Polanyi's Revenge



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Inequality and Instability: A Study of the World Economy Just Before the Great Crisis. By James K. Galbraith. New York: Oxford University Press, 2012. 336p. $29.95.

The Globalization Paradox: Democracy and the Future of the World Economy. By Dani Rodrik. New York: W. W. Norton, 2011. 368p. $26.95 cloth, $17.95 paper.

The Price of Inequality: How Today's Divided Society Endangers Our Future. By Joseph E. Stiglitz. New York: W. W. Norton, 2012. 448p. $27.95 cloth, $16.95 paper.

". . . the origins of the cataclysm lay in the utopian endeavor of economic liberalism to set up a self-regulating market system."
—Karl Polanyi[1]

Since the beginning of the financial crisis of 2008, authors and politicians have regularly reminded us of the duality of crisis and opportunity. For policymakers, administrations, and candidates, crises represent opportunities to implement long-desired reforms, seize the public stage, and reframe old arguments in the light of today's urgency. As the books under review here suggest, crises also represent opportunities for scholars. Arguments that failed to gain traction in the past may now deserve new attention as they help us understand the roots of crisis or how to recover from it. New audiences may be receptive to established arguments, now that other policies have caused us grief and are failing to promote recovery. The public and decision makers could be open to listening to these arguments and their implications, now that crisis has revealed deep vulnerabilities.

The three books that form the basis of this review essay represent trenchant critiques of neoliberal economic theory and policies, including such foundational policy assumptions as globalization, deregulation, and reliance on sophisticated financial tools. Their authors, three prominent economists, draw our attention to problems of market failure, such as rampant rent-seeking behavior and externalities. By focusing on entrenched dilemmas that include widening inequality and the compatibility of democracy and globalization, they remind us of the ways in which markets are embedded in social institutions, and how such institutions both respond to markets and shape market outcomes. All bring with them a deep commitment to understanding the functioning of society and how to maintain the benefits of global capitalism while reining in its most destructive implications.

While Dani Rodrik, Joseph E. Stiglitz, and James K. Galbraith do not directly discuss Karl Polanyi, it is difficult not to see the shadow of his masterpiece, *The Great Transformation* (1944), in these passionate yet scholarly books.[2] In his study of the rise of capitalism in England, Polanyi demonstrated how a strong state and a market economy relied on each other and should be understood as a unified entity. Markets do not exist in a vacuum, but in the context of social and political institutions. When neoliberal economists and policymakers assume that markets function perfectly and proceed to dismantle the institutions that both support them and mitigate their most negative effects, disasters such as economic crises and social instability follow. Rodrik, Stiglitz, and Galbraith thus invite, at least implicitly, renewed interaction between political scientists and economists in the context of globalization and crisis. The arguments, data, and policy prescriptions that the authors offer may not be entirely novel, but crisis has created the opportunity for us to consider them anew.

In this essay, I address the major claims of these books and how they interact (or not) with relevant ongoing research in political science. If Polanyi is correct, and the economic wreckage of 2008 suggests that he is, only attention to the relationship between political institutions and the market can indicate a way forward.

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Rodrik: The Choice between Democracy and Globalization

Dani Rodrik has long been a critic of "deep globalization" or "hyperglobalization." His 1997 book *Has Globalization Gone Too Far?* immediately predated a deep Asian financial crisis, and this crisis in turn demonstrated that even economies that play by the neoliberal rules of globalization can be deeply harmed by it.[3] In 2008 he published *One Economics, Many Recipes,* which argued that a range of different approaches to structuring economies and politics was consistent with growth and globalization.[4] This book took direct aim at technocrats in Washington and in international financial institutions (IFIs) who attempted to fit a one-size-fits-all policy prescription to a divergent set of developing countries. Both of these books called on policymakers and the public to rethink dogma about the inevitability of unconstrained globalization and the sorts of policies and institutions on the domestic level that are required to support it.

In *The Globalization Paradox,* Rodrik restates these themes and builds on them. His central contribution in this book is to argue that a political trilemma exists in the world economy, along the lines of the famous Mundell-Fleming trilemma. This theorem holds that states cannot simultaneously have full capital mobility, stable exchange rates, and monetary policy autonomy. Rodrik does not develop his trilemma with the same mathematical rigor as Mundell and Fleming, but its logic is nevertheless worth contemplating. The nature of the trilemma is the following: It is impossible for states to simultaneously pursue democracy, self-determination, and globalization. If we choose to maintain democratic decision-making processes, we need to give up either the existence of independent nation-states or deep globalization. If we insist on self-determination, either democracy or globalization has to give. And if we are committed to unlimited globalization, we cannot have both democracy and self-determination. Based on Rodrik's extensive body of work, his preferred solution to this trilemma is not difficult to predict: He recommends putting constraints on globalization, such that independent states can continue making policy decisions democratically.

