right if you can, but don't be deluded into thinking that reform ends there. Genuine reform requires the creation of a new set of interactions between government and the private sector, one that provides for an environment of policy stability and predictability, that discourages rent-seeking activities, and that improves on the governments' ability to discipline the private sector. In other words, the change that is needed is not only in policy, but also in policy *making*. The East Asian experience is full of clues as to what the end-product should look like. But we know much less about how to get there". (p. 2972)

Scholarly interest in the theory of long-run growth revived after a hiatus of two decades or so with the publication of the very influential papers of Lucas (1988) and Romer (1986). Lucas motivated his approach by arguing that neo-classical growth theory cannot account for the observed differences in growth across countries over time and its prediction that international trade should induce rapid movements toward equality in capital–labor ratios and factor prices is evidently counter-factual. Romer also looked for alternative theories to escape from what he viewed as a strong implication of the neoclassical growth model that in the absence of technical change, there can be no sustained growth in per capita output in the long run.

In Lucas's (1988) model each individual acquires productivity-enhancing skills by devoting time to such acquisition and away from paying work. The acquisition of skills by a worker not only increases that worker's productivity but by increasing the average level of skills in the economy as a whole, it has a spill-over effect on the productivity of all workers. In fact sustained growth in per capita output occurs in the Lucas model even if there is no spill-over effect because the marginal return to time devoted to skill accumulation is constant and not diminishing. In Romer (1986) long-run growth is driven by the accumulation of knowledge by forward-looking profit-maximizing agents, with the creation of knowledge by one firm having a positive external effect on the production possibilities of other firms. Thus in the aggregate the marginal product of knowledge does not diminish.

Readers of the still growing literature spawned by the papers of Lucas and Romer might be misled into thinking that indefinite scale economies, and positive externalities to human capital, knowledge capital and so forth postulated in most recent growth models, are essential to generating sustained-growth in per capita output in the long run and for endogenizing such growth. In fact, as was well understood by earlier growth theorists, in the neoclassical constant returns to scale model, sustained growth in the long run is possible if the marginal product of capital is bounded away from zero as the capital-labour ratio grows indefinitely. Indeed, as noted earlier, the models of Lucas and Romer in effect ensure this in different ways. Further it can be shown that increasing scale economies are neither necessary nor sufficient for generating sustained growth [Srinivasan (1993b)].

Bardhan points out that many of the growth models of the 1950's and 1960's endogenized technical progress in significant ways. Kaldor and Mirrlees (1962) endogenized technical progress (and hence the rate of growth of output) by relating productivity of workers operating newly produced equipment to the rate of growth of investment per worker. And there was the celebrated model of Arrow (1962) of "learning by doing" in which factor productivity was an increasing function of cumulated output or investment. Uzawa (1965) also endogenized technical progress by postulating that the rate of growth of labor augmenting technical progress was a concave function of the ratio of labor employed in the education sector to total employment. The education sector was assumed to use labor as the only input. Lucas (1988) in fact based his human capital accumulation process on Uzawa's model. Besides in the literature on induced innovation [Ahmad (1966), Boserup (1965), Kennedy (1964)] technical change was, by definition, endogenous.

Bardhan correctly argues that the contribution of recent authors is therefore neither in building models that generate sustained growth in per capita output nor in endogenizing technical progress. It is in the formalization of technical progress "in terms of a tractable imperfect-competition framework in which temporary monopoly power acts as a motivating force for private innovators. The leading work in this area (is) what has been called neo-Schumpeterian growth theory . . . Growth theory has now been liberated from the confines of the competitive market framework of earlier endogenous growth models in which dynamic externalities played the central role (even considering the models of Kaldor who repeatedly emphasized the importance of imperfect competition in the context of endogenous technical progress, the current models drawing upon the advances in industrial organization theory are more satisfactory). In particular, the emphasis on new goods and the fixed costs in introducing them provides valuable new insights. The major impact of this literature on development theory has been in the area of trade and technological diffusion...". (pp. 2985–2986)

This impact is best seen by contrasting the effect of trade liberalization in a standard Heckscher–Ohlin type constant-return-to-scale-neo-classical model of international trade with that from other models. In such a model, if all markets are competitive and trade restrictions take the form of tariffs (so that the economy operates on its production possibility frontier), the removal of tariffs yields a once-and-for-all real income (welfare) gains. These gains would accrue even if resources do not move freely across production activities in response to changed incentives, but would obviously be greater if they do. There would be no effect on *long-run growth* even if part or all of the gains in real income is invested, again under the assumption that the marginal product of capital diminishes to zero as capital accumulates indefinitely relative to other inputs. This is not to say of course that there would be no growth effect in the short to medium run, particularly if there are frictions in resource movements across sectors. Of course, if the marginal product of capital does not diminish to zero, say, if it is constant as in the one-sector Harrod–Domar or two-sector Mahalanobis–Fel'dman models, trade liberalization could affect long-run growth [Srinivasan (1993c)]. In the models of new growth theory the mechanism in which long-run growth is influenced by openness to foreign trade and investment is different.

