

Special Section: Political Economy and the Future of Latin American Politics

The field of Latin American politics has undeniably achieved major advances in the last several decades. Nevertheless, one detects a growing intellectual unease and a sense that the excitement engendered by the pathbreaking work and heated debates of previous decades—focusing on authoritarianism, democratization, and market restructuring and related structural transformations—has waned and perhaps given way to a certain “normalization” of our intellectual enterprise.

To explore the current status of political science research on Latin America at this critical moment, I am very pleased for LAPS to publish a provocative essay, “Latin American Political Economy: Making Sense of a New Reality,” by Juan Pablo Luna (Universidad Católica de Chile), María Victoria Murillo (Columbia University), and Andrew Schrank (University of New Mexico). Luna, Murillo, and Schrank have each made signal contributions to recent comparative politics scholarship on topics such as representation, citizenship, labor unions, inequality, development, and the state. Now, they join together in a strong critique of much currently fashionable work in Latin American politics, which they contend promotes a “social science that rewards the testing of context-free theories and disdains the derivation of context-sensitive” research. They go beyond diagnosis to call for the revitalization of a more “grounded” Latin American political science that is more capable of theorizing and guiding empirical research in the early twenty-first century.

To reflect on the provocation issued by Luna, Murillo, and Schrank’s manifesto, I have invited brief critiques from Tulia Falleti (University of Pennsylvania), Evelynne Huber and John D. Stephens (University of North Carolina, Chapel Hill), Fernando Limongi (Universidade de São Paulo), Alfred P. Montero (Carleton College), Kenneth Roberts (Cornell), and Ben Ross Schneider (MIT). While sympathetic to Luna, Murillo, and Schrank’s manifesto, these leading political scientists nevertheless raise doubts and advance friendly amendments. Offered the opportunity to respond to their critics, Luna, Murillo, and Schrank, in effect, opt to *redoblar la apuesta*. They decry what they view as the deleterious consequences stemming from the pervasive and frequently uncritical or unthinking hegemony of rational choice and institutionalist theories and attendant methods dominating the mainstream of political science as practiced in the United States and increasingly by younger scholars at many of Latin America’s leading universities. They insist that they are not interested in new (or old) orthodoxies, and they are agnostic about specific methodologies or theoretical approaches. To the contrary, Luna, Murillo, and Schrank advocate a revitalization of the study of Latin American political economy, and indeed of comparative politics and of political science as a discipline, premised on a vigorous and open-ended debate among scholars of all persuasions.

My hope is that the debate launched here in the pages of *LAPS* by Luna, Murillo, and Schrank and their critics will continue at the upcoming 2014 LASA meeting on the panels on “New Areas of Research in the Political Economy of Latin America” and “Is It Necessary to Revitalize Latin American Political Economy?” as well as at the first annual meeting of REPAL (Latin American Political Economy Network), to be held June 9–10, 2014 at the Universidad Católica de Chile, in Santiago. For more information on the upcoming REPAL conference, see <http://redconomiapoliticaamlat.com>.

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Editor

Latin American Political Economy: Making Sense of a New Reality

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This brief article is designed to highlight an unfortunate discrepancy in the field of Latin American political economy. The field's *raison d'être* has never been more compelling. Latin American societies have simultaneously been experiencing profound transformations—including democratic consolidation, demographic transition, and the growth of identity politics—and a palpable sense of *déjà vu* animated by the recovery of commodity prices and the return of populism. In a nutshell, the region is undergoing a deep economic transformation, which takes place in the context of unprecedented levels of political participation.

The economic transformation is undoubtedly reshaping the ways societal, political, state, and business actors interact in the national, subnational, and international arenas. Yet against this backdrop, the field itself is in crisis, for the putative heirs to the legacy of scholars like Albert Hirschman, Guillermo O'Donnell, and Fernando Henrique Cardoso have all but abandoned their methods—leaving “serious research gaps in the study of a more grounded political economy in Latin America in the early twenty-first century” (Gwynne 2004, 244).

What specific events and processes would a more grounded political economy explain? Although the debt crisis of the 1980s ushered in a “lost decade” that undercut stability and augmented inequality throughout the region, the late twentieth century as a whole constituted a critical juncture in which broadly convergent political and policy transitions—toward democracy and free markets—gave rise to different outcomes on the two sides of the Panama Canal. On the northern side lies a revitalized regional economy in which Mexico and Central America supply the United States and Canada with labor-intensive manufactured goods and services—not to mention illicit drugs and immigrant labor. On the southern side lies a commodity export boom driven by Asian demand for food and minerals, which sparked particularly strong growth in the early twenty-first century. “The recession that began in late 2008 clearly curbed these growth trends,” explain Luis Bértola and José Antonio Ocampo, “but had much less of an impact than the two preceding crises had” (Bértola and Ocampo 2012, 250).¹

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Nevertheless, the divide is political as well as economic, for the northern route to globalization has been largely—if by no means exclusively—forged by conservative regimes in deference to U.S. authority, and the southern route has, for the most part, been plowed by left-leaning governments that aspire to autonomy and—in the case of Brazil—regional hegemony. Regardless of which path they have chosen, however, most of the region's polities are now procedural democracies that offer their citizens the opportunity to make claims in a way that was not possible in the previous wave of globalization, at the onset of the twentieth century, or in the subsequent decades of oligarchic or authoritarian rule.

These opportunities not only increase voice and representation—albeit in differing degrees and manners in different times and places—but simultaneously place pressures on policymakers, who are suddenly held to greater standards of responsiveness by their constituents. Some of the consequences include the expansion of women's and minority rights; the diffusion of new social policies, including conditional cash transfers like Bolsa Familia in Brazil and Oportunidades in Mexico; and a noticeable, if modest, decline in income inequality. But there are, in all likelihood, other responses that have not yet been mapped, as well as demands that have gone unrepresented. Take, for example, the revival of primary product exports from South America. While mining, hydrocarbon exports, and the shift from extensive to intensive agriculture have contributed to an economic boom, they have simultaneously transformed the region's socioeconomic and class structures—in both rural and urban areas—in unforeseen ways. Nor are they alone. The growth of trade and investment between Asia and Latin America is no less consequential for the latter region. The cultivation and trafficking of narcotics are not only destabilizing in their own right but are concentrated in countries that are already confronting significant—if by no means uniform—political and administrative challenges. And economic shocks like these are likely to interact with policy and institutional reforms introduced in the 1990s (e.g., trade liberalization, privatization, decentralization, and social service reform) to produce particularly unanticipated and variable outcomes.

Unfortunately, however, social scientists have done little to generate empirically grounded studies that can help us understand, let alone respond to, Latin America's transformation. On the contrary; they have begun to import alien theories and methods that are unlikely to inform policymakers or practitioners in the region. For instance, Latin Americanists have imported rational choice accounts that place the individual front and center and have driven the organizations that shape individual behavior—e.g., firms, unions, business associations, social movements, and political parties—to the sidelines (Evans 1995); sociological accounts that presuppose the importance of “culturally specific practices” (Hall and Taylor 1996, 946) without paying much attention to Latin culture (Centeno 2007); institutional accounts that assume that rules and procedures are stable and binding when in much of the region they are neither (Levitsky and Murillo 2009); and experimental and quasi-experimental methods that prioritize the estimation—and downplay the generality or importance—of causal effects (Huber 2013, 1–3). The consequences are potentially disastrous, for history suggests that by taking Latin America on its own terms, we

will simultaneously improve our understanding of the region and enhance our ability to contribute to comparative social science; but that lesson is at risk of being forgotten by scholars who are in thrall to a “universal” standard of theory, method, and—perhaps most important—graduate training.

