Review Articles

INEQUALITY, GROWTH, AND DEMOCRACY

By DIMITRI LANDA and ETHAN B. KAPSTEIN*


I. INTRODUCTION

THE elaboration of endogenous growth theory over the last decade has resulted in an explosion of innovative research on the political economy of economic development and reform. Although the initial articulation of this theory focused on technological innovation and its sources as the primary determinants of economic growth, its most basic theoretical insight—that growth can be both explained and controlled—has spawned an ambitious research program aimed at understanding the relationship between growth and such distinctly political factors as the policy of economic openness, income and wealth inequality, and redistributive policy, among others. The goal of this review is to assess the implications of this research for one of the key theoretical challenges confronting modern political science: articulating the causal relationship between economic development and democracy.

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Contemporary analysis of the political determinants of growth has been advanced along two paths: in cross-section time-series studies of the determinants of economic growth, sometimes referred to as “Barro regressions,” after the pioneering work by Robert Barro that forms the core of his *Determinants of Economic Growth*, and in political economy models that explore the causal linkages between long-run economic performance and features of government policy and institutions.\(^1\) Taken as a whole, the results of these studies, buttressed by empirical evidence of the political path dependence of the economic well-being of transition economies,\(^2\) make clear that any convincing explanation of long-run economic performance requires an integrated account of the role of domestic political institutions.

But the political lesson of economic development is deeper than just that “politics matters.” If it matters at all, it is because citizens respond to economic conditions politically as well as economically; and given the dependence of political outcomes on underlying political institutions, there is no prima facie reason why their response must necessarily take place within the confines of the institutional status quo.\(^3\) Whatever else it is, the political lesson of economic development, then, is one of the consequential importance of the comprehensive exercise of the political agency of citizens—of their interests and capabilities as purposeful political actors.

To account for the political consequences of economic development and change, we must inquire not only into the economic effects of existing political institutions but also into whether these institutions are ultimately sustainable under given economic conditions and if not, into what other political mechanisms or institutions should be expected to arise or persist in their stead. The works we discuss in this article and

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the larger literature to which they belong contribute to such an inquiry both directly—by identifying the relevant causal relationships and offering empirical evidence—and indirectly—by providing a platform for raising questions heretofore left unaddressed.

Unlike the neoclassical political models of Seymour Martin Lipset and Samuel Huntington—models that were optimistic in their underlying analysis of the relationship between economic growth and political democratization—these works also direct us to a less determinate and, arguably, more politically nuanced stance on reform policies promoting economic development. The basic growth facts of the last decade underscore the existence of the underlying complexity. For example, as the IADB report documents (chap. 1), the fast pace of recent economic growth in Latin America has been accompanied by substantial increases in economic inequality. Philippe Aghion and Jeffrey Williamson, Dani Rodrik, and contributors to the Tanzi and Chu volume posit that economic openness—one of the most widely recognized factors contributing to economic growth and development—has led to increases in inequality in Western industrialized democracies, Latin America, and, possibly, Africa and to rising economic volatility in East Asia. The political consequences of growth with inequality—a classic theme in political economy—are hardly straightforward and are likely to depend on the capacity of domestic institutions to carry out redistributive measures. That capacity, generally assumed to exist by the economics literature, is often lacking in practice, especially in the nascent democracies of the developing world, and it is at the heart of much of the political-economic conflict in these countries. Examining the feasibility of redistributive policy is one of the central contributions of this review article.

Is economic openness responsible for the increase in economic inequality in the last two decades? What are the causal connections between inequality and economic growth, and what are their consequences for the stability and consolidation of democratic regimes? Why are some democracies more capable than others of adapting to the challenges posed by economic change? What sorts of policy and institutional adaptations should democratic governments pursue to minimize the negative economic and political consequences of economic shocks that affect the distribution of income and wealth?

We discuss the answers to these questions offered by the works under review in the next three sections. We begin in Section II with a reappraisal of the relationship between democracy and growth and economic development that provides a distinct political motivation for the
analysis of the causes and consequences of inequality and growth. In Section III we turn to the consideration of the distributive effects of free trade and economic openness, which have been among the key motivations behind the extensive recent work on the effects of income and wealth distribution on growth, including four of the five works under review. Section IV examines the main arguments in that body of work, focusing on the extent to which they cohere with the consequences of the exercise of political agency for the structure and content of political and economic institutions. This examination suggests the importance of supplementing these arguments with a systematic inquiry into the political feasibility of redistributive policies. Section V discusses the key components of such an inquiry and sketches some promising approaches to it. Finally, Section VI offers a brief redux.

II. GROWTH AND DEMOCRACY

Perhaps one of the most robust and enduring empirical findings of post–World War II social science is the existence of a strong positive relationship between economic development and democratization.4 Originating in Lipset’s observation of an “income threshold” for democracy, this finding receives unequivocal support in chapter 2 of Barro’s book. Drawing on the normalized Freedom House measure of democracy as political rights,5 Barro shows that GDP per capita and life expectancy are highly significant predictors of democracy and civil liberties, “firmly establish[ing] the general link between democracy and the standard of living” (p. 67).6

Although both this finding and Barro’s empirical analysis are quite convincing, the controversial aspects of the relationship between democracy and economic development become apparent as soon as one probes the nature of the underlying causal mechanism. Barro hardly overstates the problem when he asserts that “theoretical models of the effects of prosperity on democracy are not well developed” (p. 52). Although, in light of the amount of research devoted to the investigation

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6 This finding is, of course, one of the best established empirical generalizations in political science and sociology; for a recent reformulation, see Adam Przeworski and Fernando Limongi, “Modernization: Theories and Facts,” *World Politics* 49 (January 1997).
of this relationship, the weakness of its theoretical foundations may be surprising, it is emblematic of the more global deficit in the analysis of the causes and mechanisms of institutional change—a deficit that political scientists have lamented for the last two decades.\(^7\)

The importance of articulating a convincing causal explanation of the effects of economic development on democracy, however, has a distinctly political rationale, as well as a scholarly one. Advancing the cause of democratization often requires not only immediate political pressure from domestic (and external) agents but also knowledge of the mechanisms by which democracy can be strengthened in less direct but, possibly, no less effective ways. The status of economic growth and development as a potential object of universal policy consensus recommends the promotion of these goals as a particularly promising approach to democratization. However, the weakness of the causal analysis of the relationship between development and democracy detracts from the possibility of targeting its critical “moving parts” and hence from accelerating the process of democratization.

Although Barro does little to develop a causal theoretical analysis of the relationship between democracy and economic prosperity or to offer interpretations of his findings that could guide a theoretical inquiry, his findings are nonetheless suggestive and provocative. The explanation of the relationship between economic development and democracy at the heart of his analysis is a version of modernization theory, the key thesis of which links these concepts via “sequences of industrialization, urbanization, education, communication, mobilization, and political incorporation, among innumerable others: a progressive accumulation of social changes that ready a society to proceed to its culmination, democratization.”\(^8\) However, Barro’s evidence also bears closely on another class of mechanisms (reliant on the political effects of economic inequality), offering important implications we consider below.

Tracing its conceptual lineage to what we now refer to as the Lipset hypothesis, Barro’s version of the modernization thesis stresses the role of education and, to a lesser extent, urbanization. According to Lipset’s Aristotelian argument, democracy requires intelligent participation in public affairs and what Lipset calls a “receptivity to democratic political tolerance norms,”\(^9\) which, in turn, call for a society with sufficient eco-

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\(^7\) See, for example, William H. Riker, “Implications from the Disequilibrium of Majority Rule for the Study of Institutions,” *American Political Science Review* 74 (June 1980).

\(^8\) Przeworski and Limongi (fn. 6), 158.

nomic resources to educate the masses. Widespread education thus emerges as a necessary condition for democracy, with wealth and (in Lipset’s version of the argument) an enlarged middle class the means for creating expanded educational opportunities.

