POVERTY AND INEQUALITY IN LATIN AMERICA

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SCHEMING FOR THE POOR: THE POLITICS OF REDISTRIBUTION IN LATIN AMERICA. By WILLIAM ASCHER. (Cambridge, Mass.: Harvard University Press, 1984. Pp. 348. $25.00.)

PERSISTENT POVERTY: UNDERDEVELOPMENT IN PLANTATION ECONOMIES OF THE THIRD WORLD. By GEORGE L. BECKFORD. (Morant Bay, Jamaica: Maroon Publishing, 1983. Pp. 244. $9.25.)

INGRESO, DESIGUALDAD Y POBREZA EN AMERICA LATINA. Edited by PHILIP MUSGROVE. (Rio de Janeiro: Banco Interamericano de Desarrollo, 1982. Pp. 374.)

CONSUMPTION AND INCOME DISTRIBUTION IN LATIN AMERICA. Edited by ROBERT FERBER. (Washington, D.C.: Organization of American States, 1980. Pp. 484. $15.00.)

THIRD-WORLD POVERTY: NEW STRATEGIES FOR MEASURING DEVELOPMENT PROGRESS. Edited by WILLIAM PAUL MCGREEVEY. (Lexington, Mass.: Lexington Books, 1980. Pp. 215. $23.95.)

THE GAP BETWEEN THE RICH AND THE POOR: CONTENDING PERSPECTIVES ON THE POLITICAL ECONOMY OF DEVELOPMENT. Edited by MITCHELL A. SELIGSON. (Boulder, Colo.: Westview Press, 1984. Pp. 418. Cloth $42.00, paper $19.95.)

Since international comparisons of income distribution began, the extreme inequality characterizing Latin American countries has been a source of commentary. Almost all of these countries have levels of productivity that would permit eradicating serious poverty were inequality not so extreme, a respect in which Latin America differs from the poorer countries of Asia and Africa. The fact of inequality thus has particularly tragic implications in this part of the Third World. At the same time, the relatively fast growth of most Latin countries in the postwar period has naturally focused attention on the question of whether and to what degree growth can gradually eradicate poverty via a process often referred to as the "trickle-down effect," whereby without significant restructuring of the socioeconomic system, the poor nonetheless share the fruits of growth.

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General understanding of the roots of continuing poverty in Latin America and changes in its nature and severity has been advanced by various kinds of research that I will classify arbitrarily into three categories: first, elaboration of broad socioeconomic theories or frameworks (dependency, neoclassical, neo-Marxist, and similar approaches), or of narrower hypotheses relating to only a part of the overall system; second, description of what has happened; and third, testing of theories that have been elaborated.

Any such classification tends to exaggerate the appropriate degree of separation among types of research—a broad theory, to be of lasting interest, must have been framed in the light of empirical evidence, and useful description must include a look at the variables central to the theories. In fact, it seems that the separation has often been undesirably wide, which has contributed to the slow progress of the social sciences in reaching important and defensible conclusions on such questions as why inequality is so extreme in Latin America (particularly the relative importance of domestic and international factors leading to this result); how the impressive postwar growth in the region has affected poverty and income distribution; and what, if anything, socioeconomic policy can do about poverty and inequality. But progress *is* being made, necessarily slowly because of the serious problems of inadequate information and the difficulty of constructing rigorous tests of the hypotheses on the table. Economic research has taught researchers enough about the proximate sources of inequality and its technical avoidability to throw the spotlight on politics and why governments are unable or opt not to take the steps needed to reduce poverty. Political science, sociology, and other disciplines, in turn, have provided many good leads on this latter question.

The studies reviewed here span a considerable range of research approaches and hence vary in the kind of contribution they make. Collectively, they nicely reflect the state of the field; at present, its most glaring weakness seems to me the scarcity of solid empirical work on what (for want of a better term) I will refer to as the "details" of the processes determining poverty and inequality, whether those processes are primarily economic, political, or sociocultural in nature. It is tempting to say that this area has suffered from too much loose theorizing and too much dogmatism, although to make the case persuasively one must have a good feel for how theory and empirical work interact.

