

Review Article

THE REGIME QUESTION

Theory Building in Democracy Studies

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- Collier, Ruth Berins. *Paths toward Democracy: Working Class and Elites in Western Europe and South America*. New York: Cambridge University Press, 1999, 230 pp.
- Diamond, Larry. *Developing Democracy: Toward Consolidation*. Baltimore: Johns Hopkins University Press, 1999, 362 pp.
- Kitschelt, Herbert, Zdenka Mansfeldova, Radoslaw Markowski, and Gabor Toka. *Post-Communist Party Systems: Competition, Representation, and Inter-Party Cooperation*. New York: Cambridge University Press, 1999, 457 pp.
- Linz, Juan J., and Alfred Stepan. *Problems of Democratic Transition and Consolidation: Southern Europe, South America and Post-Communist Europe*. Baltimore: Johns Hopkins University Press, 1996, 479 pp.
- Morlino, Leonardo. *Democracy between Consolidation and Crisis: Parties, Groups, and Citizens in Southern Europe*. New York: Oxford University Press, 1998, 390 pp.

MODERN scholarship on transitions to democracy and its workings can be organized into three groupings, which differ in terms of their comparative strategies. A first body of literature was generated in the 1960s and 1970s and includes classics such as Seymour Lipset's *Political Man*, Barrington Moore's *Social Origins of Dictatorship and Democracy*, Robert Dahl's *Polyarchy*, and Reinhard Bendix's *Kings or People*.¹ These studies offered different arguments, which continue to be debated to this day. But they also employed a shared strategy of comparison that was fairly representative of the scholarship at the time. First, they tended to focus on big cases, such as England, the United

* I thank Carol Leff, James Mahoney, Richard Snyder, and Jay Verkuilen for their valuable assistance. I also have benefited immensely from frequent exchanges with Sebastián Mazzuca.

¹ Lipset, *Political Man: The Social Bases of Politics* (New York: Doubleday/Anchor Books, 1960); Moore, *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston: Beacon Press, 1966); Dahl, *Polyarchy* (New Haven: Yale University Press, 1971); Bendix, *Kings or People: Power and the Mandate to Rule* (Berkeley: University of California Press, 1978).

States, France, Prussia/Germany, Russia, Japan, China, and India, and thus were guilty of a “large-nation bias.”² Second, they were anchored for the most part in the experience of countries from Western Europe that had democratized in the late nineteenth and early twentieth centuries, and because of the focus on large countries, they put overwhelming emphasis on the cases of England and France. The standard approach to comparison was to cast cases beyond Western Europe, with the obvious exception of Germany, as contrast or negative cases and to analyze them in terms of their divergence from the path blazed by the classic cases of England and France.³

Subsequent scholarship on democratization has gone beyond this earlier literature in a number of ways. A second body of literature, published largely since the late 1980s, continues to focus on Europe while making some important departures. This literature broadens the intra-European scope, by adding lesser-known smaller cases to the set of analyzed cases.⁴ Relatedly, it moves beyond the stark counterposition between England and France as positive cases and non-European cases as negative cases. Greater attention is paid to the positive experiences of European cases other than England and France. Moreover, a range of negative experiences from the heartland of Europe during the interwar period are scrutinized.⁵ This new strategy of comparison has generated important insights and findings that have enriched our knowledge about democracy.⁶

The bulk of research on democracy produced in recent years falls, however, into a third category that makes an even more radical break with the literature of the 1960s and 1970s. This literature looks to the

² On the large-nation bias, see Stein Rokkan, *Citizens, Elections, and Parties: Approaches to the Comparative Study of the Processes of Development* (New York: David McKay, 1970), 49.

³ Of course, the other exception that seemed unavoidable, no matter how far the large-nation bias was pushed, was India, which stood out as a non-Western case of democracy.

⁴ Important works that avoid this problem include Thomas Ertman, *Birth of the Leviathan: Building States and Regimes in Medieval and Early Modern Europe* (New York: Cambridge University Press, 1997); John D. Stephens, “Democratic Transition and Breakdown in Europe, 1870–1939: A Test of the Moore Thesis,” *American Journal of Sociology* 94 (March 1989); and Stefano Bartolini, *The Political Mobilization of the European Left, 1860–1980: The Class Cleavage* (New York: Cambridge University Press, 2000).

⁵ See Gregory M. Luebbert, *Liberalism, Fascism, or Social Democracy: Social Classes and the Political Origins of Regimes in Interwar Europe* (New York: Oxford University Press, 1991); and Dirk Berg-Schlosser and Jeremy Mitchell, eds., *Conditions of Democracy in Europe, 1919–1939: Systematic Case Studies* (London: MacMillan, 2000).

⁶ For synthetic discussions of this literature, see Dirk Berg-Schlosser and Gisèle De Meur, “Conditions of Democracy in Interwar Europe: A Boolean Test of Major Hypotheses,” *Comparative Politics* 26 (April 1994); Thomas A. Janoski, *Citizenship and Civil Society: A Framework of Right and Obligations in Liberal, Traditional, and Social Democratic Regimes* (New York: Cambridge University Press, 1998), chaps. 6, 7; and Ertman, “Democracy and Dictatorship in Interwar Western Europe Revisited,” *World Politics* 50 (April 1998).

large set of cases that made transitions to democracy in the post-1974 years, thus shifting the empirical focus of analysis to Southern Europe, East-Central Europe, Southeast Europe, and the vast Soviet empire and to Latin America, East and Southeast Asia, and Africa. Moving to center stage then as positive cases are countries that had previously played virtually no role in theorizing about democracy and that, at best, had entered into consideration as cases where democracy had failed to take root.

This new comparative strategy is having a major impact on theorizing. Given the diversity of cases in this literature, problems related to the generalizability and even the applicability of previous theories became hard to ignore. Indeed, because the new cases are drawn from such disparate regions of the world, it is hardly surprising that insights that seemed to explain the European experience do not travel well and that explanatory variables that had simply not been the focus of attention in the European-based scholarship are now being explored. Moreover, as models developed in one context were transferred to new cases, the critical role of implicit assumptions, many times never acknowledged or articulated, has become obvious. Thus, the study of new cases has given urgency to the tasks of theory building and theory testing in a key subfield of comparative politics.

The five books under review provide evidence of the progress being made by scholars of democracy in tackling these tasks. Indeed, significant advances can be identified by comparing these books to *Transitions from Authoritarian Rule*, the 1986 work by Guillermo O'Donnell, Philippe Schmitter, and Laurence Whitehead that represents in many ways the starting point of this new body of literature and the baseline against which its evolution can be measured.⁷ First, concerning the scope of the empirical analysis, though post-1974 Southern Europe and Latin America—O'Donnell, Schmitter, and Whitehead's focus—continues to be the subject of research (Collier, Linz and Stepan, and Morlino), these works broaden the scope of comparative analysis in two directions. They add a historical dimension by studying European and Latin American cases that democratized in the late nineteenth and early twentieth centuries (Collier).⁸ In addition, they incorporate into the debate a broader set of European cases (Collier) and the new post-communist cases (Kitschelt et al., Linz and Stepan). Even truly global perspectives are offered (Diamond).

⁷ O'Donnell, Schmitter, and Whitehead, eds., *Transitions from Authoritarian Rule: Prospects for Democracy* (Baltimore: Johns Hopkins University Press, 1986).

⁸ See also Dietrich Rueschemeyer, Evelyne Huber Stephens, and John D. Stephens, *Capitalist Development and Democracy* (Chicago: University of Chicago Press, 1992).

