Introduction

The Challenge of Democratic Consolidation

The tide of countries making the transition from authoritarian rule in the late 1980s and early 1990s was initially greeted with euphoria. It was taken as a hopeful sign that the post–cold war era would be characterized by a community of nations—developed and developing alike—with a commitment to democratic institutions and democratic values (Fukuyama 1991). Nonetheless, as countries move from the installation of democracy to its consolidation, optimism has faded. While it is true that many countries have succeeded in moving away from their authoritarian pasts, few seem poised today to assume the mantle of a fully democratic future.

Any number of factors have worked to thwart the consolidation of democracy in third world polities. Many countries are riven by economic crisis, prompting political polarization, declining participation, and an escalation of crime and civil violence (Londregan and Poole 1990; Edwards and Tabellini 1991). In other countries, ethnic rivalries have come to the fore, calling preexisting boundaries of political community into question (Horowitz 1985; Young 1993). Finally, many states lack even the basic institutions of a modern democratic polity and are locked into a historical pattern of patron-client relationships and personalistic rule (Sandbrook 1985; Van de Walle 1994).

But the constraint that this study will focus on is of a different order. It stems from the fact that in many of these new democracies, newly empowered leaders are operating within institutional forms and structures that they did not create. Rather, they find themselves trapped within an institutional environment that is designed and controlled by their authoritarian predecessors.

This book offers an explanation for the existence of these authoritarian institutional legacies. It develops a systematic logic of “institutional insulation” that links the incentives and constraints facing power holders in the transition to the nature of the institutions that subsequently emerge. Specifically, I trace these vestigial institutions to strategic behavior by previous authoritarian elites seeking to guard against the inevitability of democracy. By entrenching their preferences in institutional footholds, the authoritarians ensure that the kinds of policies they favor will continue to be pursued long after they have left office. A defining feature of these enclaves of de facto authority is thus their potential to compromise the power and policies of successor governments.
Exiting authoritarian elites may employ any number of institutional strategies in order to protect their interests. For example, military regimes frequently set up mechanisms that allow for military representation in top policy-making bodies and/or shield military officials from dismissal or oversight (Stepan 1988; Agüero 1992). Electoral rules may also be designed to disproportionately advantage certain partisan interests over others (Geddes 1995; Londregan 2000). Or the authoritarians might try to cement in place certain social privileges before leaving power, such as institutionalizing the gains from land reform.

Another way that authoritarians can seek to protect their interests is to increase the autonomy of institutions that would otherwise be subject to the vicissitudes of the democratic political process. In the realm of the economy, one institution that lends itself to such insulation is the central bank. As the agency empowered to administer the country’s currency, the central bank plays an important role in shaping the overall direction of the economy. By making the central bank autonomous, control over monetary policy is effectively removed from the hands of politicians.

At first glance, central bank autonomy might seem to offer a somewhat unconventional lens through which to explore this line of reasoning. After all, an autonomous central bank is generally seen as an unmitigated “good.” On the theoretical front, it is thought to enable governments to credibly commit to macroeconomic stability. On the empirical end, there is a demonstrated relationship between central bank autonomy and low inflation. This study departs from the received wisdom and offers an alternative, less uniformly benign interpretation of central bank reform. Its starting point is the observation that all political actors should not value central bank autonomy equally. Rather, central bank autonomy is likely to be a source of conflict between those groups that favor low inflation and those who wish to use the economy for political ends. This assertion thus sets the stage for a redistributive perspective on central bank reform, in which central bank autonomy serves as a strategic tool through which conservative governments seek to limit the policy choices of their successors. I argue that where authoritarian elites fear the populism that may be endemic to new democracies and know that a change of regime is imminent, they can be expected to create an autonomous central bank to lock in a commitment to price stability over the long haul.

To lend support to this argument, the study draws upon evidence from the so-called third wave of democratization sweeping the globe over the past twenty-five years. It focuses primarily on two Latin American countries—Chile and Mexico—both of which undertook central bank reforms amid a transition from authoritarian rule. The study concludes by suggesting how
this framework might be extended to institutional contexts beyond Latin
America and to insulation strategies other than central banks.

At the broadest level, then, this is a book about efficiency versus redistri-
bution in institutions. It is about how people in power protect their interests
against subversion in the future. At a more pragmatic level, it is also an
attempt to grapple with a recurring feature of the institutional landscape of
the contemporary developing world. As I will be at great pains to show, the
institutional constraints that new democracies face as they emerge from
authoritarian rule are neither accidental nor surprising. Rather, they are the
inevitable by-product of the very logic of the transition itself.