THE DECLINE OF LATIN AMERICAN POLITICAL ECONOMY: WHY HAS IT VANISHED WHEN WE NEED IT MOST?

Why has political economy fallen out of fashion in the region at precisely the moment when it has so much to offer? Several candidate explanations come to mind, and we suspect that it is their interaction that has conspired to marginalize political economy in Latin America.

First, the demise of authoritarian rule has brought the region back into the social scientific mainstream. When Latin American polities were subject to military or civilian dictatorship, their observers were all but unable to import theories that had been designed with liberal democracies in mind, and therefore had little choice but to develop grounded approaches of their own making. With the consolidation of Latin American democracy, however, Latin Americanists were allowed to import foreign theories, and in so doing, to speak to broader audiences. By the dawn of the twenty-first century, therefore, Guillermo O’Donnell would acknowledge that “new approaches, most of which originated in the United States, are highly influential” in the region. “In particular, research on some institutions that in many countries barely existed, if at all, before the demise of authoritarian rule (such as parliaments, political parties, and party systems) has been conducted vigorously” (O’Donnell 2001a, 810).²

Second, ready-to-use datasets and software that allow for the testing of universal theories are more common and inclusive than they were in the past. For instance, the World Bank has “begun to position itself as a knowledge Bank” (World Bank 2011, 1) by making a number of global datasets available. Other donor agencies and multilateral organizations have followed suit. And public opinion surveys of Latin American populations are growing more common and less costly by the year.

Third, cost and publication pressures have conspired to make the utilization of prepackaged data an attractive alternative not only to intensive fieldwork (see, e.g., Stevenson 2005, 15) but to the interdisciplinary training that is all but required of political economists. Graduate students, in particular, are warned that interdisciplinary training will increase their expected “time to degree” (Glasgow 2008, 10), and their departments are understandably wary of the attendant increase in costs.

Fourth, the “counterfactual” view of causality has taken hold among academic as well as policy-oriented social scientists. Graduate students are taught not only that the isolation of causal effects is their *raison d’être* but that there can be “no causation without manipulation” (Holland 1986), and their (increasingly experimental) methods therefore begin to drive, rather than to derive from, their questions.

And finally, the globalization of social scientific training, publication, and careers ensures that these trends reverberate throughout the hemisphere. While

O'Donnell's northern colleagues rejected "the ethnocentric view that the role of 'the natives' was to furnish data for theories to be made in the North" (O'Donnell 2001a, 810), their successors are in many ways less cosmopolitan (O'Donnell 2001b, 27) and more influential (Anderson 2003; Hall 2007), and the disciplinary biases that have devalued the study of political economy in North America therefore threaten to bear similar consequences in the South.

The result is a social science that rewards the testing of context-free theories and disdains the derivation of context-sensitive ones. While the questions thereby addressed are not necessarily relevant to Latin America, the results—whether compatible or incompatible with existing theory—are likely to be highly publishable, especially if they are the products of "sophisticated" methods. And contemporary Latin Americanists are therefore less likely to look for primary sources with which to develop new conceptual or analytical tools than to test theories imported from other regions with datasets compiled from available sources and methods that are increasingly baroque.

Our point is neither to dismiss the importation of alien theories and methods *tout court* nor to belittle the work of scholars who are legitimately uninterested in regional specificities. We have no interest in substituting one orthodoxy for another. But the social sciences are currently skewed toward the general and away from the specific, and we therefore wish to highlight the persistent need for research that takes regional political economies on their own terms by collecting new data, generating original hypotheses, and formulating and testing the sorts of context-sensitive but comparative theories that are indispensable to policymakers and ill suited to experimental evaluation.

REVITALIZING LATIN AMERICAN POLITICAL ECONOMY: TOWARD A VIABLE ALTERNATIVE

Our goal is to promote empirical research that will lead to the formulation and evaluation of new theories that are both context-sensitive and subject to broader comparisons, and our methodological posture is therefore eclectic. We believe that the questions that animate our research should be of practical as well as academic interest and should drive, rather than derive from, our methods.

Our push is for empirically based research that seeks to produce new theories to be empirically tested (and contrasted with those produced elsewhere). The emphasis on empirically grounded theory generation and testing involves an attitude of methodological eclecticism, in which the diversity of methods driven by the need to understand the problems at hand overcomes the tendency to select problems to study based on the tools for answering them. As we recognize the value of serious methodological training, we believe that methodological diversity calls for more collaborative work in which the common empirical interest of scholars is magnified by the diversity of their tools to understand their puzzle of interest.

Collaboration is also crucial in diminishing the compartmentalization produced by specialization as our knowledge of any particular topic moves forward. While spe-

cialized studies have contributed in many cases to the depth of knowledge on a given topic, we want to be able to establish connections between political institutions, the process of representation, and structural changes in the economy driven by technological innovation and international shocks. To avoid simplification, we need to rely on collaborative teams that can, at the same time, look in depth at particular processes in particular contexts but also collaborate on comparative studies that generate theory that is both policy-relevant and useful to the lives of citizens in the region.

We are seeking to produce comparative work of a regional scope that also contributes to our general understanding of the tensions generated by development and democracy. Latin America is a region not only in a geographic sense but also in a cultural sense, given common colonial legacies from the Iberian Peninsula and a common postcolonial history that produced international borders cutting across prior precolonial and colonial divisions. Whereas this sense of regional belonging has contributed to both a regional worldview and external images of the region, it underscores the common processes of experimentation that the region has experienced and the importance of generating region-specific knowledge of policy relevance.

The research we have in mind is necessarily cross-disciplinary, but the disciplinary knowledge it demands is not necessarily being produced—partly because the disciplines that have traditionally contributed to political economy are focusing their efforts in other areas. Political scientists—seeking to understand how democratic politics work—have focused on political institutions and electoral behavior to a much larger extent than on political economy. Sociologists tend to operate on a macroscale that pays less attention to the sorts of differences and details we have in mind. Historians have been emphasizing cultural history at the expense—rather than to the betterment—of economic history. Economic anthropology is in decline. And economists themselves—including most development economists, who could, in theory, produce the tools necessary to understand Latin American political economies—tend to value abstract theoretical models and methodological innovations more than applied work.

Young scholars throughout the social sciences therefore have little incentive to invest the time necessary to understand the complicated relationships involved in political economy—at least not if they want to have successful disciplinary careers. But as the current generation ages into retirement, the field will lack both political economists with knowledge of Latin America and the institutional infrastructure needed to train new ones. More important, these deficits are emerging at precisely the moment when the simultaneous economic divergence (toward manufacturing and services in Mesoamerica and primary commodity production in South America) and political convergence (toward more inclusive democratic processes across the region) will demand sensitivity to local context. We are therefore proposing efforts to revitalize a rigorous, comparative, and empirically grounded political economy of Latin America that would address—but not be limited to—the following issue areas and questions:

Patterns of democratic politics. In the years before the Latin American debt crisis, political economists developed powerful tools and theories with which to make

sense of the region's politics. Latin Americanists introduced concepts such as bureaucratic authoritarianism, organic statism, and reactionary despotism, for example, and debates over patronage, populism, and the like reached their apogee in the region. Are Latin American political economists making equally valuable contributions today? If so, what are they? If not, why not? And what might be done about it?