Second, these books make important contributions to theory building. Some authors continue to focus, as O'Donnell, Schmitter, and Whitehead did, on democratic transitions (Collier, Linz and Stepan), sharpening and otherwise improving upon existing theory. Others address new questions pertaining to posttransitional politics, variously formulated in terms of the concepts of democratic consolidation (Diamond, Linz and Stepan, Morlino) and democratic quality (Kitschelt et al.). Likewise, efforts at theorizing continue to focus in part on the strategic choices of actors, a central element in O'Donnell, Schmitter, and Whitehead's work (Collier, Linz and Stepan). But these works also introduce or bring back into the debate a range of different independent variables: from the state (Kitschelt et al., Linz and Stepan) and the nature of the prior, nondemocratic regime (Kitschelt et al., Linz and Stepan) to the role of class actors (Collier), political culture (Diamond), and institutions such as parties and the party system (Kitschelt et al., Morlino). In short, these works advance an important research program.

The books under review leave unresolved, however, a range of fundamental problems that have hampered research on democracy. One shortcoming is that key concepts used to define dependent variables are frequently employed in a diverse and unclear fashion, making it hard to know whether different scholars are actually focusing on the same research questions. A second problem is that efforts at causal theorizing have generated a bewildering number of potential explanatory factors that are rarely formulated as clearly specified general causal models. A third troubling trend is that empirical tests have tended to rely on only a small set of cases or on only a small set of explanatory variables, raising doubts about the generalizability and/or the validity of causal claims. As a result, knowledge cumulation has been seriously impeded and the findings produced by this literature have been, at best, tentative.

These problems must be resolved if scholars are to capitalize on the many strengths of this literature. Thus, this article not only highlights some of the central achievements of research on democracy but also, in a constructive spirit, addresses its chief limitations. The focus of the discussion, however, should be clarified at the outset. As indicated, the testing of theories is a problem for this literature. But an assessment of the efforts at theory testing in the books under review calls for a discussion of complex methodological issues that, for reasons of space, cannot be treated adequately here. I thus limit my discussion to issues of theory building. In addition, although other works are discussed, the analysis is not comprehensive in the sense of covering all the relevant literature since the publication of *Transitions from Authoritarian Rule*. Rather,

taking the five books under review as a point of reference, this article is concerned, in particular, with the state of the art of research on democratization and democracy in Europe, South America, and post-Soviet Eurasia.

Operating within these constraints, this review article is organized as follows. It first considers the manner in which dependent variables have been formulated in terms of the concepts of democratic transition, democratic consolidation, and democratic quality and offers recommendations for better conceptualizing and measuring the outcomes of interest. The second section turns to the specification of causal theories and highlights two distinct tasks: the development of thick and general theory and the definition of causal models. The conclusion offers a brief restatement of the basic accomplishments of democracy scholars and the challenges they still face.

CONCEPTUALIZING AND MEASURING THE DEPENDENT VARIABLES

The most obvious virtue of the literature on democratization and democracy is that it squarely addresses one of the most fundamental issues in the study of politics: the *regime question*, understood here in terms of the choice of procedures that regulate access to state power.⁹ Thus framed, two questions lie at the heart of research on regimes. One pertains to the origins of different types of political regimes—the concern of students of *democratization*; a second question concerns the stability of different types of political regimes—the concern of students of *democracy*.¹⁰ A central goal of democracy studies, then, is the construction of a theory of the origins and stability of democracy.

The relative ease with which this goal can be stated notwithstanding, students of democratization and democracy have had mixed success in pinning down their dependent variables. To a large extent, research has been organized around three concepts: democratic transition, democratic consolidation, and democratic quality. But either these concepts have not always been clearly specified or they have been specified in different ways, so that multiple definitions of the same concept bearing an unclear relationship to each other coexist awkwardly in the literature. Relatedly, the boundaries between concepts have been fre-

⁹ Here I am drawing on the conceptual analysis by Sebastián Mazzuca, “Democracy and Bureaucracy: Access to Power versus Exercise of Power,” in David Collier and Gerardo L. Munck, eds., “Regimes and Democracy in Latin America,” vol. 1, “Theories, Agendas, and Findings” (Manuscript, 2001).

¹⁰ These same general questions, of course, can be asked with reference to authoritarianism.

quently transgressed, generating a confused semantic field that obscures the relationships between key concepts. Finally, the procedures for measuring concepts have not been explicitly addressed or consistently applied. If theory building is to proceed in an orderly manner, it is imperative that scholars address a number of problems regarding the conceptualization and measurement of their dependent variables.

DEMOCRATIC TRANSITION

One problem with the concept of *democratic transition*, key to the work of Collier, Linz and Stepan, and even Diamond, concerns the tendency of scholars to define the underlying concept of democracy in different ways. Thus, Collier (pp. 23–32) defines democracy in terms of contested elections with mass participation. In turn, Linz and Stepan (p. 3) include these same criteria but add another that might be labeled the agenda-setting power of elected officials.¹¹ Finally, Diamond's definition goes even further. Indeed, by adopting the Freedom House Index (p. 12), he implicitly uses a definition of democracy that hinges not only on the contested nature of elections and on the agenda-setting power of elected officials but also on seemingly extraneous issues such as freedom from war and socioeconomic rights—though, curiously, not on the right to vote.¹² These divergent definitions of democracy do not necessarily represent a problem. Rather, inasmuch as scholars are explicit about how their definition relates to other definitions, this divergence opens up the possibility of comparing the implications that follow from different definitions, a fruitful exercise. However, frequently the relationship between different definitions is not noted and divergent definitions are treated as essentially equivalent. As a result, efforts at theorizing become shrouded in confusion.

A second problem relates to measurement and, most critically, how to track change empirically along the identified attributes of democracy and how to pinpoint key thresholds. In this regard, the literature on democratic transitions, drawing upon O'Donnell and Schmitter's influential notion of a "founding election,"¹³ has tended to measure demo-

¹¹ Collier (pp. 24, 29–32) does discuss a related issue in addressing the problem of unelected legislators. However, this aspect of democracy is not incorporated in the scores given each case.

¹² On this and other problems with the Freedom House Index, see Gerardo L. Munck and Jay Verkuilen, "Conceptualizing and Measuring Democracy: Evaluating Alternative Indices," *Comparative Political Studies* 35 (February 2002). Diamond's use of Freedom House data is somewhat puzzling, in that he explicitly rejects "the incorporation of social and economic desiderata into the definition of democracy" and argues for "a purely political" conception of democracy (pp. 8, 18, 24). Such a disconnect between the formal definition of a concept and the actual definition (whether explicitly or implicitly) used in measuring the concept is, unfortunately, quite common in the literature.

¹³ O'Donnell and Schmitter, *Transitions from Authoritarian Rule: Tentative Conclusions about Uncertain Democracies* (Baltimore: Johns Hopkins University Press, 1986), 61.

cratic transitions in terms of a dichotomization of a single attribute. Hence the common practice of offering one date, frequently indicated with great precision, on which whole cases cross the threshold from nondemocratic to democratic regime. This practice has some merit. After all, competitive founding elections are unquestionably important, in that their introduction is an event fundamental enough to alter a country's political dynamics. Nonetheless, existing approaches to measuring democratic transitions fall short of the mark in failing to capture how democratization might proceed at different paces along the various attributes of democracy, something not captured in aggregate measures, and also how democratization might entail crossing more than one threshold.

