Abstract: A principal approach to theorizing about economic reform in developing countries has been to assume that market-oriented policies have the properties of public goods, in that their benefits are widespread and their costs concentrated. This article reviews several books, one of them from the World Bank, that suggest that skepticism about these policies has entered the mainstream, calling into question this benchmark approach to reform. In the context of ongoing debate over which policies are best for developing countries, the review offers a framework for future study of reform, arguing that while past work has yielded important insights on how societal divisions and institutional characteristics affect reform, these insights now need to be combined with scholarship on how governments learn and form preferences about policies.

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Economic reform has been the subject of one of the most important literatures on the politics of developing countries in the last thirty years. Beginning in the early 1980s, this body of work took a comparative approach to understanding why some countries progressed further in market-oriented reform than others. As Joel Hellman wrote in a seminal article, the “central paradox” that this literature sought to address was, “if reforms ultimately make all or a majority of a country’s citizens better off, why are they so politically contentious, especially in democratic systems?”

Hellman’s phrasing was revealing. Most of the major works in this literature shared a common assumption: these market-oriented “reforms ultimately make all or a majority of a country’s citizens better off.” In fact, one of the principal approaches in the literature was to liken market-oriented reform implementation to a public good, in the sense that, as Stephan Haggard and Robert Kaufman said, “the costs of reform tend to be concentrated, while benefits
are diffused.” As Hector Schamis discussed, this approach was taken by both economists and political scientists: “In much of the discipline of economics a liberal economic order is treated as a public good….A wealth of research has addressed [these liberal-oriented reforms]… under the heading ‘the politics of economic adjustment.’ Scholars in this research paradigm, for the most part political scientists, have provided a rather straightforward answer [for why reforms are so difficult]. Paradoxically, however, and despite their greater sensitivity to political factors, their explanation does not depart significantly from propositions rooted in the neoclassical paradigm.” That explanation, generally building on Mancur Olson’s path-breaking work on public goods, was that interest groups that would “lose” from reform were better able to solve collective action problems than the “winners,” and could therefore prevent such policies from being implemented for the betterment of society.

These scholars’ focus on the effects of interest groups led many of them to examine the kinds of domestic institutional conditions under which reforms might proceed more rapidly. In particular, studies focused on which institutions were able to block the negative influence of interest groups on the policymaking process. These institutions tended to fall at the extremes of the distribution of possible arrangements: either benevolent dictators that could stand firm against interest groups, or very democratic institutions that could diminish the power of these groups. In other words, the literature predicted that countries with these institutional conditions would be better at producing public goods in general, and market-oriented reforms in particular.

However, serious questions about the benefits of market reforms—of what was called the Washington Consensus—were being raised even as these works were being written. Doubts rose initially based on the East Asian Miracle, continued through the crises around the world in the 1990s, and became stronger as the growth results of many “reformed” countries disappointed.
These experiences spawned important works questioning the benefits of orthodox policies, but for whatever reason, the literature about the determinants of market-oriented policy reform rarely incorporated their skepticism. In fact, even recent studies of such reform continue mostly to take the benefits of orthodox policies for granted.

As scholars begin to assess the lessons of the recent global economic crisis for their work, this reform literature, which relied so heavily on the merits of market-orientation, seems worthy of revisiting. The five books reviewed here include the work of some of the most prominent scholars working in development, including four Nobel-Prize winners. They indicate that the disagreements over the benefits of these policies have become significant enough that assuming they are public goods can no longer be a viable research strategy. Strikingly, even the World Bank’s Commission on Growth and Development, which includes the father of economic growth theory Robert Solow, concludes that “no generic formula exists” for growth, “orthodoxies apply only so far,” and that governments should “pursue an experimental approach to the implementation of economic policy” (pp. 2-3). Skepticism about orthodox policies has clearly entered the mainstream.

Most importantly for the purposes here, the disagreements represented in these books have significant implications for the literature on economic reform. Clearly, future theories of reform can no longer ignore disagreements over the effects of orthodox policies. However, the implications are important even for existing scholarship. If, as several of the works here suggest, the policies that lead to economic growth are instead heterodox—if, in other words, heterodox policies are the real public goods—then the predictions of much of the existing literature change dramatically. For example, instead of being associated with market-oriented reform—as
discussed above—benevolent dictators and strong democracies should be associated with heterodox policies.

The literature on economic reform is important enough—and the implications of these disagreements sufficiently significant—that understanding the nature of these debates about economic policy deserves attention. This review first examines the areas of agreement and disagreement in these works, focusing on why many scholars have come to doubt whether market-oriented reforms are indeed public goods. The third section turns to the lessons for scholars interested in economic reform, examining what can be gleaned from the existing reform literature and what research directions these debates regarding economic policy suggest. A brief final section concludes.

