Review Article

What Do We Know About Authoritarianism After Ten Years?

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Jennifer Gandhi, *Political Institutions under Dictatorship*, New York, Cambridge University Press, 2008.

Steven Levitsky and Lucan Way, *Competitive Authoritarianism: Hybrid Regimes After the Cold War*, New York, Cambridge University Press, 2010.

Beatriz Magaloni, *Voting for Autocracy: Hegemonic Party Survival and its Demise in Mexico*, New York, Cambridge University Press, 2006.

Pablo Policzer, *The Rise and Fall of Repression in Chile*, Notre Dame, University of Notre Dame Press, 2009.

Dylan Riley, *The Civic Foundations of Fascism in Europe: Italy, Spain, and Romania, 1870–1945*, Baltimore, Johns Hopkins University Press, 2010.

Dan Slater, *Ordering Power: Contentious Politics and Authoritarian Leviathans in Southeast Asia*, New York, Cambridge University Press, 2010.

After two decades in which democratization was pretty much the only game in town, the study of authoritarian regimes has recently become one of the hottest subfields in comparative politics. The "transitology" paradigm, which conceived of authoritarian regimes as theoretically interesting insofar as they told us something important about their democratic successors, now has the taste of ashes. Even by the standards of a faddish discipline, the magnitude of the switch in scholarly focus from democratization to authoritarianism has been remarkable.

As always, real-world developments were primarily responsible for this shift. The third wave of democracy had runs its course by the turn of the twenty-first century, leaving a host of regimes that did not fit neatly into existing categories. Of course, the signs of democratic slippage were already apparent by the mid–1990s, and Fareed Zakaria's 1997 piece in *Foreign Affairs*, titled "The Rise of Illiberal Democracy," underscored what many specialists already knew.[1] Two years later, Barbara Geddes's attempt to summarize what scholars had learned about democratization over the last twenty years succeeded instead in raising a host of research questions about authoritarian regime types and their durability.[2] Some of these issues had been highlighted in previous work on authoritarianism, much of which focused on either military regimes or totalitarianism.[3] Yet the rise of regimes that were neither purely democratic nor authoritarian led to many new questions. After several years devoted to constructing typologies, a young cohort of scholars turned to the issue of regime trajectories. To paraphrase one representative work in this strain, what explained the persistence of "authoritarianism in an age of democratization?"[4] After a decade of work in this vein, it is worth pausing and considering what we have learned.

The books that comprise this review provide a good overview of the existing research programs on authoritarianism.[5] Two are particularly concerned with the origins of authoritarian regimes. Dan Slater's *Ordering Power* focuses on the construction of "authoritarian leviathans" in Southeast Asia; Dylan Riley's *Civic Foundations of Fascism in Europe* offers a new theoretical perspective on the development of fascism in Italy, Romania, and Spain. The other four books concentrate on the durability of authoritarian regimes, though in so doing they also speak to the issue of democratization.[6] As the title suggests, Jennifer Gandhi's *Political Institutions under Dictatorship* argues that democratic-looking institutions, particularly legislatures, are not merely "window-dressing" but essential weapons in a dictator's arsenal. Pablo Policzer's *The Rise and Fall of Repression in Chile* is one of the few existing studies of coercive institutions, and shows that repressing a society is a much more complicated organizational endeavor than is usually assumed. *Competitive Authoritarianism*, by Steven Levitsky and Lucan Way, combines international and domestic organizational variables in a theoretical framework to explain the trajectories of thirty-five nondemocratic regimes over the past fifteen years. Beatriz Magaloni's *Voting for Autocracy* not only demonstrates that hegemonic parties can successfully buy elections, but also shows how running up huge electoral majorities preserves authoritarian durability by dissuading defections by hegemonic party elites and demonstrating to both voters and the opposition the futility of challenging the system.

Taken together, these books use very different methodologies to make a number of important points. I highlight four in this review article. First, by taking the institutions of authoritarian regimes seriously, several scholars are able to gain real traction on the question of durability. Rather than pointing to exogenous shocks, they are able to locate the reasons for authoritarian stability or breakdown in longstanding patterns of behavior, both formal and informal. Second, several of the books demonstrate the continued utility of comparative historical analysis for explaining both past and contemporary regime types. Third, with the exception of Magaloni, and to a lesser extent Gandhi, the books move away from patronage-based accounts of authoritarian durability, pointing to other, noneconomic factors, which allow or prevent elite cohesion. Fourth, Slater and Riley in particular seek to explain differences between authoritarian regimes, such as their degree of infrastructural power and pattern of state-society relations.

These are all important contributions, and will help define future research on authoritarianism. But the books should appeal to a broader readership because each, in its own way, latches onto a more general concept in comparative politics. Slater's goal is to demonstrate that contentious politics—which he defines as "non-routine political events involving considerable popular mobilization" (p. 5)—should be treated as an *independent* variable, as well as a dependent one, in theory building. Riley engages the literature on civil society. Levitsky and Way bring international variables back into the study of regime change. Policzer demonstrates the utility of a principal-agent framework in organizing coercion. Gandhi embraces the new institutionalism and formal modeling. Magaloni synthesizes decades of insights about elite and voter behavior to craft new models of both authoritarian durability and regime transition.

After analyzing what these books contribute to our understanding of the origins and persistence of authoritarian regimes, the latter half of this review essay raises three issues that emerge from my reading of them, and which I believe future work on authoritarianism should think about more explicitly. The first is the relatively weak theory of institutions and organizations that characterizes several of them, and other works in this vein. It is not enough to identify institutions or organizations as key independent variables in explaining the origins or persistence of authoritarianism. The causal mechanisms linking these variables to outcomes need to be better spelled out. Given the wealth of theorizing about institutions and organizations in several subfields of political science, including those that would seem far removed thematically from the authoritarianism, such as the political economy of advanced industrial societies, future work would do well to borrow from these insights.

Second, we need better evidence to test our hypotheses. While work on authoritarian regimes should obviously not be held to the same evidentiary standards as work on advanced industrial democracies, I am not convinced that the situation is as hopeless as it appears at first blush. An entire generation of Sovietologists overcame hurdles in collecting evidence, and contemporary researchers have worked fruitfully in places, and on topics, that would seem ill-suited for field work.[7]

Third, while the last decade of research on democratic-looking institutions such as legislatures and political parties in authoritarian regimes has been productive, there are many other compelling issues to explore. Coercion, for example, remains the core feature of authoritarian regimes, and several of the books in this review refocus our attention on the role of fear, violence, intimidation, and surveillance in the origins and maintenance of authoritarianism. Yet our understanding of the coercive institutions of modern authoritarian and hybrid regimes is pretty thin, especially when compared to studies of totalitarianism in which organizations like the Gestapo, KGB, and Stasi figure prominently. We need to know much more than we currently do about the myriad of security forces—militaries, paramilitaries, presidential guards, secret police, militias, armed thugs—that comprise the coercive apparatus of contemporary authoritarian regimes.

Origins of Authoritarianism

How are authoritarian regimes constructed in the first place, and what types of legacies do these foundations leave? Slater and Riley both adopt comparative historical analysis (CHA) to get at these questions. This research tradition has enjoyed something of a rebirth in comparative politics over the last decade. Of course, one could claim that a mode of social scientific inquiry that traces its lineage from Barrington Moore, back to Karl Marx and Max Weber, and ultimately back to Aristotle, was never likely to disappear entirely from view. Still, self-conscious efforts to define and defend best practices in the field of CHA have appeared prominently in some of the most visible publication outlets.[8] Both Slater and Riley continue in this vein. Both draw on the work of Gregory Luebbert—one of the most influential recent exemplars of CHA– although Slater does so more explicitly than Riley.[9] Both also make contentious politics the starting point of their analysis.