In the article version of chapter 2 of his book Barro suggests another interpretation of the emergence of democratic values, one that emphasizes the role of wealth in the formation of individual needs:

With respect to the effects of economic development on democracy, the analysis shows that improvements in the standard of living—measured by a country’s real per capita GDP, infant mortality rate, and male and female primary school attainment—substantially raise the probability that political institutions will become more democratic over time. Hence, political freedom emerges as a sort of luxury good.10

Implicit in this position, which has strong parallels in the economic analyses of the emergence of environmental standards and labor rights, is the view of polities as consisting of consumers with hierarchies of needs. With rising incomes, consumers cum citizens become more willing—and more able—to supplement the necessities of life with such luxury goods as clean air, better working conditions, and democratic governance.11

Unlike the relatively plausible first (Aristotelian) interpretation, the assertion of the consumer hierarchy of preferences is causally dubious. As Amartya Sen’s classic argument,12 echoed by Rodrik in chapter 4 of his book under review, maintains, because of their effects on the transparency of government and the elimination of corruption, effective democracy and political freedom may be significant contributors to the alleviation of economic crises and to the increase in economic welfare and may thus be preferred by citizens as a means of achieving the satisfaction of their critical material needs.13 Unlike the education-based theory of the origin of democratic values, which has natural referents in the measures of educational attainment, the view of political freedom or democracy as a luxury good is also considerably more difficult to test

13 Note that this claim and Sen’s argument are distinct from the assertion that democracy always promotes economic growth. We leave the latter assertion without consideration in this review. For an influential review of the empirical evidence, see Adam Przeworski and Fernando Limongi, “Political Regimes and Economic Growth,” Journal of Economic Perspectives 7 (Summer 1993).
directly, and, despite invoking it, Barro does not offer any independent empirical assessment of it.

Meanwhile, the test of the education-based theory generates mixed, but generally disappointing, results. While primary education turns out to be a significant predictor of democratization (p. 70), secondary and higher schooling are not. Given that the latter are much more significant predictors of growth (pp. 19–21), these findings indicate that education provides at best a very partial explanation of the relationship between economic development and democracy.

Even less successful in explaining this relationship is another proxy of the modernization argument—the rate of urbanization. Although the simple correlation between democracy and urbanization is “strongly positive” (p. 63), once the standard of living is held constant, urbanization fails the significance test in the democracy regression. In sum, Barro’s results offer little support for the existence of intervening “modernization” variables in the causal chains from economic development to democracy; if these variables matter for democracy, their effect, with the exception of primary schooling, appears to be fully captured by the measures of the standard of living.

A similar conclusion is suggested by another result, which concerns a class of causal mechanisms that explains the relationship between economic development and democracy in terms of the impact of economic development on economic inequality. Among these mechanisms are Tocqueville’s argument that expanding civic organizations serves as a check on dictatorship and as a way of inculcating values and skills necessary for democratic governance (extended by Lipset and Robert Putnam, respectively), as well as the argument by Rueschemeyer et al. that capitalist development changes the distribution of power between the landlord and the urban working classes. Adding Gini coefficients measuring inequality and years of schooling to his regression model for democracy, Barro finds neither to be statistically significant (p. 69). However, the

16 Barro mentions both of these arguments (pp. 51–52) in passing without further analysis.
17 These findings are at odds with the results in Edward Muller, “Economic Determinants of Democracy,” in Manus I. Midlarsky, ed., Inequality, Democracy and Economic Development (Cambridge: Cambridge University Press, 1997). Barro’s results are more convincing, however, because of his more sophisticated estimation technique and the use of the second inequality indicator, which bolsters the plausibility of this result in the face of objections concerning the quality of data and the crudeness of the monetary measures of inequality. His results are confirmed by Posner (fn. 11), who uses a country sample that is larger than Muller’s.
possibility that inequality is simply irrelevant for democracy, though nominally consistent with this finding, flies in the face of the conjunction of highly significant relationships between economic development and democracy and between inequality and growth that are among the most robust empirical findings in the recent political economy literature. The same evidence, then, lends plausibility to the remaining possibility—that the effects of inequality on democracy are fully captured by other variables in the democracy regression, the most causally reasonable of which is GDP.

What are the implications of this conclusion for the relationship between economic development and democracy? Perhaps most importantly, the direction of causality from higher economic development to lower inequality to stronger democracy is empirically suspect. The fact that the introduction of the inequality measure does not affect the magnitude and significance of the GDP coefficient (p. 69), combined with statistical insignificance of that measure, suggests that democracy is affected by growth but not via inequality. This conclusion is supported by the emerging empirical consensus that fails to corroborate the evidence reported in the 1970s and 1980s that growth lowers inequality. As Michael Bruno, Martin Ravallion, and Lyn Squire show in their contribution to Tanzi and Chu, once country-level effects are taken into account, “there does not appear any systematic tendency for [income] distribution to improve or worsen with growth” (p. 127). Thus, like the effect of postprimary education and urbanization, the effect of inequality on democracy must go through growth (rather than vice versa), or these effects must be fully confounded with the effect these variables have on growth; that is, whenever they affect democracy, they also affect growth. This last possibility, in particular, may hold one of the keys to the elusive explanation of the relationship between economic development and democracy.

18 The benchmark studies in the literature on the relationship between inequality and growth include Alesina and Rodrik (fn. 1) and Persson and Tabellini (fn. 1). Roberto Perotti, “Growth, Income Distribution, and Democracy: What the Data Say,” *Journal of Economic Growth* 1 (June 1996) and Benabou (fn. 1) present comprehensive discussions of the available cross-country regressions. More recent work indicates that the relationship between inequality and growth tends to be more negative for the poor countries than for the rich ones; see, e.g., Robert Barro, “Inequality and Growth in a Panel of Countries,” *Journal of Economic Growth* 5 (March 2000).


20 These results are obtained using an improved data set on inequality, introduced in Klaus Deininger and Lyn Squire, “A New Data Set for Measuring Income Inequality” *World Bank Economic Review* 10 (September 1996).

21 We return to this possibility in the penultimate section of the review.
Suggesting that we look to the growth effects of inequality and distribution-affecting policies for critical mediation, these implications motivate the progression of the argument considered in this review. They also suggest a methodological guideline: that explanations of the relationship between economic development and democracy that rely on the causal role of the distributive mechanism must, as a theoretical test of their plausibility, be able to give a systematic account of the concomitant effects of this mechanism on growth.

III. TRADE AND INEQUALITY

If the empirical evidence reviewed above suggests the explanatory importance of the effects of inequality on growth in developing a systematic account of the determinants of democracy, the arguments we consider next help explain why the examination of these effects has, as will be seen in Section IV, revolved largely around the impact of redistributive policy. Both classes of policy instruments affecting inequality—policies aimed, respectively, at the elimination of its causes and at the alleviation of its consequences—are likely to have an impact that extends well beyond it. Just as direct redistribution may reduce the incentives to engage in productive activities as it alleviates poverty, the causes of inequality may contribute to social welfare even as they detract from it. The prima facie desirability of particular policies that attempt to control inequality must depend, then, on the outcome of an inquiry into the nature of such causes.

That the political economy of trade and globalization has played a prominent role in such an inquiry is not surprising. International trade theory predicts the domestic pattern of winners and losers that will follow trade liberalization. This allows one to examine the political economy of globalization from a distributive standpoint and raises the possibility that trade is at least partly responsible for the surge in inequality observed in the 1980s and 1990s. Moreover, in comparison with such factors as technical or organizational change, economic opening comes close to being the result of explicit policy decisions by the government, making it potentially reversible in response to reappraisals of its consequences and political demands.