II. The debate about development strategy

The literature on economic reform arose during the ascendency of market-liberalizing policies famously summarized by John Williamson as the “Washington Consensus.” While the 1950s, 1960s, and 1970s had been characterized by active state involvement in economies, often supported by international development institutions like the World Bank, by the late 1970s there was growing criticism from both the left and the right of the results of this strategy. Weakened by these critiques, the state intervention paradigm was brought to its knees by a series of international crises, including the two oil shocks in the 1970s, the Paul Volcker interest rate shock, the collapse of commodity prices, and the Latin American debt crisis.

Following this upheaval, establishment economists agreed that the best way for countries to develop was to liberalize their markets. Williamson’s original formulation was a list of ten policies on which he believed there was a fair amount of consensus in Washington at the time—
that is, among the U.S. Treasury, the International Monetary Fund, the World Bank, and mainstream economists. These included imposing fiscal and monetary discipline; re-ordering public expenditure priorities towards pro-growth and pro-poor expenditures; creating tax systems with broad bases and marginal rates; liberalizing interest rates and prices; ensuring a competitive exchange rate; liberalizing trade; liberalizing inward foreign direct investment; privatizing state-owned enterprises; deregulating industries to ease barriers to entry and exit; and ensuring property rights. To the extent that the state-oriented industrialization of the previous decades had reflected a faith in the ability to govern economies, the Washington Consensus reflected a shift toward a greater faith in the power of markets.

This view of the world still has many proponents, representing one side of the present debate about development strategy. In this light, it is useful to begin this review with Benno Ndulu and others’ *The Political Economy of Economic Growth in Africa, 1960-2000*, which can be characterized as supporting the Washington Consensus. The two-volume set is the result of an ambitious seven-year project of the African Economic Research Consortium, a research and training institute in Kenya funded by the World Bank, various bilateral donors, and private foundations. The project combines cross-national statistical evidence (both global and within Africa) with in-depth case studies of 26 African countries. The case studies are informed by initial statistical work and then, in turn, inform the important categorization (discussed below) that distinguishes the main statistical findings of the final work. The authors are probably correct to claim that the result—with over 50 contributors—is “by far the most comprehensive country-based assessment of Africa’s growth experience to date.”

Echoing the categorization of Robert Barro, the authors focus on “opportunities” and “choices” in determining the growth outcomes of countries. Based on their case study
evidence, the authors develop three classifications of countries in terms of their opportunities: coastal, resource-scarce countries; landlocked, resource-scarce countries; and resource-rich countries. While these opportunities provide, in a sense, the baseline for growth prospects (and for the analysis, as the country case studies are organized by these categories), the principal focus of the project is on the choices that governments make in the context of these opportunities.

The authors classify those choices into five categories. Four of these the authors call “anti-growth syndromes,” consisting of “regulatory regimes that severely distort productive activity and reward rent-seeking, regimes of ethno-regional redistribution that compromise efficiency in order to generate resource transfers to sub-national political interests, …regimes of intertemporal redistribution that aggressively transfer resources from the future to the present…, [and] state breakdown, [referring] to situations of civil war or intense political instability….“

The fifth category consists of countries that avoid these four syndromes, and the authors call it syndrome-free. The syndromes themselves are generally defined in relation to the Washington Consensus policies discussed above. In fact, the authors present evidence that their categorizations track very closely the World Bank’s policy evaluations of countries, arguing that this confirms the validity of the authors’ codings.

Almost all of the first volume of the project is devoted to examining the effects, internal logics, and causes of these syndromes (the second volume consists of the case studies). Not surprisingly, the authors find it is much better to be syndrome-free than to have a syndrome. Analyzing their sample of African countries from 1960-2000, the authors find that 35 percent of the country-years are characterized by the regulatory syndrome, 44 percent by the ethno-regional redistribution syndrome, and 18 percent by the intertemporal redistribution syndrome (countries
could be characterized by more than one syndrome at a time). Fourteen percent of the country-years are characterized by the state breakdown syndrome, and 25 percent are syndrome-free. The authors analyze the effects of syndromes on growth collapses (defined as a three-year period in which the centered moving average of growth is negative) and sustained growth (a five-year centered moving average of over 3.5 percent). They find that the probability of sustained growth in the presence of one or more syndromes is 20 percent, but that the probability rises to 44 percent in syndrome-free countries. In contrast, the probability of a growth collapse is nearly 50 percent for countries displaying one or more syndromes, but below 20 percent for syndrome-free countries.

Given the similarity between their coding scheme and the World Bank’s own policy evaluations, the message of the results is an implicit affirmation of the Washington Consensus and therefore validation of the dominant approach in the economic reform literature discussed above. Rather than put the blame for Africa’s dismal economic performance on the policies of the World Bank and IMF, the authors of this study argue that there has not been enough Washington-style reform in Africa. As mentioned above and discussed in further detail below, the policies of the Consensus have come under much theoretical and empirical criticism, and thus, for supporters of the Consensus, a study of this depth that demonstrated the effectiveness of market-oriented policies in Africa would be quite valuable.