Slater laments the fact that "contentious politics receives far more attention as a *product* rather than a *producer* of political phenomenon" (italics in original, p. 275). There is a massive literature on the causes of revolutions, riots, civil wars, strikes, and protests. Yet there are far fewer works, according to Slater, that treat these phenomena as independent variables. His central argument is that different patterns of contentious politics explain regime outcomes in Southeast Asia. By outcomes, he means different types of authoritarian regimes, and is explicitly not concerned with explaining the presence or absence of democracy. "Since democracy broke down everywhere in postwar Southeast Asia," Slater argues, "the region lacks the requisite variation to support any systematic explanation for democratic breakdown" (fn.33, p. 12). What do vary, however, are the strength of state infrastructures in Southeast Asia and the durability of the regimes that control them.

Following Luebbert, who linked the outcomes of liberalism, fascism, and social democracy in Western Europe to the responses to mass unrest in the wake of WWI, Slater begins his analysis with the contentious politics following the end of WWII. Although the seven cases (Burma, Indonesia, Malaysia, Philippines, Singapore, South Vietnam, and Thailand) would appear to fit a general pattern of communist insurrection amid colonial retreat, Slater instead identifies three distinct patterns of contentious politics that had long-term consequences for state building.

The first leads down a pathway of domination, illustrated by the cases of Malaysia and Singapore. Slater's central insight—one that draws from Hobbes and scholars of Hobbes like Ioannis Evrigenis—is that elites will only act collectively to maintain an authoritarian, extractive state if they view it as less threatening than the alternative.[10] To get a sense of that alternative, elites gauge the extent of the threat to their lives and livelihoods by analyzing current and past forms of contentious politics. Specifically, they need to decide whether mass unrest is *endemic* or *episodic*, and whether it is *manageable* or *unmanageable* under existing institutional arrangements. Elites are most threatened when they conceive of contentious politics as endemic and unmanageable. Although there are many potential reasons for reaching this conclusion, Slater argues that elites are most threatened when (1) contentious politics takes the form of demands for radical redistribution; (2) it touches urban as opposed to rural areas; and (3) it sparks communal in addition to class tensions. These conditions pertained in Malaysia and Singapore, where communist movements ignited urban race riots and led elites—both ethnic Malay and Chinese—to band together in what Slater calls a "protection pact." Elite collective action resulted in a strong authoritarian state underpinned by durable political parties, bureaucracies, and coercive institutions.

Slater's emphasis on noneconomic incentives for elite collective action is a welcome correction to a literature that has focused overwhelmingly on patronage as the central mechanism in authoritarian durability. While some authors, such as Magaloni, have produced theoretically sophisticated and nuanced elaborations of this basic argument that fit specific cases, the ability to buy off sectors of the population is only one part of the story. The distribution of resources alone cannot explain patterns of breakdown and persistence, since the number of authoritarian regimes that have weathered severe economic crisis is much higher than an exclusive focus on patronage would predict. Slater rightly points out that elites are not only afraid of losing licenses and contracts, but also are concerned about a return to a Hobbesian state of nature. Although he includes leftist political demands as a central variable in elite threat perception, his theory is much more grounded in security studies than in political economy.

But the argument is not simply that we should move beyond patronage-based accounts. In fact, Slater claims that authoritarian regimes can indeed be founded on patronage (what he terms elite "provision pacts"), but that such an arrangement leads down a second pathway—fragmentation. When elites do not face a form of contentious politics that is leftist, urban, and communal, they are not motivated to form a strong coalition versus a common enemy. "Flimsy coalitions produced flimsy institutions" in the Philippines, South Vietnam, and Thailand (p. 23). Authoritarianism was less durable, and state strength was lower, in these cases.

Slater illustrates his third pathway—militarization—with Indonesia and Burma. The key point here is that contentious politics took the form of regional rebellions that threatened the territorial integrity of the state more so than they threatened the elites. The army took responsibility for squashing these rebellions and, much like Brandenburg, Prussia, after the military revolution of the sixteenth century, created a military-bureaucratic state.[11] In contrast to Prussia, however, elites in Burma and Indonesia were never incorporated into state institutions, which meant that their interests were never perfectly aligned with those of the ruling military. Slater admittedly has more difficulty fitting these cases into his theoretical framework, yet they do support the broader point about how internal conflicts—as opposed to the external ones that Tilly and others normally highlight—influence state building.[12] This attention to conflict and state building is one of the many ways that Slater crosses subfield boundaries, and I would not be surprised if selections of it appeared on graduate syllabi in international relations, in addition to comparative politics.

Although Riley's story commences a half century earlier and on a different continent, there are clear overlaps with *Ordering Power*. Like Slater, Riley begins with contentious politics—in the form of associational activity that challenges the political status quo– to explain both the development of authoritarianism as well as three particular subtypes of it. As the title suggests, Riley is clearly taking on the Tocquevillian thesis that posits a causal relationship between associational life and liberal democracy. This would seem to be well-trodden territory; Sheri Berman's article on civil society in Weimar Germany, among other critiques, led the prominent neo-Tocquevillian Robert Putnam to acknowledge over a decade ago that there was a "dark side of social capital."[13] Yet Riley's analysis is novel in several respects. First, the choice of cases demonstrates that Weimar Germany was not unique in its degree of associational development. An explosion of mutual aid societies, credit associations, and cooperatives at the turn of the twentieth century transformed civil society—particularly rural civil society—in three of the least developed states in Europe.

Second, and more provocative, Riley is *not* arguing that the *type* of associational activity led to fascism. He explicitly rejects "the notion that fascism arose out of a pathological form of civil society" (p. 11). Rather, he views fascist movements as "a twisted and distorted form of democratization" (p. 2). The fascists rejected the institutions of parliamentary democracy, while claiming to represent the legitimate democratic aspirations of the people. This reframing of fascism is less audacious than it sounds: Peter Fritzsche made a similar point a decade ago in emphasizing the populist aspects of National Socialism.[14] Still, Riley's formulation is virtually guaranteed to provoke storms of protest from scholars of fascism on several grounds, not the least being that paramilitarism and glorification of violence—the defining features of fascism for many scholars—are hardly compatible with any reasonable definition of democracy.[15] Potentially even more severe is the charge that Riley overstates the intellectual coherence of fascist movements. Even the pioneer of fascist ideology, Benito Mussolini, waited until 1932 to write down its essential elements, and there is some debate about whether the components of Italian fascism ever held together, either in theory or in practice.[16]

These points will be hashed out among specialists. More important for comparativists is the causal chain of the argument. Here Riley leans heavily on Antonio Gramsci, particularly on the concepts of hegemony and organic crisis. As in most works of CHA, sequencing plays a central role. When associational development preceded the development of strong political organizations, the political crises that followed WWI overwhelmed liberal democracies and produced fascism. When political organizations preceded associations, liberal democracy withstood these challenges. The reader might legitimately ask how this explanation is different from Luebbert. Although Riley sets up his argument as a challenge to neo-Tocquevillians, neo-Weberians like Michael Mann, and Barrington Moore, the similarities and differences with Luebbert are acknowledged only in passing. Luebbert claimed that division among liberals produced weak liberal political parties in states like Italy and Germany (weak intra-class hegemony in Riley's terms).[17] He noted that this contributed to the failure of a cross-class coalition between liberal and labor parties (weak inter-class hegemony according to Riley). World War I led to an explosion of political participation, redistributive claim-making, and a variety of other pressures on liberal democracies (an organic crisis). Political scientists will also not find Riley's claim that "the rise of a fascist movement in each country was connected with the tendency of democratic forces to split and fragment" (p. 200) particularly novel, as Linz and Stepan are the best-known proponents of this line of thinking.[18]