With respect to ideas (that is, theories and hypotheses), scholars have long benefited from the broad insights provided by the Marxist tradition, the classical and neoclassical traditions in economics, dependency theory, and so on. Some of the insights are no more than common sense, others much subtler; together they add up to a lot of ideas worth testing, whether the test is of specific mechanisms that constitute part of a framework or of the whole framework. What are scarce are well-executed tests—usually for lack of testers, but sometimes because the test is difficult or involves a long wait before the real world produces the relevant evidence. On some issues, further theoretical refinements and debates are likely to be of questionable value until more serious empirical research is done.

George Beckford's *Persistent Poverty: Underdevelopment in Plantation Economies in the Third World* I find to be an exception to the proposition that theories too often float free, unencumbered by excessive contact with empirical evidence. This 1972 study, recently republished by Maroon Publishing House in Jamaica, provides a valuable holistic framework to help explain both the degree of internal inequality in plantation societies and their frequently slow economic progress. Beckford draws on economic, political, and social mechanisms to explain his pessimistic expectations with respect to the plantation economy. Consistent with the balanced professionalism of the study is a wise caution in recognizing that the framework presented is a hypothesis, not a truth. Little that is novel is found in the economic mechanisms under discussion. What is unusual is the impressive weaving together of the economic, political, and social processes. If Beckford's broad proposition that the plantation economy is an unlikely scenario for true development is accurate, the key mechanisms will have been ones on which economic science per se is not able to throw much light. Examples are the political bias in favor of the plantations relative to other sectors, the tendency to do too little research on nonplantation agricultural products, and the slavish imitation by the other classes of the planters' preference system. It is true that some plantation or former plantation economies are much more developed than others (for example, Jamaica or Cuba relative to Haiti), and these differences remain important themes for research.

The volume edited by Mitchell Seligson, *The Gap between the Rich and the Poor: Contending Perspectives on the Political Economy of Development*, does not focus specifically on Latin America but presents a broad sample of social scientists' thinking on issues of inequality. Although many of the papers selected are important, the overall impression is one of a relatively ineffective search for understanding. Seligson's objective was to provide "the clearest answers that social science has been able to offer to date" on the reasons for the large income gaps between the rich and the poor countries and within the latter. The volume includes articles providing conceptual explanations of the gaps and others testing those explanations. Despite the presence of a number of persuasive pieces and quite a few interesting ones, my sense is that if this collection does provide the "clearest answers" available, it is an unhappy commentary on social scientists' success in this area of research. Seligson's comment that "research in this area represents one of the best illustrations of a cumulative social science continually deepening its understanding of a complex problem" (p. 397) has a modicum of truth to it, but one could also ask why it has taken so long to cover what seems to me a rather short distance. Does it mean that, as a community, we social scientists have been incompetent?

Perhaps it does. Some of the slowness reflects the general absence of productive interaction between more neoclassically and quantitatively oriented economists on the one hand and political scientists, sociologists, and historians on the other. The two groups have tended to start with rather different models and theoretical inclinations and have used considerably different methodologies. In trying to explain levels of internal inequality, most economists have limited themselves to measurable economic variables such as level of development and distribution of education, and they have moved less into world-system and dependency analysis. Some may be uninterested in the broad theories; many others feel daunted by the task of seriously testing or evaluating them and simply back away in frustration. Their restricted scope of analysis has limited the interaction between their work and that of the social scientists who are responsible for the broader theories. The two main costs have been that the former group has not yet seriously addressed some of the key issues and that those who have attempted serious assessments of important political economy frameworks have only gradually taken advantage of methodological approaches pioneered by economists as well as the latter's familiarity with quantitative relationships within an economy. The second problem is exemplified in the still-flawed techniques characterizing most quantitative studies of the central propositions of dependency theory.

