Another Path to Democracy: Investigating the Relationship Between Nonviolent Resistance and Democratization

by

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It’s 3:22 am on April 11, 2014. My eyes hurt, the words on the screen don’t make sense, it’s past my bedtime (sort of), and in only a little more than twelve hours, I will have submitted my senior thesis to the Government Department, hopefully. If something went wrong, I apologize in advance. I take full responsibility. But I couldn’t be happier and more grateful right now.

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CHAPTER ONE

INTRODUCTION

In 1986, over two million unarmed Filipino civilians—including members of the military, the government, and various religious groups—protested the results of an unfair election, demanded that the repressive and dictatorial Ferdinand Marcos step down from office, and forced the president to flee the country. Since the 1983-1986 Filipino People Power movement, many civil resistance campaigns have toppled autocratic regimes around the world, leaving democratic institutions in their wake. The 1980-1989 Solidarity campaign in Poland, the 1992 Thai pro-democracy movement in Thailand, and the 1977-1982 Anti-Junta campaign in Bolivia are only some of the unarmed political campaigns that were followed rapidly by democratic transition. And the co-occurrence of the two events—unarmed political resistance and democratization—is not limited to campaigns that seek regime change. The 1999-2000 Croatian campaign called for institutional reform, the 1989-1999 Kosovo Albanian campaign demanded territorial secession, and the 1988-1999 East Timorese resistance movement insisted on independence from Indonesian occupation.

Of course, another set of civil resistance campaigns in history were not immediately, if ever, followed by democratic transition. Civil resistance campaigns in Taiwan (1979-1985), Sudan (1985), and recently, Egypt (2011) all did not lead to democratization within five years of the campaign’s end.
Meanwhile, a comparable narrative exists about civilian campaigns that choose another non-institutional strategy: violence. Rather than protest, boycott, or strike, they use guerrilla warfare or terrorism to demand the same maximalist political goals of regime change, territorial secession, and anti-occupation. While some violent campaigns have produced democratic outcomes, others have not. The 1993-2002 Hutu Rebellion in Burundi, the 2003 Liberians United for Reconciliation and Democracy (LURD) campaign in Liberia, and the 1996-1997 insurgency campaign against the Peruvian government were all followed by democratization within four years of the end of conflict. In contrast, violent campaigns in Chad (1966-1990), Uganda (1980-1986), and Ethiopia (1978-1991) were not.

Does the choice to adopt nonviolent tactics over violent tactics increase the likelihood of democratic transition? If so, two questions emerge: (1) which democratic transitions in the past few decades can be causally attributed to insurgents’ tactical choice, and which ones cannot? And (2) what causal mechanisms explain how and why tactical choice relates to democratization?

In this thesis, I address these empirical questions by quantitatively examining 211 violent and nonviolent civil conflicts that occur between 1945 and 2006, take place within 122 different countries, and represent the maximalist political goals of regime change, territorial secession, and anti-occupation. I attempt to explain some of the variation in post-conflict democratization using insurgent’s tactical choice in civil conflict as the main independent variable.

To begin, I draw from two mostly separate political science literatures: the democratization literature and the civil conflict literature. The democratization
literature can be characterized by three dominant approaches. Structural explanations for democratization, like Seymour Lipset’s modernization theory (Lipset 1959, 1995), posit that democratic transition is determined by structural pre-requisites, including factors like economic development (Bernhard, Nordstrom, and Reenock 2001), liberal political attitudes (Inglehart 1997), and social capital (Putnam 1995). The elite transitions explanation, pioneered by O’Donnell, Schmitter, and Whitehead (2013), contends that democratization results from the strategic interactions between domestic elites. The final approach investigates the role of non-elites, expressed through the size and density of social classes (Moore 1966; Rueschemeyer, Stephens, and Stephens 1992) or through the actions of non-elite actors (Bermeo 1997; Collier 1999; Wood 2000).

Some of the political scientists within the conflict scholarship examine the specific relationship between civil conflict and democratization. Many exclusively study the effect of violent civil conflicts (usually called civil wars) on democratic outcomes, although no consensus exists about whether violent civil conflict affects democratization, and if so, whether they help or hurt (e.g., Weingast 1997; Toft 2010b; Fortna and Huang 2012). Other scholars examine the effect of nonviolent civil conflicts on democratic outcomes. These scholars have found preliminary evidence suggesting that nonviolent resistance promotes democratization (Karatnycky and Ackerman 2005; Chenoweth and Stephan 2011).

My approach falls within the non-elite transitions explanation and the nonviolent civil conflict explanation. I address several existing research limitations in the broader literature from each explanation emerges. The democratization literature
under-specifies the role of non-elite actors, has difficulty explaining when democratization happens, and has a methodological overreliance on case studies. Although the civil conflict literature overcomes some of these limitations, it has other shortcomings. It tends to conflate civil conflict with civil war, has not found conclusive evidence about whether the relationship between nonviolent resistance and democratization may be spurious or endogenous, and has not explored possible causal mechanisms that may explain that relationship.

I employ a large-$n$, quantitative research design that places non-elite actors and their choices in the forefront of my empirical inquiry. In doing so, I specify a role for civilians in non-elite transitions, address the issue of generalizability, and propose that democratization, more often than previously theorized, happens in the immediate aftermath of civil conflict. I use newly developed methods to investigate causal relationships using observational data. First, I use genetic matching to design a “pseudo-experimental” sample of 144 civil conflicts that is less sensitive to spuriousness and endogeneity. This method also allows me to test the sensitivity of my results to necessary assumptions and increase our confidence in causal claims. Second, I estimate the historical impact of nonviolent resistance on democratization using causal attribution analysis. Finally, I test competing causal mechanisms and estimate their nonlinear effect using causal mediation analysis.

My research produces six important findings. First, the use of nonviolent tactics, rather than violent tactics, improves the likelihood of democratization considerably, even accounting for fifteen possible confounding variables. The relationship is unlikely to be spurious. Second, the relationship furthermore does not
appear to be endogenous. In other words, nonviolent resistance is not also the result of democratization. Third, although there is no way to prove causality using an observational research design, the previous two findings increase our confidence in the claim that choosing nonviolent tactics of resistance leads to democratization. Fourth, nearly all of the nonviolent conflicts that emerged within non-democratic contexts and transitioned to democracy shortly after the end of conflict probably would not have democratized when they did had they adopted violent, rather than nonviolent, tactics. Fifth, one-third of the democratizing effect of nonviolent tactics operates through the causal mechanism of campaign success; other hypothesized causal mechanisms find no empirical evidence. Finally, two-thirds of the democratizing effect of nonviolent tactics is not mediated through campaign success and remains unexplained. Therefore, even when nonviolent campaigns fail to achieve their stated campaign goals, they are still more likely to democratize than if the same campaign had been conducted violently.

THEORETICAL IMPLICATIONS

In contrast to the two dominant explanations of democratization, my findings suggest that non-elites do matter for democratization. While structural explanations and elite transitions explanations tend to eschew the role of non-elite actors, my findings suggest that there exists variation in patterns of democratization that cannot be explained by either structural features or the strategic interaction of elites. Although
structural features are likely to constrain non-elite actors, I also suggest that non-elite actors can affect the structure of their countries.¹

In particular, I find that a particular kind of non-elite—civilian insurgents—can play an extraordinary role. Ordinary citizens willing to organize themselves into non-institutional opposition campaigns, especially those primarily use nonviolent tactics, are the true theoretical force examined within this book. Contrary to non-elite arguments that relegate the role of non-elites to their characteristics as social classes (Moore 1966; Rueschemeyer, Stephens, and Stephens 1992), my findings imply that the choices that civilian insurgents make, such as the choice between violent and nonviolent tactics, can ultimately have an effect on the post-conflict political order (Chenoweth and Stephan 2011). Of course, this is not to suggest that insurgents always have a choice between the two tactics; but when they do, their choices can have dramatic effects. As previous scholars have argued, democracy can be forged from below (Wood 2000; Ekiert and Kubik 2001; Teorell 2010).

In addition, the actions of civilian insurgents may begin to explain a puzzle in the democratization literature: when does democratization occur? Structural explanations cannot successfully explain when democratic transitions occur (Przeworski 2000, 273), while elite transitions explanations tend to explain democratization using negotiations that are “too proximate” (Teorell 2010, 11) to the outcome. As this thesis will demonstrate, many transitions occur almost immediately after the resolution of civil conflicts.

¹ Many other scholars have also made similar theoretical arguments (e.g., Wood 2000; Schock 2005; Chenoweth and Stephan 2011).
Finally, my findings imply that the relationship between nonviolent resistance and democracy is still understudied. Although I discover one causal mechanism, campaign success, it explains only one-third of the variation in democratization explained by the variation in tactical choice, which implies one of two possible options: there is either something intrinsic about nonviolent resistance that explains most of its causal effect, or there are other operating causal mechanisms that existing theory has not described. The new empirical evidence in this thesis allows existing theories about the relationship between nonviolent resistance and democracy to be reevaluated.

POLICY IMPLICATIONS

Although my study is not designed to specifically address policy concerns, my findings do suggest a policy implication. Democracy-promoting countries may find supporting domestic insurgencies using weapons and material resources less effective than supporting insurgencies with the nonviolent interventions, such as aiding with the development of nonviolent organizational capacities. Not only might this increase their likelihood of achieving their stated campaign objectives (Chenoweth and Stephan 2011), it may intentionally or unintentionally lead to more democratic outcomes.

PLAN OF THE BOOK

The remainder of this book is organized as follows. Chapter 2 reviews and assesses the two academic literatures from which this thesis benefits and to which this thesis
contributes. I discuss the literatures’ existing shortcomings, how I attempt to
overcome them, and remaining issues: definitions, variables, and data. In Chapter 3, I
use new developments in the causal inference literature to create a “pseudo-
experimental” sample that allows me to investigate causal relationships using
observational data. I will argue that the relationship between nonviolent resistance
and democratization is not spurious or endogenous, increasing our confidence in the
claim that the relationship is causal. In Chapter 4, I estimate the historical impact of
nonviolent resistance using causal attribution analysis, finding that nearly all cases of
post-conflict democratization associated with nonviolent campaigns would not have
democratized had the same campaign used violent tactics instead. Chapter 5 uses the
causal mediation analysis framework to test causal mechanisms that might explain the
relationship between nonviolent tactics and democratization. I find that success
explains one-third of the democratizing effect of nonviolent tactics; other
hypothesized mechanisms find no support. Finally, in Chapter 6, I conclude by
summarizing my central findings, identifying the limitations of my research,
suggesting avenues for future research, and discussing the theoretical and policy
implications of my findings.
In this chapter, I review the two related but largely separate literatures from which my study benefits and to which my study contributes. The democratization literature includes explanations of democratic transitions that focus on structural determinants, the role of domestic elites, and the role of domestic non-elites. The civil conflict literature examines explanations of democratization that focus on the role of violent civil conflict (namely, civil wars) and nonviolent civil conflict. Within each literature, I survey and assess divergent explanations, discussing in particular their recurring limitations. Then I synthesize the more general shortcomings in the literature and explain how my study addresses them. After doing this with both the democratization and civil conflict literatures, I conclude by discussing remaining issues: definitions, measures, and data.

THE DETERMINANTS OF DEMOCRACY

The democratization literature is enormous and more than a half-century old, but much of the existing work falls within three broad explanations of democratic transition: structural determinants, the role of elites, and the role of non-elites. Each camp contains theoretical and empirical research explaining democratization but beginning from a different assumption about what factor or actor has causal primacy.
My study falls within the scholarly work on non-elite transitions, making three contributions to the democratization literature: it explains how non-elite actors may actively shape democratic transitions, it provides a partial explanation for when democratization happens, and it overcomes some of the literature’s methodological limitations.

**Structural Determinants**

The structural approach identifies an extraordinary range of underlying factors that are, as the argument goes, “requisite” to democracy (Lipset 1959, 102). In Seymour Lipset’s (1959) classic piece, “Some Social Requisites of Democracy,” he examines European and Latin American countries around World War I and famously proposes that modernization—measured through industrialization, urbanization, and rising levels of education—is a precondition for democracy. Although Lipset’s (1959) modernization theory has found both supporting (Boix and Stokes 2003; Epstein et al. 2006) and opposing (Przeworski 2000) evidence, the argument that structural factors create the conditions for democratic transition remains highly influential today (e.g., Teorell 2010; Bernhard, Nordstrom, and Reenock 2001). Indeed, the empirical evidence for structural determinants of democracy exists on multiple analytical levels.

At the international level, scholars have found evidence for global and regional democratic diffusion (Gleditsch and Ward 2006; Brinks and Coppedge 2006; Starr 1991), the theory that democratization in one country is transmitted through the international system, influencing other countries. There is also evidence that
democracy is made more likely by economic globalization (Rudra 2005) and by involvement with international organizations (Pevehouse 2005).

At the domestic level, empirical evidence suggests that economic conditions such as income inequality (Burkhart 1997; Muller 1995, 1988), oil abundance and exports (Dunning 2008; Ross 2001), social welfare policies (Burkhart 2000), and economic crises (Bernhard, Reenock, and Nordstrom 2003; Gasiorowski 1995) can explain democratization trends. The same goes for non-economic domestic factors, such as political attitudes and culture (Inglehart 1997), country size (Dahl and Tufte 1973), the legacy of colonialism (Bernhard, Reenock, and Nordstrom 2004), and religious fractionalization (Lipset 1995).

A related body of work examines determinants of democracy that begin with political institutions, such as constitutional frameworks, electoral laws, and party systems (e.g., Norris 2008; Bernhard, Nordstrom, and Reenock 2001; Snyder and Mahoney 1999; Bratton and Van de Walle 1997). And a subset of the work examines the democratizing effect of civic communities and social capital (Paxton 2002; Putnam, Leonardi, and Nanetti 1993; Bernhard 1993; Putnam, Serageldin, and Taboroff 1992; Almond and Verba 1989).

Although the numerous empirical studies linking structural factors with democratization have significantly advanced the theoretical understanding of democratization, this line of argumentation reveals several recurring limitations. First, even when structural theories are sensitive to issues of human agency (Lipset 1959; Geddes 1999, 2003), the role of human agents is usually “black boxed” and their characteristics “underspecified” (Teorell 2010, 18). These theories rely on
environmental triggers, so they struggle to explain the role of individual and collective social actors, including who the relevant actors may be, why they are motivated, and how they may be important. For example, structural theories do not provide much leverage for understanding whether or how elites, non-elites, or third-party peacebuilders can contribute to democratization.

Second, many of the structural factors supported by the literature, such as oil abundance and exports, religious fractionalization, political culture, or (in the most extreme case) country size, have little within-country variation and are slow to change over time, if they change at all. Consequently, most structural models of democratization can explain which states democratize, but not when they democratize, limiting their explanatory power. For example, decades of research on the relationship between economic development and democratization has not produced a threshold at which democratization happens (Przeworski 2000).

Finally, as Jan Teorell argues, “the black-boxing of causal mechanisms also means that most structural theories of democratization lack micro-foundations” (Teorell 2010, 19). What exactly are the causal mechanisms that provide the explanation for structural determinants? Until structural theories of democratization can be decomposed into empirically tested causal mechanisms, the underlying premises behind these theories remain weaker than they can be.

*Elite Transition*

By examining the strategic, interactive process between domestic elites, the second camp of scholars addresses some of the important limitations in studying
democratization outlined above. The two most influential publications in this camp are written by Dankwart Rustow (1970) and O’Donnell, Schmitter, and Whitehead (2013). Their approach, often called the “transition paradigm,” is characterized by several features. As the classic argument goes, authoritarian countries transition to democracy in three stages: liberalization, democratization, and consolidation. The political outcome—democracy or another form of authoritarianism—is based on “genuine choice” (Rustow 1970, 356), determined top-down by the interactions of domestic regime elites. Nearly always, the transition begins with some split in the regime leadership between “hardliners” and the “softliners” (O’Donnell, Schmitter, and Whitehead 2013, 16). In this view, structural factors are regarded as unimportant, and the roles of individual and collective non-elite actors are, at best, “ephemeral” (O’Donnell, Schmitter, and Whitehead 2013, 64). On this point, Samuel P. Huntington (1984) elaborates,

It is often assumed that since democracy, to a greater degree than other forms of government, involves rule by the people, the people therefore play a greater role in bringing it into existence than they do with other forms of government. In fact, however, democratic regimes that last have seldom, if ever, been instituted by mass popular action. Almost always, democracy has come as much from the top down as from the bottom up; it is as likely to be the product of oligarchy as of protest against oligarchy. The passionate dissidents from authoritarian rule and the crusaders for democratic principles, the Tom Paines of this world, do not create democratic institutions. (1984, 212)

As an implication, scholars who take this view are particularly interested in in the calculations, negotiations, bargains, compromises, and pacts that result from domestic

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2 In fact, Terry Lynn Karl calls the search for preconditions of democracy “futile” (Karl 1990, 3). Karl reaches this conclusion by examining Latin American cases in the third wave of democratization.
regime elites acting in their own interest (e.g., Huntington 1984; Karl and Schmitter 1991; O’Donnell, Schmitter, and Whitehead 2013).

The theory that elite interactions drive democratization has been heavily studied through comparative case studies on what Huntington calls “the third wave of democratization” (1991, 3). This wave of democracy came at the end of the twentieth century, following the first wave at the end of World War I and the second wave at the end of World War II. Included in the inquiry are cases in Latin America (Schmitter, O'Donnell, and Whitehead 1987; Karl 1990), South Europe (O’Donnell, Schmitter, and Whitehead 1986; Karl and Schmitter 1991), and Eastern Europe (Karl and Schmitter 1991).