The books under review grapple with this issue but do not resolve it satisfactorily. Collier's (pp. 13, 27–32) discussion of the extension of the right to vote to different segments of the population and the lingering impact of unelected bodies in the nineteenth century suggests the need for at least an ordinal scale and a disaggregated approach. Yet in turning to the systematic coding of cases (pp. 30–31), she follows the standard convention of offering a single date to mark a transition to democracy. Linz and Stepan, like Collier, are sensitive to the possibility that democratization might best be understood as a multistep process,¹⁴ and they discuss how progress along certain attributes of democracy does not necessarily entail equivalent progress along all other attributes. Moreover, they coin terms such as “incomplete democratic transition” that explicitly offer an alternative to dichotomous measures (pp. 4–5, 205, 210). Nonetheless, they still tend to code their cases in terms of a single date, that is, an aggregate and dichotomous measure of democratic transition (pp. 106, 120, 130, 155).

Diamond's measures are also aggregate and thus fail to register the different tempos of democratization processes, but he offers a somewhat different approach. In adopting the Freedom House Index, he explicitly breaks with the view that dichotomous measures are adequate and relies instead on a seven-point scale that he subsequently divides into four categories: “liberal democracies,” “electoral democracies,” “pseudo-democracies,” and “authoritarian regimes” (pp. 279–80). This approach has the ostensible advantage of identifying three thresholds in the process of democratization, rather than one. Nonetheless, these potential gains are actually illusory, because even though Diamond provides an explicit discussion of these four categories (pp. 7–17), there

¹⁴ Specifically, they discuss the duration of transitions and how constitutional questions are settled sometimes before and sometimes after founding elections are held (pp. 81–83).

is really no way to know whether the scores generated by Freedom House correspond to his concepts. Indeed, the decision to attach these labels to different segments of the Freedom House scale are based on an ad hoc and quite arbitrary decision, much like the Freedom House's decision to divide its scale into three categories: free, partly free, and not free. The possibility of greater nuance in terms of measurement is lost because it is unclear how to interpret the resulting measures and relate them to theoretical concepts.

In sum, a clearer conceptualization of democratic transitions as a dependent variable requires scholars to define more clearly what democracy is and what it is not and to assess the implications of different definitions; to acknowledge explicitly that democracy consists of various attributes and that democratization can proceed at different paces along these distinct attributes; and to recognize that movement along any of these attributes may involve more than one threshold. Above all, scholars need to move beyond the current view of democratic transitions as involving a one-shot change, whereby transitions to democracy are conceived in aggregate terms and consisting of one single point of inflection.

DEMOCRATIC CONSOLIDATION AND BEYOND

The problems with the concept of *democratic consolidation*, which is used extensively in Diamond, Linz and Stepan, and Morlino, are even more fundamental than those related to the concept of democratic transition. Originally the concept of democratic consolidation had a quite clear and distinct meaning, captured best in the literature on Latin America. In that context the process of democratic transition actually involved a process of redemocratization. Thus, because many cases had had democracy and lost it in the past, an obviously relevant concern was whether the new, democratic rules would endure and whether regression to authoritarianism would be avoided. However, as the threat of a democratic breakdown became less imminent, the concept of democratic consolidation began to be used to identify every imaginable procedure deemed to not reflect democratic norms and every sign that actors did not accept the democratic rules of the game.

The multiple and conflicting definitions of democratic consolidation created such confusion that even though scholars routinely offer measures of democratic consolidation, these scores are hard to interpret because they refer to such different, often implicit attributes. Indeed, the problems with the concept of democratic consolidation are such as to call for a thorough revision of its current uses. To introduce order in

the semantic field used in studies of democracy, I would propose three fairly major conceptual changes to deal with the two main problems with the concept of democratic consolidation: its definition in terms of (1) actors' attitudes and behaviors and (2) rules deemed not to be democratic.

The first problem is that the standard understanding of democratic consolidation has emphasized the attitude and behavior of *actors* toward the rules of the democratic game. This way of defining democratic consolidation, adopted by Linz and Stepan (p. 6), Diamond (pp. 65–70), and Morlino (pp. 12–18), has offered some insights. For example, the set of conceptual distinctions drawn by Morlino (pp. 18–22) between consolidation, persistence, maintenance, reconsolidation, and breakdown can be fruitfully added to the theoretically rich and descriptively evocative distinctions made earlier by Linz between “loyal” or “constitutional,” “semiloyal” and “disloyal” oppositions.¹⁵ Moreover, the use of survey data by Linz and Stepan, Diamond, and Morlino certainly adds content to these categories and helps to address an important aspect of politics. Indeed, this research helps to capture the sense in which, as Diamond puts it (p. 35), many democracies manage to persist without being consolidated.¹⁶

These virtues notwithstanding, this approach still does not adequately address Adam Przeworski's critique of “the entire problem of legitimacy.”¹⁷ As he correctly stresses, whether a regime is legitimate or not is not the same as whether it is stable or not. Indeed, even if issues of legitimacy might affect the stability of a regime, it is hard to offer an a priori theoretical justification for identifying which actors are relevant to consolidation beyond the standard vague response that it is necessary to study both elites and masses. Thus, a first way to trim the concept of democratic consolidation of its excessive content is to consider the attitude and behavior of actors as an independent variable rather than as part of the dependent variable and to recast the issue of which actors are relevant to the consolidation of democracy as an empirical question,¹⁸ and thus to reconceptualize democratic consolidation more

¹⁵ Juan J. Linz, *The Breakdown of Democratic Regimes: Crisis, Breakdown, and Reequilibration* (Baltimore: Johns Hopkins University Press, 1978).

¹⁶ Kitschelt et al. also use survey data, innovatively comparing the attitudes of political elites and citizens. Their object of study, however, is relations of representation, not democratic consolidation.

¹⁷ Przeworski, “Some Problems in the Study of the Transition to Democracy,” in O'Donnell, Schmitter, and Whitehead, eds., *Transitions from Authoritarian Rule: Comparative Perspectives* (Baltimore: Johns Hopkins University Press, 1986), 50–53.

¹⁸ The same goes for the issue of whether attitudes and/or behaviors are important to democratic consolidation.

simply, as a matter of the durability of rules that are the outcome of transition processes.

The second problem with the concept of democratic consolidation is its use to refer to *rules* that are not seen as reflecting democratic standards. This usage runs through the literature and the books under review. Diamond, in particular (pp. 20–21, 74–75, 49), uses the term very broadly to refer to civil liberties, respect for political liberties, barriers to political participation, constitutional constraints on state power, executive and military accountability to the law, and a host of other features he uses to characterize “hollow” democracies. These obviously important concerns deserve to be analyzed. But an argument can be made that to do so fruitfully, they should not be studied as aspects of democratic consolidation. Indeed, much of what gets discussed under the rubric of democratic consolidation actually does not refer strictly speaking to issues of consolidation, which come to the fore only after the installation of democratic rules; it pertains rather to the debate about democratic transitions.

The root of this conceptual problem is in part related to the way in which democratic transitions have been defined and measured in the literature. Essentially, after adopting the view that democratic transitions are concluded with founding elections, at which time a new phase of consolidation begins, scholars realized that this minimalist definition of democracy directed their attention away from other significant aspects of democracy. And they realized, furthermore, that democratization could proceed by partial and gradual change. However, since cases had already been coded as successful transitions to democracy, scholars belatedly compensated for the low threshold set for measuring democratic transitions by attaching more and more meanings to the concept of democratic consolidation, in the process confusing the distinct concepts of democratic transition and democratic consolidation. Thus, a second suggestion is to reconceptualize democratic consolidation by recasting as attributes of transitions to democracy the numerous attributes that over time came to be inappropriately associated with the consolidation process.