Theorizing Insulation in Transitional Democracies

For students of transitional polities, the observation that new democracies are
frequently constrained by the institutional choices of former leaders is hardly
novel. Contributors to the “transitions” literature have long recognized the
continuing influence that previous authoritarian rulers can exert over certain
areas of substantive policy-making. Valenzuela, for example, highlights a
number of “reserved domains” that governmental officials “would like to
control in order to assert governmental authority . . . but are prevented from
controlling by veiled or explicit menaces of a return to authoritarian rule”
(Valenzuela 1992, 65). Garretón similarly devotes an entire chapter to the
myriad “political proscriptions and exclusions that limit the democratic
game,” coining the term authoritarian enclave to underscore their birthplace
in the previous regime (Garretón 1989, 52).

Scholars of democratization have also speculated about the general
processes producing these authoritarian institutional legacies. We know, for
example, that during the negotiations that may precede the transition, the
interests of the outgoing authoritarians are of paramount importance. More
specifically, the “king and queen” of the transition game (propertied classes
and armed forces) cannot be placed in direct jeopardy (O’Donnell and

1. Note that authors differ in their conceptualizations of this phenomenon. Valenzuela
confines his definition of reserved domains to the loss of governmental authority over
specific policy domains, while treating informal oversight functions accorded previous rulers
(tutelary powers) and biased electoral rules as separate instances of what he calls “perverse
institutionalization.” In contrast, Garretón has a much broader definition of authoritarian
enclave, one that encompasses not only institutional mechanisms that favor authoritarian
interests but symbolic legacies (e.g., human rights violations) and privileged socioeconomic
and political classes as well. My own definition includes any formal rule or procedure set up
by the former regime to favor previous rulers and their allies. It thus encompasses all three of
Valenzuela’s spheres but corresponds to only the first of Garretón’s.
Schmitter 1986, 69). Absent certain guarantees that their interests will be protected, these elites may fail to support a democratic alternative (Przeworski 1991).

The pace and duration of the transition are also thought to bear on the authoritarians’ ability to affect the substantive and procedural landscape of the new regime. Where political change is so sudden and sweeping that incumbent elites are caught off guard, they are less likely to play a role in shaping the parameters of what is to follow. But where the transition is more gradual and protracted, elites have more time to set the terms of their withdrawal. The implication is that exiting elites are more likely to leave a strong imprint on the transition in the latter set of “top down” elite-dominated transitions than those “bottom up” transitions led by the masses (Karl 1990; Karl and Schmitter 1994).

Despite these useful insights, however, the transitions literature has yet to offer an overarching explanation for the origins of these institutional holdovers and the conditions under which they are likely to arise. Absent any explicit analytical link between the nature of the authoritarians’ interests, the pace of the transition process, and the subsequent appearance of these biased institutional forms, we are at best left with a series of correlations. When all is said and done, how and why these institutional enclaves come about remains a mystery.

In part, this theoretical lacuna is attributable to the fact that authors writing in this tradition have generally been more concerned with exposing the perverse implications of these institutional structures than with exploring their foundations (Karl 1986; Linz 1990; Sørenson 1993). Because of this overriding preoccupation with the “effects” question, the discussion of such insulation tactics has tended to be largely descriptive. This remains true, despite recent contributions where authors have speculated more precisely as to the conditions under which such insulation is likely to occur. In keeping with their broader arguments about regime change, for example, Haggard and Kaufman (1995, 109–39) maintain that the degree of economic crisis is likely to be an important factor in determining how much control exiting military elites are likely to retain under democratic rule. But given that such economic crises are in many cases caused by the very regimes whose behavior these authors seek to analyze, their argument is vulnerable to an endogeneity critique, similarly compromising any speculation about attendant insulation strategies. For their part, Linz and Stepan (1996, 66–71) suggest that we are most likely to see this sort of insulation behavior carried out by hierarchical militaries, followed by nonhierarchical militaries, civilian leadership, on down to sultanistic regimes. While this typology may be accurate, it fails to
offer much in the way of a generalized rationale for the presence or absence of these authoritarian enclaves.

In addition to this descriptive bias, a more comprehensive explanation for these authoritarian enclaves has also been limited by a fundamental assumption underlying much of the transitions literature. According to this body of work, the transition is a time of great uncertainty with a large quotient of unpredictability and accident (O’Donnell and Schmitter 1986, 4). Actors may not know their preferences, and even if they do, their actions may have unforeseen consequences that prevent them from realizing their goals. The bottom line is that because of the uncertainty that pervades transitions, we as analysts cannot ascribe purposeful intent to the actions of actors in any systematic fashion.

This study questions the uncertainty assumption. Or at least it questions that assumption where an important set of actors is concerned: authoritarian elites who know that they are losing power to democratic forces. For these elites, the world is not so uncertain. Indeed, precisely because exiting elites can foresee the impact that democracy is likely to have on their interests, they act strategically to make sure that these are not placed at risk. In the chapters that follow, I use this assertion about actors’ behavior to generate predictions about how exiting authoritarian elites use institutions to fend off the threat of democracy. I suggest why authoritarian elites choose to deploy institutions defensively; under what conditions they are likely to succeed; and why, once created, “their” institutions are likely to endure.