Riley's tripartite distinction among fascist regimes does, however, break with previous work. Like Slater's book, the attempt not only to explain an authoritarian outcome but also to say something interesting about the *type* of authoritarian regime gives *The Civic Foundations of Fascism in Europe* added value. Riley argues that the particular types of associational activity under fascism produced different patterns of state-society relations. When associational life developed autonomously from the state, as in Italy, the result was party fascism. When civil society was fostered by the landed elite and the Church, as in the Spain, the result was traditional fascism. Statist fascism, as in Romania, emerged because a statist liberal party played the dominant role in fostering the development of civil society. It is difficult to say whether one of these fascist subtypes would have proven more durable than the others. Putting aside the question of whether Spain was ever a fascist regime, and whether the brief period of fascist influence in the Romanian government counts as a fascist outcome, there seems no reason to believe that one pattern of state-society relations would have been more stable than the others.[19]

Slater has more to say on the question of durability. In his view, protection pacts are clearly the strongest foundation upon which to build an authoritarian order. A combination of historical memories of violence and institutional reproduction prevented Mahatir from falling like Suharto, whose leviathan was far less extractive and organized, when the Asian financial crisis hit. Still, it is fair to say that both Slater and Riley are primarily concerned with the origins of authoritarianism, and only secondarily with its trajectory. To understand authoritarian maintenance and breakdown, we need to turn to the other four books.

Durable Authoritarianism

In *Competitive Authoritarianism: Hybrid Regimes After the Cold War*, Steven Levitsky and Lucan Way identify a new type of regime that, like fascism, is the product of a distinct historical epoch. The title is potentially misleading, for the authors are clear that competitive authoritarian regimes and hybrid regimes are not the same thing. In fact, the former is a subset of the latter. According to their definition, a competitive authoritarian regime is a civilian regime "in which formal democratic institutions exist and are widely viewed as the primary means of gaining power, but in which incumbents' abuse of the state places them at a significant advantage vis-à-vis their opponents" (p. 5). The book is not primarily about the origins of these regimes. The authors do claim that the rise of competitive authoritarianism is a post-Cold War phenomenon, driven by a combination of a shift in Western foreign policy, political conditionality, and transnational activism. These factors combined to raise the external costs of authoritarianism, leading elites to adopt the trappings of democracy while abusing those same institutions to preserve their power.

This explanation for origins is certainly plausible, if only an initial step in explaining the rise of this new regime type. Still, because of its precise conceptualization and mass of empirical evidence, Levitsky and Way's book will likely draw wide acceptance of the notion of competitive authoritarianism among specialists. Levitsky and Way identify thirty-five of these regimes between 1990 and 2008. Compare this with fascist regimes, of which there was a handful at most, depending on how one counts. Given the mass of theories that purport to explain the origins of fascism, we can probably expect a slew of studies devoted to assessing the impact of U.S. foreign policy, political conditionality, transnational activism, and potentially a range of other causal variables to provide a convincing theory of the rise of competitive authoritarianism.

This is of course a good thing. To fault Levitsky and Way for failing to adequately explain the origins of a new regime type is a bit like criticizing Jared Diamond for taking the size and shape of continents as given, rather than beginning *Guns, Germs, and Steel* a hundred million years earlier.[20] *Competitive Authoritarianism* is one of those rare books that no student of comparative politics or international relations can afford to ignore. It is written so that, with a little guidance, it can be used in both introductory and upper-level courses in comparative politics at the undergraduate level. It is worth reading for the case studies alone, which serve as thumbnail sketches of the political histories of thirty-five countries between 1990 and 2008.

The puzzle that Levitsky and Way pose is this: what explains the diverse trajectories of competitive authoritarian regimes over the last two decades? Why did some democratize? Why did some develop into stable authoritarianism? And why did a third group become unstable authoritarian, meaning that autocrats lost out to other nondemocratic successors? They make a three-step argument that begins with international factors, turns to the domestic politics, and—in some cases—brings international power relations back in to solve the puzzle. Simply put, the subset of competitive authoritarian regimes that were tightly linked with the West, meaning that they possessed a high degree of economic, social, communication, and intergovernmental ties, by and large democratized. These regimes were clustered in Central and Eastern Europe and in Latin America. In those cases in which links with the West were low, as in Africa and Central Asia, the organizational power of incumbents—a product of coercive capacity, elite cohesion, party strength, and state control of the economy—determined the outcomes. When organizational power was high, these regimes became stable authoritarian. If organizational power was not high, then a third factor—Western leverage—determined whether they would become stable or unstable authoritarian. States that were weak economically or politically, or that counted on the support of a powerful external ally for whom democratization was not a foreign policy consideration (Russia in the past, and possibly China in the future), became unstable authoritarian.

The authors spend a chapter outlining the intervening variables connecting their three main independent variables (linkage, leverage, and organizational power) to their dependent variable (regime outcomes). This is a remarkable effort in theoretical synthesis, and it is impossible to do it justice in this review. It is a big-picture book that offers a structural explanation for regime trajectories (Luebbert is cited approvingly here as well), and therefore an alternative to contingency- and leadership-based accounts. As for Slater and Riley, historical processes leave enduring legacies that cannot be overcome easily by simply changing the formal rules of the game. The book is also a challenge to a modernization theory in that levels of economic development do not explain the patterns of democratization, stable authoritarianism, and unstable authoritarianism emerging from competitive authoritarian regimes. One of the central lessons is that institutions determine regime trajectories, even if the institutions that really matter are sticky and therefore not fundamentally altered through formal constitutional design.

Jennifer Gandhi's *Political Institutions under Dictatorship* offers a somewhat different perspective on institutions, one that draws more from a rational choice as opposed to a historical-institutionalism perspective. Like *Competitive Authoritarianism*, Gandhi's book is broad in scope and ambition. Her dichotomous conceptualization of regimes as either democracies or dictatorships means that her universe of cases is much larger than Levitsky and Way's, which allows her to run a number of statistical tests. She defends her minimalist conception of dictatorship as "regimes in which rulers acquire power by means other than competitive elections" (p. 7) in a couple of paragraphs, and thereby chooses not to engage at length with the massive literature on hybrid regimes. This will strike some students as wise, and others as misguided. Indeed, it is difficult to see how both Gandhi and Levitsky and Way can be right. If competitive authoritarianism represents a distinct and widespread regime type, then Gandhi's dichotomous categorization is clearly deficient, and the basic assumption of unit homogeneity is violated in her statistical tests. If Gandhi (and Geddes) are correct, and the useful distinction among dictatorships is whether they are monarchies, juntas, or one-party states, then introducing yet another concept like competitive authoritarianism into the subfield, as do Levitsky and Way, is a mistake. Since this debate is likely to involve larger arguments about the pros and cons of both universal and middle-range theorizing, I will merely flag it here as a potential flashpoint.

Although the universe of cases is larger, Gandhi picks a narrower range of institutions—specifically legislatures and to a lesser extent political parties—to focus upon than do Levitsky and Way. These institutions are not coercive, and they are formal rather than informal. The overarching goal of her book is to demonstrate that these democratic-looking institutions are not simply "window dressing," but that they play a central role in the construction, policymaking, economic performance, and durability of authoritarian regimes. She is extending research on formal institutions under dictatorship,[21] and her book is convincing in two respects: first, that the conventional wisdom that authoritarian institutions are empty shells and therefore not deserving of serious scholarly inquiry is wrong; and, second, that we still do not know nearly enough about them.

