

CONCLUSION

Peru's 1931 presidential election was a critical turning point in that country's political history. Prior to this event, the repertoire of go-to political practices included political militarism (including *caudillo* politics, coup d'état, and authoritarian rule), the activation of clientelistic obligations, formal and informal strategies of disenfranchisement, electoral corruption, and the brokering of deals amongst political elites. While the country had seen brief episodes of bottom-up insurrection, as well as limited experiments in top-down popular mobilization (based largely on clientelism rather than a valorization of "the people" as such), nothing like populist mobilization had been practiced on a national scale to seek elected office. After this event, populist mobilization became a tried-and-true practical modality that would be repeated time and again by those aspiring to secure or maintain political power and legitimacy. Explaining this shift has been the principal aim of this book. Ultimately, I have argued that populist mobilization entered into the Peruvian political repertoire in 1931 because organized outsider political actors—constituted as such and contingently empowered by the changing dynamics of the political field—recognized the limitations of routine political practice and had the socially and experientially conditioned understanding, vision, and capacities to modify, transpose, invent, and recombine practices that matched up with the changing social and political context of action. The product of this situated political innovation was a distinctly Latin American style of populist mobilization—a mode of practice that became increasingly routinized after the fact.

While the rise of populist mobilization in Peru was decidedly not an automatic byproduct of social-structural or political conditions, the social and political contexts of action impinged on the innovative moment in critical ways. As shown in chapter 2, changing social conditions (economic development

and depression, the expansion of transportation and communication infrastructures, internal migration and urbanization, the disruption of traditional social relationships, working class formation, and the rise of new forms of civic association) produced new grievances, made new groups of potential supporters both politically available and logistically reachable, and laid the social groundwork for political organization and mobilization. This created new possibilities for political action, especially to outsider political actors who were disposed by necessity to seek out and recognize the novel opportunities afforded by these changes. At the same time, as shown in chapter 3, four distinct periods of reconfiguration of the political field resulted in the crystallization of at least semi-organized collectivities around the charismatic figures of Victor Raúl Haya de la Torre and Luis M. Sánchez Cerro. While occupying outsider positions, the forces of Aprismo and Sánchezcerrismo were also contingently empowered to act in innovative and potentially efficacious ways coming into 1931 (by their enjoyment of broad popular support, their exposure to practices from outside the traditional Peruvian repertoire, and the political opportunities afforded by the changing field—most notably, Leguía's disempowering of the traditional political elite and the professionalized military's positive stance toward democratization).

Still, while they played critical roles in shaping and channeling the possibilities for political innovation, these social and political realities did not render it inevitable, nor did they determine what the new mode of practice would look like. Rather, as shown in chapter 4, populist mobilization only emerged when the forces of Haya de la Torre and Sánchez Cerro confronted and recognized a crisis in the applicability of routine practice to the changing social and political context of action. That they would do so was not a foregone conclusion. Other political actors facing the same situation—on the right, the fragmented remnants of the elite parties, and on the left, the doctrinaire leadership of the Partido Comunista Peruano—either failed to recognize it as problematic (in the pragmatist sense) or, in the cases of those who did recognize it as such, were unable to envision any acceptable alternative response to it. But the nontraditional political experiences of the Aprista and Sánchezcerrista leadership, which were a contingent result of their earlier political marginalization and particular paths to prominence, enabled them to develop savvy understandings of how changing conditions were unsettling their political routines, and then to cobble together novel packages of practices that were appropriate to the new context of action. As the two candidates began to enjoy increasing success from their early experiments in populist mobilization, their relational focus shifted from the adversaries who had most preoccupied them previously (for Haya, the Communists; for Sánchez

Cerro, the rightist elites and military junta) to one another. As shown in chapter 5, a competitive dynamic developed between the forces of APRA and Unión Revolucionaria, in which continued experimentation, imitation, and one-upmanship pushed them to amplify their practices right up to election day. It was through these processes that strikingly different political actors ended up converging to elaborate what can now be recognized as populist mobilization in Peru in 1931.