The author makes his case by drawing on economic history, development economics, and some just-so stories. He provides a useful corrective to a purely market-based version of twentieth-century economic history, reminding us of both the individuals who have been harmed by free trade because their governments have not been able to compensate them for the risks to which trade exposes them, and the values of the Bretton Woods architecture that softened and supported mid-twentieth-century globalization. One of the most influential aspects of the Bretton Woods compromise (or the "compromise of embedded liberalism," in John Gerard Ruggie's terms) was the fact that states could, and did, maintain controls on the flow of capital across national borders.[5] With the collapse of Bretton Woods, the growth of new technologies that allowed instantaneous movement of massive amounts of capital across borders, and the growing belief that capital markets worked efficiently, the groundwork for financial catastrophe was set. An examination of the expansion of international trade, deep financial globalization, and the experience of developing countries in this rapidly globalizing world leads Rodrik to formulate his version of the economic trilemma.

In contemplating the trilemma, Rodrik begins with the commonsense assumption familiar to any student of international relations that national sovereignty is here to stay. Although sovereignty is variously threatened and compromised,[6] no world state is within sight. The trilemma thus quickly simplifies to a dilemma: Do we want to give up democratic control over important policy decisions, or do we want to give up hyperglobalization? Given the existence of losers from globalization and the crises to which it subjects societies on a regular basis, the choice to temper globalization is an easy one. Rodrik details the ways in which deep globalization restrains domestic policies: Labor standards, corporate tax competition, health and safety standards, regulatory takings, and industrial policies top the list. In all of these areas, if a government is committed to globalization, the logic of competition and potential races to the bottom severely diminish the ability of polities to make their own policy choices.

This logic leads the author to advocate what he calls "Capitalism 3.0." This new capitalism—and its associated processes of globalization—brings us back to the insights of Polanyi and Ruggie. Rodrik derives seven principles for a "new globalization," the first of which recognizes that markets must be embedded in governance institutions. Other principles recognize the continuing independence of nation-states and the multiple ways in which they can choose to interact with the global economy. On the level of global governance, Rodrik dismisses strong institutional solutions such as the European Union, arguing that they are incompatible with the diversity of national societies and economies. Instead, the role of global governance is limited to laying down the basic rules of international exchange.

Rodrik's analysis of the political trilemma leads to one of the most intriguing of his principles: "Non-democratic countries cannot count on the same rights and privileges in the international economic order as democracies" (p. 244). If some states have chosen to give up democracy, they can be more constrained by the demands of global institutions. The author reasons that the "scaffolding" of global governance is based on negotiations among "representative governments." However, "[w]hen nation states are not democratic, this scaffolding collapses" (p. 245). In the absence of democracy, states' domestic arrangements may not reflect popular will, and it is naive to assume that

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international pressure will push states to become fully democratic. "So non-democracies need to play by different, less permissive rules" (p. 245). Rodrik's target is primarily the economic powerhouse China, and he argues that China can be subjected to tougher global rules than democratic states because it cannot make the case that its policies on labor standards, environmental standards, and such are responsive to a democratic process.

The discussion of the political trilemma engages with debates in political science on many levels, and I would suggest that it would be strengthened by more directly addressing those debates—even as political scientists could benefit from Rodrik's nuanced assessment of globalization, development, and crisis. As I have indicated, some of his most penetrating insights have been anticipated by the work of scholars of international and comparative political economy (IPE and CPE). Ruggie, for example, has studied the way that liberal economics has been embedded in democratic institutions and welfare states.[7] Stephen Krasner draws our attention to the ways in which sovereignty has been compromised or reinterpreted.[8] Within IPE, Robert Keohane and Joseph Nye long ago argued that international agreements and institutions respond to state demands under conditions of interdependence.[9] In the wake of the Great Recession, these seminal insights demand new analysis and updating. For example, with respect to the manner in which welfare states insulate citizens from the risks created by globalization, what aspects of democratic governance facilitate or hamper this crucial function? Do factors such as unlimited campaign contributions from corporations (following the *Citizens United* decision) constrain the growth of the welfare state, thus suggesting that globalization should also be tempered?