Bardhan points out:

> "A major result in the new literature is to show how economic integration in the world market, compared to isolation, helps long-run growth by avoiding unnecessary duplication of research and thus increases aggregate productivity of resources employed in the R & D sector (characterized by economies of scale). World market competition gives incentives to entrepreneurs in each of these countries to invent products that are unique in the world economy". (p. 2986)

Further

> "... the new models of trade and growth bring into sharp focus the features of monopolistic competition particularly in the sector producing intermediate products and, in some models, the Schumpeterian process of costly R and D races with the prospect of temporary monopoly power for the winner – aspects which were missing in most of the earlier macroeconomic growth models...". (p. 2990).

In Bardhan's assessment

> "probably the most important contribution of the new growth theory is to formally draw our attention to the process of introduction of an ever-expanding set of new goods and technologies (in the case of developing countries, often imports of new types of produced inputs) and the large fixed costs usually associated with it. These fixed costs underline the fundamental importance of nonconvexities and imperfect competition in economic analysis". (p. 2992)

We noted in our Introduction to Part 7 that the representation of imperfect competition in applied general equilibrium models is limited. Its representation in endogenous growth theory is similarly simple or even simplisitic. Also, as Bardhan points out,

> "The new literature in some ways diverts our attention from the abiding concern of development economists with the problems of structural transformation and with those of reallocation of resources from traditional sectors to other sectors with different organizational and technological dynamics. But it does serve a purpose in focusing attention on the serious nonconvexities involved in the process of diffusion and adoption of new goods and technologies in a developing country". (p. 2992)

Non-convexities in growth models often generate multiple long-run equilibria, some of which are unstable, and the possibility arises that initial conditions determine to which equilibrium the economy converges, if it does converge, in the long run. These facts were well-known to and discussed by earlier growth and development theorists. Recent literature goes further in recognizing that in such a set up, temporary shocks instead of having only transitory effects could have permanent or long-run effects (so-called hysteresis). In the early development literature, it used to be argued that a poor economy is caught in a "low-income-equilibrium trap" while a high-income-equilibrium is feasible. The task of the development state was seen as to institute appropriate policies to enable the economy to break out of the trap. The recent literature points out that this is a simplistic view: just because there are many equilibria that can be ranked in terms of welfare, does not necessarily mean that there is room for government action to steer the economy away from a "bad" equilibrium to a "good" one. How a particular equilibrium gets established, whether through a "historical" accident or through self-fulfilling expectations of economic agents, is not a simple matter. As Bardhan points out

> "This 'history versus expectations' dichotomy in the dynamic process of how a particular equilibrium gets established has been further analyzed by Krugman (1991) and Matsuyama (1991) and the relative importance of the past and expected future is shown to depend on some parameters of the economy (like the discount rate and the speed of adjustment)". (p. 2994)

Xavier Sala-i-Martin (1994), a major contributor to the recent empirical growth literature, has provided an admirably short, though naturally sympathetic, survey of it. He points out that the early contributions to the literature tested an implication of the neoclassical growth model, viz. given diminishing returns to capital, other things being equal, countries with low amounts of capital per worker will grow faster, so that a negative relation should exist between the growth rate and the initial level of income. The hypothesized negative relation (the so-called convergence hypothesis) was not found in the Summers–Heston set used to test it. This rejection was taken as evidence against the neoclassical model.

Later researchers viewed this rejection, not as that of the neo-classical model, but a restricted version of it in which all countries had the same steady state growth path. The neo-classical model in fact suggests only that the growth rate of an economy will be inversely related to how far it is from its own steady state. The more general hypothesis, namely that of *conditional convergence* (each economy converges to its own steady state) was tested in two ways: by confining the test to a sample of economies which a priori could be deemed to have the same steady state (e.g. regions within a country), or by controlling for differences in steady states by including variables in the model to proxy the steady state of each economy. Tests, again with Summers–Heston data or with data on regions such as states of the United States, did not reject the conditional convergence hypothesis. The speed of convergence as estimated by many of the studies was found to be remarkably similar, around 2 percent per year, which is *slow* relative to a six percent rate associated with a neo-classical model with a plausible share of capital of 30 percent or less. The slow speed of convergence also implies that the effects of exogenous shocks or of policy shifts, even though they eventually wear out, could take very long to do so.