While Sánchez Cerro won the election by a significant margin, what mattered most for the history of political practice in Peru was that populist mobilization had produced impressive results for both candidates. As discussed in chapter 6, the practice resonated with popular audiences and decidedly trumped the practical approaches of all others who had tried to take advantage of the political moment. This fact was recognized by Haya de la Torre and Sánchez Cerro, but also by later Peruvian politicians, who would go on to repeat the practice in new situations. Populist mobilization became routinized—not in the sense that it became chronic, but in the sense that it became something that *could* reasonably be done, and done successfully. Through this process, the Peruvian political repertoire was revolutionized.

Distillation of a Pragmatist Approach to Repertoire Change

In the introductory chapter, I sketched out the intellectual scaffolding for a pragmatist approach to repertoire change based on the concept of *situated political innovation*. Now, at the conclusion of what has been an extended exercise in theoretically informed historical explanation, it is worth taking a moment to distill the lessons learned more schematically. I have suggested that it is useful to understand repertoire change in a particular way: as a product of the elaboration and routinization of a new mode of political practice. This understanding of the outcome suggests certain imperatives for the explanatory framework. It must be able to account for the elaboration of a new practice, for why that practice looked as it did and not otherwise, and for why that practice became routinized. In what follows, I will address each of these points in turn, doing my best to specify clearly what I take to be the abstract steps of my theoretical argument.

First, how to account for the elaboration of a new political practice? Clearly creativity is involved—but not the sort of romantic and highly individualized genius that is so often attributed to the artistic, scientific, and technological savants of the world (cf. Becker 1982; De Nora 1997; Lamont 1987). Creativity is a universal human endowment, even if habit is a common human response to situations (Joas 1996). The question is not *who* is exceptionally creative, but

rather *what prompts social actors to respond in creative rather than habitual ways to particular situations?* The pragmatist approach outlined in the introductory chapter suggests that political innovation happens when habitual responses to a given situation fail to yield adequate results for political actors, producing a problem situation. But this must be unpacked and extended a bit.

It is not enough for a habitual response to fail. If it is to trigger a shift to creative action, this failure must be recognized as such, and its reasons must be attributed to the inadequacy of the habitual response to the situation. That is, the failure and its reasons must be recognized and understood. At the same time, it is not always necessary for things to come to the point of *actual* failure. Because human action is often undertaken with a view to the future, in anticipation of possible outcomes, a shift from habit to creativity can also be occasioned by a *projection* that routine action is likely to fail in the face of a given situation. Of critical note here is that the emergence of a habit-disrupting problem situation requires recognition, understanding, and future projection, all of which are conditioned not by an individual's cognitive capacities alone, but also by his or her socially shaped experiences and experiential learning; and this is all the more the case when the actor in question is a collective one, in which pooled experiences are brought to bear in processes of deliberation.

Once a problem situation has emerged for a collective political actor, novel ways of responding to it must be discovered through creative understanding, projection, and experimentation. This can involve modifying routine practices, transposing practices from other times and places (from sometimes more, sometimes less, analogous situations), transposing practices from non-political domains, or inventing new practices out of whole cloth. Unless an entire strategic package is imported wholesale from another context (in which case, we would no longer be speaking of innovation per se), the modified, transposed, or invented practices must be recombined in a way that gives at least the impression of internal consistency and is appropriate to the situation at hand. All of this emerges through a self-corrective process of trial and error over time and requires creativity, flexibility, and a savvy understanding of the practice-situation relationship. Again, it is not special cognitive abilities or inexplicable genius that enable successful creativity of this kind, but experience, exposure, knowledge, and discernment that are products of the social trajectories and relational positionings of those making the strategic decisions.