For example, should we rethink extreme capital mobility, even among rich countries? While some developing countries have experimented with capital controls, among rich countries impediments to the flow of capital across borders are typically regarded as anathema. Rodrik suggests, for instance, that we reconsider the Tobin tax: a tax on international financial transactions. The idea of such a tax would be to throw some sand into the wheels of global capital flows, limiting levels of speculation and providing countries some breathing room to implement distinctive policies. Even a very small tax, the logic goes, would dissuade speculators and allow capital flows to respond more accurately to economic fundamentals.

While the concept of the Tobin tax has been around since the 1960s, its discussion has never gone much beyond the purely academic. From a political perspective, the challenges of implementing a global Tobin tax are immediate and huge: Who would implement and collect this tax? Who would decide at what level it would be set? Who would monitor whether investors are paying the tax, and impose fines or other punishments if they are not? These challenges are daunting, and perhaps the only response is that the notion of the Tobin tax is politically naive and not worth pursuing. But Rodrik's analysis presses political scientists to take more seriously the Tobin tax and related notions, including other forms of capital controls. Can we devise political institutions—on the national, regional, or global level—that address such challenges, even if imperfectly? What would the underlying conditions be for these policy ideas to have some chance of success?

Rodrik's proposition that nondemocratic countries have fewer rights in the global economy than democratic states also raises the possibility for fruitful engagement between political scientists and economists. The implications that the author draws from his analysis are striking from the perspective of IPE scholars working on international institutions such as the World Trade Organization. For example, he argues that "there could be a lower hurdle for imposing restrictions on a non-democratic country's trade in cases where that trade causes problems in an importing country" and that "[i]f there is a requirement that compensation be paid to exporting countries when an escape clause is triggered, the requirement could be waived when the exporting country is non-democratic" (p. 246). As with the concept of sovereignty, Rodrik does not delve deeply into the meaning(s) of democracy, or how to know it when we see it.[10] As any political scientist who has recently taught or sat in a seminar on comparative politics (or probably international relations) would know, the conceptualization and operationalization of democracy is one of the central issues in our discipline (see more discussion on this point below). Majoritarian rule over policymaking is certainly part of the equation, and this is the aspect of democracy that he has uppermost in his analysis.

Our definitions of democracy, however, also include protection of minority rights, rights to associate and to a free press, the existence of an independent judiciary, and the like. How would a fuller conceptualization of democracy affect the nature of Rodrik's political trilemma and the principles of globalization that he espouses? In addition, most analyses of democracy by political scientists see it as consistent with the delegation of policymaking authority, under specified conditions, to technocrats or experts. That is, some insulation of economic policy from the push and pull of day-to-day politics is generally considered not a fundamental threat to democracy; delegation does not equal abdication.[11] Overall, Rodrik's thoughtful development and explication of the globalization paradox raise empirical and theoretical issues that deserve new looks and assessments by political scientists, even as these issues are already part of our intellectual heritage.

Stiglitz: Inequality Is Even Worse Than You Thought

For a full-throated defense and explication of Keynesian economic principles, one need look no further than the Nobel laureate Joseph Stiglitz. He is known for combining

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his insights as an academic economist with lessons learned during his time in government, in particular on President Bill Clinton's Council of Economic Advisors, and as chief economist of the World Bank. In *Globalization and Its Discontents,*[12] Stiglitz turns his ire on the World Bank, International Monetary Fund (IMF), and WTO. Like Rodrik, he argues that these institutions fail by insisting on one-size-fits-all policies for developing countries. He followed with *Freefall,* written in the wake of the 2008 financial crisis.[13] In *Freefall,* Stiglitz explains how huge financial institutions combined with deregulation to create massive instability.