Her argument is simple and parsimonious. She assumes that dictators want to stay in power, and that they must deal with threats when they arise. If those threats are not severe, they do not require anything beyond the normal instruments of coercion. But, Gandhi argues, if those threats are significant enough, dictators need to make concessions to outside groups. Since these concessions take the form of policy compromises, institutions are needed in order to signal preferences and to forge agreements. "For the potential opposition, assemblies and parties provide an institutionalized channel through which they can affect decision-making even if in limited policy realms. For incumbents, these institutions are a way in which opposition demands can be contained and answered without appearing weak" (p. xviii). In short, institutions are instruments of co-optation; and this point is the book's main theoretical contribution. This account of origins is clearly inspired by rational choice and functionalism, and Gandhi uses evidence from the case studies of Kuwait, Ecuador, and Morocco in her second chapter to illustrate that her deductions are empirically supportable.

Turning to the effects of institutions, Gandhi argues that institutionalized dictatorships produce outcomes different from those of noninstitutionalized ones. Her measure of institutionalization combines political parties and legislatures as follows: a dictatorship scores a 2 if it possesses a legislature with multiple political parties, a 1 if the regime controls all the seats within the legislature, and a 0 if either political parties are not allowed in the legislature or the legislature is closed (p. 191). Using institutionalization as her independent variable, Gandhi then explores its effects on the provision of public goods, economic performance, and regime survival. She finds that institutionalized dictatorships provide more public goods than noninstitutionalized ones, which supports her argument about policy concessions. Economic performance is higher under institutionalized dictatorships, and Gandhi suggests that this relationship supports her claim that institutionalization forces autocrats to enter into policy compromises that increase the provision of public goods and limits their ability to extract rents.

Her findings on regime survival, which ultimately is her most important dependent variable, are not as clear. Her statistical tests yield no relationship between institutionalization and regime survival, and she is forced to concede that this result could constitute a major strike against her claim that institutions preserve authoritarianism. Yet Gandhi offers another interpretation. Since ultimately the degree of threat determines whether or not dictators will construct institutions to co-opt other elites, lack of institutionalization could simply signal a lack of threat, while strong institutions suggest a strong potential threat. "Because observed institutions reflect a best response on the part of…dictators," she writes, "these institutions do not advantage or disadvantage any of them in terms of power" (p. 178). In this sense, she would not disagree with a line of scholarship that claims that strong political parties and other types of quasidemocratic institutions, in addition to the ones pointed out by Slater and Levitsky and Way, aid regime survival.[22]

It is worth reflecting at this point on how the works reviewed thus far compare to Beatriz Magaloni's *Voting for Autocracy*. To be clear, Magaloni is concerned with a smaller subset of authoritarian regimes than either Gandhi or Levitsky and Way. She includes only those in which "one political party remains in office uninterruptedly under semi-authoritarian conditions while holding regular multi-party elections" (p. 32). She terms these cases hegemonic-party autocracies, and asks two basic questions about them: why they persist, and why they go to such effort to create supermajorities through elections when minimal winning coalitions would be enough to ensure their dominance. Magaloni ingeniously answers the first question by way of the second. In short, by running up huge electoral margins of the victory, the hegemonic party creates an image of invincibility that keeps potential elite defectors bound to it, demonstrates to voters that they have no other viable choice, and exacerbates the coordination dilemmas that opposition forces must face.

Since the book appeared in 2006, and since Magaloni's arguments already have influenced much of the work on authoritarianism, I will focus on two points and leave aside some of the book's subsidiary arguments. The first is her causal mechanism linking patronage to authoritarian stability. Like Slater, Magaloni focuses on maintaining elite cohesion as the central issue in authoritarian durability, and her definition of hegemonic parties as "collusive pacts among ruling party politicians to divide the spoils of office among themselves" (p. 79) is essentially the same as Slater's "provision pacts." Yet while Slater views these pacts as inferior to "protection pacts" in creating durable authoritarianism, Magaloni marshals original empirical evidence to show how the Mexican PRI (her book is primarily a study of Mexico) distributed payments to its voters and punished opposition voters by starving their constituencies of public funds. The primary contribution here is empirical, for the notion that a hegemonic party buys at least some of its support is, of course, not new. What Magaloni effectively demonstrates is that the PRI was remarkably adept at targeting particular constituencies where they were electorally vulnerable, primarily through a government poverty relief program (PRONASOL), and that this strategy worked. Still, Magaloni is forced to address the limits of this strategy in the second half of the book. Leaving aside the econometric and game theoretic models that purport to explain Mexico's democratic transition, Magaloni actually comes quite close to Slater's conclusion when she writes that "economic recession translates into a decrease in economic resources available for patronage and pork, making the hegemonic party more vulnerable to elite divisions, voter defection, and opposition entry" (p. 263).

The second point is Magaloni's conception of when and why institutions are important. On first blush, her account appears to be deeply institutional and therefore in line with the other books included in this review. The PRI, after all, is an institution that seeks to ensure its own survival, and she specifically defines electoral institutions as a "means to regularize payments to their supporters and punishment to their enemies" (p. 19). Yet, in fact, Magaloni departs from treating institutions as causal forces in their own right and instead conceives of them as "*endogenous* to the electoral game" (p. 260, italics in original). It is the ability of hegemonic elites to win supermajorities that matters, for that is what ultimately allows them to shape electoral rules in their favor. So power comes first, and institutions are constructed to preserve it. This may seem like an obvious point, but it is central to keep in mind when dealing with authoritarian regimes. There is a danger in granting too much causal power to constitutions that can be rewritten and legislatures than can be summarily dismissed.

Indeed, it is the asymmetric attention that quasidemocratic institutions have received thus far in the literature that renders Pablo Policzer's *The Rise and Fall of Repression in Chile* such a useful corrective. He writes that "the goal of this book is to understand how authoritarian regimes organize and in some cases reorganize their coercive institutions" (p. 15). He correctly notes that there is a dearth of theoretical treatments of organizations such as the police, the military, and other organizations that monitor their societies and engage in violence against perceived enemies of the regime. Although Slater, and to a lesser extent Levitsky and Way, deal with coercive institutions to some degree, these institutions are not the theoretical focus of their work. In this sense, they share the general assumption that coercion is not a problem as long as rulers maintain the support of those who are doing the coercing.

Policzer's central insight is that organizing coercion is a problematic undertaking, even under the most auspicious conditions for dictators. There are several different manifestations of what Policzer identifies as "the coercion problem." Dictators must craft coercive institutions that can deal with threats without undermining support for the regime. They must also not allow these institutions to become alternative power centers. As Geddes and others have pointed out,[23] most dictators do not fall as a result of mass opposition movements, but rather are replaced by small groups of elites, often military elites. The organization of coercion thus becomes a classic principal-agent problem that demands a form of monitoring by the principal (the dictator) to keep tabs on the agent (the coercive institution).

But monitoring is no easier in dictatorships than in democracies, and the common types of monitoring introduce their own trade-offs. The dictator can pursue a strategy of what Policzer terms "internal monitoring," whereby the dictator sets up his own institutions to keep tabs on the coercive institution. Alternatively, he can rely on a system of "external monitoring," whereby the dictator relies on groups outside of his control—such as opposition forces within the country, foreign governments, or international NGOs—to keep track of the agent. The former strategy is analogous to what students of American politics refer to as a "police patrol," while the latter is the equivalent of a "fire alarm."[24] Both strategies have costs. Internal monitoring can be extremely expensive, create alternative centers of power, and introduce a new principal-agent relationship ("who monitors the monitor?"). External monitoring can limit the ruler's autonomy, and entails at least a partial liberalization whose dynamics the ruler may not be able to control.