But it is not enough to come up with alternatives in the face of a habit-disrupting situation. For a new political practice to emerge, it must be executed. This requires both autonomy of action and the resources to act

Internal and external constraints—such as strategic disagreements amongst party leaders, subservience to external organizations, problems of coordination with allies in the political field, and legal rules or procedures—can undermine a political collectivity's autonomy of action. At the same time, a lack of social, organizational, and material resources can impede that collectivity's ability to carry out the line of action that it envisions. Only with the autonomy and resources to act can a collective political actor elaborate a new mode of practice through creative experimentation in response to a habit-disrupting problem situation.

The second imperative of the explanatory framework is that it be able to account for why the new political practice looked as it did. It is important to be clear that the characteristics of a new practice are not *dictated* by the nature of the problem situation (let alone by more general social or political conditions), as different actors might come up with any number of creative readings of and responses to a set of disruptions and opportunities. To explain why a new practice looked as it did, it is necessary to trace the social sources of experiential influence conditioning how specific political actors made sense of the context of action and generated a set of practical alternatives to match it. In the case of a collective actor, this means tracing the personal trajectories, worldviews, and social locations of all individuals who were in a position to impact the group's strategic decision making, as well as understanding the deliberative dynamics of the group and how these shaped its choices about what lines of action to pursue. Ultimately, what we must investigate if we are to explain the characteristics of a new practice is how the combined stock of social experience of a body of individuals shaped their understanding of the social and political realities of the situation, their conception of the set of possible new actions on which they might draw or which they might modify to fit that situation, their assessment of the reasons for the success or failure of their own actions (past and present), their readings of how their opponents were acting or were likely to act, and their future projections of the likely outcomes of various possible lines of action, given the nature of the situation.

Third, what does it take for a new political practice to become routinized? New practices are often—perhaps even typically—ephemeral. If political innovation is spurred by desperation in the face of a problematic situation, and shaped by desperate searching for *untried* yet hopefully still true solutions in the face of it, it only stands to reason that its product would often be inadequate or otherwise unappealing to others. For a political practice to become routinized, it must match up with the situation in a way that produces results (even if not unqualified success). It must resonate, at least to some extent,

with the intended audience. This resonance must then be recognized by others and understood as a mark of promise—as an indication that the practice might yield favorable results in future analogous scenarios. Finally, the practice must be repeated. As it is reapplied to new situations by the original or other actors, it develops a track record and becomes increasingly familiar, thinkable, doable. If this happens, the practice can be said to have entered into the political repertoire, and the repertoire can be said to have changed.

These are the nuts and bolts of my explanatory approach, but it is also critical to consider how the broader social and political environment impinges on these processes. Here, my thoughts have been particularly shaped by my engagement with the events of the Peruvian election, which is a specific type of case in a number of respects (an instance of rapid repertoire change, rippling out from a centralized rather than diffuse point of origin, in the unsettled times of a poorly institutionalized context), and so must be taken as provisional. What I am most confident in suggesting is simply *that* any study of repertoire change undertaken from a pragmatist perspective must come up with systematic ways to take broader social and political realities seriously, or risk placing too much explanatory weight on the backs of a few individuals.

I suggest that social and political conditions matter for political innovation not because of what they compel (very little), but for what they make possible. Social realities can enable certain practical options and foreclose others. For the question of innovation, changes in these conditions—especially rapid ones, even if they remain small in absolute terms—can be particularly consequential, as political actors seeking responses to problem situations may recognize and latch onto them. Such changes do not force innovation, but they afford opportunities to those seeking them. Similarly, political realities matter for how they empower, disempower, and position actors vis-à-vis one another in the field of political contention, as well as for how they establish the potential applicability of various political tools to a situation. While not automatically producing an innovative response, let alone determining the nature of that response, political realities can set the stage for situated political innovation. In these ways—and likely many others—the social and political contexts of action impinge, even if indirectly, on the innovative moment.