A second strand of empirical research [Barro (1991), Barro and Lee (1994)] regressed growth rates not only on initial income levels but in addition on a grab bag of explanatory variables "such as primary and secondary school enrollments, number of political assassinations, investment rates, and measures of distortions in capital markets" [Sala-i-Martin (1994), p. 741]. Barro and Lee found:

> "Differences in growth rates across economies are large and relate systematically to a set of quantifiable explanatory variables. One element of this set is a net convergence term, the positive effect on growth when initial real GDP per capita is low in relation to the starting levels of secondary school attainment and life expectancy. Growth depends negatively on a group of variables that reflect distortions and the size of government: the ratio of government consumption to GDP, the black-market premium on foreign exchange, and the frequency of revolutions. Growth depends positively on the ratio of gross investment to GDP but not as strongly as in some previous studies". (p. 294)

They were properly cautious about jumping to "reality from regression results to policy proposals" (p. 295).

How seriously should the growth regression results be taken? Unfortunately there are very serious data and econometric problems associated with the regressions. First, although Summers and Heston are careful to list the problems with their data, including in particular in identifying commodities that are close to being identical in different countries so that they can be priced out using a common set of prices, the users pay scant attention to their cautionary warnings. It is one thing to adjust for international differences in *price* structures as Summers and Heston do. But what they do not adjust for, and what in many cases is more serious, are *biases* in measurement of quantities [Srinivasan (1994)]. Indeed Summers and Heston (1991) themselves assign a quality rating of D + or D to the data of 66 out of their 138 countries, most of which are less developed countries, 37 of them being African countries. Data on investment are particularly unreliable. Biases as well as measurement-errors might vary in an unknown fashion over time and across countries and obviously such variations have implications for growth regressions.

Second, in their critique of the regressions, Levine and Renelt (1992) find that small changes in the right-hand side variables produce different conclusions about the links between individual policies and growth in cross-country studies. Sala-i-Martin reads their critique, not as an indictment of the non-robustness of their results, but as implying that some group of policy variables always matters. However, since policies are highly correlated with each other, he argues that data cannot tell them apart. But he does not note that this also means that it is impossible to tell which policy matters and which does not!

Third, policy indicators as well as some of the other variables often included in cross-country regressions are *endogenous*. As such, the problem of simultaneity bias arises. This is not just a technical quibble – simultaneity bias may change drastically the interpretation of the parameter estimates. Fourth, in studies involving cross-sections repeated over time, sometimes country-specific effects (fixed or random) are included. Since the other explanatory variables (particularly policy variables) might plausibly correlate with country-specific effects, as Deaton's chapter in this volume points out, the random effects estimator will be inconsistent. On the other hand, if these effects are treated as *fixed*, removing fixed effects by differencing introduces a correlation between the disturbance term in the differenced regression and its explanatory variables, if the latter include lagged values of the dependent variable. If the number of time periods over which the cross-sections are repeated is small relative to the number of countries included in each cross section, the fixed effect estimate will also be inconsistent.

Finally, whatever other insights cross-country regressions testing some version or the other of the convergence hypothesis relating to *aggregate* growth have yielded about the growth process, by their very nature, they have little to say about the microeconomic forces that together generate the aggregate outcome or about effectiveness of policies. Here again the observations of Lucas (1993) are pertinent:

> "I do not intend these conjectures about the implications of a learning spillover technology for small countries facing given world prices to be a substitute for the actual construction of such a theory. . . . What is the nature of human capital accumulation decision problems faced by workers, capitalists and managers? What are the external consequences of the decisions they take? The purpose cited here considers a variety of possible assumptions on these economic issues, but it must be said that little is known, and without such knowledge there is little we can say about the way policies that affect incentives can be expected to influence economic growth" [Lucas (1993), p. 270].

Even if one were to ignore their lack of a solid microeconomic foundations and their uncritical use of aggregate data with serious measurement errors and biases, the inference drawn from many convergence regressions could be questioned on econometric grounds as Quah (1993a, 1993b and 1994) has done. He suggests that these studies "do not at all shed light on the important, original question: Are poor economies catching up with those richer" (Quah (1994), p. 52). This is indeed the fundamental question of development and it is yet to be answered satisfactorily.

JERE BEHRMAN
T.N. SRINIVASAN

References

Ahmad, Syed. (1966) 'On the theory of induced innovation'. *Economic Journal*, 76:344–357.
Arrow, K.J. (1962) 'The economic implications of learning by doing'. *Review of Economic Studies*, 29:155–173.