However, even for pro-Consensus scholars, the research strategy of this book limits its usefulness. Instead of demonstrating the effectiveness of the policies themselves, the study focuses on “syndromes,” and it never becomes clear why the leaders of the study felt the need to aggregate policies into these categories. It is far more straightforward to quantify the liberalization of exchange rates, fiscal deficits, or even trade barriers, in order to analyze their
impacts, than it is to identify which countries have these syndromes. Unfortunately, the authors never specify the syndromes clearly enough for their categorization to be repeated by other scholars.18 This not only limits the value of the study for future research but also bypasses the most central questions in this area. How do countries know, for example, when their regulations have moved from sound regulatory regimes to syndrome-status? Or when their redistributional systems have moved from helpful to harmful?

These are central questions because many of the most successfully growing countries in the world over the past 50 years have not looked particularly “syndrome-free.” In One Economics, Many Recipes, Dani Rodrik presents two useful (and somewhat tongue-in-cheek) thought experiments. The first is to ask what would happen if an intelligent Martian was given the list of original Washington Consensus items and asked to compare its predictions with countries’ growth experiences since 1960. Rodrik argues that the Martian would find that the policies of the high-performing East Asian countries generally looked quite different than the Washington Consensus, using directed credit, trade protection, export subsidization, and tax incentives, along with highly heterodox arrangements in areas of corporate governance, financial markets, business-government relations, and public ownership.19 Meanwhile, the region that made the most determined attempt to adopt the Washington Consensus—Latin America—benefited comparatively little. “Countries such as Mexico, Argentina, Brazil, Colombia, Bolivia, and Peru did more liberalization, deregulation, and privatization in the course of a few years than East Asian countries have done in four decades…, [yet] Latin America’s growth rate has remained a fraction of its pre-1980 level.”

If the goal of the preceding thought experiment is to highlight the difference between what the Washington Consensus predicted and what happened, the goal of Rodrik’s second
thought experiment is to highlight the importance of the goals of the Washington Consensus—and to make the critical point that there is more than one way to achieve them. Rodrik considers what a western economist trained in the Washington Consensus tradition would have recommended to China in 1978. Beginning with the abolition of state-controlled food prices and the liberalization of agricultural markets (since the majority of the poor lived in rural areas), the economist would have realized that those reforms would have created additional problems, as would her solutions to those additional problems. Quickly she would have progressed down the line and seen the need for land privatization, tax reform, corporatization of state enterprises, trade liberalization, financial sector reform, and social safety nets. As Rodrik finishes cataloging the reforms she would have recommended, he notes, “The economist’s reasoning is utterly plausible, which underscores the point that the [Washington] consensus is far from silly: it is the result of systematic thinking about the multiple, often complementary reforms needed to establish property rights, put market incentives to work, and maintain macroeconomic stability.”

However, Rodrik argues, “while this particular reform program represents a logically consistent way of achieving these end goals, it is not the only one that has the potential of doing so.” In fact, China itself is evidence of this, because it protected property rights, employed market incentives, and achieved macroeconomic stability through different means. For example, in the township and village enterprises that proved central to Chinese economic growth, property rights were assigned not to individuals or the central government but rather to local communities. And market incentives were used in agriculture in a novel way that allowed farmers who had met their obligations under the state order system to sell their surplus at market-determined prices.
This is the central message of Rodrik’s book: function does not translate into form. The “higher order principles of sound economic governance—property rights, market-oriented incentives, sound money, fiscal solvency” (p. 39) do not require particular policies or institutions. There are many different policies and institutions that can accomplish these ends—probably many we have not even seen yet. This point has two implications. The first is that unorthodox policies can have beneficial results. The second is that policies that most economists find uncontroversial might be inappropriate given certain circumstances. Rodrik (p. 31) illustrates this second point by examining the conditions under which trade liberalization will lead to economic growth in neoclassical theory—the conditions include that the liberalization is complete (that is, no remaining tariffs or barriers); no other microeconomic market imperfections exist; the economy is “small” in comparison to world markets; the economy is close to full employment; there are efficient tax-transfer schemes to compensate for the redistributive effects of the liberalization; there are means to compensate for adverse effects on the fiscal balance; and the liberalization is sustainable and therefore credible. If these conditions do not hold, the theory cannot say that trade liberalization will be good for economic performance.

It is worthwhile to pause to consider the importance of this point, which appears again in Narcís Serra and Joseph Stiglitz’s *The Washington Consensus Reconsidered*. As Stiglitz writes, The intellectual foundations of the Washington Consensus had been badly eroded even before its doctrines became widely accepted. The fundamental theorems of welfare economics provided the rigorous interpretation of Adam Smith’s invisible hand, the conditions under which and the sense in which markets lead to efficient
outcomes. Under these theorems, it turned out, markets were efficient only if capital markets were impossibly perfect—at least in the sense that there be no missing risk or intertemporal markets. There could be no externalities (no problems of air or water pollution), no public goods, no issues of learning, and no advances in technology that were the results either of learning or expenditures on R&D. Greenwald and Stiglitz went further and showed that there also could not be any imperfections of information, changes in the information structure, or asymmetries of information. 26 These problems are serious in any economy, but are at the heart of development.