Policzer applies this framework to Chile under Pinochet, focusing on the first five years of the dictatorship (1973–1978). Using archival and secondary sources, he explains how Pinochet experimented with different forms of monitoring until an equilibrium was reached. He documents how the blind coercion in the immediate aftermath of the coup introduced principal-agent issues that led Pinochet to construct an internal security service, the DINA, which was charged with monitoring the armed forces. This type of internal monitoring, however, did not solve the problem as the DINA began to engage in its own high profile acts of violence—including the assassination of Chilean opposition figures on foreign (including US) soil—that damaged Pinochet internationally. In addition, the DINA failed to keep accurate tabs on the four branches of the military, and thereby failed in its basic task of monitoring. Pinochet eventually dismantled the DINA and replaced it with the CNI, which was not under his direct control. He also began to rely on a form of external monitoring for information. In short, Policzer uses a parsimonious explanatory framework that helps us understand why Pinochet—and indeed why any dictator—would replace his own internal security service with another, and why he would embrace the seemingly counterintuitive strategy of relying on opposition and outside actors for information about his own coercive institutions.

Institutions and Organizations

All six books emphasize institutional and organizational factors. This is unmistakable in the works of Gandhi and Policzer. The other authors, Magaloni in particular, give structural factors a more prominent role. Linkage and leverage, according to Levitsky and Way, are primarily determined by geographic and material factors. Riley's notion of hegemonic politics appears to be deeply social-structural. Slater focuses on geographic and class variables to explain patterns of elite collective action. Yet institutions and organizations are clearly central to their accounts as well. In Levitsky and Way's framework, the degree of "organizational power" determines regime outcomes when linkage is weak. Riley's key transmission mechanism from contentious politics to fascism is voluntary associations. Slater relies on institutions to explain how provision pacts persist after the violence that produced them has subsided. Thus, for all of these authors, institutions or organizations are critical causal variables.

Each book largely succeeds in demonstrating that institutions and organizations under dictatorship are not just "window dressing." Legislatures and political parties are not merely rubber stamps for autocrats—although they are certainly these things as well—but forums for policy compromises and tools of cooptation. State capacity and party strength help determine whether internal opposition or exogenous shocks lead to authoritarian breakdown. Administering terror involves a host of organizational decisions and processes. To the extent that these points were ignored in previous studies of authoritarianism, these books represent an important correction.

However, given the enormous body of scholarship devoted to institutions and organizations, one might claim that each of these works adopts an overly simplistic view of how institutions and organizations matter. To varying degrees, the authors all use institutions and organizations as convenient placeholders for a variety of different causal mechanisms that are left underspecified. This is true whether the inspiration is rational choice institutionalism, as it is for Gandhi and Policzer, or historical-institutionalism, as it clearly is for Slater and arguably is for Levitsky and Way and Riley.

To pick a couple of examples, consider first Gandhi's central claim that institutions are required for policy concessions in dictatorships. Why? Because once a dictator is threatened enough to concede something to the opposition, institutions are required to lower transaction costs by identifying reliable bargaining partners and revealing information. Although certainly in line with rational choice assumptions, this leaves two issues unanswered. First, why are institutions the only means of making policy compromises? Why can't ad hoc bargaining between potential threats work equally well, and why might this not be the preferred option of dictators?[25] Put another way, why do we need the intervening variable of "institutions" between the independent variable "threat" and the dependent variable "policies"? Second, even if institutions are in fact doing much of the work, is it possible to say more about them than that they are venues for policy compromise? Doesn't this depend on the particular type of legislature or political party? The decision to classify regimes on the basis of whether they have multiple, single, or no institutions does not allow us to pick up institutional variation among dictatorships, much less to see how institutions shape behavior at the micro level.

Slater's treatment of institutions also fails to unpack the causal mechanisms linking institutions to outcomes. To be sure, part of this has to do with the fact that Slater is concerned with institutions largely as a dependent rather than an independent variable. His causal arrows run from contentious politics, to elite coalitions, to institutions. Yet he does claim that protection pacts are reproduced through two types of mechanisms—the first is attitudinal and the second is institutional. "The more powerful of the two is institutional," Slater claims, for "merely by organizing actors in particular ways at the outset of a new political dispensation, leaders create structures that assume a movement of their own. Expectations converge, relationships are forged, and interests adapt to prevailing institutional frameworks" (p. 18). This is, of course, the language of path dependence, and one cannot fault Slater for relying on the logic of path dependence to support a part of his argument. Yet these mechanisms strike me as too general to provide a convincing account of institutional reproduction. It is unclear what the increasing returns of institutional arrangements are, how loyalty to existing institutions is sustained, and where endogenous institutional change might come from.[26] Although Slater has dealt with the latter issue in other places, the case studies in *Ordering Power* do an excellent job of describing and measuring the institutions of the authoritarian leviathans, but are less geared toward demonstrating how institutions shape the calculations of political actors and make alternative outcomes less likely when the world around the actors changes dramatically.[27]

Riley's book also fails to specify exactly how organizations—in this case voluntary associations—are linked to fascist outcomes. He notes that "associations provided organizational resources to the fascists" (p. 68) and that the "fascists had entrenched themselves in civil society" (60–61). The causal mechanism would thus appear to be the colonization of preexisting groups by fascists, and the use of these groups to extend their influence in society and, ultimately, in government. This is a familiar story, although it has certainly been told more in the German context than in the Italian, Romanian, or Spanish cases.[28] But an alternative account would be that voluntary associations—particularly those with leftist goals—provoked a counter reaction from elites, and that it was not associations themselves but the threat of their influence that led to fascism. Looking at the evidence, it is difficult to tell which of these causal mechanisms is more compelling. Since Riley does not spend much time teasing out the micro mechanisms of his theory, which is not surprising given his reliance on Gramscian concepts, the reader is left wondering about the precise role that associations play.

To some extent, each of the authors can be forgiven for privileging macro over micro factors. With the exception of Policzer, their concern is demonstrating that institutional variables provide a more convincing account than the alternatives. Given the number of both countries and years that each author attempts to cover, there is scarcely room for a micro-level account of institutions. Yet having discovered that "institutions matter" in understanding authoritarian regimes, the next step for scholars is to unpack exactly how they do.

Finding the Evidence

One potential problem with pursuing richer and more nuanced types of institutional analysis is that finding the evidence to support such accounts might be challenging at best, impossible at worst. The problems of conducting field work in authoritarian or hybrid regimes are obvious. Access is often denied, and field work can be dangerous to both the researcher and his/her interview subjects. To provide one illustration, Lisa Wedeen's book on symbolic power in Syria was based on two years of field work, yet she was unable to include much in the way of interview material with government officials or ordinary citizens.[29] While there may be strategies for overcoming some of these problems, it would be foolish to hold students of authoritarian or hybrid regimes to the same standards of data quality as students of advanced industrial democracies.[30]

Given this limitation, how well do the six books deal with issues of evidence? Each uses a very different approach to theory testing, and taken together the books outline the menu of methodological options. As one would expect, each approach has strengths and weaknesses.

Policzer's diachronic comparison of the Chilean case, not surprisingly, provides the greatest level of detail of the six books (though Magaloni's exhaustive treatment of budget cycles under PRI hegemony as well as her analysis of PRONASOL funding patterns stand out as well). Policzer pieces together evidence from many different sources, including: minutes of government meetings under the Junta, testimony collected by the Chilean National Committee on Truth and Reconciliation, declassified CIA documents, contemporary newspaper reports, and interviews conducted in the mid–1990s. Given the theoretical focus on organizational dynamics and elite decision making, Policzer needs every bit of this material to make his account compelling and to demonstrate the utility of his principal-agent framework. The result is a study that largely succeeds in its aim to "shine a light" on "the darkest spaces of politics" (p. 15). The historical narrative also makes for a good read, one that should find a wide readership among students of Latin American politics and that can be used in undergraduate teaching.