The "contending explanations" of inequality presented in Part 2 of *The Gap between the Rich and the Poor* are without exception interesting, notwithstanding the fact that several seem to be partial theories masquerading as complete ones. I do have the unsatisfying feeling that the high points of the conceptual debates have too often been conceded to authors or groups of questionable merit. This feeling was strong as I read Immanuel Wallerstein's crisp article, "The Present State of the Debate on Inequality," and was unable to place much of the more impressive research and thinking on inequality with which I am familiar in either of his two major categories: the Rostovian, following Walter Rostow's *The Stages of Economic Growth*, or the "encrusted version of evolutionary Marxism." Many competent thinkers have never taken either of these views seriously as a description of the world—as interesting hypotheses, yes, but as persuasive theories, no. I wonder whether Wallerstein's criticisms of the "rigid developmentalist framework" do not constitute tilting at a straw man. Perhaps Rostow's blueprint in *The*

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Stages of Economic Growth was taken seriously by many liberal economists for a decade and by many politicians for much longer. Some of its ideas certainly enjoy continued support, but thinking has evolved enough so that the volume itself is now largely passé (off the reading lists), unlike another prominent book from the 1950s, Arthur Lewis's *The Theory of Economic Growth*, which continues to impress by the richness of its insights. Serious rigidities undoubtedly remain in mainstream "development economics," but movement has occurred, as well as some useful confrontations between theories and evidence. Certainly, Wallerstein raises interesting points in the elaboration of his world-system perspective. But I am inclined to agree with Tony Smith's "Reiterating the Identity of the Peripheral State" when he describes as ideological the "insistence, within the dominant mode of analysis for studying the impact of imperialism on the periphery, that social developments in Africa, Asia, and Latin America be seen within a historical and global context dominated by the force of imperialism" (p. 133). In fact, Smith's judgment of many of the dependency and world-system theorists seems valid to me: because these thinkers are ideologically motivated, and although their arguments may have much merit, they tend to overstate their case. His quotes from Baran's discussion of India leave little doubt as to the carelessness of this author's historical observations, and Smith's parallel criticisms of other leading authors in this area strike home.

On the empirical side, the papers selected by Seligson include several that are typical of Western economists' approach, such as the contributions of Irma Adelman and Cynthia Taft Morris, Gary Fields, and Norman Hicks, with their focus on identifying the economic correlates of inequality and ascertaining how economic growth is related to inequality. While subsequent work has cast doubt on some of the conclusions or hypotheses presented by these authors, they are relatively sound methodologically and their logic is transparent. But as noted above, many social scientists would conclude that such essays pay no attention to some central hypotheses on the determination of inequality. Not so the series of empirical papers designed as tests of one or another aspect of dependency and world-system theories. Their authors are not economists, and the literature interacts little with that of the economists. Over the last few years, this literature has improved strikingly in technical sophistication and attention to the quality of empirical information. But I find that even the best of these studies contain methodological problems that leave virtually all of the conclusions open to serious question. It is a frustrating case of valiant efforts at improvement still falling short of what is needed, and here a closer link with economists would have proved useful. Although economists as a group may lack imagination and systematically shy away from many of the big issues, they have at least developed some expertise in statistical techniques.[1] They were evidently not involved in designing or critiquing the methodologies employed here.

An exchange between Volker Bornschier, Christopher Chase-Dunn, and Richard Rubinson on the one hand and Robert Jackman on the other serves to illustrate the point. The first three authors reached the conclusion, based on their own study and a review of earlier ones, that foreign investment and foreign aid have negative effects on both economic growth and income distribution. Although their study is a serious one, it suffers from methodological problems that leave its results unpersuasive. Jackman's study reaches different conclusions (that the level of foreign investment does not significantly affect the growth rate of GDP per capita, but the growth of that investment has a positive effect) and is better specified than most of those he criticizes (including Bornshier et al.). It is nonetheless rather far from satisfactory, partly because of unavoidable data deficiencies and partly because of doubtful elements in its own specification of the model and interpretation of results. Similar problems also plague the interesting studies by Erich Weede and Horst Tiefenbach, by Volker Bornschier, and by Edward Muller.