If the assumptions of the original models do not hold, other policies might work better. For example, as Rodrik discusses, Thomas Hellman, et al. have shown that in the context of asymmetric information and suboptimal savings, creating a moderate level of rents for banks can improve both the quality and level of financial intermediation compared to financial liberalization. 27 Rodrik himself has argued that in the presence of scale economies and interindustry linkages, firms face a coordination problem in producing socially profitable investments—a problem that can be solved by a coordinating industrial policy. 28 And Bruce Greenwald and Stiglitz have shown that if technological spillovers within countries and across industries are important for growth, then there is an argument for using trade barriers to protect infant economies (as opposed to just infant industries). 29

In sum, while the Washington Consensus approach retains its supporters, scholars have offered both theoretical and empirical reasons to doubt whether Consensus policies are public goods, always best for developing countries striving for economic growth. As Rodrik notes, the
massive literature on the causes of economic underdevelopment can generally be classified into works that focus on government failures and those that focus on market failures. The Ndulu et al. book is an example of the former; the Rodrik book is generally an example of the latter. Broadly speaking, the formative experience of the former camp is the “failure” of import substitution industrialization (ISI) policies in the 1960s and 1970s. The latter camp argues that ISI was not as much of a failure as its critics say. For this latter group, the formative experiences are the East Asian Miracle and the “failure” of Washington Consensus policies in the past two decades. As evidenced by the Ndulu et al. book, however, many scholars remain unconvinced about the failure of the Consensus.

It is interesting in this context to consider how the region one studies can influence the side of the debate on which one falls. Africa is a continent whose population-weighted average annual growth rate was effectively zero during the forty years under study in the Ndulu et al. project. In other words, if an African country were growing at all during this period, it was doing better than average. In the rest of the developing world, the per capita growth rate was the highest ever seen, at 3.63 percentage points. As the saying goes, when you find yourself in a hole, the first thing to do is stop digging: it is perhaps not surprising that many scholars of Africa have focused on asking “What went wrong?” As discussed above, Ndulu et al. draw attention to their finding that syndrome-free countries have a 44 percent probability of sustained growth and only about a 20 percent probability of growth collapse. In other regions of the world, it might seem odd to characterize as “good” a policy environment within which one in five countries experiences a growth collapse, and more than half the countries do not experience sustained growth. Scholars of other regions, in contrast, seem to focus on the countries that have grown rapidly, asking “What went right?” The Ndulu et al. study is a reflection of the mistrust that
many scholars have of governments’ ability and willingness to intervene successfully in markets. The Rodrik study is a reflection of the belief of other scholars that government intervention has been fundamental to the economic success stories of recent decades.

This relative stalemate is well represented in Serra and Stiglitz’s The Washington Consensus Reconsidered, the result of a 2004 conference in Barcelona that sought to generate a “Barcelona Consensus” as an alternative to the Washington one. While many edited volumes suffer from a lack of cohesion amongst their papers, this collection is useful precisely because of the wide range of opinions that it collects. Alice Amsden’s chapter praising the performance of industrial policies in the developing world appears next to Guillermo Calvo and Ernesto Talvi’s chapter praising the Washington Consensus and warning of the dangers of capital controls.34 Jeffrey Frankel’s chapter on the benefits of trade openness stands next to Martin Khor’s chapter questioning those benefits and the fairness of the international trade system in general.35 The authors often do not seem to agree even on basic principles, as evidenced by the spirited exchange between Stiglitz and Williamson about how the phrase “Washington Consensus” should be used and interpreted.36 In its wide range of viewpoints, the book is an accurate representation of the current divisions in the field of development.

To return to the issue that motivates this review, the key question for scholars of economic reform is when these divisions become significant enough that researchers can no longer plausibly assume for the purposes of theory-building that certain policies have the properties of public goods. In this context, it is interesting to note that Serra and Stiglitz were able to fashion a Barcelona consensus—called the “Barcelona Development Agenda,” perhaps to avoid the word “consensus” entirely. As might be expected, the seven lessons agreed upon are not controversial or particularly specific. They include the importance of institutional quality,
such as rule of law, property rights, and “an appropriate balance between market and state” (p. 58); the danger of large debts (private and public), poorly regulated banks, and loose monetary policies; the need to improve multilateral trade and financial arrangements; and the need to advance the prospects for labor mobility (international migration) and the environment.