Yet the book also has the feel of a mystery that is never entirely resolved. The problem is that Policzer often lacks the type of "smoking gun" evidence needed to support his theory. In the absence of the minutes of some key meetings (some of which may eventually be declassified), written correspondence between Augusto Pinochet and Manuel Contreras, or interviews with key participants (which may or may not be reliable), Policzer is necessarily tentative about his conclusions. Even his central contention that the purpose of the DINA was to monitor other coercive institutions rests on a combination of logical deductions and counterfactual reasoning. The holes in the evidence give a speculative aura to what is otherwise a meticulously researched study. It also stands as a warning to other scholars looking to undertake qualitative research on the workings of coercive institutions. If an exhaustive examination of all the existing evidence of a single case by a country expert still leaves the reader with some unanswered questions, then what are the chances that a less intensive case study of the "dark spaces of politics," such as those in article-length treatments or as parts of a multiple case comparison, can surmount a reasonable evidentiary hurdle? Although the field needs more work like Policzer's, this is an important issue to consider.

Gandhi adopts a methodological strategy very different from that of Policzer. Although she draws upon three case studies to "provide some intuitions" to inform her arguments, these "qualitative snapshots" really do not succeed in demonstrating exactly how institutions work in authoritarian settings (p 44). The reliance on secondary sources and the absence of fine-grained detail render her narratives about authoritarian institution-building in Kuwait, Morocco, and Ecuador plausibility probes rather than rigorous case studies. Gandhi is up front about this limitation, and the empirical heart of the book is comprised of a statistical test of the observable implications of her theory (which she lays out formally in chapter three). Chapter four tests her predictions that institutionalized dictatorships spend more on public goods and provide their subjects with more liberties and freedoms since they allow for policy compromises with the population. Chapter five assesses her claim that institutions have a positive impact on economic growth since they provide for political stability, greater information, and policy predictability. Chapter six analyzes the relationship between institutions and regime survival. Here Gandhi predicts a statistically insignificant relationship according to the following logic: "because dictators formulate their institutional strategies as a best response to the conditions they face, those rulers who choose to rule with institutions should not survive significantly longer than those who govern without them (xxiv)."

It is striking that Gandhi runs into evidentiary challenges similar to those of Policzer. Lacking good data on public spending in dictatorships, she is forced to rely on the "crude proxy" of military spending (p. 111). She finds a positive relationship here. She also finds that civil liberties are more expansive in institutionalized as opposed to noninstitutionalized dictatorships. Yet when she analyzes the effect of institutions on social spending—which is arguably the best test of her argument about policy compromise—she finds no relationship. Puzzling through this unexpected finding, Gandhi suggests that the issue is either that the data are poor (only 17 percent of the sample included data on social spending) or that her causal mechanism linking institutions to social spending is incorrect. Her findings in chapter five linking institutions to economic growth are more persuasive, although her data do not allow her to tease out the causal mechanism linking the two variables. As noted earlier, chapter six concludes with a null finding on the relationship between institutions and regime survival, which is either consistent with Gandhi's explanation that dictators adjust their strategies given the degree of threat, or is evidence against her claim that institutions are useful instruments in co-opting potential opposition.

What all this means is that Gandhi's conclusions are as tentative as Policzer's. She is forced into phrases like "not wholly unreasonable" (p. 133), "it is possible to conclude" (p. 179), and "indirectly supports the notion" (p. 161) when defending some of her proxies and interpreting some of her findings. Yet rather than criticizing her for signaling weakness, although some readers will take issue with her degree of hedging, Gandhi is refreshingly transparent about the limitations of her data and research design. Perhaps using case studies to elaborate on her statistical findings, rather than to merely establish the plausibility of her argument, would have led to greater confidence in her findings.

If both a diachronic case study and a large cross-national comparison have trouble mustering enough evidence to support their claims, then perhaps a medium N strategy is the way to go? Levitsky and Way argue that their medium N analysis allows for greater measurement validity and better illustrations of causal mechanisms than a large N approach. And with thirty-five cases worldwide, their research design allows for significant variation on both the dependant variable (regime outcome) and potential explanatory factors.

The case study evidence certainly supports the general argument. Yet some foreseeable problems emerge when trying to fit thirty-five cases into a book of less than 400 pages. With a little more than six pages devoted to each case on average, many of the studies are actually justifications of the coding (how much linkage, leverage, and organizational power) rather than an exploration of causal mechanisms. Some of the less-studied cases, such as Guyana and Mali, rely on the views of a couple of experts. There is no room to seriously entertain alternative explanations. Moreover, the brevity opens space for the charge that the authors are only marshaling evidence consistent with their explanation.

Would it have made more sense to include one longer case study (or possibly two) in each region, and bundle the other cases into an appendix? The answer depends on taste. The thirty-five cases demonstrate that the argument seems to travel well across countries and regions, and this will count as excellent evidence for some comparativists. Others will probably prefer more detail and better treatment of causal processes. Country specialists will most likely agree with the basic outlines of Levitsky and Way's case studies, where they have done a heroic job of summarizing. But country experts may also dispute the codings and readings of particular cases.

Such a reaction is less likely in Riley and Slater's cases, as both use a small N case study approach. Riley looks at three countries, while Slater analyzes three countries in depth and includes a chapter in which he looks at four other "congruent cases" to extend the argument. While neither book is based on extensive archival materials or field work (although Riley has some of the former, and Slater some of the latter), practitioners of comparative historical analysis generally have not viewed relying on secondary sources as a problem.[31] Riley draws on sources in five different languages (one cannot fault him for not using Romanian sources), and it is difficult to imagine anyone contesting Slater's grasp of Indonesian, Malaysian, or Philippine history.

As one would imagine, Riley and Slater's accounts leave more room to tease out the key causal mechanisms and marshal evidence in support of them. Yet, even here, one could take issue with some of the empirics. Riley's central argument is that associations led to fascism under conditions on nonhegemonic politics. Putting aside the thorny issues of whether the three outcomes were in fact fascist, and whether one can come up with a coding scheme that would differentiate hegemonic from nonhegemonic politics, the evidence linking voluntary organizations to fascist development is pretty thin. He asserts that, in Italy, "fascists had entrenched themselves in civil society," yet he lacks the type of micro evidence to demonstrate his case (p. 60). With regard to Spain, he demonstrates a correlation between fascist groups and associational density, though this leaves room for an alternative reading—that the fascists did not capture preexisting organizations but rather emerged to contest left-leaning voluntary associations. As for Romania, Riley provides data on the absolute number of rural associations, but there is no way of telling how large they were, how important they were, and whether the fascists succeeded in capturing them. In Riley's defense, finding good evidence of fascist takeover of existing voluntary organizations may be tricky. But a more detailed case study of at least one organization in each of the three countries would have helped his argument significantly.

Of the six books, Slater's evidence is the most persuasive (with Magaloni's a close second). The combination of the three central cases and four congruent ones allows both for empirical richness and a wider scope. Yet one problem that recurs throughout the case material is in demonstrating empirically the degree of threat that elites perceived. Slater is aware this problem, and tries to head it off in the introductory chapter, noting that "since human perceptions are not directly observable, this book's strategy is to lay out a deductively grounded framework as to what types of contentious politics should be considered most challenging and threatening to the widest range of elite actors" (p. 13). It certainly seems that elites perceived events as Slater's theory predicts, and one could claim that verifying elite views is either impossible or unnecessary. Yet while perceptions may not be observable, students of international security have long wrestled with the problem of measuring elite threat perception, and of finding evidence to demonstrate its intensity.[32] Given the centrality of elite threat to his argument, more empirical evidence would have made Slater's argument even more convincing.

Slater is certainly not alone in this regard. Gandhi takes elite threat perceptions as a given, although this strategy is not a major problem for her argument. More broadly, contemporary work on democratization has taken the strategy of deducing elite threat from levels of inequality and the mobility of elite assets.[33] This leaves no room for the possibility of misperceptions, deliberate threat inflation, or a whole host of deviations from a bare-bones, rational threat model.[34] Indeed, there are good reasons to believe that dictators in particular will perceive threats differently than one would expect given objective material factors, such as the type of contentious politics or the strength of the opposition. As seen clearly in the case of Saddam Hussein, dictators may receive limited information from their henchmen and, over time, may form ideas that are wildly at odds with reality. They may fail to take note of some threats, and deliberately inflate others. They may manufacture threats to achieve certain goals.