Insulation Strategies: Central Bank Autonomy

The starting point for this argument is the literature on bureaucratic insulation in the advanced industrial world. Contemporary theories of bureaucracy offer powerful insights into why an anticipated turnover in power may prompt politicians to use institutions defensively. In brief, the claim is that turnover matters because it signals that politicians cannot enjoy the perquisites of office forever. It also means that their opponents will someday be in power and are likely to pursue policies that work to their disadvantage. Recognizing that their days in office are numbered, incumbent politicians thus have incentives to create institutions that protect their interests from their opponents, who may, as a result, be left worse off (McCubbins, Noll, and Weingast 1987; Horn and Shepsle 1989; Moe 1990a).

It is easy to see how the thrust of this literature might accurately capture the distorting effects of the more obvious perverse institutional forms that
tend to accompany transitions, such as biased electoral laws or veto powers for the military. It is admittedly more difficult to see its ready application to central bank reform.

After all, the conventional wisdom now holds that insulated bureaucracies are an essential component of successful economic reform in developing countries (Bates and Kreuger 1993, 464–65; Nelson 1993, 436; Geddes 1994a). By providing policymakers with protection from their political constituencies, such insulation is thought to facilitate the government’s ability to use bureaucratic capabilities effectively without “threatening the economic logic of the adjustment process” (Callaghy 1989, 120). This is considered particularly important in new democracies where the reemergence of social demands heightens distributive pressures (Haggard and Webb 1994, 13–15). Central bank autonomy in many ways epitomizes the virtues attributed to such bureaucratic insulation.

And yet, it is precisely because this technocratic interpretation of central bank reform is so widely accepted that autonomy is rendered such an interesting candidate through which to explore the insulation dynamic at hand. For as I argue subsequently, there may be another story to tell in which the Pareto-improving nature of central bank reform is not a given. Rather, politicians have incentives to feel differently about the relative merits of central bank autonomy. The autonomy question is thus transformed from a simple efficiency story into a redistributive struggle over macroeconomic policy.2

The Argument

Rethinking the Credibility Literature: The Logic of Tying Successors’ Hands

Most research on central bank autonomy falls into the category of the “credibility” literature.3 In a nutshell, this literature argues that governments create autonomous central banks in order to “tie their own hands.” Models build from the premise that all politicians have the ability to use surprise inflation to generate short-term gains in output (Kydland and Prescott 1977; Barro and Gordon 1983a, 1983b). As a result, even when governments may pledge a commitment to macroeconomic stability, domestic economic agents know

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2. For a partisan take on insulation that is more in keeping with the logic of credible commitment, see Bates 1994.

3. For a thorough review of this literature, see Blackburn and Cristensen 1988; Persson and Tabellini 1994.
that politicians will always be tempted to inflate to improve their electoral fortunes.

In order to solve this commitment problem, politicians take monetary policy out of their own hands and place it under the charge of an autonomous central bank. Precisely because the central bank does not have to respond to voters’ interests, it is thought to be more likely to pursue policies conducive to macroeconomic stability (Wooley 1984; Cukierman 1992). The bottom line, then, according to the credibility literature, is that central bank autonomy enables governments to credibly commit to low inflation. And while autonomy is generally thought to come at the cost of greater output variability, on net, society is left better off by delegating monetary policy to a more conservative central banker (Rogoff 1985).

In addition to generating more than a decade’s worth of research in the field of economics, the credibility hypothesis has become the received wisdom in political science. Its influence has been particularly strong on a small but growing body of literature that attempts to explain the recent trend toward central bank reform in developing countries. Maxfield (1997), for example, argues that developing country governments make their central banks autonomous in order to signal a commitment to low inflation before international creditors and investors. She thus modifies the conventional wisdom to underscore the important role that international economic actors play as an audience for developing country reforms. The basic intuition, however, is exactly the same: central bank autonomy is executed as a means of demonstrating a credible commitment to low inflation.

Despite the widespread currency of credibility-based reasoning within the fields of economics and political science, this study suggests three reasons that it needs to be nuanced with a more distinctly political orientation. Consider first the empirical evidence from the less developed countries (LDCs). There is no doubt that international economic pressures inevitably play a role in pushing developing country governments toward central bank reform. But if these pressures were the sole determinant of central bank autonomy, then virtually all developing countries would have autonomous central banks. And yet, as we will see in chapter 2, this is simply not the case. Variation is readily apparent whether one chooses to employ formal/legal or more behavioral measures of central bank independence. Even in the face of overriding economic incentives to delegate authority over monetary policy, then, some governments must find it advantageous to maintain political control over the central bank.