There is, however, an unexpected lesson included in the Barcelona Agenda: “There is no single set of policies that can be guaranteed to ignite sustained growth” (p. 59). Almost certainly, the paragraph about this lesson was the result of careful negotiations, and it is useful to excerpt from it:

Countries should be free to experiment with policies suited to their specific circumstances, and international lending organizations and aid agencies should encourage such experimentation. But freedom to experiment is not the same as an ‘anything goes’ approach to development. Neither should this freedom be used to disguise policies that merely transfer income to politically powerful groups. The priority is to identify the most binding constraints to growth and to address them through microeconomic and macroeconomic policies. Micro interventions should be aimed at redressing specific market failures, and incentives should be contingent on improved performance by recipients.37

Agreement with this statement by the supporters of orthodox policies in *The Washington Consensus Reconsidered* is strong evidence of the doubts scholars now have regarding the direction economic policies in developing countries should take. But it is not as significant as the fact that a high-level group convened by the World Bank—with no overlap with the group
Serra and Stiglitz convened—has come to an essentially identical conclusion in *The Growth Report*. The Commission on Growth and Development was chaired by the Nobel-Prize winning economist Michael Spence and included another Nobel Prize winner (Robert Solow) and 19 senior policymakers from around the world. The report is not particularly important for its research or contents: the main empirical contribution is to analyze the experiences of the only thirteen countries that have grown at an average rate of seven percent a year for more than 25 years since 1950, a research strategy that suffers from selecting on the dependent variable. It is certainly not, as the back cover proclaims, “the most complete analysis to date of the ingredients that, if used in the right country-specific recipe, can deliver growth and help lift populations out of poverty.”

The report is, however, noteworthy because it is a prominent statement on growth from the leading international institution in development. Despite the report’s flaws, Rodrik has called it “a watershed for development policy.” Ravi Kanbur, the economist who resigned as head of the World Bank’s *World Development Report 2000/01* because of pressure to make market-oriented growth more central to the message, has called *The Growth Report* “music to my ears.” The reason is that the message of the report is far more nuanced than previous Bank reports. As the Commission itself writes,

> The report…does not provide a formula for policy makers to apply—no generic formula exists. Each country has specific characteristics and historical experiences that must be reflected in its growth strategy (p. 2)….Wedded to the goal of high growth, governments should be pragmatic in their pursuit of it. Orthodoxies apply only so far….At this stage, our models and predictive devices
are, in important respects, incomplete (p. 3)….It seems to us that the correct response to uncertainty is not paralysis but experiment. Governments should not do nothing, out of a fear of failure. They should test policies, and be quick to learn from failure. If they suffer a misstep, they should try something else, not plunge ahead or retreat to the shore (p. 31).

It should be noted that neither the Commission nor the Serra and Stiglitz group substantiate the benefits of the “experimental” approach with theory or empirics. In fact, the “experimental” approach may be fashionable because no one can really argue forcefully against it. It is essentially a loosely formed and unsubstantiated hypothesis: experimenting with policies will lead eventually to better economic growth. Unfortunately for a policy document like The Growth Report, the hypothesis does not seem particularly helpful for policymakers. For example, in what ways specifically should they experiment? The report offers little in the way of practical advice, except for a list of some things not to do—“bad ideas” (p. 68)—including subsidizing energy, imposing price controls to stem inflation, resisting urbanization, and underpaying civil servants. Nevertheless, despite its shortcomings for policymakers, the Bank’s advocacy of this experimental approach has a clear message: the days of a clear mainstream consensus about economic policy are over.

Before discussing the implications of this message for scholars of economic reform, it is worth noting that Rodrik, in One Economics, Many Recipes, has taken the current debate farther. Instead of arriving at the intellectual cacophony regarding development policy and simply concluding that countries should experiment, Rodrik uses this state of affairs to argue for the benefits of certain political institutions. Since, as he states, the functions of economic
institutions do not imply their forms (that is, there are many ways of accomplishing the same “higher order” economic principles), countries should design development strategies using local knowledge about problems and solutions. In this message, he is not so different from the Barcelona Development Agenda or The Growth Report. However, Rodrik goes a step further and argues that participatory political institutions are systematically better than other kinds of institutions at tapping local knowledge and capitalizing on it. So while policymakers can no longer say with confidence which economic policies should be enacted, they should be promoting participatory political institutions.

The evidence that Rodrik brings to bear for this argument will strike many scholars as too aggregate. He presents statistical evidence that democracies yield more predictable long-run growth rates, produce greater short-term stability, handle adverse shocks better, and deliver better distributional outcomes. However, he presents no evidence regarding causal mechanisms. In fact, the mechanisms themselves are under-theorized. What does “participatory” mean? How much participation is adequate? Can there be too much? Are there societal or institutional characteristics that make participation more or less successful? Readers who are familiar with Rodrik’s previous body of work may be disappointed that this book does little to advance his earlier arguments.43 Almost all the chapters have been published previously, and there is only minor updating to them. This is particularly disappointing in the chapter about the effects of participatory political institutions. The data presented are from his papers in the late 1990s, which use data ending around 1990, raising questions about whether the relationships continued to hold over time. Just as important, one would have liked to see theoretical advances in his arguments, for example with regard to how democracies manage social conflict better in responding to economic shocks. Nevertheless, Rodrik has pushed the intellectual debate about
development policy in an important direction, and even if he does not continue to refine his past work, others will likely benefit from doing so.