Although getting inside the head of a dictator is of course impossible, the importance of elite ideas should not be ignored. A generation of Soviet scholars relied on a host of creative practices to get inside the heads of decision makers in the Kremlin as best they could. It would be difficult to argue that any regime—with the possible exception of North Korea—is as impervious to would-be researchers as the former Soviet Union. The problems may be more formidable with historical cases, but even here contemporary journalistic accounts and archival material, where available, can help us make reasonable suppositions about how dictators—or even elites in a relatively open society like Wilhelmine Germany—perceived threats to their rule. I do not suggest that scholars abandon deductive logic, formal modeling, statistical inference, or any of the standard practices in contemporary comparative politics in favor of an interpretive practice like Kreminology. Yet given the massive technological changes that now allow a researcher to download all sorts of primary source materials, not making the effort to do so will, I believe, impoverish our work.

Future Work

Since criticizing outstanding work in our field is an easy enterprise, let me make clear that the points raised in this review are akin to those of a satisfied diner looking for extra helpings. More attention to institutions and organizations would be welcome, but let us not forget that these authors were among the first to identify institutions and organizations as important variables in the study of authoritarianism. More micro-level evidence and attention to causal mechanisms is desirable, but a reader can still learn a great deal about the workings of actual authoritarian regimes by reading any of these books.

To single out one issue that deserves some rethinking, it is the tendency to focus on the democratic-looking features of authoritarian regimes at the expense of, ironically, their authoritarian ones. Slater, Policzer, and Levitsky and Way have already started to reverse a trend toward downplaying the coercive aspects of authoritarian regimes. As Slater writes in a chapter on institutional change, "dictators use institutions first and foremost to craft collective compliance, and only secondarily to solicit policy advice or to offer influence in exchange for support."[35] One could argue that a research agenda based too closely on either Gandhi's or Magaloni's book would misdirect our attention away from the core features of authoritarian institutions. This is not to deny the value in demonstrating that political parties and legislatures in authoritarian regimes matter. Yet just as Gandhi succeeded in showing that institutions are far more consequential than the conventional wisdom would have it, Policzer's observation that coercion does not follow seamlessly from the wishes of the dictator can potentially generate the types of puzzles that make for a productive research agenda. I list four possible puzzles below.

Puzzle number one: why do some dictators succeed in building effective coercive institutions while others fail? To take one historical example, we know that Benito

Mussolini aspired to create a totalitarian society—it was he who popularized the term "totalitarian" in the first place—in which coercive institutions would enforce complete compliance and intrude into the private lives of all citizens. To this end, he founded the OVRA, which Hitler looked to as a model for the Gestapo. Yet while the Nazi secret police generally lived up to Hitler's expectations, if not to its post-facto reputation for omnipotence, OVRA was largely a failure. Indeed, there are most likely many failed cases of coercive institution building that have escaped our notice. Obviously, looking at them—and at variations among "successful" coercive institutions—will help us better understand how violence is institutionalized in authoritarian regimes.

Puzzle number two: how do authoritarians prevent coercive organizations from undermining them? A dictator need not to have read Geddes to realize that the most likely threat to their rule—and their life—will come from within the ruling class than from the opposition. Military, paramilitary, and secret police elites are likely candidates for coup attempts. What strategies do dictators use to avoid this fate while maintaining a grip on the coercive institutions that they need to survive? Is there a logic to the purges that characterize many authoritarian regimes? If the costs and benefits of strong coercive institutions are well known to dictators, why do they so often miscalculate?

Puzzle number three: why are some coercive institutions more violent than others? If Policzer is correct that Pinochet—and by extension other dictators—would have preferred calibrated to brute violence, then why do we observe very different patterns across countries? What prevents dictators from finding the "optimum" level of violence that keeps potential opposition at bay without completely undermining domestic and international legitimacy? And a separate but related question: what explains variation in the same coercive organization across time?

Puzzle number four: who exactly are the "thugs" that many contemporary dictators seem to rely on to suppress the opposition? The large literature devoted to the question of "who were the fascists" taught a lot about the social profile and organizational dynamics of fascist movements, and also provided a window into their basis of support, at a time when electoral data were either nonexistent or unavailable at the individual level.[36] Although researching this question is likely to be some combination of difficult and dangerous, asking similar questions about paramilitary organizations (or street gangs) in authoritarian and hybrid regimes is necessary in order to really understand the nature of contemporary coercive institutions.

The puzzles are many, and the theoretical and empirical challenges to solving them should not be underestimated. But these issues are likely to become even more important to the field of comparative politics over the next decade. Rather than having crested, there are good reasons to believe that research on authoritarianism will continue apace. It is too soon to tell whether the Arab Spring will lead to democratic consolidation or the reemergence of authoritarian leviathans. At any rate, the sudden collapse of what were once thought to be among the most durable of authoritarian regimes in the Middle East has already forced specialists of the region to rethink some of their theories.[37] And, if Levitsky and Way are correct that the rise of competitive authoritarianism was the result of particular historical epoch in which the geopolitical environment allowed the

West the luxury of concentrating on democracy promotion at the expense of traditional security goals, the implications for democratization are not very rosy. The unipolar moment, the end of history, or whatever else one calls the 1990s produced a wave of flawed democracies, many of which had reverted back to authoritarianism a decade later. If this is the best case scenario for democracy, what can we expect as international relations are increasingly influenced by other powers that have little inherent interest in democracy promotion, and may even seek to work against it?

NOTES

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1. Fareed Zakaria, "The Rise of Illiberal Democracy," *Foreign Affairs*, 76 (November/December 1997).

2. Barbara Geddes, "What Do We Know About Democratization After Twenty Years?" *Annual Review of Political Science*, 2 (1999): 115–44. I thank Barbara Geddes for not objecting to my use of a similar title for the present review.

3. The literature on both totalitarianism and military regimes is massive. Classic works on the former include Carl J. Friedrich and Zbigniew Brzezinski, *Totalitarian Dictatorship and Autocracy* (Cambridge: Harvard University Press, 1956); and Hannah Arendt, *The Origins of Totalitarianism* (New York: Meridian, 1951). On military regimes, see Eric Nordlinger, *Soldiers in Politics: Military Coups and Governments* (Englewood Cliffs, NJ: Prentice Hall, 1977); and Alfred Stepan, *Rethinking Military Politics: Brazil and the Southern Cone* (Princeton, NJ: Princeton University Press, 1988).

4. Jason Brownlee, *Authoritarianism in an Age of Democratization* (New York: Cambridge University Press, 2007).

5. This review is not meant, however, to be exhaustive. One of the several important research programs it does not cover is the economic performance of authoritarian regimes, and the effect of economic crises on authoritarian survival. For an example of the latter, see Thomas Pepinsky, *Economic Crises and the Breakdown of Authoritarian Regimes* (New York: Cambridge University Press, 2009).

6. Given space constraints, I do not consider the transitions from authoritarianism to democracy in this review. It should be noted that Levitsky and Way, Magaloni, and to a lesser extent Slater deal explicitly with democratization.

7. Two recent examples are Jeremy Weinstein, *Inside Rebellion: The Politics of Insurgent Violence* (New York: Cambridge University Press, 2007); and Scott Strauss, *The Order of Genocide: Race, Power, and War in Rwanda* (Ithaca: Cornell University Press, 2006).