The observed empirical variation on central bank autonomy ties to a second weakness in the credibility literature: its underlying premise that all politicians have the same incentives to inflate. Partisan theories of political
economy have long argued precisely the opposite. They suggest that parties of the left and right, for example, differ systematically over the mix of inflation and unemployment they prefer (Hibbs 1977). Early variants of these theories were discredited for their assumption of a permanently exploitable Phillips curve (the trade-off between low inflation and low unemployment). More recent work has demonstrated, however, that in the wake of uncertainty surrounding elections, politicians can capitalize upon a short-run Phillips curve in order to manipulate the economy temporarily to their advantage, whether toward low inflation (in the case of the right) or toward low unemployment (in the case of the left) (Alesina and Rosenthal 1995).

While some models of central bank reform do incorporate a short-run Phillips curve (Alesina 1988; Alesina and Gatti 1995), they typically assume that the inflationary variance generated by these short-term partisan cycles renders central bank autonomy equally attractive to all political actors. And yet, there is no reason to presume this necessarily to be the case. To the extent that left-leaning politicians can engineer these short term postelectoral booms—and voters are willing to reward them for this behavior—it is not at all clear why such politicians would want to relinquish this flexibility in favor of the long-run credibility benefits of an autonomous central bank. And to the extent that these parties also place greater weight on real outcomes than they do on inflation, they are also more vulnerable than parties on the right to the output stabilization costs assumed in most standard models of central bank independence. In short, there are sound theoretical reasons to suspect that politicians who attach different values to inflation versus other macroeconomic outcomes are likely to disagree over the desirability of central bank reform.

A third and final reason to question the credibility literature is its assumption that central bank reform is necessarily “cost free.” While a growing body of research demonstrates an empirical association between the presence of a central bank and low inflation (Grilli, Masciandaro, and Tabellini 1991; Alesina and Summers 1993), the jury is still out as to how central bank autonomy affects other long-term macroeconomic outcomes such as growth, deficits, and unemployment (Pollard 1994; Eijffinger and DeHaan 1996). And additional empirical work suggests that central bank autonomy may, in fact, increase the short-term trade-off between output and inflation (Debelle and Fischer 1994; Walsh 1995a). If the effects of central bank autonomy on these other macroeconomic variables are unknown or, indeed, adverse, this would seem to be yet another reason for left-leaning politicians to be wary of central bank autonomy.

The basic thrust of chapter 2, then, is that what is lacking in the credibility literature is politics. When the idea of an autonomous central bank is sub-
jected to closer scrutiny, it becomes increasingly clear that all political actors should not value this institutional change equally. Rather, those who favor low inflation should welcome autonomy with open arms, while those who prefer more political control over the economy have incentives to oppose it. In light of these claims, we can at least cast doubt on the credibility literature’s claim that where divergent ideological preferences exist, central bank independence represents a mutually beneficial compromise between opposing forces (Alesina and Gatti 1995). Rather, building on the work of Goodman (1992) and Cukierman (1994), it seems at least plausible to suggest that central bank autonomy may also serve as a means through which conservative governments—fearing the prospect of a leftward rotation in power—impose this institutional structure on their unwilling successors.

If the literature on partisan political economy lends plausibility to this more conflictual view of central banks in the advanced industrial world, a similar argument should also hold true for the developing world. To be sure, the redistributive “game” surrounding autonomy looks somewhat different. On the one hand, because the number one cause of inflation in developing countries is not employment but deficit spending (Cukierman 1992, 47–82), the political struggle is much more likely to center around those who stand to win or lose from the central bank’s inability to monetize deficits. On the other hand, the financial liberalization that tends to accompany autonomy will also find supporters and detractors, as it eliminates the traditional role of the central bank in the LDCs as a vehicle of preferential credit (Haggard and Lee 1993). Once we account for these adjustments, however, there is every reason to believe that the “tying successor hands” argument advanced in this chapter can serve as a plausible explanation for central bank reform in the developing world.

Central Bank Autonomy in the Transition from Authoritarian Rule

In chapter 3, I argue that the transitional political environment constitutes particularly fertile ground for extending this sort of redistributive logic. After all, the key issue on the table is really turnover of power and how such turnover affects the expectations of incumbent politicians as to how their preferred policies are likely to fare in the future. One would be hard pressed to find a political context where turnover assumes greater relevance than in the case of regime change, for it is precisely at the moment of the transition that authoritarian elites realize that they will not rule indefinitely. And while they do not know exactly what is coming their way, they do know that the situation does not look good for them. On the one hand, as previously margin-
alized groups gain a voice in the political arena, the potential for a radical change in policy must increase. On the other hand, unlike their counterparts in first world democracies who can expect one day to return to power, this may well be their last chance to influence the future course of events. Fearful of these risks inherent in regime change, the authoritarians should logically seek protection. By safeguarding their interests in well-insulated institutions, they can maintain influence over those arenas of policy they care about most.