But trade also makes what is, perhaps, the least ambiguously positive contribution to aggregate welfare (and, hence, if it is one of the causes

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of inequality, it is also one of the most complex challenges to economic policy-making). The long-run economic consequences of opening a country to trade and investment are, at least in theory, straightforward and uncontroversial: as a positive theory, international economics demonstrates that freeing trade and capital flows leads to an efficient allocation of a nation’s (and ultimately the world’s) scarce resources, resulting in more output and consumption than would be the case under protectionism. Openness should produce dynamic growth gains. For this reason, both policymakers and academic economists have considered trade liberalization a sine qua non of economic reform. But the unabashed utilitarianism of classical trade theory provides little practical guidance with respect to how this wealth ought to be distributed both within and between countries, and the once popular view that “if measures making for efficiency are to have a fair chance, it is extremely desirable that they should be freed from distributive complications as much as possible” has proven to be a poor guide to positive analysis and policy advice. Not only do the “distributive complications” actively impinge on and mold economic outcomes with the interests, logic, and timing distinct from those of economic efficiency, but, as some of the arguments surveyed in Section IV show, efficiency may sometimes be best served precisely by adopting a policy of economic redistribution.

At the heart of the analysis of the distributive effects of measures promoting growth is the “Kuznets hypothesis,” which holds that income inequality rises in the early stages of industrialization, as the productivity gap between the old and new sectors of the economy widens. Over time, its proponents argue, this increase in income inequality is reversed, as the returns to the factors of production naturally tend to-
ward convergence. The relationship between income inequality and growth could thus be represented by an inverted U-shaped curve. As with many other issues in the “old political economy” literature, this analysis allows a marginal role at best for policymakers in influencing income distribution.

As we already noted, however, a major empirical observation behind much of the new political economy literature is that of rising and persistent income inequality both in the old industrial states that constitute the OECD and in developing countries around the world. Reinforcing Bruno, Ravallion, and Squire’s direct regression results on the distributive effects of growth, these findings cast considerable doubt upon the general validity of the Kuznets model. In particular, it appears that the relative demand for skilled labor is rising while the demand for unskilled labor continues to fall, increasing the wage gap between these two factors. The causes behind these shifts in demand and in factor returns are at the center of the political economy debates over the role of globalization, technological change, and other forces in shaping labor market outcomes. And although the Kuznets curve is “the central bridge that spans theory and history over these debates, . . . what we need,” as Aghion and Williamson point out, “is less effort at establishing the Kuznets Curve as a stylized fact, and more effort at uncovering the sources of inequality change. . . . The big three commonly put forward to explain the recent inequality surge are trade, technology and labor supply” (pp. 1–3).

That trade and economic opening more generally could have distributive effects is, of course, no surprise. There are three major bodies of economic theory that hypothesize a relationship between openness and inequality: the factor price equalization (FPE) theorem arising out of the Hecksher-Ohlin-Samuelson (HOS) framework; skill-enhancing technology (SET) theories, which examine the effects of technological change (including technological change brought about by cross-national technology transfers) on factor returns; and kaleidoscopic comparative advantage theory, which examines the effects of economic interdependence on labor markets. At issue, above all, is the extent to which actual inequality can be attributed to economic openness as opposed to the other “villains” mentioned by Aghion and Williamson and which of the three theories mentioned offers the most causally relevant mechanism for such an attribution. We examine the treatment of this issue in the works under review by considering each of these theories in turn.26

26 For a useful review of the literature, see William Cline, Trade and Income Distribution (Washington, D.C.: Institute for International Economics, 1997); for a review essay on the recent work on this
According to the FPE theorem first developed by Paul Samuelson in the 1940s, two economies that adopt a policy of free trade will find (given certain strong assumptions) that the returns to the factors of production in each country will tend toward equalization at some intermediate point. This work built on an earlier theorem, developed by Samuelson and Wolfgang Stolper, which asserts that if protection were used to raise the domestic price of imported goods, lifting it and thereby decreasing the prices of these goods would increase returns to the foreign factor used intensively in its production and lower returns to the relatively less abundant and more expensive domestic factor (in the case of the Western industrialized countries—unskilled labor). In order to analyze the effects of openness on the behavior of wages in the context of this theory, trade theorists who use HOS models therefore naturally seek their evidence in international price movements. Considering such evidence in the terms of trade data for the United States in the late 1980s, Jagdish Bhagwati argues, in his contribution to Tanzi and Chu, that it shows “a slight rise in the relative prices of imports. . . . In short, the necessary empirical evidence on price behavior during the 1980s for the absolutely critical element in this particular trade explanation is exceptionally weak at best” (pp. 264–65).

But the inference that trade is irrelevant for accounting for the decrease in Western unskilled labor wages in the 1980s and 1990s, even in the context of this trade explanation, may be too quick. As Aghion argues in Aghion and Williamson:

The above argument relies on the assumption that traded goods are primarily final goods. . . . If, on the other hand, traded goods were mainly inputs into further production, the implications would be very different. A lower price for intermediaries would shift the demands for other inputs. . . . When unskilled labor is more substitutable for physical inputs than skilled labor is, cheap physical inputs increase the relative demand for skilled workers. (p. 43)

Recent studies of the shifts in the intraindustry demand for intermediate goods cited by Aghion lend substantial support to this argument. These findings have potentially significant consequences for the dynamics of human capital stock and consequently for the growth rates of nations. As Aghion reminds us (p. 55), “Endogenous growth theory tells us that educated labor is precisely what generates technological change.” The labor supply response to the relative decrease in the de-

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27Cline (fn.), 40–41.
mand for skilled labor in the unskilled labor-rich, less developed countries and the relative increase in such demand in the skill-abundant wealthy industrialized countries would then have the effect of increasing the growth rate in the latter countries and decreasing it in the former. Thus, instead of promoting convergence, globalization could sharpen the divide between rich and poor countries, while the unchecked negative effects of labor supply decisions on long-term growth in poor countries may dampen the benefits of globalization for their democratic consolidation.

Although it is able to explain the increasing skill differentials in wages in industrialized countries, Aghion’s innovative argument fails to explain the second empirical puzzle faced by the FPE models: the occurrence of the same phenomenon in the less developed countries. Just such an explanation is supplied by the “skill-enhancing technology” (SET) analysis, which has been gaining prominence among many contemporary students of the trade-inequality relationship.

Rather than considering the effects of trade on price movements, the SET analysis focuses on how cross-border transfers of SET may affect factor returns. One of the traditionally lauded effects of opening less developed economies to the world market has been the transfer to such countries of new and more efficient production technologies. These technologies, however, are likely to have a “skill bias,” disproportionally rewarding those who possess the skills necessary for production with them. The evidence from in-depth studies of nine South American and Southeast Asian countries reported in a pioneering paper by Donald Robbins suggests that technology transfers between North and South are indeed positively correlated with increased wage dispersion among workers in the receiving country and with the decline in the relative wages of the unskilled. As technologies flow to the South, labor markets become increasingly segmented into winners and losers.