Bardhan, P. (1984) *The political economy of development in India*, Oxford.
Barro, R. (1991) 'Economic growth in a cross section of countries', *Quarterly Journal of Economics*, 106:407-501.
Barro, R. and Lee, J.-W. (1994) 'Losers and winners in economic growth', in: *Proceedings of the World Bank Annual Conference on Development Economics 1993*, Supplement to the *World Bank Economic Review* and *World Bank Research Observer*. Washington, DC: The World Bank, 267-297.
Bhagwati, J. and Desai, P. (1970) *India: Planning for industrialization*. London: Oxford University Press.
Bhagwati, J. and Srinivasan, T.N. (1975) *Foreign trade regimes & economic development: India*. New York: Columbia University Press.
Bhagwati, J. (1992) *India's economy: The shackled giant*. Oxford: Clarendon Press.
Boserup, E. (1965) *The conditions of agricultural growth*. Chicago: Aldine.
Cornia, A.G., Jolly, R. and Stewart, F., eds., (1987) *Adjustment with a human face*, A study by UNICEF, Vol. I, Oxford: Clarendon Press.
Kaldor, N. and Mirrlees, J. (1962) 'A new model of economic growth', *Review of Economic Studies*, 29(3):174-192.
Kennedy, C. (1964) 'Induced bias in innovation and the theory of distribution', *Economic Journal*, 74(298):541-547.
Krugman, P. (1991), 'History versus Expectations', *Quarterly Journal of Economics*, 106:651-667.
Lee, J.H. (1992) 'Government interventions and productivity growth in Korean manufacturing industries', Paper presented at NBER Conference on Economic Growth, October, Cambridge, MA.
Levine, R. and Renelt, D. (1992) 'A sensitivity analysis of cross-country growth regressions', *American Economic Review*, 824, 942-963.
Lucas, R. (1988) 'On the mechanics of economic development', *Journal of Monetary Economics*, 22:3-42.
Lucas, R.E. (1993) 'Making a miracle', *Econometrica*, 61(2):251-272.
Matsuyama, K. (1991) 'Increasing returns, industrialization and indeterminacy of equilibrium', *Quarterly Journal of Economics*, 106:616-650.
Nehru, J. (1946) *The discovery of India*, New York: The John Day Company.
Quah, D. (1993a) 'Empirical cross-section dynamics in economic growth', *European Economic Review*, 37(2/3), (April)426-434.
Quah, D. (1993b) 'Galton's fallacy and tests of the convergence hypothesis', *The Scandinavian Journal of Economics* 95(4), (December)427-443.
Quah, D. (1994) 'Convergence empirics across economies with (some) capital mobility', London School of Economics and Political Science, Suntory-Toyota International Centre for Economics and Related Disciplines, Discussion Paper No. EM/94/275.
Romer, P. (1986) 'Increasing returns and long-run growth', *Journal of Political Economy*, 94:1002-1037.
Sala-i-Martin, X. (1994) 'Cross-sectional regressions and the empirics of economic growth', *European Economic Review*, 38, 739-747.
Salter (1959) 'Internal and external balance: The role of price and expenditure effects', *Economic Record*, 35 (August), 226-238.
Smith, A. (1937) *The wealth of nations*, New York: Random House, Inc.
Srinivasan, T.N. (1988) 'Structural adjustment, stabilization, and the poor', Working Paper, Economic Development Institute, Washington, DC: The World Bank.
Srinivasan, T.N. (1993a) 'Adjustment lending: Some analytical and policy issues', Yale University, processed.
Srinivasan, T.N. (1993b), 'Long-run growth theories and empirics: Anything new?", in: T. Ito and A. Krueger, eds., *Lessons from east Asian growth*, Chicago: University of Chicago Press, forthcoming.
Srinivasan, T.N. (1993c), 'Comments on Paul Romer: Two strategies for economic development: Using ideas vs. producing ideas', *Proceedings of the World Bank Annual Conference on Development Economics, 1992*, Supplement to the *World Bank Economic Review* and *World Bank Research Observer*, Washington, DC: The World Bank, 103-109.

Srinivasan, T.N. (1994) 'Data base for development analysis: An overview', *Journal of Development Economics* 44, June 1994, pp. 3–27.

Thomas, V., Chhibber, A., Dailami, M. and de Melo, J., eds. (1991) *Restructuring economies in distress: Policy reform and the World Bank*, Oxford and New York: Oxford University Press.

Tybout, J.R. (1992) 'Linking trade and productivity', *The World Bank Economic Review* 6, 189–211.

Uzawa, H. (1965) 'Optimum technical change in an aggregative model of economic growth'. *International Economic Review*, 6:18–31.

World Bank (1994) *World Development Report 1994*, Oxford University Press.