Another way to sharpen the concept of democratic consolidation and clarify the semantic field of regime analysis might be to introduce, as various scholars have suggested, a third overarching concept—*democratic quality*—alongside those of democratic transition and democratic consolidation. This is a promising suggestion in principle, in that it could be used to refer to issues that are distinct from those that pertain to democratic transition and democratic consolidation and thus relieve

the concept of democratic consolidation of some of the multitude of meanings currently attached to it. Unfortunately, however, as it has been used so far, the concept of democratic quality itself suffers from major flaws and is of little help in dealing with the problems associated with the concept of democratic consolidation.

The key flaw with the concept of democratic quality is that it is simply unclear what it means. Indeed, the concept peppers the discussion in the literature without ever being clearly defined, and the books under review do not redress this problem. Diamond (pp. 28, 35–36, 47–48, 65) seems to use democratic quality and democratic consolidation interchangeably, thus denying at the outset the very possibility of clarifying the semantic field by introducing this new concept. Linz and Stepan, by contrast, do distinguish between the concepts of democratic consolidation and democratic quality. Nonetheless, after apparently linking the concept of quality of democracy to “regime characteristics” (p. 5), their discussion of cases said to display a low quality of democracy refers to such things as corruption, high budget deficits, acrimonious political debate, high degrees of social conflict, the persistence of an underclass, a weakening of the rule of law, and a culture of fear (pp. 137, 161, 200–201). Not only do these features not refer to what are usually understood as regime characteristics, but, in addition, they cover such a range of issues that whatever is distinctive about the quality of democracy is never pinned down.

Finally, even Kitschelt et al. (pp. 4–9, 383), for whom democratic quality is a central concern, fail to offer a satisfactory definition. They use the concept of democratic quality to refer to issues of representation, competition, and cooperation or collaboration (pp. 43–92). But the core distinction they make between democratic consolidation and democratic quality appears to hinge on the level of analysis. Indeed, their major claim is that the literature on democratic consolidation has been holistic in its approach and that by focusing on the quality of democracy they shift the level of analysis to that of “specific mechanisms” and “concrete . . . processes” (p. 1). It is unclear how this way of conceiving democratic quality sets it apart from other concepts in the literature. After all, probably few things are as established in the literature on democracy as democracy’s status as a regime type involving representation through an electoral process, and that even though competition is inextricably linked with the notion of democracy, democracy is only sustainable inasmuch as competition is bounded. In sum, the lack of a clearly defined concept of democratic quality casts doubt on the analytic usefulness of the concept and prompts a third suggestion: the

need to define democratic quality in clear terms and, moreover, to show that it refers to some dimension of politics not already grasped by the concepts of democratic transition and democratic consolidation; if this cannot be done, the concept should be dropped from the debate.¹⁹

To recapitulate, the last suggestion is essentially cautionary, pointing to the danger of introducing into the debate new, imprecise concepts that can further clutter an already overcrowded field. In contrast, the first two suggestions seek to make the concept of democratic consolidation more analytically useful by removing its current reference to actors and rules not deemed to be democratic. Actors' attitudes and behaviors would be seen instead as potential causes of democratic consolidation, subject to empirical research. In turn, undemocratic rules would be treated as issues of democratic transition. This reconceptualization of democratic consolidation as a matter of the durability of rules that are the outcome of transition processes—and hence as essentially synonymous with *democratic stability*—has significant payoffs. It would help to focus the discussion on measurement, a major problem in research on democratic consolidation.²⁰ Moreover, when combined with the recommendations concerning the concept of democratic transition, it would clarify the semantic field and thus assist with causal theorizing by reestablishing the link between current research and the classic goal of regime analysis: the construction of a theory of the origins and stability of different types of regimes. Specifically, this reconceptualization would help to refocus attention of democracy scholars on two, more appropriately defined dependent variables: the origins and the stability of democracy.

SPECIFYING CAUSAL THEORIES

Causal theorizing about the regime question, much as efforts to conceptualize and measure the dependent variables of this research pro-

¹⁹ Probably the most fruitful way to refocus the debate about democratic quality is to define it in terms of the concept of exercise of state power, rather than access to it, a key distinction discussed by Mazza (fn. 9). Thus conceptualized, the concern with democratic quality would be shown to refer not to the regime question that is the concern of this article but to a distinct research problem that Mazza labels the administration question. This is what Linz himself appears to imply when he argues that “the quality of democracy depends on the quality of the state”; Linz, *Totalitarian and Authoritarian Regimes* (Boulder, Colo.: Lynne Rienner, 2000), 35.

²⁰ The critical point is that this understanding of democratic consolidation as involving the durability of democratic rules shows that its measurement merely constitutes an extension of the work that needs to be done to measure democratic transitions. Thus, while a measure of democratic transition must register the manner in which authoritarian rules are transformed into democratic ones, step by step along various attributes, a measure of democratic stability should simply aim at capturing the extent to which those democratic gains are retained and not lost.

gram, reveals a mixed picture. On the one hand, efforts to unravel the causes of the origins and durability of democracy have yielded a wide variety of significant ideas about potential explanatory factors. Indeed, this literature has generated important causal theories and provided an essential input for any effort at theory building. On the other hand, despite these efforts to identify potential causal factors, not enough emphasis has been put on the equally critical task of developing general theory and specifying the proposed causal theories with sufficient clarity to interpret data and hence to allow for theory testing.

Theorizing on regimes focuses on a normatively compelling outcome that is manifested in such broad and patently discernible empirical variations as to provide at least a working understanding of the explanatory challenge at hand. Indeed, even though the multiple problems with the dependent variables discussed above have a negative spillover effect on such theorizing, researchers have certainly proposed insightful arguments about a range of explanatory factors. Over the years scholars have developed theories not only about the impact of class actors but also about the role of military and external powers, economic development and economic crises, demographic trends, political culture, education, inequality, institutional rules, and the international political and economic context. The books under review make numerous contributions to this body of theory. Most noteworthy in this regard are the discussions of the role of the state understood either as an organizational apparatus (Kitschelt et al.) or in terms of membership in and the boundaries of a political community (Linz and Stepan); the prior, nondemocratic regime (Kitschelt et al., Linz and Stepan); elites and classes (Collier); decentralization and federalism (Diamond); political parties and cleavages (Kitschelt et al., Morlino); political culture (Diamond, Morlino), and civil society (Diamond). This evolving research program has thus done much to respond to a tall order: the generation of theory about one of the big questions of politics.

But one must also recognize the preliminary and partial nature of such theoretical contributions. Indeed, this literature continues to be hampered by two major drawbacks. First, its various insights do not quite add up to a clear statement of a general theory. Instead, they are only bits of theory that are unclearly related to one another. Second, theorists have largely neglected to specify their causal models explicitly and clearly. All too often, that is, they merely state that a causal factor is relevant but do not specify the form of the effect. Thus, as I seek to show, regime analysts need to put more emphasis on two critical aspects of theory building that are a requisite prolegomenon to any effort at

theory testing: (1) the development of thick and general theory²¹ and (2) the definition of causal models.