These findings suggest a causal picture of income distribution in the South that is distinct from Aghion’s. Whereas the increased reliance of the North on the manufacture of intermediate goods in the South encourages the demand for, and hence the supply of, unskilled labor in the South, the SET findings indicate the presence of incentives that, all else being equal, should produce the opposite effect on labor supply: the ac-

28 In fact, Aghion’s explanation predicts precisely opposite effect in these countries—the shrinking skill differentials.
quisition of skills and a relative decrease in the supply of unskilled labor. Of course, the two technological changes could be taking place side by side, if not be part of the same process. But then there is a puzzle with respect to the net effects of these changes: the prima facie unexpected combination of rising skill differentials in the South (predicted by SET) and of the evidence (invoked by Aghion) of dramatic decreases, in response to openness, in the (relative to the world average) proportion of population pursuing secondary and tertiary education in countries with low initial stocks of skilled labor.30

The key to resolving this puzzle may lie in the ability of the unskilled to respond to the changes in their incentives. The development of capital markets and of opportunities for economic mobility more generally is critical in facilitating the response of the supply of labor to the demand-side incentives; where the opportunity to acquire new skills is highly constrained, the willingness to do so may prove inconsequential. As Nancy Birdsall emphasizes in her contribution to Tanzi and Chu, endorsing Robbins’s findings, unlike “the rich” who “are educated, have financial and physical assets, and access to capital markets . . . [and] are thus able to exploit the new opportunities that liberalization of trade and investment brings, [t]he poor in Latin America are increasingly concentrated in urban areas in low wage and unskilled jobs . . . . They benefit little, at least in the short run, from the opening of economies” (p. 293).31 By increasing the skill premium in the receiving economies, SET transfers make the constraints imposed by poor capital markets increasingly binding, preventing the more and more willing from borrowing the funds or saddling them with prohibitive opportunity costs necessary to finance education. What looked before like a puzzling conjunction of empirical findings would, then, suggest that the positive effects of economic openness on income distribution in the South (which are, in Aghion’s analysis, a consequence of the dynamics of the demand for intermediate goods) have been swamped by the negative effects on that distribution of the combination of SET transfers to the South and its woefully underdeveloped capital markets.

31 Barro (fn. 18) adopts this interpretation in presenting his regression results, which show a robust and significant positive relationship between openness and inequality. They contradict those in François Bourguignon and Christian Morrisson, “Income Distribution, Development and Foreign Trade: A Cross-Sectional Analysis,” *European Economic Review* 34 (September 1990); using older and lower quality data, Bourguignon and Morrisson find that increase in degree of protection increases income inequality.
Jagdish Bhagwati’s chapter in Tanzi and Chu articulates (although stops short of defending) still a third hypothesis regarding the effect of greater openness on wages. It is based on what Bhagwati calls “kaleidoscopic comparative advantage”: the increased volatility of industries’ narrowing comparative advantage caused by comprehensive economic integration. This development induces a degree of “footlooseness” on the part of employers, leading to “increased labor turnover [between industries which] could flatten the growth profile of earnings due to less skill accumulation,” thereby enlarging the gap between the earnings of the relatively more transferable skilled and less transferable unskilled labor (p. 277). Significantly, increasing capital mobility is sufficiently powerful to induce its own independent effect on the wage distribution, since it makes it more credible for employers to threaten to leave, thus putting further pressure on the wages of unskilled labor.

As Bhagwati notes, although the present evidence in relation to the “kaleidoscopic comparative advantage” argument is tentative, the set of effects posited by this argument “remains politically salient because the linkage seems overwhelmingly intuitive.” The outcome of this salience is the very visible public push for what he argues should be seen as a refined form of nontariff trade barriers—policies that increase the cost of production in the poor countries by imposing high-cost labor standards, requirements of intellectual property protection, environmental demands, and so on (p. 281).

Williamson’s essay in Aghion and Williamson reinforces the possibility of a distribution-motivated backlash against free trade with a historical argument that emphasizes the role of a little-considered aspect of globalization—mass migration. Williamson shows that in the late-nineteenth-century Atlantic economies, “where immigration increased the receiving country’s labor supply greatly, inequality rose sharply; where emigration reduced the sending country’s labor supply markedly, inequality declined” (p. 181). As the impact of immigration was felt on domestic wages, not just in the United States but in Argentina, Australia, Brazil, and Canada as well, immigration restrictions became increasingly common culminating in “a late nineteenth-century globalization backlash [that] made a powerful contribution to interwar de-globalization” (p. 193). Although the present conditions differ from those of the last century with respect to both the freedom of migration and the extent of international institutionalization of free trade, Williamson argues that “history does supply a warning: there is an endogenous globalization backlash in our past that could reappear in our future” (p. 193). Defusing the tensions that could ignite an antiglobal-
ization movement, however, might require the sorts of redistributive policy measures—both within and between nations—that may be particularly difficult to implement in a global economy with substantial capital mobility.\footnote{For an elaboration of this point, see Ethan Kapstein, Shari\ng the Wealth: Workers and the World Economy (New York: W. W. Norton, 1999).}

The consideration of the theories of the distributive effects of free trade suggests the following conclusions. First, regardless of whether trade is actually responsible for increases in inequality in the industrialized countries in the 1980s and 1990s, the popular public expectations of distributive consequences are sufficient to generate policy pressures that diminish the positive effects of globalization on growth and development in the poor countries. Arguably, they have done so.\footnote{Examples would include the tariff escalation in the North, which inhibits investment in value-added export industries in the South.} Given the effects of growth on the strength of democracy discussed in the previous section, this outcome would appear to have distinctly undesirable political consequences, raising the premium on the ability of governments to establish economically and politically effective programs of social insurance that inoculate against the antiglobalization pressures.

Second, although theoretical disagreements persist, the argument that free trade is responsible for widening the gap between rich and poor in less developed countries should not be ignored: independent empirical support for the explanation focusing on the interaction between trade and technological change appears to withstand the challenges of its critics. Another, more localized explanation for the negative effects of globalization on distribution, suggested in the IADB report in relation to Latin America, also survives: as trade-barriers lower, natural resources are among the few domestic commodities that can successfully compete on the international market in the short term, skewing the distribution of income and wealth in the direction of their owners, who in the Latin America constitute a very small proportion of the population (p. 70).

As the discussion in the next section indicates, to the extent that globalization is responsible for increasing inequality, its benefits for economic growth and democratic consolidation may be considerably dampened. But advocating the reversal of globalization is surely throwing the baby out with the bath water. Rather, the most important implication of the arguments attributing the increase in economic inequality to openness is that economic policy has good reasons to be aiming at the consequences of inequality, rather than at its causes.
IV. INEQUALITY AND GROWTH

In the chapter that sets the normative stage for the IADB report, the authors write: “The prevailing conclusion in recent empirical studies is that poor income distribution harms economic growth and that therefore, instead of the conflict between equality and development that used to be regarded as a serious constraint on distribution policies, the relationship is mutually reinforcing” (p. 23). Although this statement correctly identifies the present consensus that economic inequality is bad for growth, the academic literature presents a causal account and policy advice that are considerably more nuanced.

To begin with, despite the traditional rhetoric of an “equality-efficiency trade-off,” what is really at stake in the inequality debate is the conflict between redistributive policies and growth, and not the conflict between growth and equality as such. In particular, the classic arguments focused on two effects of redistributive policies. Such policies were seen to generate incentive problems for net transfer recipients, dampening their productivity and thus their economic contribution to society. At another level, in societies lacking well-developed capital markets, the tax measures associated with redistributive policies were thought to jeopardize savings and hence production that requires high-cost “lumpy” investments in capital goods, further undermining growth. Both of these effects are direct consequences of redistribution, not of economic inequality per se; and thus it is not so much that the alleged conflict between equality and growth would hinder redistribution as that a conflict between redistribution and growth, if it exists, would hinder the promotion of equality. The difference is significant, since what is easily one of the most influential present-day arguments regarding the effects of inequality on growth maintains that inequality is bad for growth precisely because redistribution is. Moreover, what has taken place in the last decade or so from the standpoint of developing a systematic theory of growth is not so much a reversal of the old view that redistribution hinders growth as the growing recognition that various causal mechanisms linking redistribution and growth can produce contradictory effects. In short, while there appears to be little dis-

34 Similar statements can be found in Aghion (see, in particular, pp. 7, 11) and Tanzi and Chu (see, for example, the introduction and the chapter by Michel Camdessus).
35 Indeed, even at the height of its prominence, the view that there is a necessary “conflict between equality and growth” was a mischaracterization of the theorized relationship between redistribution and growth.
36 See Persson and Tabellini (fn. 1); and Alesina and Rodrik (fn. 1).
sent from the position that inequality is bad for growth,\(^{37}\) appreciation of the nature of the relationship between these phenomena requires the consideration of the effects of redistributive policies.