The elite transitions explanation comes to two important conclusions. By comparing the late twentieth century outcomes of four different “modes of transition” (Karl and Schmitter 1991), which include pacts, imposition, reform, and revolution, many transition scholars agree that pacts are the most effective strategy for creating democracy. Additionally, by observing Latin American democratization during the early stages of the third wave, transition scholars conclude that democracy has “no prerequisites” (Karl 1990; Karl and Schmitter 1991; Shin 1994). This argument explicitly critiques the notion that there exist structural determinants of democracy.

This “no prerequisite” view is empirically corroborated by one of the most persuasive writings on democratization, a study conducted by Adam Przeworski et al. (2000). Their formal models suggest that the presumed causal relationship between economic development and democratization, what Lipset (1959) called modernization theory, is not actually causal. Democracy, they argue, does not emerge from
economic development. Instead, their data supports the theory of “exogenous democratization” (2000, 92), which argues that democracy survives at a higher rate in wealthier states. Exogenous democratization, they argue, explains the previously observed correlation between economic development and democratization.³

The elite transitions explanation has made many important contributions toward understanding democratization in the third wave. Particularly, the theoretical inclusion of human actors was a profound contribution. Nevertheless, this approach has several theoretical and empirical limitations.

First, Teorell calls the transition paradigm “border[ing] on tautology” (Teorell 2010, 21). The independent variable, elite calculations, poses conceptual and temporal limitations on explanation. That the independent variable is understood to be part of the democratization process is problematic, because the explanatory variable and the outcome need to be independent phenomenon. Additionally, the temporal proximity between the explanatory variable and the outcome limits the theory’s predictive and explanatory power. Rather than stand alone, the explanation diverts the same question elsewhere: if negotiated pacts create democracy, what explains why elites pursue negotiation rather than another strategy?

A second limitation lies in the explanation’s stubborn rejection of structural factors, the “no prerequisites” argument. In embracing human agency as the primary determinant of democracy, the explanation casts off important empirical trends previously found in the democratization literature. As Thomas Carother (2002) writes

³ Some researchers criticize the argument, finding evidence that economic development both increases the likelihood of democratic transition and the likelihood of democratic survival (Boix and Stokes 2003; Epstein et al. 2006).
in his article, “The End of the Transition Paradigm,” some of these structural conditions, like economic development, “clearly weigh heavily on political outcomes” (Carothers 2002, 16). Because it rejects structural arguments, the transitions explanation has difficulty recognizing how elite calculations may be constrained by the context within which elites exercise political power.

Finally, the elite transitions paradigm has failed to predict the outcomes of many relatively new democratic transitions. It was developed and empirically verified on cases exclusively within Latin America and Southern Europe. Since then, scholars have found that the theory does not generalize to other cases well. In particular, researchers have found that it cannot explain several African cases, in which democracy was not created by elites nor through pacts (Bratton and Van de Walle 1997), nor can it explain several post-Communist cases of democratization (McFaul 2002). Therefore, the theory has important empirical limitations.

**Domestic Non-Elites**

The third and last scholarly camp challenges the assumption that domestic elites are the primary human agent of democracy, examining instead the role of domestic non-elites. Essentially, these scholars approach democracy as a phenomenon created “from below” (Wood 2000). The argument takes one of two forms, depending on the proposed theoretical role of non-elites.

In the first form, the emergence of democracy is explained by the size and density of collective social classes. In the seminal book, “The Social Origins of Dictatorship and Democracy,” Barrington Moore (1966) famously argues, “No
bourgeois, no democracy” (Moore 1966, 418). The existence of a middle class, he argues, is necessary to extract democracy from rich landlords. Following in the tradition, Dietrich Rueschemeyer et al. (1992) examine the role of social classes, but instead argue that it is the political power of the working class, relative to landowners, that explains democratization. These explanations emphasize the importance of non-elite actors, but interestingly resemble structural theories of democracy. Even though non-elite actors are important, it is not their agency that creates democracy.

In the second form of the argument, democracy is explained by the strategic interactions between non-elite actors and elite actors. Of course, this resembles the transitions explanation, except relaxing the assumption that non-elite participants are essentially impotent. For instance, Ruth Collier (1999) examines 21 countries in Western Europe and South America to explain how labor unions and labor-based parties, representing the working class, can create democracy in cases where the working class is sufficiently powerful. Nancy Bermeo advances the theory that popular mobilization of non-elites can lead to democracy if their demands are not perceived by elites as too extreme (Bermeo 1997). Other scholars have studied the relationship between mass mobilization and democracy in several countries and regions: South Africa and El Salvador (Wood 2000), Poland (Ekiert and Kubik 2001), Brazil (Stepan 1989), and Sub-Saharan Africa (Bratton and Van de Walle 1992, 1994, 1997). These studies argue that popular mobilization can force elites to concede democracy, because even when elites can contain their actions, elites sometimes cannot prevent continued resistance (Wood 2000).
A subset of this approach uses formal theory to model the strategic interactions between elites and non-elites. Researchers like Boix (2003) and Acemoglu and Robinson (2006) attempt to explain when revolutionary movements produce democracies and when they do not. Under their framework, the rich generally prefer dictatorship, which allows them to keep their wealth, while the poor generally prefer democracy, which allows them to push for income redistributive policies. When the cost of repressing the poor is too high, democracy emerges as a way for elites to make a credible commitment to income redistribution (Boix 2003; Acemoglu and Robinson 2006).

Although the non-elite transitions explanation is filled with excellent research, as always, there are notable areas of weakness. The most significant limitation is the overreliance on comparative case studies. Only one scholar in this literature corroborates his or her theory with systematic, large-n evidence. That scholar is Boix (2003), who finds evidence for his theory; however, his findings have since been disputed (Perotti 1996; Ross 2006). Further progress requires systematic corroboration of the assumptions and mechanisms upon which these theories are premised.

Problems in the Democratization Literature

As the scholarly research within these three explanations show, the broader democratization literature exhibits three recurring issues that I attempt to address: (1) an under-specification of the role of non-elite actors, (2) an inability to explain when democratization occurs, and (3) a methodological overreliance on case studies.
First, a prevailing view among scholars who study democratization is still that democratization results from the strategic negotiations among regime elites (Przeworski 2000). To the extent that non-elite participants can play a role in negotiations, they are sometimes incorporated into the same framework. For instance, Wood argues that in South Africa and El Salvador, insurgents were able to forge a path to democratization, because their circumstances allowed them to participate in negotiations as a “counter-elite” (2000, 11). However, as studies in Africa and elsewhere show, non-elite human agents can be important forces in democratization, even when they do not function as elite negotiators (Bratton and Van de Walle 1997). As a whole, the democratization literature needs to better specify the role of non-elite actors in democratic transitions.

Central to my approach is the presumption that democratization may result from an interactive process between domestic elites and non-elites. Through participation in violent and nonviolent civil conflicts, I will argue that ordinary citizens may actively influence political outcomes. Their actions may force democratic concessions, displace regime elites entirely, or contribute to a society that values and expects nonviolent means of political contention.

Second, the structural determinants and elite transitions literature tend to have trouble explaining when democratization occurs. Even Przeworski et al. (2000), who make the most systematic, rigorous attempt to address both the structural and elite transitions explanations, say, “We find it difficult to explain why dictatorships die and democracies emerge” (2000, 137). They model the transition to democracy as “pretty much random” (2000, 273), focusing instead on the survival of democracy. A
satisfactory explanation of democratization ideally needs to also explain when democratization happens, not just why and how.

Of course, by studying civil conflicts, I also attempt to explain when democratization happens, not only why or how. It is important to notice that this study does not argue that civil conflicts are “pre-requisite” to democracy. It argues that democracy, more often than the democratization literature presumes, is created by civil conflicts driven from below. Therefore, our best estimation of when democratization occurs is not “pretty much random,” but rather, in the period after certain civil conflicts.

Finally, the empirical evidence that arises from the elite and non-elite transitions explanations have an overreliance on comparative case studies (O’Donnell, Schmitter, and Whitehead 1986; Schmitter, O'Donnell, and Whitehead 1987; Karl 1990; Karl and Schmitter 1991; Przeworski 2000; Mattes and Bratton 2007; O’Donnell, Schmitter, and Whitehead 2013; Stepan 1989; Bratton and Van de Walle 1997; Wood 2000; Ekiert and Kubik 2001). The theoretical work in the non-elite transitions approach similarly relies on heavily on case studies (Acemoglu and Robinson 2006). Although there is much to be learned from these case studies, as with any methodology, there are limitations. The methodological overreliance on case studies makes difficult credible generalizations about how human agents affect the process of democratization. However, understanding general trends about how human agents can affect and create democratic transitions is too important to ignore.

My particular large-$n$ empirical and statistical approach allows me to uncover patterns about civil conflicts and democratization that are generalizable across space
and time. This overcomes one of the most obvious methodological limitations of the democratization literature. I counterbalance the overreliance on comparative case studies and contribute to the methodological diversity of the democratization literature.

**CIVIL CONFLICT AND DEMOCRACY**

A separate literature examines the specific relationship between civil conflict and democracy. Like the domestic non-elite transitions explanation, this literature relaxes the assumption that domestic regime elites are the only relevant actors in democratization. By studying civil conflict, the domestic struggles between elites and non-elites, they allow for the possibility that violent and nonviolent rebel actions may substantively shape their governments. Although nearly all of the literature operationalizes civil conflict as a violent phenomenon (namely, civil war), there has also been important scholarship in the study of nonviolent conflicts. My study falls within the latter category. I make three contributions to the existing literature: I treat violent and nonviolent tactics as strategic alternatives, I address the related issues spuriousness and endogeneity in a key study, and I test competing causal mechanisms (for the first time) to better explain the relationship between nonviolent resistance and democratization.

*Violent Conflict*

Nickerson and Wantchekon (2001) find that as much as 40 percent of civil wars between 1945 and 1993 resulted in some improvement to democracy. Virginia Page
Fortna writes, “while many countries democratize after civil war, many do not” (2012, 801). Under what conditions does democracy emerge from civil war? The theoretical and empirical work comes to divergent conclusions.

Some researchers, drawing from the democratization literature, argue that civil war outcomes determine the emergence of democracy. Barry Weingast (1997) uses a game-theoretic model to show that negotiated pacts create the foundation for democracy after civil war, particularly if they are self-enforcing. If they punish parties who defect, negotiated pacts solve credibility problems associated with disarming and creating democratic institutions. Elisabeth Wood (2000, 2001) finds that in South Africa and El Salvador, violent insurgents acted as effective “counter-elites,” forcing negotiations with regime elites that led to democratization. More broadly, Gurses and Mason (2008) studies civil wars that ended between 1944 and 1997, finding that negotiated settlements frequently create the conditions for democratization.

But Monica Duffy Toft (2010a, 2010b) expands the set of civil wars, including each one that ends between 1940 and 2000, and comes to the opposite conclusion. Toft finds that every civil war outcome except one hurts the prospects of democratization in the long run (ten to twenty years after the conclusion of civil war). She finds that negotiated settlements, ceasefires, and government victories are associated with less democratic outcomes, while rebel victories are associated with more democratic outcomes. Toft explains, “when ‘good’ rebels win... reforms tend to be dramatic and far-reaching, resulting in fewer grievances among survivors” (2010b, 66). In short, there is no consensus about the effect of negotiated pacts on democracy.
Using game theory, Wantchekon and Neeman (2002) propose an alternative explanation. In their model, the prospect of democratization depends on a two-stage game that includes two warring factions and the citizens. Citizens prefer democracy, because democracy protects them (relatively) against “expropriation.” This democratic feature also benefits each warring faction. Democracy results when the political preferences of citizens are diverse enough, such that each warring faction believes it can win post-war elections. Although the model is used to explain democratization in Lebanon (1975–90) and El Salvador (1981–92), no further attempt to generalize the model has been made.

The scholarly work also examines a range of other factors believed to contribute to democratization, although there is little consensus about their effect. Researchers do not agree about the effect of pre-war levels of democracy (Sambanis 2000; Fortna 2008), ethnic partitions as part of settlement (Sambanis 2000), or even economic development (Sambanis 2000; Fortna 2008).

To address some of these mixed findings, Fortna (2012) attempts to correct some of the methodological shortcomings in previous studies. She uses multivariate analysis and examines democratization in the short- and long-term. Her results show that negotiated settlements only have a short-term effect, peacebuilding does not facilitate democratization, economic development helps democratization, and oil hurts it. Simply put, civil wars have little to no effect on democratization. Instead, the determinants of democratization after civil war are “much the same for post-civil war societies as for other societies” (2012, 801).
To summarize, the work on post-civil war democratization finds no consensus about whether civil war helps democratization, hurts democratization, or whether it affects democratization at all. Scholars disagree about how civil war outcomes—particularly, negotiated settlements or one-sided victories—affect democratization. Although two well-specified theories about the relationship between civil wars and democracy exist (Weingast 1997; Wantchekon and Neeman 2002), the evidence in each case is either mixed or non-existent. Despite years of excellent research, an understanding of the relationship between violent civil conflict and democracy remains elusive.

Nonviolent Conflict

Although scholars have written many descriptive accounts of nonviolent civil conflict, the theoretical and empirical work examining its relationship to democratization is relatively small. These researchers study two distinct but related processes. One analyzes the effectiveness of nonviolent resistance in toppling existing political regimes, while the other analyzes its effectiveness in creating democratic transitions.

According to the first camp of scholars, nonviolent resistance can be effective in dismantling authoritarian regimes, provided it follows certain strategic prescriptions. Generally, successful nonviolent campaigns have clear goals, maintain strict nonviolent discipline, and provoke defections from the military (Ackerman and

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4 For example, see Ackerman and DuVall (2000), Sharp and Paulson (2005), and Zunes, Kurtz, and Asher (1999)
In contrast to other scholars in this literature, who rely on comparative case studies between successful and unsuccessful nonviolent campaigns, Chenoweth and Stephan (2008; 2011) analyze over 300 violent and nonviolent civil campaigns between 1900 and 2006, treating nonviolent resistance as a strategic alternative to violent resistance. They identify mass participation as a primary causal mechanism explaining why nonviolent campaigns tend to be more successful than violent campaigns (2008; 2011). They also find that nonviolent campaigns are twice as likely to achieve their stated campaign goals as violent campaigns. Overall, these scholars find a robust relationship between nonviolent resistance and success.

The second camp examines the relationship between nonviolent resistance and democratic transition. Two studies have attempted to empirically and quantitatively describe this relationship. Adrian Karatnycky and Peter Ackerman (2005) use Freedom House data on democracy—which measures democracy using a seven-point scale—to examine 67 democratic transitions between 1972 and 2005. Using simple bivariate descriptive statistics, they find that nonviolent resistance preceded 50 of the 67 democratic transitions included in their sample and led to improvements in democracy that were, on average, more than twice as large as those from “top down” elite negotiations.

Chenoweth and Stephan (2011) also contribute to this work, developing a preliminary model of the relationship between nonviolent resistance and democracy, which improves upon Karatnycky and Ackerman’s (2005) study in several ways: they
examine a much larger set of campaigns (1900 to 2006), they include both successful and unsuccessful democratic transitions (i.e., they do not select on the dependent variable), and they create a multivariate model to account for two possible confounders: pre-existing Polity score and duration of conflict. Using this model, they estimate that, relative to violent campaigns, nonviolent campaigns increase the probability of post-conflict democracy by 0.45.

Why might this be true? What causal mechanisms might explain why nonviolent resistance campaigns are more likely to democratize than violent campaigns? Several theories exist, although none have been empirically verified. Chenoweth and Stephan (2011, 207) propose three possible explanations. I call the first explanation the *mobilization hypothesis*. They argue that nonviolent campaigns have a participation advantage relative to violent campaigns. They are more likely to mobilize mass participation, because they tend to have lower physical, informational, moral, and commitment barriers. Furthermore, this participation “encourages the development of democratic skills and fosters expectations of accountable governance, both of which are less likely when transitions are driven by opposition violence” (2011, 207). In other words, the argument is that the means can inform the ends: citizens engaged in nonviolent resistance are more likely to create a politically engaged citizenry (e.g., democracy) in the post-conflict future.

Their second explanation relies on the finding that nonviolent conflicts are more likely than violent ones to achieve their stated campaign goals. I call this explanation the *success hypothesis*. Success after nonviolent campaigns in particular can act as a causal mechanism for democratization in two ways: it reinforces the
expectation that “the postconflict political regime will also be nonviolent in its relationship with constituents” (2011, 208), and it creates in the collective memory the wisdom that nonviolent techniques “can be effective in winning power” (2011, 208). The collective memory and associated expectations may create democratizing political constraints the post-conflict regime.

Their final explanation lies in the common nonviolent campaign practice of creating parallel institutions that are alternative to official state institutions. I call this the democratic institutions hypothesis. These institutions are often relatively consensual and often run in opposition to authoritarian institutions that “are more likely to operate by means of secrecy and martial values” (2011, 208). For instance, the anti-apartheid campaign in South Africa established elected local governments and court systems to better represent the black townships. Theoretically, Chenoweth and Stephan (2011) argue that these democratic institutions “might aid in the transition to a democratic system” (2011, 208).

The scholarly work investigating the relationship between nonviolent civil conflict and democracy is characterized by preliminary evidence and three theories. As the empirical work is in its infancy there are several ways existing models can be improved. In particular, models have not accounted for potential spuriousness, potential endogeneity, and they have not tested alternative causal mechanisms.

The first issue is spuriousness. A number of plausible theoretical mechanisms may confound the observed relationship between nonviolent resistance and democracy, especially because no scholars have rigorously accounted for them. In particular, many structural factors, like economic development, are strongly
correlated with democracy and are unaccounted for in current models. Without accounting for these potential confounders, it is not unreasonable to expect that the observed effect may actually result from some other third variable. More generally, this also calls into question the relationship’s causal validity, which is, of course, one of the primary analytical questions within this scholarship.

Second, the other potential causal issue is endogeneity. The relationship between nonviolent resistance and democratization may be endogenous, in which case democratization also leads to nonviolent resistance. It is plausible that the factors that lead to democratic transition also prompt nonviolent campaigns to emerge.