DEVELOPING THICK AND GENERAL THEORY

The identification of causal factors in the literature on regimes has proceeded, as is standard, according to a combination of inductive and deductive thinking. Thus, these efforts at theorizing inevitably reflect the cases that are the theorist's empirical point of reference. Inasmuch as the scope of the proposed theory is explicitly stated, this is not a problem, as there is then no danger of misunderstanding the generalizing ambition of the theory and overstating it. But even though such a form of theorizing is entirely legitimate and valuable in itself, it also provides a stimulus for more synthetic and encompassing theory-building efforts. This is so because new theories capture the imagination of the scholarly community, in part by highlighting some novel aspect of social and political phenomena. Thus, as various theories are introduced into the debate, it is only natural that scholars will ask how they might be combined so as to offer a fuller, richer explanation. Yet the development of such thick theories is hardly a simple matter, given that existing theories typically vary in scope and thus potentially do not overlap or only partially overlap. To develop such thick theories appropriately, it is necessary to recast theories originally developed with specific cases in mind so that they can be extended or applied to other cases, that is, as general theories.

The development of theory that is simultaneously thick and general poses an enormous challenge for regime analysts and requires a significant shift in perspective concerning a key methodological issue. In a nutshell, if this key challenge is to be tackled successfully, it is necessary to dispel a deeply ingrained belief that only similar cases can be compared—a misconception that restricts a priori the prospects for general theory. There is no basis for such a restrictive methodological understanding of the comparability of the cases under consideration. To be sure, it is important to recognize that cases can be compared only on the basis of a scale of measurement inasmuch as all cases are similar in the sense of possessing, if only potentially, the attribute that underpins the scale. But it is equally crucial to note that cases can also be compared in terms of both similarities and differences in kind, that is, with regard to whether they do or do not (potentially) possess certain attributes and not just in terms of the degree to which they exhibit a certain attribute.

²¹ I draw the notion of thick, as opposed to thin, theory from Michael Coppedge, "Thickening Thin Concepts and Theories: Combining Large N and Small in Comparative Politics," *Comparative Politics* 31 (July 1999).

Once this point is acknowledged, it is a fairly small step to recognize why there is no logical basis to the standard argument that researchers face an inexorable trade-off between working with concepts that are thick and relatively singular or working with concepts that are thin and relatively general. There is certainly some truth to this argument, but the demanding nature of this task should be recognized as a practical difficulty and not a logical impediment. Analysts would be wrong to conclude, therefore, that any effort at developing general theory must of necessity rely on thin concepts formed on the basis of few attributes. Indeed, inasmuch as analysts generate a concept tree, branching outward as more attributes are introduced and assigning all cases to membership right down to the lowest level of the tree, there is no reason they cannot work with thick concepts, without sacrificing the goal of building general theory.²²

The failure of regime analysts to understand this methodological issue is seen most clearly in the context of the long-standing and heated debate about the comparability of the transitions in Southern Europe and Latin America, on the one hand, and Eastern Europe and the Soviet Union, on the other hand, in terms of a factor generally referred to as the stateness variable. Stateness problems, which arise when “there are profound differences about the territorial boundaries of the political community’s state and . . . as to who has the right of citizenship in that state” (Linz and Stepan, 16), were not addressed in the early literature on transitions in Southern Europe and Latin America. But events in the communist bloc in the late 1980s and early 1990s changed all this. Indeed, not only did the postcommunist world considerably expand the set of cases where an analysis of the regime question seemed particularly relevant, but it also offered a stark measure of the relevance of the stateness variable to theories of regime change—because twenty-two of the twenty-eight postcommunist cases were new states.²³

However, the introduction of a new variable into the debate, though triggered in large part by analyses of Eastern European and post-Soviet Eurasian cases, does not mean that these cases are not comparable to

²² Sartori’s influential analysis of concepts is the standard point of reference for the argument that efforts at generalization require analysts to move up the ladder of generality and use concepts formed on the basis of very few attributes. See Giovanni Sartori, “Concept Misformation in Comparative Politics,” *American Political Science Review* 64 (December 1970), 1041, 1043–44. In his less formal analysis, however, Sartori also recognizes the possibility of a “tree-type arrangement,” which has radically different implications for the construction of thick, general theory. See idem, “The Tower of Babel,” in Sartori, Fred W. Riggs, and Henry Teune, *Tower of Babel: On the Definition and Analysis of Concepts in the Social Sciences* (Pittsburgh, Pa.: International Studies Association, 1975), 17.

²³ The count of twenty-eight includes Mongolia; the twenty-two new states include all fifteen post-Soviet countries, the Czech Republic, Slovakia, Slovenia, Croatia, Bosnia-Herzegovina, Serbia and Montenegro, and Macedonia.

the Southern European and Latin American cases. As various authors have argued, the best strategy of research, at least in its initial stages, might very well be to focus on cases characterized by a stateness problem, that is, a specific value on the stateness variable.²⁴ But it is a different matter to argue that, because such theories usually reflect the characteristics of a certain region and are therefore restricted in scope, it is impossible to generate general theory. Such a statement confuses two issues: the issue of whether any generalizations are ascertained, which is an empirical question that can be answered only after the relevant cases have been compared; and the prior issue of theory building, which centers on showing how the relevant cases are to be compared. The introduction of a new variable should thus push researchers to show how it applies to all cases—either in terms of its absence, its presence, or its degree of presence—and to conduct an empirical investigation of how the diverse ways in which it applies affect the process under consideration.²⁵

Other examples demonstrate that even when democracy scholars have been sensitive to these methodological issues, the pursuit of thick *and* general theory has still been elusive. Collier (pp. 4–14) notes that the literatures on classes and elites have been segregated, such that the analysis of earlier episodes of transitions and the mainly European experience are analyzed in terms of classes, while the more recent transitions are studied in terms of elites. As a result, the generalizability of various hypothesis about the role of labor in democratic transitions, Collier's specific concern, has never been properly addressed. Seeking to redress this problem, then, Collier offers a textbook example of how to recast the debate about labor in more general terms. She does this by extending the analysis of classes from the classic Northwestern European cases to the more recent Southern European and Latin American cases and, likewise, by extending the analysis of elites from the recent Southern European and Latin American cases to the classic Northwestern European cases. However, this valuable effort to apply concepts to a broader set of cases is more of an example of how to develop general theory than of how to develop thick theory, in that it is explicitly concerned with unraveling the role of a fairly narrowly defined set of actors.

Linz and Stepan's (chaps. 3, 4) analysis of the effect of the prior, nondemocratic regime on democratic transitions and democratic stability offers another example of theory that is more general than thick.

²⁴ This argument has been articulated by Valerie Bunce, "Comparing East and South," *Journal of Democracy* 6 (July 1995), 95, 98–99.

²⁵ Linz and Stepan tackle the challenge of theorizing about the stateness issue in this way (pp. 16–37). But these authors fall short in their effort to test their theory, in that they do not present the requisite data for all their cases and thus are not able to draw systematic comparisons.

Their analysis, based on an updated presentation of Linz's 1975 comprehensive map of twentieth-century regime types and subtypes, has the virtue of being based on a common set of regime attributes—pluralism, ideology, leadership, and mobilization—that are applied systematically to a very large set of cases.²⁶ Thus, they do elaborate a thick typology—one that combines a range of elements, even cast at different levels of analysis, that is also very general. But after introducing this rich typology as a springboard for theorizing, Linz and Stepan's proposed theory of the impact of nondemocratic regimes on regime change is, at least in the formal presentation in Part I of their book, very thin. It thus squanders the considerable effort put into developing a multidimensional typology. That is, rather than theorizing about the impact of the various attributes they use to disaggregate the concept of regime, they offer a theory that links highly aggregate concepts such as authoritarian, totalitarian, posttotalitarian, and sultanistic regimes to their outcome of interest (see their summary table on p. 56). In contrast to Collier, who from the outset is more concerned with building general theory than with building thick theory, Linz and Stepan hint that their goal is to develop a theory that is both thick and general, yet offer instead a theory that is in fact more general than thick. In sum, though significant efforts have been made to tackle this demanding aspect of theory building, it still has not been adequately addressed and thus should be a priority for regime analysts.