Dependency and development. Perhaps no field in the social sciences has been more influenced by Latin American debates than development theory. Latin Americans introduced dependency theory in the mid-twentieth century. They provided the focal point for the so-called Washington Consensus at the end of the twentieth century. And in between, they paid careful attention to the political economies of natural resources and sectoral imbalances, and in so doing laid the foundations of what would later be christened the "resource curse." What is the status of Latin American development theory today? Does the region still offer general lessons? If so, what are they? If not, why not? And what might be done about it?

State formation and the rule of law. Latin American states have been derisively portrayed as "paper Leviathans" (Centeno and Ferraro 2013): imbued with enormous powers in theory, but unable to make use of them in practice. Issues of state formation are central to debates over corruption, crime, and violence, on the one hand, and to the broader politics of institutional weakness, on the other. What do we know about Latin American states and their influence? What should we know? Why is our knowledge lacking? What might be done about it?

Inequality and identity. Inequality is not only central to debates over social justice but is also a prominent source of social mobilization. Latin America has been widely portrayed as the most unequal region in the world, and inequalities of gender, sexuality, race, ethnicity, and nationality have recently begun to capture attention that had previously been reserved for more traditional class inequalities. Which inequalities are most central to Latin American political economy today? Which inequalities have taken a backseat? Do the answers vary by country or regime? Why or why not? Where should students of inequality focus their efforts? Why?

International linkages. Mexico and Central America have always been subject to heavy-handed U.S. intervention, and during the Cold War era Latin America as a whole was frequently dismissed as "America's backyard." In the post-Cold War era, however, U.S. influence over South America has declined, as the revival of commodity exports has gone hand-in-glove with a turn to Asian markets and the consolidation of Brazil's role as a regional power. U.S. influence over Mexico and Central America is more likely to be rooted in economic ties than military troops. What are the sources and implications of these developments? Are regional institutions, powers, and trade and investment agreements having tangible effects on Latin American political economies? If so, how? If not, why not?

These questions have, for the most part, gone unaddressed by contemporary scholars, and they are unlikely to be addressed by scholars who remain wedded to currently regnant theories and methods. Rational choice approaches are insensitive to culture, after all, and are unlikely to shed light on either the "noncontractual ele-

ments of contract” that are indispensable to functioning markets or the “nonbureaucratic elements of bureaucracy” (Evans 1989, 573) that are indispensable to functioning states. Alternatives to rational choice accounts rarely map to existing data sources or allow the manipulation of proposed “causes” that is indispensable to the experimental approach. And the revitalization of Latin American political economy will thus demand the kind of intensive fieldwork and middle-range theorizing that are at risk of becoming lost arts.

Our goal, however, is not to replace a particular theoretical or methodological fashion with another. Instead, we hope to develop social scientific knowledge that is relevant for understanding the political and economic changes experienced by Latin America in recent years. The goal is thus twofold. On the one hand, we want to generate knowledge that accounts for the most decisive aspects of the region’s current situation and is useful for advancing practical discussions that are altogether too often uninformed by systematic and empirically grounded knowledge. On the other hand, we want to promote a rigorous comparative understanding of the problems generated by the dynamic tension between democracy and development, and in so doing, to account for the rich and evolving experience of Latin America.

NOTES

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1. See Eva Saiz’s interview with Ricardo Lagos (2013) for a more recent discussion of the northern and southern trajectories.

2. For a more sanguine take on the same process, see Ames 1996, 11, and the powerful rejoinder by Anderson (2003).

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Comment on
*“Latin American Political Economy:
Making Sense of a New Reality”*

Evelyne Huber
John D. Stephens

We are in full agreement with the basic argument that we need a better understanding of economic and political structures and processes and their inter-relationships. We are also in agreement with the critique of much recent political science and economics research on the region, with its overemphasis on models of rational actors and their preferences, be they voters or consumers or investors, and its neglect of collective actors and power relations. Simply put: policy preferences are highly imperfect predictors of votes, since many policy issues and other factors (like retrospective pocketbook voting) influence votes; and votes are highly imperfect predictors of policy, since governments face all kinds of constraints in translating their own preferences and promises into policy. Therefore, we need to return to a focus on the macro political economy, where the key protagonists are the state, social classes, business, unions, and political parties.

So what are the key questions on a political economy research agenda, and what kinds of literature should be built on? Arguably, the keys to success in economic and human development in the medium and longer run are industrial upgrading, tax reform, and human capital formation. Industrial upgrading should generate better jobs and more wealth; tax reform should channel a larger share of that wealth to the state; and state investment in health, education, and training should provide the labor force for a more productive economy and improve the quality of life of citizens. Social science should elucidate pathways to these goals.

Various governments have recognized these imperatives, but why has progress been so slow? A tremendous concentration of ownership in the economy has occurred over the past two decades, and the major investors have holdings in multiple sectors. Does this diversification make it unnecessary for them to invest in new, more advanced sectors? Are they simply too comfortable to respond to government incentives? As to foreign investment, are governments not trying hard enough to attract such investment in technologically advanced sectors, or is the labor force in most Latin American countries inadequately prepared? If so, does investment in human capital have to precede industrial upgrading? We certainly need more research on changing structures of ownership, production, employment, and invest-

ment decisions, in order to identify possible points of leverage for governments to stimulate higher value added and higher wage production. This means that we need to build on the literature on models of capitalism and on the capitalist class (Schneider and Soskice 2009; Zeitlin and Ratcliff 1988).

By the same token, we need a better understanding of the new developmental state. In the new information and knowledge economy, the key to success is not just physical and financial capital but also human capital. So investment by the state and by joint ventures in infrastructure and natural resources has to be complemented with investment in human capital. How can the state bring those forms of capital together and foster productive collaboration? How can the state bring into the relationship stakeholders beyond business, such as universities and research institutes? Here we can build on the newer literature on the developmental state (Evans and Heller forthcoming).

As noted, one of the central challenges is to strengthen the extractive capacity of the state. This is primarily a political problem, requiring the formation of political alliances around fiscal policies, including both the tax and the spending side. Given the highly skewed income distribution in Latin America, the ability to tax the top decile of income earners effectively is crucial. Forming a numeric majority coalition in support of this proposition should be easy, but the problem is one of differential power, particularly power of the purse strings for campaign financing. With the costs of Latin American election campaigns soaring and regulation sorely lagging behind, financial support from high-income earners—be they individuals or decisionmakers in corporations—is crucial and affords these actors an entirely disproportionate degree of influence on the political agenda. The literature on campaign financing and regulation is in its infancy in Latin America and needs to be built up (Garretón 2005; Fuentes 2004). The issue of campaign financing is intimately related to another important topic of political economy that is underresearched: the structure of media ownership and the role of the media in forming public opinion and reflecting it back to politicians.

Looking at political economy from the other side, the side of the less powerful, there are crucial questions to be investigated as well. Under what conditions can popular protests and pressures stimulate positive responses? How can they strengthen political actors committed to a progressive agenda in the sense of investment in the human capital and welfare of the lower two-thirds of the population, and when do pressures generate defensive reactions? How can effective alliances be built? Here the literature on social movements (new and old) and on state response to popular pressure is relevant.