Third, while the empirical literature has found evidence for a potential causal relationship, it has not empirically tested whether any of the hypothesized causal mechanisms actually mediate the relationship between nonviolent resistance and democracy. As a result, scholars do not know which existing explanations have more or less explanatory power.

Problems in the Civil Conflict and Democracy Literature

Within the broad civil conflict and democracy literature, I attempt to address three important theoretical and methodological limitations: (1) a conflation of civil conflict with civil war, (2) the related causal issues of confounding and endogeneity, and (3) the lack of research on causal mechanisms.

First, I follow the lead of Chenoweth and Stephen (2008; 2011) by contributing to a conception of civil conflict that includes both violent and nonviolent civil conflicts. The current civil conflict literature focuses overwhelming focuses on
the relationship between civil war and democratization (Weingast 1997; Sambanis 2000; Wood 2000; Wantchekon and Neeman 2002; Gurses and Mason 2008; Toft 2010a, 2010b; Fortna and Huang 2012). However, recent evidence suggests that the effect of civil conflict on democracy may depend on the strategic choices that rebels make, namely, whether they use primarily violent or nonviolent tactics (Karatnycky and Ackerman 2005; Chenoweth and Stephan 2011). I contribute to this work by including in my research both violent and nonviolent campaigns, distinguishing between the two in quantitative models.

Second, I simultaneously address concerns of confounding and endogeneity. The current research on civil conflicts (in general) and nonviolent conflicts (in particular) are limited in their ability to make causal statements. Indeed, observational data pose particular problems for causal inference (Stuart 2010; Diamond and Sekhon 2013). The concern is exacerbated by the literature’s minimal or nonexistent treatment of potential confounding variables. Using matching techniques from the causal inference literature (Rubin 2001; Ho et al. 2007; Stuart 2010; Sekhon 2011; Diamond and Sekhon 2013), I nonparametrically condition my results on a wide range of potential confounders; create a well-balanced, “pseudo-experimental” sample; generate causal estimates; and finally, test the sensitivity of my results to potential additional confounders. Although I still cannot prove causality, my relatively “careful design” (Stuart 2010, 1), through advanced matching techniques, reduces the concern for spuriousness or endogeneity, while increasing our confidence in a causal relationship.
Finally, I address the current lack of scholarly research on potential causal mechanisms. Although there are theories about why nonviolent civil conflicts may have a different effect on democratization than violent civil conflict (Sharp 2008; Chenoweth and Stephan 2011), no empirical studies have been conducted to test the relative explanatory power of competing causal mechanisms. I address this using a new technique in the causal inference literature called causal mediation analysis, which gives researchers the new ability to make unbiased estimates of the effect of causal mechanisms in non-linear systems (Imai, Keele, and Tingley 2010; Imai, Keele, and Yamamoto 2010; Imai et al. 2011). This general framework allows me to test the three competing theories discussed earlier: the mobilization hypothesis, the success hypothesis, and the democratic institutions hypothesis.

**DEFINITIONS AND DATA**

In the remainder of this chapter, I justify several methodological choices necessary to begin my empirical analysis. I define democracy, violence, and nonviolence, discussing both how I conceptualize and measure them, as well as the strengths and weaknesses inherent. Then I conclude the chapter by describing the data used throughout.

*Defining and Measuring Democracy*

In this study, I adopt a minimalist, dichotomous definition of democracy. Democracy requires some elements of each of the following: competitive political participation,
competitive executive recruitment, open executive recruitment, and constraints on the chief executive (Marshall et al. 2002). In this approach, minor deficiencies in one category can be compensated by strengths in another, but only to an extent. Failing to meet the minimum procedural threshold results in the categorization of a country as nondemocratic.

In order to measure whether a country is democratic or nondemocratic, I use the popular 21-point Polity IV index (Marshall, Jaggers, and Gurr 2002). Although the original variable ranges from -10 (fully authoritarian) to 10 (fully democratic), I follow Polity IV’s recommendation and consider countries democratic when they score 6 or higher. This corresponds to the minimum conceptual threshold for democracy.

Of course, conceptualizing and measuring democracy in this way has some drawbacks. For one, it only attempts to describe democracy as an outcome, while ignoring other important aspects of democracy, such movement along a continuous scale (Fortna and Huang 2012), democratic consolidation (O’Donnell, Schmitter, and Whitehead 2013; Diamond 1997), or democratic survival (Przeworski 2000). It also ignores the fact that democracies that fulfill the minimal procedural requirements may nevertheless be qualitatively different; for instance, Przeworski et al. (2000) distinguish between parliamentary, mixed, and presidential democracies. Finally, a minimalist definition of democracy risks unintentionally omitting relevant attributes of democracy, such as participation (Munck and Verkuilen 2002).

Nevertheless, conceptualizing and measuring democracy as minimalist and dichotomous has important analytical advantages. It creates clear standards and
identifies necessary, common characteristics for democracy (Boix, Miller, and Rosato 2013). This avoids the analytical risk of explaining too many irrelevant attributes. It also allows me to revisit the important theoretical question of when democratic transitions happen. As researchers have discovered, the characteristics that explain the emergence of democracy are not the same ones that explain the consolidation of democracy (Przeworski 2000; Brown 2011). Decades of research have not yet produced a parsimonious, comprehensive explanation for the emergence of democracy.

Based on my characterization of democracy, Table 2.1 compares the resulting number of democratic and nondemocratic countries within civil conflicts cases. As the table shows, the vast majority of civil conflict cases take place within countries I characterize as non-democratic, and a majority of them remain non-democratic after the conclusion of conflict. However, the number of countries that transition to democracy within five years of conflict resolution is still substantial.

<table>
<thead>
<tr>
<th>TABLE 2.1 Frequency of Democracies and Non-Democracies Within Civil Conflict Cases, 1945 – 2006</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
</tr>
<tr>
<td>1 Year Before Conflict Resolution</td>
</tr>
<tr>
<td>Frequency (Percent)</td>
</tr>
<tr>
<td>Democratic</td>
</tr>
<tr>
<td>Non-Democratic</td>
</tr>
<tr>
<td>Missing Values</td>
</tr>
<tr>
<td>Total</td>
</tr>
</tbody>
</table>
Defining and Measuring Violence and Nonviolence

It is also necessary to discuss some of the complications in conceptualizing and measuring the main independent variable: the primary strategic tactic adopted by nonstate combatants during civil conflict. Chenoweth and Stephan (2008; 2011) conceptualize and measure primary strategic tactic dichotomously. Nonstate combatants in a civil conflict campaign either rely on primarily violent tactics or primarily nonviolent tactics. Violent civil conflicts typically include campaigns of insurgency, guerrilla warfare, civil war, or terrorism. Meanwhile, nonviolent civil conflicts are not so different. They include all the same campaigns, but differ in that their combatants are primarily unarmed. Based on this conceptualization, the difference between civil conflict tactics is not due to “moral,” “philosophical,” or “principled” reasons, but rather, “pragmatic” reasons (Schock 2005, xvi).

Still, there is tremendous gray area between the two categories. Often, civil conflict campaigns do not rely exclusively on one tactic or another. In fact, according to the NAVCO 2.0 data set (Chenoweth and Lewis 2013), which is discussed later, 48 percent of campaigns coded as primarily nonviolent included a violent radical flank at some point. Disaggregating the data further reveals another complication: the primary tactic within campaigns varies can vary from year to year. Within the full 1,331 campaign-year observations included in NAVCO 2.0, 342 campaign-years take place within primarily nonviolent campaigns. Of the 342 nonviolent campaign-years, 50 (17 percent) are described as primarily violent. Similarly, there are 1,384 campaign-years that take place within primarily violent campaigns, but 53 (4 percent) of the campaign-years are characterized as primarily nonviolent.
Despite complications, I argue that measuring primary tactical choice dichotomously can be advantageous. The overall, primary tactical choice of a campaign is more likely to have an effect on democratization than individual or year-to-year strategic choices. Additionally, existing explanations about the relationship between nonviolent resistance and democracy do not posit causal mechanisms that operate via year-to-year strategy changes or through violent radical flanks within nonviolent campaigns. Thus, a dichotomous measurement better operationalizes the theoretical relationship. I leave inquiries about the effect of violent radical flanks or year-to-year strategic and tactical shifts for future research.

The Cases

Before moving into empirical analysis, I address one final consideration: the data. All civil conflict observations used in this project come from Erica Chenoweth and Orion Lewis’s Nonviolent and Violent Campaigns and Outcomes (NAVCO) data project (Chenoweth and Lewis 2013). Specifically, I use the second iteration of their data set, NAVCO 2.0, which includes fewer cases but more variables. Included in NAVCO 2.0 are the universe of known political campaigns that fall between 1945 and 2006 while obeying two campaign criteria:

1. “There is a series of coordinated, contentious collective actions with at least 1,000 observed participants… Once participation during peak events no longer reaches 1,000, we consider the campaign effectively concluded.

2. “We only include major campaigns that claimed ‘maximalist’ goals at some point during their lifespan. These include goals of regime change,
secision, or the removal or a foreign occupier.” (Chenoweth and Lewis 2013, 417)

As mentioned earlier, the result of these criteria are 250 major violent and nonviolent civil conflicts that occurred between 1945 and 2006. They take place in 122 unique countries, located across the globe, in Asia, Latin America, Africa, and Eastern and Western Europe.

There is one potential concern about drawing conclusions from NAVCO 2.0. Like other data sets on nonviolent resistance, it is affected by underreporting bias (Chenoweth 2011). Although the campaigns in NAVCO 2.0 reflect an expert consensus sample, the sample may be biased toward campaigns that are large, mature, and successful—especially among nonviolent campaigns. This is because relative to violent campaigns, nonviolent campaigns still receive much less scholarly attention. Furthermore, NAVCO 2.0 may exacerbate the problem by excluding campaigns that do not have the maximalist political goals of regime change, anti-occupation, or secession. These omitted campaigns may be lower profile, but they are important for studying the causal dynamics and political effects of nonviolent tactics, which do not necessarily discriminate between larger and smaller campaigns.

Nevertheless, NAVCO 2.0 is better for this research project than its alternatives. The deliberate inclusion of both violent and nonviolent campaigns, with the 1,000 annual participants criteria, allow violent and nonviolent campaigns in the data set to be analytically comparable. This makes NAVCO 2.0 more useful than, for instance, Swarthmore’s Global Nonviolent Action Database (GNAD), which only describes nonviolent campaigns. It is also an improvement upon the Minorities at
Risk—Organizational Behavior (MAROB) data set, which includes much of the same variables, but is limited to campaigns in the Middle East (Chenoweth and Lewis 2013). I choose NAVCO 2.0 because its extensive number of comparable cases across space and time maximize the ability to discover general patterns.
CHAPTER THREE

NONVIOLENT RESISTANCE AND DEMOCRACY: A CAUSAL LINK?

In Chapter 8 of *Why Civil Resistance Works*, Chenoweth and Stephan (2011) estimate the effect of nonviolent resistance on post-conflict democratization. Using all the nonviolent and violent campaigns in their data set, over 300 cases between 1900 and 2006, they estimate a logistic regression with existence of democracy (Polity $\geq 6$) five years after the end of conflict as the dependent variable and tactical choice, logged duration, and the Polity score one year before the end of conflict as independent variables. The marginal effect of nonviolent tactics, as compared to violent tactics, is 0.45 and statistically significant at the 0.01 significance level.

Additionally, Chenoweth and Stephan (2011) include in their online Appendix an endogeneity test. They estimate a two-stage instrumental variable model using instruments for both the dependent and main independent variable, finding that the instruments retain a positive, statistically significant relationship. They conclude that they are “more confident” that the two are not endogenous, and that “nonviolent resistance can be an active cause of democracy” (Chenoweth 2011, 29-30).

What can we make of these findings? One reaction is to notice that the marginal effect is substantively large. In civil conflict, the choice to adopt nonviolent tactics is associated with a nearly 0.50 increase in the probability of democracy, holding other variables at their means. Put another way, adopting nonviolent tactics,
rather than violent tactics, nearly implies that post-conflict democracy is more likely to result than not.

Yet two characteristics of the research design limit our ability to draw conclusions about the causal relationship between nonviolence and democracy. The first has to do with the model and the second has to do with the nature of the data. Beside the main independent variable, the model only controls for two other variables, conflict duration and the Polity score one year before the end of conflict. Knowing that democratization has many structural determinants, one natural question is to wonder whether the observed correlation between nonviolent tactics and democratization is driven by a third, perhaps structural, factor. I call this potential explanation the spuriousness hypothesis. The observed relationship might actually be driven by a confounding variable.

A second, related concern relates to the nature of the data. As the data is observational, rather than experimental, assignment into the nonviolent group, as opposed to the violent group, is not random. This means that the treatment may not be independent from the outcome—one concern is that the relationship between nonviolent resistance and democracy could be endogenous. I call this the endogeneity hypothesis. This hypothesis posits that nonviolent resistance could also be a result of democratization. As with spuriousness, this endogeneity concern weakens the causal argument.

The concern remains, even given Chenoweth and Stephan’s (2011) endogeneity test. Although the results of this test lead them to argue that nonviolent resistance can be an “active cause” of democratization (2011), there is no reported
check or diagnostic for the quality of the instrumental variables. It is difficult to evaluate the results without knowing how well the instruments are correlated with the variables they seek to instrument. Furthermore, even if nonviolent resistance and democracy are known to be independent events, the two-stage instrumental variable model does not account for the potential systematic differences between violent and nonviolent campaigns. As a consequence, the evidence for causality is still inconclusive.

In this chapter, I attempt to better account for concerns about spuriousness and endogeneity using advanced matching techniques (Rubin 2001). I use propensity score matching and genetic matching to create a sample that achieves balance across a wide range of structural covariates, pairing essentially identical violent and nonviolent campaigns. Campaigns in the sample are effectively randomly assigned to violent or nonviolent tactics. This approach has two advantages. First, it allows me to nonparametrically condition my statistical estimates on these structural covariates. Second, after I adopt a minimum assumption, it allows me to produce unbiased estimates of the nonviolent treatment effect on the likelihood of democracy.

In short, I extend Chenoweth and Stephan’s (2011) important work, comparing their estimates with estimates calculated from a balanced, effectively random sample. I find no evidence for the spuriousness hypothesis or the endogeneity hypothesis. Instead, my estimates are essentially identical to Chenoweth and Stephan’s (2011) estimates. Although the results are consistent with Chenoweth and Stephan’s work, my findings provide an important robustness test that furthers our
confidence in the causal claims about the link between nonviolent resistance and democracy.

**CAUSAL INFERENCE USING OBSERVATIONAL DATA**

Causal inference using observational, rather than experimental, studies is more difficult, because assignment into treatments is nonrandom. As a consequence, even in large samples, units in one treatment may systematically differ from units in another (Rosenbaum and Rubin 1983). One way to improve the research design of a nonexperimental study is using matching methods (Austin 2011; Stuart 2010; Ho et al. 2007; Rosenbaum and Rubin 1985, 1983). Matching methods can improve causal inference using observational data for four reasons.

First, matching is designed to balance the covariate distributions of the treatment and control groups, so that each unit in the treatment group is paired with an essentially identical unit in the control group. This results in a sample that is more carefully designed than in a typical observational study, as the treatment and control groups are not systematically different from each other on observed covariates. Elizabeth Stuart writes, “precisely because nonexperimental studies do not have the benefit of randomization, they require even more careful design” (Stuart 2010, 1).

Second, matching complements traditional parametric analysis—such as ordinary least-squared or logistic regression analysis—by reducing model dependence. It is a method of preprocessing, allowing researchers to account for control variables and observable confounders before specifying a parametric model.
This reduces the number of parametric modeling adjustments, making causal estimates less dependent on specific modeling choices (Ho et al. 2007).

Third, matching reduces the potential for bias. Misspecifying the functional form of potential confounders in a parametric model can introduce bias. Because matching is nonparametric, it reduces the potential for bias. It can also reduce the variance and mean squared error of causal estimates (Ho et al. 2007).

Finally, Stuart (2010) suggests that matching is advantageous compared to other techniques that reduce endogeneity, such as instrumental variable or structural equation modeling, because diagnosing the performance of matching methods is relatively straightforward. Balance and sensitivity testing make the quality of the causal estimate more transparent.

I make two assumptions necessary for causal inference using observational data. The treatment assignment of one unit must be assumed to be unrelated to the potential outcomes of other units, which is also known as the stable unit treatment value assumption (SUTVA). Furthermore, I invoke strong ignorability: conditional on observed covariates, the treatment assignment is independent of the outcome. Although matching does not allow this assumption to be relaxed, matching on a wide range of potential confounders reduces the likelihood that there are unobserved confounders. Notice that SUTVA and strong ignorability are not radical assumptions: both are also assumed in classical regression techniques—the former implicitly and the second explicitly (Stuart 2010).

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5 Strong ignorability is also known as no-omitted-variable bias, exogeneity, or unconfoundedness.
MATCHING

I attempt to create a balanced sample using two multivariate matching methods: propensity score matching and genetic matching. In the first and older approach, I use a logistic regression to estimate the propensity score, which is the probability of assignment into the nonviolent treatment group (Rosenbaum and Rubin 1985, 1983). The propensity score is used as a distance metric in matching one essentially identical control observation to each treatment observation (or vice versa). In the second approach, I use a genetic evolutionary search algorithm developed in R called GenMatch to nonparametrically weight each observed covariate and automatically maximize its balance over the treatment and control groups. The properties of this more generalized method does not require knowing the propensity score, and it is known to reduce the bias and mean squared error of causal estimations and produce better balance on observed covariates (Diamond and Sekhon 2013).