The central critique of this literature, which we develop in the present and following sections, is that, to the detriment of our knowledge and policy advice, the effects of redistribution that have been considered have been almost exclusively the economic ones. A systematic political agency–based analysis of the interactions between redistributive policies aimed at promoting economic equality and the political ramifications of such policies is critical for determining not only the feasibility of such policies but also their comprehensive political and economic effects. It may also be instrumental in bridging the gap between the theoretical analysis of redistribution and the existing empirical evidence.

We begin by considering the causal mechanisms that support the assertion that economic inequality is bad for growth. The works under review suggest two distinct classes of such mechanisms.\(^{38}\) The first explains the negative effects of inequality on growth in terms of the political responses to inequality in the relevant politicoeconomic contexts. The most prominent mechanisms in this class—embodied in the “endogenous fiscal policy” and the “sociopolitical instability” models of inequality and growth—are a point of departure for several papers in Tanzi and Chu and constitute a foil for some of the central arguments advanced in Aghion’s essay in Aghion and Williamson. The second class of mechanisms, developed in detail in that essay, relies on distinctly economic responses to demonstrate that redistributive policies aimed at the alleviation of inequality could have positive instead of negative growth effects. By implication, then, failing to implement redistribution in the relevant economic environments is tantamount to letting inequality dampen growth.

The endogenous fiscal policy model of inequality and growth developed in the papers by Persson and Tabellini and by Alesina and Rodrik introduces the endogenous taxation and redistribution choice in a dynamic model of endogenous economic growth. The median-voter theorem, which establishes a key background result, assures the existence

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\(^{37}\) Among the exceptions is Posner (fn. 11), who argues that the variation in the average income fully accounts for the effects of the distribution of income on growth.

\(^{38}\) There are, of course, other mechanisms in the literature as well, but the four we discuss in this section have been the focal points of the literature in the past decade and are of particular consequence for the works under review.
of a unique tax/transfer equilibrium corresponding to the policy most preferred by the median voter when the preferences of all voters can be represented as single-peaked over the same unidimensional tax-policy space. An implication of the determination of the tax-transfer policy by the optimal choice of the median voter is that greater inequality increases redistribution away from the well-off and thus adversely affects their savings decisions. Since it is the well-off who bear the weight of investment in this economy, the lower savings rates lead to slower growth. Less formalized versions of this argument include the association of lower growth with the high policy volatility that is caused by political business cycles as well as the corollary of the infamous “race to the bottom”: populist demands for increasing wages make countries less attractive to investors, leading to lower growth rates (Iglesias, in Tanzi and Chu, 14–15).

In a second prominent model of inequality and growth, that of Alesina and Perotti, economic inequality is posited to lead directly to sociopolitical instability, including threats of coups and grass-roots revolts. The reasons for this effect, at least in the case of the latter, may include the frustrated expectations of fairness (as Michel Camdessus suggests in Tanzi and Chu, 2) but also, and perhaps more plausibly, the revolting parties’ self-identification with the bottom of the socioeconomic pyramid and with low expectations of social mobility. Correspondingly, the manifestations of political instability associated with the actions of the net losers from redistributive policies (for example, attempted or successful coups) may be caused by threatened or actual expropriation, which is likely to increase with greater inequality. The resulting instability, then, creates an environment unfriendly to both savings and investment that, in turn, lowers growth rates.

39 The somewhat distinct versions of this model were introduced in the papers by Persson and Tabellini (fn. 1) and Alesina and Rodrik (fn. 1). The papers that pioneered the median-voter approach to redistribution are Kevin W. S. Roberts, “Voting over Income Tax Schedules,” *Journal of Public Economics* 8 (December 1977); and Arthur H. Meltzer and Scott F. Richard, “A Rational Theory of the Size of Government,” *Journal of Political Economy* 89 (October 1981).

40 The differences between the methodological approach of this model and its forerunners in the analysis of the effects of inequality is well described by the following quote from Joseph Stiglitz’s contribution to Tanzi and Chu: “[E]arlier literature emphasizing the role of government in redistribution took the pretax distribution of income as given. The new emphasis is on how government can change the pretax distribution of income” (p. 36).

The sociopolitical instability and the endogenous fiscal policy models of the effects of inequality are inextricably linked in a way we discuss below. Note for now, however, the basic shared conclusion: a higher growth path, and hence stronger democracy, requires the alleviation of inequality. But the endogenous fiscal policy model also implies that the most obvious way of accomplishing this—by direct redistribution—is one of the culprits in fostering lower growth. A possible response, then, is to institutionalize a non-distributive economic policy, as political economy would have recommended less than two decades ago—but this would fail to address the threat to growth from inequality’s contributions to the erosion of sociopolitical stability.42 Meanwhile, since the creation of meaningful opportunities for socioeconomic mobility must almost inevitably be financed by taxation, finding a comprehensive equalizing policy that does not have the discouraging effect on savings seems unlikely.43 The most plausible prospect for promoting greater equality, while maintaining growth is, then, to counteract the damage to growth from lower savings by the well-off with positive effects on growth from the net transfer recipients.

Aghion’s essay presents two prominent causal mechanisms manifesting these effects. The first of these is induced by a combination of a diminishing-returns production technology and imperfect credit markets. Diminishing returns on marginal units of production inputs—the central reason for the growth convergence results in the neoclassical growth model—imply that transferring a unit of a production input from a larger investor to a smaller one (that is, redistributing toward equality) increases the return on that unit. The higher the degree of inequality in diminishing-returns economies, the greater is the gap between the actual and the potential growth paths.

Critically, as Aghion emphasizes, this conclusion depends on the absence of efficient lending/borrowing mechanisms: if they are present, the (optimal) individual investment decisions with and without redistribution are indistinguishable.44 Andrea Brandolini and Nicola Rossi echo this point in Tanzi and Chu: “Market imperfections redesign en-

42 It would leave other inequality-based threats to growth unaddressed, as well, as we discuss below.
43 As we note in the next section, there may also be a way of creating positive growth effects without decreasing the welfare of the net taxpayers, but its plausibility as a general and comprehensive measure seems doubtful.
44 The critical conditioning role of capital markets is also emphasized in Bruno, Ravallion, and Squire’s chapter in Tanzi and Chu (see, in particular, p. 133). As our discussion of the distributive consequence of trade in the previous section indicates, this role is particularly ominous in the light of the effects of underdeveloped capital markets on deepening the distributive gaps created by the SET transfers.
itlement rules by restricting differently agents’ opportunity sets. From a positive point of view, this means that the development of social institutions aimed at removing market imperfections may simultaneously reduce inequality and raise productivity.” (pp. 83–84). The empirical evidence presented by Brandolini and Rossi lends indirect support to the mediating role of credit institutions, offering a glimpse of what might be the net effects of redistribution. Using data drawn from the Luxembourg Income Study of seventeen industrialized countries (critically, the countries with the best-developed credit markets), the authors find no evidence of a negative relation between inequality and growth in their sample, correctly interpreting it as casting doubt not on the existence of such a relation, but on the explanatory power of the endogenous fiscal policy model, in which this relation is not qualified by the assumption of imperfect credit markets (p. 89). We return to the mediating role of credit markets below, where we consider its much less recognized political effects.

Whereas in the previous mechanism redistribution improves growth by creating more efficient investment opportunities, redistribution in the model of Aghion and Bolton increases individual incentives to undertake the costly efforts that determine the success of a fixed-cost investment, for example, an investment in education. As repayment is a function of the borrower’s initial endowment, the higher that endowment, the authors show, the higher is the (externally unobserved) effort level the borrower will choose in equilibrium and hence the higher is the probability of her entrepreneurial success. Given that growth in this economy is a function of successful high-stakes production, redistributing income toward borrowers from rich lenders increases borrowers’ effort levels and hence the total output of the economy. As Aghion notes, this result turns the traditional incentive argument against redistribution on its head: “Redistribution may sometimes be growth-enhancing as a result of incentive effects only!” (p. 18). The relationships between the variables are complex and depend on various factors such as market imperfections and institutional development.
captured in this model have another important consequence: although Aghion makes no mention of it in Aghion and Williamson, in the original paper the authors show that it is possible to define a redistribution formula such that the growth losses from the disincentives to the taxpayers (akin to those that are the driving force of the model of endogenous fiscal policy) are more than compensated by the growth gains from the positive incentives to the transfer recipients.