III. The implications for theories of economic reform

As discussed at the beginning of this review, the literature on economic reform has largely taken for granted that countries should move in a market-oriented direction. The central paradox of the literature has been, “If [market-oriented] reforms ultimately make all or a majority of a country’s citizens better off, why are they so politically contentious, especially in democratic systems?” As evidenced in the previous section, however, there are now significant and important disagreements among economists regarding the direction policies in developing countries should take. If it is not clear that market-oriented reforms make all or a majority of a country’s citizens better off, what can we say about the existing literature?

In thinking about the implications of the current climate within economics, it is useful to keep in mind that upheavals in the dominant economic paradigm have happened before. As mentioned above, the period from the 1940s to the early 1970s was dominated by state-oriented development, following the market catastrophes of the 1930s. The relatively successful experience of the Soviet Union and other Eastern European countries seemed to indicate that the statist model was best for spurring economic development, and the World Bank was a major supporter of this approach, heavily financing state industry and infrastructure throughout the period. It might therefore be interesting to see how scholars studying reform during that period changed their approach with the onset of the Washington Consensus.

Unfortunately, however, the politics of reform were not a major focus of western scholars during those earlier decades. Reflecting the era, the post-War period saw political scientists far
more interested in the determinants of poor countries’ democratization and stability than their economic development.\textsuperscript{45} Whereas today’s scholars might have expected to see several studies on the determinants of success in implementing “import substitution” policies during these years, a search for that phrase in the \textit{American Political Science Review} from 1944 to 1980 yields just three research articles.\textsuperscript{46}

Nevertheless, in line with this expectation, political scientists interested in economic reform during these decades did tend to focus not on market-oriented reform, but rather reform in the opposite direction: “policy decisions on economic matters in which the Government has decided to intervene.”\textsuperscript{47} A prominent example is David Cameron’s 1978 examination of which countries had “become more influential as providers of social services and income supplements, producers of goods, managers of the economy, and investors of capital.”\textsuperscript{48} The dominance of the interventionist ideology of the time was such that the countries in Cameron’s study were not the typical import substitution countries that usually come to mind, but rather advanced industrial countries. It is a sign of the quickness with which paradigms can change that within twenty years of Cameron’s study, Joel Hellman saw no need to specify the meaning of “reform” in either the title or the text of his article mentioned above, “The Politics of Partial Reform.”\textsuperscript{49} His idea of “partial reform” referred to an implicit ideal of a “fully” reformed economy, and he assumed it was unnecessary to clarify that he was talking about market-oriented reform. Hellman was not alone in making this assumption.\textsuperscript{50} As Adam Przeworski noted in the early 1990s, “The very term ‘reform’ has in the last few years become synonymous with a transition from an administered to a market economy. Twenty years ago this term conjured up distribution of land to peasants in Latin America or tinkering with the planning system in Eastern Europe.”\textsuperscript{51}
More surprising than the speed with which scholars’ focus turned was the depth of their conviction regarding the benefits of market-oriented policies. Echoing the quotation above from Haggard and Kaufman, Timothy Frye writes, “conventional wisdom on the politics of economic reform…emphasized that [market-oriented] reforms produced concentrated costs on specific groups in the short run and dispersed benefits for society in the long run.” Despite the fact that state-oriented development had been in fashion only one or two decades previously, these new theories of economic reform did not incorporate the possibility of change or flexibility in recommended policies. Scholars seemed uninterested in developing a theory of economic reform that could account for both the implementation (partial or not) of Washington Consensus policies and the implementation (partial or not) of policies with higher degrees of government involvement in the economy. This is likely a function of the fact that the literature on economic reform took off during the period of the Washington Consensus. With the decline of the Consensus, there is now an opening—and a need—to develop a more flexible theoretical framework.

How might such a framework look? In Figure 1, I offer a diagram of how one might think of the process that produces changes in policy. Though simple, this figure is able to accommodate much of the existing literature on policy reform, and it is useful for highlighting how that literature might provide a foundation for future work. Moving from left to right, the diagram begins with whether or not a given leader’s preferences are orthodox or heterodox. Assuming for the sake of simplicity that a leader will push hard in her preferred policy direction, the extent of policy change in that direction is determined by how much resistance the leader faces in establishing her ideal policy. It is on this “conflict” phase that most existing work on the political economy of reform has focused. This research has generally centered either on (a) how
differing preferences in society generate conflict that hinders reform, or (b) how institutions can alleviate that conflict.

Figure 1: A simple model of policy change

With regard to how differing preferences in society generate conflict that hinders reform, the existing literature has indicated that the structure of relevant social and political clashes can take a number of forms, any one of which can block policy reform in a given issue area even if the government is committed to change. Joan Nelson, for example, enumerated many of the groups affected in different ways by market-oriented reform. The divisions include informal versus formal sector workers; public versus private employees; subsistence farmers versus commercial agricultural laborers; and the lowest classes versus urban middle sectors versus upper-middle class and elites.