Finally, if there is any concrete, explanatory lynchpin to all of this, it is the importance that I have discovered, through engagement with the Peruvian case, of the status of being a *contingently empowered outsider*. Those at the margins of the political field are more likely to perceive the shortcomings of routine practice, to feel themselves compelled to take strategic risks, and thus to seek out and identify opportunities in the social and political context that more central actors have not yet recognized or accepted as necessary. Such

outsiders are also more likely to have experience with nontraditional political practices, through exposure to and engagement with politics outside the boundaries of what more central political actors routinely encounter in the course of conducting their more traditional political business. And they are more likely to enjoy a measure of autonomy in their strategic choices, as they are less beholden to powerful interests, affiliates, and allies. Rarely are such outsiders in a position to act—or at least to act successfully. But at moments when contingencies in the dynamics of the political field elevate marginal political actors to positions from which they might feasibly make a go of it, they are particularly well situated to act in innovative ways. If they are successful, such contingently empowered outsiders can be the engines of rapid repertoire change.

Implications of the Approach

This approach to repertoire change, derived largely from pragmatist theories of action and foregrounding processes of situated political innovation, differs from the prevailing macro-historical approach, pioneered by Charles Tilly and advanced by others, in at least three respects. The first of these has to do with the pace of repertoire change. In his work on the development of modern protest repertoires in France and Great Britain, Tilly (1986, 1995, 2006, 2008) was engaged in explaining repertoire change that transpired gradually, over the course of decades. But as the Peruvian case illustrates, while repertoires may often change gradually, they can also be transformed through dramatic moments of radical innovation. Sidney Tarrow (1995) recognized this fact nearly two decades ago and suggested the need for more careful qualitative studies of such moments, but his call fell largely on deaf ears. I have attempted to answer Tarrow's call not only by focusing on a case of rapid change, but also by offering an approach that might be useful in explaining similar cases. Second, studies of gradual repertoire change have also typically, because of the nature of their cases, been focused on change that unfolds in a relatively diffuse way—that is produced by multiple actors responding independently to similarly structured situations. But repertoire change can also ripple out from a more socially centralized point of origin. In Peru, it was instigated by relatively small groups of political leaders responding to one another on the national stage. While similar processes of creative recognition and adaptation are no doubt involved either way, variation in the focal breadth of the overall change process arguably impinges on these in critical respects. Third, a focus on gradual and diffuse repertoire change predisposes the researcher to search for slow-moving and macro-level explanations, which inevitably

shifts the lens away from localized and eventful interactions and processes toward broader structural factors. The approach advanced here suggests that it is necessary to attend to the situational processes by which structural conditions translate into action, as such conditions are not in themselves sufficient for producing new political practices. Rather, new practices are the result of constrained yet innovative action undertaken at moments when the reproduction of old practices no longer suffices for the situation at hand.

More generally, by showing that the rise of populist mobilization in Peru was neither the automatic result of changing social or political conditions, nor a natural outgrowth of the ideological orientations, institutional affiliations, or social origins of those who first practiced it, this study challenges approaches in political sociology whose implicit or explicit theories of action have assumed otherwise. I have made the case that political practice is neither wholly determined by structural conditions nor idiosyncratically contingent. While at least relatively autonomous from structural conditions, it remains patterned in distinct ways and so requires explanation on its own terms. Through zeroing in on how political actors work through their strategic options in relationally and processually unfolding situations, it becomes clear that social and cultural experiences condition their understandings of these situations, shape their habits and routines, color their senses of the strategic options, and provide materials for creative action that can have truly significant consequences. In this way, this book can be understood as specifying one mechanism by which culture channels political processes and outcomes—thus playing a critical role in the balancing act between political stability and change (see Sewell 2005, 318–72). At the same time, it suggests the importance of attending to how socially patterned but still autonomous social action in eventful situations can play a critical role in producing macro-historical outcomes (*ibid.*, 1996).