The demonstration that redistribution may have a positive rather than a negative effect on growth suggests that the pessimistic policy implication of the endogenous fiscal policy model may be overdrawn. But the development of this thesis does not address merely the economic feasibility of redistributive policies; it also has distinct implications for the larger question of the relationship between inequality and growth: in the population of economies identical to the postdistributive one in all relevant respects with the exception of a less equal income or wealth distribution, inequality is bad for growth. Put somewhat differently, the last two mechanisms suggest that, in the absence of an equality-based redistributive policy, an economic shock that leads to net losses in the incomes of the less well-off may be harmful to growth, even if it increases the incomes of the wealthy by the same or larger amount.

Although the models just discussed differ in their posited causal chains, all of them predict the negative effects of inequality on growth. Consider, however, the distinct responses to inequality on the part of the agents in the societies captured in the first two models. Whereas in the first model inequality is posited to induce redistribution via voting, in the second it is understood to induce behavior that challenges the legitimacy of the political regime. One may, perhaps, dismiss the significance of this difference as being between what are essentially complementary political responses, but that would be to lose sight of something important that affects not only the interpretation of these two models of inequality and growth but also of all four models discussed above: the political feasibility of preferred policy choices.

Taking seriously the political agency of the potential transfer recipients, sociopolitical instability is most plausibly viewed as a conditional substitute for the redistributive policies envisaged in the median-voter model of endogenous fiscal policy and not as their complement. Assuming that voters prefer signaling their attitudes by participating in

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48 Unfortunately for the coherence of their arguments, however, the empirical tests of these two models have been interpreted as at once a wholesale refutation of one and a vindication of the other; see, for example, Perotti (fn. 18).
the (costless) electoral channels of political representation rather than by engaging in costly protest activity, political instability is best seen as a consequence of the absence of redistribution, rather than as an intrinsic consequence of inequality. The realities of pluralist politics, which reward the better organized and the better resource-endowed groups, determine the extent to which government policies are sensitive to the demands of the median voters and, in turn, both electoral and extra-electoral voter response to this (in)sensitivity.

Ultimately, factors that affect the voters’ assessment of the defensibility of a socially and politically disruptive action will almost surely include expectations about their comparative welfare under the existing and the alternative political regimes and about their ability to affect the choice of policy (including redistributive policy) relevant to their welfare. Thus, the economic arguments in favor of redistributive policies and increases in educational expenditures unanimously endorsed by Aghion, Rodrik, and the contributors to the Tanzi and Chu volume and to the IADB report have a distinctive (in the immediate sense, growth-independent) political dimension. The high wealth elasticity of educational attainment and the relative inability to finance entrepreneurial ventures by borrowing—hallmarks of the highly inefficient capital markets that are typical in LDCs—result in low expectations about the social mobility of the poor and their capacity to adjust to economic change. Pursuing regime-preserving political and economic strategies in such circumstances is likely to yield them future prospects that differ little from their current conditions, producing a situation that undermines the legitimacy of existing political regimes and increases the appeal to the poor of politically destabilizing efforts to improve their welfare.49 While these efforts could lead the government to adopt a fairer economic policy, their effect in nascent democracies is likely to be doubly negative, undermining both the stability of the democratic regime (directly and by inducing the politically treacherous populist economic policy) and the growth-dependent democratic consolidation.50

One of the most cogent available defenses of a general institutional and policy-making structure that can help avert such politically and

49 This fact distinguishes the developing countries from the advanced industrial democracies, where public access to capital markets is significantly more open, offering better prospects for social mobility and, in turn, greater political stability. Significantly, as Roland Benabou and Efe Ok show in a recent paper, a lower degree of social mobility also leads to higher demands for redistribution, closing a vicious circle of the political effects of inequality in LDCs. See Benabou and Ok, “Social Mobility and the Demand for Redistribution: The POUM Hypothesis,” NBER Working Paper no. 6795 (November 1998).

50 These effects correspond, respectively, to the “exogenous” and the “endogenous” prosperity-hence-democracy arguments; see Przeworski and Limongi (fn. 6).
Economically suboptimal outcomes is presented in chapter 4 of Rodrik's book. Rodrik argues in this key chapter that the ability of societies to cope with fundamental conflict-inducing shocks, such as those associated with economic openness, requires the presence of "institutions of conflict management" that "help adjudicate distributional contests within a framework of rules and accepted procedures—that is, without open conflict and hostilities" (p. 100). These institutions function as both outlets for "voice" and providers of social safety nets. They include democratic governance that is accessible to nonelites and free from corruption, civil liberties, an independent judiciary, the rule of law, and the institutionalization of social insurance.

Critically, the existing empirical evidence, some of it presented in Tanzi and Chu, strongly supports the explanatory power of the socio-political instability model over that of the model of endogenous fiscal policy. If efficient capital markets and effective institutions of conflict management (acting, to a considerable extent, as a median-voter-like redistributive mechanism) mediate the causal effects of inequality by determining the political response of the affected members of the society, this evidence may be seen as underscoring the substantive force of the following—by now familiar—thesis. Political institutions, including electoral regimes, are properly construed as reflecting both agents’ expectations of the policy outcomes associated with particular institutional choices and the underlying power relationships in the society.

Given initial inequality, the institutions of conflict management advocated by Rodrik have distributive effects that favor some parties (namely, economically and hence politically weaker) over others (the correspondingly stronger). The conflict over policy choices should thus be expected to spill over into overt or covert conflict over the adoption of institutions, including the institutions of conflict management. It is noteworthy that three of Rodrik’s five key case studies that are intended to demonstrate the difference that institutions of conflict man-

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51 For evidence of the little support for the positive relationship between the degree of inequality and marginal tax rates, see, notably, Perotti (fn. 18). A chapter by Enrique Iglesias in Tanzi and Chu cites Argentina, Brazil, and Peru in the 1980s and Chile in the early 1970s as examples of the negative effects of populist economic policies (p. 15), but as the author himself notes, the real issue in these countries was the deficit financing of populist reforms, which led to stagflation. The cross-country evidence of the relationship between inequality and sociopolitical instability is reviewed in Alberto Alesina's contribution to Tanzi and Chu (and, more extensively, in Philippe Aghion and Peter Howitt, *Endogenous Growth Theory* (Cambridge: MIT Press, 1998), chap. 9.

52 For a recent elaboration, see Knight (fn. 3).

53 This point distinguishes democracies from nondemocracies in degree, at best; most societies that score relatively high in the standard measures of political and civil rights have a highly means-sensitive political process.
agement make in political and economic outcomes are the countries in which the antecedent degree of inequality (relatively low in South Korea and high in Brazil and Turkey) correlates with, respectively, the relative presence and the relative absence of these institutions.54

Rodrik’s institutional argument and the defenses of redistributive policies presented in Aghion’s essay, the IADB report, and Tanzi and Chu offer convincing assessments of the effects of their policy recommendations—if these could be implemented without resistance. All, however, fail to consider the political feasibility of implementing these recommendations.55 As we argue in the next section, the significance of this omission goes beyond simple incompleteness of analysis (which is most glaring in the endogenous fiscal policy model’s assumption of redistribution as a fait accompli when the preference of the median voter calls for it). Perhaps most importantly, it creates uncertainty about the net political and economic effects of pursuing a redistributive economic policy. Here the redistributive advice may not only be difficult to implement but also may be contrary to the best political and economic interests of society (as witnessed by the history of post–World War II South American coups linked to redistribution).56

At another level, the evidence presented in the IADB report and in Alesina’s chapter in Tanzi and Chu suggests that the feasibility of economic redistribution varies from case to case. Thus, for example, whereas most Latin American countries have moderately progressive social spending, it is generally regressive in Brazil; or whereas the relative welfare of the very poor has improved in the post-1974 Chilean and the post-1982 Mexican economic adjustments, it appears to have worsened in 1960s–70s Mexico and 1980s Venezuela (Alesina, 307–8, 313–14; IADB, 190–94). As these authors correctly note, the extent of the redistribution brought about by such programs is closely linked to the loca-
tion of socioeconomic fault lines in these societies. Thus, consequences of inequality for growth cannot be fully predicted without accounting for the variation in effective, politically feasible redistribution.