However, the reform literature focused on institutions has illustrated that the mere existence of these social divisions does not mean that they will actually become manifest. The bulk of this work has focused on the ways that political institutions—such as regime type, executive-legislative relations, and the nature of the party system—do or do not enable interest
groups to resist reform.\textsuperscript{54} In addition, a smaller body of work has argued that institutions can facilitate understanding and acceptance of reform,\textsuperscript{55} an argument found in Rodrik’s work.\textsuperscript{56} In either case, the frontier of this literature exactly parallels that set out by Rodrik with regard to institutions and growth—a better understanding is needed regarding how the desired function of institutions translates into the form they should take. In other words, more work is needed on how particular institutions do or do not help manage particular kinds of conflict. As Barbara Geddes wrote many years ago, “Political institutions that can be expected to contribute to successful liberalization in one country with a particular distribution of interests would not be expected to have the same effects in a country with a different distribution of interests.”\textsuperscript{57} Unfortunately, little work has shed light on these complementarities. As Helen Milner wrote, “Theories that incorporate both preferences and institutions seem most valuable, since we know that both matter. Very few studies, however, bring together theories of preference formation and institutional influence.”\textsuperscript{58} Some exceptions exist, but this remains an important area for future work.\textsuperscript{59}

More fundamentally, given the waning of the Washington Consensus, the literature on economic reform must evolve to account for the likelihood that market-oriented policies are not public goods, and that well-intentioned leaders may choose heterodox policies. In simple terms, the existing literature can be characterized as having only focused on the top half of Figure 1—where leaders have orthodox preferences—with little to say about what might happen in the bottom half. It is unclear, however, that political conflict occurs in similar ways when the reform initiative is heterodox. And even more fundamentally, if it cannot be assumed that a leader’s preference should be orthodox, understanding a leader’s preferences about policy becomes critical to understanding the direction in which reform will go.\textsuperscript{60}
With regard to this last point regarding leader’s preferences, some scholars have examined the role of external forces, such as the World Bank, the IMF, and the overall dominance of orthodox development ideas. Others have emphasized how leaders are influenced by their core constituency groups. Still others have emphasized internal factors, such as the confidence a leader has in his or her economic team, the composition of that team, or even the ideology and beliefs of the leaders. In general, a leader’s preferences are probably determined by some combination of the nature of that leader’s supporters and what policies the leader thinks will benefit them most. However, we are far from understanding in a systematic way how leaders make this link. As Milner wrote in her review of theories of trade policy reform, “Our models of… preferences [of policy makers] seem the most underspecified and post hoc. There are few theories about the conditions under which policy makers will abandon ideas that produce bad results and what ideas they will adopt in their stead.”

Covadonga Meseguer’s *Learning, Policy Making, and Market Reforms* is a worthy attempt to fill this lacuna. The title of Meseguer’s book suggests that she focuses exclusively on market-oriented reform; but more than most works, her study is really about reform, in either an orthodox or heterodox direction. In four chapters, she examines countries’ decisions to liberalize trade, privatize, open their capital accounts, or enter agreements with the IMF. However, in three of these chapters, she also examines countries’ decisions to move in the opposite direction: deciding to nationalize industries, close capital accounts, and leave agreements with the IMF. In terms of Figure 1, therefore, she is seeking to explain why leaders are in the top half of the figure (orthodox preferences) or in the bottom half (heterodox preferences).

The message of Meseguer’s book is that an important part of this explanation lies in countries’ learning. Interestingly, just as the discussion regarding development policy has
moved to topics of experimentation, scholars have been taking increasingly sophisticated approaches to the topic of how leaders learn from others.\textsuperscript{67} Meseguer builds on this literature by developing a theory of leaders as “rational learners,” meaning that they have preconceived beliefs about the mapping from policies to outcomes, and that they update those beliefs in response to new experiences and information. Leaders may have their preconceived notions (e.g. ideologies), but those notions are not rigid. If new experiences and information consistently contradict those notions, leaders’ ideas about policies can change. Her theory is nuanced enough that she can accommodate the various degrees to which leaders respond to their own country’s experience with policies, the experiences of other countries in their region, and the experiences of countries around the world, as well as the difference between “normal” times and times of crisis, when attention to new information is heightened.