Table 3.1 lists the 15 potential confounders with which the propensity score was calculated and the genetic optimization was performed. Each variable is a structural “pre-treatment” condition that may affect assignment of units into nonviolent or violent treatment groups (Cunningham 2013; Chenoweth and Lewis 2013; Fearon and Laitin 2003). In other words, these variables may increase the likelihood of choosing one tactic versus the other. For instance, I include variables from Fearon and Laitin’s (2003) seminal study on the determinants of civil war, in order to condition for the structural features that may predict violent conflict.

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For an example of a recent political science publication that uses GenMatch, see Mayer (2011). For further discussion of the genetic algorithm and its benefits, see Diamond and Sekhon (2013) and Sekhon (2011).
A few notes need to be made about variable inclusion. First, Stuart (2010) argues that it is far less costly to include irrelevant variables, those unassociated with treatment assignment, because the bias resulting from omitted variables is greater than the increased variance that irrelevant variables may create. Stuart recommends including, rather than excluding, potentially irrelevant variables. Indeed, some studies match their samples on more than 100 covariates (e.g., Mayer 2011).

<table>
<thead>
<tr>
<th>Variable</th>
<th>Type</th>
<th>Description</th>
<th>Source</th>
</tr>
</thead>
<tbody>
<tr>
<td>GDP per capita</td>
<td>Quantitative</td>
<td>Chain-weighted based on PPP in constant 2005 international dollars</td>
<td>Penn World Table (2012)</td>
</tr>
<tr>
<td>Polity (t-1)</td>
<td>Quantitative</td>
<td>21-point POLITY IV regime characteristics</td>
<td>Marshall, et al. (2002)</td>
</tr>
<tr>
<td>Mountainous terrain</td>
<td>Quantitative</td>
<td>Proportion of state territory that is mountainous, logged</td>
<td>Fearon and Laitin (2003)</td>
</tr>
<tr>
<td>Ethnic fractionalization</td>
<td>Quantitative</td>
<td>Probability that any two people in state are from different ethnic groups</td>
<td>Fearon and Laitin (2003)</td>
</tr>
<tr>
<td>Religious fractionalization</td>
<td>Quantitative</td>
<td>Probability that any two people in state are from different religious groups</td>
<td>Fearon and Laitin (2003)</td>
</tr>
<tr>
<td>New state</td>
<td>Categorical</td>
<td>1 = Newly independent state at conflict end, or 1 year prior; 0 otherwise</td>
<td>Fearon and Laitin (2003)</td>
</tr>
<tr>
<td>Political instability</td>
<td>Categorical</td>
<td>1 = 3+ change in Polity score in 3 years prior to conflict end; 0 = otherwise</td>
<td>Fearon and Laitin (2003)</td>
</tr>
<tr>
<td>Region</td>
<td>Categorical</td>
<td>Region of the world, Western democracies omitted</td>
<td>Fearon and Laitin (2003)</td>
</tr>
<tr>
<td>Oil exporter</td>
<td>Categorical</td>
<td>1 = Over 30 percent of foreign revenue is oil exports; 0 = otherwise</td>
<td>Fearon and Laitin (2003)</td>
</tr>
</tbody>
</table>
Second, notice that the strong ignorability assumption relies on these 15 covariates. It assumes that assignment into violent and nonviolent groups is independent from the potential outcome of democracy, conditional on the 15 variables listed in Table 3.1. By including these variables, strong ignorability is improved upon previous studies, which have accounted for few (if any) potential confounders (Chenoweth and Stephan 2011; Karatnycky and Ackerman 2005). At the end of this chapter, I use sensitivity analysis to examine this key identifying assumption more closely.

Finally, although matching is generally a nonparametric way to preprocess data, propensity score matching assumes correctly specified or known propensity scores, while genetic matching is completely nonparametric. Both reduce model dependence, but genetic optimization matches units with fewer assumptions (Sekhon 2011). Nevertheless, research shows that misspecifying the propensity score model does not introduce as much bias as misspecifying outcome models (Zhao 2004; Drake 1993).

In both matching scenarios, I use one-to-one nearest neighbor matching with replacement. Both one-to-one matching and matching with replacement may increase variance while reducing bias (Smith 1997). First, I match one-to-one, because although the size of my control group—violent campaigns—is twice as large as my treatment group, it is still not large enough to produce several good matches for each nonviolent campaign. Second, I match with replacement in order to reduce bias. In
matching with replacement, researchers need to monitor the number of times control units are matched with treatment variables (Stuart 2010). In both matched samples, most control units are matched only one or two times, so estimates are unlikely to be driven by a few, overrepresented control cases. I correct for non-independence of repeat control units through frequency weighting, automatically generated in most matching procedures.

The full raw data include 211 observations, which include the conflicts that do not have missing values for either the dependent variable or the main independent variable. Propensity score matching yields a carefully constructed sample of 160 observations, 80 campaigns in each treatment group. Genetic matching yields 144 observations, 72 campaigns in each treatment group. In the following section, I analyze the covariate distribution balance on each sample.

**BALANCE DIAGNOSTICS**

If matching produces good balance, there is evidence to show that differences in post-conflict democratization between the violent and nonviolent campaigns are not systematically the result of differences in observed covariates. Assuming SUTVA and strong ignorability, we can be more confident that differences in post-conflict democratization are caused by the use of nonviolent tactics.

I diagnose the covariate distribution balance of the full sample, the propensity score matched sample, and the genetic matched sample, examining whether balance is achieved over the 15 potential confounders listed in Table 3.1. In line with best practice, I also check for balance on higher-order transformations, by including a
squared term for each quantitative covariate (Sekhon 2011; Stuart 2010). In total, I conduct balance diagnostics on 20 covariate terms, using four criteria to determine the covariate balance of each sample: the standardized difference of means, the variance ratio, empirical quantile-quantile plots, and the minimum bootstrapped Kolmogorov-Smirnov p-values. These statistics are generated using the R package \texttt{GenMatch}.

\textit{Standardized difference of means.} Also known as standardized bias, this measurement was first used by Paul Rosenbaum and Donald Rubin (2001). It is defined for each covariate as follows:

\[
\text{Standardized Difference of Means} = \frac{\bar{x}_t - \bar{x}_c}{\sigma_t}
\]

where \(\bar{x}_t\) is the covariate mean for the treatment, \(\bar{x}_c\) is the covariate mean for the control, and \(\sigma_t\) is the standard deviation of the full treated group. For accurate estimates, Rubin (2001) argues that the absolute standardized difference of means should be less than 25 percent.

\textit{Variance Ratio.} Rubin (2001) also argues for another covariate balance measure: the variance ratio, or the ratio between the variances of the treatment and control groups. As a guideline, Rubin says the variance ratio of each covariate should be between 0.5 and 2.

\textit{Empirical Quantile-Quantile Plot.} Stuart (2010) defines balance as “the similarity of the empirical distributions of the full set of covariates in the matched treated and control groups.” Under this definition, a natural graphical balance

---

\footnote{I include more extensive covariate balance statistics on each in the Appendix. For each of the 20 covariate terms, I detail the treatment mean, the standardized difference, the variance ratio, the T-test p-value, and the bootstrapped KS p-value.}
diagnostic is the empirical quantile-quantile plot, which compares two distributions. Using empirical-QQ plots, I display the distributions of the treatment and control groups for each covariate.

*Minimum bootstrapped Kolmogorov-Smirnov p-values.* For each covariate, I calculate the Kolmogorov-Smirnov statistic using bootstrapping methods, resampling using 2,000 simulations.\(^8\) Sekhon (2011) encourages researchers to use bootstrapped KS tests to compare the means of matched covariates. The bootstrapped KS test is better than the classic t-test, because it “provides correct coverage even when there are point masses in the distributions being compared” (Sekhon 2011). In this context, greater \(p\)-values are desirable, because they indicate higher that the means between treatment groups are not significantly different. The minimum \(p\)-value is, therefore, indicates whether the means of covariates significantly differ between treatment groups.

**Full Raw Data**

Balance diagnostics reveal that the raw data Chenoweth and Stephan (2011) use to estimate the causal effect of nonviolent tactics on post-conflict democratization is unbalanced. In Figure 3.1, 14 of the 20 covariates fall outside Rubin’s (2001) guidelines for standardized difference of means; their absolute values are higher than 20 percent. These covariates are not adjusted for in the logistic regression either, which demonstrates that the nonviolent and violent campaigns compared in the logistic regression systematically differ along, at least, these 14 covariates, posing

---

\(^8\) For publication quality \(p\)-values, Sekhon (2011) recommends calculating the bootstrapped Kolmogorov-Smirnov statistic using at least 500 bootstrap samples.
problems for causal inference. In Figure 3.2, three covariates fall outside Rubin’s guidelines for the variance ratio. In Figure 3.3, some covariates—namely, population, religious fractionalization, and religious fractionalization, squared—show good balance. However, the remaining covariates show imbalance. Finally, the minimum bootstrapped Kolmogorov-Smirnov $p$-value for the full raw data is 0.000, suggesting that the treatment group shows systematically different means on at least one covariate. Overall, these balance diagnostics indicate that their model estimates may be systematically biased.

**FIGURE 3.1** Standardized Difference for Full Data
**FIGURE 3.2** Variance Ratio for Full Data
FIGURE 3.3 Empirical QQ-plots on Full Data

**Propensity Score Matching**

Now I evaluate the covariate distribution balance for the propensity score matched sample. In Figure 3.4, only four covariates fall outside Rubin’s (2001) suggested range, a marked improvement from the raw data. Figure 3.5 shows that only two covariates fall outside Rubin’s variance ratio guidelines. In Figure 3.6, most covariate distributions show improvements in balance compared to the raw data. However, the fractionalization variables—ethnic fractionalization, religious fractionalization, and their squared terms—are particularly imbalanced. Finally, the minimum bootstrapped
KS $p$-value is 0.002, suggesting that there still exist systematic differences between the treatment group means along at least one covariate. The first two measures suggest that balance on observable covariates between treatment groups has improved drastically over the full raw data, but the second two measures reveal that balance has not been achieved. Consequently, making credible causal inferences using the propensity score matched sample remains difficult.

**FIGURE 3.4** Standardized Difference for Propensity Score Matching
FIGURE 3.5 Variance Ratio for Propensity Score Matching
Finally, I examine the covariate balance for the genetic matched sample. Figure 3.7 shows remarkably well-behaved standardized differences. After matching, every covariate except for one is closely dispersed around 0. The misbehaving covariate only slightly exceeds Rubin’s 20 percent mark. Figure 3.8 shows that the variance ratio of every covariate except for one falls between 0.5 and 2. The empirical QQ-plot in Figure 3.9 shows significantly balanced covariate distributions. Only two covariates could be much better balanced: POLITY IV and the squared term of
religious fractionalization. Finally, the minimum bootstrapped $p$-value is 0.110. The treatment means along the covariate for which we are least confident are not statistically different at the 0.10 significance level. Compared to previous samples, the genetic matched sample achieves balance. This means that every nonviolent campaign is successfully matched with an essentially identical campaign that is violent. As these balance diagnostics suggest, no observed systematic differences between the matched violent and nonviolent campaigns exist. As a result, causal inference using the genetic matched sample yields more reliable estimates.

**FIGURE 3.7** Standardized Difference for Genetic Matching
FIGURE 3.8 Variance Ratio for Genetic Matching
I run Chenoweth and Stephan’s (2011) logistic regression model on each of the aforementioned samples. The genetic matched sample is the only sample upon which excellent balance is achieved, which means that the causal estimates from the third column are theoretically unbiased, while the causal estimates from the first and second columns are biased by systematic differences in the covariate distributions of violent and nonviolent campaigns. However, as Table 3.2 shows, the estimates between columns are not actually very different. After accounting for statistical
uncertainty, the estimates are essentially the same. The marginal effect of adopting nonviolent tactics is positive, statistically significant, and is estimated to increase the probability of democracy by a value between 0.42 (0.13) and 0.48 (0.07), conditioning on the 15 covariates in Table 3.1 and holding Polity and conflict duration at their means.9

<table>
<thead>
<tr>
<th>TABLE 3.2 Logistic Regression Estimates on Post-Conflict Democracy</th>
</tr>
</thead>
<tbody>
<tr>
<td>Model</td>
</tr>
<tr>
<td>Treatment Estimate</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Polity Score (t-1)</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Conflict Duration</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Unique N</td>
</tr>
<tr>
<td>LR $\chi^2$</td>
</tr>
<tr>
<td>Prob $&gt; \chi^2$</td>
</tr>
<tr>
<td>Pseudo $R^2$</td>
</tr>
</tbody>
</table>

Legend: * p<.10; ** p<.05; *** p<.01

Note: Rather than log-odds or odds ratios, marginal effects are reported. The estimates denote the increase in probability associated with a one-unit increase in the independent variable. The constant is naturally omitted, because it has no marginal effect.

Alternatively, the same estimates can be interpreted as odds ratio. The odds of post-conflict democratization are between 11.9 [4.30, 38.22] and 20.4 [8.09, 59.10] times higher for nonviolent campaigns than for violent ones.11 Despite covariate imbalance in the raw sample, the original estimate turns out to be fairly accurate. The

---

9 Standard errors reported in parentheses.
10 These are not Chenoweth and Stephan’s (2011) exact estimates. They use data between 1900 and 2006; I use data between 1945 and 2006.
11 95 percent confidence interval reported within square brackets.
only substantive difference between the results from genetic matched sample and the
original estimates is that conflict duration, a control variable, loses statistical
significance.

Figure 3.10 graphs the predicted probability of democracy as a function of
Polity score and tactical choice. As illustrated, the Polity score and tactical choice are
additive effects. The higher the Polity score, the higher the predicted probability of
democracy. Within any given Polity score, nonviolent campaigns have a consistently
higher predicted probability of democracy than violent campaigns. The effect is more
pronounced when the Polity score is closer to 0 and less pronounced at the ends of the
spectrum, namely, under relatively durable authoritarian and democratic regimes.

FIGURE 3.10 Predicted Probability of Democracy\textsuperscript{12}

\textsuperscript{12} 95 percent confidence interval bars shown.
SENSITIVITY ANALYSIS AND ROBUSTNESS TESTING

In this section, I conduct sensitivity analysis and additional robustness testing. The argument that the treatment estimate is causal rests on strong ignorability, the assumption that there are no unobserved confounders or omitted variables. I perform a Rosenbaum sensitivity test to investigate how sensitive my findings are to violations of the key assumption (Rosenbaum 1987). The quantity of interest estimated by the Rosenbaum sensitivity test is the sensitivity parameter $\Gamma$, which measures “the degree of departure from random assignment of the treatment” before the result changes (Keele 2010). In other words, $\Gamma$ measures the amount of unobserved confounding necessary for my findings to be become null. If the sensitivity parameter is estimated to be $\Gamma = 2$, then a hypothetical unobserved covariate would have to change the odds of assignment into the treatment by a factor of 2 before changing the result. Higher $\Gamma$ values correspond to less sensitive results, while lower $\Gamma$ values correspond with more sensitive results.

Using the R package *rbounds*, I conduct the Rosenbaum Sensitivity Test for Wilcoxon Signed Rank $P$-value (Keele 2010). The $p$-value of the nonviolent treatment variable remains below the conventional 0.05 significance level until the sensitivity parameter $\Gamma = 3.5$. This means that my results can tolerate some violations to strong ignorability and still hold true. To be precise, a hypothetical unobserved confounder would need to systematically change the odds of assignment into either the nonviolent or violent treatment groups by a factor of 3.5 before the result is not statistically significant at the 0.05 significance level. I conclude that the presumption of strong ignorability is reasonable. Because the findings holds true even
in the face of moderate hidden bias that results from unobserved confounders, it seems likely that nonviolent resistance causes post-conflict democratization.

Table 3.3 shows the result of my robustness testing. Each column displays the result from re-estimating the logistic regression using the genetic matched sample on a different time frame: two, five, ten, and twenty years after the end of conflict. The treatment effect of nonviolent resistance remains robust up until ten years after the end of conflict. That the estimates decrease over time and eventually lose statistical significance is not too surprising, suggesting that nonviolent resistance has the strongest effect on the country’s regime type immediately after conflict resolution, a weaker effect in the medium-run, and no effect in the long-run. Notice, too, that the number of observations drops over time, as there is less data on long-run political outcomes. Of course, there is no 20-year Polity data for campaigns that occurred recently; their long-run political outcomes not been yet been realized. Overall, robustness testing reveals that the effect of nonviolent resistance on democracy is robust only in the short and medium-run. For the relatively few campaigns whose long-run outcomes can be observed, the statistical effect appears to diminish, although the estimate remains positive.
TABLE 3.3 Comparison of Logistic Regression Estimates on Short- and Long-Term Post-Conflict Democracy

<table>
<thead>
<tr>
<th>Model</th>
<th>2 Years</th>
<th>5 Years</th>
<th>10 Years</th>
<th>20 Years</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment Estimate</td>
<td>0.42 ***</td>
<td>0.42 ***</td>
<td>0.22 **</td>
<td>0.14</td>
</tr>
<tr>
<td></td>
<td>(0.12)</td>
<td>(0.13)</td>
<td>(0.11)</td>
<td>(0.12)</td>
</tr>
<tr>
<td>Polity Score (t-1)</td>
<td>0.03 ***</td>
<td>0.03 ***</td>
<td>0.03 ***</td>
<td>0.03 **</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Conflict Duration</td>
<td>0.05</td>
<td>0.04</td>
<td>-0.05</td>
<td>-0.04</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.06)</td>
<td>(0.07)</td>
</tr>
<tr>
<td>Unique N</td>
<td>144</td>
<td>144</td>
<td>121</td>
<td>82</td>
</tr>
<tr>
<td>LR $\chi^2$</td>
<td>25.82</td>
<td>21.11</td>
<td>24.24</td>
<td>18.02</td>
</tr>
<tr>
<td>Prob $&gt; \chi^2$</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>Pseudo $R^2$</td>
<td>0.15</td>
<td>0.12</td>
<td>0.19</td>
<td>0.20</td>
</tr>
</tbody>
</table>

Legend: * p<.10; ** p<.05; *** p<.01
Note: Rather than log-odds or odds ratios, marginal effects are reported. The estimates denote the increase in probability associated with a one-unit increase in the independent variable. The constant is naturally omitted, because it has no marginal effect.