V. Politically Feasible Redistributive Policy

The construction of a politically plausible account of policy choice requires a systematic determination of (1) when agents will resort to one as opposed to another response to the status quo level of inequality and redistributive policy and of (2) the net effects these responses may be expected to produce. In what follows we discuss several approaches to creating such an account. In order to capture the central issues of endogenous political response, we frame the discussion by considering the distinct economic and political circumstances that determine the degree of opposition to redistribution on the part of the net taxpayers.57

The first set of circumstances corresponds to what may be thought of as the best possible world—one in which the benefits of redistribution for the wealthy could be shown to outweigh the costs of their forgone income shares. If convincing, arguments for redistribution in such circumstances would make the problem of political feasibility moot. The difficulty, of course, is that although there may be particular conditions under which such arguments can be made, there are significant obstacles that prevent them from becoming the comprehensive solution. Given the status of education as a “general purpose technology” (that is, one that increases the productivity of the economy across the board, for example, by generating innovations that may be expected to spread to every field), improving human capital markets may be thought to come close to being a universally preferred redistributive policy. However, even assuming that the net taxpayers are sufficiently “patient,” their assent is likely to require substantial reassurances about the ultimate distribution of benefits from education. In the absence of mechanisms that can provide this and thereby reduce the “risk” of such “investments,” the redistributive policy is unlikely to become politically feasible. This realization suggests a role for governments that is somewhat distinct from that of merely increasing expenditures on the education of the current have-nots. To increase the political feasibility of the tax-transfer policies necessary to finance such expenditures, governments may do well to supplement these policies with the creation of

57 Even where some results are already available, the key aim of our sketch is to suggest directions for further research in relation to possibilities they uncover.
risk-reducing mechanisms, for example, by structuring taxation as an input in a trust with guaranteed individuated returns. Significantly, the collective action problems associated with implementing the exclusion from sharing in the economy-wide benefits of education make the provision of such mechanisms a distinctly political function.

Although not completely implausible, this approach is at best partial, as it relies on the ability (and credibility) of governments to create and maintain such institutions. The latter condition is particularly difficult to satisfy in the emergent democracies, whose highly volatile economic and somewhat fragile political systems are not likely to inspire a great deal of confidence in the would-be taxpayer-cum-investor. But help in this regard can be sought elsewhere. Given the relatively broad domestic political appeal of such projects, the interests of the international community in the economic and political welfare of such societies may be well served by acting as their financial guarantor.

A more likely scenario, however, is one in which redistribution is perceived by the wealthy to be contrary to their economic interests, the protection of which motivates them to undertake actions (commensurate with their costs) to block the selection or the implementation of redistributive policies. In such circumstances the predominant model of redistributive economic decision making in the political economy literature (including the endogenous fiscal policy approach to inequality and growth)\(^58\)—the median-voter model—is arguably one of the least plausible models of policy choice.

The median-voter model is best characterized as a model of the political environments with complete (in the relevant scope) and effective voter control over government policy. In particular, societies must be thought of as having successfully resolved two central problems of the extraelectoral policy determination, problems that plague both advanced democracies and the more recent democratic converts: agenda control and bureaucratic capture. In the first case the median voter is denied the opportunity to cast a vote that could, in principle, be consequential for the determination of policy. In the second, even if such a vote could be cast, the problems of moral hazard, augmented by the difficulties of monitoring and effectively punishing noncompliance, make it unlikely that the median voter’s preferred choice will be implemented by the executive.

Paradoxically, the most plausible substantive interpretation of the political and economic environments in which these problems are least

\(^{58}\) The exception is some of the recent work on the political economy of transitions. We discuss an important representative of this work below.
rampant (and hence, in relation to which the median-voter framework is most plausible) is as those in which the scope of inequality and the potential for redistribution are relatively insignificant. This is so because the singular predominance of the electoral-based policy determinants implicit in the median-voter argument signals the relative absence (or, alternatively, the relatively equal distribution) of rents to be captured by influencing the policy via extraelectoral channels (for example, lobbying)—a property that rather unambiguously characterizes the more economically equal political democracies.

In short, all else equal, the lower the degree of economic equality, the less likely we are to see the predominance of voting-based policy determination, making the median-voter choice an exceedingly poor predictor of policy in more unequal societies. Therefore, although the median-voter model may be useful as a theoretical device allowing the identification of a purely democratic-majoritarian policy choice, the political economy of inequality and redistribution would do well to supplement it with explicit strategic bargaining sensitive to the political effects of the inequitable control of economic resources.

A promising approach to establishing the feasibility of redistribution along these lines was recently offered by Daron Acemoglu and James Robinson in the context of a model of democratic transitions. They explicitly introduce the breakdown outcomes of the extraelectoral bargaining—the credible threats of rebellions by the poor and of coups by the wealthy elite—into the median-voter model of taxation and redistribution. The more numerous poor, who include the median voter, set the tax rate when the political regime is a democracy; the rich set it if it is an autocracy. Under autocracy, the poor also choose whether to stage a costly revolution, expropriating the capital stock of the rich in that period, while the rich, who cannot credibly commit to a tax-transfer

59 Similar dynamics characterizes the justification of redistributive programs as insurance mechanisms: the greater the gap between the rich and the poor, the less plausible is the possibility that the rich's welfare can drop substantially enough to make it worthwhile to bring up the welfare of the poor as a protection for the rainy day.

60 In an influential review of the literature, Benabou (fn. 1) extends the endogenous fiscal policy analysis of redistribution and growth by endogenizing the possibility of the voting distribution being truncated at the bottom or at the top, and thereby producing political institutions with, correspondingly, “positive or negative wealth bias.” But Benabou’s results (positive wealth bias implies lesser and negative greater danger to growth) contain a bias of their own by failing to treat the “excluded” portions of the population as political agents, whose responses to such an exclusion should be expected to affect the resulting policy or the nature of the political regime. At least with respect to the truncation at the top, it is difficult to see how the outcome could constitute a plausible political equilibrium of a larger background interaction.

61 Acemoglu and Robinson, “A Theory of Political Transitions” (Manuscript, Department of Economics, MIT, 1999).
rate that would favor the poor in that period without extending to them the franchise, choose whether to democratize by extending the franchise (which could, at least partially, protect their capital stock). Under democracy, the rich choose whether to stage a costly coup, which would allow them to set a lower tax rate, but at an additional cost of possibly inciting a revolution in the following period.

Acemoglu and Robinson show that even when the rich decide against democratization (when the expected costs of a revolution are sufficiently high and hence its attractiveness to the poor can be feasibly offset), they may still prefer to redistribute away from themselves to prevent a revolution. For the given parameter conditions, the higher the degree of inequality (and hence, all else being equal, the more attractive the revolution is to the poor), the greater is the extent of voluntary sharing by the rich. Meanwhile, the political survival of democratic regimes requires that governments at times adopt rigid constraints on their preferences for redistributive policies, as, arguably, occurred in the 1990s in Brazil and El Salvador.