DEFINING THE CAUSAL MODEL

Another critical aspect of theory building is the definition of the causal model—a complicated task about which regime analysts have a lot to say. Nevertheless, to a large extent, they have failed to appreciate the importance of getting beyond statements of mere association to explicitly defining the actual form of such an association. This is a critical oversight. For when this task is not adequately addressed, the causal model is left unspecified and tends to be portrayed by default, as assuming linearity, additivity, and unifinality.²⁷ Valuable theoretical insights are thus lost and misrepresented.

²⁶ Linz's classic 1975 work has been recently reprinted. See Linz (fn. 19). See also H. E. Chehabi and Linz, eds., *Sultanistic Regimes* (Baltimore: Johns Hopkins University Press, 1998).

²⁷ The importance of specifying the proposed causal model is emphasized in Stanley Lieberson, *Making it Count: The Improvement of Social Research and Theory* (Berkeley: University of California Press, 1985), chaps. 4, 9. On the use and pitfalls of default models, see Andrew Abbott, "Transcending General Linear Reality," *Sociological Theory* 6 (1988). It bears noting that the failure to define the functional form with much precision and the tendency to rely on default models are hardly problems restricted to the literature on regimes. See Nathaniel Beck and Simon Jackman, "Beyond Linearity by Default: Generalized Additive Models," *American Journal of Political Science* 42 (April 1998), 596–99.

Indeed, the literature on regimes is full of insights about complex causation that are not captured in standard default models. To start with the issue of *linearity*, regime analysts propose numerous theories that diverge from three key assumptions that underlie linear models. First, the assumption that a causal effect is constant—that the strength of the effect is the same at all values of the explanatory variable—is frequently challenged. O'Donnell's well-known thesis about the origins of bureaucratic authoritarianism posits, for example, that at a certain level of economic development, the association between development level and democracy not only weakens but actually changes from positive to negative.²⁸ Likewise, when Linz and Stepan (p. 244) invoke Dahl's oft-cited thesis that democratization occurs when the costs of repression become greater than the cost of toleration,²⁹ they are proposing a threshold model of causation, which is essentially the same as that proposed by game-theoretic tipping models.³⁰ Second, the assumption that a causal effect is symmetrical—that the effect is not altered by the direction of change in the independent variable—goes counter to much theorizing about regimes. For example, an asymmetric causal model is implied by the argument that the causes of democratic transition are different from those that bring about the breakdown of democracy³¹ and is also a key feature of path-dependent theories of regime change.³² Finally, the assumption that a causal effect is proportional—that the strength of the effect is commensurate with the magnitude of the change in the explanatory variable—is often contested. Some critical juncture models posit, for example, that “large consequences may result from relatively ‘small’ . . . events”;³³ in much the same vein, tipping game models essentially posit that small changes can lead to a shift in equilibrium and hence generate a large impact.³⁴

Going beyond the problem of specifying monocausal theories, regime analysts once again offer many arguments that suggest the in-

²⁸ O'Donnell, *Modernization and Bureaucratic Authoritarianism: Studies in South American Politics* (Berkeley: Institute of International Studies, University of California, 1973), 85–91.

²⁹ Dahl (fn. 1), 15–16.

³⁰ Przeworski (fn. 17), 54–56; Roger Petersen, *Resistance and Rebellion: Lessons from Eastern Europe* (New York: Cambridge University Press, 2001).

³¹ O'Donnell and Schmitter (fn. 13), 18–19; Adam Przeworski and Fernando Limongi, “Modernization: Theories and Facts,” *World Politics* 49 (January 1997).

³² Ruth Berins Collier and David Collier, *Shaping the Political Arena: Critical Junctures, the Labor Movement, and the Regime Dynamics in Latin America* (Princeton: Princeton University Press, 1991); James Mahoney, *The Legacies of Liberalism: Path Dependence and Political Regimes in Central America* (Baltimore: Johns Hopkins University Press, 2001).

³³ Paul Pierson, “Increasing Returns, Path Dependence, and the Study of Politics,” *American Political Science Review* 94 (June 2000), 251. See, for example, Mahoney (fn. 32), 7.

³⁴ Timur Kuran, *Private Truths, Public Lies: The Social Consequences of Preference Falsification* (Cambridge: Harvard University Press, 1995), 73–74.

adequacy of the most simple multivariate model, which specifies the theoretical link between the multiple explanatory factors in terms of *additivity* and thus posits that different variables affect the dependent variable independently of each other. Indeed, there is no lack of ideas about what John Stuart Mill calls “chemical” as opposed to “mechanical” causation and what Charles Ragin labels “conjunctural” or “combinatorial” causation.³⁵ Breaking with the assumption of additivity, various theories posit fairly standard interaction effects, which define how the effect of one variable is affected by the value of other variables. Examples include theories that identify how the role of specific classes hinges on the presence of coalitional partners (Collier, 11, 21–22) or the pace of economic development,³⁶ how the impact of protest groups depends upon aspects of the political opportunity structure such as the presence of elite splits,³⁷ and how the effectiveness of state repression varies in light of the ability of societal groups to solve their collective action problems.³⁸ More complex interaction effects are also proposed. Thus, various scholars propose models based on counteracting tendencies, arguing, for example, that actors’ choices could override and cancel out the negative effect on democratization of class structure or level of development³⁹ or, somewhat similarly, that political crafting can overcome the negative effects of plural societies on democratic stability (Linz and Stepan, 27, 29, 33–37).⁴⁰ Others propose cumulative models, whereby the effect of certain causes are understood as building upon and being enabled by other factors. Indeed, it is a fairly standard in this literature to distinguish between temporally distant and proximate factors, underlying and precipitating factors,⁴¹ or structural and voluntarist factors⁴² and to argue that the effect of the latter factor in each pair cannot be understood in isolation from the former factor.

Finally, the literature on regimes has broken with the assumption of *unifinality*, that is, the assumption of a one-to-one relationship between

³⁵ Mill, *A System of Logic: Ratiocinative and Inductive*, 8th ed. (1843; New York: Harper and Brothers, 1874), 266–71, 315–17; Charles Ragin, *The Comparative Method: Moving beyond Qualitative and Quantitative Strategies* (Berkeley: University of California Press, 1987), 24–25.

³⁶ Lipset (fn. 1), 45–57.

³⁷ Sidney Tarrow, *Power in Movement: Social Movements, Collective Action and Politics* (New York: Cambridge University Press, 1994), chap. 5.

³⁸ Rasma Karklins and Roger Petersen, “Decision Calculus of Protestors and Regimes: Eastern Europe,” *Journal of Politics* 55 (August 1993), 598–604.

³⁹ Przeworski, *Democracy and the Market: Political and Economic Reforms in Eastern Europe and Latin America* (New York: Cambridge University Press, 1991), 96–97.

⁴⁰ See also Arend Lijphart, *Democracy in Plural Societies* (New Haven: Yale University Press, 1977).

⁴¹ Przeworski (fn. 39), 1–7.