The upshot of all these problems is that the issues addressed in this literature are certainly not settled yet and stand no chance of being resolved until more refined analyses are carried out. At this point, it seems unclear whether the dominant hypotheses linking dependency or capitalist penetration and income distribution will prove valid. The inductive component of their development appears to have been too small to facilitate their being close approximations to real world processes. Still, any judgment at this time is premature. I agree with Fernando Henrique Cardoso that "empirical tests of dependency theory have largely missed the target" because the tests have been ahistorical. Although empirical verification is necessary, one must also recognize that if the phenomena under discussion are complex and involve much historical detail, such testing may border on the impossible and alleged "tests" may be irrelevant. But while I doubt that the more subtle strands of dependency theory will ever be proven or disproven by empirical tests, many other (often narrower) hypotheses could be, if only the level of analytical sophistication could be raised.

Ideology and paradigm have a lot to do with the way individuals think about inequality issues. It is to be hoped that the advance of empirical research will narrow the gap between the world-system or dependency theorists with their focus on factors external to a country and others (among them many economists) who focus more on internal factors. (The distinction between external and internal factors tends to crumble under the microscope but is still useful at a first level of discus-

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sion.) In a volume like Seligson's, with many authors whose theories stress external factors, one is struck by the infrequent discussion of demographic phenomena. Yet any consideration of the changing gap between richer and poorer countries must note these relationships, and if they are not given weight, explain why. During the period 1960–1980, for which the World Bank has well-organized statistics, GNP in the low-income non-Communist countries grew at 0.9 percent per year slower than in the industrialized market economies (3.6 percent to 4.5 percent) and population grew faster by about 1.5 percent (2.4 to 0.9 percent).[2] Thus the majority (60 percent) of the 2.4 percent per year gap in growth of per capita income (1.2 percent versus 3.6 percent) was related to the faster population growth in the low-income countries. Hence any discussion that sets out to explain the widening gap in per capita incomes by focusing on mechanisms that would work through different rates of output growth would appear to be barking up the less important tree. Similarly with intracountry inequality, longer-run analyses that do not include population growth as a possible contributory factor run the risk of frivolity. Unfortunately, including population growth is difficult because the rate of population growth changes only gradually, and its effects on income distribution are probably felt only after considerable lags. But its potential importance is so obvious that not to include it among the major possible determinants of inequality is strange indeed.

A similar anomaly arises in the analysis of external factors, where significant theoretical presumptions could hardly be more contradictory. On one hand is classical economic theory, where the Heckscher-Ohlin model suggests that when poor, heavily populated countries specialize in and export the labor-intensive goods in which they are expected to have an advantage, they will not only grow fast but their income distribution will improve as well.[3] Taiwan, with the best documented postwar history of inequality reduction among all Third World market economies, is often cited in support of this view.[4] On the other hand are the various theories (Marxist, dependency, and similar approaches) whose predictions as to the impact of economic interchange, at least with countries of the metropolis, are much more negative. Where the truth lies remains to be seen; each side can point to cases and mechanisms that lend it credence. But what is depressing is the failure of each literature to take advantage of the other, much less to move toward a serious, professional attempt to integrate major insights from all sides.

While George Beckford's book presents a valuable holistic framework for thinking about inequality and persistent poverty in a specific setting—the plantation society—and Seligson's volume presents a range of hypotheses and a number of meritorious but often flawed empirical analyses, two studies respond to the lack of richly detailed case studies. Miguel Urrutia's *Winners and Losers in Colombia's Economic Growth of the 1970s* focuses on the trends in income distribution in Colombia during the 1970s and the economic mechanisms at work.[5] William Ascher's *Scheming for the Poor: The Politics of Redistribution in Latin America* studies in detail the political processes associated with various reform or redistributive policies in postwar Argentina, Chile, and Peru. Many more such studies are needed.

Urrutia's work demonstrates a skill in digging out, organizing, and interpreting data that is, unhappily, still very rare in this area of research—the mark of the professional. Alternative sources are used to cross-check the validity of the conclusions drawn, data weaknesses are probed. Urrutia marshalls considerable evidence that poverty declined during Colombia's relatively rapid economic growth of the 1970s and that the white-collar middle-class lost ground to both the poorer and the richer families. Whether paid agricultural workers, one of the poorest groups, gained ground as fast as Urrutia claims seems doubtful to me, but this issue is unresolvable at present due to a dearth of empirical evidence.