In *The Price of Inequality,* the author pushes these ideas further and concentrates on the ways in which market failures combine with government policy to create increased inequality in the United States. This book is remarkable, in part simply for the figures on inequality that it presents. Even for someone already aware of widening income and wealth gaps in the United States, the figures that Stiglitz pulls together are eye-opening. For example, "the six heirs to the Wal-Mart empire command wealth of $69.7 billion, which is equivalent to the wealth of the entire bottom 30 percent of U.S. society" (p. 8). He refers to the poverty gap, which is the percent by which the average income of the poor falls below the poverty line. The US poverty gap is 37%, near the bottom of the Organization for Economic Cooperation and Development. It is exceeded or matched only by Mexico (38.5%), Korea (36.6%), and the devastated economy of Spain (40%) (p. 16). As we all know, CEOs have benefited from massive gains in income over the last few decades, while middle-class incomes have stagnated. Stiglitz notes that CEOs did see a slight dip in their income in 2008, but had fully recovered by 2010, when the ratio between CEO compensation and that of the average worker returned to the precrisis level of 243:1 (p. 3). Of the additional income created in 2010 compared to 2009, 93% went to the top 1% (p. 3).

To what can we attribute these massive, growing gaps in income and wealth? Stiglitz puts most of the blame on market failures, including especially rent-seeking behavior facilitated by government policy. He also turns some attention to the effects of globalization, although this plays a relatively minor role in his overall story. With respect to rent-seeking behavior, political science would seem to provide firm support for his analysis. While no market is perfectly efficient, political markets seem especially inefficient. Agencies and lawmaking processes can be captured by interest groups, such that these groups benefit from devoting resources to lobbying and influence buying, rather than to economic activity that is directly productive.

Two established literatures, both of which showcase contributions by both economists and political scientists, provide theoretical and empirical foundations for Stiglitz's current claims. Studies of the "iron triangle"—or, less pejoratively, relations between agencies and their clientele—affirm the persistence and effects of rent-seeking behavior, while also indicating institutional factors that might put limits on it.[14] The sources of the resource curse, in which an abundance of natural resource wealth results in extreme rent seeking rather than investment in productive activities, also lie in institutional and political factors.[15] Stiglitz's analysis might rest on even firmer foundations if he integrated the insights of political scientists about the specific ways in which institutional factors channel the pursuit of rents. However, interdisciplinary work leaves little doubt that political market imperfections lead to rent seeking of the sort that he describes in depth, with all of its attendant negative economic effects.

Stiglitz's experience at the World Bank leads him to recognize failures of policymaking in the IFIs as well. While the author does not quite make the analytical leap, as he attributes poor policies in the IFIs to bad ideas and ideologies, political science research suggests that rent seeking may also be at the root of the failures of these institutions. For example, in his study of the role of the IMF in the postcommunist transitions, Randall Stone consistently finds that political interference by major powers such as the United States undermined the technocratic policies favored by IMF staff.[16] Rent seeking, in the form of the pursuit of political advantage, skews and biases IMF policy.[17] Perhaps even more troubling, recent studies have found that rent seeking and collusion seem to cross the boundaries of international organizations. Axel Dreher, Jan-Egbert Sturm, and James Raymond Vreeland find evidence that states that are temporary members of the United Nations Security Council are more likely to come under the purview of IMF programs, hypothesizing that having an IMF program in place allows the United States to manipulate these states' votes on the Security Council.[18]

With respect to globalization, if we compare Stiglitz's analysis to the work of scholars of CPE and IPE, the accuracy of his account seems more mixed. Stiglitz agrees with and builds upon Rodrik's "impossible trinity," arguing that globalization creates races to the bottom that undermine democratic decision making. In CPE, studies of the nature of races to the bottom have been framed as about convergence: Do the pressures of financial markets force a convergence in economic policy, to the sorts of policies favored by international capital? Here, the degree of convergence in tax and other fiscal policies has never been as strong as a simple economic model would suggest. More recent analyses have added a substantial amount of nuance to the story. For example, Layna Mosley's work makes a number of important contributions.[19] Globalization itself needs to be disaggregated, as the impact of different dimensions of globalization varies; unlimited financial flows may have significant effects, while governments are able to mitigate the effects of increased trade flows, if they so choose. Disaggregation among different types of states is also

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important, as developed countries are better placed to insulate societies and choose among different policy packages than are developing countries. Choosing to challenge international investors does come with a price, but this is a price that many states are able and willing to pay. The price of defiance is quantitative rather than qualitative, more along the lines of paying a risk premium than being rationed out of credit markets.