DISCUSSION

In this chapter, I find no evidence for the spuriousness hypothesis. The relationship between nonviolent resistance and democracy remains robust even accounting for a wide range of structural factors that are correlated with democracy, including economic development, oil, Cold War politics, size, region (Lipset 1959; Dahl and Tufte 1973; Bernhard, Nordstrom, and Reenock 2001; Ross 2001; Dunning 2004, 2008) and the key structural determinants of civil war (Fearon and Laitin 2003). Although the list of structural factors with which matching and balancing are performed is by no means comprehensive, the results of this study reduce the likelihood that the relationship between the nonviolent resistance and democracy is spurious.
One implication is that nonviolent resistance is more likely to produce
democracy than violent resistance in a wide range of circumstances. Even after
matching every nonviolent campaign with a violent campaign that takes place within
a country that is as rich, populated, democratic, and ethnically and religiously
fractionalized, the democratizing effect of nonviolent conflicts is, on average,
sizeable and statistically significant. In addition, I find that this positive, statistically
significant estimate remains robust up until 10 years after the conflict end.

What do we make of the finding that nonviolent resistance is not robust 20
years after conflict end? There are several possible explanations. It is possible that
nonviolent conflict only has a short- and medium- run effect on democracy. This
might suggest that nonviolent conflicts create democratic transitions, but not durable
democracies. Democracies created by nonviolent conflict may tend to revert back to a
less democratic regime form. Second, it may be that in the long-run, violent
campaigns tend to democratize at a comparable rate, which is a reasonable
explanation, as there is a general international trend toward democratization
(Huntington 1991). Finally, it could be that as more countries move into the 20-year
post-conflict category, we start to see a robust, positive relationship.

I also find no evidence for the endogeneity hypothesis. Previous scholars have
used an instrumental variable approach to test this hypothesis (Chenoweth 2011);
however, diagnosing instrumental variables is not straightforward (Stuart 2010, 2).
By creating an effectively random sample of violent and nonviolent resistance
campaigns, while rigorously diagnosing the sample for covariate balance, I
transparently address the concern that the relationship between nonviolent resistance
and democracy is endogenous. I find more evidence that nonviolent campaigns are not emerging within countries that are already democratizing, because nonviolent campaigns in this sample are not systematically different from violent campaigns. My results increase our confidence that nonviolent resistance and democratization are independent events, and that nonviolent resistance makes democratic transition more likely.

Finally, I find that the model in this chapter explains *when* democratization happens, not just where. Most democratization scholars either rely on structural features that change slowly (if at all) or on elite negotiations that are “tautolog[ical]” (Teorell 2010, 21). I find further evidence to suggest a non-elite path to democracy. Democratization occurs in the wake of civil conflict; particularly, democratization is more likely to follow from nonviolent civil conflicts. Figure 3.11 and Figure 3.12 illustrate the model’s predictive power. The first map shows the predicted probability of democracy in every post-conflict country, based on calculations from its latest civil conflict. The second shows whether the same set of countries is (actually) democratic in 2005. Notice that selecting a comparison year is necessary, but arbitrary. It may be unfair to compare cases like Paraguay, whose prediction was calculated from a violent rebellion in 1947, to its country’s Polity rating in 2005.

Nevertheless, the model performs fairly well at predicting whether a country will be democratic in the future. It correctly classifies 67.3 percent of the democracies and non-democracies in 2005, represented in Table 3.4 along the diagonal, using only two variables: tactical choice and pre-existing Polity score. As discussed, some of these predictions are generated from civil conflicts in the 1940s.
Unsurprisingly, most of its incorrect classifications are false negatives, predictions that a country will be non-democratic when (in 2005) it is, in fact, democratic. However, false positives are to some extent expected, as there has been a general trend toward democratization in the past few decades, and as I compare the classifications to the (somewhat arbitrary) Polity score of countries in 2005.

In sum, I find in this chapter that the relationship between nonviolent resistance and democracy is unlikely due to endogeneity or spuriousness. By matching nonviolent campaigns with essentially identical violent campaigns, I estimate the causal effect of nonviolent resistance on the probability of democracy, finding that the effect is robust up until ten years after the end of conflict. I also find that the explanatory model has some predictive power as well, providing some evidence that democratization often happens in the wake of civil conflicts.

| TABLE 3.4 Logistic Regression Classification Evaluation, Confusion Matrix |
|---------------------------------|-----------------|-----------------|
| Legend: Count (Percentage)     | Non-Democracy in 2005 | Democracy in 2005 |
| Non-Democratic Classification   | 34 (34.7)        | 24 (24.5)       |
| Democratic Classification      | 8 (8.1)          | 32 (32.7)       |
FIGURE 3.11 Predicted Probability of Democracy Based on Latest Civil Conflict, 1945 - 2006

Post–Conflict Democracies and Non–Democracies (2005)

FIGURE 3.12 Actual Democracies and Non-Democracies, 2005
LIMITATIONS

As with all studies, there are limitations. The first limitation of this study is related to the data. Although I have tried to use developments in the causal inference literature to extract as much causal information from observational data as possible, research on the causal relationship between nonviolent resistance and democratization is ultimately limited by the fact that it is impossible to collect actual experimental data.

Similarly, the study would have benefited from data on a wider range of civil conflicts (not only maximalist goals, for instance), data on the long-run outcomes of recent campaigns, or data on several other possible structural confounders. Most attainably, the study would benefit from cross-sectional, time-series data that exists on civil conflicts and democracy.

The second limitation relates to the strong ignorability assumption. Even a perfectly balanced sample over 100 covariates could not prove that the relationship between nonviolent tactics and democratization is causal. The possibility always exists in observational research that the key identifying strong ignorability assumption is incorrect. Nevertheless, the results in this study, as well as the associated sensitivity analysis, increase confidence in the claim substantially.
CHAPTER FOUR

HISTORICAL ATTRIBUTION

If the 1986 People Power Movement in the Philippines was primarily violent, rather than nonviolent, would the resulting political regime have been democratic? What about the 1989 pro-democracy movement in Hungary, the 1973-4 Carnation Revolution in Portugal, or the 2003 Rose Revolution in Georgia? More generally, how many transitions to democracy from nondemocratic regimes would not have happened when they did if not for nonviolent resistance? Put another way, how much has the international system actually changed as a result of nonviolent resistance?

Chenoweth and Stephan (2011) argue that their quantitative analysis provides them with enough evidence to answer the question. Their analysis leads them to conclude, “The historical record clearly shows that civil resistance is an enduring force for change in the international system” (2011, 227).

But in order to truly answer the question, it is not enough to estimate the correlation between nonviolent resistance and democracy (Stephan and Chenoweth 2008; Chenoweth and Stephan 2011), or estimate how frequently nonviolent resistance precedes democratization (Karatnycky and Ackerman 2005). It is not enough, as many scholars have observed (Bratton and Van de Walle 1992, 1997; Beissinger 2007), to notice that many of the nonviolent conflicts since World War II were rapidly followed by democratization. In fact, comprehensive data about the phenomenon already exists. Of the 211 cases of violent and nonviolent campaigns
since World War II, 160 (75 percent) countries were unchanged by the conflict, 6 (3 percent) transitioned from a democratic regime to a non-democratic regime, and 45 (21 percent) democratized—that is, they transitioned from a non-democratic regime to a democratic one. Of the 45 democratic transitions, 9 (20 percent) of them followed violent conflicts, while 36 (80 percent) of them followed nonviolent conflicts.13

The question is one of causal attribution. In order to understand the counterfactual outcome—how many of the 36 countries that transitioned to democracy after nonviolent resistance (see Figure 4.1) would not have transitioned to democracy if the corresponding campaign had been violent, rather than nonviolent—one must have an understanding about the causal relationship between nonviolent resistance and democracy. Although the post-World War II era has seen an explosion of nonviolent political movements, many of which have coincided with democratic transitions, understanding the real political impact of nonviolent resistance requires statistical analysis of causal attribution, which previous scholars have not attempted. As a result, previous generalizations about how nonviolent resistance campaigns have affected the international system are backed by limited empirical support.

In this chapter, my goal is to create a quantitative understanding of the past, and in particular, the democratic transitions that followed nonviolent resistance. By applying the statistical estimates generated in the previous chapter, I investigate, more specifically, the 36 democratic transitions that followed from nonviolent resistance campaigns since World War II. Using Teppei Yamamoto’s (2012) causal attribution analysis framework, and two minimal assumptions, I compute a quantity of interest

---

13 As usual, the data come from NAVCO 2.0 (Chenoweth and Lewis 2013) and Polity IV (Marshall, Jaggers, and Gurr 2002).
called the average probability of causal attribution, defined as “the conditional probability of the absence of the outcome in the hypothetical absence of the treatment \((Y_i(0) = 0)\), given the actual presence of both \((X_i = Y_i = 1)\)” (Yamamoto 2012, 4-5). Based on my findings, I argue that if it not for the use of nonviolent tactics of political contention, fewer post-WWII post-conflict transitions to democracy would have occurred, and many that did occur would have occurred later.

**FIGURE 4.1** 36 Worldwide Countries That Experienced Democratic Transition Following Nonviolent Resistance, 1945 – 2006

**QUANTITY OF INTEREST**

In order to calculate the probability of causal attribution, I begin with the potential outcomes framework (Holland 1986; Yamamoto 2012). Let \(X_i \in \{0, 1\}\) be a binary variable that represents a political campaign’s primary tactical choice, violent
\((X_i = 0)\) or nonviolent \((X_i = 1)\) for every campaign \(i\). Let the binary variable 
\(Y_i(0) \in \{0,1\}\) represent the potential outcomes, democracy \((Y_i(0) = 1)\) or non-democracy \((Y_i(0) = 0)\), that may result from the use of violent tactics. Similarly, let 
\(Y_i(1) \in \{0,1\}\) represent the same potential outcomes for nonviolent tactics. Again, 
nonviolent conflict can lead to either democracy \((Y_i(1) = 1)\) or non-democracy 
\((Y_i(1) = 0)\). The goal is to calculate the probability of causal attribution, defined as 
\[ p_A = \Pr(Y_i(0) = 0 \mid X_i = 1, Y_i = 1) \]
by previous scholars (Pearl 1999; Yamamoto 2012). As discussed earlier, this 
quantity measures, for any given campaign, the likelihood of the counterfactual in 
which democratization would not have occurred in the absence of nonviolent tactics, 
given that democracy and nonviolent tactics actually did occur. Simply put, it 
measures the proportion of democratization cases can be causally attributed to the 
civil conflict’s choice of nonviolent tactics.

Judea Pearl (1999) proves that it is possible to partially and point identify 
the probability of causal attribution \(p_A\), depending on the set of assumptions researchers 
are willing to adopt. I make two assumptions about the relationship between 
nonviolent tactics and post-conflict democracy.

Assumption 1: Strong Ignorability 
\[ \{Y_i(1), Y_i(0)\} \perp X_i \mid W_i \]

First, I make the same strong ignorability assumption that I made in the 
previous chapter. Conditioning on a set of potential confounders \(W_i\)—which includes 
the Polity score before the end of conflict, the conflict duration, and the fifteen 
covariates upon which balance was achieved in the previous chapter—I assume that
the tactical choice between nonviolent resistance and violent resistance is effectively random. In the previous chapter, I conduct sensitivity analysis and find that the statistical estimate is fairly insensitive to violations of strong ignorability. Although this assumption is still unproved, I argue, as I do in the previous chapter, that it is a reasonable assumption.

Assumption 2: Monotonicity

For all \( i, Y_i(0) \leq Y_i(1) \)

Second, I make the new assumption of monotonicity. I assume that the primary use of nonviolent tactics, as opposed to violent tactics, cannot negatively affect the likelihood of democratization after conflict. In other words, nonviolent tactics either help or do not affect democratization—it cannot hurt it. This, too, is a reasonable assumption. None of the 100 nonviolent campaigns in NAVCO 2.0 produced a regime shift from democracy to non-democracy.\(^\text{14}\) Nevertheless, I account for the possibility that this assumption is incorrect by relaxing this assumption later. Even without assuming monotonicity, it is possible to partially identify \( p_A \), producing sharp upper and lower bounds within which the true \( p_A \) is located.\(^\text{15}\)

Under the two assumptions of strong ignorability and monotonicity, Pearl (1999) proves that the probability of causal attribution can be fully identified using the following equation.

\[
P_A = \frac{\Pr(Y_i = 1 \mid X_i = 1) - \Pr(Y_i = 1 \mid X_i = 0)}{\Pr(Y_i = 1 \mid X_i = 1)}
\]

\(^{14}\) As usual, the resulting Polity score is measured five years after the resolution of conflict.

\(^{15}\) The equation to partially identify the probability of causal attribution given strong ignorability but not monotonicity is included in the Appendix.
In this study, this equation identifies the probability of causal attribution $p_A$ that democratization would not have occurred when it did had the campaign been violent instead of nonviolent. I discuss how I estimate each term below.

**ESTIMATION**

In order to calculate $p_A$, I statistically estimate $\Pr(Y_i = 1 \mid X_i = 1)$ and $\Pr(Y_i = 1 \mid X_i = 0)$, while conditioning on the set of confounders $W_i$. In particular, I use the logistic regression model estimated in the previous chapter, whose results are conditioned via genetic matching on the fifteen potential confounders and the two controls included in the model. Using this model, I calculate the average predicted probability of democracy given nonviolent tactics $\Pr(Y_i = 1 \mid X_i = 1)$ and given violent tactics $\Pr(Y_i = 1 \mid X_i = 1)$. By estimating the predicted probabilities in a variety of circumstances, I calculate the probability of causal attribution in general, across Polity scores, and for each of the 36 countries.

**RESULTS**

Assuming strong ignorability and monotonicity, the probability of causal attribution $p_A$, calculated as an average across the 36 aforementioned cases, is 0.741. For any given country that experienced both nonviolent conflict and democratization, the chance that the democratic transition would not have occurred in the absence of nonviolent conflict is 74.1 percent. Relaxing the assumption of monotonicity produces sharp partial identification bounds with a lower limit of 0.741 and an upper limit of 1.00. In other words, the true probability is estimated to be at least 0.741.
Next, I calculate more accurate estimates of $p_A$ for any given country. I calculate an average $p_A$ for every level of the Polity score. Figure 4.2 presents the point estimates of $p_A$ (dark dots) along the Polity scale. I also relax Assumption 2, providing the partial identification bounds of $p_A$ (dark vertical lines).

As illustrated, the probability of causal attribution monotonically decreases along the Polity scale, with sharper decreases after nonnegative Polity scores. As a trend, the more democratic the country before the resolution of conflict, the less likely democratization can be causally attributed to the use of nonviolent tactics. For example, if a post-conflict democracy is fully authoritarian (-10) at the end of conflict, the estimated $p_A$ is relatively high, 0.853. Nearly all of the countries examined in this study had a Polity score less than -5 before the end of conflict.

Figure 4.2 Proportion of Democratic Transitions Attributable to Nonviolent Tactics, 1945 – 2006
At the opposite end of the scale, post-conflict democracies that are fully
democratic at the end of conflict have an estimated probability of 0.263. 26 times out
of 100, a democracy that remains democratic after nonviolent conflict can be causally
attributed to the use of nonviolent tactics. The downward trend of $p_A$ given increasing
Polity scores supports intuition: the more democratic institutions that exist in a
country, the more likely democratization happens for a reason other than the use of
nonviolent tactics.

Finally, I use the Polity score of each of the 36 aforementioned countries to
calculate their $p_A$. Figure 4.3 displays the results. The lighter the country, the more
democratic the country was before the resolution of nonviolent conflict and the more
likely country would have experienced democratic transition even if it did not
experience nonviolent conflict. The darker the country, the more likely the country
would not have democratized if not for the use of nonviolent tactics. But every
country except the 2003 Rose Revolution in Georgia has a $p_A$ greater than 0.50, so
even countries in this set with relatively low $p_A$ probably experienced
democratization as a result of nonviolent resistance.
DISCUSSION

If the 1986 People Power Movement in the Philippines, the 1989 pro-democracy movement in Hungary, the 1973-4 Carnation Revolution in Portugal, or the 2003 Rose Revolution in Georgia were primarily violent campaigns, rather than nonviolent ones, would the resulting political regime have been democratic? Using Teppei Yamamoto’s (2012) causal attribution analysis framework, statistical estimates from the previous chapter, and one additional assumption, I find that, if not for nonviolent resistance, the Philippines, Hungary, and Portugal probably would not have democratized when they did. On the other hand, Georgia probably would have democratized anyway. Respectively, I calculate that the probability of causal attribution associated with each country is 0.811, 0.731, 0.845, and 0.474.
If we adopt the probability of causal attribution of 0.50 as a threshold to separate between countries that probably democratized as a result of nonviolent conflict and countries that probably would have democratized anyway, only Georgia falls in the latter category. As Table 4.1 shows, the overwhelming majority of democratic transitions after nonviolent conflict were more likely to be caused by nonviolent resistance than by other reasons. In fact, the probability of causal attribution is greater than 0.80 in 22 (61 percent) out of 36 cases. On the other hand, the probability of causal attribution never exceeds 0.90.