In any case, studies like Urrutia's and that of Richard Webb and Guy Pfefferman on Brazil[6] leave no doubt that the fruits of growth have reached the poor in Latin America, as indeed they have in many other developing countries.[7] Immiserization of the poor en masse as part of the process of rapid postwar growth in Latin America seems to have been a figment of the social science imagination, even in countries like Brazil, where lack of government concern with the poor has been a continuing feature of policy. At the same time, the benefits reaching the poor have been more on the order of a trickle than a flood, at least in relation to those claimed by the upper groups. Distribution of income has probably changed little from its historically abominable levels during the period of fast growth. With these results looking increasingly firm, two new research frontiers should replace the debate on what happened to overall levels of poverty and inequality under generally rapid growth: first, what has happened during the economic crisis of the 1980s, and second, how policy can affect poverty and inequality. Researchers remain in relative darkness on both of these issues. Moreover, continuing study of the pre-1980 period is necessary to understand why trends were as they were, for example, why some white-collar middle-class groups seem to have been losing ground to both those above them and those below them in the income hierarchy, as reported by Urrutia for Colombia and by other authors for other countries.

To those who do not expect revolution in most Latin American countries, the question of whether, how, and to what extent redistribution can take place through reformist processes is a key issue. A quick

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assessment of attempts at redistribution via reformism in Latin America is not particularly encouraging reading. In the Peruvian case, for example, Webb has argued persuasively that many initiatives, including those of the first Belaúnde regime and the military government of Velasco, have not touched the basic distribution issue between the rural poor and the urban modern sector.[8] Although Argentina under Perón provided a different context, it remains unclear to what extent Perón's maneuvers effected a lasting redistribution.

The gradual buildup of information and understanding of the political economy of income distribution has yielded three insights. First, income inequalities are usually deeply rooted in an economy or society and are therefore difficult to change. Second, while it is not hard to identify correlates of major differences in income distribution across countries (such as land distribution), understanding of how policy variables might affect distribution, especially when their manipulation is restricted within fairly narrow ranges, is seriously inadequate. This lack of comprehension complicates the task of reformers and students of the effects of "reforms." Third, analysis of the political pursuit of redistribution, the degrees of freedom involved, the kinds of coalitions most likely to stand a good chance of achieving something, the ways in which incapacitating reaction may be sidestepped, and similar questions remain in their infancy. Many observers have assumed implicitly that these details matter little, that the potential for reform is determined within narrow bounds by the distribution of power among classes or other groups.

Lack of analyses of the politics of attempts at redistribution has reflected the sort of careless broad-brush approach that flows from a strong presumption on how things happen. This tendency is both unfortunate and ironic: ironic because if, as argued above, even full-time students of these issues find it difficult to know how some policies will affect distribution, it is evident that potential "losers" also will not always be able to see the handwriting on the wall. Reformers who understand these relationships better than threatened interest groups should have some chance of finessing their way to achieving their objectives.

William Ascher's *Scheming for the Poor* takes off from the above presumptions. It is a valuable and enlightening (perhaps even pathbreaking) study of political processes of redistribution, a first step toward serious research into the details of the politics of income distribution. To permit confident mapping of redistributive policies, a comparable understanding of the economic side—how specific policies affect distribution—would be needed. In its absence, and given incomplete understanding of how inequality has changed over time in the countries on which Ascher focuses (Argentina, Chile, and Peru), the presumptions or guesses Ascher makes may be wrong. Nevertheless, his discussion generally proceeds with exemplary caution. He rejects the overdeterministic views that either economic laws or lack of political flexibility preclude anything worthwhile being done to improve distribution, and he argues that the policy-making process, if structured carefully, can take advantage of the way that policy-making personnel, policy-making modes, redistributive instruments, symbol manipulation, and the like can affect success. These points are well made, although it must be noted that Ascher's study does not so much prove that reformers could do a lot as show why this possibility should not be discarded out of hand. Only time will tell how much optimism is indeed warranted.