⁴² Richard Snyder, “Paths out of Sultanistic Regimes: Combining Structural and Voluntarist Perspectives,” in Chehabi and Linz (fn. 26).

independent and dependent variables that implies that the same cause has the same effect.⁴³ In contrast, many theories can be seen as examples of what Mill calls “plural” causation and Ragin labels “multiple” or “heterogenous” causation.⁴⁴ Most frequently, such theories take the form of equifinality, that is, a many-to-one relationship whereby different variables or combinations of variables are held to generate the same effect. Examples include Linz and Stepan’s (chap. 4) as well as Collier’s (pp. 21–22, 167–71) multiple paths to democracy and Morlino’s (pp. 160–68, 249–52) alternative paths to democratic consolidation.⁴⁵ But some theories take the form of multifinality, that is, a one-to-many relationship whereby some variable is held to generate different effects, because of its interaction with another variable. Such a model is implicit in the standard argument that the effect of similar choices by communist rulers in Eastern Europe varied according to whether they were made before or after it was apparent that the Soviet Union was not going to resist liberalization in their sphere of influence (Linz and Stepan, 235–45, 267),⁴⁶ and in various arguments about how the same structure can be associated with different effects as a result of the choices made by actors (see, for example, Linz and Stepan, 33–37, 328–33).

The problem with this literature, therefore, is not that it refuses to delve into the world of complex causation, a choice that any serious effort to develop a theory of regimes probably cannot avoid. Rather, the problem is that the significant insights offered by this literature are rarely turned into a statement about the posited causal model that is clear enough to remove significant ambiguities about the proposed testable propositions. The root of this problem varies. Sometimes many of the most intriguing arguments are embedded deep in the analysis of cases and are not developed as explicit theoretical statements. Other times there is a disjuncture between the causal models developed in the text and the explicit summary statements that usually represent the point of departure for efforts at testing. In yet other instances, although

⁴³ In statistical terminology, this assumption is referred to as the assumption of unit homogeneity.

⁴⁴ Mill (fn. 35), 311–15; Ragin (fn. 35), 25; and idem, *Fuzzy-Set Social Science* (Chicago: University of Chicago, 2000), 40, 51–53, 102–4.

⁴⁵ See also Alfred Stepan, “Paths toward Redemocratization: Theoretical and Comparative Considerations,” in O’Donnell, Schmitter, and Whitehead (fn. 17). The notion of path is used in more than one way in the literature on regimes. Thus, it is important to distinguish these arguments about multiple paths to the same outcome from other arguments about paths, which consider multiple paths to diverse outcomes. Examples of the latter models include Moore (fn. 1); and Snyder (fn. 42).

⁴⁶ For other arguments that invoke the importance of timing and that imply that the same variable may have a different effect according to when it enters into play in a sequence of events, see Pierson, “Not Just What, but When: Timing and Sequence in Political Processes,” *Studies in American Political Development* 14 (Spring 2000).

the causal models are formulated quite clearly, the discussion of different variables is segregated, frequently into different chapters and articles or even books, thus preventing any integrated approach to theory that could explicitly address interaction effects and causal heterogeneity.

Even ambitious efforts at theory building, as provided in Linz and Stepan's *Problems of Democratic Transition and Consolidation*, fall short. Linz and Stepan's theoretical contribution is considerable and has done much to spur debate about the prior regime and the stateness issue as explanatory factors. Moreover, their treatment of theory separately from their discussion of the cases helps to make for a clear theoretical statement (Linz and Stepan, Part I). Finally, these authors signal that their explanatory variables are actually not all independent but rather are hierarchically related. They do this when they suggestively label two of their variables—stateness and prior regime type—"macro" variables and thus distinguish them from their other "actor-centered" and "context-centered" variables (Linz and Stepan, xiv–xv). The problem, however, is that Linz and Stepan do not really follow through on this idea by making their hierarchical causal model explicit. Though the prior-regime-type variable is obviously temporally prior to and, as Linz and Stepan (pp. 56, 65) convincingly argue, largely determines the value of one of their other variables—"the actors that initiate and control the transition"—the endogenous nature of the latter variable is not explicitly specified. Likewise, though the constitution-making environment, another variable, seems strongly affected by other temporally prior variables such as the old regime type and the variable they label the political economy of legitimacy, these causal links are not explicitly specified. For this reason, Linz and Stepan's valuable insights are harder to appreciate and their considerable theoretical contributions do not quite come together.

In this context, one should highlight the significant effort at theory building offered by Kitschelt, in his coauthored book *Post-Communist Party Systems* and in a set of recent related works.⁴⁷ Kitschelt's complex argument consists of several causal links. The argument hinges on his use of two "master variables" to characterize the mode of communist

⁴⁷ Herbert Kitschelt, "Formation of Party Cleavages in Post-Communist Democracies: Theoretical Propositions," *Party Politics* 1 (October 1995); idem, "Accounting for Outcomes of Post-Communist Regime Change: Causal Depth or Shallowness in Rival Explanations" (Paper presented at the annual meeting of the American Political Science Association, Atlanta, September 1–5, 1999); idem, "Post-Communist Economic Reform. Causal Mechanisms and Concomitant Properties" (Paper presented at the annual meeting of the American Political Science Association, San Francisco, August 30–September 2, 2001).

rule—the structure of the state bureaucracy and the means used to ensure societal compliance—which are themselves largely shaped by factors that preceded the onset of communist rule. These master variables first affect both the ability of the communist rulers to adapt to post-communism by converting their old sources of power into new ones and the distribution of power during the transition period. These factors, in turn, determine the mode of transition from communist rule, the choice of new formal institutions, and the cleavages that become politically salient. Finally, these factors combine to determine the extent to which the new regime is characterized by programmatic party competition and whether the dominant party coalition pursues market-oriented economic reforms (Kitschelt et al., 21–41, 49–92, 385–91).⁴⁸

This is a bold theoretical argument that has some significant virtues. On the one hand, it avoids the *tabula rasa* perspective so common in studies of postcommunism and instead draws attention to the critical significance of legacies (Kitschelt et al., 391–401). Indeed, Kitschelt essentially offers a path-dependent form of analysis that specifies how different configurations of structural factors give rise to divergent outcomes: some characterized by a mutually reinforcing relationship between democracy and capitalism, others by regimes that are hard to characterize as democracies and coalitions that do not pursue market-oriented reforms in a resolute fashion (Kitschelt et al., esp. 19–21, 69–79).⁴⁹ On the other hand, Kitschelt's theoretical argument avoids the problem of studies that merely posit correlations between macrostructural variables and that, relatedly, put undue emphasis on the weight of the past and do not consider the impact of future-oriented choices made by actors.⁵⁰ Thus, he explicitly analyzes causal mechanisms and formulates macro-micro-macro causal chains (Kitschelt et al., 1–2, 13). And he argues for a sequential model that recognizes how the weight of legacies might diminish over time and, by implication, how new, exogenous factors that enter into the picture after the end of communist rule might take on added significance (Kitschelt et al., 12, 19–20, 31–32, 35, 60, 92).⁵¹ In short, Kitschelt's work stands out as an

⁴⁸ See also Kitschelt (fn. 47, 1999), 24–30. Beyond this, Kitschelt et al. link their theoretical argument to issues of citizen-party elite relations and the extent to which popular preferences are translated into policies, aspects of their overall argument that are tested with innovative survey data. For reasons of space, however, I am unable to discuss in detail this important aspect of Kitschelt et al.'s work.

⁴⁹ This argument is more explicitly developed in Kitschelt (fn. 47, 1999), 24–31.

⁵⁰ For this critique, see Przeworski (fn. 39), 95–99.