Let us end these brief comments with some observations on theoretical and methodological issues. We are convinced that there is much to be gained from taking good theory developed in other contexts and adapting or modifying such theory to the Latin American context. This, of course, runs counter to the orthodox view of theory testing that emphasizes deductive hypothesis development and confinement of analytic procedures to rejection of the null hypothesis. However, it is consistent with a view of scientific procedure that emphasizes dialogue between

theory and data and the aim to explain unexpected results. This second view is often made explicit in comparative historical work, whereas purely quantitative or formal work tends to be presented in the orthodox fashion, even though there may be, as the saying goes, “thousands of regressions for every one that is presented in the final product.” While we remain advocates of a mixed-method approach, we would argue that for the big questions of political economy that involve power relations among collective actors and structural constraints on political choices, a comparative historical and structural approach is fundamental.

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The Latin American Way

Fernando Limongi

Murillo, Schrank, and Luna opened up Pandora's Box. From time to time, scholars need to be warned about what they are doing. They sounded the alarm, and we who study or live in Latin America should thank them for calling us to reason. They are right to call our attention to our past. We are heirs of Hirschman, O'Donnell, Cardoso, and others. Moreover, as the mere reference to these names makes clear, we are members of a community in which political engagement is a must. Latin Americanists are politically committed to the future of the region they were born in or professionally adopted. We study this region because we care about the future of those who live in it.

As the authors emphasize in their opening paragraph, "the region is undergoing a deep economic transformation, which takes place in the context of unprecedented levels of political participation." Therefore, reflection is in order. We need to pause and know why we study what we have been studying. Moreover, we need to be aware of the consequences of the methods we employ. If, in fact, the region is going through a deep transformation, then this is the right moment to analyze Latin America; what it used to be, what it is now, and what it should be in the future. One cannot miss noticing that not all these transformations point in the same direction. For instance, democratic consolidation, demographic transition, and reduction of inequality may indicate a reduction of the North-South divide, whereas the growth based on commodities and the return of populism may be read as pulls in the opposite direction.

Space limitation requires me to be sketchy. In doing so I cannot avoid simplifying their propositions and so I choose, quite arbitrarily, to focus on just some of them. Their account of the area distinguishes three moments: a bright past, a dark present, and a future open to intervention. Obviously, they write to influence the development of the latter, to put the area back on the right track. As I read it, the reorientation they propose is mainly methodological.

The way one reads their paper and processes their recommendations depends on the emphasis one puts on the different sections of the paper. There is, I would argue, an imbalance between the opening and the final sections. The opening section is bold. There is a clear recommendation, a direction Latin Americanists should take, one that is conditional on the region's specificities. The closing section, in turn, is more moderate in tone. Much of what is presented in this section revises the bold statements made earlier, calling for an eclectic approach, highlighting the need "for empirically based research that seeks to produce new theories to be empirically

tested,” a call for interdisciplinary research and the production of “comparative work of a regional scope and that also contributes to our general understanding of the tensions generated by development and democracy.” The list of substantive questions they present at the end does encompass most of what we have been doing and should be doing as a way to get a “rigorous, comparative, and empirically grounded political economy of Latin America.”

I have little—if any—disagreement with this third and final section. I fully agree with their methodological call and their substantive program. In my view, though, this program contradicts the bold calls made in the first section. If we were to follow the recommendations of the first section, I am afraid, we would jeopardize the nice program outlined at the end. Let me be more precise on this.

The closing paragraph of the opening section presents a diagnosis of the current state of affairs of the Latin American field. It has failed to “generate empirically grounded studies” that help the understanding of the region’s transformation because (the “because” is mine, but I think it is implicit) Latin Americanists “have begun to import alien theories and methods that are unlikely to inform policymakers or practitioners in the region.” Alien and imported theories and methods are the culprits: rational choice has led researchers to downplay or ignore entirely the importance of organizations that determine individual behavior; those using sociological approaches have not paid due attention to Latin culture; neo-institutionalists base their research on the supposition that “rules and procedures are stable and binding when in much of the region they are neither”; and those who employ experimental and quasi-experimental methods prioritize estimating causal effects over establishing the importance or generality of what they find.

It is not clear to me in what sense these methods (rational choice, sociological approaches, neo-institutionalism, or experimental methods) were imported to the region. One may agree that rational choice fails to explain some things that we would like to understand, that sociological analyses tend to emphasize structure too much, or that experiments are short on external validity. But these problems do not arise only when they are applied to study issues raised by or in Latin America; these are deficiencies of the methods, not of the raw material generated in the region to which they will be applied. Latin America is not unique in the sense that we need to study it in a way that differs from the way we (and others) study other parts of the world.

Thus, it is not clear to me if these criticisms are conditional or unconditional. Is rational choice a bad method and theory, or is rational choice bad only for studying Latin America? Does neo-institutionalism assume too much about institutions, or do the institutions it assumes about simply not exist in Latin America? Rational choice, neo-institutionalism, and experimental methods may be the wrong way of doing sound research, but if this is so, it holds true both in Latin America and elsewhere. These methods and theories may fail or may not provide scholars with all the answers. That is why one should follow or be open to different methods and interdisciplinary work. One should not put all the eggs in one basket. But this should not be true only for Latin America.

One may argue that Latin America is specific; that “culturally specific practices” have developed on this part of the globe. This may be true. I would argue, however, that it does not follow that to understand the region one needs theories and methods that are specific to the region, adapted to their reality. If rational choice (or any other method) works in Europe, then it works in Latin America. If any of these approaches fails to explain relevant topics of the U.S. political economy, it fails in Latin America as well. It does not make much sense to say that we should strive to develop a Latin American way of employing rational choice models, or that we should develop Latin America-specific experimental methods.

Reality may vary, but not to the point of affecting the laws that organize it. We may disagree about the specifics of these laws; some may even doubt that they exist at all. But given that we accept that the world can be understood in terms of regularities—and the authors do seem to accept this—we cannot say that the regularities are valid in some places but not in others. Adopting a general scientific approach does require denying the existence of specificities. Latin America may be different, but in order to show that this is the case, one needs to abandon the authors’ criticisms in the first section and proceed along the lines they develop in the closing section, with their “push ... for empirically based research that seeks to produce new theories to be empirically tested (and contrasted with those produced elsewhere).” This is as nice a program as one can get. In my view, to engage in this program necessarily leads one to abandon the distinction between imported and nonimported theories and methods.

If we were to look into the past, if we were to look at the work of Hirschman, O’Donnell, Cardoso, and many others, I am not sure we would find a rejection of “imported alien methods.” Dependency theory was not a method or a theory in itself. Nor was O’Donnell’s explanation for the emergence of bureaucratic authoritarian regimes based on peculiar methods. The subject, the questions were region-specific because they addressed issues raised in Latin America; the insights were personal, but not their methods or the theories they generated. Those have been widely used to study developments in other parts of the world. In my view, the towering figures in the study of Latin American political economy that Murillo, Schrank, and Luna rightfully praise adopted precisely the kind of eclectic and interdisciplinary approach the authors recommend in the closing section of the paper. They drew from the common methodological pool available to political economists to understand the reality they faced.

NOTE

I thank José Antonio Cheibub for clarifying my ideas and comments.