8. James Mahoney and Dietrich Rueschmeyer, *Comparative Historical Analysis in the Social Sciences* (New York: Cambridge University Press, 2003). See, also, the recent special issue edited by Daniel Ziblatt and Giovanni Capoccia, "The Historical Turn in Democratization Studies: A New Research Agenda for Europe and Beyond," *Comparative Political Studies*, 43 (August/September 2010): 931–68.

9. Gregory Luebbert, *Liberalism, Fascism, or Social Democracy: Social Classes and the Political Origins of Regimes in Interwar Europe* (New York: Oxford University Press, 1991).

10. Ioannis Evrigenis, *Fear of Enemies and Collective Action* (New York: Cambridge University Press, 2008).

11. Brian Downing, *The Military Revolution and Political Change: Origins of Democracy and Autocracy in Early Moderns Europe* (Princeton: Princeton University Press, 1992).

12. The classic statement is Charles Tilly, "War Making and State Making as Organized Crime," in Peter Evans, Dietrich Rueschmeyer, and Theda Skocpol, eds., *Bringing the State Back In* (New York: Cambridge University Press, 1985), pp. 169–87. See, also, Thomas Ertman, *Birth of the Leviathan: Building States and Regimes in Medieval and Early Modern Europe* (New York: Cambridge University Press, 1997).

13. Sheri Berman, "Civil Society and the Collapse of the Weimar Republic," *World Politics*, 49 (April 1997): 401–29; Robert Putnam, *Bowling Alone: The Collapse and Revival of American Community* (New York: Simon and Schuster, 2000).

14. Peter Fritzsche, *Germans Into Nazis* (Cambridge: Harvard University Press, 1998).

15. Scholars of fascism that use this definition include Michael Mann, *Fascists* (New York: Cambridge University Press, 2004); and Robert Paxton, *The Anatomy of Fascism* (New York: Knopf, 2004). On the definitional debate, see Stanley Payne, *A History of Fascism: 1914–1945* (Madison: University of Wisconsin Press, 1995).

16. Benito Mussolini and Giovanni Gentile, "The Doctrine of Fascism," in Benito Mussolini, *Fascist Doctrine and Institutions* (Rome: Ardita Publishers, 1935), pp. 7–42.

17. Mann, *Fascists*. Barrington Moore, *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston: Beacon Press, 1966).

18. Juan Linz and Alfred Stepan, eds. *The Breakdown of Democratic Regimes: Europe* (Baltimore: Johns Hopkins University Press, 1996).

19. It is odd that Riley does not spend much time defending his classification of Spain and Romania as fascist regimes as the consensus among historians is that both were conservative dictatorships—albeit with some of the trappings of fascism—as opposed to fascist ones. See Stanley Payne, *Fascism in Spain, 1923–1977* (Madison: University of Wisconsin Press, 1999). In Romania, the fascists were violently repressed first by King Carol and later by General Antonescu.

20. Jared Diamond, *Guns, Germs, and Steel: The Fates of Human Societies* (New York: W.W. Norton, 1997).

21. See, for example, Ellen Lust-Okar, *Structuring Conflict in the Arab World: Incumbents, Opponents, and Institutions* (New York: Cambridge University Press, 2005).

22. See Geddes; Brownlee; and Benjamin Smith, "Life of the Party: The Origins of Regime Breakdown and Persistence under Single-Party Rule," *World Politics*, 57 (Spring 2005): 421–51.

23. Geddes.

24. Matthew McCubbins and Barry Weingast, "Congressional Oversight Overlooked: Police Patrols versus Fire Alarm," *American Journal of Political Science*, 28 (1984): 165–79.

25. Students of international relations may be reminded of a similar debate between those who believe that, drawing on Ronald Coase, bilateral bargaining can reach pareto efficient outcomes with or without institutions. See Coase, "The Problem of Social Cost," *Journal of Law and Economics*, 3 (Autumn 1960): 1–44; and the application of Coasian bargaining in John A.G. Conybeare, "International Organizations and the Theory of Property Rights," *International Organization*, 34 (Summer 1980): 307–44.

26. James Mahoney and Kathleen Thelen, eds., *Explaining Institutional Change: Ambiguity, Agency, and Power* (New York: Cambridge University Press, 2010).

27. Dan Slater, "Institutional Complexity and Autocratic Agency in Indonesia," in Streek and Thelen, pp. 132–67.

28. See Berman; and William Sheridan Allen, *The Nazi Seizure of Power: The Experience of a Single German Town 1922–1945* (New York: Franklin Watts, 1965).

29. Lisa Wedeen, *Ambiguities of Domination: Politics, Rhetoric, and Symbols in Contemporary Syria* (Chicago: University of Chicago Press, 1999).

30. See J. Paul Goode, "Redefining Russia: Hybrid Regimes, Fieldwork, and Russian Politics," *Perspectives on Politics*, 8 (December 2010): 1055–75.

31. But perhaps it is time that it should be. As a recent debate in the *American Political Science Review* revealed, simply relying on secondary accounts for coding data is not likely to produce consensus. There are numerous reasons for this, the most important being that scholars can choose a historical account that best supports their theory. Tests of causal arguments that use primary source material do not suffer from this problem. On the debate, see Marcus Kreuzer, "Historical Knowledge and Quantitative Analysis: The Case of the Origins of Proportional Representation," *American Political Science Review*, 104 (May 2010): 369–92; Thomas Cusask, Torben Iversen, and David Soskice, "Coevolution of Capitalism and Political Representation: The Choice of Electoral Systems," *American Political Science Review*, 104 (May 2010): 393–403; Carles Boix, "Electoral Markets, Party Strategies, and Proportional Representation," *American Political Science Review*, 104 (May 2010): 404–13. For an example of testing established arguments using primary source material, see Daniel Ziblatt, "Shaping Democratic Practices and the Causes of Electoral Fraud: The Case of Nineteenth Century Germany," *American Political Science Review*, 103 (2009): 1–22. See also Daniel Ziblatt, "Does Landholding Block Democratization? A Test of the 'Bread and Democracy' Thesis and the Case of Prussia," *World Politics*, 60 (2008): 610–41.

32. For a recent contribution to this literature, see A. Trevor Thall and Jane K. Cramer, eds., *American Foreign Policy and the Politics of Fear* (New York: Routledge, 2009).

33. Daron Acemoglu and James Robinson, *Economic Origins of Dictatorship and Democracy* (New York: Cambridge University Press, 2006); Charles Boix, *Democracy and Redistribution* (New York: Cambridge University Press, 2003).

34. On this point, see Nancy Bermeo, "Interests, Inequality, and Illusion in the Choice for Fair Elections," *Comparative Political Studies*, 43 (2010): 931–68.

35. Slater, "Institutional Complexity," p. 135.

36. S. Larsen et al., eds., *Who Were the Fascists? Social Roots of European Fascism* (Oslo: Universitetsforlaget, 1980); See, also, Mann; and William Brustein, *The Logic of Evil: The Social Origins of the Nazi Party, 1925–1933* (New Haven: Yale University Press, 1996).

37. On the durability of authoritarianism in the Arab world, see Oliver Schlumberger, ed., *Debating Arab Authoritarianism: Dynamics and Durability in Nondemocratic Regimes* (Palo Alto: Stanford University Press, 2007); Eva Bellin, "The Robustness of Authoritarianism in the Middle East: Exceptionalism in Comparative Perspective," *Comparative Politics*, 36 (January 2004): 139–57; Brownlee; Steven Heydemann, *Authoritarianism in Syria: Institutions and Social Conflict*, 1946–1970 (Ithaca: Cornell University Press, 1999). For a critical reflection on the failure of academics to predict the Arab Spring, see Gregory Gause, "Why Middle East Studies Missed the Arab Spring: The Myth of Authoritarian Stability," *Foreign Affairs*, 90 (July/August 2011).