⁵¹ See also Kitschelt (fn. 47, 2001).

example of theorizing that addresses complexity in a relatively systematic way.⁵²

Kitschelt's important accomplishments notwithstanding, it should be noted that his effort at theory building could be pushed even further, in particular, to offer a closer analysis of the role of actors. As mentioned, Kitschelt et al.'s theoretical arguments do make explicit the causal mechanisms that connect macrodeterminants to macro-outcomes. But the account of mechanisms has some significant gaps. First, though Kitschelt et al. offer a clear account of the pathways out of communist rule, their argument about the convertibility of the power of communist rulers and the relative power of rulers and civil society offers an answer to only one question: *if* a transition is going to occur, *how* will it proceed? Indeed, because the structural factors discussed by Kitschelt et al. are held to remain constant throughout the period of communist rule, they could certainly not explain, at least on their own, *why* a transition occurred *when* it did or, as the question is put by Przeworski, "why . . . [communism] collapse[d] in the autumn of 1989."⁵³

Concerning the impetus for such a transition, Kitschelt et al. (pp. 20–21, chap. 3) argue that such a process, due to its inherent contingency, is not amenable to social scientific analysis and that the best that can be done is to offer a narrative, but not an analysis, of the events that drive regime change. This view, shared by many analysts, is certainly not unreasonable.⁵⁴ Indeed, there is probably no such thing as purely endogenous change, which means that any explanation of change necessarily must invoke, as Kitschelt et al. implicitly do, an exogenous shock. Nonetheless, because Kitschelt et al. essentially foreclose a priori the discussion about the role of actors in transitional processes, we are left to wonder whether something more systematic could be said about the disequilibrium behavior of actors,⁵⁵ and also whether the structural factors Kitschelt et al. associate with pathways out of communism are as determinative as they imply or whether actors might construct multiple pathways even within similar structural settings.⁵⁶

⁵² Though Kitschelt does seek to offer a formal causal model, the proposed causal model still requires a clearer and fuller specification; Kitschelt (fn. 47, 1995), 454.

⁵³ Przeworski (fn. 39), 1.

⁵⁴ Similar arguments have been articulated recently by Pierson (fn. 33); and Mahoney, "Path Dependence in Historical Sociology," *Theory and Society* 29 (August 2000).

⁵⁵ See, for example, how Petersen (fn. 30) goes well beyond Kuran (fn. 34) in analyzing the dynamics of tipping games used to explain the end of communist rule.

⁵⁶ The latter possibility is insightfully explored in Mahoney and Snyder, "Rethinking Agency and Structure in the Study of Regime Change," *Studies in Comparative International Development* 34 (Summer 1999), 11–16.

Second, the role of actors in posttransitional politics is likewise slighted in significant ways. Kitschelt et al. begin their discussion by drawing an insightful contrast between the forms of analysis used to study the established democracies of the West and those that are appropriate for research on new democracies (pp. 6–9). More broadly, they emphasize that politics in new democracies must be understood, at least initially, as “a non equilibrium process” (Kitschelt et al., 95, 45, 12–13). This is a profound statement and the basis for Kitschelt et al.’s emphasis on legacies. Nonetheless, their analysis is not always consistent with this problematization of the assumption of equilibrium behavior. Thus, not only do they seem to contradict themselves, by stating that the East-Central European countries they study (Poland, Hungary, the Czech Republic, and Bulgaria) could be considered “consolidated” democracies by the early to mid-1990s (pp. 9, 403, 384), but they are also silent about the process whereby actors decide to play by the rules of democratic game, that is, the politics that leads, as Jon Elster puts it, to “the establishment of a new, post-transitional equilibrium.”⁵⁷

In a nutshell, a key task confronting theorists is to build upon existing structural and path-dependent forms of analysis but also to analyze as far as possible (rather than treat as an exogenous shock or remain silent about) both the process whereby the old regime rules are overturned and the process whereby the new regime rules become accepted. Indeed, a core concern of theorizing about the regime question should be to inquire whether something systematic can be said about the ongoing contest between actors seeking to reproduce the status quo and those favoring the production of new rules or, to put it differently, about the constant interplay between equilibrium and disequilibrium behavior. Framing the theoretical puzzle of regime analysis in these terms has the important advantage that any resulting theory would be applicable equally to the study of democratic transition and to the study of democratic stability.⁵⁸ In this sense, this undertheorized issue holds out the as yet unrealized promise of a unified and potentially strong theory on the regime question. Thus, a top priority of regime analysts

⁵⁷ Jon Elster, “Reason, Interest and Passion in the East European Transitions,” *Social Science Information* 38 (December 1999), 499.

⁵⁸ The only difference would be the definition of the status quo, which is authoritarianism with regard to the question of democratic transition and democracy with regard to the question of democratic stability. In this sense, it is also worth pointing out that such a framing would essentially posit that democratic transition and authoritarian stability are merely alternative outcomes of the same process and, likewise, that democratic stability and transition to authoritarianism can be thought of in the same way. Thus, this framing would help to unify the study of democracy and authoritarianism.

should be to develop such a theory and to derive from it clearly specified causal models that can be tested.

CONCLUSION: ACCOMPLISHMENTS AND CHALLENGES

The accomplishments of the scholarship on the regime question are considerable. Indeed, focusing on a range of new cases, recent research has broken new ground and has had an unsettling but salutary effect. New concepts that better grasp the realities under consideration have been introduced, new theoretical perspectives have been developed, and new causal arguments have been proposed and assessed. This research has advanced our understanding of what Linz rightly identifies as one of the main axes of conflict in the twentieth century: the battle between the forces of democracy and those of authoritarianism.⁵⁹ Nonetheless, the future progress of this research program hinges upon the ability of scholars to tackle some fairly fundamental challenges.

This article has focused on the challenge of theory building and discussed some key areas in need of further research. Concerning the preliminary task of defining dependent variables, it argues that the concepts of democratic transition, democratic consolidation, and democratic quality, as currently conceptualized in the literature, do not provide a clear focus for causal theorizing. It recommends, rather, that the proper subject matter of regime analysis is the origins and stability of regime types and suggests how the semantic field of democracy studies could be clarified by focusing on the concepts of democratic transition and democratic stability. Relatedly, it argues that democracy scholars have made unwarranted use of aggregate and dichotomous measures, and it advocates the use of more disaggregate and nuanced measures. Concerning causal theories, it shows that researchers have identified a range of potential explanatory factors and proposed suggestive complex causal models. Nonetheless, it also argues that democracy scholars have rarely formulated clearly specified general causal models and identifies some pitfalls to be avoided as scholars tackle two key tasks: the development of thick and general theory and the definition of causal models.

Theory building, moreover, is not the only challenge faced by democracy scholars. Indeed, social scientific research consists of three core elements: theories, data, and inferences. Thus, going beyond theory building, scholars must generate data that are appropriate in light of the proposed theories, a demanding task that requires attention to the

⁵⁹ Linz (fn. 19), 20–24.

methodology of measurement. And they must address the problem of inference, which is concerned with the relationship between theory and data, an equally demanding task that has long been at the heart of empirical methodologies.⁶⁰ These distinct and complex challenges require as much attention as theory building and they all play their part in determining whether research yields any substantive findings. Thus, even though this article has emphasized theory, given that the manner in which this challenge is resolved affects how other challenges should be faced, it is essential to place the challenge of theory building in context. Indeed, as scholars continue to wrestle with the regime question, progress will require a coordinated, multipronged effort aimed at producing better theories, better data, and better inferences.

⁶⁰ Relevant discussions of these issues include Coppedge (fn. 21); Henry E. Brady and David Collier, eds., *Rethinking Social Inquiry: Diverse Tools, Shared Standards* (Lanham, Md.: Rowman and Littlefield and Berkeley Public Policy Press, 2002); and Munck and Verkuilen (fn. 12).