Substance and Methods of a Grounded Political Economy

Kenneth M. Roberts

The plea by Murillo, Shrank, and Luna (MSL) for a more “grounded” political economy approach to the study of Latin America is much welcome, and it merits a serious debate among scholars of the region. To help advance that debate, this essay raises several issues regarding the substance and methods of the political economy they advocate.

A central concern is the ontology of political economy itself, as the term is widely but often loosely used in reference to highly disparate research agendas. Many scholars claim to “do” political economy, but they mean quite different things by it: for some it is a method, for others it provides a theoretical paradigm, and for still others it serves as a substantive focus of inquiry. Clearly, the grounded political economy approach advocated by MSL is not that of the game theorists, who seek to explain strategic political behavior through a set of concepts, methodological tools, and theoretical models imported from microeconomics. Indeed, MSL are critical of rational choice theory, as well as experimental methods, cultural sociology, and institutionalist approaches—all examples of “alien” and universalizing theories or methods that have been imported to the region without appropriate sensitivity to local context.

A grounded political economy, however, cannot be defined by what it is not. MSL’s critique of recent scholarship leaves little doubt that their goals include a methodological reorientation of the field, but the character of this reorientation is less clear than it might be. MSL decry that the heirs of Hirschman, Cardoso, and O’Donnell “have all but abandoned their methods,” but they do not really specify what those methods were, and their ultimate pitch for methodological eclecticism leaves the reader with a rather generic set of guidelines: political economy research should be grounded in knowledge of the local and regional context, driven by important real-world problems (rather than methodological preferences), and based on empirical findings. The work of Hirschman, Cardoso, and O’Donnell was surely all those things, but it was also much more; it was often macroanalytic, based on observational data, and dependent on comparative historical methods to develop and test causal inferences (when it tested anything at all). It was, in short, strikingly at odds with intellectual fashions in the contemporary disciplines of economics and political science. Furthermore, its interdisciplinarity transcended these two fields to incorporate major insights from sociology; Latin America’s “classical” political economy entailed a healthy dose of political sociology, something that regrettably few scholars claim to practice today.

This interdisciplinarity was critical, I would argue, because classical political economy was defined more by its boundary-crossing substantive focus and theoretical concerns than by any specific set of methodological tools. (Hirschman's embrace of "trespassing" said it all.) This substantive focus explored the interrelationships between market forces, class-based interests and conflicts, and political power and institutions—the natural domains, respectively, of economics, sociology, and political science. Political regimes and patterns of representation (or exclusion) were shaped by the dynamics of capitalist development and by the cycles of social mobilization and conflict it spawned. Indeed, both political regimes and development models were understood in terms of the shifting coalitions and rivalries between actors defined largely in class terms.

This theoretical alignment of social, economic, and political spheres in the canonical works on dependency and bureaucratic authoritarianism was challenged by the dual transitions to democracy and market liberalism in the 1980s. As democracy was established in countries with the most inhospitable of social and economic terrain, scholars began to emphasize political agency and the relative autonomy of political institutions from underlying structural conditions. Likewise, as market reforms spread across the region under democratic auspices—in the absence of a Pinochet's iron fist—economic adjustment came to be seen (misleadingly) not as an elite-based political project but as a depoliticized structural imperative that followed a technocratic logic. The analytical decoupling of political and economic outcomes at the macro level seemed to relegate their points of intersection to a secondary plane.

Beyond their methodological prescriptions, MSL are clearly advocating a substantive and theoretical recoupling of political and economic spheres. Such a recoupling might start by recognizing that the transition from state-led development to market liberalism in the 1980s created a new social and economic order with identifiable political correlates—correlates that were subsequently realigned after structural adjustment finally stabilized inflationary pressures and allowed the region to enter a postadjustment political era in the late 1990s. A grounded political economy approach would therefore seek to explain the emerging alignments of states, markets, and sociopolitical actors in this postadjustment (but not necessarily postneoliberal) era. It would ask how these three-dimensional alignments have shaped (and in turn have been reshaped by) the major regional trend lines of the early twenty-first century, including the commodity boom and the differential participation of countries within it, notable but tentative declines in social and economic inequalities, the strengthening of left-leaning political alternatives, the widely varying institutionalization of democratic regimes and party systems, and growing societal contestation of political establishments and liberalized or extraction-based economic orders. None of these trend lines can be understood by analytically separating economic and political spheres, much less by detaching them from their sociological underpinnings.

An analytical recoupling of political and economic spheres need not reproduce the excessive determinism of earlier macrostructural accounts. Instead, it should aim to explain the varied and contingent outcomes of political processes that are shaped and constrained by economic and social forces but that retain nonetheless a measure

of autonomy for the strategic behavior of political agents and the conditioning effects of institutional arrangements. As MSL suggest, a wide range of methodological tools can contribute to such explanations, but their leverage will be enhanced when they are empirically grounded in contextual knowledge.

Studying Political Economy in Latin America: Gaps and Methods

Ben Ross Schneider

Luna, Murillo, and Schrank (LMS) do our field a very valuable service by raising questions on the state of research on the political economy in Latin America and where it should be going. They cover a lot of ground in a short space, leaving a smorgasbord of issues to mull over, question, or endorse. To my mind, some of the most promising critiques in the memo are that there are gaps in the field and that those gaps are due in part to methodological fashion. Yet the discussion of gaps in LMS is somewhat ungrounded and underspecified. Closing the essay with a call for more research on democracy, development, state formation, inequality, and international linkages seems to miss the fact that most recent research is actually related to these five topics. Many readers probably would react by saying to themselves that they have been conducting some research in one or more of these areas.

But LMS are onto something here, so I would offer a friendly amendment to say that the gaps arise less in what we are studying than in how. Consider this hypothetical. Imagine sending new graduate students to take a comprehensive seminar on democracy in Latin America covering several decades of intense research, and then requiring them to spend a summer reading through the accumulated research archives of the Inter-American Development Bank (IDB) and the World Bank (far and away the largest funders of research on Latin America). At the end of this immersion into the contemporary study of political economy, if you asked the students, “who is Carlos Slim and what is his impact on policymaking, development, and democracy?” they would not have a clue. More important, these hard-working students would not know that every country of Latin America—even small economies in Central America—has a few dozen business groups that are smaller than Slim’s Grupo Carso but still tower over their own economies and polities (for overviews, see Bull et al. forthcoming; Colpan et al. 2010). These vast, diversified, family-owned conglomerates, best known as business groups, control large swaths of their economies and wield enormous political power. Why, then, does nearly everyone ignore them?

It is hardly controversial to suggest that big business has disproportionate political influence. It bears repeating, however, since recent general books on democracy in Latin America do not take business into account.¹ These books do not reference a substantial literature or debate on business and democracy because it does not exist. Scholars of democracy argue intensely over issues like clientelism, party col-

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lapse, and relations between the executive and legislative branches, but not about business. It makes little sense to focus exclusively on political inputs like public opinion, voting, parties, and the institutions that shape them if, in fact, policy outputs depend on other actors, like business, which distort these inputs or twist later stages of policymaking and implementation or simply prevent policies and reforms from ever getting on the agenda.