Naturally, not all transitions will be equally threatening or—by extension—warrant the same degree of insulation. In addition to spelling out a general logic of transitional insulation, then, chapter 3 suggests the conditions under which it is likely to occur. How much insulation the authoritarians will be willing to undertake, I argue, depends on two factors. First, this is likely to be affected by the intensity of threat, as determined by the extent to which the preferences of exiting authoritarians and incoming democrats are expected to diverge. Second, the proximity of threat, or the relative imminence of the transition to democracy, should also condition insulating behavior. The incentives to insulate are expected to be stronger the more the preferences of the authoritarians differ from those of their likely democratic successors and the closer the onset of democratization.

With this general argument about transitional insulation as a backdrop, I then apply its logic to central bank reform. The central intuition driving the model presented in chapter 3 is that democracy offers different distributional incentives than authoritarian rule. If nothing else, the onset of elections means that politicians need to be more sensitive to voters’ needs (Garrett and Lange 1996, 61–63). In particular, there are incentives for newly elected politicians to choose policies that benefit social groups who recently obtained a voice in the political arena (Alesina 1994, 45–47). Politicians in the developing world are also more susceptible to the “time inconsistency” problem—both because they are more vulnerable to being overthrown (Ames 1987, 25–27) and because voters typically have shorter time horizons (Haggard and Kaufman 1995, 157–58). Even where the democrats are not rabid populists, then, we can still safely say that politicians in emergent democracies are likely to want greater flexibility in the short term to appeal to different constituencies via increased social spending (Haggard and Kaufman 1989; Hunter and Brown 1998).

This more interventionist economic scenario does not always constitute a threat to outgoing authoritarian elites. That depends on the economic policy preferences of the authoritarians, which are determined here by the interests of dominant economic actors (capital). My assumption is that while democratically elected politicians are forced to balance the competing demands of business and voters when designing economic policy, authoritar-
ian governments are free to respond entirely to the concerns of business. Drawing on Frieden (1991a), the model then posits two types of authoritarian regimes. For those more protectionist authoritarian regimes that cater to the interests of “sheltered” and asset-specific capital, those in power have no reason to fear the future in a strictly economic sense. After all, their constituents (import competing industries and nontradables) stand to benefit from the sorts of increased spending and preferential credit policies likely to be associated with democracy. These authoritarian governments thus have no incentive to limit the central bank’s ability to monetize government deficits or to act as a development bank. To the contrary, they represent those sectors that have historically benefited from the lowered cost of capital entailed in the more lax fiscal and credit policies associated with dependent central banks in the developing world (Haggard and Maxfield 1993).

But there is also a set of important cases where the preferred policies of the authoritarians will not mirror those of the democrats. These are of course those authoritarian regimes that cater to a different type of capital—predominant in the “exposed” and non-asset-specific sectors of the economy (financial capital, foreign capital, and tradables). For these sectors of the economy that value macroeconomic stability, economic openness, and financial liberalization, the specter of intervention inherent in democratization does constitute a “strong threat.” Accordingly, the creation of an autonomous central bank should be a very attractive strategy. Because an autonomous central bank views credit policy through the lens of macroeconomic management, it is likely to resist policies that result in excess spending, loose monetary policy, and high inflation. By establishing a monetary regime where populist economic alternatives are virtually off the agenda, conservative authoritarian regimes can also set up powerful constraints over the types of policies that democratic governments can pursue in the future.

The model yields three basic outcomes that can be thought of as points along a continuum. Where there is no clash between the economic policy preferences of the outgoing regime and the incoming democrats (i.e., the intensity of threat is weak) and/or democracy is a good way off, there should be no change to the legal status of the central bank. This set of outcomes corresponds to—among other things—the transition from a protectionist authoritarian regime, and it accounts for those null cases where a transition occurs but no autonomous central bank is created. In contrast, where there is a sharp preference divergence and regime change is imminent, the authoritarians will have strong incentives to insulate, and autonomy should be full. This set of cases encompasses those market-oriented regimes where democracy does represent a potential threat to the economic interests of the outgoing elites. In between, we should expect a set of middle-range outcomes to
occur when the policy divergence between the exiting authoritarians and incoming democrats is lower and/or the democratic threat is more distant. In this last set of cases, autonomy should only be partial.

In all cases, autonomy is measured using the four sets of formal indicators developed by Cukierman (1992). These include the relative stringency of the terms affecting central bank lending to the executive, the extent to which the central bank is given control over the formulation of monetary policy, the degree to which the objectives of the central bank are centered on the goal of price stability, and the scale of executive influence over the composition and tenure length of the central bank’s governing board.

Explaining Institutional Persistence

In sum, this book suggests why credibility explanations may be inadequate for understanding the process through which autonomous central banks are created and why an alternative more political logic may also be at play. But while I privilege this more political explanation, I do not claim that credibility explanations never matter or do not capture part of the story. In fact, this study brings in credibility at the end of the story to explain why, once these autonomous central banks are established, we can expect them to remain.

After all, central bank autonomy only serves as an effective insulation strategy insofar as it is difficult to undo. If the authoritarians had no reason to believe that their institutional creations would last, they would be loath to undertake the effort to build them in the first place. How, then, can they be so sure that these central bank reforms will not be overturned?