These findings broadly support claims made by previous scholars that nonviolent resistance has significantly transformed political regimes in the international system (Karatnycky and Ackerman 2005; Chenoweth and Stephan

<table>
<thead>
<tr>
<th>$p_A$</th>
<th>Frequency</th>
<th>Percent</th>
</tr>
</thead>
<tbody>
<tr>
<td>Less than 0.50</td>
<td>1</td>
<td>2.78</td>
</tr>
<tr>
<td>0.51 – 0.60</td>
<td>0</td>
<td>0.00</td>
</tr>
<tr>
<td>0.61 – 0.70</td>
<td>4</td>
<td>11.11</td>
</tr>
<tr>
<td>0.71 – 0.80</td>
<td>9</td>
<td>25.00</td>
</tr>
<tr>
<td>0.81 – 0.90</td>
<td>22</td>
<td>61.11</td>
</tr>
<tr>
<td>0.91 – 1.00</td>
<td>0</td>
<td>0.00</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td>36</td>
<td>100.00</td>
</tr>
</tbody>
</table>

**NOTE:** The Polity score is measured one year before the resolution of the nonviolent conflict.
2011). In particular, the findings also individually corroborate the conclusion of many comparative political scientists that nonviolent resistance was important for creating democracy in countries such as the Philippines, Thailand, Nepal, and Brazil (e.g., Stepan 1989; Schock 2005).

Furthermore, the generally high probability of causal attribution associated with nonviolent campaigns reflects, most importantly, that nonviolent conflicts tend to target durable authoritarian regimes. I find that the more authoritarian a country is before post-nonviolent conflict democratization, the more likely democratization is a result of nonviolent conflict, rather than something else. This corroborates the conclusion of previous scholars that nonviolent conflicts may be a uniquely potent weapon against authoritarian regimes (Sharp 2008, 4). As my findings show, many of post-conflict democracies that followed nonviolent campaigns can be causally attributed to the presence of nonviolent resistance (and the relative absence of violent resistance).

LIMITATIONS

In addition to the limitations inherited from the previous chapter, this study is further limited by an inability to test the new assumption I adopt: monotonicity. Although there are no cases in which nonviolent conflict caused a democratic country to transition away from democracy, it is impossible to know whether nonviolent conflict may have features that hurt the prospects for democratization in some cases, but not others. Nevertheless, I also relax this assumption and find that the resulting partial identification bounds reveal the same empirical trend as the point estimates.
How and why do nonviolent campaigns lead to democracy? Chenoweth and Stephan (2011, 207) propose three potential causal mechanisms: through the large participation of citizens, through the memory of nonviolent victory, and through the creation of democratically oriented parallel institutions. I briefly review each explanation and propose a way to test their relative effects.

The first hypothesis, what I call the *mobilization hypothesis*, relies on Chenoweth and Stephan’s (2011) finding that nonviolent campaigns have a participation advantage relative to violent campaigns. Because they have low physical, informational, moral, and commitment barriers, nonviolent campaigns tend to mobilize mass participation. For instance, the 1983-1986 People Power movement in the Philippines and the 2004 Orange Revolution in Ukraine both drew millions of civilians. This participation, they argue, may also be linked to more democratic futures. As they explain, participation “encourages the development of democratic skills and fosters expectations of accountable governance, both of which are less likely when transitions are driven by opposition violence” (Chenoweth and Stephan 2011, 207). In other words, democratic participation creates the skills and expectations that make democracy more likely.
The second hypothesis, the *success hypothesis*, is related to Chenoweth and Stephan’s (2011) central finding, that nonviolent resistance campaigns are more likely than violent campaigns to achieve their stated campaign goals. Regardless of the goal, nonviolent campaigns are likelier to succeed. (Of course, sometimes success means regime change, though even then, the removal of one regime does not necessarily mean the installation of democracy.) In this explanation, nonviolent campaigns are likelier to succeed, and when they succeed, likelier to create regimes that “will be nonviolent in its relationship with its constitutions” (2011, 208). In addition, it creates a collective national memory around the fact that civilians can keep their governments accountable without resorting to violence.

Finally, the *democratic institutions hypothesis* points to a particular phenomenon that often takes place during civil conflicts. In many nonviolent campaigns, like the Otpor movement in Serbia or the anti-apartheid campaign in South Africa, alternative democratic institutions that ran parallel to the government were created. Often, these institutions are more consensual or democratic than authoritarian institutions that “are more likely to operate by means of secrecy and martial values” (Chenoweth and Stephan 2011, 208). As a consequence, the establishment of these institutions may directly aid in the transition to democracy.

No research has empirically tested any of the proposed causal mechanisms. This chapter attempts to decompose the causal effect of nonviolent resistance on democracy into direct and indirect pathways through which nonviolent tactics work. By using the relatively new causal mediation analysis framework, I test each of Chenoweth and Stephan’s (2011) three explanations as independent mediators,
finding that empirical evidence only supports one of them. Nonviolent resistance does not appear to transmit its causal effect on democratization through mobilization or democratic institutions. However, some of the democratizing effect is transmitted through the success mechanism, but it is moderated by insurgents’ tactical choice.

![Path Diagram of Hypothesized Causal Mechanisms](image)

**FIGURE 5.1** Path Diagram of Hypothesized Causal Mechanisms

**UNCOVERING MECHANISMS USING CAUSAL MEDIATION ANALYSIS**

In order to test the hypothesized causal mechanisms, I use the formal causal mediation analysis framework, recently developed and advanced by Kosuke Imai, Luke Keele, and Teppei Yamamoto (2013; 2013; 2011; 2010; 2010). Unlike traditional methods, which rely on the linearity and no-interaction assumption, their framework provides a flexible structure under which I can reliably estimate the effects of the hypothesized causal mechanisms.

Most commonly, social scientists estimate causal mechanisms under the framework of linear structural equation modeling (LSEM), using methods such as difference-in-coefficients or products-of-coefficients (Baron and Kenny 1986). These
methods are based on models that are necessarily linear, and although many social
scientists incorrectly apply them to nonlinear cases, Imai et al. (2011) demonstrate
that doing so produces biased estimates. Since this research project relies on
nonlinear systems, such as the logit model, using LSEM to estimate causal
mechanisms would be inappropriate.

Furthermore, LSEM assumes no-interaction; it imposes the restriction that the
effect of the causal mechanism cannot depend on the baseline treatment status (Pearl
2012). In this research project, the baseline treatment is nonviolent resistance, while
the control treatment is violent resistance. Assuming that the effect of various
mechanisms does not depend on whether rebels use nonviolent resistance is
restrictive. I later demonstrate that this can also result in misleading findings.

The causal mediation analysis framework proposed by Imai et al. (2011)
provides unbiased estimates in nonlinear systems, while allowing researchers to relax
the no-interaction assumption. In fact, because it is nonparametrically identified and
requires minimal assumptions, causal mediation analysis is generalizable to an
extraordinary range of statistical estimators, including nonlinear, nonparametric, or
semi-parametric models, and can accommodate multiple mechanisms, moderated
mediation, and treatment noncompliance (Yamamoto 2013; Imai and Yamamoto
2013; Imai et al. 2011; Imai, Keele, and Yamamoto 2010; Imai, Keele, and Tingley
2010). Increasingly, political scientists are using this causal mediation analysis
framework in their research (Brader, Valentino, and Suhay 2008; Tomz and Weeks
One final advantage is that causal mediation analysis comes with straightforward sensitivity analysis, allowing researchers to analyze the sensitivity of results to the key identifying assumption: sequential ignorability. Although the assumption is fundamentally untestable, even in randomized experiments, it is required to estimate the effect of causal mechanisms (Imai et al. 2011). Rather than invoke an assumption without further discussion, the sensitivity analysis provided alongside causal mediation analysis allows researchers to transparently discuss the strength of their findings in relation to untestable assumptions.

**APPLYING THE FRAMEWORK**

The causal mediation analysis framework builds on the potential outcomes framework introduced in the previous chapter. A causal mechanism is defined as a mediator $M(t)$ that takes two potential outcomes, $M(1)$ or $M(0)$, depending on the treatment of nonviolent or violent resistance, respectively. In any given campaign, only one outcome is realized; the other one remains counterfactual. Expanding the potential outcomes framework, Imai et al. (Imai et al. 2011) thus defines the average causal mediation effect (ACME) for an individual campaign $i$ to be

$$ACME_i(T) = Y_i(T, M_i(1)) - Y(T, M_i(0))$$

where $T$ represents the treatment (nonviolent or violent), and $Y$ represents the probability of democracy. The ACME is the change in the probability of democracy caused by the manipulating the mediator to take the outcome it would realize under the opposite treatment, while holding the treatment constant. This notation allows for
non-linearity or interaction effects, as we can estimate the average causal mediation effect for nonviolent and violent cases separately.

\[ ACME_i(0) = Y_i(0, M_i(1)) - Y_i(0, M_i(0)) \]
\[ ACME_i(1) = Y_i(1, M_i(1)) - Y(1, M_i(0)) \]

In the above equations, \( Y_i(0, M_i(0)) \) and \( Y_i(1, M_i(1)) \) are observable, while \( Y_i(0, M_i(1)) \) and \( Y_i(1, M_i(0)) \) are counterfactual. We can also define the average direct effect (ADE), which composes the remaining, unmediated effect of the treatment.

\[ ADE_i(T) = Y_i(1, M_i(t)) - Y_i(0, M_i(t)) \]

In words, the average direct effect is the change in the probability of democracy induced by changing the treatment, but holding the outcome of the mediator constant.

Again, we can define the ADE for violent and nonviolent campaigns separately.

\[ AD_i(0) = Y_i(1, M_i(0)) - Y_i(0, M_i(0)) \]
\[ ADE_i(1) = Y_i(1, M_i(1)) - Y_i(0, M_i(1)) \]

The ACME and ADE of every campaign can be averaged to produce the overall estimated effects of \( ACME, ACME(0), ACME(1), ADE, ADE(0), \) and \( ADE(1) \).

Using these, we can define two additional quantities of interest. First, the total effect

\[ Total \ Effect = ACME + ADE \]

which is the sum of the indirect and direct effects of nonviolent resistance on the probability of democracy, and is theoretically equivalent to the average treatment effect estimated in Chapter 3. Second, we can define the proportion mediated,

\[ Prop. \ Mediated(t) = \frac{ACME(t)}{Total \ Effect} \]
which represents the proportion of the total effect explained by the average causal mediation effect. The higher the proportion mediated, the higher the effect of the ACME relative to the ADE. Using the causal mediation analysis framework, each of the discussed quantities of interest are point identified and can be used to make valid inference under Imai et al.’s (2010) assumption of sequential ignorability.

Sequential ignorability is a two-part assumption. First, I hold the strong ignorability assumption invoked in Chapter 3 and Chapter 4, assuming that assignment into violent or nonviolent treatments is ignorable, or essentially random, conditional on observed pretreatment covariates. By creating a balanced sample over a large vector of possible pretreatment confounders and demonstrating that the results are reasonably insensitive to violations of the strong ignorability assumption, as I do in Chapter 3, I make the assumption more credibly, although there is ultimately no way to eliminate the possibility of unobserved confounders in observational research.

Second, I assume that the observed mediators are ignorable, conditional on the treatment status and the same observed pretreatment covariates. Although similar to the first part of sequential ignorability, this part is a strong assumption, because it requires no posttreatment confounding. Imai et al. (2010) demonstrate that there is no way to test whether this assumption is true, and it is impossible to satisfy this assumption, even by randomizing both the treatment and the mediator.

In this project, the second part of sequential ignorability implies that if there are multiple causal mechanisms, they are not causally related to each other. For example, if I find that participation and success both mediate the effect of nonviolent resistance, then under the second part of sequential ignorability, I must assume they
are causally independent from each other. This would be problematic, because Chenoweth and Stephan (2011) have discovered a relationship between participation and success. But, as I later show, I do not need to address this problem, as there is evidence for only one causal mechanism; I find no evidence for posttreatment spuriousness. However, the second part of sequential ignorability is still concerning, so I later conduct analysis to show how sensitive my results are to sequential ignorability.

**POTENTIAL MEDIATORS**

I use and modify existing variables from the NAVCO 2.0 data set to measure the hypothesized mediators (Chenoweth and Lewis 2013). Table 5.1 summarizes how each concept is measured. Additionally, I report the number of observations, mean, and standard deviation of each variable as they relate to the genetic matched sample created in Chapter 3.

Participation is measured by the number of people participating in a campaign’s peak event, the largest event that occurs during the course of the campaign. As Table 5.1 shows, the peak event of nonviolent campaigns is, on average, more than seven times larger than the peak event of violent campaigns. In my models, I normalize the participation variable by using its logarithm, consistent with Chenoweth and Stephan (2011).

Success is coded 1 if the campaign achieves its stated strategic objectives of regime change, anti-occupation, or territorial secession during the course of the campaign; 0 otherwise. Consistent with Chenoweth and Stephan’s (2011) finding, the
campaigns in the genetic sample reveal that strategic success occurs more than twice as often in nonviolent campaigns as it does in violent campaigns.

Parallel institutions measures the presence of alternative institutions created by the violent or nonviolent campaign. Specifically, I measure the presence of any of the following parallel institutions: an education system, a social welfare system, a judicial system, traditional media, or new media. As Chenoweth and Stephan write, these are consensual institutions “necessary to manage conflicts and power relationships nonviolently” (Chenoweth and Stephan 2011, 208). As Table 5.1 shows, the violent campaigns and nonviolent campaigns represented in the genetic sample are, interestingly, both likely to create nonviolent, democratic parallel institutions.

<table>
<thead>
<tr>
<th>Variable</th>
<th>Definition</th>
<th>Group</th>
<th>N</th>
<th>Mean</th>
<th>St. D.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participation</td>
<td>Number of people in campaign’s peak event. 500 = 1 – 999; 5,000 = 1,000 – 9,999; 50,000 = 10,000 – 99,999; 250,000 = 100,000 – 499,999; 750,000 = 500,000 – 1 million; 1,000,000 &gt; 1 million</td>
<td>Violent</td>
<td>68</td>
<td>54,150</td>
<td>64,764</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Nonviolent</td>
<td>66</td>
<td>405,800</td>
<td>383,226</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Total</td>
<td>134</td>
<td>227,300</td>
<td>324,071</td>
</tr>
<tr>
<td>Success</td>
<td>1 = campaign achieves stated objectives; 0 = otherwise</td>
<td>Violent</td>
<td>72</td>
<td>0.264</td>
<td>0.444</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Nonviolent</td>
<td>72</td>
<td>0.736</td>
<td>0.444</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Total</td>
<td>144</td>
<td>0.500</td>
<td>0.502</td>
</tr>
<tr>
<td>Parallel Institutions</td>
<td>1 = campaign creates parallel education, social welfare, court, or media institutions; 0 = otherwise</td>
<td>Violent</td>
<td>59</td>
<td>0.864</td>
<td>0.345</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Nonviolent</td>
<td>66</td>
<td>0.727</td>
<td>0.449</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Total</td>
<td>125</td>
<td>0.792</td>
<td>0.408</td>
</tr>
</tbody>
</table>
ESTIMATION

To estimate the direct and mediated effects of nonviolent resistance on the probability of democracy, I use the R package mediation (Tingley et al. 2013), developed by the creators of the causal mediation analysis framework. For each hypothesis, I estimate two necessary statistical models: the mediator model, which uses the mediator as the dependent variable and the treatment and pretreatment covariates as independent variables, and the democracy model, which uses democracy as the dependent variable and the mediator, the treatment, and the same pretreatment covariates as independent variables.

I estimate each model using appropriate statistical techniques. Because the democracy variable is binary, the democracy model is always estimated using a logistic regression.\(^{16}\) Logistic regression is also used to estimate the mediator model for the success variable and the parallel institutions variable, for the same reason. The (lagged) participation variable is continuous, so I model it using ordinary-least-squared regression. In all models, the estimates are conditional on the pre-existing Polity score,\(^ {17}\) which was a statistically significant predictor in Chapter 3’s model of democracy, and the fifteen other possible confounders that are accounted for by using the matched sample from Chapter 3.

Then, for each hypothesis, I pass both models into the package’s mediate() function with two parameters. I relax the no-interaction assumption, including an

---

\(^{16}\) Again, the binary variable describes whether a state is democratic (Polity score greater than or equal to 6), five years after the end of the violent or nonviolent campaign. Later, I test the robustness of my findings on whether a state is democratic two, ten, and twenty years after the end of conflict.

\(^{17}\) Measured one year before the end of the conflict.
interaction term in each democracy model, taking advantage of the fact that the causal mediation analysis framework allows interactions between the mediator and the baseline treatment status. This allows me to uncover causal mediation effects when the proposed mediator is moderated by whether a campaign is violent or nonviolent. Furthermore, I calculate nonparametric bootstrap confidence intervals for each point estimate using 1,000 resamples. Because this project represents the first attempt in testing and modeling the causal mechanisms between nonviolent resistance and democracy, and because there is no empirical evidence to suggest the need for more sophisticated models, I do not try to test for additional moderators or multiple, causally-dependent mechanisms. I reserve more complicated causal mediator models for future research.

RESULTS

The results appear in Figure 5.2, Table 5.2, and Figure 5.3. The first figure compares the ACME, ACME(0), and ACME(1) point estimates and 95 percent confidence intervals for the three hypotheses. At the 0.05 statistical significance level, it reveals no evidence for either the democratic institutions hypothesis or the mobilization hypothesis. None of the average causal mediation estimates are statistically significant, nor are the associated interaction terms.\(^\text{18}\) There is evidence for the success hypothesis, which I discuss later.

If we accept the 0.10 significance level, there is marginal evidence for only the mobilization hypothesis. ACME(0) for the participation mediator is -0.110 ["embali"]

\(^{18}\) Full estimation results for the democratic institutions hypothesis and the parallel institutions hypothesis reported in the Appendix.
0.182, 0.011] \( (p = 0.08) \), which is estimates the change in the probability of democracy induced by changing participation to the outcome realized under nonviolent resistance, while remaining violent. In other words, the counterfactual scenario in which violent campaigns attract as many participants as nonviolent campaigns—large-scale violence—is related to lower probability of democracy after conflict. Because ACME(1) is not statistically significant, the effect does not work the other way: nonviolent resistance campaigns that attract as many participants as violent campaigns are unrelated to either higher or lower probability of democracy.