The literature on campaign finance is minuscule, even though we all know that each election costs more than the last and that virtually the only source to fund them is business and wealthy individuals. (Some pioneers include Samuels 2001; Griner and Zovatto 2004; Boas et al. forthcoming.) Most countries have high or few limits on campaign contributions, weak monitoring and enforcement capacity, or both. Moreover, family control of big business grants an added advantage, in that family owners can develop long-term relations with politicians (and bear long-term grudges against politicians who cross them).

What about multilateral institutions (World Bank, IDB, IMF, and others) in Washington? You might think that development economists would be more interested in business groups. Or that researchers concerned with market reforms or, now increasingly, with industrial policy and public-private collaboration would turn their attention to big business. For example, in the wake of privatization, many business groups expanded into public utilities, telecoms, finance, media, energy, and other regulated sectors not subject to international competition. Reformers in the 1990s often drafted regulations for newly privatized sectors in haste, but once incumbent firms figured out the benefits, they made it costly to fix the regulations. Yet virtually nothing in the vast research troves of the World Bank and the IDB analyzes business groups.

Why, then, is big business so absent in research on democracy and development? No single, simple answer comes to mind, but at least part of the explanation is related to theoretical and methodological trends. For instance, the costs of acquiring information on business are very high. It is much easier to download data on congressional votes, public opinion, elections, and institutional indices, or even run a survey or experiment, than to collect information on campaign finance, on business structure and strategies (political and economic), or on business contacts with policymakers. A related bias comes from a tradition of building up theories from individuals (methodological individualism). Such an approach sidelines theorizing about huge organizations like MNCs or business groups. Furthermore, although nearly all scholars agree that institutions matter, North's definition dominates institutional analysis (North 1990). In the Northian view, if we get the institutions—the rules of the game—right, then individuals and organizations, such as firms, will behave; so, better to study the rules than the players. Yet we neglect the players, these mammoth players, at our analytic peril.

Without incorporating big business, our understanding of democracy and development will be partial, biased, and naive. By neglecting business, theories of democracy will tend to overestimate the independence of political institutions and view politics generally as open, participatory, transparent, and open to significant

redistribution. Development theories and policies that do not take into account the structures and strategies of business groups (and their roots in oligopolistic markets) will remain abstract and ungrounded.

In sum, LMS are right to look for gaps and probable methodological culprits. However, the problem it is not that we lack research on areas like democracy and development; it is that this research is missing the most powerful and consequential players.

NOTE

1. For reviews, see Ames et al. 2012; Karcher and Schneider 2012. For a related critique of research on U.S. politics, see Pierson 2013. For recent exceptions in Latin America, see Fairfield 2013.

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Theory Production: Made In or For Latin America?

Tulia G. Falletti

The essay by Murillo, Shrank, and Luna constitutes a much-needed and welcome wake-up call for those of us who study Latin America—and for political scientists more generally. The authors make a plea for “a rigorous, comparative, and empirically grounded” study of Latin American political economy. I fully agree with their diagnosis of this field and their recommendations. I also praise the authors for defining political economy broadly—rather than narrowly, through a focus on research methods. They understand political economy to encompass all the economic, social, and political factors that are either contextual conditions or consequences of major macroeconomic transformations. Thus the authors lay out an important research agenda for the study of Latin American political economy that includes not only issues of economic development and inequality, but also patterns of democratic politics, state capacities, the rule of law, identity politics, and international linkages, among others. For the authors, the major political and economic transformations that the region has undergone since the start of the twenty-first century—in its postneoliberal era—cry out for a contextualized research agenda and, I would add, open a host of opportunities for theoretical and conceptual innovation.

In this brief reaction note, I aim to accomplish two tasks. First, I would like to take issue with what the authors appear to imply about the processes of theory production and deployment. Second, I will illustrate with an example the host of advantages and interesting questions and puzzles that emerge from taking seriously Murillo, Shrank, and Luna’s grounded approach to the study of political economy. While I agree with the authors’ assessment of what is currently considered mainstream in the study of Latin American political economy (and of political science more generally), I disagree that the geographic location of theory production is relevant. As the authors note, certain types of data proliferation and availability (such as survey data and national-level indicators), combined with the North American academy’s emphasis on deductive reasoning, are yielding research with limited capacity to address broad research questions, and add little value to our knowledge of the region. The movement toward experimental research design (whether natural or survey experiments) in the social sciences, for example, has a high cost in terms of the type of questions or puzzles that can be tackled. Meanwhile, important “observational” macropolitics problems are going grossly understudied.

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However, Murillo, Shrank, and Luna place unwarranted emphasis on the geographic location of theory production. In my opinion, where theories are generated is less important than what amounts to the status of a theory. The recent political science scholarship, for instance, in its urge to present “original theories” (original arguments or interesting findings no longer make the publication cut of the field’s top-tier journals), has demoted the epistemological status of theory. Often, what is called “theory” does not amount to anything more than a few individual-level assumptions (loosely labeled “microfoundations”) and an ad hoc model of limited relevance beyond the study from which it stems. In my view, it is irrelevant whether theories are “foreign,” “alien,” or “imported,” as the authors write (after all, Guillermo O’Donnell produced most of his scholarship outside of Latin America). Instead, we need to reflect on what is the minimal epistemological threshold that must be met when proposing a theory.

Moreover, the inductive process of theory generation needs to be granted a more prominent seat at the political economy (and political science) table, as well as the accumulated knowledge that results from applying and, when necessary, adjusting existing theories. A study of Latin American political economy that is empirically grounded and attentive to contextual causal effects is likely to inductively generate theories that can then be tested in other cases or regional contexts. As argued elsewhere (Falleti and Lynch 2009), taking context seriously requires an understanding of complex causality and the multiple pathways in which context, even under the presence of the same causes and causal mechanisms, can lead to varying outcomes. Understanding these processes and generating these theories requires in-depth knowledge of the researched cases and often intensive fieldwork or archival research.

A grounded research approach to the study of Latin American political economy can open (and answer!) hosts of interesting and politically important questions and advance our concepts and theories. This is best illustrated with an example. Among the major economic changes the region has witnessed in the recent past is the expansion of its extractive frontier. (For an excellent in-depth analysis of this process, see Riofrancos in progress.) Since the early 2000s, the Andean countries have been relying more heavily on the extraction of nonrenewable natural resources.¹ These economic changes are bringing about important social and political conflicts. Indigenous communities in areas potentially affected by exploration or exploitation of natural resources are confronting the state and corporate actors and demanding the implementation of the collective right to prior consultation before projects begin.

The study of this relatively new participatory institution, in turn, confronts scholars with questions that are impossible to answer within the conceptual framework of liberal or representative democracy. To start with, the right to prior consultation is a collective, not an individual, right. And while theorization exists in political theory regarding communal or group rights, the subfield of comparative politics lags significantly behind (for a noteworthy exception, see Yashar 2005). Moreover, the recent Latin American reality of constitutional recognition of group rights and claims, plurinationality, and legal pluralism (for an excellent comparative analysis of legal pluralism, see Hayward in progress) invites us to think differently about issues

of sovereignty and the mechanisms for aggregation and representation of interests. In the recent past, scholars of Latin America with grounded knowledge of the region have thus proposed the concept of “postliberal” democracy, which sheds significant light in terms of understanding some of its contemporary regimes (e.g., Ardití 2008; Ciccariello-Maher 2007; Schmitter 2003; Yashar 2005).