This argument resonates with recent work in comparative-historical sociology and the study of contentious politics that recommends increased attention be paid to event sequences (Abbott 1983), path dependence (Mahoney 2000), turning points and critical junctures (Abbott 1997, Collier and Collier 1991, Mahoney 2001), and the temporality of historical processes generally (Pierson 2003). In particular, it joins with others who have emphasized the need to understand the internal dynamics of critical situations or events when endeavoring to explain (at least some types of) macro-historical stability and change (Abbott 2001; Ermakoff 2008 and 2015; Gould 1995; Kurzman 2004; Sewell 2005; Wagner-Pacifi 2010). Such an emphasis should not be interpreted as a blanket call to shift the focus entirely from macro-historical to micro-historical sociology, but rather as a reminder that each must inform the

other through the careful and systematic integration (indeed, even analytical dissolution) of these levels of analysis. This enterprise requires, among other things, a careful consideration of our views about social action.

My own response to this imperative has been to develop a pragmatist approach to situated political innovation that I believe provides a critical set of tools for understanding the internal dynamics of eventful moments of political change. One of my broader goals in so doing has been to demonstrate the utility of pragmatist theories of action for historical research on politics. As a few have recently argued, such theories hold great promise for comparative-historical sociology (Biernacki 2005; Gross 2010; Schneiderhan 2011), but this potential has yet to be fully realized. Most likely, this is because pragmatism is often assumed to deny the power, even the relevance, of those structural conditions that the subfield has long regarded as critical to historical explanation. But this book stands as evidence that embracing a pragmatist perspective does not have to mean giving short shrift to the broader social and political realities that shape specific contexts of action. In transposing pragmatist theories for use in explaining macro-historical outcomes, however, some creative adaptation is required. We must think seriously about how to understand the relationship between broader social and political conditions and more localized contexts of action, about how to move from the level of the individual to that of organized collective actors, and about how access to resources and the characteristics of the institutional environment impinge on interactions between collective actors differently than they do on interactions between individuals. I have been wrestling with these questions over the course of writing this book, and my answers remain provisional at best; yet I remain optimistic that pragmatism has much to offer to scholars of historical change and contentious politics. In particular, given its emphases on habit and human creativity, it seems to me uniquely poised to enhance our explanations of political stability and change.

Whither Populism Studies?

I have thus far avoided explicit discussion of “populism” per se. This choice may have disappointed some who would have liked me to have engaged more explicitly with the perennially vexing “problem of populism.” But there are only so many directions a book can take without distracting from its main argument. Still, the apparent resurgence of populism (however defined) on the world stage compels me to close this book with a few more general comments. In Latin America, it has been patently clear for more than two decades now that populism is not a relic of an earlier developmental stage, but rather

an important and persistent feature of the political landscape (see de la Torre and Arnson 2013). With some Latin American neo-populists advancing neo-liberal agendas (Weyland 1996), and others being associated with the region's political “left turn” (Cameron and Hershberg 2010), populism shows no signs of fading away in the region. At the same time, the term “populist” has been increasingly used to describe right wing politicians in Western Europe—such as France's Jean-Marie Le Pen and Italy's Silvio Berlusconi—as well as a range of xenophobic political movements in Eastern Europe and the former Soviet republics (Berezin 2004; Betz 1994; Held 1996; Jagers and Walgrave 2007; Rydgren 2006; Taggart 2004; Učeň 2007; Weyland 1999). It is even coming back into use in descriptions of U.S. politicians, from various “Tea Party” candidates to Bernie Sanders to Donald Trump. While recent political developments in Latin America and elsewhere should not be assumed to be similar simply because the same term has been applied to them, the times clearly call for a reinvigoration of comparative populism studies.

I have made the case for the advantages of approaching populism from the perspective of practice, under the rubric of populist mobilization, and think that doing so has a few advantages. It sidesteps persistent debates about whether populism is fascist or socialist, reactionary or progressive, authoritarian or democratic, militarist or civilian, urban or rural, and so on, by recognizing that politicians of various stripes can employ populist mobilization in the pursuit of a wide range of social, political, and economic agendas. It also opens up a new set of research questions. This book has focused on explaining the rise of populist mobilization in a specific context. But once the practice has been assimilated into a given political repertoire, what explains why some politicians take it up at certain points in time while others do not—especially if the practice is not necessarily tied to any particular ideological stance or policy agenda? How does it get implemented in various concrete settings, how is it organized, and what are the consequences of different ways of practicing it? Finally, what are the social and political consequences (intended and unintended) of populist mobilization as a practice? Might it, for example, lead to outcomes like social polarization or institutional destabilization that are independent of—and possibly unrelated to—whether the practice is driven by leftist or rightist agendas?