The explanation I offer is rooted in the concept of reputation. Reputation is the view formed of an individual or organization by another based on past experience and that is subsequently used as a basis for forecasting future behavior (Kreps 1991). Once established, an autonomous central bank acquires tremendous reputational value in the eyes of international creditors and investors as a symbol of commitment to macroeconomic stability. Regardless of their policy preferences, new governments should thus be reluctant to tamper with autonomy. To do so would be to risk the massive outflow of foreign capital, with disastrous consequences for the economy. While incoming governments may not like living with an institution that denies them short-term flexibility over the economy, the instantaneous costs associated with abolishing an autonomous central bank autonomy are sufficiently dramatic—and politically salient—to make this a moot point (Calvo and Mendoza 1996).4 While my argument thus shares with Maxfield

4. While I posit the international constraint as the overriding source of institutional persistence, it is buttressed by a host of domestic factors described in chapter 3.
an emphasis on international capital as a powerful force in shaping central bank reforms in the developing world, we differ critically in how we assess its significance and implications.

The thrust of my persistence argument, then, is that politicians in new democracies who inherit autonomous central banks are likely to view these as a liability. This does not mean that these politicians are impervious to the long-run destabilizing effects of inflation. To the extent that central bank autonomy is associated with macroeconomic stability over time, it may come to serve as a valuable institutional anchor in countries otherwise prone to volatile inflationary cycles. Indeed, central bank autonomy may have a number of positive long-run externalities (for economic stability, political stability, etc.) that leave these new democracies better off. My purpose, however, is not to evaluate autonomy’s virtues from the standpoint of a given nation’s long-term social welfare. It is to suggest the potential costs it may have for the short-term interests of political leaders in office and the constituencies they represent. To the extent that it deprives these politicians of a crucial lever of economic control with which to respond to pent-up societal demands, they may view autonomy as compromising their short-term office-seeking objectives.

When all is said and done, the question really comes down to how we choose to understand institutions. Do we assume an efficiency-based view in which institutions reflect mutually beneficial bargained agreements (cf. Williamson 1985; Milgrom and Roberts 1992), or do we adopt a more redistributive perspective in which institutions embody a set of interests that one group of more powerful actors imposes on another with concomitant costs (Levi 1990; Moe 1990a; Knight 1992)? If one takes the latter tack, then I am agnostic about whether or not central bank autonomy is ultimately “good” for these new democracies. What I do claim is that it might be potentially bad for the democrats whose control over the economy is sharply decreased just when flexibility is needed most.

Research Design

As a vehicle for exploring some of the theoretical points made in this book, chapters 4 through 7 are devoted to a paired comparison of two transitional democracies where central bank reforms were undertaken—Chile (1989) and Mexico (1993). In order to undertake this comparison, I employ a comparable case methodology (Lipjhardt 1975). The idea is to focus on cases that are

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5. This method is also referred to elsewhere as “systematic comparative illustration” (Smelser 1968), “controlled comparison,” and “most similar systems design” (Przeworski and Teune 1982).
similar in a large number of important characteristics but dissimilar with respect to the variables with which a relationship is hypothesized. The over-riding virtue of such a strategy is that it offers a large measure of control, thereby circumventing the “many variables, small number of cases” problem that tends to plague comparisons among a handful of countries.

To this end, I privilege the two dimensions of the threat variable thought to influence central bank reform in the transition from authoritarian rule: the intensity of threat (as measured by the sectoral support base of the authoritarian regime and the democrats’ economic agenda) and the proximity of threat (as measured by the relative strength of the incoming opposition). And since a major rival hypothesis explains this outcome as a function of the need for international economic credibility, I also assess the relative plausibility of this international explanation for each of the countries in question. By focusing on key variables and subjecting each case to the same data requirements, this study meets the prerequisites of George (1982) for a structured, focused comparison intended to sharpen the theoretical rigor of small-\( n \) studies and to improve their potential for generalizability.

As noted earlier, my argument is meant to hold for all transitions except those where political change is so sudden and so sweeping that incumbent authoritarian elites lack both the time and the ability to enact this sort of self-serving institutional reform.\(^6\) This study traces the course of central bank reform within two “top down” transitions: Chile (1980–89) and Mexico (1988–93). While there is no logical imperative for focusing exclusively on two Latin American countries, Mexico and Chile lend themselves to the comparison at hand.