Despite its many qualities, however, Meseguer’s book commits the opposite error committed by the “conflict” literature on market-oriented reform discussed above. Instead of assuming the direction of preferences and focusing on what blocks market-oriented reform, she essentially assumes that nothing will block policy reform, and that policies are directly determined by leader’s preferences. That is, as opposed to ignoring the first part of Figure 1, she ignores the part about political conflict. She does not do this explicitly, but this approach underlies her statistical analyses, which study the correlation between her measures of leaders’ beliefs and policy outcomes without considering possibly intervening social or institutional variables.\textsuperscript{68} This is equivalent to expecting that learning always has the same average effect on the probability of policy change. However, for all the reasons discussed above regarding resistance to reform, it is clear that policy change is less likely in certain circumstances than others, regardless of how much governments have learned. Because Meseguer conflates these
scenarios, there is a concern that she is not correctly estimating the effect of learning or leader’s preferences.\textsuperscript{69}

The next step in the research agenda on the political economy of reform is therefore to begin to bring these two camps of literature—on leaders’ preferences and conflict over reform—together in a more synthetic way, so that the various steps in the chain represented in Figure 1 are included. The demise of the Washington Consensus has revealed a major shortcoming in much of the work on reform that assumed Washington Consensus policies had the properties of public goods. However, that work may still hold important lessons for those scholars working on leaders’ preferences and the effects of learning about policy. A better comprehension of economic reform will only occur when an understanding of why leaders choose the policies they do is combined with an understanding of the conflict potential reforms generate.

\section*{IV. Conclusion}

In the mid-1970s, just before the ideological swing began that would result in the Washington Consensus, Robert Ayres published an article in the \textit{American Political Science Review} entitled “Development Policy and the Possibility of a ‘Livable’ Future for Latin America.”\textsuperscript{70} It is an interesting article to revisit in today’s context, as Ayres catalogs the “legacy of problems” that state-oriented development strategies produced, focusing on problems with growth rates, employment, and income distribution. In several places, it seems one could substitute “Washington Consensus” and have a recent article cataloging the failures of the more recent paradigm. The paper is also notable because Ayres—a proponent of focusing on issues of income distribution over growth—cites several of the most prominent proponents of state-led development as being similarly disappointed with the results, calling for such things as “a new
type of development in Latin America,”71 a “new development strategy,”72 and “imaginative use of other policy tools.”73 Ayres notes, “What is interesting, however, is that [none of these authors] … is at all precise about what the ‘new type of development’ or ‘the new developmental strategy’ or the ‘imaginative policy tools’ might be.”74

The parallel is clear between that time of rethinking development strategy and the current one reflected in the books reviewed here. What is unclear is whether the Washington Consensus will be replaced by some new consensus (as happened with state-led industrialization), or whether experimentation will continue for years or decades, with wide variation in the directions that policy reforms take. The argument of this review has been that theories of policy reform should be able to account for reform under either of these scenarios. Future theories of reform should be judged not on how well they predict and explain market-oriented reform, but rather on how well they predict and explain all kinds of reform. From this perspective, it seems unlikely that assuming one kind of reform has the properties of a public good will be productive.

This review has argued for the benefits of a more synthetic approach to the study of reform, incorporating not only the existing work on how social conflict and institutions interact with one another to hinder or enable reform, but also analyses of how governments form preferences about which policies they want to enact. More work is needed not only theorizing about the causal mechanisms underlying these different aspects of the reform process, but also their interaction. This theoretical and empirical undertaking will no doubt be challenging, but the enterprise should leave us with a far richer—and more accurate—understanding of how the policy reform process really works.


The four Nobel-Prize winners are Robert Solow and Michael Spence (on the Commission on Growth and Development) and Paul Krugman and Joseph Stiglitz (in the Serra and Stiglitz volume).


18 In fact, at times, the coding of these syndromes, and finding that they lead to less growth, seems to border on tautology: “A behavior pattern within which several countries achieved sustainable growth could not reasonably be regarded as a syndrome.” Collier and O’Connell, “Opportunities and Choices,” 90.


20 Ibid., 22.

21 Ibid., 22-23.


32 Collier and O'Connell, "Opportunities and Choices."

33 Ibid.


38 Commissioners included Montek Sing Ahluwalia from India, Kemal Derviş from Turkey, Pedro-Pablo Kuczynski from Peru, Trevor Manuel from South Africa, Ngozi N. Okonjo-Iweala from Nigeria, Robert Rubin from the U.S., Ernesto Zedillo from Mexico, and Zhou Xiaochuan from China.


46 Ayres, "Development Policy and the Possibility of A "Livable" Future for Latin America."


49 Hellman, "Winners Take All: The Politics of Partial Reform in Postcommunist Transitions."


56 Rodrik, One Economics, Many Recipes: Globalization, Institutions, and Economic Growth.


Geddes, "How Politicians Decide Who Bears the Costs of Liberalization."


In statistical terms, she does not interact her belief variables with these moderating variables. For example, if leaders in dictatorships on average face less resistance to their policy preferences, then we would expect learning to have a systematically greater effect in these regimes. If this is true, Meseguer is probably underestimating the effect of learning in dictatorships, and overestimating the effect in democracies, because she does not interact the learning variables with a variable that measures political regime.

Ayres, "Development Policy and the Possibility of A "Livable" Future for Latin America."


Ayres, "Development Policy and the Possibility of A "Livable" Future for Latin America," 507.