**FIGURE 5.2** Average Causal Mediation Effects of Hypothesized Causal Mechanisms
Table 5.2 shows in-depth point estimates and confidence intervals for the success hypothesis, which is the only hypothesis that finds strong support. I discuss each column one at a time. The no-interaction column shows the average causal effects, assuming violent and nonviolent campaigns are related to the mediator in the same way. Looking at only the no-interaction column, it appears as though there is no causal mediation effect. The point estimate is 0.034, and the 95 percent confidence interval $[-0.036, 0.134]$ includes zero ($p = 0.26$). As expected, nonviolent resistance on average has a positive and statistically significant direct effect ($p = 0.00$) and total effect ($p = 0.00$) on the probability of democracy. The proportion of the average total effect explained by the ACME is 0.100 $[-0.117, 0.429]$, although it is statistically insignificant at the same level as the ACME ($p = 0.26$).

The next column demonstrates the value of relaxing the no-interaction assumption. While assuming no-interaction, which is imposed by many causal mechanism models, we might conclude that nonviolent resistance is unmediated by success. This column shows that nonviolent resistance is mediated by success, except it works through the interaction between success and the baseline treatment status—in other words, the causal mechanism is moderated by insurgents’ tactical choice. ACME(1) and ADE(1) are both positive and statistically significant, suggesting that nonviolent resistance increases the likelihood of democracy both directly and indirectly. The success mediator is responsible for an increase in the probability of democracy of 0.112 $[0.021, 0.293]$ ($p = 0.02$). The proportion of the total effect explained by this indirect effect is 0.33 $[0.060, 0.995]$ ($p = 0.02$). The average direct
effect is 0.382 [0.229, 0.582] \((p = 0.00)\). In total, nonviolent resistance tactics increase the probability of democracy in nonviolent campaigns by

\[
ACME(1) + ADE(1) = 0.112 + 0.382 = 0.494
\]

Accounting for the causal mechanism of success, the effect of nonviolent resistance on nonviolent campaigns is higher than estimated in Chapter 3 of 0.42, and also higher than Chenoweth and Stephan’s (2011) original average estimate of 0.45.\(^{19}\)

| TABLE 5.2 Estimated Direct and Mediated Effects of Success on Post-Conflict Democracy |
|-------------------------------------------|-----------------------------------------------|-----------------------------------------------|
|                                          | No interaction                                | With interaction                             |
|                                          | Average effect                               | Under treatment | Under control |
| Mediation 0.033                          | [-0.036, 0.134]                              | 0.112*           | -0.045        |
| Direct 0.303*                            | [0.134, 0.471]                               | 0.382*           | 0.225*        |
| Total 0.337*                             | [0.190, 0.504]                               | 0.337*           | 0.337*        |
| Prop. Mediated 0.100                     | [-0.117, 0.429]                              | 0.333*           | -0.134        |

Note: * \(p < 0.05\). Each cell shows a point estimate and its corresponding 95% confidence interval, calculated using 1,000 nonparametric bootstrap simulations. The second column show estimates under the no-interaction assumption; the third and fourth relax this assumption.

The final column shows that violent campaigns do not benefit from the hypothetical nonviolent treatment in the same way that nonviolent campaigns do. If violent campaigns are hypothetically treated with nonviolent resistance, holding the mediator’s outcome constant, the probability of democracy increases by 0.225 [0.001, 0.406].

\(^{19}\) Chenoweth and Stephan’s (2011) do not attempt to model the effect of success on the probability of democracy either independently or as a causal mechanism of nonviolent resistance.
0.406] \( (p = 0.05) \), which is lower than the equivalent value for nonviolent campaigns, ADE(1). However, the effect is still positive and statistically significant. Meanwhile, ACME(0) is statistically insignificant. If the likelihood of success was elevated in violent campaigns to match that of nonviolent campaigns, but the treatment remained violent, there would be no change in the probability of democracy. These results show that success does not mediate the broader relationship between tactical choice and democratization, only the relationship between nonviolent tactics and democratization.

Figure 5.3 illustrates the results another way, showing estimates from the nonviolent column of Table 5.2 and estimates from the mediator and democracy models. The mediated effect is shown above the success variable, the direct effect is shown along the dotted line, and the estimated causal increase in probability between variables is shown along the solid arrows. Nonviolent resistance increases the probability of success by 0.40 (0.31) \( (p = 0.00) \), while the interaction of nonviolent resistance and success increases the probability of democracy by 0.41 (0.28) \( (p = 0.02) \). In fact, the success mechanism explains an estimated one-third, 0.33 \([0.060, 0.995]\), of the relationship between nonviolent resistance and democracy.

![FIGURE 5.3 Path Diagram of the Average Causal Mediation Effect on Nonviolent Campaigns](image-url)
To illustrate the intuition behind the findings, Figure 5.4 shows the predicted probability of democracy given three parameters: the nonviolent treatment, the success outcome, and the pre-existing Polity score. Although the figure obscures the causal mediation framework by modeling the success mediator as exogenous, rather than endogenous, to the use of nonviolent tactics, it shows two aspects of the estimation results particularly well. First, it shows that even when violent campaigns are guaranteed to achieve success, the probability of democracy is essentially equivalent to the case in which the violent campaign fails. Second, it shows that adopting nonviolent resistance, even when the success rate is extraordinarily low, is

\[ \text{Calculated using the democracy model specified under the Estimation section. Statistical uncertainty estimates omitted to reduce clutter.} \]
predicted to significantly increase the probability of democratic outcomes relative to even successful violent resistance campaigns.

**ROBUSTNESS TESTING AND SENSITIVITY ANALYSIS**

To test the strength of my findings, I perform both robustness testing and sensitivity analysis. In Table 5.3, I compare the nonviolent treatment column of Table 5.2 to the equivalent quantities of interest in alternative specifications of the dependent variable—whether democracy exists two, ten, and twenty years after the end of nonviolent campaigns—and find that the results are robust.

As Table 5.3 shows, the success mediator is robust until ten years after the end of nonviolent conflicts, with point estimates that are consistently over 0.10. In fact, the ACME(1) may even be disproportionately higher two and ten years after the end of conflict than it is five years after the end of conflict. The proportion of the total effect that is mediated by success is 0.515 [0.16, 1.15] ($p = 0.01$) after two years and 0.551 [0.05, 1.99] ($p = 0.04$) after ten years. Even twenty years after the conflict, when the point estimate is statistically insignificant, the estimated average causal mediation effect remains relatively high. Thus, the effect of nonviolent resistance on the probability of democracy transmitted via elevated success rates is robust many years after the end of conflict.
TABLE 5.3 Estimated Direct and Mediated Effects of Success on Short- and Long-term Post-Conflict Democracy

<table>
<thead>
<tr>
<th></th>
<th>With interaction (Treatment)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Average 2 Years</td>
</tr>
<tr>
<td>Mediation</td>
<td>0.181* [0.06, 0.33]</td>
</tr>
<tr>
<td>Direct</td>
<td>0.410* [0.23, 0.59]</td>
</tr>
<tr>
<td>Total</td>
<td>0.351* [0.19, 0.50]</td>
</tr>
<tr>
<td>Prop. Mediated</td>
<td>0.515* [0.16, 1.15]</td>
</tr>
</tbody>
</table>

Note: * p < 0.05. Each cell shows a point estimate and its corresponding 95% confidence interval, calculated using 1,000 nonparametric bootstrap simulations. For simplicity, only treatment interaction reported. Full models for each are reported in the appendix.

Similarly, I find that the remaining average direct effect of the nonviolent treatment is robust over time. Although ADE(1) after ten years is statistically insignificant, ADE(1) is robust in every other time frame, including two, five, and even twenty years after conflict. The average direct effect of nonviolent resistance is positive as many as twenty years after the end of the nonviolent campaign. These robustness tests suggest that the findings uncovered in this chapter are not products of chance, but rather products of a forceful underlying relationship.

I use sensitivity analysis to analyze the sensitivity of the ACME(1) effect to violations of sequential ignorability. Because Tingley et al. (2013) have not yet implemented a method to test sensitivity when the mediator and outcome models are both binary, I re-estimate the causal mediation analysis using the continuous Polity
variable as the dependent variable in the outcome model. The estimation results discussed earlier are essentially equivalent.

\[
\text{ACME}_1(R^2_M, R^2_Y), \ sgn(\lambda_2\lambda_3) = 1
\]

FIGURE 5.5 Sensitivity of the Average Causal Mediation Effect on Nonviolent Campaigns to Violations of Sequential Ignorability

In Figure 5.5, the contour lines represent the true ACME(1), $R^2_M$ and $R^2_Y$ represent the proportion of the mediator and outcome model explained by unobserved confounders, and the bolded contour line represents the combinations of $R^2_M$ and $R^2_Y$ that result in a zero ACME(1). ACME(1) is positive, and the result from this study
holds, as long as the product of $R_M^*$ and $R_T^*$ does not equal or exceed 0.181. For instance, ACME(1) equals zero if unobserved confounders explain 44 percent of the variation in success of campaigns and also 41 percent of the variation in the political outcome of democracy. This finding is even less sensitive to potential violations of the key assumption than the estimation results from Chapter 2.

DISCUSSION

My primary finding is that the success mechanism exists, and that it is moderated by insurgents’ tactical choice. The effect of nonviolent resistance can thus be decomposed into two separate effects. The direct effect increases the probability of democracy by 0.382 [0.229, 0.582], while the indirect effect increases the probability by 0.112 [0.021, 0.293]. The latter represents the average effect transmitted through the success mediator, which explains one-third of the democratizing effect of nonviolent tactics. More specifically, nonviolent resistance increases the likelihood of success by 0.40 (0.31), and the co-occurrence of success and nonviolent resistance increases the probability of democracy increases by 0.40 (0.31). Furthermore, I find evidence that the success mediator is robust over time. The mediated effect is positive and statistically significant until 10 years after the end of conflict, while the direct effect remains robust 2, 5, and 20 years after the end of conflict. Accounting for success mediator, the total effect of nonviolent resistance remains robust until 20 years after the end of conflict.

To find evidence that the relationship between success and democracy is moderated by tactical choice may be surprising. Recognizing that violent and
nonviolent campaigns are often alternative strategies used toward the same political goals—for example, violent insurgencies in Nepal (2006), Burma (1988), and El Salvador (1977-1991) fought for democracy—it may seem strange that even successful violent campaigns are significantly less likely to democratize than successful nonviolent campaigns.

However, this finding is consistent with and finds support for the theoretical explanation proposed by Chenoweth and Stephan (2011, 207). Nonviolent campaigns may increase the probability of democracy through success in two ways. First, successful nonviolent campaigns demonstrate that nonviolent tactics can be effective in attaining power, which informs the collective memory. In contrast, successful demonstrations of violence often lead to more violence (e.g., Collier and Sambanis 2002). Second, successful nonviolent campaigns may create an expectation that the postconflict regime will treat civilians nonviolently. The regimes that follow successful violent campaigns, however, often use violence to consolidate power (Chenoweth and Stephan 2011, 208).

I also find that the remaining two-thirds of the democratizing effect of nonviolent tactics is not explained by any of the hypothesized causal mechanisms. This suggests that there is either something intrinsic about nonviolent tactics that explains most of its causal effect, or there may be other causal mechanisms operating. In any case, this evidence allows existing theories to be reevaluated. Is something else going on? If so, what might it be? Further research on the causal mechanisms of nonviolent resistance is needed.
What to make of the two other hypotheses? In regard to the mobilization hypothesis, one theoretical implication is that the causal mechanisms that facilitate the success of nonviolent resistance campaigns are not the same causal mechanisms that facilitate democratization. Although mass mobilization may be the primary reason why nonviolent resistance campaigns succeed at achieving their campaign goals (Chenoweth and Stephan 2011), it does not appear to systematically facilitate democratization. Although the mobilization mechanism appears to show no evidence, the underlying explanation may still be true. There may be other mechanisms by which people foster their democratic skills that do mediate the relationship between nonviolent resistance and democracy, although this study does not discover any.

As for the democratic institutions hypothesis, a similar conclusion can be reached. There is no evidence that the presence of nonviolent, democratic parallel institutions systematically mediates the relationship between nonviolent resistance and democratization. However, it remains possible that particular parallel institutions matter, or that some democratic institutions are more consensus-based than authoritarian alternatives. Again, further research is necessary.

LIMITATIONS

In addition to the limitations outlined in Chapter 3, this study is further limited by relatively underspecified theory and by the inability to prove the key sequential ignorability assumption. The theories explaining how exactly nonviolent resistance leads to democracy are important contributions, but they fall short of tracing the causal process from beginning to end. For instance, while the participation
explanation makes intuitive sense—the fostering of democratic skills may increase the likelihood of democratization—it is unclear how democratic skills lead to the creation of important democratic institutions. Additionally, the key sequential ignorability assumption cannot be proved, even by randomizing both the treatment and the mediator does not satisfy the assumption (Imai et al. 2011, 770). However, as with Chapter 3, sensitivity analysis estimates exactly how insensitive my findings are to potential violations of the key assumption. My results can handle moderate violations of sequential ignorability.
CHAPTER SIX

CONCLUSION

Does the choice to adopt nonviolent tactics of civil resistance, rather than violent ones, lead to more democratic outcomes? If so, which democratic transitions can be attributed to nonviolent resistance, and which ones cannot? What causal mechanisms explain how and why there exists a relationship between nonviolent resistance and democratization? In investigating these questions, my research produces six central findings, which I summarize.

First, the observed relationship between nonviolent tactics and democratization is unlikely to be spurious. Accounting for several previously identified structural determinants of democracy and violent insurgency—such as economic development, oil, Cold War politics, country size, region, population, rough terrain, and political instability—does not diminish the empirical correlation between nonviolent tactics and democratization.

Second, the relationship between nonviolent tactics and democratization does not furthermore appear to be endogenous, supporting the evidence from an alternative test by Chenoweth (2011). The democratizing effect of nonviolent tactics remains sizeable, even when comparing violent and nonviolent campaigns that are essentially identical—as though insurgent tactics were randomly assigned. This increases confidence in the claim that nonviolent resistance is not also the result of democratization. I find that the observed relationship is insensitive to moderate
violations of the key strong ignorability assumption. The result holds as long as hypothetical unobserved confounders do not systematically increase the odds of choosing one tactic over another by a factor greater than 3.5.

Third, although there is no way to prove causality using observational research designs, the previous two findings increase our confidence in the claim that choosing nonviolent tactics of resistance increases the likelihood of democratization. The causal arrow does not appear to run the other way, nor is there evidence that the relationship is spurious. Moreover, this relationship is robust over time. I find that nonviolent resistance remains correlated with democratization for up until ten years after the end of conflict.

Fourth, nearly all of the nonviolent conflicts that emerged within non-democratic contexts and resulted in democratization probably would not have democratized when they did had they adopted violent, rather than nonviolent tactics. This is true for the 35 out of 36 cases between 1945 and 2006. In one case, the 2003 Rose Revolution in Georgia, democratization probably would have occurred anyway.

Fifth, one-third of the estimated democratizing effect of nonviolent tactics operates through the causal mechanism of campaign success. Because nonviolent campaigns are more likely than violent campaigns to achieve their stated campaign goals, and because successful nonviolent campaigns are more likely to democratize, success mediates the relationship between nonviolent resistance and democratization. On the other hand, I find no evidence for a mediated effect through mobilization or the presence of democratic parallel institutions.
Finally, two-thirds of the estimated democratizing effect of nonviolent tactics is not mediated through success and remains unexplained. Therefore, even when nonviolent campaigns fail to achieve their stated campaign goals, they are still more likely to democratize than if the same campaign had been conducted violently. Moreover, this illustrates in another way the previous finding that democracy is more likely to result from failed nonviolent campaigns than from successful violent campaigns (Chenoweth and Stephan 2011, 216).

LIMITATIONS

My research has been limited by several challenges that fall within one of three broad categories: data availability, necessary assumptions, and theory. To begin, there are nearly always observations or variables that are unavailable but that could have improved the generalizability or conclusiveness of empirical contributions. In my research, time-series, cross-sectional data were only available for civil conflicts between 1945 and 2006 and whose strategic goals were maximalist: regime change, territorial secession, or anti-occupation. It is unclear whether the findings in this study hold for civil conflicts before 1945 or that demand smaller goals, such as policy changes or institutional reform. Furthermore, the long-term political outcomes of civil conflicts are similarly uncertain, as many of the nonviolent civil conflicts examined in this research occurred in recent history. Additionally, although I am able to test many possible structural confounders, the full range of potential confounders is much larger than the set of collected data.
A second limitation relate to the untestable yet unavoidable assumptions necessary in observational studies. No amount of data on observed structural confounders, or sensitivity analysis, for that matter, can prove that there exist no unobserved confounding variables or omitted-variable bias. In the case of sequential ignorability, necessary for causal mediation analysis, even an experimental design does not satisfy the assumption’s full requirements (Imai et al. 2011, 770). Therefore, some of the causal claims tested in this thesis may ultimately be impossible to prove, although attempting to is important.

The remaining challenges relate to limitations in current theory. Although scholars have attempted to explain the causal mechanisms that lead violent and nonviolent tactics to different political outcomes (Chenoweth and Stephan 2011; Sharp 1973, 2008), many theoretical questions remain: what conditions affect the tactical choice between violent and nonviolent strategies? Are there features of nonviolent campaigns that simultaneously hinder the likelihood of democratization? What other causal mechanisms, if any, may account for the remaining two-thirds of the democratizing effect of nonviolent tactics? Further research is necessary to answer important theoretical questions.