At the time of the reform in question, both countries could lay claim to a powerful capitalist class with a vested interest in central bank autonomy. Both were more or less equally vulnerable to the international economy. And they each had authoritarian regimes facing a major domestic political challenge and/or the advent of democratization. In spite of these relatively parallel economic and political contexts, Chile opted for a fully autonomous central bank, while Mexico created a partially autonomous institution. By controlling for both powerful capitalist actors and international economic context, I thus show how variation on the proximity of democratization in turn yielded variation on the corresponding degree of autonomy afforded the central bank. The implicit null hypothesis is that where central bank autonomy is absent, it

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\(^6\) In a sense, one could thus consider the pace of the transition to constitute what Smelser refers to as a “parameter” in this research design: for example, “a condition that is known or suspected to influence the dependent variable but which, in the investigation at hand, is made or assumed not to vary” (Smelser 1968, 70).
is due either to the extremely distant nature of the transition or to a lack of significant difference in the economic policy preferences of the authoritarians and the democrats. Such a case is considered briefly in chapter 8.

A comparative case methodology is particularly suited to a study of this nature, which relies heavily on such inherently qualitative phenomena as the “fear of impending democratization” and the “proximity of threat” that would be difficult to measure in the context of a large-n, quantitative study. In order to adequately capture such concepts I need to re-create the decision-making calculus of policymakers with respect to how they viewed the options before them at the time of the reforms in question. This requires close and detailed attention to the two cases at hand, which can only be achieved through extensive field research. Such field research entails detailed journalistic accounts; archival research at on-site university libraries and policy-research organizations; and extensive interviews with government officials, academics, and representatives of the media and private sector.

This intensive form of data collection is useful on two levels. On the one hand, it facilitates assessment of the validity of alternative explanations. On the other hand, by employing the within-case formula of George (1982) for process tracing, historical archives and interviews can be used in order to specify more explicitly the intervening steps between my hypothesized causal mechanisms and resultant institutional outcomes. Such process tracing is particularly useful where the Mexican case is concerned, as the incomplete nature of the Mexican transition obscures clear-cut inferences with respect to the reform in question.

By thus combining a controlled case comparison with a research design that allows for both positive and negative instances of the phenomenon under study, I am able to isolate the independent effect of my independent variable on the institutional outcome at hand, thereby replicating—where possible—a quasi-experimental research design. As with any methodology, there are some drawbacks. First, as is often the case in small-n studies, the observations used for analysis were deliberately selected. The explanatory reach of this sort of “retrospective research design” is inherently limited, as it is much more dependent on my preconceived notions about the empirical world than what might have emerged were my observations drawn from a large, random sample of cases (King, Keohane, and Verba 1994, 141–42). Moreover, because only high and medium values of autonomy are examined in detail, the conclusions are vulnerable to selection bias (correlation between selection rule and dependent variable) (King, Keohane, and Verba, 1994, 130).

While problematic, these methodological weaknesses do not, in my view, constitute a serious problem. Of necessity, qualitative studies almost never employ a random selection rule. While in an ideal world I would be able to
select cases exclusively on the basis of explanatory variables, the present research design moves me in the direction of such a goal while in the meantime allowing for a well-controlled comparison. The study also both allows for and is premised upon a dependent variable that actually varies, thereby distinguishing it from a great deal of allegedly comparative research in comparative politics. Finally, undertaking a subsequent case analysis where observations are selected only on the basis of the central independent variable can also mitigate any problems associated with selection bias. Chapter 8, “Central Bank Reform in Comparative Perspective,” begins to do precisely this.

A second methodological issue raised by this study relates to the microfoundational nature of the argument at hand and the fact that I assign a set of preferences to the actors in the model I develop. I believe this approach is justified on two grounds. First, I ascribe what I believe to be reasonable preferences to the actors in question: greater flexibility over the economy (in the case of the democrats) and the interests of dominant capitalist actors (in the case of the authoritarians). The idea is to employ a weak assumption about what motivates actors in order to generate a strong conclusion. Second, since actors frequently act strategically when relaying their preferences with regard to a specific event, it is often difficult to take them at their word. More objective—albeit indirect—indicators of their preferences can often provide a more accurate reflection of their state of mind. Finally, I also attempt—where possible—to provide independent evidence for my assessments of actors’ preferences in the case studies themselves.

A third issue concerns the determination of the appropriate “cutoff” points for the different variables in play. The key challenge here is how to code a “strong” versus a “weak” threat and, similarly, how to distinguish between “high” and “low” autonomy. While the latter can be assessed using widely accepted measures (i.e., the Cukierman index), the former is much more difficult to specify. Because it is not possible to establish explicit boundaries for these “threat” thresholds, they must be determined empirically. While this arguably generates a somewhat subjective evaluation of this variable, it is also the case that even if this were a quantitative study, these parameters would need to be established empirically.

A final concern arises from the partial nature of the Mexican case analyzed herein. For unlike the full or no insulation variants in which the relationship between the independent variable and the outcome is fairly straightforward, partial insulation is—by definition—an inherently murky concept to get one’s hands around. But rather than abandoning the case altogether—or forcing it into a box where it does not belong—I chose instead to work from the basic theory that I have developed in this book and use it to explore the interesting nuances of a particular case. To the extent that we can think of
Mexico as a point on a continuum, it offers a nice point of comparison with Chile within the same overall theoretical rubric. In keeping with the recent trend toward analytic narratives (Bates et al. 1998), I am thus able to use my argument to shed light on an important case in new and interesting ways, while at the same time allowing the case to enrich the underlying theoretical model.