By articulating the conditions for feasible redistribution, this model supplies an important piece of the growth and redistribution puzzle that is not addressed by the works under review. But it also underscores the importance of another, related consideration that eludes them. From the political agency standpoint, showing that redistribution is good for growth stops short of showing that actually pursuing a redistributive policy is good for growth as well. Even if redistribution may be politically feasible, the conditions of its feasibility in the Acemoglu-Robinson model capture precisely what the sociopolitical instability model of inequality identifies as detractors of growth. As we argued above, a good case can be made that inequality is bad for growth and that, holding constant the political effects, redistribution may be beneficial. However, in the comprehensive political-economic analysis of redistribution, which must include the effects of sociopolitical instability, the net growth effects of a democratic government’s pursuit of a redistributive policy are ambiguous. Importantly, the political effects are also

62 Somewhat surprisingly in the context of the median-voter arguments (but fairly intuitive, once the possibility of power capture by the rich is introduced), Acemoglu and Robinson (fn. 61) also show that when the cost of taxation function is sufficiently convex, the higher the degree of inequality in the democracy, the lower is the average tax rate. This follows because higher inequality (and hence greater redistribution) makes coups more appealing to the rich—an effect that, in the interests of preventing the coups, must be offset by lowering the extent of redistribution.

ambiguous. This is clearly the case for the second-order effects, via the growth-democracy connection. But it is also true of the more direct effects. Because redistribution indirectly contributes to incentives for economic elites to subvert the democratic rule and given the well-documented path dependencies in the histories of military-political coups, the net effect of a democratic government’s pursuit of a redistributive policy may well be contrary to its larger political goals. A convincing argument for or against redistributive policies and a comprehensive explanation of these policies’ effects in particular economic and political environments will require the lessening of these ambiguities.

Another issue to be considered is that of the effects of redistributive policies in the institutionally more complex political environments. The binary choice of complete democracy versus autocracy is too coarse to explain the institutional effects of inequality and redistributive policies in day-to-day politics. A glimpse at the nature of these effects is offered by the recent evidence of a very significant positive relationship between the degrees of inequality and the increase in the extent of presidential powers in the postcommunist states. While the corresponding policy effects remain to be seen, this relationship points to the importance of considering microinstitutional changes for understanding the dynamics of the political effects of inequality.

Finally, future analysis should also focus on the effects of adding complexity to economic environments. Such effects may be consequential even without assuming the credible threat of regime switching, as in the following model. Assume that rather than choosing whether to orchestrate a coup, the rich simply choose whether to veto the redistributive policy selected by the relatively poor median voter.

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65 Another level of complexity is introduced with the ubiquitous “voting with the feet,” which is assumed away in the competing interests models discussed in this section, but which is, of course, increasingly relevant in the globalizing economy.

66 Tim Frye, “Presidents, Parliaments, and Democracy: Insights from the Post-Communist World,” in Andrew Reynolds, ed., *Constitutional Design: Institutional Design, Conflict Management, and Democracy in the Late Twentieth Century* (Oxford: Oxford University Press, 2000); and idem, “Cashing In: The Dynamics of Post-Communist Presidential Power” (Manuscript, Department of Political Science, Ohio State University, 2000). In the most dramatic case—Russia—a 75 percent increase in the value of a Gini coefficient corresponds to a 150 percent increase in the presidential power (based on a version of a scale of presidential powers developed in Matthew Shugart and John Carey, *Presidents and Assemblies: Constitutional Design and Electoral Dynamics* [Cambridge: Cambridge University Press, 1992]).

67 In the Acemoglu–Robinson model (fn. 61) the economic environment does not include production decisions by the agents.
with exercising a veto bearing a cost proportional to the size of the net tax being blocked. To make the underlying logic particularly stark, assume also that the poor cannot engage in effective regime change (perhaps simply because of the difficulties of overcoming the collective action problems when means are limited). In each period, following the sequential decisions by the poor (of what tax to levy) and by the rich (of how much of the tax to veto), production with diminishing marginal returns takes place, with inputs to production consisting of the post-tax-and-transfer endowments.68 In recent work69 we show that even if the cost of vetoing a tax equivalent to one unit of wealth is less than that unit, the rich may still prefer to bear the burden of taxation. Because of the nature of the production technology, a unit of endowment transferred to the poor generates a higher return in their hands than when it is invested by the rich. By accepting a tax in earlier periods, the rich effectively save in additional tax or in the cost of vetoing later on.

One of the implications of this model is that redistribution is more feasible politically in societies whose economies have more diminishing returns to production. This implication suggests that under the political conditions that create the cost variability of a policy “veto,” the causal mechanism in the diminishing-returns-based argument for redistribution (discussed in the previous section) may, in fact, contribute to establishing its own political feasibility.

Another and perhaps more important implication concerns the causal mechanisms connecting economic development and democracy. The less the wealthy choose to veto, the closer the policy outcomes are to the choices of the median voter, the more effective are the political rights of the masses, and hence the greater is the extent of democracy. Given that redistribution is more politically feasible in economies that experience more rapidly diminishing returns in production, it follows that democracy is likely to be more fully realized (stronger) in countries that rely on such production technologies. An important support for this argument comes from the neoclassical growth model. In this model, which predicts the (conditional) convergence of production paths across countries, such technologies are associated with higher levels of GDP and, correspondingly, with greater degrees of democracy.

The diminishing-returns model of feasible redistribution may be seen as offering a causal mechanism that links democracy, economic

68 Recall that diminishing-returns production technology is one of the driving forces behind the growth convergence results.
69 Dimitri Landa, “Income Redistribution and Democratization” (Manuscript, Department of Political Science, University of Minnesota, 2000).
prosperity, and inequality in a way that is broadly congruent with the interpretation of Barro’s results on the effects of inequality broached at the end of Section II: the conjunction of inequality and diminishing returns simultaneously determines both the extent of democracy and the extent of economic growth. Moreover, we should expect to see precisely the regression results reported by Barro: in the absence of controls for the nature of the production technologies, the coefficient on GDP (which already reflects both the production technology and inequality) is potentially more significant than the coefficient on inequality itself, and there is a strong positive relationship between democracy and development.

VI. Conclusion

Social scientists have been cognizant of the importance of economic inequality in determining the nature of political regimes at least since the rise of the thesis of economic determinism in the middle of the nineteenth century. The arguments reviewed in this article provide a more nuanced explanation of that relationship, namely, that the effects of inequality on the quality and stability of democracy are transmitted through economic growth or are confounded with the effects of growth in a way that calls for an integrated causal account.

A plausible approach to creating such an account lies in the consideration of the political feasibility of redistributive policies, which is explicitly or implicitly assumed by the existing models linking inequality and growth. We argue that this assumption is problematic from the standpoint of developing both a positive theory of inequality and normative policy advice. In the first case these mechanisms may be seen as underdeveloped in that their posited consequences fail to take account of the conditions of political feasibility of redistributive policies, which should be properly seen as endogenous to political theories of inequality. In the second case the policy advice produced without such an endogenous account is likely to be biased in a way that may jeopardize rather than advance the cause of democratic consolidation. We outline several approaches to offering such an account.

It may, perhaps, be viewed as ironic that our response to what we have criticized as the omission of the analysis of political feasibility is a return to the consideration of economic mechanisms. The irony is only apparent, however. Even in politically more transparent societies, like those of Western democracies, political processes are characterized by rampant problems of imperfect information and adverse incentives that
make it difficult for voters to control the policy choice in the face of activism by special interests. In societies with a less institutionalized democratic process, not only the policy outcomes but also the stability of democratic rule as such may be a function of private actions that are essentially beyond voter control. Under such circumstances, the best hope for the adoption of politically advantageous policies is likely to be appeals to interests that are shared by those who would bear the economic costs of such policies, whether they be policies of direct redistribution or of the creation of potentially conflict-ridden “institutions of conflict management.” Establishing the conditions for the existence of such interests may be the biggest contribution political economy can make to the democratic project.