The deep concern about inequality that Stiglitz expresses echoes recent work by political scientists in the fields of both American and comparative politics. Building on their substantial scholarly work, Jacob S. Hacker and Paul Pierson's recent book targeted at a more general audience, *Winner-Take-All Politics,* [20] summarizes their argument that the source of growing inequality lies directly with government policy in Washington, and in turn traces policy to the activities of interest groups. Like Stiglitz, Hacker and Pierson see less impact of trends in globalization and technological change than of the choices that government makes. A recent symposium in *Perspectives* on Hacker and Pierson's work locates their analysis in the broader discipline, and argues in part for bringing a more systematic political-economy perspective to bear on these issues.[21] This analysis applies as strongly to Stiglitz's work, as more constructive engagement with the work of political economists who study potential races to the bottom, such as Mosley, would only strengthen our understanding of the sources of rising inequality.

While political economists in both the economics and political science disciplines recognize the link between public policy and inequality, one issue that demands further attention concerns why this link is apparently so poorly understood among the general public, and why the government has consistently been able to implement policies that exacerbate inequality. Larry Bartels, in his extensive work on public opinion and public policy, finds that Americans are aware of rising inequality and are worried about it, but are unable to draw the causal link between government actions and economic outcomes.[22] What model of politics allows such a situation to persist? While Stiglitz does not explicitly articulate his model of politics, implicitly he relies on a standard interest group–agency capture model. In this model, policy would naturally resist public pressure. But the work of Bartels suggests that the puzzle goes deeper than this: It is not simply that public demands for different policy are rebuffed, but that the public is not able to formulate such demands coherently. Elaborating a political model that draws consistent links among public opinion, government action, and economic outcomes remains a pressing task for students of American and comparative political economy. Jeffrey Winters and Benjamin Page, in their discussion of the possible coexistence of democracy and oligarchy, take a step toward developing one possible such model.[23]

Stiglitz is ecumenical in the literatures that he addresses, with an extensive bibliography that is unusual for books aimed at a general audience. His influences range from standard Keynesian analysis to his own experience in government to behavioral economics. All, though, lead to repeated stories of market failure and capture that lead to massive inequalities, which in turn create political and economic instability. His reading of behavioral economics even leads him to suggest that this process may become a self-confirming cycle, as individuals give up on politics and the hope of reform. In contrast, he offers a detailed critique of specific government policies, ranging from globalization to fiscal policy to monetary policy to deregulation. In all of these areas, he puts forward a reform agenda.

The reforms Stiglitz promotes do not read as especially novel, as they echo many of the favored policies of the Clinton and Obama administrations. However, the lingering effects of the 2008 crisis and its resonance in current political debates make this reform agenda as timely as ever. Bringing the insights of political science to bear on the role of institutions in determining whether reform occurs, and how, could highlight a crucial area for interdisciplinary scholarship.

Galbraith: Inequality and Modern Financial Capitalism Go Hand in Hand

Problems of inequality—its sources, its effects, and even how to define and measure it—have long been central to the work of James K. Galbraith. His works rely more heavily on the presentation of statistics and data than do the books for the general market by Rodrik and Stiglitz reviewed here. In particular, Galbraith is interested in wage inequality, arguing that it is a good proxy for inequality more generally. His 2000 book *Created Unequal* examined the sources of growing wage inequality, casting doubt on the claim that this trend is created primarily by technological change.[24] Instead, he pins inequality on factors that can be manipulated by the government, such as the minimum wage and interest rates. Thus policy, not technology, accounts for growing inequality. Galbraith's 2009 book, *The Predator State,* takes on a more polemical tone and directs its fire at conservatives.[25] Here, he develops his arguments about the capture of the regulatory process, claiming that conservatives do not press for "free market" policies, but instead for policies that directly put dollars in their pockets, such as tax breaks for the wealthy and deregulation of the mortgage market.

In *Inequality and Instability,* Galbraith focuses on measures of inequality in the years before the crisis of 2008. He has been working with a team at the University of Texas at Austin to develop indicators of inequality that go beyond the relatively scarce data used by other economists working on this topic. Much of the work on inequality has focused by necessity on states as units, on a limited number of states, and on a limited time period. Galbraith urges us to consider measures of inequality that, while perhaps not as precise or disaggregated, cover a much

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broader span of units, regions, and years, and give us a fuller picture of trends in inequality. A debate about the validity of his measures will play out in other venues. Relying heavily on Theil T statistics, which allow the creation of inequality indices at different geographic scales with relatively aggregate data, Galbraith and his team have produced an abundance of studies of inequality. The wealth of data produced by this group should be of interest to political scientists studying inequality but disappointed with the limited scope of the data sets most commonly used. This book pulls together some of the best and most pressing of the team's studies.