This book unfolds as follows. Part 1 amplifies some of the main theoretical points highlighted in this introduction. Using the literature from American politics on bureaucratic insulation as a backdrop, chapter 2 counters traditional efficiency-based views of central bank reform that emphasize improved welfare with a more redistributive perspective that emphasizes autonomy’s potential costs. In chapter 3, I suggest how such an approach might be extended to a transitional political environment.

Part 2 bolsters these theoretical claims with empirical evidence. I begin with an in-depth examination of two transitional democracies in which central bank reform took place—Chile and Mexico—in order to highlight how variation in the proximity of the democratic threat yielded correspondingly different levels of central bank autonomy. The Chilean central bank reform of December 1989—examined in chapters 4 and 5—presents an almost textbook case of the sort of institutional insulation we should expect to see in the wake of a transition to democracy. In the presence of an imminent threat of regime change under the leadership of the center left, authoritarian elites with a vested interest in macroeconomic stability responded by creating a highly autonomous institution. And despite vociferous criticism from the regime’s opponents prior to the passage of this legislation, it has persisted under democratic rule without modification.

In contrast, the Mexican central bank reform of December 1993—considered in chapters 6 and 7—better exemplifies a case of what one might call “partial insulation.” As in Chile, reform impetus in Mexico grew from a baseline concern over how the incumbent government’s economic policy interests might be compromised by future left-leaning rulers. But because the democratic threat facing the authoritarian regime was still distant, the resultant autonomy afforded the central bank was less than complete. Chapter 7 ends with a postscript explaining the reform’s relevance to the dramatic events of 1994 and noting the argument’s implications for Mexico’s ongoing process of democratization.

Part 2 concludes with chapter 8, “Central Bank Reform in Comparative Perspective,” in which I examine three cases of central bank reform in transitional political settings similar to Chile and Mexico: two where reforms succeeded (South Africa and Pakistan) and one case of failed reform (South
Korea). Part 3 elaborates on possible empirical extensions of the argument and returns to the “big picture” questions of the relationship between democratization and institutional change.

Conclusion

It is important to emphasize that this study does not try to provide a comprehensive explanation for central bank reform in all developing countries. As noted previously, the argument is strictly delimited to that class of cases commonly referred to as transitions from authoritarian rule. While I believe that the argument can speak to a number of important cases—elaborated in chapter 8—I do not suggest that mine is the only—or even necessarily the most prevalent—means through which central bank reforms are likely to surface in the third world.

Nor does this study try to establish a lexical ordering of preferences as to which—of several potential contenders—is likely to be the most important insulation strategy for a given authoritarian government. An authoritarian regime in the twilight of its rule might reasonably attempt to insulate its preferences in the military, electoral, and economic spheres. Rather than attempting to catalog and prioritize the variety of options available, this book instead highlights one specific insulation strategy—central bank autonomy—and explains under what conditions the onset of democratization may lead to its appearance.

That said, this study has three much more fundamental objectives. First, I hope to use the specific institutional case of central bank reform in order to illuminate a much more widespread insulation phenomenon. This illustrative research design is thus meant to serve as the first step in the development of a theory to explain one of the most pervasive—and potentially consequential—institutional phenomena confronting third world democracies today. Down the road, it might serve as a theoretical basis for understanding the origins of a variety of authoritarian enclaves—not just central bank autonomy but civil-military relations, electoral laws, and the other biased institutional forms featured in the transitions literature, as I suggest in chapter 9.

Second, I also hope to make a contribution to the burgeoning literature on central bank autonomy by suggesting an alternative, more political logic to the conventional credibility-based wisdom. The “free lunch” mentality that has pervaded the credibility literature for nearly two decades has gone unchallenged far too long. As theoretical and empirical developments increasingly point to the short-run costs that may be attendant upon central
bank reform, it is time to rethink our assumptions to cultivate a more nuanced, redistributive understanding of this institutional outcome.

Finally, this work is intended to enrich the literature on institutions more broadly by extending a redistributive approach to the institutional landscape of the third world. For just as political scientists have been all too willing to embrace an economic rationale for central bank reform, so too have they tended to employ similar reasoning in their analysis of institutions. As I argue in chapter 9, however, it is not at all clear that such an approach is the only—or even the most appropriate—way to understand institutions, particularly in new democracies. After all, these are polities that tend to be characterized by large asymmetries of power, where certain actors—by virtue of the resources they possess coming into the transition—are in a position to impose certain institutional outcomes on other players in the “democratic game.” Knowing this, we need to discriminate more carefully in our application of the institutionalist literature coming out of the first world. This book can thus also be read as a normative plea for a more textured political analysis of all institutions—central banks and otherwise.