But rather than making the case that scholars should stop talking about “populism” and start talking about “populist mobilization” (I have presented a limited version of this argument elsewhere, see Jansen 2011), I would plead humbly for more conceptual precision—especially in empirical case studies and comparative research—regardless of the definition on which one ultimately settles. Populism stands alongside nationalism and fascism as notoriously difficult

to conceptualize.¹ As Ernesto Laclau (1977, 143) explained long ago, “few [terms] have been defined with less precision. . . . We know intuitively to what we are referring when we call a movement or an ideology populist, but we have the greatest difficulty in translating the intuition into concepts.” This problem remains a thorn in the side of the literature. Populism is often identified, at the most basic level, as a regime or movement in which leaders claim some affinity with “the people” (Knight 1998, 226). Indeed, this is the journalistic sense of the word. But as this definition might apply to virtually any modern regime—ever since the idea that legitimacy ascends from “the people” superseded the notion that it descends by divine or natural right (Calhoun 1997, 70)—it is hardly a sufficient conceptual foundation. The term has been used to describe a wide array of historical phenomena, from Maoism to fascism to Peronism. It has been used to describe movements, regimes, leaders, ideologies, policies, and state structures. Most historical case studies use the concept as little more than a generic label. Worse still, in such studies, most cases end up being treated as exceptional—a rhetorical move that, in stressing the uniqueness of individual populist experiences, creates the false impression of their incomparability. This inhibits both systematic attempts at comparison (of which we are in dire need) and the development of cumulative theoretical knowledge more generally. If we cannot agree on a definition, let us at least be clear about what we are explaining before we explain it.

Perhaps such clarity would help bring political sociologists back around to studying the phenomenon. While some of the most prominent early populism scholars were sociologists, few have engaged the topic in recent years.² Most broad studies of political forms fail to incorporate populist cases.³ The most striking example of this omission may be McAdam, Tarrow, and Tilly’s much heralded *Dynamics of Contention* (2001): although wide-ranging in its treatment of contentious politics, this work does not include a single populist

1. On the difficulties of conceptualizing populism, see de la Torre 2000; Ionescu and Gellner 1969b, 1–3; and Stein 1980, 9. Of nationalism, Minogue (1969, 199) writes that, “in the course of two centuries, a great variety of radically different movements have come to shelter under the broad conceptual umbrella;” Brubaker (2004, 132) similarly describes how nationalism “has been marked by deep ambivalence and intractable ambiguity.” Likewise, Mann (2004, x, 4–5) notes that fascism has often been used in a loose sense and that conflicting idealist and materialist accounts have failed to produce an adequate theory.

2. The only exception seems to be the U.S. case, which has received a modest share of attention from political sociologists (see, for example, Bonikowski and Gidron 2016; Braunstein 2015; Gerteis 2003 and 2007; Redding 1992; Schwartz 1976; and Soule 1992). Mabel Berezin (2009) and Carlos de la Torre (2000) are among the few sociologists to have recently engaged with international cases.

3. Lipset’s *Political Man* (1960) is a rare exception.

case among its fifteen core examples. The most significant impediment for political sociology has probably been a general suspicion of the slipperiness of the concept, and so an uncertainty about how populist cases should be incorporated into broader comparative frameworks. Such caution, as just noted, has been warranted. But it is also lamentable, in that it has meant that political sociology has systematically neglected a phenomenon that is now reasserting itself in a significant way on a global scale. Sociologists engaged in the study of contentious politics should take notice.