As Murillo, Shrank, and Luna argue, the study of Latin America was at the forefront of theory generation 30 to 50 years ago (Cardoso and Faletto 1979; O’Donnell 1986 [1973]; O’Donnell and Schmitter 1986). We ought to build on that research tradition while at the same time incorporating the scholarship generated in the study of other regions (such the welfare state literature on Western Europe or the authoritarianism literature on the Middle East). I disagree that useful theories must be regionally bounded or domestically generated. I wholeheartedly agree, however, that useful theories need to stem from the rigorous, comparative, and empirically grounded knowledge of our research cases.

NOTE

1. In Bolivia, for example, the government’s royalties from hydrocarbons grew sixfold between 1996 and 2011 (Falleti and Riofrancos 2013). In Ecuador, revenues from hydrocarbon exports also grew sixfold during the same period (Ruiz Acosta and Iturralde 2013, 94, 96).

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Comment on “Latin American Political Economy”

Alfred P. Montero

I am partisan to the general view that political economic research questions, approaches, and insights that were once fruitful in the field of Latin American politics and development have been marginalized. I use the passive voice intentionally because it is precisely the authorship of this marginalization that remains unclear in my mind after my iterated readings of the memo by Murrillo, Schrank, and Luna (MSL). The memo couches the core problematic in terms of the foregone opportunities in Latin America, but it blames primarily systemic tendencies in the larger discipline of political science. Should blame not be cast specifically on *us*? That is, the tenured and well published but still young and creative political scientists who cut our intellectual teeth on the great research questions of Latin American development, only to find ourselves following the very conventions of narrow causal analysis and institutionalism that this memo argues have impoverished the field? As I watched the panel and pondered the interventions by members of the audience of the same generation (more or less), it appeared to me that the loss we are commiserating about is the result of an “inside job.”

In that regard, the memo prepared by MSL could use more introspection. It strikes me that any reader, even the most disinterested, may stipulate the systemic causes articulated in the text: the ease of doing research with “prepackaged data,” the constraints of time and costs, peer review and publication conventions that privilege context-free theories. But these forces do not explain why the coterie of established scholars noted above have opted (and it is a choice at this point!) to “speak to broader audiences” while asking narrower research questions. The memo calls for research “that takes regional political economies on their own terms by collecting new data, generating original hypotheses, and formulating and testing the sorts of context-sensitive but comparative theories that are indispensable to policymakers and ill-suited to experimental evaluation.” But the memo does not explain why *we* have not done this.

I find that the main claim, that all political scientists (or most, or a significant plurality) are creatures of convention, is unpersuasive, particularly because it lacks specificity. The truth may be worse: *we* reproduce these conventions by reinforcing the same expectations of context-free social science in graduate students and assistant professors on the tenure track. Reading the list of issue areas and questions that conclude the memo is a difficult task because each cries out for context-specific and

interdisciplinary research that is still doable through concerted action (read: collaboration and non-convention-reinforcing intellectual behavior). But instead of reading this list as an answer to the classic question, “What is to be done?” I read the list more mindful of the question, “What has become of *us*?”

Perhaps the memo could develop a more specific *j'accuse!*, but even more fruitful would be an effort to move beyond that to consider the causes for willful choices to engage in context-free social science. Why has our generation of scholars, dedicated as we are to the improved understanding of Latin American politics and development, accepted that analyses of historical and cultural context are less important and less desirable? Perhaps we have been so taken with the contemporary dynamics of the region as to believe that the past weighs less on the present than it actually does. Maybe we believe our theories too much to listen more carefully to what our respondents and contacts say to us. Or maybe we are listening too much to contacts and respondents who claim that the present is different and the past has nothing to teach us. Perhaps we are willing to acknowledge the insights and concepts of our predecessor scholars in the field, but too interested in producing our own new insights and concepts. The answers to these and other questions might help us to understand our hand in the current problem. To paraphrase the President's well-worn campaign dictum, “We may be the ‘us’ and ‘they’ that we are looking for.”

I hesitate to go further to note causes that are specific to our generation—those trained during the 1990s—but there are undoubtedly many that may arise after careful conversations. I am reminded of the simple but powerful finding in the excellent volume by Richard Snyder and Geraldo Munck, *Passion, Craft, and Method in Comparative Politics* (2007), that scholars' conventions are a product of their experiences. How else could we understand World War II veteran Robert Dahl's concern with “polyarchy,” or World War II-era ex-OSS analyst Barrington Moore's sociohistorical analysis of the pathways to democracy and dictatorship? Might there be similar “generational” explanations for the paucity of political economic explanations in Latin America? How would we weigh their importance in comparison to alternative explanations?

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Authors' Response

Our memo was read by a distinguished group of sympathetic colleagues, all of whom have contributed to the study of Latin American political economy and most of whom share our concerns but helpfully push us for further clarification. For instance, Evelyne Huber and John Stephens “are in full agreement with the basic argument” we advance. Tulia Faletti endorses both our diagnosis and our recommendations. And Ben Ross Schneider thinks we are “onto something here,” though—like the other contributors—he offers caveats, suggestions, and amendments. While Ken Roberts also welcomes our call for “a more ‘grounded’ political economy,” he wonders just what the term means. Is “political economy” a method, a paradigm, or a substantive focus of inquiry? Our own view is that it is a focus of inquiry, one that involves the “substantive and theoretical recoupling of political and economic spheres” he describes, and in so doing analyzes how economic interests and processes shape the political agendas and actions of individual and collective agents, and vice versa.¹

To the extent that there is disagreement among the contributors, it begins with the status (or value) of the “alien theories and methods” we decried in the original memo. For instance, Tulia Falletti finds our emphasis on the geographic origins of theories and methods “unwarranted.” Huber and Stephens “are convinced that there is much to be gained from taking good theory developed in other contexts and adapting or modifying such theory to the Latin American context.” And Fernando Limongi shares their views but dismisses “the distinction between imported and nonimported theories and methods” in the first place. “If rational choice (or any other method) works in Europe, then it works in Latin America,” he argues. “If any of these approaches fails to explain relevant topics of the U.S. political economy, it fails in Latin America as well.”

Our own view is twofold. First, we believe that these approaches are a mixed blessing for current efforts to understand U.S. politics as well. (We are less familiar with the literature on European politics and therefore remain agnostic in that regard.) While behavioral and rational choice approaches have contributed much to our knowledge of public opinion, institutions, and interest groups, for example, they are partly responsible for “the peculiar blindness among scholars of American politics to contemporary issues of political economy” (Hacker and Pierson 2010, 279). After all, the field of U.S. politics emphasizes institutions, attitudes, and processes (e.g., Congress, public opinion, electoral behavior) to the exclusion of substantive political and economic problems (e.g., inequality), and we therefore share Jacob Hacker and Paul Pierson’s sense that “mainstream Americanists have paid a huge, largely unacknowledged price for their particular pattern of intellectual specialization”; to wit, the marginalization of a political economy that has, at least until recently, remained a “vibrant part of the subfields of comparative politics and international relations” (Hacker and Pierson 2010, 279).²

Second, we worry that the relative costs of these theories and methods are likely to be even higher in Latin America, where inequality is greater (Muñoz 2013), institutions are weaker (Levitsky and Murillo forthcoming), and cultural norms are in many ways different from those in the United States.³ In fact, Ethan Watters notes that there are literally “hundreds of cultural differences that individually and in endless combinations influence our conceptions of fairness, how we categorize things, our method of judging and decisionmaking, and our deeply held beliefs about the nature of the self, among other aspects of our psychological makeup. We are just at the beginning of learning how these fine-grained cultural differences affect our thinking” (Watters 2013).