In summarizing the lessons suggested by the study of these three countries, Ascher emphasizes the choice of instrument, presentation, linkages among instruments, and timing. Thought-provoking conclusions include the idea that "the traditional emphasis on cultivating support even if it tends to mobilize opposition is counterproductive" because "the support of beneficiaries is of limited political value: those who have already benefited from redistribution are not likely to behave in grateful ways, and those who may benefit from contemplated redistributive policies are often incapable of being mobilized sufficiently to help the government vis-à-vis typical opposition tactics. This assessment contrasts with Albert Hirschman's proposition that the reformer generally underestimates the support he can marshall, and overestimates the strength of the opposition.[9] Our cases do not confirm this; on the contrary, both support and opposition can be treacherous" (p. 310). Another valuable observation is that "Allies from among the nonpoor have been essential to every redistributive success we have seen" (p. 311).

The capacity of the reformer to draw a competent political and economic judgment on how to proceed is central to the kind of cautious optimism to which Ascher's work lends some basis. With respect to technical economic expertise, Ascher's optimism seems reasonable as he comments on the tradition of high-quality economists in Chile and the progress in Argentina and Peru as well, observing that "after Allende it will most likely be more difficult for any government expert, no matter what his ideological orientation, to deliberately disregard the constraints he is trained to recognize." But unfolding events often confound reasonable predictions. It is ironic that after the debacle of Allende, a different set of Chilean economists should have made one of the more flamboyant misjudgments of any group of policymakers in Latin America in believing that the "law of one price" would allow them to eradicate domestic inflation via a fixed exchange rate. Perhaps the only thing shared by these technicians of Pinochet's and Allende's governments was overconfidence. The gradual increase in the technical

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sophistication of economists seems to have been more than matched by an increase in self-confidence, especially of the young and inexperienced, rather than being accompanied by the humility and skepticism clearly called for by the historical record. Further, as is well known in developed countries, it is one thing to have the technical expertise in the country and another for the government to draw on it (witness the phenomenon of Reaganomics). Optimism there must be, but caution as well.

The three collections of essays edited respectively by Robert Ferber, Philip Musgrove, and William McGreevey reflect the detailed side of quantitative research on inequality. None claims earthshaking results, but all add their bit to the advance of general understanding.

Analyzing the relationship between family or personal income and a set of microvariables, such as the levels of education and experience and regional location, has a considerable history in economics. Such analyses focus mainly on the proximate sources of inequality, although some also attempt to dig deeper into the origins of earnings differences. In principle, this body of work should help to elucidate the mechanisms through which the effects predicted by wider-ranging theories of inequality operate and should complement that literature. The recent efforts coordinated by ECIEL, including Ferber's *Consumption and Income Distribution in Latin America* and Musgrove's *Ingreso, desigualdad y pobreza in América Latina*, have enhanced understanding of the details, such as how inequality is associated with characteristics of the family head, with size of family, and with source of income (paid work, independent employment, capital, and other). Much of this literature discusses in detail the correlates of inequality—in much more detail than the general reader can be expected to digest, and sometimes the reader finds little help in sorting out the results that really matter from a more general perspective. More work needs to be done to bridge these microeconomic studies and the theories of inequality. In-depth analyses of large data sets like those from ECIEL tend to focus almost exclusively on their own data set. This approach permits useful conclusions about the correlates of income but usually does not reveal much about the existence or souces of inequality trends. Nor are the data apt for analyzing the effects of economic policy, structural features of the economy, and other such aspects.