As in his previous work, Galbraith's main concern is to chart trends in inequality and to account for them. Here, he argues forcefully that the main culprit in rising inequality, in the United States and elsewhere, is financial markets, especially stock markets. One of the most attractive features of his approach to measuring inequality is the resulting ability to discuss it at different scales and levels of analysis in a meaningful way. The 13 chapters of Galbraith's book run the gamut: studies of inequality in developing countries (China, Argentina, Brazil, and Cuba); the relationship between inequality and democracy; inequality within America and its relation to elections; and the relationship among inequality, unemployment, and wages within Europe. Within each of these chapters, the author takes on some piece of neoliberal conventional wisdom, such as the idea that the lack of flexibility within European employment and wage structures accounts for high unemployment in that part of the world. In each case, he presents powerful countervailing evidence that should open broader debates among economists and political scientists.

For example, Galbraith's approach to data collection allows him to focus on inequality between urban and rural regions in Europe (rather than just examining inequality on a country level). Using these regional data, he shows that higher inequality is associated with *higher* unemployment, in contrast to the views held by many economists. His explanation for the finding is that high inequality leads to high rates of migration between cities and rural areas and long employment search processes, which in turn create chronic high unemployment. If this analysis is correct, the appropriate policy response is not for governments to introduce more flexibility into the labor market, as flexibility itself is part of the problem. Instead, directly targeting industrial policy at poor regions, or efforts to increase wage compression, would more likely have a beneficial effect on employment figures.

In one important respect, Galbraith's approach differs from the analysis of Rodrik discussed earlier. Although both economists are critical of neoliberal deregulatory policies and unconstrained globalization, they disagree about the aspects of these broad categories of policies that have created high inequality and other economic and social ills. Rodrik aims much of his fire at trade, which creates risk and dislocates individuals who are not adequately compensated by their governments. Galbraith considers the impact of trade on income distribution, most directly in the case of China. He finds that changes in inequality show little relationship to patterns of exports. Instead, in China as in the United States, financial markets seem to have a more significant and direct effect on inequality. In China, more important contributors to inequality across regions are the speculative activities associated with housing booms and the 2008 Beijing Olympics. China did experience an export boom after joining the WTO in 2001, and this did contribute to more inequality, but this effect was greatly magnified by speculative capital flows into sectors that might benefit from growing exports. In the United States, looking at between-county income inequality since the 1980s, Galbraith finds a direct correlation with financial and credit bubbles. These bubbles allowed the vast enrichment of a very few (as Stiglitz discusses), which in turn translated into higher inequality. These patterns mean that business cycles have become exaggerated, creating tremendous instability as seen in the Great Recession. While Rodrik's analysis of financial globalization is largely consistent with Galbraith's, their disagreement on the relative impact of trade flows is worthy of further research.[26]

From the perspective of CPE and IPE, Galbraith's iconoclastic treatment of inequality and regime type (Chapter 5) raises challenges to the use of democracy as a central explanatory variable. Galbraith notes the work of political scientists on the measurement of democracy (pp. 101–5), but largely dismisses it. He notes that political scientists developing standard measures of democracy focus on the concepts of participation, competition, and the quality of elections. He critiques these attempts to standardize measures of regime type and to make them more objective on the grounds that the "meaning of the term *democracy* has changed over time" (p. 103; emphasis in original). It is possible for countries to mimic the trappings of democracy without actually allowing voters any real choice; for example, elections may be free and fair, but the candidates may not offer distinctive policy options.

Having dismissed the widely used data sets on regime type by political scientists without actually addressing any specific data set in detail, Galbraith presents a "practical" alternative. He argues that regime type is multidimensional, so that coding democracy either as a binary measure or on a continuum would be a mistake. He also wishes to include ideology as a measure of regime type (for example, communism or Islam); to distinguish among types of autocracies (fascism vs. communism); to code levels of political chaos, such as civil war; and to account for the time required to allow democratic institutions to take root (p. 105). The considerations lead him to rely on a flexible typology of regime type. The typology is categorical rather than ordinal—no "more" or "less" democratic here. It takes the form of an array of dummy variables, six of them to be

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specific: dictatorship, communist regime, Islamic republic, social democracies, pseudodemocracies, and recent democracies (p. 109).