Why have Latin Americanists begun to ignore these social, institutional, and cultural differences at precisely the moment they are being recognized elsewhere in the social sciences? Is the problem one of “systemic tendencies in the larger discipline of political science,” as we allegedly suggested in the original memo, or an “inside job” perpetrated by scholars like ourselves, as implied by Al Montero?

We believe that this is a false dichotomy, and that the perpetrators are responding to (and reproducing) incentives that are shaped not only by their larger disciplines but by the societies they study. Take, for example, democratization. By abandoning dictatorship in the midst of an overwhelming economic crisis, Latin America underscored the “relative autonomy of political institutions” discussed by Roberts and opened the door to the rediscovery of contingency. Only by taking the “autonomy of politics” seriously, after all, could scholars like O’Donnell and Schmitter (1985) understand the emergence of democracy in an otherwise inhospitable environment, one that more structural accounts would have portrayed as incompatible with free and fair elections (e.g., Acemoglu and Robinson 2006). Whereas Latin America criticisms of “structuralism” emerged in response to the political violence that characterized much of the region in the 1960s and 1970s, they gained force in 1989, when the fall of the Berlin Wall arguably gave them their coup de grâce.

But the Third Wave of democratization not only bolstered scholarly faith in the autonomy of politics. It brought Latin America back into the mainstream of political science more generally, for Latin America boasts more than a dozen presidential democracies, more than any other world region, and their consolidation opened the door to the study of institutions, elections, and political parties in new and distinct environments. The prevalence of presidentialism, in particular, allowed scholars to import theories from the United States without necessarily paying attention to contextual factors—like the different design and enforcement of Latin American presidential and legislative powers, the prevalence of clientelism and nonprogrammatic linkages, and the “serial replacement” of institutions as well as leaders (Levitsky and Murillo forthcoming; Hochstetler 2006)—that limited or altered their relevance or interpretation.⁴ Consider, for example, roll-call votes of legislators. They have different implications in the United States and Latin America, but they are frequently analyzed and interpreted in the same way.

Analyses like these are made easier by the existence and proliferation of large-scale datasets (e.g., public opinion surveys, governance indicators, and the like) that

are themselves, at least in part, the endogenous products of social and disciplinary transformations. The second culprit, therefore, is the professionalization of the social sciences themselves, and the incentive structure thereby created, on both sides of the Rio Grande. Our memo describes the incentive structure in the United States in terms of pressure to complete a Ph.D., publish in mainstream outlets, build a successful academic career, and obtain funding, all of which are easier with the aid of “off the shelf” datasets and theories that resonate with those used to understanding a U.S. “case” that is itself treated as normative in the social sciences. (Consider the fact that Americanists—and Americanists alone—never have to present case selection criteria or defend what is, in essence, a case study approach to their research.) But the incentive structure south of the border is similarly pernicious, for publications in English-language journals, collaboration with North American institutions, and training in the United States are often valued more than publishing in the local language, engaging scholars elsewhere in Latin America, and getting Ph.D.’s at home or elsewhere in the region. Furthermore, these incentives are frequently hard-wired into Latin American funding allocation decisions by means of crude performance metrics, and the focus on the formal institutional analysis of politics is therefore, if anything, more intense in the region than in the United States.

If U.S. politics gave us theories and methods that could easily make sense of the Latin American reality, these trends would be largely salutary. But given “the peculiar blindness among scholars of American politics to contemporary issues of political economy” (Hacker and Pierson 2010, 279), we are skeptical, and we therefore want to conclude by emphasizing the role of fieldwork not only in fostering data collection (which allows us to test and refine our original theories) but in fostering ties to Latin American-based scholars who—despite the aforementioned pressures—are nonetheless carrying out valuable research and theory-building efforts of their own. Unfortunately, however, U.S.-based scholars who use Latin America as a testing ground for theories derived elsewhere too often view Latin American-based scholars less as peers than as primary sources in the context of “fieldwork” visits that are growing ever briefer in the context of declining budgets and accelerating time pressures in the U.S. academy.⁵ And we thus view the marginalization of political economy in contemporary Latin America less as a matter of individual choice than as the product of a perfect storm: sociopolitical changes within the region that allow for the importation of foreign approaches; institutional and budgetary pressures in the United States and Latin America that encourage the importation of foreign approaches; and individual decisions that reproduce and accelerate these trends. Political analysis has thereby been rendered increasingly disembodied from its economic, historical, and sociocultural underpinnings, and our understanding of relevant political processes and outcomes has grown increasingly simplistic and narrower as a result.

We are neither proposing a new orthodoxy nor rejecting imported theories or methods *tout court*. Indeed, we are agnostic about methodological or theoretical approaches in the abstract. Our call is for approaches that illuminate our objects of study rather than limiting our objects of study to those that are illuminated by par-

ticular approaches. An approach that is invaluable in one context might prove counterproductive in another, after all, and methods and theories—if not epistemologies—are therefore inherently context-dependent.⁶ For example, approaches that illuminate the policy preferences of citizens as such, like methodological individualism, may be less helpful to students of collective actors and organizations that tend not only to aggregate but to modify individual preferences along the way. We therefore want to reiterate that we are less interested in the origin of theories or methods *per se* than in a) issues of internal and external validity that allow us to understand the problems that drive our quest, b) the relevance of cross-disciplinary and interdisciplinary work, and c) the importance of empirical analysis (and data collection) rather than the imposition of models onto reality.

NOTES

1. In our view, therefore, “political economy” would include, but by no means be limited to, two narrower approaches: positive political economy, which is defined in terms of method; and international political economy, which is more of a theoretical paradigm.

2. An emerging literature, which includes work by Larry Bartels (2008) and Martin Gilens (2012), has begun to bring structural factors back into the analysis of politics and policymaking in the United States by focusing on segmented responsiveness. Additionally, Gilens demonstrates that the status quo bias engendered by the relative unresponsiveness of the U.S. political system influences the strength of formal rules once they are adopted. This is a crucial point that might help illuminate the conditions that make “weak institutions” more prevalent in Latin America as well.

3. By “cultural norms” we do not mean invariant cultural traits but contextual factors that change over time and space, and in so doing, contribute to causal heterogeneity (i.e., equifinality and multifinality).

4. Among the recent Latin American contributions to political science, the study of political parties has benefited from an emerging literature on clientelism and nonprogrammatic linkages that provides a nice example of the benefits we expect from more contextualized analysis.

5. The compression of fieldwork times is evident, in particular, when considering recent trends in dissertation fieldwork activities. Whereas a decade ago doctoral students would request year- (or at least semester-)long affiliations with Latin American institutions, they now request shorter periods—and requests of a month or less have become increasingly common. The nature of fieldwork activities has also changed. Primary sources and original fieldwork are being replaced by the gathering of already existing datasets, by “interviews” with local academics (who were always pivotal in providing contacts and references but are increasingly viewed as primary sources in their own right), and by requests to run experiments on university students.

6. In that sense, we are broadly sympathetic to falsificationist epistemologies, notwithstanding the many limits to their effective pursuit.

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