FUTURE RESEARCH

Scholars who wish to build upon this research should investigate the ways in which tactical choice affects, not only the transition to democracy, but also the survival, durability, and consolidation of democracy, which are just as, if not more, important (Diamond 1997; Gasiorowski and Power 1998; Przeworski 2000). A related inquiry
might examine whether there are more nuanced qualitative differences between the
democratic regimes that emerge from different tactical choices. For instance, does
tactical choice systematically affect the creation of illiberal (Zakaria 1997),
parliamentary (Przeworski 2000), presidential (Przeworski 2000), or mixed
(Przeworski 2000) democracies? Scholars might also draw from the elite transitions
literature and investigate whether tactical choice affects modes of conflict resolution,
through negotiated pacts, impositions, or reform (Karl and Schmitter 1991). Finally,
future research may examine the network or diffusion effects that may be associated
with nonviolent resistance. The Color Revolutions at the turn of the century and the
recent Arab Spring suggest that nonviolent campaigns in one country may inspire
nonviolent campaigns in another; they also suggest that the outcome of nonviolent
resistance in one country may also have a network effect on the outcome of
nonviolent resistance in another country. To my knowledge, such an effect has not
been empirically tested or quantitatively estimated.

THEORETICAL IMPLICATIONS
This thesis contributes to the broad view that non-elites do matter for
democratization. As my findings demonstrate, there is variation in democratization
trends that cannot be explained by structural or elite explanations. Even accounting
for a range of structural determinants of democratization, the relationship between
tactical choice and democratization remains robust. Although structural factors may
constrain the role of human agents, and although elite interactions can clearly play an
important role both in and out of political opposition campaigns, the theoretical role of non-elites cannot be ignored.

Particularly, I show that the choices of one kind of non-elite—civilian insurgents—can influence their political regimes. Unlike non-elite explanations that assign causal primacy to collective social classes (Moore 1966; Rueschemeyer, Stephens, and Stephens 1992; Collier 1999) or define the relevant actors by their economic grievances (Acemoglu and Robinson 2006), I find that even ordinary civilians—for example, students, activists, church members, media elites, educators, ethnic groups, and former members of the military—who mobilize nonviolently in order to achieve political goals, may intentionally or unintentionally force democratic concessions. As some scholars have argued, democracy can be forged from below (Wood 2000; Ekiert and Kubik 2001; Teorell 2010).

My findings also begin to explain when democratization occurs, not just why or how. Structural explanations tend to have trouble explaining when democratic transitions occur (Przeworski 2000, 273), while elite transitions explanations tend to explain democratization using negotiations that are “too proximate” (Teorell 2010, 11) to the outcome. My various robustness tests over time have shown that the democratizing effect of nonviolent campaigns, and the effect of the success mechanism, can be observed as quickly as two years after the end of conflict. This suggests that democratic transitions often occur immediately after the conclusion of civil conflict.

Finally, my findings have implications on the theoretical understanding about why and how nonviolent campaigns lead to democratization. While I find no
evidence for the mobilization hypothesis or the democratic institutions hypothesis, there evidence does suggest that success mediates the relationship between nonviolent resistance and democracy. This may lend support to Chenoweth and Stephan’s (2011) theory, which offer two possible explanations. First, successful nonviolent campaigns demonstrate that nonviolent tactics can be effective in attaining power, which informs the collective memory. Second, successful nonviolent campaigns may create an expectation that the postconflict regime will treat civilians nonviolently. Both scenarios are more difficult following violent campaigns, whether successful or not (Chenoweth and Stephan 2011, 208).

This thesis performs some of the preliminary analysis necessary for academically understanding why nonviolent resistance appears to lead to democratization. However, many important questions remain: what other causal mechanisms may explain the observed relationship? Does nonviolent resistance affect the consolidation of democracy, or the type of democracy that emerges? Why do nonviolent campaigns sometimes not lead to democracy? Are there ways to incorporate the democratizing effect of nonviolent resistance into violent resistance? Further research is needed.

POLICY IMPLICATIONS

Even though our understanding about how nonviolent resistance leads to democracy is young, the results of this preliminary analysis provide at least one policy implication. Like the tactical choice that insurgents face, countries that wish to promote democracy in states facing civil conflict may broadly distinguish between
two kinds of foreign policy: nonviolent intervention and violent intervention. Nonviolent intervention may include declaring public disapproval, imposing economic sanctions, enforcing an arms embargo, withdrawing investors, or building organizational capacity. Violent intervention may include direct military support or material support in the form of weapons or money. Because successful violent campaigns are less likely to democratize than even failed nonviolent campaigns, democracy promoters should be particularly wary about violent intervention. Even when material or military support results in the demolition of an authoritarian regime, the likelihood of post-conflict democratization is relatively low. Rather, democracy promoters may find that more democratic outcomes may be produced when they support the nonviolent efforts of insurgents.
### APPENDIX

#### CHAPTER 3

**TABLE. Balance Statistics**

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<tr>
<td>Gen</td>
<td>144</td>
<td>39.68</td>
<td>42.79</td>
<td>-5.61</td>
<td>0.08</td>
<td>0.9</td>
<td>0.12</td>
</tr>
<tr>
<td>Polity (t-1), squared</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Full</td>
<td>211</td>
<td>40.54</td>
<td>39.76</td>
<td>3</td>
<td>0.85</td>
<td>0.86</td>
<td>0.52</td>
</tr>
<tr>
<td>Prop</td>
<td>150</td>
<td>40.54</td>
<td>47.01</td>
<td>-23.71</td>
<td>0.75</td>
<td>0.19</td>
<td>0.22</td>
</tr>
<tr>
<td>Gen</td>
<td>144</td>
<td>40.54</td>
<td>36.99</td>
<td>13.03</td>
<td>0.97</td>
<td>0.33</td>
<td>0.19</td>
</tr>
<tr>
<td>Mountainous terrain, squared</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Full</td>
<td>211</td>
<td>675.16</td>
<td>1113.65</td>
<td>-38</td>
<td>0.45</td>
<td>0.04</td>
<td>0.26</td>
</tr>
<tr>
<td>Prop</td>
<td>150</td>
<td>675.16</td>
<td>774.1</td>
<td>-8.55</td>
<td>0.75</td>
<td>0.59</td>
<td>0.48</td>
</tr>
<tr>
<td>Gen</td>
<td>144</td>
<td>675.16</td>
<td>658.74</td>
<td>1.42</td>
<td>1.03</td>
<td>0.78</td>
<td>0.23</td>
</tr>
<tr>
<td>Ethnic frac.,</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Full</td>
<td>211</td>
<td>0.26</td>
<td>0.35</td>
<td>-33</td>
<td>0.91</td>
<td>0.04</td>
<td>0.06</td>
</tr>
</tbody>
</table>
CHAPTER 4

Under strong ignorability, but not monotonicity, the identification bounds are calculated by

$$\max \left\{ 0, \frac{Pr(Y_i = 1 \mid X_i = 1) - Pr(Y_i = 1 \mid X_i = 0)}{Pr(Y_i = 1 \mid X_i = 1)} \right\} \leq p_A$$

$$\leq \min \left\{ 1, \frac{Pr(Y_i = 0 \mid X_i = 0)}{Pr(Y_i = 1 \mid X_i = 1)} \right\}$$

### TABLE. Frequency of Regime Categories in Countries That Experienced Democratic Transition Following Nonviolent Resistance, 1945 - 2006

<table>
<thead>
<tr>
<th>Polity Category</th>
<th>Frequency</th>
<th>Percent</th>
</tr>
</thead>
<tbody>
<tr>
<td>-10 to -5</td>
<td>25</td>
<td>69.4</td>
</tr>
<tr>
<td>-4 to 0</td>
<td>7</td>
<td>19.4</td>
</tr>
<tr>
<td>1 to 5</td>
<td>4</td>
<td>11.1</td>
</tr>
<tr>
<td>Total</td>
<td>36</td>
<td>100.00</td>
</tr>
</tbody>
</table>

**NOTE:** The Polity score is measured one year before the resolution of the nonviolent conflict.

### TABLE. Democratic Transitions After Nonviolent Campaigns, 1945 - 2006

<table>
<thead>
<tr>
<th>Campaign Name</th>
<th>Country</th>
<th>Polity (t = -1)</th>
<th>Polity (t = 5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Anti-Jimenez</td>
<td>Venezuela</td>
<td>-3</td>
<td>6</td>
</tr>
<tr>
<td>1958-1958</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Movement</td>
<td>Country</td>
<td>Year Start</td>
<td>Year End</td>
</tr>
<tr>
<td>----------------------------------------------</td>
<td>---------------</td>
<td>------------</td>
<td>----------</td>
</tr>
<tr>
<td>Greece Anti-Military</td>
<td>Greece</td>
<td>1973-1974</td>
<td></td>
</tr>
<tr>
<td>Carnation Revolution</td>
<td>Portugal</td>
<td>1973-1974</td>
<td></td>
</tr>
<tr>
<td>Bolivian Anti-Junta</td>
<td>Bolivia</td>
<td>1977-1982</td>
<td></td>
</tr>
<tr>
<td>Argentina pro-democracy movement</td>
<td>Argentina</td>
<td>1977-1983</td>
<td></td>
</tr>
<tr>
<td>Solidarity</td>
<td>Poland</td>
<td>1980-1989</td>
<td></td>
</tr>
<tr>
<td>Pakistan pro-dem movement</td>
<td>Pakistan</td>
<td>1983-1983</td>
<td></td>
</tr>
<tr>
<td>People Power</td>
<td>Philippines</td>
<td>1983-1986</td>
<td></td>
</tr>
<tr>
<td>Anti-Pinochet Movement</td>
<td>Chile</td>
<td>1983-1989</td>
<td></td>
</tr>
<tr>
<td>Diretas ja</td>
<td>Brazil</td>
<td>1984-1985</td>
<td></td>
</tr>
<tr>
<td>Uruguay Anti-Military</td>
<td>Uruguay</td>
<td>1984-1985</td>
<td></td>
</tr>
<tr>
<td>Anti-Duvalier</td>
<td>Haiti</td>
<td>1985-1985</td>
<td></td>
</tr>
<tr>
<td>South Korea Anti-Military</td>
<td>South Korea</td>
<td>1987-1987</td>
<td></td>
</tr>
<tr>
<td>Anti-Noriega</td>
<td>Panama</td>
<td>1987-1989</td>
<td></td>
</tr>
<tr>
<td>Bangladesh Anti-Ershad</td>
<td>Bangladesh</td>
<td>1987-1990</td>
<td></td>
</tr>
<tr>
<td>Timorese resistance</td>
<td>East Timor</td>
<td>1988-1999</td>
<td></td>
</tr>
<tr>
<td>Bulgaria Anti-Communist</td>
<td>Bulgaria</td>
<td>1989-1989</td>
<td></td>
</tr>
<tr>
<td>Hungary pro-dem movement</td>
<td>Hungary</td>
<td>1989-1989</td>
<td></td>
</tr>
<tr>
<td>Benin Anti-Communist</td>
<td>Benin</td>
<td>1989-1990</td>
<td></td>
</tr>
<tr>
<td>Mongolian Anti-communist</td>
<td>Mongolia</td>
<td>1989-1990</td>
<td></td>
</tr>
<tr>
<td>Period</td>
<td>Movement Description</td>
<td>Country</td>
<td>Under treatment</td>
</tr>
<tr>
<td>-------------</td>
<td>----------------------------------------------------------</td>
<td>---------------</td>
<td>-----------------</td>
</tr>
</tbody>
</table>

**CHAPTER 5**

**TABLE.** Estimated Direct and Mediated Effects of Democratic Institutions on Post-Conflict Democracy

<table>
<thead>
<tr>
<th></th>
<th>With interaction</th>
</tr>
</thead>
<tbody>
<tr>
<td>Average effect</td>
<td>No interaction</td>
</tr>
<tr>
<td></td>
<td>Under treatment</td>
</tr>
<tr>
<td></td>
<td>Under control</td>
</tr>
<tr>
<td></td>
<td>With interaction</td>
</tr>
<tr>
<td>----------------</td>
<td>------------------</td>
</tr>
<tr>
<td><strong>Mediation</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Direct</strong></td>
<td>0.379*</td>
</tr>
<tr>
<td></td>
<td>[0.176, 0.525]</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td>0.325*</td>
</tr>
<tr>
<td></td>
<td>[0.149, 0.474]</td>
</tr>
<tr>
<td><strong>Prop. Mediated</strong></td>
<td>-0.165</td>
</tr>
<tr>
<td></td>
<td>[-0.525, 0.140]</td>
</tr>
</tbody>
</table>

Note: * p < 0.05. Each cell shows a point estimate and its corresponding 95% confidence interval, calculated using 1,000 nonparametric bootstrap simulations. The second column show estimates under the no-interaction assumption; the third and fourth relax this assumption.
<table>
<thead>
<tr>
<th></th>
<th>Mediation</th>
<th>Direct</th>
<th>Total</th>
<th>Prop. Mediated</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.061</td>
<td>0.290*</td>
<td>0.351*</td>
<td>0.173</td>
</tr>
<tr>
<td></td>
<td>[-0.016, 0.147]</td>
<td>[0.123, 0.454]</td>
<td>[0.187, 0.500]</td>
<td>[-0.052, 0.465]</td>
</tr>
<tr>
<td></td>
<td>0.181*</td>
<td>0.410*</td>
<td>0.351*</td>
<td>0.515*</td>
</tr>
<tr>
<td></td>
<td>[0.055, 0.334]</td>
<td>[0.229, 0.590]</td>
<td>[0.187, 0.500]</td>
<td>[0.157, 1.149]</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.059</td>
<td>0.170</td>
<td></td>
<td>-0.169</td>
</tr>
<tr>
<td></td>
<td>[-0.171, 0.041]</td>
<td>[-0.032, 0.360]</td>
<td></td>
<td>[-0.598, 0.114]</td>
</tr>
</tbody>
</table>

Note: * p < 0.05. Each cell shows a point estimate and its corresponding 95% confidence interval, calculated using 1,000 nonparametric bootstrap simulations. The second column show estimates under the no-interaction assumption; the third and fourth relax this assumption.

### TABLE. Estimated Direct and Mediated Effects of Success on Post-Conflict Democracy (10 years later)

<table>
<thead>
<tr>
<th></th>
<th>Mediation</th>
<th>Direct</th>
<th>Total</th>
<th>Prop. Mediated</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.114*</td>
<td>0.128</td>
<td>0.241*</td>
<td>0.471*</td>
</tr>
<tr>
<td></td>
<td>[0.027, 0.253]</td>
<td>[-0.067, 0.328]</td>
<td>[0.075, 0.422]</td>
<td>[0.096, 1.590]</td>
</tr>
<tr>
<td></td>
<td>0.133*</td>
<td>0.147</td>
<td>0.241*</td>
<td>0.551*</td>
</tr>
<tr>
<td></td>
<td>[0.017, 0.321]</td>
<td>[0.061, 0.370]</td>
<td>[0.075, 0.422]</td>
<td>[0.051, 1.993]</td>
</tr>
<tr>
<td></td>
<td>0.095</td>
<td>0.108</td>
<td></td>
<td>0.392</td>
</tr>
<tr>
<td></td>
<td>[-0.026, 0.243]</td>
<td>[-0.123, 0.332]</td>
<td></td>
<td>[-0.106, 1.482]</td>
</tr>
</tbody>
</table>

Note: * p < 0.05. Each cell shows a point estimate and its corresponding 95% confidence interval, calculated using 1,000 nonparametric bootstrap simulations. The second column show estimates under the no-interaction assumption; the third and fourth relax this assumption.

### TABLE. Estimated Direct and Mediated Effects of Success on Post-Conflict Democracy (20 years later)

<table>
<thead>
<tr>
<th></th>
<th>Mediation</th>
<th>Direct</th>
<th>Total</th>
<th>Prop. Mediated</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.114*</td>
<td>0.128</td>
<td>0.241*</td>
<td>0.471*</td>
</tr>
<tr>
<td></td>
<td>[0.027, 0.253]</td>
<td>[-0.067, 0.328]</td>
<td>[0.075, 0.422]</td>
<td>[0.096, 1.590]</td>
</tr>
<tr>
<td></td>
<td>0.133*</td>
<td>0.147</td>
<td>0.241*</td>
<td>0.551*</td>
</tr>
<tr>
<td></td>
<td>[0.017, 0.321]</td>
<td>[0.061, 0.370]</td>
<td>[0.075, 0.422]</td>
<td>[0.051, 1.993]</td>
</tr>
<tr>
<td></td>
<td>0.095</td>
<td>0.108</td>
<td></td>
<td>0.392</td>
</tr>
<tr>
<td></td>
<td>[-0.026, 0.243]</td>
<td>[-0.123, 0.332]</td>
<td></td>
<td>[-0.106, 1.482]</td>
</tr>
<tr>
<td></td>
<td>Point Estimate</td>
<td>Confidence Interval</td>
<td></td>
<td></td>
</tr>
<tr>
<td>----------------</td>
<td>----------------</td>
<td>-----------------------</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mediation</td>
<td>0.062</td>
<td>[-0.054, 0.182]</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.117</td>
<td>[-0.042, 0.285]</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.008</td>
<td>[-0.161, 0.191]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Direct</td>
<td>0.269*</td>
<td>[0.012, 0.501]</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.323</td>
<td>[0.028, 0.572]</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>0.215</td>
<td>[-0.052, 0.487]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td>0.332*</td>
<td>[0.104, 0.521]</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.332*</td>
<td>[0.104, 0.521]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Prop. Mediated</td>
<td>0.188</td>
<td>[-0.190, 0.894]</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.351</td>
<td>[-0.156, 1.395]</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.025</td>
<td>[0.635, 0.798]</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: * p < 0.05. Each cell shows a point estimate and its corresponding 95% confidence interval, calculated using 1,000 nonparametric bootstrap simulations. The second column shows estimates under the no-interaction assumption; the third and fourth relax this assumption.
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