Page constraints and writing for a general audience limit Galbraith's ability to explain or justify his coding of these dummy variables in any detail. However, he finds results that appear impressive by any statistical standards. Dictatorship, pseudodemocracy, and recent experience of democracy lead to higher inequality. Communism, Islamism, and social democracy lead to less inequality. All of these findings are consistent with commonsense expectations of the effects of these ideologies and institutional depth. The overall fit of Galbraith's relatively simple models are also impressive, with 46% of cross-country inequality explained (p. 109). The author emphasizes in particular the large positive effect of new democracy on inequality, claiming that this variable wipes out a time trend found in other work.

Galbraith's conclusion should give political scientists pause: "that the approach of measuring democracy along a confected scale of values, and especially on the basis of the current-moment characteristics of the country, is prone to generate inconsistent relationships" (p. 111). In contrast to studies that find inconsistency or nonlinearities in the relationship between regime type and inequality, Galbraith offers a straightforward conclusion: "that political regime types have had important effects on economic inequality when they meant to" (p. 113). In other words, ideologies, religions, and beliefs have a more direct and consistent relationship to economic outcomes than standard measures of regime type that may rely on false precision.

To what extent is Galbraith's work on regime type a fundamental challenge to conceptualization and measurement of democracy by political scientists? Some facts argue in his favor. Increasingly, political scientists are using disaggregated measures of regime type rather than simple categorizations of Polity scores. For example, Beth Simmons's wide-ranging study of the effects of international human rights commitments on state practice often finds that these effects are best identified by considering such disaggregated measures of regime type as the existence of a free press.[27] Other political scientists are disaggregating among regime types, for example, focusing on the contrast between "personalistic" and "machine" autocracies.[28]

In a massive project, Michael Coppedge and John Gerring, along with their collaborators, develop a program for reconceptualizing democracy as multidimensional and collecting accurate data on each of its dimensions.[29] They identify six conceptions of democracy, each rooted in history and philosophy: electoral, liberal, majoritarian, participatory, deliberative, and egalitarian.[30] These dimensions of democracy lead them to the specification of 33 separate components of democracy, ranging from measures of suffrage to limitations on executive power. One of their 33 components is "party ideology," which may at first glance appear similar to Galbraith's ideology measure. However, consistent with the approach of most institutional comparativists in political science, the proposed measure of party ideology is not about its content but whether parties have "well-defined, consistent, and coherent" ideologies.[31] Coppedge and Gerring call for coding these 33 components in a disaggregated fashion, allowing for the possibility that some scholars may choose to aggregate them in different ways, depending on their underlying notion of democracy.

This sort of work in comparative politics is consistent with Galbraith's discomfort with "confected" scales that mix together many distinct categories, some of them imprecisely measured, and with subjective weights given to each. On the other hand, the measures currently used by comparativists and other political scientists, such as Polity scores, have the benefit of being grounded in bodies of theory, and of a long history of use and study. They also have the distinction of not conflating institutional features and ideological commitments (a problem with Galbraith's work). Given growing worries about widespread reliance on aggregated measures such as Polity and the results that Galbraith presents, perhaps space has opened for a dialogue between comparativists and political economists on the conceptualization and measurement of regime type. Galbraith offers an appendix (pp. 113–17) in which he lists the coding of each country in his data set on each binary variable, in each year. It is possible that comparativists engaged in operationalizing regime type could benefit from a close study of this appendix, to see if it offers any important correctives or directions forward.

Political Economy, Inequality, and Globalization

While written by economists, the books under review in this essay all highlight a central question for students of CPE and IPE: What is the role of government in the new global economy? This is not a new question, nor one that has been neglected. But it remains an open one, and the Great Recession brings it to bear with more urgency than ever. Rodrik, Stiglitz, and Galbraith, though in disagreement on some specifics, all illustrate the ways in which naive faith in the perfection of markets, and policymakers who are captured by this ideology, create conditions of economic and social inequality and instability. Their work intersects with that of CPE and IPE at many points, but research on both sides of the aisle could benefit from a deeper engagement between the two disciplines. These prominent economists are strikingly critical of some members of their own discipline. They charge that economists have been captured by neoliberal ideology, refusing to question their core assumptions and methods in spite of overwhelming empirical evidence that these assumptions are not valid. The policy prescriptions that follow from this

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ideology have been devastating to economies and societies. Yet these authors do not make a direct turn to political science as the answer to these failings in their own discipline; while drawing on the occasional work in IPE or CPE to make a point, these books do not demonstrate a serious engagement with those bodies of literature.