In *Third World Poverty*, McGreevey reviews issues in measuring development performance in the first chapter. His useful survey goes beyond pure measurement issues to review some of the major analytic questions and how they may be dealt with empirically. Most of the other essays in this volume are useful discussions of research methodology, more directly valuable to the specialist or the researcher than to the lay person. In his essay on employment as an indicator of poverty levels, Henry Bruton starts by asserting that "existing series of employment and unemployment cannot be used as an indicator of changes in the economic conditions of the low income groups in developing countries. Alternatives to unemployment series are available that would serve as more appropriate indicators of improvements in the quality of life of the very poor. . . ." This statement will not be news to specialists in this area but is an important point for wider dissemination. The idea that in low-income countries, the unemployed are generally the poor (with its corollary that the level of unemployment could be a proxy for the economic situation of the poor) has long been contested and is now fairly widely rejected,[10] although neither is it true at the other extreme that unemployment is not a factor in poverty for many individuals.[11] More refined measures distinguishing different settings for unemployment could perhaps contribute to the measurement of poverty. Also, it must be borne in mind that many Latin countries are now semi-industrialized, so one would expect their openly unemployed to be closer to the lower end of the income distribution than is true in the poorer LDCs. In other words, one would expect the Latin countries to be moving toward the industrial country model, where open unemployment is increasingly sensitive to economic cycles and disproportionately affects the poor. The recent recessions in countries like Brazil, Chile, and Venezuela seem, in their impacts if not their causes, to be akin to industrial country recessions, and it appears that the poor are major victims.

Even a modest understanding of the determinants of inequality and poverty, and of the political processes associated with poverty-related policies, is an ambitious goal. Social scientists have thus far not made much progress along this path, but encouraging signs have emerged: detailed, serious analyses of the political process like Ascher's; impressive exposition of frameworks like Beckford's; competent case studies like Urrutia's; the move toward methodologically sounder empirical tests of some of the theories; and the many relatively narrow, but useful, studies of specific parts of the puzzle. These works are evidence of progress, which with luck will mean that a decade hence careful students will not have to be agnostic on so many issues as they now must be. But achievement of even this modest-sounding goal will require that a higher share of studies fall in one or another of the just-cited categories than has been true in the past.

NOTES

1. A methodological lag suffered by all the empirical studies of the determinants of income distribution in Seligson's collection is the failure to move to simultaneous equations models, now fairly standard in economics. Such models offer advantages when the mechanisms under discussion involve sequences of effects rather than a set of determinants that all impact directly on the dependent variable. Thus when it

is well known that such variables as land distribution, distribution of education, and a few others are direct or proximate determinants of inequality, but that other more systemic variables (such as those used in much of the literature reviewed here) underlie them, a simultaneous equations approach is desirable.

2. Based on figures from the World Bank, *World Development Report 1982,* 110–13. Note that the middle-income developing countries were gaining on the industrial ones.
3. A recent study linking trade patterns and income distribution is Gary Fields, "Employment, Income Distribution, and Economic Growth in Several Small Open Economies," *Economic Journal* 94 (Mar. 1984):74–83.
4. The Taiwanese experience is reviewed by Shirley W. Y. Kuo, *The Taiwan Economy in Transition* (Boulder, Colo.: Westview, 1983). The link between Taiwan's outward orientation and the level of inequality is analyzed in greatest detail in John C. H. Fei, Gustav Ranis, and Shirley W. Y. Kuo, *Growth with Equity: The Taiwan Case* (Oxford: Oxford University Press, 1979).
5. Miguel Urrutia, *Winners and Losers in Colombia's Economic Growth of the 1970s* (Oxford: Oxford University Press, 1985).
6. Guy Pfefferman and Richard Webb, "Poverty and Income Distribution in Brazil," *Review of Income and Wealth,* series 29, no. 2 (June 1983):101–24.
7. For a useful recent review, see Gerald Meier, *Emerging from Poverty: The Economics That Really Matters* (Oxford: Oxford University Press, 1984).
8. Richard Webb, *Government Policy and the Distribution of Income in Peru* (Cambridge, Mass.: Harvard University Press, 1977).
9. The reference is to Albert Hirschman, *Journeys toward Progress* (New York: Anchor, 1963).
10. See Alan Udall and Stuart Sinclair, "The Luxury Unemployment Hypothesis: A Review of Recent Evidence," *World Development* 10 (1982):49–62.
11. See, for example, Rakesh Mohan and Nancy Hartline, *The Poor of Bogotá: Who They Are, What They Do, Where They Live,* World Bank Staff Working Paper no. 635 (Washington, D.C.: World Bank, 1984).