In the midst of World War II, Karl Polanyi drew on the economic history of England to draw attention to the interaction of markets and political institutions, showing that the embeddedness of markets in institutions created a role for governments to insulate and compensate their citizens from the forces of global capitalism while finding ways to reap its rewards. Markets did not spontaneously emerge but were based on social institutions and on government policy. As Polanyi argues, "[t]here was nothing natural about *laissez-faire*; free markets could never have come into being merely by allowing things to take their course. . . . The road to the free market was opened and kept open by an enormous increase in continuous, centrally organized and controlled interventionism."[32] Recent years suggest that we all may need to relearn this lesson. Government policy created the conditions, such as rapidly escalating inequality and encouragement of highly risky financial activities, that allowed markets to collapse in 2008. Government policy is central to the question of how to address underlying imbalances and stabilize the system, and the only route to a deeper understanding of policy and institutions is collaboration among political economists in the disciplines of both economics and political science.

The books reviewed in this essay suggest the following issues and questions as especially promising for interdisciplinary work: A central set of issues revolves around the conceptualization of regime type. Both economics and political science are tentatively moving away from a unidimensional conception of regime type that ranges from autocracy to democracy, suggesting that our measurements of type should instead be multidimensional. At the same time, work in comparative politics and international relations, as well as most political economy, continues to rely on established aggregate measures. Are new measures necessary, and if so, of what, and how should such data be collected? How would a richer understanding of regime type influence the working of Rodrik's political trilemma, and can his conclusion that nondemocratic states should be held to higher standards be sustained? This last question could prove fertile ground for scholars studying international organizations and institutions, where the argument that the economic rules applying to member states should be conditioned on regime type is novel, and suggests some intriguing theoretical directions.

Another major set of issues concerns the nature and intensity of races to the bottom that globalization might engender. While Rodrik and Stiglitz both express great concern about such races, work in CPE has found that the constraints of globalization are quite a bit looser than these economists imply.[33] Another set of issues demanding new or renewed research agendas is suggested by the aspects of globalization that these authors neglect. For example, these books are silent on the issues of labor migration and on remittances. Labor migration exists in a tight symbiosis with capital and trade flows;[34] how does it fit into the frameworks established by these authors? Similarly, remittances have come to constitute a large share of capital flows to developing countries, and they have strong countercyclical effects;[35] to what extent do they challenge the claims that Rodrik, Stiglitz, or Galbraith make?

Finally, what about inequality? While the books reviewed here paint a grim picture of growing inequality and its sources, they say less about the implications of income and wealth gaps. Generally, inequality may lead to instability, but of what sort, and how can policies and institutions mediate this relationship?

The scope for interdisciplinary approaches to these questions, and others raised in these books, is massive.

Notes

1 Polanyi 1944, 29.
2 Polanyi 1944.
3 Rodrik 1997. It should be noted, however, that the Asian economies recovered more quickly from this deep crisis than history—or recent experience—would lead us to expect.
4 Rodrik 2008.
5 Ruggie 1982.
6 Krasner 1999.
7 Ruggie 1982.
8 Krasner 1999.
9 Keohane 1984; Keohane and Nye 1977.
10 James Galbraith's book, discussed at length in this essay, is likewise dismissive of discussions among political scientists about how to define and measure democracy.
11 But see Roberts 2010 on the record of often-failed attempts to delegate policymaking to the discipline of independent experts.
12 Stiglitz 2003.
13 Stiglitz 2010.
14 Weingast 1981.
15 Robinson, Torvik, and Verdier 2006.
16 Stone 2002.
17 Copelovitch 2010.
18 Dreher, Sturm, and Vreeland 2009.
19 Mosley 2003.
20 Hacker and Pierson 2011.
21 See Box-Steffensmeier 2011, 643; Stepan and Linz 2011.
22 Bartels 2005.
23 Winters and Page 2009.

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24 Galbraith 2000.
25 Galbraith 2009.
26 For another evaluation of alternative sources of inequality, see Noah 2012.
27 Simmons 2009.
28 Weeks 2008.
29 Coppedge and Gerring 2011.
30 Ibid., 254.
31 Ibid., 256.
32 Polanyi 1944, 139–140.
33 In addition to Mosley 2003, see Boix 1998; Brooks and Kurtz 2007; Garrett and Lange 1991; Haupt 2010; and Mosley and Uno 2007, among many other works.
34 Peters 2012.
35 Singer 2010.

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